Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15617	0	0	0	0	I find it very suprizing that this chapter does not address at all cryospheric "natural" hazards such as snow avalanches, GLOFs, etc. It should be made clearer why this is the case and where such assessments take place. For example, on snow avalanches, Chapter 12 is quite cursory. There is a need for further harmonization and interlinkages between the chapters. [Samuel Morin, France]	Noted. Glacier and snow related hazards are covered in chapter 12.
61707	0	0	0	0	Please include a complete table of all acronyms/abbreviations used in the chapter. It would be much easier to reference a table at the beginning or end of the chapter than to flip through searching for the definition of an abbreviation. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Acronyms are not listed in the chapter but are available via the IPCC glossary.
101655	0	0	0	0	We, as representatives of an early career scientist community, are very concerned about the many placeholders and “dummy” figures within the report at the Second Order Draft (SOD) stage. This stage is the last chance for the science community to provide input and to evaluate the accuracy of the reported science. Therefore, whatever will be placed in these placeholder positions is not undergoing a scientific peer-review, which is an essential process for ensuring the robustness of the report as a whole. For example, Chapter 9, which we focused our review on, has 80 instances of placeholders in the text, which largely concerns numbers but also some interpretation. A lot of the figures seem not close to final (excluding formatting issues) and contain dummy figures that do not allow us to fundamentally assess the underlying science. Overall, these problems would be acceptable in the First Order Draft, but are problematic in the final review stage of the SOD. We understand that these issues are not induced by the authors, but rather by the overall timeline and planning of the report, since many results are missing at the time of finishing the SOD. Therefore, we suggest that more time needs to be attributed to the period between the finalization of CMIP simulations and the submission of SOD and no more changes to the scientific content (no more placeholders or dummy figures) should be allowed in the SOD. This period between the two stages of finalizing CMIP and the SOD is essential for ensuring the robustness of the underlying science of the report. [IAPSO ECS group review, United States of America]	Noted. Placeholders and dummy figures do not appear in the Final Government Draft.
101657	0	0	0	0	There needs to be consistency in labeling subpanels of figures either by referring to a), b), etc. or top, left, right, bottom, etc. panels. However, the former is preferred. These labels also need to be referred (e.g. Figure 9.1a) to in the text when describing a figure, which is currently not the case. [IAPSO ECS group review, United States of America]	Accepted. In most figures this approach has been taken, unless the figures are so matrix-ordered that it is not necessary.
101659	0	0	0	0	All figures showing trend maps are missing information on the significance of the trends. Stippling should indicate their significance otherwise it cannot be estimated how robust conclusions on changes of spatial patterns are. [IAPSO ECS group review, United States of America]	Accepted. For SOD there was still discussion about details of stippling. This is now accomplished in all relevant figures.
101661	0	0	0	0	We think "large" might be better than "deep" for the uncertainty since it is the clearer language (applies to the entire chapter). It is even less clear what is meant by a "deep" uncertainty when it is being mixed with "large" uncertainty. [IAPSO ECS group review, United States of America]	Rejected. Deep uncertainty is referred to in chapter 1 and SROCC.
85279	0	0	0	0	General point: I am sure reference format is decided at a high level by IPCC but I find the name and year rather than numeric reference style breaks up the sentences much more and makes them harder to read, especially as there are often long lists of references. However, I guess it also ensures that the reference author names are actually read, which one obviously can't do with numeric references? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is standard IPCC format
80485	0	0	0	0	Could you use one consistent factor for conversion between Gt and mm SLE. IMBIE used 360 mm SLE/Gt. ISMIP6 used 362.5 mm SL/Gt following Gregory et al 2019. [Heiko Goelzer, Belgium]	Accepted. 362.5 is used following Gregory et al. (2019). This is stated as a footnote in box 9.1
85357	0	0	0	0	Apologies if any of my comments include typos, are in error and/or are not clear. Feel free to drop me an e mail on patrick.hyder@metoffice.gov.uk if you would like to clarify any of them. I opted to try to read the whole chapter and comment on it as a whole, which obviously meant in many sections I have made comments on topics for which I am not an expert. I was therefore not sure if to include all of my comments but decided to include them all mainly in the hope that any misunderstandings on my part might help to identify for where complex issues the material could benefit from clarification for other non-expert readers. Obviously feel free to ingire any of them that are not relevant [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted
86645	0	0	0	0	One comment I forgot to make in my original comment set was to say how impressed I was by the chapter overall, what a mammoth effort it obviously was and what a staggering achievement it is to have managed to get it completed on the very tight time-scales. [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted

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86647	0	0	0	0	One comment I forgot to make in my original comment set was to say how impressed I was by the chapter overall, what a mammoth effort it obviously was and what a staggering achievement it is to have managed to get it completed on the very tight time-scales. [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted
14737	0	0	0	0	IPCC AR6 Ch 9 Lead/Contributing Authors are cited heavily themselves in Ch 9: 26% of all citations in the chapter text, excluding figure captions, point to Author-led studies, weighted in heavily to a minority of Lead/Contributing Authors. I'm not sure this is actually representative of the full breadth of work carried out by the relevant research communities since AR5. Below is analyis I carried out on citations versus Author citations, which I encourage Coordinating Lead Authors to review to ensure the full field is being objectively represented in the science presented in this Chapter. Total citations with Lead/Contributing Author as lead author=568 Total citations based on occurrence of "et al"=2176 Fraction of total citations that are Lead/Contributing Author-led papers=26% # of citations per Lead/Contributing Author: [('Adalgeirsdottir', 0), ('Ruiz', 0), ('Mora', 0), ('Dominguez', 0), ('Dörr', 0), ('Gan', 0), ('Gerland', 0), ('Hemer', 0), ('Hermans', 0), ('Kirwan', 0), ('Kossin', 0), ('Lowry', 0), ('Pellet', 0), ('Perry', 0), ('Ranasinghe', 0), ('Otin', 0), ('Savita', 0), ('Stouthamer', 0), ('Sweet', 0), ('Nurhati', 1), ('Capet', 1), ('Islam', 1), ('Quaas', 1), ('Rietbroek', 1), ('Xiao', 2), ('Krinner', 2), ('Derksen', 2), ('Huybrechts', 2), ('Kääb', 2), ('Lewis', 2), ('Roxy', 2), ('Drijfhout', 3), ('Mix', 3), ('Burke', 3), ('Gomez', 3), ('Solomina', 3), ('Yuan', 3), ('Hewitt', 4), ('Garner', 4), ('Hill', 4), ('Orton', 4), ('Hallberg', 5), ('Menary', 5), ('Pickering', 5), ('Smith', 5), ('Fox-Kemper', 6), ('Wahl', 6), ('Zika', 6), ('Marcos', 7), ('Roberts', 7), ('Gupta', 7), ('Jourdain', 8), ('Khan', 8), ('Yu', 9), ('Gregory', 9), ('Armour', 10), ('Hock', 10), ('Pattyn', 10), ('Church', 11), ('Frölicher', 11), ('Aschwanden', 12), ('Haumann', 12), ('Jackson', 12), ('Palmer', 13), ('Durack', 14), ('Liu', 14), ('Nowicki', 18), ('Slangen', 20), ('Kopp', 21), ('Notz', 21), ('Sallée', 21), ('Seroussi', 25), ('Edwards', 27), ('Goelzer', 27), ('Marzeion', 28), ('Bamber', 36), ('Golledge', 40)] [Jeremy Fyke, Canada]	Taken into account. The citations for CLAs and LAs has been checked. They represent 6% of the total citations and the numbers are: [('Adalgeirsdottir', 7), ('Drijfhout', 0), ('Nurhati', 0), ('Sallee', 7), ('Hemer', 0), ('Krinner', 1), ('Ruiz', 1), ('Xiao', 2), ('Mix', 2), ('Hewitt', 3), ('Fox-Kemper', 4), ('Nowicki', 10), ('Yu', 11), ('Slangen', 15), ('Edwards', 17), ('Notz', 19), ('Golledge', 20), ('Kopp', 26)]. Given the small % of total citations, we think that this demonstrates that the chapter has worked to represent a huge body of literature.
85193	0	0	0	0	General comment: From my personal non-expert perspective the statements on confidence & uncertainty did not seem fully consistent across the sections? For example, the Southern Ocean section stated reasonable confidence in several qualitative aspects model projections for a region which, although obviously critical for global climate, has huge known climate model biases, and cancelling errors, which spread through the closely coupled atmosphere-ocean-ice system. These errors are linked with many deficiencies in process-representation which I would expect to have implications for the fidelity of and uncertainties in many aspects of projections (e.g. see Hyder et al, 2018). Are the confidence statements in this section fully consistent with those of the Meredith et al, 2019, Special Report on the Ocean and Cryosphere in a Changing Climate, which I understood to suggest low confidence in many aspcts of Southern Ocean projections, given the known prevalent model errors? By contrast, for example the tropics section stated low confidence in projections, given the many biases? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The confidence statements have been rechecked to ensure that language has been used correctly.
85195	0	0	0	0	General comment: I found the level of technical detail in some sections fairly daunting and wonder if it might be useful to start with the summary overview paragraphs which link to the chapter summary before providing all of the technical detail rather than as you have done at least for some sections summarising at the end? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Space constraints mean it is not possible to do this.
85197	0	0	0	0	General comment: In their current form I found quite a few of the figures hard to see the detail in. However, I assume that in the full report they may be presented in a larger format. I also thought that for many of many of the figures, without too much effort, the colour scales and levels could be adjusted to make them much clearer? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Yes, the resolution and size of the figures has been improved wherever possible. The SOD was lower resolution, so that even upon zooming in the figures were not clearer.
85199	0	0	0	0	General comment: For me, it seems important to make the key point about the implifications of polar amplification clearly in the chapter, chapter summary and summary for policymakers (assuming that it is evident observations and projections of Arctic SSTs and/or sea-ice changes as well as near surface temperature changes over Arctic land)? In particular, it seems important to include an expert statement (linked to detail in the SST and sea-ice sections of this chapter) along the lines of that even with medium scenarios and waming of 2-3 deg C, many models suggest Arctic warming over 10 deg C in some months or in some cases even in the annual mean change (with obvious implications for impacts and potential feedbacks on the cryosphere, ecosystems and other linked aspects of regional and global climate)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Polar amplification is discussed in chapter 10.

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85201	0	0	0	0	General comment: For me, it seems important to make the key point about the implifications of changes in mountain glaciers and snow on the seasonal cycle of water supply via river flows in the chapter, chapter summary and summary for policymakers (unless it is outside the remit of this chapter)? For example, with less ice storage and melt the seasonal cycle of river flows will tend to revert towards the very different seasonal cycle of precipitation (minus evaporation) potentially necesitating huge and costly changes in societal infrastructure. For example, as I understand it one billion people depend on water from Himilayan glacial melt in some months of the year (though I am not a topic expert so this would need checking and perhaps it is more relevant to the working group II impacts report so does not need stating in this chapter)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Some of this content is covered in chapter 12.
85203	0	0	0	0	General point for IPCC report as a whole: some experts think it is important to state that given national commitments the RCP8.5 scenario is now fairly unlikely to be realised, e.g. see https://www.nature.com/articles/d41586-020-00177-3 "Emissions – the 'business as usual' story is misleading"? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. As far as possible other scenarios are referenced as well but this depends on availability of projections.
85213	0	0	0	0	The palaeo-climate information in the chapter is obviously really important to set the temporal context to past and potential future changes. Would a summary diagram be helpful with an irregular stretched time-scale covering Earth's full climate history with time-series of temperature, CO2 and sea-level and the names of the myriad of different palaeo-climate periods clearly labelled? As a non-expert I found I was often having to spend time on the web trying to understand the basic climate background to many palaeolimate periods refered to in the various sections. This type of summary overview diagram would have helped considerably? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Documentation of the basic climatic background is addressed primarily in Chapter 2, and in the TS, to help non-expert readers gain a big-picture view of paleo records. However, given space limitations in AR6, a general introductory primer on paleo data was not possible. In Chapter 9, specific aspects of paleo records are focussed where needed to assess key ocean, ice, and sea level processes, where relevant to understanding the mechanisms contributing to projections and their uncertainties. We have added more callouts to the other chapters were other aspects of paleo records are assessed. This decision to spread paleo information where relevant among the various chapters was mandated in the new outline structure for AR6.
131581	0	0	0	0	Whole chapter: Given the high importance of sea level change for small islands globally one would expect in this chapter detailed information on this topic. E.g.different scenarios for different regions - but islands are not mentioned at all in the whole chapter. There is a table on climate extremes for small islands in Chapter 11 and a section on Small Islands in chapter 12. The Small Islands section in chapter 12 (12.4.7.4) is also referencing to chapter 9. [Hans Poertner and WGII TSU, Germany]	Noted. This chapter is on process understanding. Regional information is in subsequent chapters.
131583	0	0	0	0	Whole Chapter: information on Africa appear to be underrepresented in this chapter - there is only one information on Africa and only in the FAQ without reference. This is also striking as there appears to be no author from Africa involved in the chapter. [Hans Poertner and WGII TSU, Germany]	Noted. The oceans around Africa are considered-e.g., the Agulhas current, tropical oceans and the Eastern boundary upwelling zone. It is unfortunate that we do not have a contributing author representing Africa but the author team has made a conscious effort to be as diverse as possible with contributing author invitations.
109247	0		0		Linkage with Chapter 12 text, figures and tables are not very strong. Relevant section links between Chapter 9 and Chapter 12 are shown in SPM and TS. In the context of regional climate information for impacts, a number of sections/subsections of Chapter 12 can be referred/linked from Chapter 9. Relevant sections are 'Climate Impact Driver for open and deep oceans' (Table 12.1, Table 12.2, section 12.3.6) , 'Coastal and oceanic' subsections for each continental region (12.4.1-9), 'snow and ice' sub-sections for each continental region (12.4.1-9), 'Open and deep ocean' section (12.4.8, Table 12.10, Figure 12.14), Polar and Terrestrial Section (12.4.9) , projections of extreme SL (Figure 12.7) etc. [A.K.M Saiful Islam, Bangladesh]	Accepted. Work with the polar regions group, sea level contacts, and through tables linking to regional ocean information improved in FGD since SOD.
62233	0				It would be clarifying to state that the estimate ranges for all projections are not linear and the median value provided is a measure of the distribution of the variable and should be not expected to be in the geometric center of the limits. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The statistical properties of the projections are clarified in the revised chapter

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39965	0				Assessment conclusions should be provided in all sub/sections in a structured traceable account of how these statements were derived. For example, sections / subsections can start with previous IPCC report conclusions (AR5 or AR6 Special Reports) and then provide an update of the more recent literature, clearly laying out the lines of evidence. Each section / subsection can then conclude with assessment statements that must include IPCC confidence language. Some sections currently read more as a review of the literature rather than an assessment of our current understanding of the literature. [TSU WGI, France]	Accepted. This is now the case in all sections.
67873	0				This chapter is closely related to the findings and developments in Chapter 8. In general this chapter is balanced and comprehensive, incorporating the latest scientific findings. [Ruandha Agung Sugardiman, Indonesia]	Noted
67875	0				Consistency between summary and descriptions in the chapter. [Ruandha Agung Sugardiman, Indonesia]	Noted. Traceability between main text and executive summary is improved.
71971	0				This is a long and in some places dense chapter. I focussed mostly on the sea level sections and was pleased to see the projections are what I would expect in an updated AR5 set of projections. However, the authors need to be clearer in exactly how the final projections were completed and in particular on how the likely/very likely ranges are justified. Given that you stress the deep uncertainties, how do you justify specifying very likely ranges for the projections. I was very surprised to see how incomplete the chapter was at this very late stage. [John Church, Australia]	Noted. The very likely range for sea level projections is not presented in the chapter given the deep uncertainty that exists.
67877	0				Some parts are still in the form of "placeholder" [Ruandha Agung Sugardiman, Indonesia]	Noted
67879	0				This chapter is special, as it describies robustness not just limitations. [Ruandha Agung Sugardiman, Indonesia]	Noted
22575	0				Chapter 9 is somewhat of an outlier in how it is using confidence and likelihood language. Most other chapters (to date - I am reading chronologically) are using opening statements to recap AR5 / SR findings then undertake the assessment text making parsimonious use of the confidence / likelihood language text, followed by a summary finding statement couched in the uncertainty / likeelihood language. Chapter 9 embed confidence / likelihood throughout and has no closing assessment statement per section. From my perspective three potential issues arise from this: i) there is much more frequent use of confidence / likelihood qualifiers in chapter 9 than preceding chapters; ii) it is less clear what the delta between AR5 / SRs and AR6 is and; iii) the traceability between the assessment text and the ES statements is somewhat less clear than is the case for other chapters. A whole-of-report perspective would call into question whether moving more toward the style adopted by many other chapters would be helpful here. [Peter Thorne, Ireland]	Accepted. This is now the case in all sections-generally on a paragraph level.
22583	0				There is a tendancy to cite very many papers to support key points. I suspect that in many cases not all the citations included are absolutely necessary to support the findings and that judicious thinning may be appropriate. As an assessment and not a review it is only necessary to quote the key papers supporting a point being made - there is no need to be comprehensive. There is also in some sections a propensity for self-citation which should be avoided to the extent possible. [Peter Thorne, Ireland]	Noted. The number of papers cited has been reduced and self-citation minimised.
132411	0				In order to have a single location to check marine extremes and a possible coordinated assessment on this topic between chapters 9 and 11, it could possibly be considered to expand Box 9.1 to cover different types of marine extremes (including marine heatwaves and extreme sea levels, but also coastal extremes) and to provide there a synthesis on this topic (the material on marine heatwaves could in this case be moved to the main text and just be summarized in the cross-chapter box). [Sonia Seneviratne, Switzerland]	Rejected. The sea level extremes fit better into section 9.6. The cross-chapter box has been converted to a chapter box as the topic of marine heatwaves is too narrow for a cross-chapter box.
96831	0				_Scenario-dependence of SLR: We are surprised about the main message currently conveyed by Ch9 that there is no scenario dependence on the future Antarctic sea-level rise contribution, particularly evident from figure 9.20. This is not consistent with previous information from the science community that ice mass loss non-linearities are dependent on future warming (e.g. De Conto and Pollard 2016). In fact, most results presented in table 9.2 point to a scenario dependence. Is it possible that the CMIP5 model uncertainty (e.g. the regional southern ocean temperature issues. see e.g. WGI AR5 Figure 9.15) could mask scenario dependent ice sheet model responses? There are still more studies to be assessed (placeholders in table 9.2) and we hope that they will assess CMIP5 forcing dependence e.g. by comparing RCP2.6 and RCP8.5 ice sheet responses forced by the same model. Please thoroughly check the results, as the current scenario independence message could send a wrong message to policy makers. [Nicole Wilke, Germany]	Taken into account. AIS projections are assessed both from ISMIP6 and LARMIP2 plus insight from other projections for drivers. The assessment is clear that ice loss from AIS is likely to continue this century with increasing, scenario dependent ice discharge mitigated slightly by increased snowfall which is also scenario dependent. Mass loss is assessed as increase post 2100. The response has been looked at carefully both in terms of different models forcing ice sheets and the regional response. The uncertainty in the Southern Ocean temperatures seen in WG1 AR5 figure 9.15 is reflected in large sensitivity of projections to basal melt.

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96833	0				Ch9 reads frequently like a very thorough and detailed review starting with papers in the mid 2000s, and not so much like an assessment. However some paras are assessments, but it is not clear if all the details from the review - like part is needed in this report. Please streamline the chapter. [Nicole Wilke, Germany]	Noted. The text has been revised to have clearer assessment statements
96835	0				Ch9 reports many findings that have been already stated in the SROCC. Overlap with the SROCC should be avoided and the text could be shortened significantly. It should be clearly stated what is new compared to the SROCC, see for example Ch 2.3.3.1 or 2.3.3.2. It is not sufficient to state that if not otherwise noted results confirm former reports. If there are no new findings, the text should be substantially shortened instead of repeating the assessment of the SROCC. [Nicole Wilke, Germany]	Accepted. All sections now state clearly the advances since SROCC.
22597	0				It is good that subjects are considered together - this is a better structure than several other chapters. The ocean sections have integrated observations, attribution, model evaluation and projections along with theory without the use of subheadings. The cryosphere sections seem to have split these up. I personally find the ocean section approach more accessible. Regardless, the heterogeneity is something that ideally should be addressed so that the chapter as a whole reads as one coherent unit without obvious stylistic changes. [Peter Thorne, Ireland]	Taken into account. It is very difficult to make the chapter completely heterogeneous and the differences in approaches are there for considered reasons.
96837	0				The chapter gives a good overview on the current activities, however, it seems to be like „all inclusive“; it should be shortened where possible, maybe in cases of redundancy to other text parts of the report. Please think about the potential target audiences. [Nicole Wilke, Germany]	Accepted. The final text has removed repetition especially with AR5 and SROCC.
96839	0				We doubt that all the regional details given here are all relevant for the AR6, suggest to focus the selection on AR6 relevant topics. [Nicole Wilke, Germany]	Taken into account. The chapter aims to focus on policy relevant regions.
96841	0				We note considerable overlap with other chapters of the AR6 WG1 report and strongly urge the authors to provide consistent and coherent information across chapters, avoiding duplications. [Nicole Wilke, Germany]	Accepted. The final text summarises relevant points from previous chapters rather than repeat.
41035	0				There are inconsistencies regarding the closure of the sea level budget. P81, L34-35 ""closing the observational GMSL budget for the period 1900-1990" is in contraction with: p 110 L12-14 "The sea level budget for the 20th century has not been closed due a lack of observations from many of the ocean and cryosphere components, but more recent periods show consistency among observational budgets" [TSU WGI, France]	Accepted. This is reconciled in FGD.
41039	0				There are many sections in chapter 9 that are not focusing so much on the improvements since the last reports and, more importantly, when sections do focus on recent improvements they tend to highlight assessments and improvements since AR5 and not SROCC. Additionally some sections cite parts of previous reports without explicitly mentioning that those are previous IPCC reports. [TSU WGI, France]	Accepted. AR5/SROCC starting points are clearer.
22639	0				There is a tendancy to mix scenario nomenclature. Some sections use high / low emissions nomenclature. Others use high / low mitigation scenarios. These are diametrically opposite constructs but a non-expert could easily be confused, Suggest standardising within the chapter (and across the report) to avoid the potential for reader confusion here. [Peter Thorne, Ireland]	Taken into account.. The chapter uses high/low emissions scenario where possible
40051	0				Could you please make sure that sea levels projections/contributions are expressed with the same units throughout the chapters (or at least by sections). for instance it's weird to have units in meters in p53 L38-39 and L47-48 but then in centimetres at the beginning of the same paragraph (p53, L29). [TSU WGI, France]	Taken into account. The chapter uses m and mm where possible.
22649	0				It feels to me like box 9.1 comes way too early. By the time a reader going sequentially gets to the sea-level section this is ancient history text. It would surely make more sense appearing just prior to section 9.6 as a primer for the substantive sea level assessment text which would then immediately follow? [Peter Thorne, Ireland]	Noted. The box ties together topics from across chapter 9 and we therefore think it is best to start the chapter with an overview of these processes rather than wait until 9.6.
93577	0				There is very little reference to the cryosphere in Tibet in this chapter (one mention concerning permafrost). This region was not covered by SROCC (as neither polar nor high-mountain area), so should get more attention here. [Miriam Jackson, Norway]	Noted. Tibet is mentioned where appropriate to this assessment

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40341	0				Has Greenland contribution to sea level rise accelerated or not? It is not that clear from the text. (e.g. p51, L17-18 "There is high confidence that submarine melt and iceberg calving contributed to the acceleration of mass loss from Greenland," vs executive summary p6, L11-17 "Greenland was likely close to mass balance in the 1990s and there is high confidence that annual mass losses have been consistently negative since the early 2000s", which doesn't mention any acceleration.) [TSU WGI, France]	Accepted. The inconsistency between the main text and the ES has now been resolved. Rather than acceleration we refer to changes in the mass loss over the IMBIE period.
22701	0				General work on figures to increase their accessability would be useful. Figures should be able to be used in talks and lectures as standalone items. This means paying attention to things like adding self-describing titles (pertains to almost every single figure presently) and ensuring font sizes are easily legible. I have called out some, but by no means all, cases individually [Peter Thorne, Ireland]	Accepted. All figures have been improved, at least somewhat, with this expectation in mind.
100015	0				One key uncertainty of future cryospheric change, the Antarctic ice-sheet response to warming, has potentially severe consequences on long-term sea-level rise, which is a prime concern for SIDS. The current chapter assessment points to a scenario-independent response of the Antarctic ice-sheet (based on ISMIP6) despite all other lines of evidence showing a clear scenario dependence. We urge the author team to rigorously vet the presented main results, as it would have potentially extremely detrimental consequences to arrive at the 'for the Antarctic ice sheet it doesn't matter which emission scenario is realised' narrative for the wrong reasons. [Caroline Eugene, Saint Lucia]	Taken into account. AIS projections are assessed both from ISMIP6 and LARMIP2 plus insight from other projections for drivers. The assessment is clear that ice loss from AIS is likely to continue this century with increasing, scenario dependent ice discharge mitigated slightly by increased snowfall which is also scenario dependent. Mass loss is assessed as increase post 2100. The response has been looked at carefully both in terms of different models forcing ice sheets and the regional response.
39605	0				Fig. 3.14 of the AR5 report shows that sea level rise seems to follow a 60-70 years cycle which is documented in a number of papers and is possibly related to the Atlantic Multidecadal oscillation: Schlesinger and Ramankutty, 1994; Ogurtsov et al., 2002; Klyashtorin and Lyubushin, 2003; Loehle, 2004; Zhen-Shan and Xian, 2007; Carvalo et al., 2007; Swanson and Tsonis, 2009; Scafetta, 2009; Akasofu, 2010; D'Aleo and Easterbrook, 2010; Loehle and Scafetta, 2011; Humlum et al., 2011; Chambers et al., 2012; Lüdecke et al., 2013; Courtillot et al., 2013; Akasofu, 2013; Macias et al., 2014; Ogurtsov et al., 2015, Ollila 2017. The apparent acceleration of sea level rise might be related to the last ascending phase of the natural cycle. See Fig. 2a of http://dx.doi.org/10.1016/j.earscirev.2016.02.005. Professor Nix-Axel Mörner published tens of peer-reviewed papers on sea-level rise which minimize the alarmism. He is even not cited in this Chapter. This expert reviewer recommends that his findings which contradict the paragraphs of this chapter should be cited and discussed. Parker and Ollier (Phys. Sci. Int. J. 6,119 (2015)) reports an average sea level rise of only 1 mm/year by considering more than 500 tide gauges, viz. more than the 240 of NOAA. Donchyts et al (Nature Climate Change 6,810, (2016)) reported an average extension of lands on seas. Luijendijk, A. et al, 2018 (The state of the world's beaches. Nature Sci. Rep. DOI:10.1038/s41598-018-24630-6) also show an average extension of beaches. How to reconcile such an extension with sea level rise? Major revision of the paragraphs about sea level rise, therefore, is strongly recommended. [François Gervais, France]	Rejected. Unsubstantiated claims and personal opinions
84155	0				The Antarctic ice-sheet response to warming has potentially severe consequences on long-term sea-level rise, which is a serious concern for SIDS. The current chapter assessment points to a scenario-independent response of the Antarctic ice-sheet (based on ISMIP6) despite all other lines of evidence showing a clear scenario dependence. We urge the author team to rigorously vet the presented main results, as it would have potentially extremely detrimental consequences [Jeffers Cheryl , Saint Kitts and Nevis]	Taken into account. This has been looked at carefully. Scenario independence of the Antarctic ice sheet was evident in AR5 Chapter 13. However, the projections of the Antarctic ice sheet have been revised incorporating both revised surface temperatures in the emulator (which are consistent with the climate sensitivity in chapter 7) and to include the results of LARMIP which is more sensitive to basal melt. The revised projections give greater scenario dependence than in second order draft. Low confidence projections are also shown from projections that allow Marine Ice Cliff Instability and from Structured Expert Judgement which also have scenario dependence.

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40639	0				Please check the use of this IPCC uncertainty language term. Are you able to provide a traceable account to assigning this uncertainty statement? Note that likelihood statements are quantified terms - phrases like likely and very likely have quantifiable probabilities associated with them. Please check it has been used correctly here. Please refer to the IPCC guidance note on uncertainty: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf or the presentation from the pre-LAM activities [TSU WGI, France]	Taken into account. The confidence statements have been rechecked to ensure that language has been used correctly.
107461	0				Section 9.4 has a paragraph to introduce the section (page 48, lines 29-37) which is very useful. No other 9.x sections have this, but would improve the structure and layout of the sections if an introductory paragraph was included for all 9.x sections [Jennifer Walker, United States of America]	Rejected. Space constraints mean that this is not possible for each section. A decision on whether to maintain this for 9.4 will be made after internal draft.
35783	0				Let me say at the outset that this chapter presents a heroic and often successful attempt at tackling a sprawling and complex problem at the same time that the literature continues to grow rapidly. The need to produce a clear and useful chapter are high because policy makers worldwide will without doubt be focused on the output of this chapter. However, significant problems remain, including the lack of a single voice throughout the text on some key issues. Despite attempts by the authors to assure that the relationships between findings from AR5, SROCC, and this chapter are clear, the presentations of 9.4 and 9.6 on this point are quite opaque. For example, in a key area, projection of the AIS component and its affect on total SLR, the description of differences between AR5, SROCC and this report's findings are unclear and at one place in the text, potentially misleading and appearing to contradict the tables. At very least, an opportunity is missed to explain why total SLR for the upper end of the high scenario is 0.2m less in this chapter than SROCC - an explanation users will want to hear. A second problem area is the dismissal of MICI in 9.6 and 9.7. For example, box 9.3 presents a dismissive critique of MICI but justifies this assessment in a poorly written paragraph lacking sufficient substantiation. The tone of 9.6 and 9.7 with regard to MICI is inconsistent with the much more even-handed treatment in 9.4. A third area of concern is a lack of clarity, consistency, and/or appropriate terminology with respect to the use of uncertainty language. Fourth is a missed opportunity to clearly elucidate in a unified way the reasons for greater confidence in processs-based models than was the case in AR5 - instead, arguments are scattered about several places. A fifth area that needs work is the lack of a highlighted and unfied discussion of projections for 2100-2300 and longer term commitment. Finally, after presenting diverse approaches to projection, the authors present no bottom line message that policy makers can take away from the complex picture laid out. They do not sufficiently justify why the ISMIP/emulator approach is used nor why LARMIP results are not used to make projections. They do not explain how policy makers can use the integrated/probabilistic projections or those from elicitations, as SROCC did. Details are given in my subsequent comments. [Michael Oppenheimer, United States of America]	Taken into account. We thank the reviewer for an extremely constructive review of the chapter. These points have all been considered carefully. 1. The comments on MICI discussion are accepted and single voice should now be more apparent from stronger editing. 2. Text and tables have been revised to explain traceability to AR5 and SROCC especially in 9.4 and 9.6. 3. The differences between SROCC and our projections is explained in 9.6. 4. The comments on MICI are accepted and have been carefully rewritten in box 9.4 (was 9.3) and 9.7. 5. Uncertainty language has been carefully considered throughout. 6. Confidence statements in process-based models have been made and this has been unified in an ES statement on ice sheet models. 7. The 2100-2300 discussions are now a thread throughout the chapter. 8. The figures support the message for policy makers in a better way than previously giving clarity to the low confidence projections. 9. LARMIP and structured expert judgement are now both used in the ice sheet and sea level projections.
88011	0				Chapter 9 is, despite some placeholders, in a very well shape. Congratulations. The only note I would like to make is on a too extensive use of acronyms. They may be reduced to some extent, particularly if they are not generally familiar to a broader community. [Georg Kaser, Austria]	Taken into account. Acronyms are spelled out where possible.
107467	0				Some sections about projections are called "Future projections' while others are just 'Projections.' Should be consistent [Jennifer Walker, United States of America]	Accepted. This has been harmonised.
107469	0				Some sections about observations are "Observed changes," others are "Recent Observations." Should be consistent [Jennifer Walker, United States of America]	Accepted. This has been harmonised.
32211	0				We think that the structure of the document is very clear for non-physicists scientists, a point which has to be underlined. [Eric Brun, France]	Noted
32213	0				The focus on sea level change is well appreciated. It is an aspect that we were waiting for, considering the level of potential impacts such evolution would have on small islands, coastal populations and maritime policies. [Eric Brun, France]	Noted
32215	0				The emphasis on ocean stratification, salinity changes and regional circulation are important aspects which were not identified in such a way in the previous reports. [Eric Brun, France]	Noted
32217	0				We would like to underline the quality of the complementary elements in the chapter, especially boxes, cross chapter boxes and frequently asked questions which are remarkable. One point of improvement is in the figures. Several of them are not very understandable, either because of their size or the lack of legend. [Eric Brun, France]	Accepted. The figures have been significantly improved since the SOD. Thanks for the compliment on other aspects.
40921	0				The fact that " increased ocean heat content in response to anthropogenic CO2 emissions is essentially irreversible on human timescales" (p24, L33-34) seems to be absent from the executive summary and SPM. [TSU WGI, France]	Accepted we have now added the notion of irreversibility in the ES

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32219	0				It seems difficult to stress the progress made on these topics in comparison with the AR5 report, bearing in mind that the SROCC has appeared in the meantime; but it would be interesting to make a table or a paragraph summarising the progress made and the new points identified as sensitive in the future. For example it seems to us that "salinity" driver was not so visible in the SROCC. [Eric Brun, France]	Noted. Advances since SROCC are highlighted in the text.
32221	0				As a conclusion, Chapter 9 is of very high quality for the reader. We feel that the combination of model projections, presence or absence of observational data and knowledge of processes is widely developed, making it possible to identify priorities in order to advance the knowledge. [Eric Brun, France]	Noted
40413	0				Ice mass loss is sometimes expressed as a negative number, sometimes as a positive (e.g. p69, L3 vs L21). Could you please ensure consistency [TSU WGI, France]	Accepted. Mass loss and gain is used with positive numbers, if a sign is used this is described with mass change (to interpret - as mass loss and + as mass gain)
41437	0				Heartfelt thanks to the author team for their hard work to provide the SOD. Generally, the chapter appears to be in good shape with a couple of issues to iron out before the final draft. For the FGD, it would not only be very interesting but also extremely important, from a policymaker perspective, to see the chapter assessment for the full set of SSP marker scenarios, including the crucial SSP1-19 pathway. [Alexander Nauels, Germany]	Noted
40927	0				The fact that there seem to be "no tipping point for the loss of Arctic summer sea ice" , which is to me an important conclusion of chapter 9, seems to be absent from the SPM. [TSU WGI, France]	Noted, this information has been passed on to the SPM team.
41439	0				From what i can see, two major SLR-related issues would need attention for the final draft: 1) AIS SLR projections informed by ISMIP6: From Seroussi et al 2020 it is clear, that the AIS SLR projections from ISMIP6 not only depend on a wide range of ISM reponses, but are, to a large degree, the result of regionally very divergent CMIP5 forcing input for the same scenario. If the emulator does not account for this issue (and for the imbalance in models used under individual scnearios), the assessment would potentially arrive at the dangerous scenario-independent AIS SLR projection message for the very wrong reasons. Please ensure that the underlying methodology that would lead to such a headline statement is robust. In addition, the general impression the reader gets from reading the AIS sections is rather negative in terms of progress made. In reality, the ice sheet modelling community has achieved a lot of progress which would be nice to reflect more in the general 'tone'. 2) Long-term SLR beyond 2100: It is crucially important that the global post-2100 SLR assessment is forwarded and coordinated better with Ch04 (which is currently lacking a comprehensive assessment of this topic). Also, it is crucially important to add a figure to section 9.6.3.5 in order to provide the line of sight for the ES and figure SPM.7 panel f. [Alexander Nauels, Germany]	Taken into account. 1) AIS projections are assessed both from ISMIP6 and LARMIP2 plus insight from other projections for drivers. The assessment is clear that ice loss from AIS is likely to continue this century with increasing, scenario dependent ice discharge mitigated slightly by increased snowfall which is also scenario dependent. Mass loss is assessed to increase post 2100. The general tone of the achievements of the community has been improved both in ES, 9.1 and 9.4. 2) Long term SLR beyond 2100 including projections for 2300 are now included in 9.6.3.5 along with a table and a figure showing timing uncertainty for sea level horizons which cover 2300.
88039	0				Make sure that the SSPs are written in the same way throughout the chapter, e.g. SSP1-2.6 in the text but SSP126 in figures. [Georg Kaser, Austria]	Accepted. Implemented as requested,
64361	1	1	1	1	B Marzeion, Regine Hock, B Anderson, A Bliss, N Champollion, K Fujita, M Huss, W Immerzeel, P Kraaijenbrink, J-H Malles, F Maussion, V Radic, D R Rounce, Akiko Sakai, S Shannon, R S W van de Wal, H Zekollari Partitioning the Uncertainty of Ensemble Projections of Global Glacier Mass Change, Earth Future, DOI: 10.1029/2019EF001470. [roderik van de wal, Netherlands]	accepted, the updated GlacierMIP results are presented
64363	1	1	1	1	I am surprised to see that the paper by Marzeion et al. 2020 is not used as baseline for the glacier projections, B Marzeion, Regine Hock, B Anderson, A Bliss, N Champollion, K Fujita, M Huss, W Immerzeel, P Kraaijenbrink, J-H Malles, F Maussion, V Radic, D R Rounce, Akiko Sakai, S Shannon, R S W van de Wal, H Zekollari Partitioning the Uncertainty of Ensemble Projections of Global Glacier Mass Change, Earth Future, DOI: 10.1029/2019EF001470. [roderik van de wal, Netherlands]	accepted, the updated GlacierMIP results are presented
64397	1	1	1	1	Why are there no model studies used for the estimate of the Antarctic contribution over the period 2100-2300. It looks like only the old expert judgement is used. SROCC did estimate that why not use that? [roderik van de wal, Netherlands]	Taken into account. The 2300 projections are now based upon a combination of process models, literature projections, and structured expert judgement.
64399	1	1	1	1	SROCC put a lot of emphasis on the period 2100-2300 yielding a very different estimate than AR5 for high scenarios. It is worth emphasizing this point [roderik van de wal, Netherlands]	Taken into account. The main projections are now extended through 2150, and projections for 2300 are also discussed.
64401	1	1	1	1	It seems that the updated GlacierMIP results are not used Marzeion et al. 2020 are not used… [roderik van de wal, Netherlands]	accepted, the updated GlacierMIP results are presented
147	1	1	1	1	Not a fan of the Oxford comma in the title. Suggest change to "Ocean, cryosphere and sea level change" or "Changes in the Ocean, Cryosphere and Sea Level". [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Implemented as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64403	1	1	1	1	The Kopp method used for TWS seems rather ad-hoc, more should be possible [roderik van de wal, Netherlands]	Noted. Unfortunately, there is limited additional literature on this topic since AR5.
64405	1	1	1	1	I object against the use of the very likely range for the projections based on models. I think our physical understanding is not sufficient to allow for a very likely range. This view-point has been taken by AR5 and SROCC what is the reasoning why this concept is left now? there is little progress in ice sheet models which justifies this [roderik van de wal, Netherlands]	Noted. The very likely range for sea level projections is not presented given the deep uncertainty that exists.
64443	1	1	1	1	In general there is not much justitifiction for arbitrary choices which are made throughout the chapter [roderik van de wal, Netherlands]	Taken into account. The confidence statements have been rechecked to ensure that language has been used correctly.
64445	1	1	1	1	A Table clarifying the difference in projections between ar5, srocc and Ar6 should be very helpful [roderik van de wal, Netherlands]	Accepted. Added Table 9.8 comparing AR5, SROCC, and AR6.
64449	1	1	1	1	Paleo sea level does not seem to digisted equally in the different chapeters [roderik van de wal, Netherlands]	Taken into account. The paleo sea level in 9.6.2 is framed in the context of the role of ice sheets in large changes in sea level, including issues of long-term lags and the related processes
64451	1	1	1	1	there is very (too) little emphasis for extremes [roderik van de wal, Netherlands]	Rejected. Sea level extremes are included in 9.6.4 with links to chapters 11, 12 and the Atlas. Marine heatwaves appear in a box and link to chapters 11 and 12.
78053	1	1	1	1	You have made good progress with projections of SLR since the last draft, but many important quantitative assessments have not yet been finalised. For instance, the likely ranges for important terms in GMSLR have not yet been evaluated, and in some cases it appears that the methodology hasn't been entirely decided. The description and assessment of regional RSLR is missing; there seems to be no text on this at present, and only a placeholder in the Exec Summ, whereas there was a substantial section about it in the AR5, in recognition of its practical importance. I appreciate that you have been hampered by the late availability of CMIP6 results, but my concern is that this is the last draft of the chapters for review. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These items have all been addressed.
78055	1	1	1	1	In the AR5, there is an argument for why 5-95% model ranges for sea level projections were assessed as "likely". Does the same argument apply in AR6? AR5 did not give "very likely" ranges, but you have done so (though not in the Exec Summ). The basis for that assessment of probability doesn't seem clear to me. Although useful if justified, this is a serious step, which should not be taken without sound footing. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The likely range presented is the 17-83rd range consistent with the emulator in chapter 7. The very likely range for sea level projections is not presented given the deep uncertainty that exists.
12437	1	1	1	37	It was a nice read of chapter-9, it is very focused to several topics and most of assessments are comprehensive. The general comments are (1) the chapter need a thorough proof-reading and remove some low-priority discussions such as year-to-year variation and associated processes if words limitation is an issue. (2) The time scales should be better separated, now there are come mixture of the discussion on decacal-scale and long-term changes, it is easy to generate confusion. (2) the CA list is heavily developed country weighted (>90%), especially UK/US weighted (a rough count shows >10 from UK, >14 from US). This is not good for IPCC. [Lijing Cheng, China]	Taken into account. 1. The chapter has been proof-read. 2. Time scales have been made clear. 3. The author team has made an effort to be as diverse as possible with careful consideration of contributing author invitations taking into account experience as well as nationality.
96843	1	1	116	1	It is unfortunate that there are multiple occasions throughout the chapter were placeholders were required due to presently unfinished analysis which makes it harder to review the chapter. Some of the figures contain placeholders as well as budget numbers given are incomplete. [Nicole Wilke, Germany]	Noted
46513	1	1	116	25	An extensive set of review papers focussing on the state-of-the-science of ice sheet and ocean interactions, published in Oceanography in 2016 (volume 29, issue no. 4), seems to be largely absent from or severely under-cited in this chapter. [Stephen Price, United States of America]	Noted. We thank the reviewer but note that the assessment has focussed on post-SROCC papers where possible (since 2016 papers were available to SROCC assessment).
29659	1	1	223	55	It seems that the assessment of confidence is a bit confusing. For high confidence, should it be more citation to support? In many palces, a high confidence is supported only by one citation. [Aixue Hu, United States of America]	Noted. The number of papers cited are appropriate to the confidence statement to reflect the amount of evidence and agreement.
67115	1	1	233	70	There are far too many acronyms. Although many are frequently used in specialized disciplines, IPCC reports should be accessible to non-experts. Unfortunatly this is a trend in the recent IPCC reports and some chapters are not easily accessible by a broader audience or even scientists from other fields. This report should not make the same mistake. Please avoid acronyms as much as possible. Often they can be shortened when the context is clear or even just deleted. [Regine Hock, United States of America]	Taken into account. Acronyms are spelled out where possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67117	1	1	233	70	I commend the authors on a nicely written and well-researched chapter. However, I thought this chapter should only update SROCC, but, it reads largely as if SROCC did not exist. It essentially repeats to a large degree the very same messages, background information and findings found and detailed in SROCC often based on the same cited pre-SROCC literature. Only rarely a point is made that conclusions/results differ from SROCC. A count yields 28 times the occurrence of 'Since AR5' while only 9 cases of 'Since SROCC or Since AR5 and SROCC.' If the intention was to build on SROCC and not repeat SROCC the chapter unfortunately fails to a large extent to deliver a succinct update; the chapter could be shortened greatly and focus largely on what is new instead of somewhat rerwording statements/conclusions with essentially identical content as in SROCC. It is overall not clearly articulated for all components what is new and especially what is different. For example, how do the results, where updated SSP/CMIP6 calculations are available, differ from the SROCC CMIP5 simulations? [Regine Hock, United States of America]	Accepted. All sections now state clearly the advances since SROCC.
67119	1	1	233	70	It appears that confidence levels are not consistent with SROCC in all instances. Each message should be carefully checked and compared to the level SROCC assigned and checked if a difference in this report compared to SROCC is supported by new literature. [Regine Hock, United States of America]	Accepted. AR5/SROCC starting points are clearer.
44927	1	1	300	70	New paper published: Toda, M., Watanabe, M. Mechanisms of enhanced ocean surface warming in the Kuroshio region for 1951–2010. Clim Dyn 54, 4129–4145 (2020). https://doi.org/10.1007/s00382-020-05221-6. This aspect of enhanced warming near the continents should be assessed on Chapter 9. It affects tropical cyclone intensities, as described by Mei, Wei, Xie, Shang Ping. Intensification of landfalling typhoons over the northwest Pacific since the late 1970s. Nature Geoscience. 2016, vol. 9, no. 10, p. 753–757. [Masaki Satoh, Japan]	Accepted. Reference considered in both the SST section and the western boundary current section
10693	1	1			Whole Chapter: I strongly discourage the use of "Medieval Warm Period" and "Little Ice Age" throughout this chapter. They are inaccurate terms, and puts in the readers mind that it was warm/cool uniformly over some ill defined periods (Neukom et al., "No evidence for globally coherent warm and cold periods over the preindustrial Common Era", Nature 2019) . Different studies (and chapters in this report!) use inconsistent dates to define the periods, so one person's "LIA" is often different to another persons "LIA". Dates should be used instead to be clear what periods are being referred to. The word "Medieval" refers to a historical period, 5th to 15th century, in mostly western Europe. It does not look good to continue using the term to refer to periods of climate changes in other parts of the world which are not Europe. The "Little Ice Age" was not a little ice age (Lockwood et al, "Frost fairs, sunspots and the Little Ice Age", Astronomy and Geophysics, 2017). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	accepted. These names for intervals have been removed.
65823	1	12	5	14	Suggest clarification and nuance. The text states that "The ocean has warmed at all levels and continue to do so (very high confidence), contributing to future sea-level rise even under low emissions scenarios. Since the 1970s, most ocean warming has been in the Southern Ocean …" However, the text also states (at 9-13, 7-9) that "Since the 1970s, the temperature around the subpolar Southern Ocean has decreased ..." [Kushla Munro, Australia]	Accepted. We have now clarified that text.
7635	1	24	1	24	Change "Dan Lowry (New Zealand)" to "Daniel Lowry (New Zealand/USA) [Daniel Lowry, New Zealand]	Accepted. Implemented as requested
64831	1	28	1	28	Maria Santaloria Otin should be corrected with Maria Santolaria Otin [Martin Ménégoz, France]	Accepted. Implemented as requested
65825	1	32	5	32	Suggest changing the term "predicted" to "projected". [Kushla Munro, Australia]	Noted. The use of the word predicted has been replaced by projected in the text.
24543	1		223		I feel honoured to have a privilege to go through that chapter and greatly appreciate the excellent work done by all the authors in compiling this excellent work. I am pleased to convey that no substantive error was observed in the draft document. Wanted to go through other chapters as well as but due to the time constraints could not do that. [Asif Inam, Pakistan]	Noted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68911	1				Paleoclimate information has been successfully distributed across the WG1 report, as envisaged by the scoping documents. The Paleo BOG has now developed key messages to consolidate and convey the most policy-relevant paleoclimate content, and to advance it to the summary documents (TS & SPM). Information about the cryosphere and oceans that predates the mid-19th century is essentially absent from the CH9 Executive Summary, which is unexpected considering these are the slowest moving components of the climate system, and considering paleo is the only source of observational evidence on multi-centennial time scales, and considering future changes will be driven by both human and natural causes. The Paleo BOG looks to CH9 to include critical information needed to address two of the key messages from paleoclimate and to include the outcome of the assessment in its Executive Summary, including: (cont.) [Darrell Kaufman, United States of America]	Noted. See responses to comments 68913 and 68915
68913	1				Paleo key message I. (model veracity) How well do models with paleoclimate forcings simulate large-scale Earth system changes? Justification: Models are the basis for detailed climate projections of future climate and its impacts. Future climate conditions are beyond the range of human observations; therefore, past climate enables an evaluation of model performance under forcings that may be of similar magnitude to future climate change. To address this key message, please provide an assessment of how well cryosphere (ice sheet, sea ice and permafrost) and ocean (circulation and salinity) models that are run with paleoclimate forcings accurately simulate the proxy evidence. For example, see AR5 Fig. 9.18 which compares models and observations for LGM salinity and temperature. Is it fair to say that the proxy data support the models? Or is a better characterization that the models do not include all existing feedbacks and might misrepresent key processes needed to correctly simulate the dynamics of climate change on long timescales (cf. SR1.5; Fischer et al. 2018)? Or should both the point and counter points be made? (cont.) [Darrell Kaufman, United States of America]	Noted. There was unfortunately not space to evaluate many PMIP simulations in this process-focussed chapter, so after considerable discussion, a group decision was made to leave that to other chapters. Nevertheless, paleo informs the process discussion which is the focus of Chapter 9, particularly at longer timescales and related to ice, sea level, and ocean heat content. The new PMIP4 results relevant to constraints on climate sensitivity and are addressed in Chapter 7.
68915	1				Paleo key message II. (multi-centennial climate) What are the long-term effects of sustained warming across the Earth system? Justification: Paleoclimate provides the only empirical evidence for how the slow-responding components of the climate system operate over centuries and millennia, including their irreversibility. To address this key message, please provide summary ES statements and underlying chapter text that assesses the paleo evidence for (1) ice-sheet stability and sea level rise associated with long-term warming; and (2) century- to millennial-scale hemispheric seesaws in heat transfer and lags in the Earth's energy budget imposed by deep ocean circulation. [Darrell Kaufman, United States of America]	Taken into account. In ES the paleo information is summarised as response to warming levels. A assessment of the effects of sustained warming on the full earth system based on paleo data and models is beyond the scope of Chapter 9, which focussed specifically on the processes contributing to ocean, ice, and sea level change. For issues of assessing long-term warming and potential irreversibility, 10,000 year model projections are noted related to ocean heat content, ice sheets and glaciers, and sea level. Issues related to paleo hemispheric seesaws were thoughtfully considered by the chapter team, and a difficult decision was made to cut discussion that appeared in an earlier draft, due to space limitations.
98209	1				At the Chapter 1 drop-in meeting on 4 June, a strong case was made to add an assessment of the times series of global sea level and temperature that are inferred from marine oxygen isotopes, as illustrated in several places in the report, including Fig. 1.3, TS.2, CCB2.1, and maybe elsewhere. Although sea level and temperature estimates based on marine isotopes have been a mainstay of paleoclimatology for decades, and some version of the marine stack has probably been presented in every AR, I am in favor of doing this assessment because these data should be held to the same standard as other climate metrics in the report. To do so, however, will require additional text and at least two complementary approaches: (1) comparison with proxy evidence for sea level and temperature for multiple reference periods: MH, LGM, LIG and MIS11, which can be done in CH2, and (2) scrutiny of the assumptions involved in converting marine d18O to sea level and temperature and how they are handled by different reconstructions, which could be done in CH9. [Darrell Kaufman, United States of America]	Noted that oxygen isotopes have been assessed in essentially every AR, and in SROCC. Given that strong previous coverage, without major new breakthroughs in the published literature relevant to ocean, ice, or sea level processes at a precision informative for AR6, and with limited space, Chapter 9 needed to focus on new developments in paleo that are specifically insightful into ocean, ice, and sea level processes relevant to projections, rather than evaluating the inner workings of the proxy measurements themselves.
2515	2	3	2	3	sea level change should be sea-level change as in BOX9.1 line 6 (hyphen also missing in line 17, 27, 34, etc.?) [Tim Hermans, Netherlands]	Noted. This has been corrected.
130547	2	11	2	11	The titile for sub-section "Air-sea fluxes and winds" is not appropriate for this Chapter. I suggest to replace it by "wind stresses". [Panmao Zhai, China]	Noted. This has been changed.
7347	2	29	2	32	Adjust the spelling of Sea-Ice or Sea Ice or sea-ice to make it consistent. Same goes for Coverage and coverage. [Svenja Halfter, Australia]	Noted. This has been corrected.

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64365	3	4	3	4	Terrestrial Cryosphere is a confusing term why are ice sheets not terrestrial? Why not make the grouping according to sea level components, all sea level components in one paragraph [roderik van de wal, Netherlands]	Taken into account. Terrestrial cryosphere has been removed from title of 9.5
11151	3	25	3	25	the font of the section number 2.6.2, is not same with others. [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
130549	3	27	3	27	Please consider to drop "in the literature and their evaluation" in the sub-section title. [Panmao Zhai, China]	Accepted. This has been changed.
7829	3	34	3	34	Cross chapter box [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Rejected-BOX is correct
1707	4	11	4	11	There are numerous acronyms used in Chapter 9. All of the acronyms used in the chapter should be listed at the end of the Table of Contents on page 11. [Michael Kennish, United States of America]	Taken into account. Acronyms are not listed in the chapter but are available via the IPCC glossary.
90677	4	22	4	25	I suggest to elaborate more about the decline of Arctic sea ice, "Based on passive microwave SMMR and SSM/I data collected since 1979, the decline in extent at the end of summer (in late September) has been at 11% per decade, reaching a record minimum in 2012 of 3.39 million km2, 44 % below the 1981-2010 average, and 16% below the previous 2007 record. Minimum extents from 2007, 2016, and 2019 are all statistically tied for the second lowest." [Thian Yew Gan, Canada]	Accepted. Such information is provided by chapter 2.
90679	4	27	4	28	I will rephrase the sentence, "Human activities are responsible for more than half of the observed loss of Arctic summer sea-ice area" to, say, "the observed loss of Arctic sea ice in late summer since passive microwave data became available in 1979 has been primarily attributed to global warming caused by risng concentration of greenhouse gases due to human activities." [Thian Yew Gan, Canada]	Noted. This sentence has been removed from the ES and is now only discussed by chapter 3.
90681	4	35	4	36	Given the uncertainties associated with long-term projections of climate scenarios, the sensitivity of the Arctic to global warming impact, the climate feedbacks, the high annual variability of Arctic sea ice (Olonscheck et al., 2019), etc., I suggest to rephrase the sentence to, "The Arctic Ocean could lose most of its sea-ice cover in September around 2050s"? Olonscheck, D., T. Mauritsen, and D. Notz. 2019. Arctic sea-ice variability is primarily driven by atmospheric temperature fluctuations. Nature Geoscience. doi:10.1038/s41561-019-0363-1. [Thian Yew Gan, Canada]	Noted. This sentence has been revised to now read: The Arctic Ocean will likely become practically sea-ice free (sea-ice area below 1 million km2) in the September mean for the first time before 2050 in all considered emission scenarios.
90683	4	43	4	43	I agree that sea ice extent in the Antarctic has not changed much since 1979. However, it may have marginally increased with a trend of 0.8±0.7 %/decade over 1979-2019? https://nsidc.org/data/seaice_index/archives Together between GRACE and the Input-Output method (IOM) estimates, the averagesea-level budget contribution from Antarctica over 2005-2015 is 0.42 mm yr−1 . [Thian Yew Gan, Canada]	Noted, however, this trend is not significant.
90487	4		100		It seems this chapter should have a consistent, specific, and limited number of ways to write paleo-observations. Currently, all of the following words are in use throughout the chapter: "paleo-observations", "paleo-proxies", "paleo-reconstructions", "paleo-evidence", "paleo-environmental proxy records", "paleo-environmental proxy data", "paleo-proxy data", "paleo-records", "paleo data". [Holly Kyeore Han, Canada]	Taken into account. These terms are not always interchangeable depending on context. Therefore, while minimizing the variety of terms to the extent possible, we used various terms where they had contextual meaning.
90685	5	1	5	3	I cannot quite make sense of the statement, "Greenland was likely in a state close to balance in the 1990s, with a mass balance of about 19 ± 23 Gt/yr between 1992 and 1997"? [Thian Yew Gan, Canada]	Noted. However, this is not what the text says. The revised text does not say this either.
130545	5	1	8	8	In ES, new knowledge on since AR5 and SROCC has not highlighted. [Panmao Zhai, China]	Noted. We now write upfront that we focus on new knowledge throughout the ES
78849	5	1	8	20	The Executive summary of this chapter nees acareful style improvement, due to grammar errors and someodds formulations. [MONICA TOLOTTI, Italy]	Accepted. Text improved
62299	5	1	8	20	Some sections of the Executive Summary describe projected changes of the Antarctic Ice Sheet while others describe just the West Antarctic Ice Sheet. I know this is not always possible but, wherever it is possible, it would make it clearer if both were discussed in tandem. For example, on page 8, lines 1-4 describe West Antarctica whereas lines 11-20 describe the entire Antarctic Ice Sheet. Here, I recommend adding a statement about the entire ice sheet to the paragraph between lines 1-4 and a statement about the West Antarctic Ice Sheet to the paragraph between lines 11-20, if possible. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The revised text now combines Greenland and Antarctica in the same paragraphs, and West Antarctica is not singled out.
97967	5	1	8	20	There are numerous cases of missing "s", "the" a couple of sneaky "meters" [Come on folks, the Queen's thanks]. I note that these issues persist through the chapter main text [Paul Durack, United States of America]	Accepted. Text improved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101663	5	1	8	22	There is an inconsistency in the language between the different sections in terms of attribution to "human" causes. Some sections clearly mention the "human" influence on the changes (e.g. sea ice) whereas other (e.g. ocean warming) do not mention it, despite an apparent influence. A more consistent language would be desirable, especially since this chapter clearly targets a "human attribution" (page 9, line 4). [IAPSO ECS group review, United States of America]	Accepted. We now address attribution more consistently
111727	5	1	8	22	Overall I found the structure and flow of the ES much improved on the FOD. It is now very reader-friendly. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you very much!
65831	5	2	6	3	Suggest indicating the projected year in which the Arctic is likely to become practically ice free. [Kushla Munro, Australia]	Noted. Because of internal variability, we cannot specify a particular date but remark that the ice-free Arctic will be observed before 2050
129287	5	3	5	3	[RISK] The word "possible" would seem to need some amplification as what the chapter really does is focus on the central and most defensible estimates of how sea level change and not at all the full range of possibilities. For example, paleoclimatic analyses would suggest that the equilibrium sea level sensitivity might be as much as 15-20 meters of sea level change per °C (based on coming out of last Glacial Maximum on the cold side, and perhaps similarly on the warm side based on the Eemian and then over tens of millions of years). In the business-banking-investment-national security world where due-dilgence and contingency analyses are supposed to consider the worst plausible scenario, describing what this chapter comes to conclude as "possible" does not seem, even with uncertainties, to come close to what the "worst plausible scenario" would be, leading to the assessments of decisionmakers/policymakers in these other communities to mistakenly plan for rates and amounts of sea level rise that are way under what would seem to be possible based on paleoclimatic changes. IPCC (and this chapter) really need to indicate that projections are generally quite central estimates and generally exclude the full range of what is possible. [Trigg Talley, United States of America]	Taken into account. Revised text gives attention to low likelihood high-impact scenarios, at sustained warming levels between 3-5dC.
83265	5	3	5	3	State here (upfront) what the cryosphere is i.e., what it's component parts are. [Robert Massom, Australia]	Noted. We think that such definition is not necessary.
112453	5	3	5	3	For the sake of clarity I would delete this part "...at the process-level…" [Pedro LLanillo del Rio, Germany]	Accepted. This has been removed.
14669	5	3	5	4	Is "at the process-level" necessary in this first sentence of Executive Summary? [Jeremy Fyke, Canada]	Accepted. This has been removed.
3045	5	3	5	4	The meaning of "at the process-level" is unclear, especially when considering that this is an "executive summary". Possibly, the woring can simply be removed. [Daniel Farinotti, Switzerland]	Accepted. This has been removed.
90687	5	3	5	5	The amount of mass loss for the GIS could be larger than 3900 ± 300 Gt? More recent published datasets provide an average ice mass loss from Greenland of 171 Gt yr−1 for 1993 to 2016, increasing to 272 Gt yr−1 for 2005 to 2016 because of accelerating mass loss up to 2012, in which a record mass loss of over 400 Gt was estimated (van Den Broeke et al., 2016). van den Broeke et al., 2016, On the recent contribution of the Greenland ice sheet to sea level change, The Cryosphere, 10, 1933–1946, https://doi.org/10.5194/tc-10-1933-2016 [Thian Yew Gan, Canada]	Noted. However, this is not what the text says. The revised text does not say this either.
97935	5	3	5	5	The opening sentence is a mouthful, breaking this apart might make for easier reading [Paul Durack, United States of America]	Accepted. Sentence has been rewritten
86807	5	3	5	8	Appreciate that the findings, if not otherwise stated, confirms or strengthen findings from SR1.5 and SROCC, but could the findings in the executive summary also state this. E.g. about ocean and heat uptake, there were findings in SROCC strenghtening the knowledge concerning sea temperature warming also in the deep sea below 2000 meters. Could the summary highlight which findings that are merely confirming previous findings and which are strengthening or different from previous findings? [Oyvind Christophersen, Norway]	Taken into account. This is very difficult to do in the Executive Summary given the structure and length constraints. The novel aspects are highlighted in the introduction.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99101	5	3	5	8	After reading the Executive Summary, I was very disappointed because of the lack of coverage of the risks of major loss of ice from the Greenland and Antarctic ice sheets, and the consequent sea level risk, with much higher than recent rates of change. While this is clearly a difficult area to develop high confidence in, there are substantial indications of growing risk. And given that it is indicated here that paleo-reconstructions are acceptable evidence, it needs to be pointed out in this summary that the paleo data suggest an equilibrium sea level sensitivity to changes in global average temperature of something like 15-20 METERS per degree C, from 20,000 years ago when sea level was down 120 meters and then up to about 4C warmer than present when there are really no indications of major ice sheets existing. And from 20 ka to 8 ka (so 120 centuries), sea level rose on average a meter per century as the global average temperature rose at an average rate of 1 C per 2000 years. Global average temperature is now up 1 C and could well rise 2-4 C this century, and coming out with an assessed value for the total rise by 2100 of under 1 meter seems to completely ignore the potential contributions from the ice sheets. I just do not see how this chapter can not point out this situation as a potentially catastrophic risk even though modeling of ice sheet flows, ice shelf thinning, surface melt water going down through the ice so carrying heat well into the ice sheets, etc. are hard to calculate. There is clear observational evidence of serious deterioration of the Greenland and West Antarctic ice sheets and suggesting that nothing is likely to come of this until after 2100 seems to be ignoring a huge risk and be contradicted by the paleo record making very clear that ice sheets have in the past deteriorated and collapsed much more rapidly than they can be built back up. In any case, even the commitment to future ice loss and sea level rise needs to be avoided. In my view, this whole summary needs to be revised to include full consideration of risks and not just based on what we are learning how to calculate. [Michael MacCracken, United States of America]	Taken into account. Revised text gives attention to low likelihood high-impact scenarios, at sustained warming levels between 3-5dC.
83271	5	3	5	8	There are elements of thiss whole chapter, and the initial Executive Summary, that overlap with similar material in Chapter 2, and should cross-rewference Chapter 2. This could occur upfront in this initial paragraph - also explicitly statong how Chapter 9 differs from Chapter 2. and also how the 2 chapters complement each other. [Robert Massom, Australia]	Accepted. We now more often add references to chapter 2 in the ES
78319	5	4			"instrumental observations" or "direct measurements" instead of "modern observations". [Michael Tsimplis, China]	Accepted. Revised as suggested.
49951	5	5	5	6	I find this characterization of "Unless otherwise noted" incredibly problematic in its wording, sending a very poor message. I read it as, "We didn't do the work, we just confirmed the previous findings if we didn't learn anything." Instead, this statement should be stronger, emphasizing that advances in the science since AR5 have served to strongly confirm what we knew from that time (except in a very few cases, noted below). Overall, this sentence should be rewritten to be more direct about how our scientific advances have almost totally confirmed or furthered what we knew for AR5. [Daniel Gilford, United States of America]	Accepted. We now write upfront that we focus on new knowledge throughout the ES
90689	5	5	5	6	The Antarctic Ice Sheet mass lost since 1992 could exceed about 2500 ± 500 Gt of ice? The IMBIE team (2018) combined satellite data of Antarctica's changing volume, flow and gravitational attraction, and modelling of its surface mass balance to show that Antarctica had lost 2,720 ± 1,390 billion tonnes of ice between 1992 and 2017, which corresponds to a mean sea level rise of 7.6 ± 3.9 mm. They estimated ice loss from the West Antarctica to increase from 53 ± 29 to 159 ± 26 Gt per year; ice-shelf collapse has increased the rate of ice loss from the Antarctic Peninsula, but estimated mass gain in East Antarctica over 1992–2017 to be about 5 ± 46 Gt per year. IMBIE (2018), Mass balance of the Antarctic Ice Sheet from 1992 to 2017, Nature, Vol 558, 219, https://doi.org/10.1038/s41586-018-0179-y [Thian Yew Gan, Canada]	Noted. However, this is not what the text says. It is unclear what text the reviewer refers to. The revised ES text gives the IMBIE numbers.
129289	5	5	5	8	[RISK] If one goes back to the earlier assessments, the potential for collapse of ice streams is generally not accounted for because it is hard to say that there is sound scientific understanding of how much change could happen and how quickly. There have been substantial advances since those earlier reports (both observations of ice sheet changes, of the role of meltwater going down moulins, of melting of the entire surface of the Greenland Ice Sheet, etc.) and the point that should be emphazied here is that there have been some significant advances in this area and all point to the potential for much greater sea level rise than earlier studies (that, for example, had the net effect of Antarctic changes as pulling sea level down during the 21st century, and with high confidence!). [Trigg Talley, United States of America]	Taken into account. We explicitly state that there is a potential for such processes to substantially increase the ice sheet sea level contribution, but have to acknowledge that there is deep uncertainty concerning this issue.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
97937	5	5	5	8	In Ch3, the opening para summarizes the key findings of Ch3, which links back to the SAR, TAR, AR4 and AR5. Not sure whether such an approach would work for Ch9? Oceans became a player in TAR anchored off the results of Levitus et al 1997 (WOA94, WOA98) and 2000 (https://doi.org/10.1126/science.287.5461.2225) [But you guys already knew that] [Paul Durack, United States of America]	Noted. We feel that links to the most recent reports (SROCC and SR1.5) are most important and so decided to stick to those.
78321	5	5	5	8	The way this is written undermines the message that it supports andit strengthens previous reports. Of course it is important to keep the statement that there are points of difference.Perhaps making the basic sttaement and then saying: "however earlier findings with respect to....... have not been confirmed". [Michael Tsimplis, China]	Noted. We now write upfront that we focus on new knowledge throughout the ES
103747	5	6	5	6	What is meant by 'strengthen related findings from the (...)' ? Consider rephrasing [Philippe Tulkens, Belgium]	Accepted. We now write upfront that we focus on new knowledge throughout the ES
80783	5	6	5	6	What is meant by 'strengthen related findings from the (...)' ? Consider rephrasing [Louise Sandberg Sørensen, Denmark]	Accepted. We now write upfront that we focus on new knowledge throughout the ES
65833	5	6	6	6	Suggest changing to "...poorly understood regional processes mean there is low confidence in regional projections of Antarctic sea-ice.." for clarity. [Kushla Munro, Australia]	Accepted. We now highlight the processes at the regional level.
34927	5	10	5	33	Detailed Comments by SOD Chapter – Chapter 9: The SOD points to a 4cm rise in GMSL between 1971 and 2015, which corresponds to just under 1mm/y. Land-based observations confirm that GMSL continues at 1-2mm/y, while satellite figures point to ~3mm/y. Neither dataset points to any acceleration of any significance. Even if GMSL is rising at the latter ~3mm/y figure, that would indicate a GMSL rise of about 25cm by 2100, which does not point to any climate emergency. See general comment #6 above. [Jim O'Brien, Ireland]	Noted. On contrary. As outlined in Chapter 2 - satellite altimetry data show a very likely acceleration of 0.084 ± 0.025 mm yr-2 over the satellite era (Nerem et al., 2017; Cazenave et al., 2018). This acceleration is primarily due to acceleration in GrIS and AIS mass loss and the impacts of increased GHG concentrations on all contributing processes (high confidence); (Velicogna et al., 2014; Harig and Simons, 2015; Slangen et al., 2016; Dieng et al., 2017; Chen et al., 2017; Cazenave et al., 2018; Fasullo and Nerem, 2018; Oppenheimer et al., 2019). The acceleration uncertainty (0.025 mm yr-2) includes effects of natural climate forcings, such as the acceleration after the temporary decline following the eruption of Mount Pinatubo (0.01 mm yr-2), and effects of internal variability, such as ENSO/PDO modes (0.01 mm yr-2) (Nerem et al., 2018). Compared to the full period 1993-2014, the acceleration tripled in the period 2005-2015, to 0.27 ± 0.17 mm yr-2. This increase is primarily tied to interannual-to-decadal variability in land water storage (0.11 ± 0.02 mm yr-2) and thermosteric sea-level rise (0.12 ± 0.06 mm yr-2) (Yi et al., 2017); it would thus be premature to project a continued increase in this elevated acceleration
49953	5	11	5	11	I think a word or few is missing before "continue to do so"... do you mean "will continue" or "continues"? [Daniel Gilford, United States of America]	Accepted, this has been rewritten.
261	5	12	5	12	Typo: "continue" should be continues [THOMAS Wagner, United States of America]	Accepted, this has been corrected.
80647	5	12	5	12	it should be 'continues' and not 'continue' [Helene Jacot Des Combes, Marshall Islands]	Accepted, this has been corrected.
61709	5	12	5	12	"ocean has warmed at all levels" - I recommend using "layers" or "depths" instead of "levels" to be more clear and consistent with terminology later in the chapter. In the rest of the chapter, "levels" is usually associated with sea level (elevation) not layers within the ocean (mixed layer, etc.) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The terminology 'level' has been removed
51485	5	12	5	12	The ocean has warmed at all levels..' - it would be helpful to clarify what you mean by 'levels' here, or whether this should in fact be 'depths'? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The terminology 'level' has been removed
99103	5	12	5	12	It seems to me the first point has to give an explanation of the multiple issues of concern, including that there are multiple ways in which global warming is contributing to sea level rise. So, fine, the first one to cover can be the thermal expansion component, but this needs to be preceded by an explanation that there are multiple factors contributing and this summary will be treating them separately and then adding them together at the end. [Michael MacCracken, United States of America]	Not Applicable. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101665	5	12	5	12	"the ocean has warmed at all levels" We suppose it means pressure levels, maybe precising it would be clearer. [IAPSO ECS group review, United States of America]	Accepted. The terminology 'level' has been removed
51491	5	12	5	12	In the SPM, warming extends through the entire warming column is assigned as high confidence, but in Ch 9 executive summary it is written as very high confidence - please could you ensure this is consistent across the report. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Confidence level has been adjusted for consistency
18755	5	12	5	12	"The ocean has warmed at all levels"?? I believe that the confidence is high for warming up to a depth of only 2000 meters. There is very little warming in the deep oceans so far. [Govindasamy Bala, India]	Accepted. The reference to 'all levels' has been removed
103749	5	12	5	12	Which levels does this sentence refer to? : ''The ocean has warmed at all levels' [Philippe Tulkens, Belgium]	Accepted. The terminology 'level' has been removed
15195	5	12	5	12	"will continue to do so" would better summarize the assessment [Simon Donner, Canada]	Accepted. Statement entirely revised
99451	5	12	5	12	Should this be a "virtually certain" statement given that term is used later in the paragraph. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. All Confidence level has been adjusted for consistency within and across chapters
143	5	12	5	12	"and continues to do so" - grammar [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Statement entirely revised
80785	5	12	5	12	Which levels does this sentence refer to? : ''The ocean has warmed at all levels' [Louise Sandberg Sørensen, Denmark]	Accepted. The terminology 'level' has been removed
62125	5	12	5	12	"At all levels" could be clarified to "at all water depths" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The terminology 'level' has been removed
30643	5	12	5	12	What is meant by 'all levels'. Is this depths? In that case, it should be specified. [Frank Pattyn, Belgium]	Not applicable. The terminology 'level' has been removed
132535	5	12	5	12	"continue" should be "continues" [Kyle Armour, United States of America]	Not applicable. Statement entirely revised
2519	5	12	5	12	at all levels->at all depths [Tim Hermans, Netherlands]	Not applicable. The terminology 'level' has been removed
733	5	12	5	12	“The ocean has warmed at all levels and continue to do so” this needs fixing grammatically e.g. “The ocean has warmed at all levels and will continue to do so” or “The ocean has warmed at all levels and continues to do so” [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Statement entirely revised
35047	5	12	5	12	The ocean has warmed at all levels and continue to do so. It needs to be either "….the ocean……... and continues…." OR "….the oceans …. and continue…." Also this statement seems to conflict with that in Ch2 p67 ll 13-14 that talks about heat loss from the deep ocean. [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Statement entirely revised.
39175	5	12	5	13	Does the high confidence include the finding that ocean warming contributes to future sea level rise even under low emission scenarios? [Lourdes Tibig, Philippines]	Noted. No change to text required. Yes is the answer.
129291	5	12	5	13	Is this strictly true (i.e., no single region with no warming)? What about NA cold blob? [Trigg Talley, United States of America]	Accepted. Revised and now clarified that some regions have cooled
109863	5	12	5	13	Re "losses have been consistently negative …" Losses are negative. Do you mean "changes have been negative"? [Donald Forbes, Canada]	Taken into account. Text corrected accordingly.
30641	5	12	5	13	The link between warming and sea-level rise is not clear, although it is explained below. Since this is a highlighted text it should make sense as a stand-alone sentence. I would propose to write: …leading to ocean expansion and hence sea-level rise ...' [Frank Pattyn, Belgium]	Not applicable. We now discuss thermal expansion in a different statement.
14671	5	12	5	23	Needs a read for unclear/imprecise statements. [Jeremy Fyke, Canada]	Accepted. Statement entirely revised
34667	5	12	5	23	This key message needs to be cleaned up a bit. For instance, the word "will" is missing in the first sentence. The next sentence is ambiguous as to the locations with the most warming (e.g., was it the upper 2000 m across the globe or just in the Southern Ocean and North Atlantic). The third sentence ends with the somewhat confusing phrase "changing water masses." The fifth sentence contains the paradoxical phrase, "spatial patterns of present warming will continue to warm." And the last sentence is overloaded with numbers; just report on projected changes for 2-3 SSPs instead. [Russell Vose, United States of America]	Accepted. Statement entirely revised
12439	5	12	5	33	Temperature assessment is after 1971, salinity is since 1950. There is an inconsistency here. It is well-known that temperature observations are much more than salinity, the spatial and temporal persistence of temperature change is also much better than salinity. Consequently, we should have better and longer temperature time series and higher confidence for temperature than salinity changes. However, it is the opposite. Please collabrate with chapter-1,2 to reconsider and resolve this issue. The literature cited by this chapter has already got sufficient informatin to revise this choice of start time for temperature and salinity. [Lijing Cheng, China]	Not applicable. We removed salinity from the ES. But we accept the comment and we have now revised and report salinity change over the same period as temperature, since 1971

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65835	5	12	6	13	Suggest changing to: "Greenland was likely close to mass balance in the 1990s and there is high confidence that its annual mass balance has been consistently negative since the early 2000s." for clarity. [Kushla Munro, Australia]	Taken into account. Text rewritten to improve clarity.
35049	5	13	5	13	The statement "Since the 1970s, most global warming…….." is ambiguous. It is either "the most pronounced global warming" or "the ocean has warmed most in the Southern Ocean…." [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Statement entirely revised.
51489	5	13	5	14	...and within the upper 2000 m' - it would be helpful to clarify if the observed warming reported here is in the upper 2000m of the ocean globally, of just the basins mentioned here (North Atlantic and Southern Ocean). Please could you clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Statement entirely revised.
101667	5	14	5	14	For clarity, it would be good to define the latitude range for the "Southern Ocean" here. [IAPSO ECS group review, United States of America]	Accepted. We do not include latitude range, but now clarify that is "in water-mass formation regions of the Southern Ocean"
61329	5	14	5	14	The probability quantifier 'likely' for the 1971-2015 thermosteric sea-level change is not raised in 9.2.4 (p39, line 24). This is inconsistent. I did neither find supporting information in 9.6.1.1. Please verify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. Text revised
78323	5	14			The meaning is unclear - is the heating on the top 2000m of the two basins or in the two basins and the 2000m globally? [Michael Tsimplis, China]	Noted. Statement entirely revised.
129293	5	15	5	15	For ocean heat content, the global estimates from EN4 -- Ishii et al. (2017), Cheng and Zhu (2016), Zanna et al. (2019), and Levitus et al. (2011) -- have all been extended to 2018. Therefore, suggest to show the 1971-2018 trend instead of 1971-2015 trend. That helps with comparing with other terms over the same period. [Trigg Talley, United States of America]	Accepted. We now assess long term trend from 1971-2018
103751	5	15	5	15	Over years -> Over periods of years [Philippe Tulkens, Belgium]	Not applicable. Statement entirely revised.
80787	5	15	5	15	Over years -> Over periods of years [Louise Sandberg Sørensen, Denmark]	Not applicable. Statement entirely revised.
195	5	15	5	15	"over years to a decade" - meaning is unclear. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Statement entirely revised.
3277	5	15	5	15	For ocean heat content, the global estimates from EN4, Ishii et al. (2017), Cheng&Zhu 2016, Zanna aet al. 2019, and Levitus et al. 2011 have all been extended to 2018. Therefore, I suggest to show the 1971-2018 trend instead of 1971-2015 trend. That helps with comparing with other terms over the same period. [Thomas Frederikse, United States of America]	Accepted. We now assess long term trend from 1971-2018
80649	5	15	5	17	This sentence is unclear and it looks like somethig is missing at the end of it, maybe a comma between 'surface' and 'changing' [Helene Jacot Des Combes, Marshall Islands]	Noted. Statement entirely revised.
101669	5	15	5	17	"Over years to a decade, regional heat patterns are dominated by internal and circulation variability that does not affect global heat content while at longer time scales the pattern is dominated by additional heat gained at the surface changing water-masses." - We found this sentence not very clear. To our knowledge "internal variability" does affect the global ocean heat content. Is the statement referring to the global climate system as a whole or to the ocean? Please be more clear and possibly specify that there are variations in global ocean heat content due to internal variability. [IAPSO ECS group review, United States of America]	Accepted. Sentence has been rewritten
100137	5	15	5	17	I don't understand what this sentence means, especially "which changes water-masses": Over years to a decade, regional heat patterns are dominated by internal and circulation variability that does not affect global heat content while at longer time scales the pattern is dominated by additional heat gained at the surface which changes water-masses. [Carlye Peterson, United States of America]	Noted. Statement entirely revised.
111425	5	15	5	17	Wording is unclear. What do you mean by "regional heat patterns"? When you say "changing water-masses", do you mean changing (increasing) the heat content of water masses? [James Renwick, New Zealand]	Noted. Statement entirely revised.
20159	5	15	5	17	Better insert "up" after "years"; better add a comma after "content". Also "surface changing water-masses" is unclear. [philippe waldteufel, France]	Not applicable. Statement entirely revised.
78325	5	15	5	17	Unclear meaning.: (pattern and patterns) and their signiifcance. I guess the point is that the heat changes are detected at time scales longer than a decade at the upper waters. [Michael Tsimplis, China]	Noted. Statement entirely revised.
61333	5	16	5	16	Consider adding the confidence qualifier '(high confidence)' based on statements on page 22 line 1-3: '[…] consistent with regional change of temperature over such relatively short time-scale being dominated by mode of variability and/or decadal change in ocean current (high confidence).' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Statement entirely revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71973	5	16			This is not quite correct - by chaning SST climate variability can result in changes in heat content. This needs minor rewording. [John Church, Australia]	Accepted. Sentence has been rewritten
61711	5	17	5	17	"heat gained at the surface changing water-masses". I don't understand what the "surface changing water-masses" is referring to or what the end of the sentence means. Perhaps: "…additional heat gained at the surface AND changing water masses" (maybe?) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Statement entirely revised.
99105	5	17	5	17	I don't understand the phrase "at the surface changing water-masses"--please clarify. [Michael MacCracken, United States of America]	Noted. Statement entirely revised.
101671	5	17	5	17	"....is dominated by additional heat gained at the surface changing water-masses." - The wording at the end of the sentence is unclear, and needs rephrasing. We suppose it means that because additional heat is gained at the surface, water-masses are changing? In which case rephrasing such that changing water-masses is understood as a consequence of heat gained at the surface would be clearer, like "additional heat gained at the surface, consequently changing the water-masses". [IAPSO ECS group review, United States of America]	Not applicable. Statement entirely revised.
62281	5	17	5	17	The phrase "at the surface changing water-masses" does not make sense to me. Is this referring to water masses that change the surface? If so, the hyphen should be moved: "by the surface-changing water masses." My understanding is that heat is gained at the surface so, potentially, this phrase should just be "at the surface." If this phrase means something else, it is not clear to me, and I do not have a suggestion other than to clarify it. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Statement entirely revised.
99683	5	17	5	17	awkward language [Peter Clark, United States of America]	Noted. Statement entirely revised.
34423	5	17	5	17	The end of this sentence is unclear. I'd suggest to write "… while at longer time scales the pattern is dominated by additional heat gained at the surface affecting water masses" [Claire Waelbroeck, France]	Not applicable. Statement entirely revised.
99453	5	17	5	17	Phrase "surface changing water-masses" is not clear. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Statement entirely revised.
106623	5	17	5	17	water-masses -> water masses [Kevin Bulthuis, United States of America]	Not applicable. Statement entirely revised.
99107	5	17	5	18	I don't understand what is meant here--is the point that past emissions (so up through 2020) would commit the ocean to warming through 2040 even if emissions went to zero today? Seems plausible, but needs to be more clearly stated [Michael MacCracken, United States of America]	Accepted. We agree the sentence was unclear. It has been removed and the scenario-dependence up to 2040 time-scale clarified
78327	5	17	5	18	Unclear meaning: Is it that the heating will continue until 2040 at least for all scenarios or that the level of confidence would rise to virtual certain? But then, having restricted warming to 2 basins abd the top 2 km what will become virtually certain, the certainty in the global average or the more restricted areas or depth.? [Michael Tsimplis, China]	Noted. Statement entirely revised.
61335	5	18	5	18	I did not find any support for the probability qualifier 'virtually certain' in any of the referenced sections. In Sect. 9.2.2.1, I could only find a confidence statement (p22, lines 43-46): 'The ocean will continue to warm over the 21st century (very high confidence) (Figures 9.6, 9.7), […]'. Please verify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. All Confidence level has been adjusted for consistency within and across chapters
3021	5	18	5	18	"... until 2040..." should be "... through 2040..." because 2040 is a lower bound and using the word 'until' makes it sound like the warming will stop exactly then [David Trossman, United States of America]	Accepted. Minor rewording done
30725	5	18	5	20	The use of uncertainty tem "high confidence" should be consistent with "Page 13 Lines 53-54 - very high confidence" [Iskhaq Iskandar, Indonesia]	Accepted. All Confidence level has been adjusted for consistency within and across chapters
80651	5	18	5	20	I suggest to remove 'to warm' after 'will continue' [Helene Jacot Des Combes, Marshall Islands]	Not applicable. Statement entirely revised.
78329	5	18	5	20	Sentence does not make sense at all. [Michael Tsimplis, China]	Noted. Statement entirely revised.
71975	5	18			Wrming will not stop in 2040. What you mean is the uptake between different scenarios diverges little prior to 2040. Minor rewording needed. [John Church, Australia]	Accepted. Minor rewording done
61229	5	19	5	19	"similar spatial patterns of present warming will continue to warm at a rate dependent on (…)". Patterns do not warm, so I would suggest "spatial patterns of present warming with persist with a warming rate dependent on (…)" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Statement entirely revised.
6741	5	19	5	19	"similar spatial patterns of present warming will continue to warm at a rate" could be changed to "warming will continue with a spatial pattern similar to that of present warming and at a rate". It is not the patterns that warm. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Statement entirely revised.
99455	5	19	5	19	Phrase "… patterns .. continue to warm .." is unclear, please rephrase. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Statement entirely revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35051	5	19	5	19	The statement " .. Similar spatial patterns of present warming will continue to warm at a rate dependent on present…" is clumsy I suggest "…. the present patterns of wrming will continue at a rate…." [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Statement entirely revised.
34425	5	19	5	20	This sentence is unclear too. Here is a suggestion: "After 2040, warming will continue with spatial patterns similar to the present ones, at a rate dependent on present and future emissions (high confidence)." [Claire Waelbroeck, France]	Not applicable. Statement entirely revised.
65837	5	19	6	19	Suggest changing to: "It is very likely that the Antarctic Ice Sheet has experienced net mass loss since at least the early 1990s and it is likely that …" for clarity. [Kushla Munro, Australia]	Noted. Statements have been rewritten.
3141	5	20	5	20	"Between 1996-2014 and 2100"should be changed to "1995-2014" [Hui Wang, China]	Not applicable. Text removed
103753	5	20	5	20	It is unclear what time period this refers to: 'Between 1996-2014 and 2100'. [Philippe Tulkens, Belgium]	Not applicable. Text removed
80789	5	20	5	20	It is unclear what time period this refers to: 'Between 1996-2014 and 2100'. [Louise Sandberg Sørensen, Denmark]	Not applicable. Text removed
15533	5	20	5	20	Re: 1996-2014. Likely a typo error. It should be 1995-2014 (CMIP6 reference period). Please check and revise. [SAI MING LEE, China]	Not applicable. Text removed
2517	5	20	5	20	historical period is mean of 1996-2014, but future only 2100. That should also be a mean of 19 years then [Tim Hermans, Netherlands]	Not applicable. Text removed
85205	5	20	5	20	What meaning period for 2100 in 'between 1996-2014 and 2100' [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
41441	5	20	5	20	This is probably a typo, please correct to 1995-2014. [Alexander Nauels, Germany]	Not applicable. Text removed
99155	5	20	5	22	It needs to be noted that thermosteric sea level rise is only one of the factors contributing to sea level rise, and over multi-centennial to multi-millennial timescales, it has been only a minor term in comparison to variations in the amount of ice on land. Otherwise the reader will get a misimpression that the overall sea level risk is pretty small. [Michael MacCracken, United States of America]	Not applicable. Text removed
23481	5	20	5	23	it would be better if confidence limits expressed in ± pattern [Saurabh Rathore, Australia]	Noted. We have now revised the confidence levels.
78331	5	20			Consider adding a sentence on what the heating experienced from 1900 to today has added to sea level (with error bars) to provide a point of reference for the numbers that follow. [Michael Tsimplis, China]	Rejected. This information is covered by chapter 2.
61339	5	21	5	22	Although projected numbers for the thermosteric sea-level contribution are correct, the probability qualifier 'likely' is not given in Sect. 9.2.4 (p39, lines 25-28). Please be consistent. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. All Confidence level has been adjusted for consistency within and across chapters
112455	5	21	5	23	Please indicate here what the acronym SSP stands for(shared socioeconomic pathways). Perhaps these paragraph would be clearer in a table. [Pedro LLanillo del Rio, Germany]	Rejected. SSP acronym is an editorial matter that is dealt with at a report level; table is not appropriate for Executive summary.
99685	5	25	5	25	"surface-intensified" - awkward language [Peter Clark, United States of America]	Rejected. English formulation
145	5	25	5	25	"surface-intensified temperature and salinty changes" is unclear. Do you mean "Intensified temperature and salinity changes at the surface have increased the stability of the upper ocean stratification" [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, we changed to surface-intensified warming
18757	5	25	5	26	Does this statement refer to the global mean surface ocean? I believe that the salinity has increased in the subtropical ocean in association with a decrease in precipitation there which would mean a decrease in stratification in the subtropical region due to salinity changes. Sentence may be revised for clarification. [Govindasamy Bala, India]	Rejected. No the reviewer assumption is not correct (temperature-related change in stratification overcompensate for loss of salinity-driven stratification). We discuss this aspect in the section 9.2.1.3 upper ocean stratification and mixed-layer depth
112457	5	25	5	26	Either "Have increased the upper-ocean stratification" or "have increased the stability of the upper-ocean", but using both is redundant. [Pedro LLanillo del Rio, Germany]	Accepted. Text revised
61343	5	25	5	26	It is odd the the bold face text comprises the first part of the second sentence. This is inconsistent with the previous text block. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Statement entirely revised (including font).
97939	5	25	5	29	Reported salinity changes are not just "near-surface" these extend to full-depth, and such consistent changes have been reported across numerous studies [Paul Durack, United States of America]	Not applicable. Text removed
39793	5	25	5	31	"more than ten times higher than reported SROCC". The quantified assessment result should be cautious. [TSU WGI, France]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12441	5	25	5	32	It is better to separate salinity from stratification, given the fact that the later is mainly due to temperature change. [Lijing Cheng, China]	Not applicable. Salinity Text removed
78333	5	25	5	33	What is this paragraph reporting? The first sentence is about stability, the opening words suggest intensification at the surface as if the forcing is uniform and stronger near the surface rather than driven through the surface. The second sentence talks about patterns in salinity but do these really determine the stability? Then there is the sea surface T reporting which is more robust. Why are these relevant anyway? [Michael Tsimplis, China]	Accepted. Paragraph reconsider and now only focus on stratification
99109	5	26	5	26	I think a sentence needs to be added indicating why consideration of stratification is important, so what are the implications of this--changes in ocean overturning, reduced upwelling of ocean nutrients, etc. It is not said where this is happening, and that too would be useful to indicate--e.g., it is occurring in the North Atlantic and this is slowing the vital over-turning of the ocean and the mixing down of excess CO2 into the deep ocean, etc. [Michael MacCracken, United States of America]	Rejected. Implications are discussed in the text and cannot be discussed here for text length constraints
3047	5	26	5	26	The bold font should be removed from "It is..." onwards; so far, bold font highlighted complete sentences, which is not the case here. [Daniel Farinotti, Switzerland]	Noted. Statement entirely revised (including font).
61945	5	26	5	26	"it is virtually certain that near-surface" not in bolt anymore [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Statement entirely revised (including font).
129295	5	26	5	27	[CONFIDENCE] Can authors review their assignment of confidence for the executive summary statement related to ocean salinity? Chapter 9.2.1.2 states "... stronger evidence for increased contrast between high- and low-sea surface salinity regions since the 1950s. In response, we revise the AR5 reported confidence in such change from very likely to extremely likely (Section 2.3.3.2)." Are the authors considering the word "certain" as a certainty qualifier for this statement? If not, revise wording to "... it is certain that global sea-surface temperature has increased since the beginning of the 20th century at a very likely rate of 0.053±0.002°C per decade which accelerated to 0.087±0.007°C per decade since 1979." [Trigg Talley, United States of America]	Accepted. All Confidence level has been adjusted for consistency within and across chapters
67121	5	26	5	27	more pronounced than what? [Regine Hock, United States of America]	Not applicable. Text removed
103755	5	27	5	27	certain -> certain [Philippe Tulkens, Belgium]	Not applicable. Statement entirely revised
80791	5	27	5	27	certain -> certain [Louise Sandberg Sørensen, Denmark]	Not applicable. Statement entirely revised
62127	5	27	5	27	"Certain" is not included in IPCC language. Please clarify [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This is an English word though.
112459	5	27	5	28	I would indicate that you meant the average global surface temperature, as as you know, there are other regions were a cooling has been observed due to changing wind patterns like the upwelling band in the Southern Ocean (Purich et al., 2016, doi: 10.1038/ncomms10409) or the Eastern South Pacific upwelling region (England et al., 2014, doi: 10.1038/NCLIMATE2106). I see you included reference to this regions in page 13. [Pedro LLanillo del Rio, Germany]	Accepted. The ES now better reflect varying regional pattern of warming (including cooling)
129297	5	28	5	29	Revise to state 1979-2018. Section 9.2.1.1 states "The estimated global mean SST warming is 0.087±0.007°C per decade (90% confidence interval) from 1979 to 2018 (updating AR5 estimate of 0.072±0.024°C per decade from 1979 to 2012), faster than the mean warming rate over the past century." [Trigg Talley, United States of America]	Accepted. We now state long term change and 2011-2020 change
6743	5	29	5	29	The figure of 0.087ºC/decade for SST increase since 1979 needs reconciling with what is shown in Table 2.4. The figure quoted here is equivalent to an increase in SST of 0.34ºC over 39 years. The figures given in Table 2.4 of Chapter 2 shows a 1980-2018 temperature change that is in the range 0.5-1.0ºC for the datasets shown. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We solved cross-chapter inconsistencies
67123	5	29	5	30	SROCC's 2.3% refers to the upper 200 m (what is it here?). It also refers to the increase between the 1971-1990 average and the 1998-2017 average. This is different than how this is expressed here (as a rate per decade instead of a difference between two 20yr periods and therefore numbers not directly comparable. Can this be expressed the same way and made directly comparable in which case probably the 'ten times higher' changes? [Regine Hock, United States of America]	Noted. Please note that the comparison is done in the underlying text in Section 9.2.1.3.
129299	5	29	5	31	Is this a single paper? Is this an accepted result, or has it been challenged? It seems extraordinary that the rate stated here is 10 times higher than in SROCC, but it's not backed up with extraordinary evidence. [Trigg Talley, United States of America]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129301	5	29	5	31	It is not really commonly known what the measure of oceanic stability is, so very hard to interpret what a 5-20% per decade change means. For non-experts, it would really help to clarify what this means in terms of the strength of the ocean overturning current or something more widely known. And it needs to be said how important it is for this value to be 10X the previous estimate. That is a huge change, but does it matter? [Trigg Talley, United States of America]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more line of evidence and note potential larger stratification increase when focusing at the base of the mixed layer. In addition, we clarify the impact of change in stability
79897	5	29	5	31	According to section 9.2.1.4, the 'new analysis' referes to Sallée et al. submitted. Although I'm sure it will be a great and robust work- as all previous J.B Salléé works- for future IPCC reports (not necessarily this one) I might recommend to avoid highlighting conclusions based on one single work, specially if it has not been published yet. To this respect, I think it must be mentioned also for future IPCC reports that the citation of submitted works is frequent in the report (54 works, approximately 3.7% of total references). Most scientific journals tend to dissuade against this practice. I can imagine that a lot of work is done in relation with the report but perhaps a more restrictive policy should be considered to include these works as citations. I also assume these papers will be in press when the report will be published, but in any case the review process is affected by this fact. [Somavilla Raquel, Spain]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer
51487	5	29	5	31	A table/summary of the latest updates to values since the SROCC would be advantageous here [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Change since SROCC are now made more explicit both in the executive summary and underlying text.
20539	5	29	5	31	Which QUANTITY has increased? While there is no problem for understanding stratification as a phenomenon, to assign a rate of increase you need to define it as a quantity, expressed in specified units [philippe waldteufel, France]	Noted. Stratification is a physical quantity expressed in s-2
18025	5	29	5	31	What are the implications of this large increase in stratificaion for oxygenation of the ocean interior? Why is there no mention of oxygen in the executive summary? [Lisa Levin, United States of America]	Noted. Oxygenation is assessed in Chapter 5, as now mentioned in section 9.2.1
97941	5	29	5	31	The 5-20% per decade equates to a ~25 to ~100% change to stratification over the 49 years of analysis (1970-2018). How can you physically justify such large numbers? Intuitively, a 100% quantification suggests a complete and permanent decoupling of the upper ocean with the intermediate and deep, which realistically doesn't seem possible. I don't question that there is a profound change to global ocean stratification, however, these numbers certainly capture attention [the paper was submitted to Nature, they had to], but will also draw very strong criticism from the sceptic community. I suggest a revisit of this statement with a more quantitative and qualified description [Paul Durack, United States of America]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer
23483	5	29	5	31	5-20% per decade of -----? is it possible to mention the specific time period and its rate e.g. 5-20% per decade of zzz degree C for the period of xxxx-yyyy. [Saurabh Rathore, Australia]	Accepted. We now revised and state the specific period: 1970-2018
34669	5	29	31	31	Is this based on a single analysis (Sallée et al., submitted)? [Russell Vose, United States of America]	Noted. Yes it was. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so we base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer
71977	5	29		30	WOW, that is large. What does "surface" stratification mean? Minor rewording needed. [John Church, Australia]	Accepted. We have clarified the metric.
61231	5	30	5	30	"at the rate of (…), more than". Not clear, perhaps change by "at a rate of (…), which is more than" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Text revised for clarity
12443	5	30	5	30	This is an extraodinary assessment.This number here, 5~20% per decade, shows a huge difference compared with SROCC and AR5 assessment on stratification change (<=1% per decade). That means ocean stratification has increased up to 100% in the past 50 years (this is a shocking news and media will likely pick this up). I have strong reservation to this assessment, I will detail my comments below. Here, just point out that this number is not comparable with previous numbers in SROCC and AR5, because the current 5-20% represents only one layer of ocean (mixed layer bottom), but both SROCC and AR5 do surface and 200m difference. So it is better to clarify the definition. [Lijing Cheng, China]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer
89801	5	30	5	30	What is meant by 'stratification' here - is it defined here as just the density difference between the surface and 200m? [Peter Croot, Ireland]	Accepted. The metric is now clarified.
61947	5	30	5	30	add confidence after 5--20% per decade (medium confidence?) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Confidence level added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101673	5	30	5	31	"...stratification has increased from 1970 to 2018 at the rate of 5-20% per decade, more than ten times higher 31 than reported by SROCC (medium confidence)." - SROCC reports an increase by 2.3% between roughly 1980 and 2012 in the upper 200m, i.e. a bit more than three decades. Therefore, the lower bound of the estimate provided here (5% per decade) is not more than ten times larger. We would rather provide a range here. However, overall, the comparison is not quite adequate given that SROCC reports an average of the upper 200m and that the 5-20% comes from the pycnocline. So, if a comparison is made, numbers must refer to the same average (vertical layers). [IAPSO ECS group review, United States of America]	Noted. We have entirely revised that statement. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer
68533	5	30	5	31	Looking deeper into the chapter, the 5-20% per decade increase in stratification is for changes in "the summer pycnocline". SROCC did not assess percentage changes in the local stratification of the summer pycnocline (the 15 m below a diagnosed mixed layer depth), but rather the percentage change in the year-round average of the 0-200 m stratification. Note that the percentage and spatial averaging operators do not commute. One metric is tends to give large values (by over-weighting changes when the stratification is weak), while the other is a more cautious approach that will de-emphasize poor data. This is an apples-and-oranges comparison of completely different quantities, and hence the comparison with the SROCC finding is invalid. The phrase "more than ten times higher than reported by SROCC" should be removed as it is simply inaccurate. [Robert Hallberg, United States of America]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer. We however note that, in contrast to what the reviewer suggests, commuting percentage and spatial averaging operators would not explain the much larger change of summer stratification at the base of the mixed-layer, compare to change of the 0-200 m stratification. There is something more profound going on
69219	5	31	5	31	The confidence level in Executive Summary seems to be inconsistent with the underlying chapter (9.2.1.4 page 17, line 19-21). Please revise. [Kaoru Magosaki, Japan]	Accepted. All Confidence level has been adjusted for consistency within and across chapters
61345	5	31	5	31	The medium confidence expressed here is not consistent with the statements made in Sect. 9.2.1.4. For the SROCC stratification increase estimates you giva a 'very likely' probability (page 17, line 11). You confirm later that the upper ocean stratification has 'very likely' increase bsince 1970 (page 17, line 19). Yet you give 'high confidence' on the increase in summer pycnocline of 5-20%. Note that you specify here the 'summer' pycnocline in contrast to the executive summary. Please check consistency on confidence and terminology. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The SOD assessment was proposing a new metric from a single paper. We now use a consistent metric with SROCC and AR5 (0-200 stratification), so base our assessment on more lines of evidence and note potential larger stratification increase when focusing at the base of the mixed layer
22555	5	31	5	32	I'm not sure I entirely understand what this final sentence is trying to say. Are you trying to say that the patterns are likely to continue? Also, how does this chime against the substantive assessment in chapter 7 which suggested key aspects of the pattern might not persist and might be responsible for some discrepancies between lines of evidence on ECS and chapter 4 also noting similarly that particularly in the equatorial Pacific the current patterns are unlikely to be maintained? [Peter Thorne, Ireland]	Not applicable. Text on pattern of surface change is now removed from the ES
34671	5	32	5	33	Something about this sentence is unclear, but I cannot quite put my finger on it. Maybe I don't understand how "changes…can persist" (isn't that a contradiction), and I'm not sure I understand how they will vary based on emissions. [Russell Vose, United States of America]	Accepted. Statement entirely revised.
90695	5	35	5	37	The mean contribution of glaciers to SLR is likely 0.74 ± 0.18mmyr-1 for 2005-2016, which works out to be about 270 ± 65 Gt yr-1 given the global surface area of the oceans is 361 x10^6 km2. Therefore, this means that in 2006-2015 the amount of glacier melt of -278± 113 Gt yr-1 is representative even though the range of ± 113 Gt yr-1could be a bit high. However, based on my calculation, this means the global glacier melt was likely 270/361± 65/361 (-0.74± 0.18) kg m-2 yr-1 instead of -390±160 kg m-2 yr-1 which is too high? [Thian Yew Gan, Canada]	Noted, the contribution of glaciers to SLR is revised with a new publication (Hugonnet et al, 2021) and consistency between numbers and units carefully ensured.
112461	5	35	5	47	A wind-driven spin-up of subtropical gyres have been described using Argo Data (Roemmich et al., 2007 doi: 10.1175/JPO3004.1) and also stronger upwelling has been detected for the Eastern Pacific (Engalnd et al., 2014 doi: 10.1038/NCLIMATE2106). The strengthening of the southern Hemisphere westerlies also have changed the circulation and thus the water masses present in the Bransfield Strait (Dotto et al. 2016 doi: 10.1002/2015JC011228) [Pedro LLanillo del Rio, Germany]	Noted. We summarise here major systems, focusing on new findings and cannot discuss all circulation changes
89321	5	35			This section is misleading. For most readers of this point it will not be evident that the term "robust observed changes..." refers only to the short RAPID interval and thus contains an implicit and debatable value judgement, and that this section ignores all the evidence for a long-term AMOC weakening but only considers the short-term 2007-2011 weakening, a period too short to attribute to anthropogenic forcing. Most readers would come away with the impression that there is no evidence for an AMOC weakening caused by anthropogenic warming. I strongly disagree with this point and so did the SROCC. [Stefan Rahmstorf, Germany]	Noted. The conflicting evidence for a long-term trend over the historical period is discussed in 9.2.3. We agree that the "RAPID statement" is confusing. This is rephrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129307	5	37	5	37	What is meant here is that a change in the North Atlantic overturning circulation cannot be established with two-sigma significance. While that may well be the case due to limited data, this phrasing will be used to say that scientists can't find any change in this parameter -- and that is wrong. As is suggested, there are solid reasons to indicate that there will be such change. The phrasing needs to be changed to get rid of such jargon, as past experience makes clear this ends up causing all sorts of confusion in presenting the results. So, perhaps say something like "While there are indications that the North Atlantic overturning circulation is slowing, as would be expected due to increased stratification, this change is still emerging from what appears to be natural variability in the limited historical record of this parameter." [Trigg Talley, United States of America]	Accepted. The statement was confusing and is rephrased.
55071	5	37	5	37	Delete "Robust" as the first word of this sentence. The underlying chapter sections do not support describing the observed changes in AMOC strength as being statistically Robust. [Nancy Hamzawi, Canada]	Noted. The statement refers to the RAPID measurements and was confusing. It is rephrased.
99111	5	37	5	37	Since the Second Assessment and the controversy over the detection and attribution that had Ben Santer as lead author, it has been learned by experience not to use terms that represent jargon from the field of statistics. So, get rid of "Robust" (which in statistics means one does not have two-sigma evidence--meaning you don't yet have 20 to 1 likelihood) and say what the situation is in clear words or the finding will go into the public discussion suggesting that there is no evidence of this. It is a lesson that seemingly has to be learned over and over--don't use jargon that means something special to those in the field and something possibly very different in public discussion. So, perhaps say: Observed changes in the Atlantic Meridional Overturning Circulation can, at this time, only tentatively be attributed to anthropogenic factors, but the AMOC ..." [Michael MacCracken, United States of America]	Noted. The statement refers to the RAPID measurements and was confusing. It is rephrased.
67125	5	37	5	37	unfortunate formulatoin. Best not to start with what is not happening [Regine Hock, United States of America]	Accepted. The statement was confusing and is rephrased.
35053	5	37	5	37	The significance of the word Robust is unclear. Suggest drop it. [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement was confusing and is rephrased.
129303	5	37	5	38	[CONFIDENCE] Add low confidence intervals to this statement in the executive summary. See page 30: "Based on these results we have low confidence in the reconstructed AMOC weakening over the last 150 years, low confidence that the presently observed AMOC reduction is anthropogenically forced and low confidence that the AMOC has been declining since the mid-twentieth century." [Trigg Talley, United States of America]	Noted. The statement refers to the RAPID measurements and was confusing. It is rephrased.
129305	5	37	5	38	The meaning of 'robust' is ambiguous. What's meant here is 'statistically significant'? [Trigg Talley, United States of America]	Noted. The statement was confusing and is rephrased.
96845	5	37	5	38	The heading of the paragraph is unclear and does not match the information given in section 9.2.3.1. What is meant by 'robust observed changes'? The summary below indicates that weakening is in the range of natural variability and time series are short anyway. Please modify. [Nicole Wilke, Germany]	Accepted. The statement was confusing and is rephrased.
76697	5	37	5	38	“Are not linked” → “can not be linked/attributed”? The former formulation suggests that we can exclude the possiblity that anthropogenic effects are present in contemporary changes. While I understood we can not see them under the natural variability [Roelof Rietbroek, Germany]	Noted. The statement refers to the RAPID measurements and was confusing. It is rephrased.
62129	5	37	5	38	"Robust" addresses the fact that there is strong evidence that there have been changes in the AMOC, but does address the level of agreement on these findings. Please expand confidence language to include the level of agreement on this topic [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The statement was confusing and is rephrased.
115143	5	37	5	38	Is the meaning of this sentence that 1.These changes have been robustly observed, and that we do not know whether or not they are linked to antruopogenic causes? Or that we know that they are not linked to anthropogenic causes? [Natalya Gomez, Canada]	Accepted. The statement was confusing and is rephrased.
99457	5	37	5	39	Given how poorly many aspects of the AMOC are simulated (see 9.2.3.1) the "likely" assessment appears overconfident. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. In 9.2.3 it is further explained why we can have confidence in projected overall AMOC decline despite many aspects of the AMOC being poorly represented.
129309	5	37	5	43	So that readers know why this paragraph is here, there needs to be mention of why potential changes are an important consideration (so for changes in weather in Europe, around the NH due to effects on the wave pattern, and so on). This summary is supposed to help those with technical knowledge in science understand what is happening and why this is important. [Trigg Talley, United States of America]	Noted. The statement was confusing and is rephrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104509	5	37	5	44	This part is highly surprising with quite strong statements despite having very little and basically no robust data. This sentence should be re-formulated: "Robust observed changes in the Atlantic Meridional Overturning Circulation (AMOC) are not presently linked to anthropogenic causes". There is no consensus whether there is a robust change in recent decades. So we cannot detect nor can we falsify anthropogenic causes. Why is there a focus on an AMOC weakening from 2007-2011 based on which dataset and AMOC definition? What is the historical period here, which data? There is an absolute long-term cooling trend in the subpolar gyre region SE of Greenland (warming hole) with the coldest SST in 2015 in more than 120 years. As discussed elsewhere in this chapter, Rahmstorf et al. 2015, Caesar et al. 2018 and based on proxies Thornalley et al. 2018 provide remarkable evidence for an unprecedented slowdown of the AMOC in recent decades and perhaps even relative to 1600 years. To say "high confidence" that AMOC would be within natural variability given these prominent studies and the lack of reliable long-term data is not acceptable. I would strongly disgree with "high confidence". It should also be clearly said that simulations do NOT include meltwater from Greenland - so they may strongly underestimate a potential AMOC decline. That these models even without Greenland meltwater show an AMOC decline is even more remarkable. References: already cited in this chapter. [Frederik Schenk, Sweden]	Accepted. The statement was confusing and is rephrased.
132537	5	37	5	47	Be sure this is consistent with Chapter 4's assessment of future AMOC changes. [Kyle Armour, United States of America]	Noted.
78335	5	37	5	47	1.The previous paragraphs reported on changes until 2100 without stating anything about after 2100. This refers to "deep uncertainty" making the reader wonder whether this is somethin that does not exist for the previous sections. [Michael Tsimplis, China]	Noted. The statement was confusing and is rephrased.
71979	5	37		41	What about the changed AMOC inferred by Caesar et al. (Nature 2018) and assessments elsewhere in this report. [John Church, Australia]	Noted. The conflicting evidence for a long-term trend over the historical period is discussed in 9.2.3. This statement, however, is confusing and is rephrased.
14673	5	38	5	39	should "are not presently linked to" -> "are not presently attributable to"? [Jeremy Fyke, Canada]	Noted. The statement was confusing and is rephrased.
129311	5	39	5	39	[CONFIDENCE] Is this major finding based on only 5 years of data? That is a pretty slim record to be saying anything at all about the change in circulation. How is that related observations over a longer period are not being used? If there is a potentially counter-veiling tendency due to the changes in salinity due to ice sheet melting, then mention that. [Trigg Talley, United States of America]	Noted. The statement was confusing and is rephrased.
32905	5	39	5	39	should say "The weakening of the AMOC between 2004 and 2017" see Smeed et al. (2014, 2018) [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is a typo.
55073	5	39	5	41	A sentence that would better reflect the contents of the referenced subsections would be something like "Variations in the strength of AMOC observed by the RAPID array (2004-2016) are within the range of natural variability inferred from reanalysis for the historical period (high confidence)". [Nancy Hamzawi, Canada]	Noted. The statement was confusing and is rephrased.
96847	5	39	5	42	Here it is stated that the projected AMOC change are only weakly dependent on the Scenario. This differs significantly from the results of the SROCC. Chapter 6 of the SROCC stated: "Now based on up to 27 model simulations, the decrease of the AMOC is assessed to be of –2.1± 2.6 x 106 m3 s–1 (–11 ± 14%, likely range) in 2081–2100 relative to present-day (2006–2015) for RCP2.6 scenario and –5.5 ± 2.7 x 106 m3 s–1 (–32 ± 14%) for RCP8.5 scenario. The numbers here differ for both scenarios. Regarding the AR6 Figure 4.4, only one CMIP6 simulation is available for SPP1-1.9 so a statement of medium confidence seems to be overrated. Please modify. [Nicole Wilke, Germany]	Rejected. We agree that the statement has to be reworded. However we discuss here CMIP6 simulations that show different behaviour than in CMIP5. Hence some discrepancy with AR5 and SROCC. The figure in 4.4 is still incomplete and updated in the FGD.
81701	5	39			2004-2011 seems a rather short timescales to detect forced trends [Laure Zanna, United States of America]	Accepted. The statement was confusing and is rephrased.
81703	5	39			Chemke et al argued that post 2000' there is a decline in heat transport that is forced (maybe AMOC, maybe the gyres). This paper might need to be assessed in the context of AMOC/heat tranport decline. [Laure Zanna, United States of America]	Accepted. Chemke et al is discussed in 9.2.3 but this point is not highlighted in the ES.
99113	5	40	5	40	Regarding the "range of variability"--again, I presume this is a reference to two-sigma limits and due to limtiations in the observations I'd imagine this is a large range, limiting signal to noise. So, because of the limitations in observations, this statement is true, but so also is the possibility that this weakending could be associated with human influences were the original hypothesis flipped around. In my view, this sentence is thus quite one-sided as one can't rule out either possibility--and that is what needs to be said, not this one-sided statement. [Michael MacCracken, United States of America]	Noted. The statement refers to the RAPID measurements and was confusing. It is rephrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7831	5	40	5	40	Results from reanalysis (my paper!) show AMOC variability, however cannot show that the variability is natural since they use observed surface fluxes which have imprints of forced changes included in them. Hence should remove reanalysis from this list. Also Chapter 2 (P6, L26) gives a 'high confidence' to the observed weakening around 2008 - maybe include here? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement was confusing and is rephrased.
38087	5	40	5	40	Change "reanalysis" into "reanalyses". [Junhee Lee, Republic of Korea]	Not applicable. Statement is revised and this particular point no longer included.
2521	5	40	5	40	reanalysis->reanalyses [Tim Hermans, Netherlands]	Not applicable. Statement is revised and this particular point no longer included.
99115	5	42	5	44	While this might seem reasonable, the wording hides the risk if there is serious loss of mass from the ice sheets other than by freshening by surface melting of the ice sheets. So, this statement is really not allowing for ice stream movement and accelerated calving and then melting of the ice bergs. As such, I think the statement is just not sufficiently defensible to be so sure that there will not be a collapse before 2100--there is a growing risk throughout the century and could be a triggering if ice stream collapse occurs. [Michael MacCracken, United States of America]	Not applicable. Statement is revised and this particular point no longer included.
111725	5	42	9	44	This statement is on the face of it weaker than in SROCCC (unlikely rather than very unlikley). I think this deserves comment, especially noting the statement at the top of the ES that all conclusions confirm or strengthen conclusions from previous reports. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This points is further motivated in 9.2.3.
129313	5	43	5	43	[RISK] What does "with deep uncertainty" mean and how can there be such assurance that this will all be beyond 2100. [Trigg Talley, United States of America]	Accepted. The statement was confusing and is rephrased.
61233	5	43	5	43	"deep uncertainty". Perhaps replace by significant uncertainty. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The statement was confusing and is rephrased.
62131	5	43	5	43	Deep uncertainty is not IPCC language [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The statement was confusing and is rephrased.
3023	5	43	5	44	Why is there only mention of meltwater fluxes from Greenland and Antarctica? The CMIP5 models show that future AMOC weakening is associated with ocean warming (Levang and Schmitt, 2020, in J. Clim.) and other model experiments show that warming dominates over the freshwater flux effect (Haskins et al., 2020, in Clim. Dyn.). There is also a growing consensus that Arctic sea ice melting will further weaken AMOC (Sévellec et al., 2017, in Nature Clim. Change; and Liu et al., 2019, in J. Clim.). [David Trossman, United States of America]	Accepted. The statement was confusing and is rephrased.
97943	5	44	5	47	"..are predicted to respond.." I was left wanting to know more, at list a little tidbit. You have indicated 9.2.31 through 5 hold this info, but a little more descriptive text would appease me [Paul Durack, United States of America]	Noted. We agree, but are bound by severe constraints in word limit.
61471	5	45	5	45	observation limitations and course model resolutions. It seems that course model reoslutions are a limitation for marginal seas, coastal regions and WBCs and should be included as a factor here [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised.
65827	5	45	5	47	Suggest indicating in what ways circulation (Southern Ocean Overturning, Indonesian Throughflow etc.) are likely to respond to atmospheric circulation, e.g. state that the response is likely to vary according to circulation system, region etc. [Kushla Munro, Australia]	Rejected. Note that we are bound by severe text limits.
100139	5	46	5	46	…upwelling systems<comma> and Indonesian [Carlye Peterson, United States of America]	Noted. Text has been revised
26355	5	47	5	47	circulation (high confidence) . -> check ponctuation [María Santolaria-Otín, France]	Noted. Text has been revised
101675	5	49	5	49	It is surprising that this paragraph does not mention sea-ice thickness, and the shift from multi-year to seasonal ice in the Arctic. See for example: Kwok, R. (2018). Arctic sea ice thickness, volume, and multiyear ice coverage: losses and coupled variability (1958–2018). Environmental Research Letters, 13(10), 105005. [IAPSO ECS group review, United States of America]	Noted. This is covered in the Executive summary of chapter 2.
61473	5	49	6	8	Executive Summary: Maybe separating Arctic from Antarctic in the text will help and will be more easy for who is reading. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now have one paragraph each for the Arctic and the Antarctic
263	5	51	5	51	First sentence needs a "has" before declined. [THOMAS Wagner, United States of America]	Noted. This sentence has been re-written

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129315	5	51	5	51	If one gives a very specific year, there needs to be an explanation of why. Is this when good observations started or what? If no reason, then say "over the past 5 decades" or something similar (and authors could even add "since the CO2 warming influence took control over the sulfate aerosol cooling influence" though that is probably something to leave to the detection-attribution chapter). If one does not provide some explanation, the reader is left to wonder why 1979 was special. And were the changes really "consistent" (i.e., the same amount or percentage per decade)? And, of course, there is the question of how long and stable the baseline was before that, and is what is happening now more significant than what happened in the 1930s in the Atlantic basin? The focus should be on the decadal average and make clear that there is still variability on an annual basis, so note that the decadal average trend has been consistently downward over the past 5 decades. [Trigg Talley, United States of America]	Accepted. The specific observational period has been removed from this paragraph. We also explicitly state the forced relationship on decadal scales, and emphasize the super-imposed internal variability.
62283	5	51	5	51	The term "seasonal area" is vague. Does this mean "annual average area" or is it referring to area during one particular season? If the latter, I suggest rewording to "summer area" or something similar. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This has been re-written and the term "seasonal area" is no longer used.
14675	5	51	5	51	"Arctic sea ice retreated and seasonal area declined consistently since 1979." -> "Arctic sea ice has retreated and seasonal area has declined consistently since 1979. " [Jeremy Fyke, Canada]	Accepted. These sentences have been rewritten.
65829	5	51	5	52	Please check the grammar, suggest changing to: "Arctic sea ice has retreated and seasonal area has declined consistently since 1979. The Arctic Ocean will likely become practically ice-free in September before 2050 for all emission scenarios." [Kushla Munro, Australia]	Accepted. These sentences have been rewritten.
99117	5	51	5	52	On the Arctic sea ice, how the metric of interest has become the September sea ice cover being completely gone is a mystery to me and is very unfortunate. The metric would much better be the amount of heat being taken up during the warm season and then released during the cold season when warming of the Arctic affects Northern Hemisphere mid-latitude weather. And the Arctic uptake of heat in the summer is really not affected much by how much ice there is in September, but when the albedo of the snow atop the sea ice melts and the icee starts thinning and cracking up, etc. It would be very refreshing if this assessment refocused/made clear on the heat uptake and then consdquent effect on NH weather and made clear that the exact amount of ice in September is really not the metric to be using. [Michael MacCracken, United States of America]	Noted. September sea-ice area can be seen as a simple integrated measure of the heat that has accumulated throughout summer, which makes this choice plausible. In addition, we assess primarily published literature, which focuses almost exclusively on September sea-ice area.
97945	5	51	5	53	For the Northern Hemi, September is climatologically the warmest SST month (March in S Hemi) so the "..ice-free in September.." made sense to me, but may not to a reader less tuned into the SST peaks and troughs through the year regionally. Suggest changing to make that point (warmest month of year) a little clearer [Paul Durack, United States of America]	Noted. To keep this brief, we assume that most readers will be familiar with the fact that September is the month of minimum sea-ice coverage in the Arctic
39177	5	51	6	2	The way the statement is worded, does it mean that because Arctic sea-ice area in all months is inearly related to global mean atmospheric temperature and cumulaive ganthropogenic CO2 emissions, it does not imply a tipping point for the loss of Arctic summer sea-ice? [Lourdes Tibig, Philippines]	Accepted. This is the intended meaning. We have now re-written this for clarity.
265	5	51	6	7	Thickness/volume needs to be addressed. Estimates of 70% volume loss are out there. Overland, Kwok, others. [THOMAS Wagner, United States of America]	Noted. This is covered in the Executive summary of chapter 2.
83269	5	51	6	7	This high-level sea ice section also needs to mention/highlight the thinning and decreasing age of Arctic sea ice, and the key lack of sea ice thickness information around Antarctica. [Robert Massom, Australia]	Noted. This is covered in the Executive summary of chapter 2.
96849	5	51	6	7	This statement is very similar to the statement in Ch4 (page 4-7, line 54 to page 8 line 1). Please streamline. [Nicole Wilke, Germany]	Noted. For a minimum level of self-containment, we repeat this statement from chapter 4, but now have a stronger focus on the underlying processes explaining this finding.
96851	5	51	6	7	Very similar text only a few lines apart (statement about ice free), please streamline. [Nicole Wilke, Germany]	Accepted. These sentences have been rewritten.
34929	5	51	6	7	The SOD claims an unprecedented loss in Arctic sea ice over the last 1000 years. See rebuttal comment #7 above. [Jim O'Brien, Ireland]	Noted. See reply there.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83615	5	51			P9-5 Executive summary Lines 51-55 state: “Sea ice 50 51 Arctic sea ice retreated and seasonal area declined consistently since 1979. The Arctic Ocean will likely 52 become practically ice-free in September for the first time before 2050 for all emission scenarios. No 53 significant trend in overall Antarctic sea ice is detected from 1979 to 2018. Arctic summer sea-ice area 54 during the last decade was the lowest since at least 1850, and humans have very likely caused at least half of 55 Arctic summer sea ice loss since 1979.” This appears to ignore completely the body of work reconstructing arctic ice movement prior to 1850. Three relevant refs are Babich, V.V., A. V. Dar’in, I. A. Kalugin, and L. G. Smolyaninova, , 2016, Climate Prediction for the Extratropical Northern Hemisphere for the Next 500 Years Based on Periodic Natural Processes, Russian Meteorology and Hydrology Vol. 41 No. 9 Cabedo-Sanz, P., Belt, S. T., Jennings, A. E., Andrews, J. T., and Geirsdóttir, Á. (2016). Variability in drift ice export from the Arctic Ocean to the North Icelandic Shelf over the last 8000 years: A multi-proxy evaluation. Quat. Sci. Rev. 146, 99–115. doi:10.1016/j.quascirev.2016.06.012. Humlum, O. , Jan-Erik Solheim , Kjell Stordahl, 2011, Identifying natural contributions to late Holocene climate change, Global and Planetary Change 79 (2011) 145–156 Lüdecke H-J and , C.O.Weiss, 2017, Harmonic Analysis of Worldwide Temperature Proxies for 2000 Years. The Open Atmospheric Science Journal, 11, 44 -53. Cabedo-Sanz 2016 uses the IP25 proxy for arctic sea ice cover (hole MD99-2275) fig 5, It is very clear that a) Using 1850 as a starting point, as written in AR6, is quite misleading. Year 1850 was an extreme cold year with maximum area of ice cover (not surprising – it was the end of the LIA). It also coincides with maximum extent of the Alps Great Aletch glacier, and minimum temperatures in the Ge et al reconstruction of historical temp in China (see figure attached to next review comment)	Rejected. None of these references give a process-level understanding of the observed sea-ice retreat of the past fourteen years, while there is ample literature available that shows the clear relationship throughout this period between anthropogenically-caused global warming and Arctic sea-ice loss.
51493	5	52	5	52	The phrase 'practically ice-free' is a bit ambiguous, could this be changed to something like 'almost completely', or perhaps just remove practically, as per the underlying text in lines 2-3 of page 6? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The term "practically" is used in the literature to refer to a sea-ice area of below 1 million km2 as also defined in the main text of the chapter. We agree that this term is somewhat ambiguous, but do not think that "almost completely" is less ambiguous.
42621	5	52	5	52	Perhaps add a since in the sentence, i.e. "The Arctic Ocean will likely become practically ice-free in September for the first time since XX before 2050 for all emission scenarios". [Sofie Schöld, Sweden]	Noted. Unfortunately, we do not have sufficient proxy records available to add such time, even though that would be very helpful indeed.
129317	5	52	5	53	So, if one looks for a multi-decadeal trend for Antarctica over the same period, the latest declines do not get mentioned. What is lost is a statement that there are multiple forcings acting here, so doing a linear analysis is not really very helpful. What one had was an atmospheric circulation change due to changes in stratospheric ozone that tended to increase the area of sea ice, and, with that declining and the CO2 influence growing, the sea ice is showing a retreat. While this may well be happening, it's very unlikely there is yet enough data and analysis to say this with any statistical confidence. The text could well say the changes are not inconsistent with the changing dual influences but there is not enough of a record to say this with high statistical confidence. Saying there is no trend over this period is simple, but that is not what would be expected (increasing CO2 is not the only forcing on the world), and the phrasing here is basically showing no sophistication in understanding the world around us. [Trigg Talley, United States of America]	Accepted. The Antarctic paragraph has been re-written to emphasize more the process-level understanding of the regional changes and the substantial interannual variability.

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99119	5	52	5	53	This is a really misleading conclusion based, I presume on doing a linear fit to the data over the indicated period. There are two major forcings active, namely the effects on atmospheric circulation by stratospheric ozone depletion, which pushes sea ice cover away from the continent so enhances the area, and global ocean warming, which tends to shrink the cover. So, the ozone effectwas dominant in late 20th and into 21st century and now, with ozone depletion declining and global warming growing, the sea ice is starting to shrink. Applying a linear trend analysis just makes no sense if one knows anything about the causes and interactions. So, a more nuanced statement is needed--and helping make clear to the public (and especially the denier groups) that the world responds to more than one influence and so just because sea ice was not steadily declining does not refute global warming, etc. Please rewrite this and show that some understanding of physics has been used (and I would note that the earlier chapter on observations does make the point that there are two different trends, etc.--so this chapter should as well). [Michael MacCracken, United States of America]	Accepted. The Antarctic paragraph has been re-written to emphasize more the process-level understanding of the regional changes and the substantial interannual variability.
15205	5	52	6	2	Does the finding that it is "extremely likely that the remaining sea-ice has become thinner" from page 44, line 30-31 warrants mention in the Executive Summary? Ice area is only mentioned here, whereas thickness is important to Arctic peoples (not to mentiong for indication of potential future melt) [Simon Donner, Canada]	Noted. This is covered in the Executive summary of chapter 2.
15197	5	52	6	7	The summary is fine, but I'd recommend explicitly in stating that Antarctic sea ice is not expected to show a signficiant trend (because of wind forcing, etc.). The assessment here, though accurate, may further the misconception among non-experts that the lack of a decreasing Antarctic sea ice trend is a sign of a lack of warming (in the region). [Simon Donner, Canada]	Accepted. The Antarctic paragraph has been re-written to emphasize more the process-level understanding of the regional changes and the substantial interannual variability.
29237	5	52			Chapter 9, Executive summary, sea ice Antarctica. It is suggested that there is no significant trend. I am no expert but the paper by Parkinson et al. 2019 PNAS may give some extra insights [Francesca Sangiorgi, Netherlands]	Noted. Since the Parkinson paper was published, Antarctic sea-ice area has recovered substantially in most months, so there still is no significant trend.
90441	5	52			I am not quite sure what the word "practically" has to mean. Is "ice-free" different from "practically ice-free"? [Holly Kyeore Han, Canada]	Noted. "Practically" is a term used in the literature to refer to a very small remaining sea-ice cover of less than 1 million km2
83617	5	52			P9-5 Lines 51-55 : Attached a figure from Asten 2019 combined with Cabedo-Sanz et al 2016 fig 5. If the figure (a *.jpg) does not survive the distribution of the xlsx spreadsheet of Review comments, feel free to email me at michael.asten@monash.edu Asten, M.W., Kuan-Hui Elaine Lin, and Carl Otto Weiss, 2019, Common natural climate cycles observed for European glaciers, temperature proxies for China, and for a global temperature proxy covering the Common Era, Geophysical Research Abstracts, Vol. 21, EGU2019-11662-1, 2019. [michael asten, Australia]	Noted. We can only assess per-reviewed results.
83619	5	52			picture jpg [michael asten, Australia]	Noted. We can only assess per-reviewed results.
8977	5	53	5	53	Antarctica sea ice decreased slowly and then very abruptly in the last 4 years. This summary of no trend is not justified by the observations and even misleading. The drop in sea ice cover since 2016 is enormous! [Eric Rignot, United States of America]	Noted. The large interannual variability is now explicitly stated. Note, however, that Antarctic sea ice has recovered in most month over the past few years.
103757	5	53	5	53	Calrify if you refer to Antarctic sea ice extent, thickness or volume [Philippe Tulkens, Belgium]	Accepted. We now specifically refer to sea-ice area
80793	5	53	5	53	Calrify if you refer to Antarctic sea ice extent, thickness or volume [Louise Sandberg Sørensen, Denmark]	Accepted. We now specifically refer to sea-ice area
85207	5	53	5	53	Is it worth mentioning recent sharp drop in Antarctic sea ice in this summary paragraph or is its duration not sufficiently robust to warrant mentioning here? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sharp decline was followed by several years of recovery in the most recent records, which is why we do not explicitly mention it here.
71981	5	53			But I thought there were substantial regional variations of loss/gain. [John Church, Australia]	Accepted. These regional patterns are now explicitly highlighted in the new paragraph on Antarctic sea ice.
99121	5	54	5	54	So, a phrase needs to be added here, having it say: "since at least 1850 when direct observations started to become available, and likely back for several thousand years based on indirect indicators (so to a time when the Earth's orbit around the Sun led to considerably more solar radiation reaching the Arctic during summer). Humans have ..." (so make the second phrase in the sentence a separate sentence). [Michael MacCracken, United States of America]	Noted. This is no longer covered in the ES of this chapter.
88235	5	54	5	54	Would it be better to say "...human induced warming has.." or "..anthropogenic warming has…" rather than "…humans have very likely …." since the influence of human activity is through climate warming not directly on the sea ice. [Sharon Smith, Canada]	Noted. This is no longer covered in the ES of this chapter.

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91071	5	54	5	54	While not exactly contradictory, the statement in the Atlas says "September sea-ice 19 minimum very likely having reduced by 12.8 ± 2.3% per decade during the satellite era (1979 to 2018) to 20 levels unprecedented for at least 1,000 years (high confidence)" which is quite a bit stronger than "since 1850" [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is no longer covered in the ES of this chapter.
38089	5	54	5	54	"at least" can be deleted. [Junhee Lee, Republic of Korea]	Noted. This statement has been removed from Chapter 9 ES.
129319	5	54	5	55	In saying a year, it needs to be made clear why that year was chosen. In this case, modify to something like "since the availablility of a spatially representatives set of observations became available in the mid-19th century." For the "1979", suggest just saying "over the last 5 decades". [Trigg Talley, United States of America]	Accepted. We now no longer mention these specific periods, or if we do explain why (e.g., satellite observations since 1979)
99123	5	54	5	55	Is there really any other possible factor than global warming (and deposition of dark particles). I'd urge rewriting this phrase as: "Anthropogenic factors, both greenhouse gas warming and depositon of dark particles, are the only known processes that could be accelerating the ice loss in the way that has been observed." [Michael MacCracken, United States of America]	Noted. We now more explicitly highlight the process understanding of the observed loss in Arctic sea-ice area
52033	5	55	6	1	We need to be a bit careful about statements like "Arctic sea-ice area in all months is linearly related to global mean atmospheric temperature and cumulative anthropogenic CO2 emissions" because it implies direct causality between CO2 emissions and sea ice area without the intermediate warming step (sea ice responds to global temperature, which in turn responds to CO2 emissions). Perhaps adding "and hence" or "in turn" or something here will stop this being potentially (or even deliberately) misunderstood? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been re-written to clarify this.
6745	5	55	6	1	It needs to be clarified that the linear relationship between Arctic sea-ice area and global-mean surface temperature applies to long-term trends. It does not appear to hold (strongly at least) for sub-decadal variability. Arctic summer sea-ice area decreased strongly in the 2000s, at a time when there was a slowdown in the increase in global-mean temperature. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The impact of internal variability is now emphasized explicitly
86809	5	55	6	2	Arctic sea-ice area in all months is linearly related to global mean atmospheric temperature and cumulative anthropogenic CO2 emissions, which implies no tipping point for the loss of Arctic summer sea ice. Please consider to include information about irreversibility for Arctic summer sea ice extent and also please consider distinguishing between one-year and multi-year sea ice if appropriate. [Oyvind Christophersen, Norway]	Noted. For space constraints, we did not specifically point out the lack of irreversibility as this is largely implied by the lack of a tipping point. This remains true in principle for the difference between multi-year and first-year sea ice, which is why we do not specifically highlight this here, either
129321	5	55	6	2	[CONFIDENCE] Saying this "is" the relationship seems overstated given the variabiility, etc. The comparison is for trends in decadal averages, so this means there are only five points which would seem to suggest saying something like "Decadal-average Arctic sea-ice cover in all months over the last five decades appears to be roughly linearly related to the change in the global average temperature and cumulative anthropogenc CO2 emissions, ... ". There also needs to be clarification of whether this relationship is for average amount, percentage change, or something else (and how does the trend in ice cover thickness fit in?). Extrapolating into the future, what happens when all the ice disappears; the ice is also thinning, so what is the effect of this, etc. If the relationship is to percentage change, then this would imply that the absolute amount of ice cover change is dropping as warming continues (and this would be amplified by convergence of longitudinal lines, etc.). So, this statement is really incomprehensible and to draw a conclusion that there is no tipping point seems totally unjustified based on what is said here. Is this being said for every month of the year with changing solar radiation changing by season? How can this ES finding be made with absolutely no qualification or liklihood as IPCC lexicon requires? [Trigg Talley, United States of America]	Accepted. The relationship has been explained more explicitly. It only refers to sea-ice area, as also discussed in the original text.
99125	5	55	6	2	Theobservational record that leads to "is linearly" seems pretty limited as a basis for inferring that this will continue and that it implies "no tipping point". Given all the caution in other statements, this seems out of place. [Michael MacCracken, United States of America]	Accepted. This logical inference has been removed from the text.
62133	5	55	6	2	Address confidence in linear relationship and tipping point [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, confidence level has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51965	5	55	6	2	It is not clear that such a linear relationship would necessarily imply no tipping point. A counterexample might be formed by considering that summer ice area depends on other variables, which even with a weak correlation, may be able to produce a catastrophe or a sudden change (in a dynamical systems sense, e.g. the change from one wing to another of the Lorenz attractor). This seems to be an attempt to oversimplify more justified statements, such as in 9.3.1.1 - page 44, lines 14-24. Suggest that the summary statement is revised. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This logical inference has been removed from the text.
61959	5		8		in general sea level rise and global sea level rise(GSLR): one term should be used in the summary [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now use GSLR throughout
80653	6	1	6	1	the 2 of 'CO2" should be subscript. This error occurs at other places in the chapter (e.g. page 14 line18) so it should be checked and corrected in the whole chapter [Helene Jacot Des Combes, Marshall Islands]	Accepted. We have corrected this throughout.
97947	6	1	6	1	"atmospheric temperature AND cumulative anthropogenic CO2 emissions" these things aren't an AND, temperatures respond to CO2 emissions. AND -> due to might be more appropriate [Paul Durack, United States of America]	Accepted, we have re-written to now say "thus"
52035	6	1	6	2	I don't like the framing of "which implies no tipping point for the loss of Arctic summer sea ice" here. Although I get the argument that there is high confidence in no tipping point, I don't think it captures the whole story as it is not only global temperature rise that the sea ice can respond to. There is enough warm water trapped in the Arctic Ocean (in the 'Atlantic warm-layer') to melt the sea ice many times over. In theory mixing processes (wind/wave-induced) could cause the halocline to break down allowing this to melt all the ice. Granted the present models don't simulate this mechanism but then they don't really represent these sort of processes very well in high-latitudes (mixing, shelves & shelf-break, tides), so it could be possible. In summary I think it's fine to say here that our current understanding, based on present modelling studies, is that there will be no tipping point for Arctic sea ice and to give this "high confidence". However I don't think it should be said in this way - i.e., that it can be inferred from the linear realtionship with GMST/CO2. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This logical inference has been removed from the text.
14677	6	1	6	2	"no tipping point" -> "no abrupt, non-linear tipping point". More fundamentally, does past linear behaviour robustly imply futrue linear behaviour? I don't think it does. [Jeremy Fyke, Canada]	Accepted. This logical inference has been removed from the text. The term tipping point is now defined in AR6 along the lines outlined by this comment, so the explanation is not needed here.
132539	6	1	6	2	This is correct (Arctic sea ice has no tipping piont), but note this is inconsistent with Chapter 4's assessment, perhaps due to different defintions of tipping points and reversibility (see Chap 4 pages 92-96). [Kyle Armour, United States of America]	Noted. Chapter 4 has revised that statement and now are consistent with the assessment in chapter 9
38091	6	1	6	2	"which implies no tipping point for the loss of Arctic summer sea ice." is not necessary to be included in the summary. [Junhee Lee, Republic of Korea]	Noted. We feel it is useful to keep this information in the executive summary as public debate often talks about the possible impact of the ice-albedo feedback.
62019	6	1	6	8	The sub-sections included in the terrestrial cryosphere section are glaciers, permafrost and seasonal snow cover. How about seasonal ice cover (lakes and rivers)? Maybe it's covered in other chapters? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Lake ice is not covered in this chapter.
33405	6	1			Change: "…CO2…" by "…CO2…". [Guiomar Rotllant, Spain]	Accepted. We have corrected this throughout.
267	6	2	6	3	Ice free is probably not true. Northwest passgea prbably won't even be open; I think we should explain effectively ice free and what it means. [THOMAS Wagner, United States of America]	Noted. The term "practically" is used in the literature to refer to a sea-ice area of below 1 million km2 as also defined in the main text of the chapter.
99127	6	2	6	3	Again, that September sea ice cover is the metric seems to me unfortunate, and if it is to be used, its usefulness and basis need to be explained as the additional uptake of energy for this happening as compared to the present decreased cover seems like a very small term. [Michael MacCracken, United States of America]	Noted. September sea-ice area can be seen as a simple integrated measure of the heat that has accumulated throughout summer, which makes this choice plausible. In addition, we assess primarily published literature, which focuses almost exclusively on September sea-ice area.
67127	6	3	6	3	cumulative since when? [Regine Hock, United States of America]	Noted. The linear relationship is independent of a specific start time, so this is not emphasized here.
103759	6	3	6	3	Which statement does the (high confidence) refer to? [Philippe Tulkens, Belgium]	Accepted. The confidence level is now attached specifically to individual statements throughout this paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98675	6	3	6	3	Move the period from after "GtCO2" to after "(high confidence)". [Sonya Legg, United States of America]	Accepted. This has been corrected.
110965	6	3	6	3	Space is missing between "Gt" and "CO2". Period is to be put after the parentheses. [Ning Zhao, Germany]	Accepted. The term GtCO2 is a technical term that we decided to leave unchanged. We have moved the period.
80795	6	3	6	3	Which statement does the (high confidence) refer to? [Louise Sandberg Sørensen, Denmark]	Accepted. The confidence level is now attached specifically to individual statements throughout this paragraph.
97949	6	3	6	3	"..CO2 emissions reach 1000 GtCO2..". First up, cool, what does 1000 GtCO2 mean, how much warming, which scenario(s), which year(s) and is this CO2 equivalent or pure CO2? Some clearer text would be useful [Paul Durack, United States of America]	Noted. This statement is independent of a given warming, year and scenario.
26357	6	3	6	3	1000 GtCO2. (high confidence) -> check ponctuation 1000 GtCO2 (high confidence) . [María Santolaria-Otín, France]	Accepted. This has been corrected.
61949	6	3	6	3	put confidence level after GTCO2 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The term "likely" implies "high confidence"
90701	6	3	6	4	I suggest to revise the statement, "it can only be stated with medium confidence that active layer thickness increase is a pan-Arctic phenomenon". For example, a progressive increase in ALT has been observed across Northern Hemisphere (Luo et al., 2016), except Northern Alaska and West Siberia located in continuous permafrost zones (Shiklomanov et al., 2012). Luo, D., Q. Wu, H. Jin, S. S. Marchenko, L. Z. Lü, S. Gao, 2016, Recent changes in the active layer thickness across the northern hemisphere, Environ. Earth Sci., 75, 555. Shiklomanov, N. I., Streletskiy, D. A., Nelson, F. E., 2012, Northern Hemisphere Component of the Global Circumpolar Active Layer Monitoring (CALM) Program, Proceedings of the Tenth International Conference on Permafrost, Salekhard, Russia, June 25-29, 2012. [Thian Yew Gan, Canada]	Rejected. The reviewer's suggested references are pre-SROCC papers (2012, 2016). We build our assessment mainly on SROCC, on the evidence presented in Chapter 2 which shows pervasive ALT increase in the European and Russian Arctic, but no absolutely clear pan-Arctic increase, and on recent additional research, in particular on Streletskiy et al. 2017 and O'Neill et al., 2019. These papers show that emergence of a clearer global picture is hampered by (1) uneven distribution of observing sites, (2) substantial variability among the existing sites, strongly influenced by local conditions (soil constituents and moisture, snow cover, vegetation); (3) interannual variability, and (4) thaw settlement in ice-rich terrain. Therefore, although we do think, as the reviewer does, that that the ALT increase is indeed a pan-Arctic phenomenon, we can, at this stage, only state this with medium confidence as in SROCC.
111731	6	3	6	5	I think this sentence is too technical for the ES and doesn't add value. Suggest delete it. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This sentence has been re-written to emphasize that different drivers combine to cause the observed evolution of Antarctic sea ice.
99459	6	3	6	5	The statement made here seems rather strange, suggest either clarifying or removing. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This sentence has been re-written for clarity.
129323	6	3	6	7	[CONFIDENCE] Can authors review their assignment of confidence for the executive summary related to Antarctic Sea Ice? In Chapter 9, it states: "The evolution of mean Antarctic sea-ice area is the result of opposing regional trends, with slightly decreasing sea-ice cover during the period 1979 to 2018 in the Amundsen Sea and the Bellingshausen Sea, particularly during summer, and slightly increasing sea-ice cover in the eastern parts of the Weddell Sea and the Ross Sea (medium confidence; see Figure 9.17)" There is medium confidence related to some part of Antarctic sea ice noted in Chapter 9. The SROCC Summary for Policymakers also notes high confidence related to the topic of Antarctica and sea ice: "Antarctic sea ice extent overall has had no statistically significant trend (1979-2018) due to contrasting regional signals and large interannual variability (high confidence)." [Trigg Talley, United States of America]	Accepted. This has been corrected in the main part of the chapter to now read "high confidence" in the observed changes throughout.
129325	6	3	6	7	It would help here to say that there are two apparently counter-acting influences -- namely, ozone depletion and global warming. Hard to say if all one is doing is looking for linear trends, but if one thinks about each forcing and its changing magnitude, some insight would seem likely to emerge. Is this chapter really supposed to just ignore theoretical considerations and do trend analyses? [Trigg Talley, United States of America]	Accepted. The Antarctic paragraph has been re-written to emphasize more the process-level understanding of the regional changes, the substantial interannual variability and their possible drivers.
83267	6	3	6	7	This doesn't adequately capture what is happening to Antarctic sea ice. For example, substantial and contrasting regional change is occurring in different sectors around Antarctica, bit only in area of coverage but also in sea ice seasonality (annual timings of sea ice advance and retreat and resultant duration of the annual sea ice season). The lack of understanding of seasonal processes and the role of the ocean is not mentioned or captured here either. [Robert Massom, Australia]	Accepted. The Antarctic paragraph has been re-written to emphasize more the process-level understanding of the regional changes, the substantial interannual variability and their possible drivers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111729	6	3			Just for clarity - do you mean cumulative emissions after today, or since pre-industrial? [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The linear relationship is independent of a specific start time, so this is not emphasized here.
32401	6	3			"GtCO2. (high confidence)" Unclear, if high confidence belongs to the previous sentence of the part following: "Slight Antarctic ..." [Olaf Eisen, Germany]	Accepted. The confidence level is now attached specifically to individual statements throughout this paragraph.
61951	6	4	6	4	in ice are likely the result from [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This sentence has been re-written.
12165	6	5	6	5	Maybe I misunderstand, but it seems we are stating that we have high confidence that “climate projections require accurate modelling of regional variations”. This is not wrong, but we are here giving a confidence level to something else than an observation of the climate. [Thomas Lavergne, Norway]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
99129	6	5	6	7	This seems like a real cop-out. There needs to be an explanation indicating that there have been competing forcings and that, perhaps along with changes in salinity due to freshening, is what is making near-term regional projections difficult. [Michael MacCracken, United States of America]	Accepted. We now provide a more detailed discussion of regional trends in Antarctic sea ice.
111427	6	6	6	6	The word "infer" should be "imply". [James Renwick, New Zealand]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
15227	6	8	6	18	The likelihood statements presented here with respect to GMSL contribution from Greenland and Antarctica, though reasonable from my read of the literature, are also incomplete. They fall into the communications trap that befell AR4 and other assessments. To avoid this, the executive summary needs to state i) that models are not able to represent all ice melt processes, ii) that the deep uncertainty (mentioned for WAIS) implies the possibility of much greater GMSL by 2100 and beyond from ice sheet melt than found in model results (otherwise, deep uncertainty can be interpreted as a chance of lower rise than models project) and iii) provide the higher numerical estimates from expert elicitation studies (even though those estimates may be low confidence). The Hinkel et al. paper in Nature Climate Change, among others, speak to the need to express the full range of possibilities, including the low probablity long tail risks. This is one of the most critical issues facing AR6 -- the pandemic is evidence that as scientists we need to, and governments want us to, highlight the long tail risks. [Simon Donner, Canada]	Taken into account. Uncertain processes and deep uncertainty are explicitly mentioned in the revised draft.
355	6	9	6	9	I found the choice of the units (in m and not providing rate) quite disturbing. For me, it is much more difficult to gauge the magnitude of the contribution in this unit and it implies providing many decimals. But I guess this is choice made for good reasons. [Etienne Berthier, France]	Noted. Mass loss is given in Gt, and SLR in m for consistency across the chapter and the report.
99133	6	9	6	28	So, given these findings, cautiously as they are stated, and the deep uncertainty about them that exists as models have yet to show they can treat ice movement (as the ice itself warms) and thinning of the ice shelves that hold back the ice streams, how is it that the risk of significant loss, even up to rates of a few meters per century, can so assuredly be discounted in making the estimate for likely change this century. The risk is huge, even if not confirmed with two-sigma results from models. [Michael MacCracken, United States of America]	Taken into account. Most of the uncertainty is related to the upper tail of the projections. So the given likely range implies that we leave room for more than 16% (closer to 33%) chance of exceeding the upper limit of our given range. However, the currently available evidence does not suggest a >33% chance of higher outcomes. Moreover, here in WG1 we treat the hazard, not the risk.
32453	6	11	6	11	Surely it is certain that Greenland has lost mass, from multiple lines of evidence. (i.e. not just virtually certain) [Robert Colman, Australia]	Noted. Virtually certain (99%) is a high enough probability for any practical purposes. See C129329
99687	6	11	6	12	statement on Antarctica specifies "by the end of this century" - should do same here [Peter Clark, United States of America]	Accepted. The Greenland and Antarctica statements are now more integrated and aligned with each other where this makes sense.
62067	6	11	6	12	This statement feels vague without a clarification regarding whether 'will continue to lose mass' referes to all emission scenarios and whether there is confidence statement for that? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Added by the end of this century and under all emission scenarios.
62135	6	11	6	12	Continue to lose mass until when? Consider modelling the statement for Greenland on the statement below made for Antarctica (lines 19-20 , page 9-6) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Mass loss numbers are specified for 2100.
3049	6	11	6	12	A time to which the projection "will continue to lose mass" is referring to is missing. [Daniel Farinotti, Switzerland]	Accepted. Time scale explicitly added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129329	6	11	6	13	No data suggests that the Greenland Ice Sheet has not been losing mass. Cannot that be said without the first six words of the sentence? After all, just above, it was stated without any qualification that there is a linear realtionship between ice cover and temperature that it is asserted will continue as linear decades into the future. If the 1-in-100 exception to this statement is about whether this trend began in the 1990s or 1980s or 2000s state that, but that GIS is losing ice needs to be said without qualification. Given the next sentence, how about changing line 11 to "The Greenland Ice Sheet transitioned from a relatively steady mass during the latter 20th century to clearly losing mass in the 21st century." [Trigg Talley, United States of America]	Taken into account. The new wording of the ES does not suggest equilibrium before 1990.
129327	6	11	6	17	Can the authors review their ranges for sea level rise as they compare to the SROCC values reported in 2019? The ranges for RCP2.6 and RCP8.5 seem to differ slightly from what was stated in the SROCC. SROCC SPM states: "In 2100, the Greenland Ice Sheet's projected contribution to GMSL rise is 0.07 m (0.04-0.12 m, likely range) under RCP2.6, and 0.15 m (0.08-0.27 m, likely range) under RCP8.5. In 2100, the Antarctic Ice Sheet is projected to contribute 0.04 m (0.01-0.11 m, likely range) under RCP2.6, and 0.12 m (0.03-0.28 m, likely range) under RCP8.5. The Greenland Ice Sheet is currently contributing more to sea level rise than the Antarctic Ice Sheet (high confidence), but Antarctica could become a larger contributor by the end of the 21st century as a consequence of rapid retreat (low confidence). Beyond 2100, increasing divergence between Greenland and Antarctica's relative contributions to GMSL rise under RCP8.5 has important consequences for the pace of relative sea level rise in the Northern Hemisphere." [Trigg Talley, United States of America]	Taken into account. SROCC starting points are now given systematically in the main body of the chapter. However, this is a new assessment based on new available evidence, so it is not problematic as such that numbers differ from those given in the SROCC.
34931	6	11	6	17	The SOD claims that the Greenland ice sheet state is unprecedented over centuries. See rebuttal comment #8 above. [Jim O'Brien, Ireland]	Noted. Unclear which SOD ES statement this refers to. The word "unprecedented" does not occur in the cited paragraph. In this context, the reviewer may want to read https://www.nature.com/articles/s41586-020-2742-6
97953	6	11	6	28	I wonder whether the quantified statements 0.0106+-0.0009m SLR (Greenland 1992-2018) and 0.0069+-0.0014m SLR (Antarctica 1992-2018) should be referenced back to earlier estimates, so an increased RATE since AR5/SROCC etc? On this point one of the major SLR challenges is the multi-century aspect, with baked in SLR no matter what is done. Some forward >2100 statements could be useful to link this to SLR here [Paul Durack, United States of America]	Noted. SROCC and AR5 starting points are made much clearer in the revised draft. However, the allotted space for the ES does not allow to present these numbers, unfortunately. The multi-centennial commitment is referred to in both ice sheet and sea level statements
49955	6	12	6	13	Mass losses being "negative" is a bit confusing, as it suggests potential increase in mass. The sign convention should be clarified or different language should be used to make this more clear that the ice-sheet has lost mass each year since the early 2000s. [Daniel Gilford, United States of America]	Taken into account. Sentence changed.
100141	6	12	6	13	"...losses have been consistently negative…" negative losses is confusing, is it negative mass balance? Negative mass losses sounds like a gain, which I don't think is what the authors are trying to communicate here [Carlye Peterson, United States of America]	Taken into account. Sentence changed.
103761	6	12	6	13	mass losses -> mass changes (it doesn't make sense to talk about negative mass loss) [Philippe Tulkens, Belgium]	Taken into account. Sentence changed.
80797	6	12	6	13	mass losses -> mass changes (it doesn't make sense to talk about negative mass loss) [Louise Sandberg Sørensen, Denmark]	Taken into account. No negative mass losses in the revised version.
15779	6	12	6	13	Reword "Greenland was likely close to mass balance in the 1990s and there is high confidence that annual mass losses have been consistently negative since the early 2000s" to something like "Greeland ice sheet was likely in balance, i.e. did not experience net loss or gain of ice mass in the 1990s. There is high confidence that it has been experiencing mass loss since early 200s." [Olga Sergienko, United States of America]	Noted. No statement on state before 1990 is made in the revised version, so this comment is not applicable.
29633	6	12	6	13	" annual mass losses have been consistently negative since the early 2000s" it seems losses and negative contradict to each other. I would suggest to reword this sentence. [Aixue Hu, United States of America]	Taken into account. No negative mass losses in the revised version.
2523	6	12	6	13	negative ice mass losses suggest ice is growing [Tim Hermans, Netherlands]	Taken into account. No negative mass losses in the revised version.
66451	6	12			Just a suggestion of changing the "close to mass balance" to "close to equilibrium in mass balance"/ "close to steady state" (steady state term from Cogley et al.)/other term, where "mass balance" as a description of glacier's state would be avoided. IPCC Glossary decribes mass balance as: "Difference between the mass input (accumulation) and the mass loss (ablation) of an ice body (e.g., a glacier or ice sheet) over a stated time period, which is often a year or a season." Therefore, I guess, a Reader who - just after reading this line of the Report - had checked in Glossary what "glacier balance" is, might be a bit confused?... [Barbara Barzycka, Poland]	Noted. No statement on state before 1990 is made in the revised version, so this comment is not applicable.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32403	6	12			As "mass balance" is a fixed term, it sounds a bit strange to use "close to mass balance". Suggested rewrite: "was close to a balanced mass budget" [Olaf Eisen, Germany]	Noted. No statement on state before 1990 is made in the revised version, so this comment is not applicable.
29611	6	12			“Greenland was likely close to mass balance…” It seems that there is a word missing in this sentence? [Villasenor Tania, Chile]	Noted. No statement on state before 1990 is made in the revised version, so this comment is not applicable.
110967	6	13	6	13	Negative mass loss sounds confusing. Is the mass gaining or losing? If losing, maybe negative mass "changes" should be used. [Ning Zhao, Germany]	Taken into account. No negative mass losses in the revised version.
97951	6	13	6	13	"..losses have been consistently negative since the early 2000s" does that mean that they have been (-loss equals) accumulating? [Paul Durack, United States of America]	Taken into account. No negative mass losses in the revised version.
10233	6	13	6	13	"negative" "loss" sounds confusing (loss already connotating negative). [Katsumata Katsuro, Japan]	Taken into account. No negative mass losses in the revised version.
15535	6	13	6	14	The mass loss of the Greenland Ice Sheet associated with 0.0106 m of sea level rise is huge, i.e. 3800 Gt. It is suggested to be reflected in the Executive Summary for the readers to appreciate the significance. [SAI MING LEE, China]	Noted. Future changes of the ice sheet masses are provided in m SLR for consistency across the report, although the reviewer's comment is very sensible.
39643	6	13		14	"Greenland was likely ….since early 2000s" apparent contradiction with the statement in bold L11. [TSU WGI, France]	Noted. No statement on state before 1990 is made in the revised version, so this apparently perceived contradiction cannot occur in the revised version
49957	6	14	6	14	to *global mean* sea-level rise [Daniel Gilford, United States of America]	Taken into account. No negative mass losses in the revised version.
101677	6	14	6	14	"with surface melt slightly exceeding losses through ice flow" -> "with losses due to surface melt slightly exceeding losses through ice flow" [IAPSO ECS group review, United States of America]	Taken into account. This sentence was clarified: 'This mass loss is driven by both discharge and surface melt, with the latter increasingly becoming the dominating component of mass loss..."
67129	6	14	6	14	too many decimals. 1/10 of a millimeter seems not warranted; same on line 21 [Regine Hock, United States of America]	Noted. Mass loss rates are now given in Gt, and based on the IMBIE assessment.
67131	6	14	6	14	ice discharge may be better than ice flow, since the flow itself is not a mass loss component [Regine Hock, United States of America]	Taken into account. The revised text explicitly mentions ice discharge instead of flow.
103763	6	14	6	14	sea level rise -> GMSL [Philippe Tulkens, Belgium]	Accepted. Acronym GMSL introduced in the paragraph and used afterwards.
84861	6	14	6	14	The number 0.0106 ± 0.0009 m likely stems from a preprint of the IMBIE paper fro Greenland and has been updated to 10.8 ± 0.9 mm (0.0108 ± 0.0009 m) in the final published and online version of the IMBIE paper. [Jan Wuite, Austria]	Taken into account. The observed mass changes are now given in Gt.
80801	6	14	6	14	sea level rise -> GMSL [Louise Sandberg Sørensen, Denmark]	Accepted (see response to comment #103763)
97955	6	14	6	14	..surface melt slightly exceeding losses through ice flow. Could this be better worded? So mass loss due to melt of the (grounded) icesheet, vs calving or mass losses at the ocean/icesheet interface or icesheet periphery that are not due directly to surface melt processes [Paul Durack, United States of America]	Taken into account. Wording revised and clarified (e.g. ice discharge instead of ice flow)
15783	6	14	6	14	Replace "through ice flow" with "through ice discharge from the grounded part" [Olga Sergienko, United States of America]	Taken into account. The revised text explicitly mentions ice discharge of grounded ice instead of flow.
90703	6	14	6	16	Besides snow cover of the NH has been decreasing, and the delay in the onset of snow season, shorter snow cover durations is estimated between –0.7 and –3.9 days per decade, depending on location and time period, but all spring snow cover duration trends estimated are negative, from surface observations (Bulygina et al., 2011). Bulygina, O., P. Y. Groisman, V. N. Razuvaev and N. N. Korshunova, 2011: Changes in snow cover characteristics over Northern Eurasia since 1966. Environmental Research Letters, 6, doi:10.1088/1748-9326/6/4/045204. [Thian Yew Gan, Canada]	Noted. This is a pre-SROCC and pre-AR5 reference that is consistent with the evidence provided.
69625	6	14	6	21	Suggest quoting these values in mm, not metres. [Nicholas Golledge, New Zealand]	Noted. Past mass losses are in Gt, projections remain in m for consistency with the rest of the report.
41025	6	14		23	The use of ice flow vs dynamical loss should be consistent in the executive summary for both GrIS and AIS [TSU WGI, France]	Taken into account. AIS and GIS are now treated consistently in the same paragraphs.
71983	6	14			This is a small error bar! [John Church, Australia]	Noted. Mass loss rates are now given in Gt, and based on the IMBIE assessment.
32137	6	14			"losses through ice flow". Ice flow itself does not cause loss. The loss occurs by ice flow across the grounding line/into the ocean. Consider adding "into the ocean" or using discharge or calving. [Anja Wendt, Germany]	Accepted. The revised text explicitly mentions ice discharge instead of flow.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8979	6	15	6	15	It is not correct to state that Greenland ice sheet mass is only removed from surface melt. Half of the loss is from glacier dynamics and the largest changes down the line will be from glaciers. [Eric Rignot, United States of America]	Noted. The SOD did not pretend that surface melt was the only contribution to mass loss. The revised paragraph states that the mass loss increase is primarily (implying not exclusively) caused by SMB losses.
99131	6	15	6	16	I do not understand how this statement can be made with "high confidence". Surface melt was very unlikely to have been the primary cause of the disintegration of the major ice sheets from 20,000 to 8,000 years ago--movement of ice streams likely carried large amounts of ice to the ocean where it melted. I don't think there is any way that surface melt as calculated by models would support this conclusion during either the rate of loss coming out of the Last Glacial Maximum or of going into the Eemian and so see no justification for the statement--and the observed pace of movement of some of the ice streams suggests that this is a very important factor. [Michael MacCracken, United States of America]	Noted. The ES paragraph explicitly refers to the most recent decades where the available evidence suggests that the dominant cause mass loss increase was surface melt.
79899	6	15	6	17	Here, as in other places later in the document it could be useful to introduce as reference the total increase in GMSL (6.5 (3.9--9.9)? based in Table 9.A.3) or the percentage that GrIS contribution represents in total GMSL increase [Somavilla Raquel, Spain]	Noted. Space in the ES is too limited for this unfortunately. A similar suggestion was declined for seasonal snow cover, for example, so not listing these numbers is consistent with the rest of the ES.
129331	6	15	6	17	[RISK] How can this statement be made with such high confidence given the potential for ice sheet movement, the increasing flows of surface meltwater into moulins that takes heat well down into the ice sheet, and insights available from paleoclimatic analyses that show the ends of glaciations occur far faster than can be explained by just melting of surface water. The documentary "Chasing Ice" shows glaciers retreating far faster than is a result of just the surface energy balance. From 20ka to 8ka, sea level rose 120 METERS from a relatively modest redistribution of solar radiation and an increase in the CO2 concentration from 200 to less than 300 ppm. On average for those 120 centuries, sea level rose an average of about 1 m/century while the global average temperature was going up, on average, only about 1°C every 2000 years. There is no way that just changes in the surface energy balance (more solar in summer, but very cold winter temperatures due to low CO2 concentration) could have caused this much change. On page 5, line 3, it was said that this chapter would consider what is "possible". The statement here seems to rule out what clearly seems possible based on the paleoclimatic record and the increasing flow of ice sheets that is occurring. This finding is not justified: The range of values here shows far too small a range of possibilities. And, then for West Antarctic below, the dominant loss seems to be from ice discharge, so what is said with high confidence is contradicted by the West Antarctic situation. Also note that on page 5, line 5, it says that the chapter will provide "long-term commitments" so give projections out beyond 2100 -- and given the Eemian was only 1°C or so warmer than preindustrial, explain how sea level rise was up 4-8 meters with just that 1°C change in global average temperature. [Trigg Talley, United States of America]	Taken into account. The increased dominance of SMB losses over dynamical losses is a consequence of the current and future geometry of the ice sheet. The glacial ice sheet configuration has been very different from today's, making comparisons difficult. Once the ice sheet is small enough to be cut off from the ocean, losses will necessarily essentially come from surface melt. The text is clarified to better convey this point.
106625	6	16	6	16	0.03 to 0.12 -> 0.03-0.12 (for consistency) [Kevin Bulthuis, United States of America]	Accepted. Implemented consistently.
96853	6	16	6	17	The figures for SLR are lower than in SROCC (Chapter 4, Page 33, Table 4.2) and partially lower than in AR5. A statement to this discrepancy is missing. [Nicole Wilke, Germany]	Noted. This is discussed in the main chapter. Place for discussion is too limited in the ES.
15537	6	16	6	17	Re: the Greenland ice sheet will contribute 0.07 (0.03 to 0.12) m and 0.13 (0.09-0.19) m to GMSL by 2100 under SSP1-2.6 and SSP5-8.5 respectively. Apparently, these projections are adopted from the emulated ISMIP6 results which are lower than the AR5 projections by about 13% (P.93, lines 22-25). As stated in P.53, lines 28-32: The SROCC assessed the projected GrIS contribution to GMSL as unchanged from AR5 based on the good agreement with more recent modelling results with multiple climate and ice sheet models and taking into account a recent expert elicitation study. The SROCC was released only about six months before this AR6 WGI SOD review. If there are any significant developments in the projection methodologies which justify the replacement/retirement of the SROCC projection methodology, elaboration should be given clearly in the main text in order to support the proposed statement in the Executive Summary. However, discussion in this aspect in the main text is clearly lacking. It is intriguing that the direct ISMIP6 results are much higher than (almost double) the emulated ISMIP6 results according to Table 9.1 and there is no dicussion on why the direct ISMIP6 results were not taken into account. Please review and consider revision. [SAI MING LEE, China]	Taken into account. The results from the emulator are assessed in the chapter. Changes relative to SROCC, in particular due to results from ISMIP6 and LARMIP (as described in box 9.3 of FGD) are explained in detail.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106677	6	16	6	17	0.07 (0.03 to 0.12) and 0.13 (0.09-0.19): It would be interesting to specific what the values 0.07 and 0.13 represent (mean or median values) and what the uncertainty ranges (0.03 to 0.12 and 0.09-0.19) are (likely range, very likely range, 33%-66% probability interval,...) [Kevin Bulthuis, United States of America]	Taken into account. The revised ES now includes these numbers as likely range and more detailed explanation for the ranges is described in text
61675	6	16	6	17	You chose to provide the emulated numbers from Edwards et al. (submitted) in the executive summary. I tried to find out in the relevant section (Section 9.6.3.2.1, page 93 L3-29) what was the reason behind this choice, instead of using the direct ISMIP6 results from Goelzer et al. (2020) but couldn't find a justification. You say in page 93 L18-20 that 'The primary sea-level projection results presented here, in Figures 9.29 to 9.31 and Tables 9.6 and 9.7, are based upon the emulated ISMIP6 results, driven by the FAIR temperature projections.' But why is that the case? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The results from the emulator are assessed in the chapter. However, a more detailed description of the choices of ice sheet projections is given in 9.4.1.2
61677	6	16	6	17	The numbers coming from emulated ISMIP6 results are the ones given in Section 9.6.3.2.1 (page 93 L22-29) but they differ from the numbers in Section 9.4.1.2 (page 53 L46-48), while I think this is the same study (Edwards et al., submitted). Am I missing something? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The results from the emulator are assessed in the chapter. However, clarity in any differences between the projected ice sheet contributions in 9.4 and those in 9.6 is made clearer in the text.
101679	6	17	6	17	Note that the abbreviation "GMSL" is not defined in the upper part of the summary (before line 15 on page 7). It should be spelled out in the summary for the non-expert to understand its meaning. (applies to the entire summary) [IAPSO ECS group review, United States of America]	Accepted. This is done now.
103765	6	17	6	17	SSP5-8.5 respectively -> SSP5-8.5, respectively [Philippe Tulkens, Belgium]	Taken into account. Text revised, the word "respectively" does not occur any more in this place.
80799	6	17	6	17	SSP5-8.5 respectively -> SSP5-8.5, respectively [Louise Sandberg Sørensen, Denmark]	Taken into account. Text revised, the word "respectively" does not occur any more in this place.
91083	6	17	6	17	I presume that it is a chapter-wide convention to only quote the likey range in these summary paragraphs. If that's the case then this comment is probably irrelevant. However, for non expert readers, many will not dig into the detail and even those that do will find it difficult to see (without it being pointed out) that the very likely range is far from Guassian and the median is closer to the 5% than it is to the 95%. This is far less apparent if limited to the 17-83% range. May be a sentence could be added to highlight this fact that is true for the AIS and GrIS. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The work likely indicates that this is the likely range, thus the 17-83% range.
3051	6	17	6	17	The acronym "GMSL" should be defined here (it is only done so at P.7 L15). [Daniel Farinotti, Switzerland]	Accepted. This is done now.
33407	6	17			Describe abbreviation when first cited: GMSL. [Guiomar Rotllant, Spain]	Accepted. This is done now.
106671	6	19	6	20	and LIKELY that it will loss by the end of the century under all emissions scenarios: I am in general quite skeptical about the assessment of uncertainty using words like very likely and likely. Here the sentence is quite strong in my opinion and our certainty about the mass loss from the Antarctic ice sheet by the end of the century is definitely dependent on the emission scenarios. I have got no idea on how this assessment of LIKELY mass loss has been determined. Is it also valid for an emission scenario like RCP 2.6 for which numerical models also predict a mass gain by the end of the century? [Kevin Bulthuis, United States of America]	Noted. The use of the uncertainty language is calibrated across the entire IPCC and we have no means of adapting to our own preferences, or to those of our readers. The likelihood used as a basis for this assessment is obtained as explained in the main chapter body and indeed encompasses all scenarios.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129333	6	19	6	28	[RISK] With earlier IPCC assessments having had Antarctica as a hole gaining mass, now proven wrong, it is hard to really provide an indication of how much loss could occur. There is no consideration here, apparently, of the potential of ice sheet collapse (as has happened for some ice streams on the Peninsula) and would suggest some of the larger ice shelves are very vulnerable, and their loss could lead to much faster ice stream flow. What is clear from paleo studies is that ice sheets, even those on land, can collapse rapidly -- much more rapidly than they can build up. And the LGM collapse occurred at a time when the CO2 concentration was less than 300 ppm, so quite strong winter cooling was occurring. For the present century, the CO2 concentration will be over 400 ppm and going higher, which will limit wintertime cooling, so in the summer, one gets higher temperatures and melting and in the winter not nearly as much cooling. It is not credible to think that the breakup will be occurring slowly when global warming is occurring at something like 40X the average rate during the LGM deglaciation and SL rise then was 1 meter per century. How can it be said that what is concluded here covers the range of what is possible (which the business/banking community, etc., might consider having a 1% likelihood) that is promised on page 5, line 3. This paragraph does not cover the long-term changes and commitment promised on page 5, line 5. The papers showing a significant dependence on assumptions regarding the shear strength of ice shelves (and which can explain paleo varations in Antarctic ice extent) make clear there is a very significant uncertainty and there is no consideration of this in the results. [Trigg Talley, United States of America]	Taken into account. The revised ES paragraph clearly mentions to potential for higher outcomes.
18759	6	19	6	28	For the sake of completeness, a statement about the fate of East Antarctic icesheet in the 21st century may be made here. [Govindasamy Bala, India]	Taken into account. For the sake of concision, we will not indicate separate contributions from West and East Antarctica here.
112463	6	19	6	28	It might be insightful to indicate that there is a tipping point for the west antarctic Ice Sheet due to most of the ice-shelves being grounded below current sea-level. [Pedro LLanillo del Rio, Germany]	Noted. We are not sure that there is indeed "a" tipping point for the entire West Antarctic Ice Sheet, so we prefer not to mention this.
34933	6	19	6	28	The SOD claims that the Antarctic has lost ice mass since the early 1990s. See rebuttal comment #9 above. [Jim O'Brien, Ireland]	Rejected. The measurements are unambiguous. Multiple lines of evidence (different methods) all suggest mass loss since the 1990s. Ongoing grounding line retreat is inconsistent with decadal stability.
111733	6	19	6	28	Many readers will be looking for guidance from Chapter 9 on plausible storylines for accelerated WAIS mass loss and possible impacts on sea level. I understand that deep uncertainty means you can't assign probabilities to outcomes, but it is still possible to provide useful information to manage coastal risk (e.g. through increased resilience). For example, storylines on plausible extreme rates and timescales of sea level rise. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is treated in the chapter (Box 9.4 and section 9.6), but the level of detail provided in the ES cannot be increased to the requested level.
15781	6	20	6	20	In "The grounded Antarctic Ice Sheet…" add 'part' or 'portion' after 'grounded' [Olga Sergienko, United States of America]	Accepted. Implemented "grounded part".
15539	6	20	6	21	The mass loss of the Antarctic Ice Sheet associated with 0.0069 m of sea level rise is huge, i.e. about 2500 Gt. It is suggested to be reflected in the Executive Summary for the readers to appreciate the significance. [SAI MING LEE, China]	Accepted. The current mass loss is indicated in Gt now.
79901	6	20	6	23	As in the previos comment, it would be useful to have the reference value of GMSL increase to know more intuitively how much Antarctic Ice sheet contributes to GMSL rise. [Somavilla Raquel, Spain]	Noted. The current and projected sea level change rates are indicated in the ES statements referring to section 9.6, further down.
49959	6	21	6	21	to *global mean* sea-level rise [Daniel Gilford, United States of America]	Accepted. We now write global mean sea level, and use GMSL afterwards.
103767	6	21	6	21	sea level rise -> GMSL [Philippe Tulkens, Belgium]	Accepted. GMSL used now.
84863	6	21	6	21	The number 0.0069 ± 0.0014 m for 1992-2018 is provided, which probably comes from the IMBIE paper for Antarctica. However, the online version of this paper mentions 7.6 ± 3.9 mm for 1992-2017. [Jan Wuite, Austria]	Taken into account. Mass loss rates now in Gt for the entire ice sheet.
80803	6	21	6	21	sea level rise -> GMSL [Louise Sandberg Sørensen, Denmark]	Accepted. GMSL used now.
106673	6	21	6	21	0.069+-0.0014 m: What does 0.060 represent (mean or median value) and what does the uncertainty range represent? [Kevin Bulthuis, United States of America]	Taken into account. Mass loss rates now in Gt for the entire ice sheet. It is the total with likely ranges, as the text indicates.
65847	6	21	13	21	Suggest editorial change to: " A marginal improvement … was noted …" [Kushla Munro, Australia]	Editorial. Text changed in section 9.2.1.1 where this comment belongs to (wrong page number given by reviewer)
71985	6	21			This is a small error bar! [John Church, Australia]	Noted. Mass loss rates are now given in Gt, and based on the IMBIE assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99689	6	22	6	22	"over" to "from" [Peter Clark, United States of America]	Accepted. Implemented as suggested where appropriate.
149	6	22	6	22	"over the last decades" - specify how manay decades. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Exact periods are now given.
62137	6	22	6	22	Clarify if this is referring to 1992-2018 period mentioned above, or to a different period [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Exact periods for Greenland and Antarctica are now given.
62139	6	22	6	24	Please add a statement about the East Antarctic Ice Sheet [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The revised ES makes no statements about parts of the ice sheets in order to be as concise as possible.
103769	6	23	6	23	Is ice shelf basal melting considered a dynamical loss? Or does it drive the dynamical loss? [Philippe Tulkens, Belgium]	Taken into account. As we refer to the sea-level contribution in the corresponding place in the revised ES, we refer to ice discharge from the grounded part here, and consider, as explained in the main chapter text, that ice shelf basal melt is a driver of this increased mass loss.
106627	6	23	6	23	Is it really entirely true to state that ice-shelf basal melting dominates current dynamical losses? I would argue that so far there is also a not negligible impact of calving (mass losses due to calving are approximately equal to mass losses due to ice-shelf basal melting). But I agree that in the future, ice-shelf basal melting will (certainly) dominate mass losses in West Antarctica. This sentence would benefit from further clarifications. [Kevin Bulthuis, United States of America]	Taken into account. We write now that the ice sheet mass loss is driven by ice shelf mass loss. The detailed discussion is given in the main chapter text.
80805	6	23	6	23	Is ice shelf basal melting considered a dynamical loss? Or does it drive the dynamical loss? [Louise Sandberg Sørensen, Denmark]	See C103769: Taken into account. As we refer to the sea-level contribution in the corresponding place in the revised ES, we refer to ice discharge from the grounded part here, and consider, as explained in the main chapter text, that ice shelf basal melt is a driver of this increased mass loss.
129335	6	23	6	24	[CONFIDENCE] Can the authors review their level of confidence associated with the Antarctica Peninsula and West Antarctic Ice Sheet or clarify the statement in summary? Page 59 of Chapter 9 associated high confidence with mass loss of AIS and ice discharge rates in these areas: "There is high confidence that mass loss of the AIS has been dominated by high ice discharge rates over the West Antarctic Ice Sheet (WAIS) and the Antarctic Peninsula." [Trigg Talley, United States of America]	Not applicable. The revised ES makes no statements about parts of the ice sheets in order to be as concise as possible.
15785	6	23	6	24	The sentence " Ice shelf basal melting dominates current dynamical losses and will remain the dominant driver of West Antarctic mass losses" does not make sence in the context of this paragraph. The sentences before and after this one refer to sea level contributions, ice-shelf basal melting does not contribute to sea level rise. [Olga Sergienko, United States of America]	Taken into account. The ES was restructured, with separate paragraphs for current state and projections. This should clarify the issue.
115145	6	23	6	24	This sentence reads that surface melting and hydrofracture of ice shelves are never going to be important. Could a time bound be added for how long into the future there is high confidence about basal melting dominating dynamic loss? Before 2100? [Natalya Gomez, Canada]	Accepted. Centennial time scale explicitly added.
104427	6	23	6	24	SfcMelt-shelves. Ch9, ES: "Ice shelf basal melting dominates current dynamical losses and will remain the dominant driver of West Antarctic mass losses (high confidence)." ....=> I suggest adding a sentence about the important role of surface melt and hydrofracturing in recent ice shelf collapses - this concerns mostly the AP ice shelves, but the importance is expected to increase strongly in the future for all major WAIS ice shelves. There is some inconsistency in Atlas and Ch9 statements about the role of surface melt in the recent changes in ice shelves (Atlas stressing the role of surface melt for the AP ice shelves - need to add a note about basal melt as primary factor for WAIS ice shelves referring to Ch9; and in Ch9 the role of surface melt is mostly referred as highly uncertain, while many studies show its importance in recent AP ice shelf collapses and at larger scale in future projections). [Irina Gorodetskaya, Portugal]	Taken into account. The revised ES text is shortened and does not allow to provide additional detail. However, the revised main chapter text clarifies the role of hydrofracturing.
49961	6	24	6	25	Can you quantify how much it offsets the dynamic loss, either in terms of percentage or total mass? Without context, it's not clear how impactful a 4-8% change in precipitation is. [Daniel Gilford, United States of America]	Noted. This is extremely hard to quantify as there is deep uncertainty concerning the dynamic losses. Note that for the central estimates, we indicate that there is weak scenario difference due to this compensation, indicating that both increase with warming (until 2100, that is).
3053	6	24	6	25	The time of projection ofr the mentioned increase in Antarctic snowfall is unclear and needs to be specified. [Daniel Farinotti, Switzerland]	Accepted. The time scale (until 2100) is now indicated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8981	6	25	6	25	What the summary does not say is that observations do not show any increase in snowfall. So this statement from models should be counterbalanced with the fact that observations do not concur. [Eric Rignot, United States of America]	Noted. The text on current state/evolution and projections is now placed in two different paragraphs. It is true that the observations concerning precipitation increase in the Antarctic are unclear, and this is treated in the main chapter text. A mention of these unclear trends would add confusion - the missing clarity of the observations is to a large part caused by sparse networks, difficulties in observing precipitation in this extreme environment and high interannual variability.
106629	6	25	6	25	dynamic losses -> dynamical losses (for consistency) [Kevin Bulthuis, United States of America]	Taken into account. Sentence reformulated.
96855	6	25	6	26	The SROCC found a scenario-dependency of the AAIS contribution to SLR. Does this finding not hold anymore? Please refer to the SROCC and explain the new finding. [Nicole Wilke, Germany]	Taken into account. The numbers presented in the ES do not show such a clear scenario dependence as found by SROCC. The revised text (9.4.1.2) expands on why scenario dependence is not as clear as for the SROCC.
89385	6	25	6	27	"[...] with little scenario dependence [...]" seems to contradict part of the literature and statements in Chapter 9 later on (see for instance also Table 9.2). In particular, even within the ISMIP6 and LARMIP-2 intercomparisons it has been shown that there is in fact a strong scenario dependence when considering simulations with higher melt sensitivities based on oceanographic studies. [Ricarda Winkelmann, Germany]	Noted. However, if viewed across the available ensemble of opportunity, this is not the case. The detailed discussion is reserved to the main chapter.
89387	6	25	6	27	"[...] will contribute 0.12 (0.00-0.26) m to GMSL [...]" -- Does this range include all latest projections based on ISMIP6 and LARMIP-2? In Reese et al. 2020 (https://doi.org/10.5194/tc-2019-330) for instance, the authors give a higher upper limit of 35.9 cm by 2100 when including the ice-sheet's historic trajectory and higher (arguably more realistic) melt sensitivities. Also, a larger spread seems to result from Table 9.2 ? [Ricarda Winkelmann, Germany]	Taken into account. The sea level contribution have been revised in light of new published work. The approach that takes into account both ISMIP6 and LARMIP is now clearer in the main text (9.4.1.2) and includes an historical component (see Box 9.3)
106681	6	25	6	27	I find the whole sentence a bit unclear. The authors state at the beginning of the sentence that the contribution of the Antarctic ice sheet to sea level by the end of the century has little scenario dependence but the claim at the end of the sentence that there is deep uncertainty for the high emissions scenarios. Is there something missing? In the Technical Summary, the end of the sentence is "There is deep uncertainty regarding the Antarctic contribution beyond 2100 linked to potential destabilization of the West Antarctic ice sheet". It would make sense to also state this explanation in the executive summary in Chapter 9. [Kevin Bulthuis, United States of America]	Noted. Scenario independence refers to the likely ranges provided. The deep uncertainty refers to the insufficiently quantified upper tail of the distribution (and indeed is relevant particularly for the high emissions scenarios). So there is no contradiction.
76733	6	25	6	27	“little scenario dependency is likely for the AIS” seems to contradict the “low confidence for the projected melt rates” since melt rates are the drivers of the mass loss for the AIS. Please see also the specific comments on page 62. [Ronja Reese, Germany]	Noted. These are the likely ranges based on the available projections.
61953	6	26	6	26	instead of "little scenario dependence" say something about the scenario like " under the low/current emission scenarios etc. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The reason for the weak dependence is explained in section 9.4.1.2 as we do not have the space in the ES, and the revised ES text now only provides numbers.
15541	6	26	6	26	Re: the Antarctic ice sheet will contribute 0.12 (0.00-0.26) m to GMSL by 2100 with little scenario dependence. Apparantly, the projection is adopted from the emulated ISMIP6 results (P.62, lines 20-23) with an adjustment in the reference period. To support the proposed statement in the Executive Summary, discussions on (i) why the direct ISMIP6 results were not taken into account; (ii) how the emulated ISMIP6 projection results outperform the SROCC projections thus making it justifiable to replace the SROCC projection should be included in the main text. [SAI MING LEE, China]	Taken into account. The emulator results are included in the ES, and how these numbers compare to the direct ISMIP6 simulation is explained in 9.4.1.2. Traceability to SROCC also clearer in the revised version of 9.4.1.2 .
106683	6	26	6	26	0.12 (0.00-0.26): What do these numbers represent (median or mean value, what is the uncertainty range, is this a mean over several emissions scenarios)? These numbers would require clarification. [Kevin Bulthuis, United States of America]	Taken into account. These are the mean values across the scenarios, with likely ranges as indicated by the presence of the word "likely" in the revised text.
38093	6	26	6	26	"with little scenario dependence" is rather ambiguous. [Junhee Lee, Republic of Korea]	Taken into account. These words are no longer in the ES (although the sea level numbers provided do suggest a weak dependence). Furthermore, we hope that the revised text in 9.4.1.2 clarifies this by indicating the reasons for thee weak dependence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34673	6	26	6	27	I'm a little confused by this sentence. It says that Antarctic ice sheet loss will contribute about 0.12 m to GMSL with little scenario dependence, but uncertainty is large for high-emission scenarios. Presumably that implies that some models depict much larger losses for high-emission scenarios, but the mean change is still about 0.12 m? [Russell Vose, United States of America]	Noted. Scenario independence refers to the likely ranges provided. The deep uncertainty refers to the insufficiently quantified upper tail of the distribution (and indeed is relevant for the high emissions scenarios, as stated in the revised text). So there is no contradiction.
8983	6	27	6	27	Incomplete information again. Deep uncertainty from what? It should say here that there is deep uncertainty about the impact of glacier dynamics on projections and that the conjectures is that the projections are underestimates. [Eric Rignot, United States of America]	Accepted. The reasons for the deep uncertainty are expressed clearly in the revised version.
96857	6	27	6	27	Please add: …high emissions scenarios beyond 2100 linked to the potential destabilization of the West Antarctic Ice Sheet. (TS-39, line 32-33). [Nicole Wilke, Germany]	Taken into account. We indicate now that this is the case on the horizon 2100 and beyond. But we do not provide geographical detail, in part because destabilization of parts of East Antarctica cannot be excluded.
99461	6	27	6	27	Consider rephrasing thus "… dependence except possibly for the highest emissions scenario where there is deep uncertainty … contribution." [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We indicate that this is the case for the high emission scenarios.
115147	6	27	6	27	Could there be a statement added about deep uncertainty beyond 2100? [Natalya Gomez, Canada]	Accepted. We indicate now that this is the case on the horizon 2100 and beyond.
3055	6	27	6	27	The meaning of "deep uncertainty" is unclear. If it is standard IPCC wording, it should be italics. [Daniel Farinotti, Switzerland]	Rejected. Not part of the IPCC calibrated uncertainty language. It is a standard expression defined in the glossary.
55069	6	27	6	29	Please clarify if the deep uncertainty related to the Antarctic ice sheet contribution to GMSL applies after 2100 or before 2100. Being explicit about this important conclusion would be helpful. [Nancy Hamzawi, Canada]	Accepted. We indicate now that this is the case on the horizon 2100 and beyond.
71987	6	27			Suggest add something like in the latter part of the 21st century and beyond. [John Church, Australia]	Accepted. We indicate now that this is the case on the horizon 2100 and beyond.
30727	6	29	6	29	I think it is better just to present the range rathern than putting a specific value "(1.2 [0.9-1.7] mm yr-1)". I found in many other place of the text showing this kind of value. [Iskhaq Iskandar, Indonesia]	Noted. Unclear what page this refers to. There is no text on page 6, line 31.
44095	6	30			Why do the authors single out European glaciers while a huge part of the most vulnerable global population (mountainous LDCs) is affected by glacier changes in much larger regions, like the Himalayas. Please include more regional information so that particularly vulnerable areas are captured adequately. [Lamin Mai Touray, Gambia]	Accepted, text revised
90707	6	31	6	37	The global mean SLR is estimated at 3.1 ± 0.3 mm yr−1 and acceleration of 0.1 mm yr−2 over 1993-present (Nerem et al., 2018; Legeais et al., 2018). Ocean thermal expansion, glaciers, Greenland and Antarctica respectively contribute 42 %, 21 %, 15 % and 8 % to the global mean sea level over the 1993–present period (Cazenave et al., 2018). However, Bamber et al. (2018) estimated a more modest global mean SLR of 1.76 ± 0.11 mm yr−1 over 2007-2011 and 1.85 ± 0.13 mm yr−1 over 2012-2016, respectively. Nerem et al., 2018, Climate Change Driven Accelerated Sea Level Rise Detected In The Altimeter Era, Proc. Natl. Acad. Sci. USA, 115, 2022–2025, https://doi.org/10.1073/pnas.1717312115, 2018 Cazenave et a., 2018, Global sea level budget 1993-present, Earth System Science Data, 10(3), http://doi.org/10.5194/essd-10-1551-2018 Bamber, J. L., R. M. Westaway, B. Marzeion and B. Wouters, 2018: The land ice contribution to sea level during the satellite era, Environmental Research Letters, 13 (6), 063008, doi:10.1088/1748-9326/aac2f0. Legeais et al., 2018, An improved and homogeneous altimeter sea level record from the ESA Climate Change Initiative, Earth Syst. Sci. Data, 10, 281–301, https://doi.org/10.5194/essd-10-281-2018 [Thian Yew Gan, Canada]	Noted. These are important papers. But it is unclear which part of the text this comment refers to. It does not seem to refer to Page 7, lines 31-37, although this is a paragraph referring to sea level (but regional sea level). As these papers are taken into account in the main chapter text, we think that there is no issue to address.
67133	6	32	6	32	do you mean: and projected to lose mass under all emission scenarios [Regine Hock, United States of America]	accepted, text revised
62285	6	32	6	32	I suggest replacing "glaciers will lose mass" with "global glacier mass loss will continue" or something similar, to make it clear that this is referring to the global mass of ice in glaciers rather than every individual glacier. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
62065	6	32	6	32	Does "under future warming" mean under all modelled RCP scenarios? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65839	6	32	6	33	Suggest changing to: "Current glacier retreat is global since 1850 and under future warming glaciers will almost certainly lose mass .." for improved clarity. [Kushla Munro, Australia]	accepted, text revised
61503	6	32	6	33	Shouldn't the confidence statement used here rather be a likelihood statement if the confidence is very high? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text revised
62141	6	32	6	33	Please add to this statement to clarify until what time in the "future" the glaciers are predicted to continue losing mass (e.g. until at least 2100, as suggested below) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text revised and now includes "emission scenarios"
61955	6	32	6	43	the summary says 2 different things, one is "glaciers will diminish even if the climate stabilizes" but also "glaciers will be lost under continued warming". Sounds contradictory, needs to be made more clear for the summary. Also consider that the first sentence has very high confidence, the second one high confidence. I would suggest to delete the first sentence [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised and clarified
26485	6	32	6	43	In the summaries about the Greenland and Antarctic Ice Sheets, past, present and future sea level contributions are linked to changes in surface melt and ice dynamics. This distinction between dynamic and surface melt contributions to mass loss is currently not made for glaciers. Would it be possible to include this? [Ward van Pelt, Sweden]	noted, currently the global models used for projections do not provide this information
34935	6	32	6	43	Glaciers have grown and ebbed for centuries; five major glaciers in Greenland are recovering in the last three years. Other melting glaciers have revealed prior growth preserved for centuries underneath. See general comment #10 above. [Jim O'Brien, Ireland]	noted, global trends are reported
97957	6	32	6	43	I wonder whether the quantified statement 0.025+-0.018m should be referenced back to earlier estimates, RATES since SROCC/AR5? [Paul Durack, United States of America]	noted, updated rates from new post-SROCC estimate sated
83621	6	32			P9-6 line 32 says “Glaciers 31 32 Current glacier retreat is global and unprecedented since 1850 and under future warming glaciers will 33 lose mass (very high confidence).” As discussed in previous comments this statement ignores history, ie 1850 end of LIA was anomalous in terms of cold temperature and high area of ice cover. Glacier retreat now is not “unprecedented”, compare with Great Aletsch retreats of 1700CE and 1380 CE and 700CE, which also are largely coincident in time with retreats of Arctic ice, and warming spells in China agricultural records. (see figure inserted alongside my previous comment) [michael asten, Australia]	accepted, text revised, text meant to say that since 1850 the retreat is unprecedented
49963	6	33	6	33	to *global mean* sea-level rise [Daniel Gilford, United States of America]	accepted, text revised
103771	6	33	6	33	sea level rise -> GMSL [Philippe Tulkens, Belgium]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
80807	6	33	6	33	sea level rise -> GMSL [Louise Sandberg Sørensen, Denmark]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
112465	6	33	6	34	I would recommend using the same time period to indicate Glaciers' contribution to sea-level rise as the period used in the previous section for the ice-sheets. This would allow the reader to better ponder the relative contribution of glaciers to sea level rise. [Pedro LLanillo del Rio, Germany]	accepted, periods are coordinated between sections
15543	6	33	6	34	It is suggested to include the huge mass loss of glaicers associated with the 0.025 m of sea level rise (between 1971 and 2016) for the readers to appreciate the significance. [SAI MING LEE, China]	accepted, proportional mass loss included
61957	6	34	6	34	confidence level of sentence ending with to seas level rise between 1971 and 2016 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text revised
129337	6	34	6	35	Researchers now have GRACE-FO data over 2018, so these trends can be extended to cover 2018. That makes all terms easier to compare to each other. [Trigg Talley, United States of America]	noted, periods are coordinated between SLR contributors and for glaciers extend to 2019
62063	6	34	6	35	…mass loss since the last decades of the 20th century… Can this time frame be discussed with higher specificity (i.e. put the decade range here instead of 'since the last decades of') [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, period specified
69569	6	34	6	35	"Global glacier mass loss since the last decades of the 20th century cannot be explained without human induced warming (high confidence)." I presume this statement is based on the Marzeion et al. (2014) study in Science. I did not see that study explicitly discussedin the sections are reader is pointed to (9.5.1.1; 9.5.1.3; 9.6.3.2). As I note is my comments above on Chapter 3, I have significant concerns about the Marzeion et al (2014) study [gerard Roe, United States of America]	noted, section revised, anthropogenic influence statement is in Ch3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3279	6	34	6	35	Similar to my comment above: we now have GRACE-FO data over 2018, so these trends can be extended to cover 2018. That makes all terms easier to compare to each other. [Thomas Frederikse, United States of America]	noted, periods are coordinated between SLR contributors
65849	6	34	13	34	Suggest editorial change to: "... biases ....remain ..." [Kushla Munro, Australia]	editorial, text is revised
61961	6	35	6	35	Change "Globally, glaciers are not in equilibrium" to "on a global scale, glaciers…." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
61965	6	35	6	35	replace human with anthropogenic [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
65841	6	35	6	36	Suggest changing to: "Globally, glaciers are not in equilibrium with current climate and are likely to diminish ..." [Kushla Munro, Australia]	rejected, confidence statement not "likely"
61967	6	36	6	36	I can't find something about "glaciers will diminish even if climate stabilizes" in the projections or in the Section. Therefore, I don't think it should be in the summary [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	rejected, committed mass loss is discussed in first paragraph of projection section 9.5.1.3, text revised to clarify point
3369	6	36	6	37	low resolution global climate model (GCM) forcing': by specifying the GCM and the low resolution, you seem to suggest that this would be solved when working with a RCM at a higher resolution. However, even with high-resolution RCMs (which are often used for regional glacier projections), the uncertainty would remain very large (e.g. not being able to solve local precipitation, wind processes,...etc.). May therefore be worth reformulating to 'low resolution forcing from climate models' [Harry Zekollari, Belgium]	not applicable. Text removed
2869	6	36	6	38	I would rephrase as: "The magnitude and timing of future mass loss remains uncertain due to differences in climate scenarios, low resolution global climate model (GCM) and regional climate model (RCM) forcing, uncertainties in ice thickness distribution, glaciological models limitations in representing some processes (e.g. debris-cover impact), and limited observations for calibration (medium confidence)" [Antoine RABATEL, France]	not applicable, text removed
67135	6	36	6	41	better turning around: first the numbers, then the sentence about uncertainties (although I don't think this is adequate for the Ex.Sum) [Regine Hock, United States of America]	taken into account, text revised
99691	6	37	6	37	should this be "is dependent on emission scenario"? [Peter Clark, United States of America]	accepted, text revised
3057	6	37	6	37	The double "the double "uncertain / uncertainty" needs rewording. [Daniel Farinotti, Switzerland]	taken into account, text revised
3059	6	37	6	37	I'm not sure whether the literature offers much evidence for an increase in GCM resolution contributing to "more certain" projections of large-scale future glacier mass loss. The increased resolution might do so on regional to local scales but the sentence seems to refer to the global scale. [Daniel Farinotti, Switzerland]	taken into account, text revised
20161	6	38	6	38	Comma after "warming" [philippe waldteufel, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
3061	6	38	6	38	It is unclear which "calibration" is meant here. An explanation is required. [Daniel Farinotti, Switzerland]	not applicable, text removed
769	6	38	6	39	“Under continued warming glaciers will...” might need a comma “Under continued warming, glaciers will...” [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
55077	6	38	6	39	"Under continued warming glaciers will be lost globally..." may convey the false message that all glaciers will disappear from the face of the Earth, which is not substantiated by the chapter's contents. This could possibly be reworded as "Under continued warming, glaciers will continue to lose mass globally...", as this would not imply that glaciers will totally disappear. [Nancy Hamzawi, Canada]	accepted, text revised
2045	6	38	6	39	The statement that glaciers will be "lost" globally should not be made without clearly mentioning a time horizon. To my knowledge, the ultimate loss of all (!) glaciers (even the large Arctic ice caps) has never been closely investigated. This is due to the far time horizon (after 2100) and the high uncertainties in the climate projections. [Matthias Huss, Switzerland]	taken into account, text revised
61963	6	38	6	40	Move the sentence "under continued warming… glaciers by 2100 (high confidence)" in front of the sentence "the magnitude and timing of future warming…." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text revised
2871	6	38	6	40	I would rephrase as: "Under continued warming in the next centuries, glaciers could be lost globally. By 2100, regions with small glaciers, such as the European Alps, Caucassus, part of the HMA and low latitude mountains will lose most or all glaciers (high confidence)." [Antoine RABATEL, France]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99135	6	38	6	40	Okay, but the total potential contribution from melting glaciers is quite limited and so even though uncertain, the loss of glacier mass is likely more an issue for those relying on glacial melt for water resources than thos along coastlines concerned about sea level rise. The uncertainties and risks associated with ice sheet mass are far greater in terms of potential sea level rise in both the near- and long-terms, and so not covering this in the projections out to 2100 seems to be irresponsible--there is a critical need to be warning of the catastrophic amounts of sea level rise that could be triggered by ongoing warming. [Michael MacCracken, United States of America]	taken into account, the glaciers outside Greenland and Antarctica contributed more to SLR in last decade and the contribution of G and A is include, as well as other contributors in the SLR projection of this chapter
96859	6	38	6	40	Stating that "glaciers will be lost globally" without indicating a time scale nor a magnitude of the "continued warming" is somewhat misleading. Also the following statement on regions with small glaciers should make a point about different scenarios. [Nicole Wilke, Germany]	accepted, text revised
26487	6	38	6	40	The statement "Glaciers will be lost globally" is rather general and could potentially be made more specific by indicating what fraction of glacier volume or area would be lost. [Ward van Pelt, Sweden]	accepted, text revised
67137	6	39	6	39	glaciers will be lost globally'. This statements is misleading since it can be read that all glaciers globally will be lost, rather than just some. [Regine Hock, United States of America]	taken into account, text revised
99693	6	39	6	39	this implies they will be lost entirely - need to be more specific [Peter Clark, United States of America]	taken into account, text revised
2047	6	39	6	40	Even under the most extreme scenarios (RCP8.5) I think there is little evidence that glaciers in the European Alps and the Low Latitutides will be completely (!) lost by 2100. Although all current global and regional glacier models assume that in these regions 90% or more of the ice volume is going to be lost, it is highly likely that some ice at very high elevation will remain. The statement should be revised accordinly, as this is a statement with a far potential reach. [Matthias Huss, Switzerland]	accepted, text revised
3371	6	40	6	40	will lose most or all glaciers by 2100': not clear in most cases when a glacier stops existing: i.e. as the threshold for size is arbitrary, you could also say that a lot of very small glaciers / ice patches remain. Could avoid this by refering to the glacier volume: e.g. 'will lose close to all their present-day glacier volume by 2100' [Harry Zekollari, Belgium]	accepted, text revised
67139	6	40	6	40	all glaciers'. Don't think there is high confidence for that. Small remnant glacier will likely survive in sheltered cirques, processes not included by large-scale glacier models. [Regine Hock, United States of America]	accepted, text revised
79903	6	40	6	41	Idem as comments 2 and 3 [Somavilla Raquel, Spain]	not clear what comment 2 and 3 are so, unforntunately we cannot respond to this comment.
15545	6	40	6	41	It seems that the projections refer to P.92, lines 38-41: emulated GlacierMIP Phase 2 results. However, there is no discussion in the main text to support why the emulated results instead of the original GlacierMIP Phase 2 results (P.69, Table 9.3) should be adopted. Further discussions on the improvement of the AR6 projection methodology over AR5 should be included in order to justify the replacement of AR5 projection methodology by AR6 one. Also, the projections shown in Table 9.3 are RCP2.6 and RCP8.5, not SSP1-2.6 and SSP5-8.5. [SAI MING LEE, China]	noted, emulator results are used to proved GlacierMIP results in SSP scenarios, this is now explained in box 9.3 and text revised to clarify
100143	6	47	6	47	change "globally permafrost" to "global permafrost" [Carlye Peterson, United States of America]	Noted - not applicable. Sentence revised, not such wording remains.
3063	6	47	6	48	The sentence suggests that there "global" permafrost temperature measurements do exist. In light of the paucity of available data, that seems somewhat misleading [Daniel Farinotti, Switzerland]	Taken into account. Reformulated as "Permafrost warmed globally".
61973	6	47	6	51	add a sentence that there is medium confidence that human influence is the dominant cause for pan-Arctic permafrost changes [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Because of length constraints, we prefer not to mention this.
269	6	47	6	52	No details about mountain permafrost; if . Maybe just delete it? [THOMAS Wagner, United States of America]	Noted. Mountain permafrost has been extensively treated in the SROCC and the SROCC assessment that it has been lost is appropriate, we think, even if not much new detail is given in this report.
51497	6	47	6	52	It would be useful to signpost readers to the relevant sections in Chapter 5 (and briefly acknowledge in the text) on the projections of carbon release from permafrost thaw - currently this is not mentioned in Chapter 9's Executive Summary (but is in underlying section 9.5). While this is clearly a key topic for Chapter 5 it seems strange to mention permafrost warming and projections of volume loss in the key messages of Chapter 9, without mentioning this climate system feedback, which is has major policy implications. Suggest that reference to this could be added here, or under the later 'ocean, cryosphere and sea level change in terms of the level of global warming' section. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reader is referred to Chapter 5 in the main text. Referrals to other chapters in the ES are unusual.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129339	6	47	6	52	[CONFIDENCE] Consider the high confidence statement in the SROCC stating that permafrost temperatures have increased to record high levels. This is important for the AR6 to consider. SPM of SROCC states: "Permafrost temperatures have increased to record high levels (1980s-present) (very high confidence) including the recent increase by 0.29± 0.12°C from 2007 to 2016 averaged across polar and high mountain regions globally." [Trigg Talley, United States of America]	Rejected. This statement is repeated in the main chapter text, but as we give another permafrost temperature statement here in the ES, we think that an additional temperature statement would make the ES paragraph too long.
129341	6	47	6	52	[CONFIDENCE] Include statement for permafrost that it is virtually certain that near surface permafrost extent will shrink. For example, on page 77, the authors include the statement: "In summary, based on high agreement across CMIP6 and older model projections (see AR5), fundamental process understanding, and paleoclimate evidence, it appears virtually certain that near-surface permafrost extent will shrink as global climate warms." [Trigg Talley, United States of America]	Rejected. We have a statement giving a number that rather clearly shows that an increase of permafrost with GSAT is extremely unlikely. Unfortunately, because of length constraints, it does not appear possible for us to include the requested statement in addition to this.
67161	6	48	6	48	SROCC has 0.3 deg/decade and only in some regions. [Regine Hock, United States of America]	Taken into account. The exact number is traceable to an important, well-documented recent paper.
99673	6	49	6	50	You indicate that permafrost warming is very likely a pan-Arctic phenomenon, but it is safe to extend this to the mountains of the northern Hemisphere, e.g. European Alps, where permafrost has been warming too. In Antarctica the situation is not so clear due to short data series. [Goncalo Vieira, Portugal]	Taken into account. We mention explicitly mountain permafrost in the revised version.
3065	6	49	6	50	The two sentences should be combined as to avoid the double "pan-Arctic phenomenon" [Daniel Farinotti, Switzerland]	Taken into account. Text revised accordingly.
51495	6	49	6	52	It would be useful to reword these sentences on projections of Arctic and mountain permafrost and include the comparison of projected changes under different SSPs, rather than for 4 degrees warming only. This would also be more consistent with the approach used for the majority of other sub-sections/climate system components reported in the executive summary. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The near-surface permafrost reacts sufficiently quickly to allow us to give a temperature sensitivity that is almost independent of the scenario, which we think is preferable (and in any case much shorter) than a list of temperature changes for 4 different scenarios and possibly different moments in time.
61969	6	50	6	50	add confidence of sentence ending with "subject to interannual variations" (medium confidence) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The geographical variability is actually more important. Medium confidence is indicated, though.
24123	6	50	6	50	Active layer thickness is not the only/relevant aspect, because melting of massive ice at the top of permafrost often takes place and causes subsidence rather than changes in active layer thickness. It would therefore be better to write: "Active layer thickness increase and ice loss are ...", Additional References: (1)Shiklomanov, N., Streletskiy, D., Little, J., Nelson, F. (2013): Isotropic thaw subsidence in undisturbed permafrost landscapes. Geophysical Research Letters 40, 6356-6361.(2) Streletskiy, D. A., Shiklomanov, N.I., Little, J.D., Nelson, F.E., Brown, J., Nyland, K.E., & Klene, A.E. (2017): Thaw subsidence in undisturbed tundra landscapes, Barrow, Alaska, 1962– 2015. Permafrost and Periglacial Processes 28, 566-572. doi:10.1002/ppp.1918 [Wilfried Haeberli, Switzerland]	Noted. This is very true and we hope that the main chapter text is clear on this. Nevertheless, it remains clear that there is a widespread ALT increase. We do not state that this is the only phenomenon.
112467	6	50	6	51	pan-artic phenonmenon repeated twice, redundant. [Pedro LLanillo del Rio, Germany]	Taken into account, text revised.
71175	6	50	6	52	Here, but also in other sections of the report the authors talk about permafrost volume in the top 3 meters and how it will change by about 25+/-5%. This may be a misleading statement for several reasons. First, it is not clear what is meant by volume when referring to a depth (3 m). Then, it is also not clear if the authors refer to the ground ice loss in the permafrost, which starts below the top of the permafrost, i.e. under 0.5 - 4 m below ground surface (active layer). Finally, it can also be confusing because ground settles in response to permafrost degradation, which means that the loss in ground ice and permafrost may not be accurately represented when simply looking at the "3 m below surface" because the surface also settles and a lowering of the permafrost table in absolute elevation terms is not actually represented b this statement of permafrost degradation in the top 3 m. If the authors want to make reference to the current conditions, then this must clearly be stated, otherwise the statement is misleading and may lead to confusion when the permafrost table today is compared with future permafrost tables. In summary, thaw settlements (or thaw strains) are ignored in this statement. [Lukas Arenson, Canada]	Taken into account. This is explained in detail in the main chapter text, including the limitations of models, and therefore not misleading. Text of the ES revised to clarify this as much as possible within available space.
112469	6	50	6	55	I would suggest briefly including the idea that thawing permafrost releases methane which enhances global warming (positive feedback and a tipping point). [Pedro LLanillo del Rio, Germany]	Noted. This is a chapter 5 issue and is treated entirely in that chapter.
130613	6	51	6	51	"global air temperature" is not term for this assessment. [Panmao Zhai, China]	Taken into account. We use global surface air temperature now.
3067	6	51	6	51	"will" is probably best replaced with "is estimated to". [Daniel Farinotti, Switzerland]	Taken into account. We prefer to use the word "projected."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18429	6	51	6	51	"if global air temperature remains below 4°C above preindustrial levels". - to edit [Olga Solomina, Russian Federation]	Taken into account. The reworded text should be clearer.
61979	6	51	6	52	This sentence is confusing, because of the double negative. The meaning could be clarified by replacing 'remains below 4C above preindustrial levels' by 'remains within 4C of preindustrial levels'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised, does not use "double negation".
55075	6	51	6	52	Can this statement be reworded as "global permafrost volume in the top 3m wil decrease by about 25±5% per °C if global temperature remains below 4°C above pre-industrial levels for global temperature increases up to 4°C above pre-industrial levels (medium confidence)? [Nancy Hamzawi, Canada]	Taken into account. The reworded text should be clearer.
100145	6	51	6	52	change "volume in the top 3 m" to "volume in the upper 3 m" and change "remains below 4C above" to "remains lower than 4C above..." [Carlye Peterson, United States of America]	Taken into account. The reworded text should be clearer.
129343	6	51	6	52	This type of statement is just not substantively correct. If one loses 25% for each °C and the base is the running amount, this is saying as the temperature goes up, the absolute amount of loss goes down as the warming gets greater and there will always be some left. This makes no sense at all. The higher one goes, the greater will be the loss (though, of course, the amount available above 3 m to thaw will at some point run out). And with longitudinal convergence and not going to the pole, the amount that could thaw will run out. So, what does this statement really mean? And then there is the question of underwater permafrost, etc. Rewrite is needed. [Trigg Talley, United States of America]	Taken into account. Sentence reformulated to indicate that this is relative to the present value.
99137	6	51	6	52	I think the way this is being phrased is confusing. It seems to imply that each degree C I go up, I'll lose 25% of the permafrost that remains so that the absolute amount being lost goes down as wrming goes up. I would not think that is what is meant by the authors, so the wording needs adjustment in that further warming will also be causing deeper loss of permafrost and not just a loss of area. I would think the finding to be conveyed is that there is an accelerating loss with warming such that at 4 C, or perhaps less, virtually no surface permafrost remains. And it woudl be helpful to give an indication for clathrates. [Michael MacCracken, United States of America]	Taken into account. Sentence reformulated to indicate that this is relative to the present value. It's limited to 3°C above the present, so strong non-linearities (due to vanishing permafrost volume)are not yet relevant.
96861	6	51	6	52	It would be interesting to give values for the impact of the decreasing of permafrost (if possible, perhaps in water volume [m³], contribution to SLR [mm] or increasing CO2 [GT]. Please add such information. [Nicole Wilke, Germany]	Rejected. There are no new estimates on the permafrost water content since 2008, so it would not be very appropriate to cite this at this level.
99463	6	51	6	52	Suggest replacing "if global air temperature remains below" with "for global warming up to". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reworded text should be clearer.
65843	6	51	35	37	Suggest the section on sea surface temperature, which states that the tropical oceans are warming faster than other regions since 1950, include appropriate references. Overall, Chapter 9's description of the advancement of knowledge on tropical oceans and the role of climate change is somewhat limited compared to descriptions of observed and projected changes for other regions. The chapter places a stronger emphasis on Southern Ocean, Arctic, and Antarctic regions. The section on Tropical Oceans in Section 9.2.3.3 is only 4 paragraphs - as such we suggest further description given that it is the fastest warming region in the world. In Section 9.2.3.3, for references on the long-term warming trends in the tropical oceans, paleoclimate research could be included to place into context SST trends since the 1950s. In the first paragraph of this section, a number of mechanisms are discussed as the contributing to the long-term warming trend. Suggest providing more information on how those mechanisms have contributed to the long-term warming trend and their relative order of importance. [Kushla Munro, Australia]	Accepted. The tropical oceans are now more widely assessed. Like for all regions of the worlds, the change in temperature, salinity, marine heat wave, SST are discussed in respectively section 9.2.2.1, 9.2.2.2, 9.2.1.1, Box 9.2. In each of these section, description of change in tropical oceans, and associated processes have been carefully considered. In addition, there is a specific section on circulation change in tropical oceans. Circulation change in tropical oceans are by essence strongly coupled to climate modes, which are not covered in chap 9. We nevertheless provide in the section on circulation change in tropical oceans, a summary of where different component of tropical ocean change can be found in the different section of the AR6
61971	6	52	6	52	below 4C (give the absolute temperature) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The whole report on climate change signals as a function of GSAT changes, not absolute GSAT levels.
26359	6	52	6	52	levels. (medium confidence) -> check ponctuation levels(medium confidence) . [María Santolaria-Otín, France]	Accepted. Punctuation corrected.
65845	6	53	12	53	Suggest editorial change to: "though there is only low confidence ...: [Kushla Munro, Australia]	Editorial. Taken into account (belongs to ocean section 9.2.1.1).
116817	6		6		"if climate stabilizes" => "if the level of global warming stabilizes"? [Valerie Masson-Delmotte, France]	accepted, text revised
88237	7	1	7	1	Do you mean snow cover extent? [Sharon Smith, Canada]	Taken into account. Yes. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61981	7	1	7	11	No reference is made to Southern Hemisphere snow cover. However, in Section 9.5.3.3 page 81 line 3-4 it is stated that as global climate continues to warm, and process understanding strongly suggests (high confidence) that this 4 also applies to Southern Hemisphere seasonal snow cover. This confidence should be indicated in the executive summary. This is especially important since little data on SH snow cover is presented. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Revised ES mentions SH snow cover.
112471	7	1	7	11	Snow cover is also decreasing in the Southern Hemisphere, check this recent papers (Cordero et al doi: 10.1038/s41598-019-53486-7; Cortés et al. 2017 doi: https://doi.org/10.1002/2017GL073826; Cortes et al. 2014 doi: 10.1016/j.rse.2013.10.023 .) [Pedro LLanillo del Rio, Germany]	Taken into account. SH snow is mentioned in the revised ES.
3071	7	1	7	11	A few words on snow cover other than in the Northern Hemisphere seem necessary as well. If there is to little data to make any claim, that should be said. [Daniel Farinotti, Switzerland]	Taken into account. SH snow is mentioned in the revised ES.
129345	7	1	8	1	The phrase about permafrost here is inconsistent with what the section on permafrost said. If the loss is to be less than 40% but one loses 25% per °C, is this assuming that global warming will be less than 1.7°C or so, total? Suggest that the idea of stability here seems rather unlikely given the CO2 and/or methane that will be given off. [Trigg Talley, United States of America]	Noted. It is not clear to us what this 40% number refers to, not found in the text.
271	7	3	7	3	This needs to be more specific. Is it maximum snow cover? [THOMAS Wagner, United States of America]	Noted. We write "spring snow cover", so it is not "maximum".
67163	7	3	7	3	which variable? Snow cover is vague [Regine Hock, United States of America]	Accepted. We now use snow cover *extent* systematically.
55079	7	3	7	4	This statement is confusing as it refers to two quite different snow cover metrics in the same phrase - spring SCE (an inegrated value), with snow onset date (a point metric). For clarity I suggest revising the wording e.g. "Northern hemisphere spring snow cover extent has been decreasing since at least 1978 (very high confidence), with evidence that autumn snow cover extent has also decreased (medium confidence)." This is consistent with lines 17-18 on page 9-79. [Nancy Hamzawi, Canada]	Accepted. We only mention snow cover in the revised version.
97959	7	3	7	11	Northern Hemisphere snow cover and mass reflect the same snow store, yet you have "..snow cover.. Decreasing since at least 1978.." and "Northern hemisphere spring snow mass has been descreasing since at least 1981.." This would seem to be inconsistent statements, or at least hard to ascertain a clear message from both [Paul Durack, United States of America]	Noted. They do reflect the same element of the climate system, but observations show that, particularly on short time scales, snow cover extent and maximum mass are not perfectly correlated. In any case, the revise ES paragraph does not mention snow mass, to limit possible confusions.
101681	7	5	7	7	"There is medium confidence that hemispheric spring snow-cover loss trend extends back to the 1920s. Northern hemisphere spring snow mass has been decreasing since at least 1981 and mountain snow cover has decreased since 1950 globally." - A confidence level is given for the statement that hemispheric spring snow-cover loss extends back to the 1920s. The following sentence is phrased as if there is higher certainty that the snow-cover loss has been ongoing since at least the 1980s, but no assessment of confidence level or likelihood is actually made, which makes it unclear how to interpret this phrasing. [IAPSO ECS group review, United States of America]	Noted. The chapter has been revised and no confidence level is given for snow cover changes before 1967.
99465	7	6	7	8	No need to repeat "northern hemisphere". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. As the ES now also mentions Southern Hemisphere snow, we think it is preferable to continue specifying Northern Hemisphere at each occurrence.
26361	7	6	7	8	Northern Hemisphere (both in capitals) [María Santolaria-Otín, France]	Taken into account. NH systematically in capitals.
88239	7	7	7	7	Do you mean mountain snow cover extent? (refer to snow mass in first part of sentence) [Sharon Smith, Canada]	Noted. Mountain snow cover is not mentioned any more, so no confusion is possible any more.
129347	7	7	7	11	So, how long can loss of about 2M square kilometers per °C go on before there is no snow cover? Giving a present amount of snow cover is needed to give context. Also, the metric might better be some sort of area-days of coverage of snow, so km2-days or km2-months and then also provide some measure of the changing intergated mass of snow coverage as snow cover is in many areas a source of water for agriculture, water for irrigation and society, etc. So, the current numbers authors present are indeed numbers, but not really very useful. Consider changing the metric. [Trigg Talley, United States of America]	Taken into account. It is explicitly specified that this is valid until +3°C.
67165	7	8	7	9	which period? [Regine Hock, United States of America]	Noted. Dates are now indicated (1981-2010).
101683	7	9	7	9	Note that the abbreviation "GSAT" is not defined in the summary. It should be spelled out in the summary for the non-expert to understand its meaning. (applies to the entire summary) [IAPSO ECS group review, United States of America]	Accepted. Spelled out.
67167	7	9	7	9	avoid acronyms, esp in ex.summaries. Spell out GSAT [Regine Hock, United States of America]	Accepted. Spelled out.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3069	7	9	7	9	"GSAT" is not defined in this chapter (I understand that all acronyms should be defined in every chapter, as it is very likely that "SROCC" or "GCM" have been defined in previous chapters as well) [Daniel Farinotti, Switzerland]	Accepted. Spelled out.
61977	7	9	7	10	.."from November to May" the main text refers to the transitional season month, therefore spring an autumn (page 81, line 50) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. ES text aligned.
33409	7	9			Describe abbreviation when first cited: GSTA. [Guiomar Rotllant, Spain]	Accepted. Should have be GSAT & is spelled out now.
32405	7	10			put full stop after (high confidence). [Olaf Eisen, Germany]	Taken into account. Punctuation checked.
34937	7	13	7	37	The SOD claims GMSL has risen faster since 1900 than in any century in the last 3 millennia. See rebuttal comment #6 above. [Jim O'Brien, Ireland]	Noted. This result is presented in Section 9.6.2
3073	7	15	7	15	the acronym "GMSL" has been used earlier although it was not defined. [Daniel Farinotti, Switzerland]	Editorial comment accepted.
103773	7	15	7	15	GMSL has been defined and used previously [Philippe Tulkens, Belgium]	Editorial comment accepted.
80809	7	15	7	15	GMSL has been defined and used previously [Louise Sandberg Sørensen, Denmark]	Editorial comment accepted.
129349	7	15	7	22	[PROGRESS] Clarify where the GMSL values come from in the text. In Chapter 9, page 92, it states: "This yields a likely global-mean thermosteric contribution of 0.15 (0.13-0.17) m for SSP1-2.6, 0.19 (0.17-0.22) m for SSP2-4.5, 0.24 (0.23-0.26) m for SSP3-7.0, and 0.29 (0.28-0.31) m for SSP5-8.5 by 2100 (Figure 9.29, Table 9.6)." Also, when looking at page 92, some of these values differ from the SROCC SPM. It would be good to point to what data are being added since the SROCC to explain these changes. The authors may also want to clarify which SSP they are referring to. Also, where did this range come from in the text: "... at an average rate of 0.084 (0.059-0.090) mm yr-2 over ..." [Trigg Talley, United States of America]	This comment is noted. It refers to projected changes in sea-level, captured in following paragraphs, consistent with figures provided. No changes here.
34675	7	15	7	22	It might be easier for the reader if the units were uniform throughout this key message -- e.g., by expressing the trends in meters per decade (for consistency with the overall increase of 0.19 meters in the first paragraph). [Russell Vose, United States of America]	Noted. The ES expressed values with same units as found in main text. A consistency issue would arise if changed.
40991	7	15			the number "0.19" appears only in the executive summary (in the main text we only have the likely range). Is that an issue? [TSU WGI, France]	Noted. Revised Section 9.6.1 presents 1900-2018 GMSL rise value and likely range.
273	7	16	7	16	Does glacier include GRIS and Antarctica? Elsewhere we call them ice sheets. [THOMAS Wagner, United States of America]	Noted. Glacier does not include the ice sheets in this instance. The glaciers are the dominant contribution over period 1900-2018 (Table 9.4).
49965	7	16	7	16	It's very unclear from context what is meant by energy in this sentence. [Daniel Gilford, United States of America]	Taken into account. Revised text addresses comment.
112473	7	16	7	16	Please indicate the percentage due to melt and to thermal expansion. [Pedro LLanillo del Rio, Germany]	Not Applicable. Text revised
88241	7	16	7	16	Is ice sheet melt included here? [Sharon Smith, Canada]	Noted. Yes, is the answer. Directed to CCB9.2.
62143	7	16	7	16	Is this referring to changes in the energy balance that have driven changes in ocean thermal expansion, glacier melt, and GMSL? Consider making a clearer connection between the two, such as: For 1971-2018, all major contributors that drive changes in the energy balance, and the corresponding changes in sea level change, are known and consistent (high confidence). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Revised text addresses comment.
115149	7	16	7	16	What is meant by energy here? Could this be said differently? [Natalya Gomez, Canada]	Taken into account. Revised text addresses comment.
96863	7	16	7	17	"contributions to energy … change": this reference to ocean energy uptake comes a bit out of nowhere, considering this is the ES. [Nicole Wilke, Germany]	Taken into account. Revised text addresses comment.
67169	7	16	7	17	Sentence reads odd and does not say much. Perhaps the intended message can be sharpened here? [Regine Hock, United States of America]	Taken into account. Revised text addresses comment.
97961	7	16	7	17	"For 1971-2018 all major contributions.." you have SL and energy budgets in X-Chap Box 9.2, so why not refer to them? A simple switch out of "are known and consistent" with "budgets are well understood and close within uncertainties" (or similar) would make a stronger statement [Paul Durack, United States of America]	Taken into account. Revised text addresses comment.
71989	7	16			"glacier mass loss" - glaciers are always melting. [John Church, Australia]	Editorial comment accepted.
129351	7	17	7	17	Because trend values tend to depend quite substantially on the exact time span, explicitly include the time window here (e.g., over 1993-2018). [Trigg Talley, United States of America]	Taken into account. Revised text addresses comment.
103775	7	17	7	17	change -> budget [Philippe Tulkens, Belgium]	Taken into account. Revised text addresses comment.
99695	7	17	7	17	consistent with what? [Peter Clark, United States of America]	Taken into account. Revised text addresses comment.

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76699	7	17	7	17	Quantify the level of consistency in terms of mm/yr? Some contributions have relatively large errors. [Roelof Rietbroek, Germany]	Taken into account. Revised text addresses comment.
80811	7	17	7	17	change -> budget [Louise Sandberg Sørensen, Denmark]	Taken into account. Revised text addresses comment.
3281	7	17	7	17	Because trend values tend to depend quite substantially on the exact time span, I'd explicitly include the time window here (e.g. over 1993-2018) [Thomas Frederikse, United States of America]	Taken into account. Revised text addresses comment.
71991	7	18			This is meaningless precision. [John Church, Australia]	Not applicable. Text revised
129353	7	19	7	19	Instead of "since 1900" say "... sea level over 1900-2000 (or the 20th century) rose faster than during any other century ..." to avoid ambiguity. [Trigg Talley, United States of America]	Taken into account. Revised text addresses comment.
99467	7	19	7	19	Statement starting "GMSL increased ..." is given as fact, should there be a likelihood attached? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The confidence statement is taken from S9.6.1
62145	7	19	7	19	Please qualify this statement with a confidence statement [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account: The confidence statement is taken from S9.6.1
20163	7	19	7	19	Add "in" after "than" [philippe waldteufel, France]	Taken into account. Revised text addresses comment.
3283	7	19	7	19	Again: I'd say that instead of 'since 1900', 'sea level over 1900-2000 (or the 20th century) rose faster than during any other century' avoids ambiguity [Thomas Frederikse, United States of America]	Taken into account. Revised text addresses comment.
78337	7	19	7	21	This sentence makes a number of statements without declaring their significance.After the comma the meaning becomes obscure. [Michael Tsimplis, China]	Taken into account: The confidence statement is taken from S9.6.1 and 9.6.2
99139	7	19	7	22	In that the global climate has generally cooling since the Climatic Optimum as a result mainly of slow changes in the Earth's orbital parameters, the baseline situation was likely slow decline of sea level. I thus don't understand where the "more than half" comes from in that an early chapter talks about human emissions leading to some of the warming in the 19th century as well as the 20th. I would suggest changing "more than half" to "most of" [Michael MacCracken, United States of America]	Accepted. Text revised.
101685	7	20	7	20	"mm.yr-2" should be mm.yr-1 [IAPSO ECS group review, United States of America]	Rejected. Value refers to an acceleration, for which mm.yr-2 is correct.
62287	7	20	7	20	Add a confidence statement to the average rate of sea-level rise acceleration. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account: The confidence statement is taken from S9.6.1
62107	7	20	7	20	Please double check the units for rate of GMSL rise listed here, mm/yr^2 is supposed to be mm/yr? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Value refers to an acceleration, for which mm.yr-2 is correct.
79905	7	20	7	21	Is not possible to use for the different rates presented in the text the same period of reference (e.g. 1993-2018 as in Table 9.4). This also resolves the problem of indicating in each case the corresponding total increase in GMSL (it would be the same in all of them). Why in table 9.4 the period is 1993-12018 and in the text mostly 1992-.... Using the same period for the text and tables avoids confusion [Somavilla Raquel, Spain]	Not applicable. Text revised
51503	7	21	7	22	It is very likely that anthropogenic activities are responsible for more than half the observed GMSL change since the 1970s.' - this statement seems to be somewhat inconsistent with FAQ 9.2, which states '...since at least 1970, human activities have been the dominant cause of global average sea level rise.' Please could you ensure the attribution messaging for GMSL rise is consistent across the chapter? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text revised
129355	7	21	7	22	The way it's written now suggests that only a limited part of the observed rise is likely attributable to human activity. Could the total range (like 50-120%) be shown here? [Trigg Talley, United States of America]	Taken into account: The language statement of 'dominant' is taken from S9.6.1
129357	7	21	7	22	It would be helpful to list any other possible non-human causes of sea level rise not due to human activitiies, and also to provide a list of the full set of human activities (so aquifer depletion, reservoir construction, etc.). This statement seems very cautious. [Trigg Talley, United States of America]	Taken into account: The language statement of 'dominant' is taken from S9.6.1
96865	7	21	7	22	It would be useful to first clarify the general anthropogenic cause of SLR, and then quantify it since the 1970ies. Please consider revision: "It is very likely that anthropogenic forcings are the main driver of the observed global sea level rise and anthropogenic activities can be quantified to be responsible for more than half of the observed GMSL since the 1970s when sufficient observations became available." [Nicole Wilke, Germany]	Taken into account: The language statement of 'dominant' is taken from S9.6.1
3285	7	21	7	22	The way it's written now suggests that only a limited part of the observed rise is likely attributable to human activity. To avoid that the conclusion is drawn that a large part of what we see is natural, could the total range (like 50-120%) be shown here? [Thomas Frederikse, United States of America]	Taken into account: The language statement of 'dominant' is taken from S9.6.1
32407	7	22			Remove full stop after "1970s." [Olaf Eisen, Germany]	Editorial comment accepted.

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29635	7	24	6	27	This sentence needs to be reworded a bit. Such as "Beween 1995-2014 and 2100" is not a phase we normally use. I assume that 1995-2104 is the base period used to evaluate further sea level rise. But the use of this 1995-2014 is a bit confusing in this sentence. [Aixue Hu, United States of America]	Taken into account: The paragraph has been substantially revised.
96869	7	24	7	25	There is a discrepancy between the projected SLR in Ch9 and in Ch4 under SPP5-8.5 : in Ch9 0.6-0.9m SLR, in Ch4 0.5-1.07m in the executive summary. Please check these numbers carefully. [Nicole Wilke, Germany]	Taken into account: Chapter 9 SLR figures have been provided to Chapter 4 for consistency.
15547	7	24	7	25	The projections refer to P.95, lines 15-17. Under SSP5-8.5, the projection of GMSL rise is 0.73 m in 2100. This does not tally with the projection shown in Table 4.5 of Ch.4, in which the GMSL rise in 2081-2100 under SSP5-8.5 is already 0.73 m and GMSL rise is basically monotonically increasing. Please check and revise as appropriate. [SAI MING LEE, China]	Taken into account: Chapter 9 SLR values have been shared to Chapter 4 for consistency.
15549	7	24	7	25	Under SSP5-8.5, the projected GMSL rise in 2100 is 0.73 m relative to 1995-2014 (P.95, lines 15-17). Accounting for the sea level rise between 1986-2005 and 1995-2014, the AR6 projected GMSL rise in 2100 under SSP5-8.5 is about 0.76 m relative to 1986-2005, which is about 10% lower than the projection under RCP8.5 given by SROCC. Both SROCC and AR6 have stressed that GMSL rise has accelerated in recent decades. This AR6 GMSL rise projection may be perceived by some policymakers as an internal inconsistency between SROCC and AR6. Even worse, it may be mis-interpreted that the risk of sea level rise was overestimated previously by IPCC. It is strongly suggested to double check the validity of this GMSL rise projection. [SAI MING LEE, China]	Taken into account. Comparison with SROCC is provided in the chapter text.
78047	7	24	7	25	What level of confidence do you have in these "likely" statements? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now clarify that these likely ranges refer only to "processes in whose projections we have at least medium confidence."
34677	7	24	7	26	The first sentence of this key message is loaded with numbers. Consider reporting on projected changes for 2-3 SSPs instead. [Russell Vose, United States of America]	Taken into account: The paragraph has been substantially revised.
2525	7	24	7	29	it seems impossible that for SSP1-2.6, the lower bound of GMSL change is higher by 2100 than by 2300? [Tim Hermans, Netherlands]	Taken into account. The lower bound of the range for SSP1-2.6 in 2100 is about 0.2 m and is about 0.3 m for 2300; note the range for 2300 is not a likely range, but a low confidence range.
51501	7	24	7	30	It would be helpful here to also include GMSL estimates for a high emission scenario to 2300, for comparison with the estimate for SSP1-2.6 provided here, and a brief explanation of the dominant reason for high uncertainty at higher emission scenarios to ensure consistency with SPM section C2 which states: 'All future warming scenarios show committed sea-level rise of several metres after two millennia, with ice sheets the dominant uncertainty on century timescales.' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text now includes statements related to 2300 projections for both SSP1-2.6 and SSP5-8.5.
49967	7	24	7	30	This paragraph needs a statement about how deep uncertainty in ice-sheet contributions makes it impossible to make similar statements about extrnely uncertainchanges under high emissions scenarios. [Daniel Gilford, United States of America]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.
129359	7	24	7	30	[RISK] It needs to be said somewhere that these ranges do not include the effects of collapse of a significant ice stream. While the probability may be low, societal planning to ensure adequate safety factors (so levees are not over topped, etc.) are typically at least 1 in 100 years (The Netherlands uses 1 in 10,000 likelihood for its planning), and no way do the estimates here represent the worst plausibe conditions. It is very telling that only the Greenland Ice Sheet withstood the relatively modest changes in forcing that ended the Last Glacial Maximum, so this seemingly high confidence there will not be collapse seems highly questionable and risky to depend on. This paragraph does not cover what is "possible" . [Trigg Talley, United States of America]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.

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15199	7	24	7	30	Echoing a previous comment: The assessment of GMSL by 2100 is incomplete given the communications trap that befell AR4's and other assessments' discussion of the long-tail risk of higher GMSL. To avoid this, the executive summary needs to i) state that models are not able to represent all ice melt processes, ii) following that, state that is more possible that GMSL is above, rather than below, the "likely" range ("likely" implies a 1/3rd chance of being outside the range, but that chance is not evenly distributed between lower and higher -- it's vital to explain this), and iii) with that, provide the higher numerical estimates from expert elicitation studies (even though those estimates may be low confidence). The Hinkel et al. paper in Nature Climate Change, among others, speak to the need to express the full range of possibilities, including the low probablity long tail risks. This is one of the most critical issues facing AR6 -- the pandemic is evidence that as scientists we need to, and governments want us to, highlight the long tail risks. [Simon Donner, Canada]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.
96867	7	24	7	30	The figures for SLR are about 11 cm lower (for the median, for the 83th percentile 20 cm) than in SROCC. In SROCC were considered the SLR between the time 1986-2005 and 2100, here the SLR between 1995-2014 and 2100. But in the 9 years from 1986-2005 to 1995-2014 there was not a SLR about 11cm. Explanation of this discrepancy is missing. [Nicole Wilke, Germany]	Taken into account. Comparison with SROCC is provided in the chapter text.
91101	7	24	7	30	This summary of GMSL projections is very problematic. It will go into the SPM most likely verbatim but it excludes RCP8.5 extremely likely, unlikely ranges and that will, most probably, be completely missed by policy makers who will simply read the numbers and infer that GSML is extremely unlikely to be less than 0.2 or more than 0.9. That is not what is presented in the chapter itself and an incorrect, possibly dangerous, inference. This, IMO, is very important. There is nothing on the extremely likely range for RCP8.5. The statement is asymmetric and will certainly mislead. I suggest to include an extremely likely range based on Table 9.5. From that table it would be 0.34-2.4 m. By doing this you, correctly, acknowledge the wide uncertainty in projections for RCP8.5 and make the statement symmetric and consistent with other summaries. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now: state that " This GMSL rise is primarily caused by thermal expansion and mass loss from glaciers and ice sheets, with minor contributions from changes in land-water storage. These numbers do not include ice-sheet related processes that are characterized by deep uncertainty. Higher amounts of GMSL rise before 2100 could be caused by earlier- than-projected disintegration of marine ice shelves and onset of Marine Ice Sheet Instability around Antarctica, the onset of Marine Ice Cliff Instability around Antarctica, and faster-than-projected changes in the surface mass balance and discharge from Greenland. In a low-likelihood, high-impact storyline, under high emissions such processes could in combination contribute more than one additional meter of sea-level rise by 2100." and also present projections for 2300 with and without MICI.
111983	7	24		30	I would expect here the quantification of the share of glaciers and ice sheets melting on sea level rise in more details in this chapter, as more than a third due to thermal expansion declared in summary in chapter 4. Could be compared, at p. 87 concluded it is about half and half for 1971-2015. [Tomas Halenka, Czech Republic]	Noted. Contributions to GMSL rise from processes are dealt with in sustained warming level scenarios, and with detail in section 9.6.3
83623	7	24			P9-7 line 24 says “GMSL is likely to increase by 0.33-0.64 m under SSP1-2.6, 0.40-0.71 m under SSP2-4.5, 0.51-0.81 m 25 under SSP3-7, and 0.60-0.90 m under SSP5-8.5 between 1995-2014 and 2100. GMSL is extremely 26 unlikely to increase by less than 0.2 m or more than 0.9 m under SSP1-2.6 and SSP 2-4.5. It is virtually 27 certain that GMSL will continue to rise beyond 2100” The entire discussion of GMSL does not mention existence of a natural ~60year cycle superimposed on the GMSL rising trend from 1700CE. It is an unfortunate omission as it does influence how we judge GMSL rise in different past decades and into the future. Some relevant refs are Chambers, D. P., M. A. Merrifield, and R. S. Nerem, 2012: Is there a 60-year oscillation in global mean sea level? Geophys. Res. Lett., 39, L18607. Jevrejeva, S., Moore, J.C., Grinsted, A., Woodworth, P.L., 2008, Recent global sea level acceleration started over 200 years ago? Geophys. Res. Lett.35, L08715. The following ref extends Jevrejeva’s analysis and demonstrates a clear 65yr period cyclic component from present day back to 1750CE. Asten, M.W., 2017, Phase relations of natural 65 year SST variations, ocean sea level variations over 260 years, and Arctic sea-ice retreat of the satellite era – issues of cause and effect, Geophysical Research Abstracts, Vol. 19, EGU2017-9833, EGU General Assembly 2017. [michael asten, Australia]	Noted. The variations in the historic sea-level record are not addressed in the Executive Summary, but are discussed in Section 9.6.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
74027	7	25	7	25	I did not find in all the report and its various sections, how is the reference of sea level rise to 1995-2014 and this is not clear. Is that an average value of the sea level rise during 1995 through 2014? And do you place the origin, at 2010? A proper explanation/definition is needed, unless somehow I missed it. [Sergiu Dov ROSEN, Israel]	Noted. The explanation of the baseline period of 1995-2014 for AR6 is provided in the first paragraph of Section 9.6.3.1.
64827	7	25	7	26	GMSL is extremely unlikely to increase by less than 0.2 m or more than 0.9 m under SSP1-2.6 and SSP 2-4.5. -> from today? And until 2100? The years could be specified explicitly in this statement. [Martin Ménégoz, France]	Taken into account. The year range is now expressed explicitly.
99141	7	25	7	26	This projection seems to leave off completely the risk for significant ice stream movement, either due to greater platicity of the ice stream as it warms or to a loss of the ice shelves currently holding back very large ice streams. I think leaving off mention of this risk is simply unacceptabe. [Michael MacCracken, United States of America]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.
96871	7	25	7	26	Avoid double negation 'extremely unlike… increase by less…' please. [Nicole Wilke, Germany]	Taken into account. Sentence had been rephrased substantially.
88013	7	25	7	26	You may add "until 2100" to the sentence [Georg Kaser, Austria]	Accepted. Text revised.
38095	7	25	7	26	"GMSL is extremely unlikely to increase by less than 0.2 m or more than 0.9 m under SSP1-2.6 and SSP 2-4.5" is not necessary to be included, although it aims to recall the global warming of 1.5 and 2.0 degree. [Junhee Lee, Republic of Korea]	Taken into account. Revised text addresses this comment.
78049	7	25	7	26	I'm not convinced that "extremely likely" is justified by the evidence; you would need high confidence in the upper tail to make such an assertion, I would argue. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been revised, no longer citing an 'extremely likely' range.
41443	7	25	7	26	The scenario selection in this statement doesn't make much sense to me. Ideally, "extremely unlikely below" projections for the fully Paris Agreement scenario SSP1-19 should be shown together with another a "extremely unlikely below" statement for the highest emission scenario SSP5-85. This should be possible given the fact that the deep uncertainties around ice sheets only prohibit an "extremely unlikely above" statement. The ES needs a statement that captures information of the "worst case" SLR potential if parts of the AIS rapidly disintegrated, with a clear uncertainty caveat, of course. On page 91 lines 1 to 4 you state: "There is high confidence that GMSL rise will extremely likely be below the highest of the 95th percentile projections including MICI and SEJ [...]" This level of confidence allows for inclusion of an upper range in the ES despite the exisiting deep uncertainties. [Alexander Nauels, Germany]	Taken into account. The scenario range has been expanded, to capture Paris agreement and highest emission scenarios. A statement is now provided on the low likelihood high impact high end scenario.
96873	7	26	7	26	It is a bit awkward to read a "minimum" sea level value for SSP1-2.6 and "maximum" for SSP2-4.5, instead of SSP5-8.5, what one might expect. We understand that it will be harder to give such a value for SSP5-8.5, but perhaps it would be beneficial to make that difficulty explicit. [Nicole Wilke, Germany]	Taken into account. The revised text addresses this issue, by spanning range of scenarios.
99469	7	26	7	26	State period or confirm it is the same as in the first sentence. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The revised text explicitly states period.
62279	7	26	7	28	What about the Antarctica role. There is no mention of this polar area that should be included in the glacier and ice-sheet melting. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Individual contributions are addressed in the ice sheet statements, but -- considering only processes in which there is medium confidence -- the net AIS contribution is indeed not very sensitive to scenarios through 2100.
62147	7	26	7	28	Please add a statement about the contribution of the Antarctic Ice Sheet to GMSL change [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Individual contributions are addressed in the ice sheet statements, but -- considering only processes in which there is medium confidence -- the net AIS contribution is indeed not very sensitive to scenarios through 2100.
115151	7	26	7	28	I think a statement about Antarctica is missing here. [Natalya Gomez, Canada]	Taken into account. Individual contributions are addressed in the ice sheet statements, but -- considering only processes in which there is medium confidence -- the net AIS contribution is indeed not very sensitive to scenarios through 2100.
38097	7	26	7	28	"It is virtually certain that GMSL will continue to rise beyond 2100, with GMSL rise due to thermal expansion, glacier melt, and Greenland ice sheet melt highly dependent on emissions scenario after 2050" is rather ambiguous. [Junhee Lee, Republic of Korea]	Taken into account. The text has been revised, to improve clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99143	7	27	7	28	I simply don't agree. For Greenland, it may well be, based on, for example what likely happened during the Eemain, that there is already a commitment to major loss of ice from Greenland, and mking it seem as if this is controlled by emissions after 2050 I think is a real stretch based on how Greenland is observed to be responding. And associating what is said in this second half of the sentence with the "virtually certain" statement in the first half of the senteence is inappropriate--there is a clear risk of other contributions. [Michael MacCracken, United States of America]	Taken into account. The Eemian reference is most relevant for the discussion of long-term committed GMSL rise at the end of the ES.
76735	7	27	7	28	What about the Antarctic Ice Sheet? [Ronja Reese, Germany]	Taken into account. Individual contributions are addressed in the ice sheet statements, but -- considering only processes in which there is medium confidence -- the net AIS contribution is indeed not very sensitive to scenarios through 2100.
3075	7	28	7	28	"Antarctica" has to be mentioned as well (not only the Greenland ice sheet) since it is the largest uncertainty after 2050. [Daniel Farinotti, Switzerland]	Taken into account. Individual contributions are addressed in the ice sheet statements, but -- considering only processes in which there is medium confidence -- the net AIS contribution is indeed not very sensitive to scenarios through 2100.
14679	7	28	7	28	Note potential AIS contribution this summary of post-2100 SLR, in addition to other sources (even if this AIS contribution is highly uncertain) [Jeremy Fyke, Canada]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.
67171	7	28	7	28	this formulation may not be easily understood for what seems to be intended. Perhaps reformulate [Regine Hock, United States of America]	Taken into account. The text has been revised, to improve clarity.
62109	7	28	7	28	2300? Is this supposed to be 2100? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Statements have been restructured to clarify.
78339	7	28	7	30	Why does the report goes up to 2300 here? The deep uncertainty must mean that the whole renge of solutions is in doubt not that the predictions are uncertain in one way only. Perhaps worth saying that this uncertainty is driven by the ice sheet component rather than the others. [Michael Tsimplis, China]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.
51505	7	28	7	30	Please could you clarify briefly the primary reason for deep uncertainty under high emission scenarios for 2300 GMSL rise projections - i.e. due to the uncertainty regarding the Antarctic contribution to GMSL, to ensure consistency with section C2.4 of the SPM which states this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.
99145	7	28	7	30	This conclusion seems totally at odds with what paleorecords of sea level change have been. This may be all that is possible with surface melting, but if all processes are considered, the estimates here are far below what would be expected based on paleo examples, The notion that the world could continue to have global warming of a couple of degrees and "only" have less than 5 meters warming seems totally inconsistent with what occurred during the Eemian that had 4-8 meters of SL rise with a warming of perhaps 1 C and the CO2 level (which causes wintertime addition of heat to the ice sheets) at less than 300 ppm. I just think this conclusion is far too cautios and totally fails to present the risks of very significant SL rise. [Michael MacCracken, United States of America]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300. However, as noted in the chapter, the Eemian is not a direct analog for 1-2°C of warming because of differences in orbital forcing and polar amplification.
129361	7	28	7	30	[CONFIDENCE] With paleo indications suggesting that the equilibrium sea level sensitivity to global average temperature may be as much as 15-20 meters per °C and with sea level having risen at an average rate of 1 m per century for 120 centuries coming out of the Last Glacial Maximum while global average temperature was rising at an average of 1°C per 2000 years, and with global warming quite possibly going to 3°C over preindustrial this century before coming back to perhaps 2°C in the centuries ahead, how can the the estimates here be stated with any confidence at all? All of the values have deep uncertainty.There is still 60-70 m of sea level equivalent on land. For the Eemian (only 1°C warmer than present), sea level is estimated to have been something like 4-8 m higher than at present, and that was with CO2 concentration at less than 300 ppm. [Trigg Talley, United States of America]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300. However, as noted in the chapter, the Eemian is not a direct analog for 1-2°C of warming because of differences in orbital forcing and polar amplification.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96875	7	28	7	30	The sentence gives central projections for SSP1-2.6, but not for any higher emission scenario, instead only stating that these exhibit "deep uncertainty". In this way the sentence is biased: It risks favouring the better quantifiable results of the low emission scenario over the state of knowledge regarding higher emission scenarios. This repeats the dangerous flaw from AR5 and earlier assessments to exclude ice sheet contributions from any SLR estimate because of deeper uncertainty. For higher emission scenarios some quantitative statement - even if given low confidence - should be extracted from Chapter 9.6.3.5. Deep uncertainty should not prevent from giving the best guidance available in the Executive Summary on such an important issue. [Nicole Wilke, Germany]	Taken into account. The revised text provides projections for range of scenarios (SSP1-1.9 through to SSP5-8.5). Furthermore, it cites the low-likelihood, high-impact scenarios, and low-confidence, associated with ice-sheet instability.
110709	7	28	7	30	The expression “will likely be”, as often used here, could be somewhat misleading, as these are projections or estimates based on the current knowledge about involved processes that entered into the models for given input and boundary uncertainties, an if-then statement. Shouldn’t this be better represented in the used expressions, e.g. “is projected to be likely with the range...” or “likely estimate”? [Torsten Albrecht, Germany]	Taken into account. The revised text avoids the expression 'will likely be'
34679	7	28	7	30	Does this sentence really refer to changes after 2300? I don't think I've encountered any other key message in the entire report going that far into the future. Maybe I'm conservative in this respect, but I think it's better to leave conclusions that that have deep uncertainty in the body of the chapter rather than in the key messages. [Russell Vose, United States of America]	Rejected. Deep uncertainty regarding long-term sea-level projections, particularly under high emissions, is a key message of this chapter, consistent with the SROCC.
41445	7	28	7	30	While the reluctance by the authors to provide 2300 SLR projection ranges under high emission scenario extensions is understood, the catastrophic multi-meter SLR potential has to be made more clear. Same goes for the much higher chances to avoid rapid ice sheet mass loss when limiting warming to Paris Agreement compatible levels. This is the kind of information particularly vulnerable countries like SIDS need for their coastal planning, for example. [Alexander Nauels, Germany]	Taken into account. Deep uncertainty is explicitly referenced in the discussion of ice sheet projections, and is illustrated here by the presentation of low-likelihood/high-impact projections for 2100 and projections with and without MICI for 2300.
71993	7	28			glacier and Greenland mass loss - glaciers are always melting. [John Church, Australia]	Taken into account. Reference to glacial melt is now removed in all instances.
78051	7	29	7	29	Same comment about "extremely likely" as on line 25-26. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The confidence statements have been revised.
33411	7	32	7	33	Incomplete sentence: “Regional sea-level rise rate increased fastest in the Indo-Pacific, Northwest Pacific and subtropical North Atlantic over 1993-2015 and the projected change is largest in”. [Guiomar Rotllant, Spain]	Taken into account. The revised text completes placeholder.
51499	7	32	7	37	Please could you clarify here briefly the reasons for the large uncertainties in attribution of SLR at the regional scale, as outlined in section 9.6.1.5 - due to variability at the regional scale. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised text addresses this comment, pointing to internal modes of variability as source of uncertainties.
129363	7	32	7	37	It would help to give some explanation of why there are regional differences, so to give a sense of why uncertainties can arise. So, perhaps have the second sentence start: "Regional differences in sea level rise arise for many reasons: ..." It needs to be made clear if authors are talking about the rise out off the coast, or the rise along the coast. Basically, there is a need to know why the regional distribution matters: If it is not at the coast, is it a reflection of ocean circulation, etc. [Trigg Talley, United States of America]	Taken into account. Revised text identifies why regional differences occur, and identifies internal modes of variability dominating dynamic driven RSL.
71995	7	32		37	I do not think closing the regional budgets should be in the ES. [John Church, Australia]	Accepted. Closure of the regional budget is removed from the ES.
82865	7	33	7	33	I suggest to add "by 2040" when projections are mentioned (after "projected changes") in the heading of this point. [Sebastian Gerland, Norway]	Taken into account. Revised text completes placeholder.
23485	7	33	7	34	Pacific modulation of accelerated south Indian Ocean sea level rise during the early 21st Century DOI- 10.1007/s00382-019-04795-0 [Saurabh Rathore, Australia]	Rejected. Relevant to section 9.6.1, but not for the ES.
54501	7	33	7	34	Regional sea-level rise rate increased fastest in the Indo-Pacific, Northwest Pacific and subtropical North Atlantic over 1993-2015 and the projected change is largest in [PLACEHOLDER FOR REGIONAL PROJECTIONS]. [Maria del Pilar Bueno Rubial, Argentina]	Taken into account. Revised text completes placeholder.
62149	7	34	7	34	The use of "uncertainties" here is not consistent with IPCC language. Provide clarification of what is meant by uncertainty here by mentioning the origin of this uncertainty. This could be clarified by mentioning that regional differences in the response of the ocean-atmosphere system to internal variability precludes the ability to close local and regional sea level budgets (from page 9-39 lines 40-42 and page 9-40 lines 6-7) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Revised text alters uncertainty language.
24453	7	35	6	37	The description of expected area (50%) is better to specified as "starting from Tropics at over 50%" [Nobuhito Mori, Japan]	Not Applicable. Text no longer included in Chapter.
38099	7	35	7	35	"attribution of regional sea-level change to anthropogenic forcing also remains difficult." is rather ambiguous. [Junhee Lee, Republic of Korea]	Taken into account. Revised text refines language.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78341	7	35	7	37	grammatically incorrect and unclear, [Michael Tsimplis, China]	Taken into account. Revised text refines language.
65851	7	35	20	19	Suggest reconciling the definition of marine heat wave (9-19, 37-45) as a "deviation beyond the daily 99th percentile" with the projections of a "near-permanent MHW state". Suggest changing to: "Conditions now defined as Marine Heat Waves are projected to be near-permanent states in many parts of the ocean by the Late 21st Century ..." [Kushla Munro, Australia]	Taken into account, with altered attention to MHW in the ES (in ocean heat section, as opposed to aligned with extremes section)
99471	7	36	7	36	This implies rsl will remain within natural variability over 50% of the oceans till 2040 which seems surprising so suggest this statement is checked. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. No longer relevant in revised text
86811	7	40	7	43	Marine heatwaves have become more frequent (high confidence) and persistent (medium confidence) since 1982 and are very likely to increase in frequency, duration, spatial extent and intensity in the coming decades. Would have been helpful with a short comment on which regions would experience these MHW. [Oyvind Christophersen, Norway]	Accepted, we now mention the regions that are projected to experience larger change
51507	7	41	7	41	The bold headline 'marine extremes have become more frequent and this trend will continue in the future' is slightly misleading as Chapter 11 discusses projected changes in Tropical Cyclones, stating in the Executive Summary that 'There is medium confidence that the global frequency of TCs over all categories will decrease or remain unchanged.' Could this statement be clarified to say: 'Some marine extremes, including heatwaves and extreme still water levels, have become...'? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. We now only focus on marine heat wave
97963	7	41	7	41	"Marine extremes" do you mean "Marine extreme heating, flood and surge events have become more frequent and THESE trends.." [Paul Durack, United States of America]	Accepted. We have now clarified that we only refer to marine heat wave in this statement.
82867	7	41	7	41	I suggest to write "into the coming decades", instead of "into the future", for being more precise. [Sebastian Gerland, Norway]	Accepted. Specific periods are not provided.
38101	7	41	7	41	The definition of "marine extremes" is rather ambiguous. This term is also different from SROCC. If it means the marine heat wave, extreme sea level events and coastal hazards, it should be defined first. [Junhee Lee, Republic of Korea]	Accepted. We have now clarified that we only refer to marine heat wave in this statement
99147	7	41	7	42	"Marine heat waves" and their significance needs to be mentioned so expected reader will know what is being projected and why this is important. [Michael MacCracken, United States of America]	Accepted. We now mention the impact of Marine heat waves
129365	7	41	7	48	[CONFIDENCE] Review confidence levels based on SROCC: "Marine heatwaves have very likely doubled in frequency since 1982 and are increasing in intensity (very high confidence)." Page 19 of Chapter 6 also notes: "Under further global warming, MHWs are very likely to increase in frequency, duration, spatial extent, and intensity in all ocean basins, but with distinct spatial magnitudes (Cross-Chapter Box 9.1, Figure 1b-c) (Frolicher et al., 2018)." [Trigg Talley, United States of America]	Taken into account. The confidence levels in SROCC have been considered in revising the marine heat wave assessment.
129367	7	41	7	48	It is important to define the various terms and why they are important for people to consider. So, "marine heat waves" needs to be defined and an indication of their importance for fish and marine life, etc., just as an inserted phrase. Three-figure precision for the increase in high-tide flooding frequency does not seem at all justified when the preceding subsection just said there are large uncertainties in projections of regional climate change, which is apparently partly due to the regional change not emerging from whatever variability there is. The "11" and "14" numbers with no uncertainties just seem overly precise. Does this apply to every local station around the world or is this a global average; is it along some types of coastlines or all coastlines? The statement does not make this clear. [Trigg Talley, United States of America]	Accepted. We have now clarified that marine heat wave is an extreme in sea surface temperature. The ES statement for extreme sea level is intended as a general one to cover the globe.
112475	7	41	7	48	In this summary a brief reference to well known marine heatwaves would be a plus (Oliver et al.2018 doi:10.1038/s41467-018-03732-9 ; Hobday et al., 2016 doi: 10.1016/j.pocean.2015.12.014; Frolicher et al. 2018 doi:10.1038/s41586-018-0383-9). [Pedro LLanillo del Rio, Germany]	Accepted. Those are considered in the underlying assessment of Marine heat wave in the dedicated box
76701	7	42	7	43	Is it known where these marine heatwaves occurences are expected to increase? (i.e. are clear hotspots identifiable?) [Roelof Rietbroek, Germany]	Accepted, we now mention the regions that are projected to experience larger change
32455	7	43	7	43	surely virtually certain increase in marine heatwaves. [Robert Colman, Australia]	Taken into account. The marine heatwave section of text has been altered considerably in revised text.
78343	7	43	7	47	are extreme still water levels different from extreme water levels? Sentence is meaningless- 1% level will always occur 1% of the sample. Perhaps what is meant is that the current corresponding values will not be 1% anymore and this must be clearly tied in to mean sea level change. But my understandintanding is that this is regional and local while here it is implied as a global result. The tide gauge stations (and WHICH tide gauge stations are meant here) are not uniformly distributed so the statement becomes very confusing and irrelevant. [Michael Tsimplis, China]	Accepted. ESWL is defined, following Box 9.1, and 1% AEP is clarified. Tide gauge stations can not be defined further without adding too much detail for ES (i.e., 634 GESLA-2 database tide gauges).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62111	7	44	7	44	Please clarify what is meant by "still water levels" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. ESWL are defined in revised text.
18761	7	45	7	45	"1 % average annual probability…". Is this 1 percent or percentile? [Govindasamy Bala, India]	Taken into account. This is no longer relevant in revised text.
62113	7	45	7	47	I feel that this sentence could use some clarification; are these changes at 20% of tidal gauge stations globally? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Revised text clarifies.
99473	7	45	7	48	Is this result given for 2050 consistent with the previous statement on rsl up to 2040? Also, why use 2050 here and 2040 in the previous ES finding? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, with time horizons standardised through the ES.
99697	7	46	7	46	"are projected to" - dependent on regional sea level projections? [Peter Clark, United States of America]	Noted. ESL includes RSL projections by definition. No change.
103777	7	47	7	47	stations. (high confidence) -> stations (high confidence). [Philippe Tulkens, Belgium]	Editorial. comment accepted.
80813	7	47	7	47	stations. (high confidence) -> stations (high confidence). [Louise Sandberg Sørensen, Denmark]	Editorial. comment accepted.
26365	7	47	7	47	the tide gauge stations. (high confidence) [María Santolaria-Otín, France]	Editorial. comment accepted.
67173	7	50	7	50	in terms of the level of warming' sounds very odd and it is not clear what is meant. Better: change for given levels of global warming? [Regine Hock, United States of America]	Accepted. We now write "at specific levels of global warming"
35055	7	50	7	50	The expression "….in terms of…" seems inappropriate. Suggest replace with "… in response to…." [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now write "at specific levels of global warming"
279	7	50	8	20	Ah, now I SEE. This section is about scenarios! These three sections need to written so they have parallel structure. Maybe this should be called out somehow. It seems weirdly tacked on. [THOMAS Wagner, United States of America]	Taken into account, and the scenarios are more clearly introduced.
64829	7	50	8	20	Numbers are often given in percentage for glacier changes, whereas they are only described as square km for snow cover extent. Would it be relevant to include the loss of snow cover extent per degree of warming also in percentage? [Martin Ménégoz, France]	Noted. This is difficult because snow cover varies very strongly over the seasons. Providing the numbers would lead to a very long text.
15201	7	50	8	20	This section on findings my warming levels is a great idea. Two suggestions: i) be clear on the defintion of the warming level (following the discussion in Chapter 2 - i.e. is this GMST?), ii) try to expand the assessment summary for 2-3 C warming [Simon Donner, Canada]	Accepted. We now are specific that this is in terms of GSAT change relative to pre-industrial levels. We have expanded the assessment summary for 2-3 C warming.
12445	7	50	8	20	There is no "ocean" projection in this subsection. [Lijing Cheng, China]	Noted. We were unable to find useful ocean metrics that we could map against these warming levels based on published literature.
130551	7	50	8	20	There is no "ocean" projection in this subsection. [Panmao Zhai, China]	Accepted. We now discuss specific ocean changes at the three warming levels.
29245	7	50	8	22	The section "Ocean, cryosphere, and sea-level change in terms of the level of global warming" is excellent, and seems to really summarize many important points in terms that should be easily understood by a general audience. I am wondering if this section should be highlighted in some way--I am concerned that right now, it may be somewhat buried at the end of the Executive Summary. I think it may reach general readers more easily than many other parts of the Executive Summary, given that some of those sections retain a great deal of jargon and can be somewhat technical. [Andra Garner, United States of America]	Noted. We are grateful for the positive feedback, and have carefully considered the suggestion to move this part upfront. We have, however, decided against such step in line with the traditional expectation that people will have of an executive summary. We have, however, forwarded this request for consideration in the summary for policy makers.
111739	7	50			It is brave to try to relate everything to global warming levels in this way, but as the authors are aware the long timescales of manay ocean/cryosphere components make the response at a given warming level scenario-dependent - especially in strong mitigation/overshoot cases. This means that sometimes it is misleading to do this. I suggest a statement to this effect would be useful at the start of this section. And for cases where it is really meaningless to relate changes to global warming levels, it would be best to say so explicitly, rather than be tempted to write a highly qualified or convoluted assessment. What may be useful is to make statements that apply to global warming levels during the 21st century, across the range of SSP scenarios, where that is possble. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now only discuss implications of sustained levels of warming, with no time information for clarity.
3077	7	52	7	52	The use of bold font is not consistent. Here, it does not provide a complete statement. Same comment for the next three paragraphs. [Daniel Farinotti, Switzerland]	Noted. However, given the change in style of this part of the ES, we find this breaking of consistency acceptable and have decided to keep it.
3079	7	52	7	52	The baseline for the "future warming" is unclear. Is it against pre-industrial or against current levels? The same comment applies for the next three aragraphs as well. [Daniel Farinotti, Switzerland]	Accepted. We now are specific that this is in terms of GSAT change relative to pre-industrial levels.
67179	7	52	7	52	Better? With future warming between ….' [Regine Hock, United States of America]	Noted. We now write "At sustained warming levels between 1.5 and 2 °C"

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62115	7	52	7	52	The bolded statement for this paragraph is very vague; is this in reference to the climatic system warming as a whole, or specfic atmospheric/artic amplification warming, or oceanic temperature warming? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now are specific that this is in terms of GSAT change relative to pre-industrial levels.
62117	7	52	7	52	The bolded statement for this paragraph is very quantitatively vague; "for decades or longer", what is the minimum cutoff for being included in such a statement? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now are specific that this is in terms of sustained (i.e., long term) GSAT change relative to pre-industrial levels.
83273	7	52	7	53	What about Antarctic sea ice? Please include a similar statement here relating to the future of Antarctic sea ice. [Robert Massom, Australia]	Noted. However, given the low confidence we have in projections of Antarctic sea ice, we feel that no such statement can be included even with medium confidence.
115153	7	52	7	54	Could it be made more clear whether this statement implies anything about by when the glaciers will melt, relative to the timing of the forcing? If nothing is already implied, could some statement about this be added? The current wording is not clear to me. [Natalya Gomez, Canada]	Noted. We now only discuss the long-term consequences of specific warming levels. To ease readability we have decided against adding individual pieces of time information for the various statements.
129369	7	52	7	55	Suggest adding information on the RCP scenario to the statement since it is noted that it is under the RCP2.6 scenario that 10-60% will remain in polar regions. Page 74 of Chapter 9 states: "Two models that have run longer simulations (Levermann et al., 2013) show that under the RCP2.6 scenario, 10-60% of the glacier volume will remain in polar regions, although all low latitude and small glaciers will melt and for the RCP8.5 scenario only the largest glaciers will survive at sizes that are less than 10-20% of their present size (Marzeion et al., 2012)." [Trigg Talley, United States of America]	Noted. We prefer to keep this framed in terms of warming levels as this is the main focus of these three paragraphs.
88243	7	52	8	2	What is the time period over which these changes will occur? Is it up to 2100 as mentioned in the next sentence or is this the committed change (equilibrium conditions)? Since changes in ice sheets, glacier and permafrost lag behind air temperature changes it is important to be clear about time periods. [Sharon Smith, Canada]	Accepted. We now are specific that this is in terms of sustained (i.e., long term) GSAT change relative to pre-industrial levels.
78345	7	52	8	4	What about marine heat waves and sea level extremes? [Michael Tsimplis, China]	Noted. We were unable to find literature on long-term changes on these metrics as a function of specific sustained warming levels and so decided to focus on mean sea level instead in these paragraphs.
275	7	52	8	4	What is the point of this paragraph? I realize this is a draft; maybe these points should be placed in their respective section? It reads like a summary that should come up front. [THOMAS Wagner, United States of America]	Noted. We have carefully considered the suggestion to move this part upfront. We have, however, decided against such step in line with the traditional expectation that people will have of an executive summary. We have, however, forwarded this request for consideration in the summary for policy makers.
20541	7	52	8	4	This paragraph is not very homogeneous. At the beginning it predicts what will happen to the cryosphere elements if the warming level stays below 2°C; at the end it says what will happen in the opposite case to GMSL. [philippe waldteufel, France]	Accepted. We now discuss all items in a consistent way.
99149	7	52	8	4	Again, there is this focus on the exact amount of sea ice extent in late summer, etc.--as if this really matters. The Arctic will have much less ice during the periods of high solar radiation at temperatures like this and this will lead to substantial transfer of energy to the Arctic winter atmosphere and this have have major effects on NH mid-latitude weather. This whole focus and argument about minimum September sea ice makes no sense at all--the Arctic of 2050 is going to be very, very different than the one of the 1990s and its effect on the NH winter weather will be huge. The statement here is just much, much too sanguine about what is happening, getting distrracted by this September sea ice issue. The statement here needs to be completely refocused to the importance of a warmer Arctic through the year, its influence on melting rate of Greenland and loss of ice mass via ice streams as warmer water floods the fjords, on the effects of warmer weather on the permafrost, etc. I just find this conclusion very misleading and uninformative about the issues that really matter. [Michael MacCracken, United States of America]	Accepted. We highlight in this section some of the major changes in the cryosphere for given warming levels, which includes the loss of sea ice in September. We do not discuss the impact of these changes for weather patterns, for example. We have, however, now added the fact that the ice loss for several summer months will cause additional absorption of solar energy by the Arctic Ocean.
91105	7	52	8	4	I am not 100% familiar with the latest ISMIP projections but for 1.5-2 degs warming (i.e. > present day), surely GrIS will lose more mass than present. In all model expts I have seen ablation outstrips increased accum for any warming. Discharge may decrease eventually but this requires already a smaller ice sheet than present. I am surprised, therefore, that it is medium confidence for GrIS. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This statement is no longer part of the ES
67181	7	52	8	12	warming by X deg relative to what? [Regine Hock, United States of America]	Accepted. We now are specific that this is in terms of GSAT change relative to pre-industrial levels.

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62289	7	53	7	53	Change "all year round" to "year round" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed as suggested.
62291	7	53	7	53	Change "in most years" to "in most years until 2100" or something similar [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We now decided to discuss this in terms of long-term sustained warming levels. Given that sea ice reacts quickly to changes in temperature, the threshold for an ice-free Arctic for example should not have much time dependence.
129371	7	53	7	53	What does "partly" mean? The assertion is realy that sea ice will be down 95% or something. Saying in "most" years is again very loose. For WGII impacts assessment, these statements would have no value. Would the ice be thick enough for marine life to rest on, for hunters, for marine biological activity, enough to impede shipping, etc.? Adding in some sort of information from the paleo record might help. [Trigg Talley, United States of America]	Accepted in part: We now write: "The summer minimum Arctic sea-ice area will likely be above 1 million km2 in most years; ". We feel that "most" is clear enough, as it refers to more than half of all years.
15787	7	53	7	53	It's not clear what is meant by "in most years", which years? [Olga Sergienko, United States of America]	Noted. Most is intended to mean "more than 50 % of all years", with a more concrete statement not being possible given the uncertainty of the projections
62293	7	53	7	54	Quantify "low latitude" and "polar regions" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We do not feel that such definition is needed.
103779	7	53	7	54	Regarding the statement: 'all small low latitude glaciers will very likely melt', is it elsewhere defined what small, low latitude means in this context? And should it rather be that they will 'disappear' instead of 'melt'? [Philippe Tulkens, Belgium]	Accepted. We now write "disappear"
67177	7	53	7	54	it is unclear why the latitude glaciers are singled out here? Glaciers are projected to lose significant volume in a number of regions with little ice. The statement also contradict the latest modeling efforts (e.g. Marzeion et al.). [Regine Hock, United States of America]	Accepted. Text revised to clarify that glaciers in low latitudes, and small glaciers in other regions, will completely disappear.
80815	7	53	7	54	Regarding the statement: 'all small low latitude glaciers will very likely melt', is it elsewhere defined what small, low latitude means in this context? And should it rather be that they will 'disappear' instead of 'melt'? [Louise Sandberg Sørensen, Denmark]	accepted, text revised
69571	7	53	7	54	"all small low latitude glaciers will very likely melt; however, 10-60% of glacier volume will remain in the polar regions;" I'd be a bit concerned about the fragility of this statement. The second clause is based only on one study it seems (Levermann et al., 2013)). It is also not completely clear whether the "very likely" is intended to apply to both the first and second clauses of the sentence. [gerard Roe, United States of America]	taken into account, text revised
3081	7	54	7	54	The use of "glacier volume" is unclear here: does the statement include ice sheets as well? [Daniel Farinotti, Switzerland]	taken into account, it does not include ice sheets, text clarified
67175	7	54	7	54	replace melt by disappear or melt away. Glaciers always melt even if the mass balance is positive. [Regine Hock, United States of America]	accepted, text revised
99699	7	54	7	54	"melt" - most glaciers melt (part of their mass balance). Do you mean they will disappear? [Peter Clark, United States of America]	accepted, text revised
76703	7	54	7	54	“Glaciers” does this include the Greenland glaciers connected to the ice sheet? [Roelof Rietbroek, Germany]	taken into account, text clarified
26367	7	54	7	54	Northern Hemisphere (both in capitals) [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
129373	7	54	7	55	The phrase on snow cover extent is a double negative and no context is given for how much remains. It would be nice to know if there will possibly be more snow due to a "lake-effect". For Antarctica, it is said that not being so cold will lead to more snowfall. What about the Arctic? The Ewing-Donn hypothesis purports to explain glacial interglacial cycling (in Science, in the 1950s or early 1960s) that suggested that the reason there was not more snow in the cold of the Arctic night was the lack of an open source of water -- but now there will be much more moisture available due to thinned or no sea ice and open leads created by the wind, etc. Just as happens on the Great Lakes, might this lead to much more snowfall on land areas? [Trigg Talley, United States of America]	taken into account double negative eliminated
39833	7	54		55	"spring snow cover extent decrease will likely remain below 3·106 km2" this phrasing is convoluted. what do you mean? the extent will reduce by 3 million km2 or there will be 3 million of km2 left ? [TSU WGI, France]	taken into account, text clarified
32139	7	54			"10-60% of glacier volume will remain in the polar regions" It is not clear at this point whether this includes the ice sheets. [Anja Wendt, Germany]	taken into account, text clarified

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88015	7	54			If 10-60% is the correct number, itmay not considered suitable for the Ex Sum [Georg Kaser, Austria]	taken into account, rough percentage for the different warming level are given, with low confidence statement
101687	7	55	7	55	"3 10^6 km^2" is a value that is difficult to understand and put in context for a non-expert reader. Is there a way to convert this to a percentage change? [IAPSO ECS group review, United States of America]	Taken into account. Percent change cannot be given because it would vary a lot depending on the month of the year. We spell out "million" now.
7833	7	55	7	55	The number formatting is odd (ie using the point rather than x) [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. It's not odd, it's German. Replaced by "x".
3083	7	55	8	1	A statement about the confidence level of the "permafrost volume decrease" is missing. [Daniel Farinotti, Switzerland]	Taken into account. Provided in the revised version.
88245	7	55	8	1	This statement on permafrost volume, does ignore the fact that permafrost thaw may exceed 3 m in some places (permafrost thaw does not stop at 3m) [Sharon Smith, Canada]	Taken into account. It does not ignore that permafrost can go below 3m. It simply takes into account the fact that 1) many model do not go very deep; 2) we do not care about permafrost at 1000 m depth, it's perfectly irrelevant for centennial-scale climate change. Up to 3 m depth, permafrost reacts quickly, and is most relevant for climate.
61975	7		7		add confidence behind " since 1950 globally" (high confidence) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The revised version does not mention the mountain snow changes since 1950.
116819	7		7		Missing explanation of contributions to sea level rise and cause for acceleration explicitely here. [Valerie Masson-Delmotte, France]	Taken into account. Executive summary statement on sea level acceleration now discusses changes in individual components.
83275	8	1	8	1	The ice sheets will decline in what sense? Be more specific here. [Robert Massom, Australia]	Noted. This statement is no longer part of the ES
103781	8	1	8	1	sheet -> sheets [Philippe Tulkens, Belgium]	Accepted. Has been corrected.
80817	8	1	8	1	sheet -> sheets [Louise Sandberg Sørensen, Denmark]	Accepted. Has been corrected.
132541	8	1	8	1	Does "decline" mean volume or extent or both? [Kyle Armour, United States of America]	Noted. This statement is no longer part of the ES
62151	8	1	8	1	Please clarify what is meant by "decline". This could be interpreted as the amount of ice lost will decline (i.e. slow-down of ice loss), surface elevation lowering. retreat of the area covered by the ice sheet. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This statement is no longer part of the ES
129375	8	1	8	4	[RISK] How is this statement at all consistent with paleoclimatic evidence of what has happened in the last two glacial-interglacial cycles? The Eemian interglacial was pretty short and sea level was up 4-8 m with less than half the likely warming over this period and when the CO2 level was less than 300 ppm. This gives no indication of what the upside risk is -- and it would seem to be huge. The idea that this is all that the rise would be seems to totally exclude the likelihood of any major ice stream decaying, and this seems totally implausible, especially if this chapter is claiming to present what is "possible", even with warming of only 1.5 to 2°C . [Trigg Talley, United States of America]	Accepted. We now give specific numbers for equilibrium sea-level rise for all warming levels
76705	8	2	8	2	GMSL rise … This information is somewhat ambigious as it has been mentioned before on page 7 and has a slightly different upper bound 0.65 versus 0.64 m. Make consistent? [Roelof Rietbroek, Germany]	Accepted. Revised range to be consistent with above mention.
29637	8	2	8	3	Previously the period 1995-2014 is used, but here 1996-2014 is used. I think we should reference these periods consistently. Maybe we should consistently use 1995-2014. [Aixue Hu, United States of America]	Accepted. Revised year range to be consistent with above mention
3143	8	3	8	3	“1996-2014”should be changed to "1995-2014" [Hui Wang, China]	Accepted. Revised year range to be consistent with above mention
129377	8	3	8	3	"acceleration" of what? Surely not just ice shelf thinning. The ice shelves are going to be calving much more rapidly and their ability to restrain ice stream flow is going to drop tremendously. [Trigg Talley, United States of America]	Not Applicable. Text no longer included in Chapter.
69221	8	3	8	3	The reference period of GMSL rise is inconsistent between Executive Summary (1996-2014), chapter 9.6.3.4 (1996-2014), Figure 9.31 (1996-2014) and Table 9.7 (1995-2014). Using 1995-2014, which is defined as "modern period" in the SPM as the reference period is suggested. [Kaoru Magosaki, Japan]	Accepted. Revised year range to be consistent with above mention
41447	8	3	8	3	This is hopefully only a typo, please change to 1995-2014. [Alexander Nauels, Germany]	Accepted. Revised year range to be consistent with above mention
39179	8	3	8	4	What is meant by "per °C peak warming"? [Lourdes Tibig, Philippines]	Taken into account, and no longer relevant with text deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34681	8	3	8	4	I noticed that the Executive Summary contains a similar statement, and so I'll summarize my reponse to that comment here. I think it's just a bit sensational to make statements about conditions two millennia into the future in a key message. It's interesting science but also largely conjecture. [Russell Vose, United States of America]	Rejected. Stakeholder interest spans committed SLR scenarios over these time-frames.
52029	8	3	8	19	I find the use of "after two millennia" here a bit awkward. Does this mean 2000 years from now? In Section 9.6 "increase after 2000 years" is used. It would likely be clearer to say "by the year 4000" or something? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, and no longer relevant with text deleted.
107435	8	6	8	6	Heading should be in parallel form with the one preceding and after to read "If future warming levels remain between 2 and 3 degrees C for decades or longer" [Jennifer Walker, United States of America]	Accepted. We now specify that this is all for sustained warming
89389	8	6	8	7	"For future warming levels between 2 and 3°C, there is medium confidence […]" - Does this include the estimates by Robinson et al. 2012 (DOI: 10.1038/NCLIMATE1449)? Their estimate is 0.8 to 3.2°C, as also explained lated on in the Chapter -- does this mean the lower limit needs to be adjusted? [Ricarda Winkelmann, Germany]	Noted. We now give specific numbers for equilibrium sea-level rise
15789	8	6	8	7	"there is medium confidence that the Greenland Ice Sheet will pass a threshold where long-term mass loss becomes irreversible over centennial timescales" This sentence is misleading. Does it mean that the long-term mass loss can be reversible on different time scales? What does "threshold" mean here - Gt/yr ice, temperature degrees? [Olga Sergienko, United States of America]	Accepted. We have re-written this statement
38103	8	6	8	7	There is no mention about "Antarctic Ice Sheet will pass a threshold where long-term mass loss becomes irreversible over centennial timescales", which was described in IPCC SR1.5. [Junhee Lee, Republic of Korea]	Accepted. Such statement has been added.
277	8	6	8	9	What's the point of this? I'm not sure it rises to the level of a "key point". It's also disjointed. Why is GRIS irrevesibility in the same paragraph as Antarctic basal melt. I know, it's a draft. Don't forget to fix this! [THOMAS Wagner, United States of America]	Taken into account in revised text.
52037	8	6	8	9	Why is there nothing about an ice-free Arctic Ocean in the "future warming between 2 and 3°C" section? Can we not at least say that the likelihood of ice free summer conditions will be much higher than for warming levels between 1.5 & 2.0? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now covered.
99151	8	6	8	9	It is hard to understand what the basis is for suggesting that the threshold is so high given the very large contribution of Greenland to sea level rise during the Eemian when the global warming was only about 1 C and the CO2 concentration was below 300 ppm, which is low enough to allow the major amounts of ice to shed heat they gained in summer back to space and so re-chill the ice sheet. For the situation that we have now and are heading to with the CO2 level at 500 ppm or more, there is much less capability for the ice sheet cooling in winter, so it will be softer and more prone to tlow. In my view the Eemian is just not a basis for expecting much of Greenland ice to remain with a global warming of 2 to 3 C. I see no basis, therefore, for the conclusion here and think that the risk of destabilization is more likely between 1 and maybe 1.5 C, if that high. [Michael MacCracken, United States of America]	Accepted. We now indicate the possibility of destabilisation for the lowest warming range discussed here.
129379	8	6	8	9	[RISK] So, between 1.5 to 2°C for two millennia is 1-3 m commitment but, if one goes to 2-3°C, it will be 7 m or so from Greenland, a couple due to thermal expansion and glaciers, and then some more from Antarctica. That is quite a threshold effect, jumping way up. With melting now occurring across all of the surface of Greenland for at least some summer days, and currently at 1°C global warming, how can there be any confidence that the threshold is above 2°C? This would seem to need a great deal of justification, especially given the role of moulins and that, as surface melts, the elevation of the top surface goes to lower altitudes and the albedo of this surface drops. There is considerabe uncertainty, but it cannot be said with any confidence that the tipping point is above 2°C as the conclusion for this section seems to imply. [Trigg Talley, United States of America]	Accepted. We now give specific numbers for equilibrium sea-level rise for all warming levels
111735	8	6			Please clarify whether this refers to sustained warming or peak warming. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now specify that this is all for sustained warming
15791	8	7	8	9	It is unclear what the sentence " There is high confidence that Antarctic ice shelf basal melting will increase, but low confidence in the projected melt rates." is intended to convey here. As already mentioned, the sub-ice-shelf melting does not contribute to sea level rise itself. Changes in it (both inceraese and descrease) might lead to increased ice discharged from the grounded part of ice sheet that does contribute to sea level. However, this sentence does not say that. As formulated, the sentence is misleading. [Olga Sergienko, United States of America]	Accepted. This has been re-written for clarity
62153	8	8	8	8	Across all regions of the Antarctic? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This has been re-written for clarity

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115155	8	8	8	9	Could this be rephrased to state the range of possibilities, rather than saying simply that there is low confidence in the melt rate? How could the melt rate here compare to the melt rate under other levels of global warming? [Natalya Gomez, Canada]	Accepted. We now give specific numbers for equilibrium sea-level rise for all warming levels
62119	8	11	8	11	The bolded statement for this paragraph is very quantitatively vague; "for decades or longer", what is the minimum cutoff for being included in such a statement? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now discuss all changes only in terms of sustained warming levels.
68439	8	11	8	12	In the extreme case where all Arctic sea ice is lost for the sunlit summer months, the added forcing in the Arctic would be 21 W/m2; averaged globally its equals 0.71 W/m2 of global forcing, compared to the 1.83 W/m2 added by anthropogenic emissions of CO2 since the industrial revolution. This warming is equivalent to one trillion tons of CO2 would be added on top of the 2.4 trillion tons added in the 270 years since the industrial revolution, which would be like advancing the 2ºC guardrail by 25 years. Pistone K., et al. (2019) Radiative Heating of an Ice-Free Arctic Ocean, GEOPHYSICAL RESEARCH LETTERS 46(13):7474–7480 ("During recent decades, there has been dramatic Arctic sea ice retreat. This has reduced the top-of-atmosphere albedo, adding more solar energy to the climate system. There is substantial uncertainty regarding how much ice retreat and associated solar heating will occur in the future. This is relevant to future climate projections, including the timescale for reaching global warming stabilization targets. Here we use satellite observations to estimate the amount of solar energy that would be added in the worst-case scenario of a complete disappearance of Arctic sea ice throughout the sunlit part of the year. Assuming constant cloudiness, we calculate a global radiative heating of 0.71 W/m2 relative to the 1979 baseline state. This is equivalent to the effect of one trillion tons of CO2 emissions. These results suggest that the additional heating due to complete Arctic sea ice loss would hasten global warming by an estimated 25 years. …We examine two perhaps unrealistically extreme future Arctic cloud scenarios: at one extreme, an ice-free Arctic Ocean that is completely cloud free and at the other extreme, an ice-free Arctic Ocean that is completely overcast. For simplicity, in the latter scenario we use distributions of cloud optical thickness based on present-day observations (see Appendix A). Both of these extreme scenarios are shown in Figure 2. The cloud-free, ice-free Arctic scenario results in a global radiative heating of 2.2 W/m2 compared with the 1979 baseline state, which is 3 times more than the 0.71 W/m2 baseline estimate derived above for unchanged clouds. The completely overcast ice-free Arctic scenario results in a global radiative heating of 0.37 W/m2, which is approximately half as large as the 0.71 W/m2 baseline estimate (Figure 2b). This suggests that even in the presence of an extreme negative cloud feedback, the global heating due to the complete disappearance of the Arctic sea ice would still be nearly double the already-observed heating due to the current level of ice loss."). [Durwood Zaelke, United States of America]	Noted. The Pistone (2019) study only examines the isolated radiative heating without considering the implied change in longwave radiative fluxes, which counter-act the numbers given here.
89391	8	11	8	20	Include potential instabilities of the Antarctic ice sheet which might be triggered and lead to persistent mass loss over centuries (e.g., Joughin et al., 2014, 10.1126/science.1249055) in this paragraph? [Ricarda Winkelmann, Germany]	Accepted. Such statement has been added.
83277	8	11	8	20	Again, what will happen to the Antarctic sea ice environment? Please add a statement here about this. [Robert Massom, Australia]	Noted. We have too little confidence in long-term projections to indicate anything at even medium confidence for Antarctic sea ice
99153	8	11	8	20	This seems to me just totally beyond the pale--being far too optimistic and totally inconsistent from what is shown by the paleo record. On winter snow cover, it may be that there will actually be more snowfall on the surrounding lands, akin to Laek Effects snow, as was suggested by Ewing and Donn back in the 1950s. But I see no way that sea ice and the Greenland ice sheet will be so persistent as indicated here. During the deglaciation from the LGM, sea level war rising a meter per century when warming was well below present and the rate of warming was 1 C per century and the CO2 concentration was less than 300 ppm. That the models may suggest a more optimistic situation is no reason for not giving primary credence to lessons from paleoclimatic analyses. I think this is just an irresponsible conclusion based on what is understood of how the Earth system works and the very different climatic conditions that have existed over Earth history. [Michael MacCracken, United States of America]	Accepted. We now indicate the possibility of Greenland destabilisation for the lowest warming range discussed here.
111737	8	11	8	20	I found this paragraph quite confusing. It refers to changes taking place over multiple timescales, and it wasn't clear whether it was talking about sustained warming for the whole time or commitments/tipping points if the threshold warming was passsed just for a few decades. I suggest the paragraph would benefit from being split up to clarify. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now discuss all changes only in terms of sustained warming levels.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40457	8	11		21	isn't the information given on p81 L51-53 absent from this paragraph? i.e:"The simulated snow cover decrease is close to a linear function of global temperature change for all months (Figure 9.26b), except when snow cover vanishes, which occurs at about +2°C of GSAT change above the1995-2014 level (that is, about +3°C above the preindustrial level)" That seems relatively important to me [TSU WGI, France]	Accepted. This has been added.
62295	8	12	8	12	Quantify "most years" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Most is intended to mean "more than 50 % of all years", with a more concrete statement not being possible given the uncertainty of the projections
32457	8	12	8	12	What do you means through several summer months? There are only 3 of them. Do you mean for at least 1 month of summer? [Robert Colman, Australia]	Accepted. We have removed "summer" from this sentence.
3085	8	12	8	13	The statement about Antarctic ice sheet mass loss holds true for other warming scenarios as well, and is not peculiar to the 3 to 5°C scenario, as seems to be suggested here. [Daniel Farinotti, Switzerland]	Accepted. This has been revised.
62297	8	12	8	15	Replace "most of the glaciers in the world will very likely melt" with "most of the glaciers in the world will very likely melt completely" or something similar. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now write "disappear"
3087	8	13	8	13	"acceleration" of what? [Daniel Farinotti, Switzerland]	Accepted. This phrase has been removed.
89393	8	13	8	15	"The Greenland ice sheet will disappear after multiple millennia; most of the glaciers in the world will very likely melt […]" - Indicate that the ice loss from glaciers would happen on much (!) shorter timescales (in contrast to "after multiple millennia" for Greenland ice loss). [Ricarda Winkelmann, Germany]	Accepted. We now discuss all changes only in terms of sustained warming levels and have removed the time information
3089	8	14	8	14	the reference to "multiple millennia" seems slightly disconnected with the paragraph's bold initial sentence (L. 11) which refers to "decades or longer". [Daniel Farinotti, Switzerland]	Accepted. We now discuss all changes only in terms of sustained warming levels and have removed the time information
103783	8	14	8	14	Greenland ice sheet -> Greenland Ice Sheet [Philippe Tulkens, Belgium]	Accepted. Changed as suggested.
80819	8	14	8	14	Greenland ice sheet -> Greenland Ice Sheet [Louise Sandberg Sørensen, Denmark]	Accepted. Changed as suggested.
2049	8	14	8	15	provide a time horizon for this statement [Matthias Huss, Switzerland]	Noted. We now discuss all changes only in terms of sustained warming levels and have removed the time information
97965	8	14	8	15	"most of the glaciers of the world will very likely melt, only the largest glaciers with persist at 10-20% of their present size" In an earlier summary for glaciers the statement p6, lines 38-39 "Under continued warming glaciers will be lost globally.." these statements appear to contradict [Paul Durack, United States of America]	Accepted. Text revised to clarify, the proportion of total mass for different warming levels and regional information has been added
115157	8	14	8	15	Is the confidence missing on the Greenland ice sheet statement? I currently read it as virtually certain. And I also read that, the Greenland ice sheet will dissappear but the large glaciers will persist at 10-20% their current size after multiple millennia. Is that the intended meaning? [Natalya Gomez, Canada]	Accepted. Text revised to clarify, the proportion of total mass for different warming levels and regional information has been added
129381	8	14	8	19	These estimates of change seem very low given how much change is occurring at 1°C. It would be helpful to explain what "winter" and "spring" snow cover mean? For most people in the NH, spring is from March 20 to June 20 (roughly), so is this saying snow cover for this quite warm period really is going to be present at all at 3-5°C warming? [Trigg Talley, United States of America]	Noted. Meteorological northern hemisphere spring, the one relevant for climate, is defined March-May, and this is also specified in the figure in Chapter 9 this statement refers to. At high latitudes, quite a lot of snow still remains in May, and Figure 9.24b shows the mean spring snow extent still is about 40% of present at +5°C GSAT with respect to 1995-2014.
34683	8	14	8	19	Again, I think it's just a bit sensational to make statements about conditions two millenia into the future in a key message. [Russell Vose, United States of America]	Noted. We now discuss all changes only in terms of sustained warming levels and have removed the time information
3091	8	15	8	15	Is "melt" meant to say "melt completely"? [Daniel Farinotti, Switzerland]	Accepted. We now write "disappear"
103785	8	15	8	15	should it rather be that they will 'disappear' instead of 'melt'? [Philippe Tulkens, Belgium]	Accepted. We now write "disappear"
99701	8	15	8	15	"melt" - most glaciers melt (part of their mass balance). Do you mean they will disappear? [Peter Clark, United States of America]	Accepted. We now write "disappear"
62069	8	15	8	15	What is meant by "largest glaciers"? Would be useful to have an approximation of size threshold here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The text now clarifies this.
99475	8	15	8	15	Should "will"be replaced by "are likely to" or some similar likelihood statement (or is it certain)? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised to clarify, the proportion of total mass for different warming levels and regional information has been added
80821	8	15	8	15	should it rather be that they will 'disappear' instead of 'melt'? [Louise Sandberg Sørensen, Denmark]	Accepted. We now write "disappear"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16321	8	15	8	15	Probably want an “and” after the comma (“...melt, AND only the largest glaciers...”) [Julian Mak, China]	Accepted. Changed as suggested.
69573	8	15	8	15	"most of the glaciers in the world will very likely melt, only the largest glaciers will persist at 10-20% of their present size" Again, the second clause appears based only on one study (Marzeion et al., 2012) and so I worry about the fragility of the statement, and again it is unclear whether the "very likely applies to the whole sentence or just the first part." [gerard Roe, United States of America]	Accepted. Text revised to clarify, the proportion of total mass for different warming levels and regional information has been added
55081	8	15	8	16	The finding here about changes in snow cover in the future (for warming levels of 3-5 °C) is unclear. Future winter snow cover is expressed in terms of absolute extent (40 million km2) without any context about current extent or changes in extent. In contrast, future spring snow cover is expressed in terms of a change (decrease) in extent of 6 million km2 (again, with no information about how significant this change is relative to current spring snow cover). Perhaps % changes in extent would be more informative to include here, if possible. [Nancy Hamzawi, Canada]	Noted. Percent changes are given in the 9.5 ES statements. Here absolute numbers are given on purpose to provide a more comprehensive overall information for the reader.
71997	8	15			glaciers will disappear [John Church, Australia]	Accepted. We now write "disappear"
101689	8	16	8	16	"40 10^6 km^2" and "6 10^6 km^2" are values that are difficult to understand and put in context for a non-expert reader. Is there a way to convert this to a percentage change? [IAPSO ECS group review, United States of America]	Noted. Percent changes are given in the 9.5 ES statements. Here absolute numbers are given on purpose to provide a more comprehensive overall information for the reader.
18765	8	16	8	16	Quoting the change in snow cover extent in terms of % will enhance understanding. [Govindasamy Bala, India]	Noted. Percent changes are given in the 9.5 ES statements. Here absolute numbers are given on purpose to provide a more comprehensive overall information for the reader.
129383	8	16	8	16	Here is context for the earlier assertion that the area of snow cover decline will not exceed 3M km2 on page 7, line 55. So the assertion is that for 1.5 -2°C warming we'll see less than 10% reduction in winter snow cover. [Trigg Talley, United States of America]	Noted. Percent changes are given in the 9.5 ES statements. Here absolute numbers are given on purpose to provide a more comprehensive overall information for the reader.
35057	8	16	8	16	These expressions of the area of N Hemisphere snow cover need to expressed as % of present values OR the present day values need to be stated. [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Percent changes are given in the 9.5 ES statements. Here absolute numbers are given on purpose to provide a more comprehensive overall information for the reader.
88247	8	17	8	17	Same comment as above - thawing of permafrost will continue below 3 m and this is not considered in this statement. [Sharon Smith, Canada]	Taken into account. This is explained in detail in the main chapter text, including the limitations of models, and therefore not misleading. Text of the ES revised to clarify this as much as possible within available space
71177	8	17			Considering comments under Nr. 20, I have may severe doubts in the 60% which is reported as (medium confidence) as thaw strains are ignored. [Lukas Arenson, Canada]	Taken into account. This is explained in detail in the main chapter text, including the limitations of models, and therefore not misleading. Text of the ES revised to clarify this as much as possible within available space
39181	8	18	8	19	is this the worst-case scenario? [Lourdes Tibig, Philippines]	Noted. This statement has been removed from this paragraph as we only focus on long-term changes now
281	8	19	8	19	is 0.85 m the real upper limit for this scenario? I thought it was higher? [THOMAS Wagner, United States of America]	Noted. This statement has been removed from this paragraph as we only focus on long-term changes now
61477	8	19	8	19	I doubt if it is appropriate to say ''between 1996-2014 and 2100'' since '1996-2014' is a duration rather a time. Maybe it is better to specify a particular year. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This statement has been removed from this paragraph as we only focus on long-term changes now
49969	8	19	8	19	For clarity, should add something like "from all sources" after "committed sea level" [Daniel Gilford, United States of America]	Rejected. We do not find that this is necessary.
3145	8	19	8	19	“1996-2014”should be changed to "1995-2014" [Hui Wang, China]	Noted. This statement has been removed from this paragraph as we only focus on long-term changes now
103787	8	19	8	19	sea level -> GMSL [Philippe Tulkens, Belgium]	Accepted, we now write "global mean sea-level rise"
67183	8	19	8	19	will be' is too strong for a low confidenct statement. Formulation should be weakened. [Regine Hock, United States of America]	Accepted. We have revised the text accordingly.
11153	8	19	8	19	I doubt if it is appropriate to say ''between 1996-2014 and 2100'' since '1996-2014' is a duration rather a time. Maybe it is better to specify a particular year. [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This statement has been removed from this paragraph as we only focus on long-term changes now
80823	8	19	8	19	sea level -> GMSL [Louise Sandberg Sørensen, Denmark]	Accepted, we now write "global mean sea-level rise"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29639	8	19	8	19	again, here period 1996-2014 is used. Maybe we should use 1995-2014 consistently [Aixue Hu, United States of America]	Noted. This statement has been removed from this paragraph as we only focus on long-term changes now
41449	8	19	8	19	This is hopefully only a typo, please change to 1995-2014. [Alexander Nauels, Germany]	Noted. This statement has been removed from this paragraph as we only focus on long-term changes now
99477	8	19	8	20	Should there be a reference to deep uncertainty here? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised the text accordingly.
114869	8	19	8	20	The commitment to sea-level rise is stated as 16 m -- this is an extremely uncertain number -- it is important this uncertainty is communicated to a reader -- please think about this carefully. [Robert Nicholls, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised the text accordingly.
71999	8	24		30	In my view, the warning about longer term projections are not strong enough. I suggest making the longer term beyind 2100 a separate dotpoint. And rther than just saying there is deep uncertainty, start the dot point with something like metres over centurues for the high emission scenarios, then discuss the individual projections, including the Antarctic and Greenland potential. I would also consider saying something about potential higher sea level rise is the latter 21st century for high emission scenarios. [John Church, Australia]	Taken into account. Scenarios presented beyond 2100, and for sustained warming levels are provided, balancing various review comments.
18763	8	55	8	55	Quoting the change in snow cover extent in terms of % will enhance understanding. [Govindasamy Bala, India]	Noted. Percent changes are given in the 9.5 ES statements. Here absolute numbers are given on purpose to provide a more comprehensive overall information for the reader.
14587	9	1	110	31	Ch 9 fourth level subsections (9.X.X.X) are rather long and contain a lot of information. It would help the reader if at the end of each fourth level section a short summary para (in bold) were to be included. [Roshanka Ranasinghe, Netherlands]	Rejected. This is limited by length and the number of topics covered. Assessments are at the end of paragraphs.
83279	9	3	9	9	Provide a statement here as to how this chapter 9 ties in with and complements Chapter 2. [Robert Massom, Australia]	Noted. This part can be seen in figure 9.1.
67185	9	3	9	9	What I miss here is a clear positioning of this chapter with respect to SROCC. What is different to SROCC? Which topics are covered here that were not covered in SROCC. Different foci? Topics? [Regine Hock, United States of America]	Accepted. This section discusses the advances since SROCC and clear traceability is throughout the chapter.
99479	9	4	9	5	Observations should be included here, e.g. ".. Understanding of observed, attributed and projected future changes since AR5 ..." [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Section discusses observational advances
283	9	5	9	9	This really isn't an introduction, it's an explanation for the how the system works. Seems like the last two sentences should be framed that way. But I don't know that they are neeeded. [THOMAS Wagner, United States of America]	Taken into account. This section now sets the scene in the context of the report linking to chapter 1 and describes the advances since SROCC.
15793	9	6	9	6	Ice shelves are integral parts of the ice sheets, and cannot exist on their own. This sentence suggests otherwise. [Olga Sergienko, United States of America]	Accepted. Ice-shelves deleted from the section but discussed particularly in 9.4.2
85209	9	7	9	7	Is it worth mentioning here terrestrial freshwater storage changes in lakes, rivers, ground water, etc? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The suggested aspects are part of Chapter 8. Briefly referred to in Box 9.1
129385	9	8	9	8	There's also solid-ice discharge into the oceans that causes sea-level rise. Use "ice mass loss" instead of "melting". [Trigg Talley, United States of America]	Accepted. This is in box 9.1
3287	9	8	9	8	There's also solid-ice discharge into the oceans that causes sea-level rise. 'Ice mass loss' instead of 'melting'? [Thomas Frederikse, United States of America]	Accepted. This is in box 9.1
20975	9	8	9	8	Delete seawater replace with ocean [Ladislaus Chang'a, United Republic of Tanzania]	Noted but relevant text was deleted for FGD.
20977	9	8	9	9	Delete augmented by changes in the storage of water on land, [Ladislaus Chang'a, United Republic of Tanzania]	Accepted. Deleted
83281	9	11	9	11	Paleo reconstructions are not strictly observations - they are proxy data that generally have greater uncertainty than direct observations, while nonetheless being crucially important in extending accurate observations back in time.. [Robert Massom, Australia]	Noted but relevant text was deleted for FGD.
96877	9	11	9	35	The two paragraphs on recent observations and paleo evidence are a bit inhomogeneous, in that the paleo paragraph goes more into details and includes citations. We recommend homogenization. [Nicole Wilke, Germany]	Noted. The section has been rewritten and the balance reflects the chapter content.
12447	9	11	9	51	This is too brief for a broad topic, so not very helpful. I saw quite some assessments already in chapter-1 in a more thorough way. So my recommentation is: remove and merge the observation/model advances into sections afterwards during the assessment each parameter. This strategy has some merits: (1) reduce words here. (2) Help audience better understand the observational/model improvements behind of the scientific understanding improvement. [Lijing Cheng, China]	rejected. It is crucial to mention key progress since AR5 and SROCC. The reviewer is not clear about 'too brief' or 'too wordy"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67187	9	12	9	12	Advances since when? Since SROCC? [Regine Hock, United States of America]	noted, Advances particularly since SROCC are described in this sections.
100147	9	12	9	13	Advances in instrumental observations have improved understanding of key processes and the state of the ocean and cryosphere assessed in this chapter. [Carlye Peterson, United States of America]	Taken into account. This has been given in earlier Chapters like Chapter-1 and linked to here.
15795	9	12	9	13	Remove "assessed in this chapter". [Olga Sergienko, United States of America]	Taken into account. The section has been revised.
17917	9	12	9	17	Advances in observational capabilities are allowing us to improve knowledge of the state of the ocean and cryosphere and understanding of key processes occurring within them. Ocean observations have increased in both spatial and temporal resolution, including satellites measuring surface ocean properties (such as temperature, salinity, and colour), biogeochemical sensors mapping ocean changes at all depths (for example, pH and dissolved oxygen) installed on ships, moorings, gliders, autonomous vehicles (AUVs) and Argo floats, and ocean circulation monitoring programmes such as those measuring the Atlantic Meridional Ocean Circulation (AMOC). [Patricia Lopez Garcia, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been revised talking about observational advances and links to chapter 1.
91085	9	12	9	21	Somewhere it is important to explain the difference between mass balance and sea level equivalent (SLE). Not so important for the GrIS but important for the WAIS and marine sectors of the EAIS. Too many satellite studies are really sloppy on this and equate the two. It really matters for WAIS sea level contribution. Not sure how consistently it's treated in modelling studies but much easier to handle in those than EO ones. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This point is addressed as required in the text.
96879	9	13	9	13	The SST measurements should please be included here, they are for instance most valuable to infer changes in ocean circulation and sea level. [Nicole Wilke, Germany]	Noted. SST is assessed in section 9.2.1.1 and we don't discuss in detail in 9.1 any longer
100155	9	13	9	17	Re-written: Ocean observation spatial and temporal coverage is improved by ocean circulation monitoring programs (i.e., measuring the Atlantic Meridional Ocean Circulation (AMOC)), satellites (mapping surface ocean properties, e.g. temperature, salinity, colour), mooring buoys and Argo floats (mapping changes across depth), and ship-board biogeochemical (e.g., pH, DO) sensors. [Carlye Peterson, United States of America]	Taken into account. The text has been revised talking about observational advances and links to chapter 1.
69073	9	13	9	17	This sentence speaks about the increase in ocean observastions (spatial and temporal). While the absoulte nubmer of observations has increased, many regions are still poorly sampled. In addtion, many observational program are under serious stress due to flat or decreases in funding, as example the RAPID AMOC monitoring system was recently not funded. These pressure need to be mentioned - the current observing system is not sustained and indeed increased observations in the deep ocean, marginal seas, and biogoechemistry are not budgeted for in many national-led observing program. [Bernadette Sloyan, Australia]	Noted. Gaps are described in parameters' own sections and the final statement of uncertainty of the chapter
35059	9	13	9	17	This list of improvements in observations is inconsitent and ambiguous. Suggest reword to Ocean observations have increased in both spatial and temporal coverage. These improvements have come from satellites measuring surface ocean properties (e.g., temperature, salinity, colour), sensors on ships, moored buoys and surface drifters and from Argo floats mapping ocean changes across depths and including biogeochemistry (e.g., pH, dissolved oxygen).. There are now ocean circulation monitoring programs such as those measuring the Atlantic Meridional Ocean Circulation (AMOC). [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Section has been rewritten and links to chapter 1 on observational advances.
61487	9	14	9	14	In addition to temperature, salinity, and colour, satellites measure sea surface height as well. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted but relevant text was deleted for FGD.
55083	9	15	9	15	Suggest change: "..biogeochemical (e.g., pH, dissolved oxygen) sensors in ships, mooring buoys..." to ".biogeochemical (e.g., pH, dissolved oxygen) sensors deployed from ships, mooring buoys..." [Nancy Hamzawi, Canada]	Noted. Relevant text was deleted for FGD.
100149	9	15	9	15	...ship-board biogeochemical (e.g., pH, dissolved oxygen) sensors [Carlye Peterson, United States of America]	Noted. Relevant text was deleted for FGD.
83837	9	15	9	15	biogeochemical (e.g., pH, dissolved oxygen), compared to pH and DO, the term of "dissolved organic carbon" is a more representative component within biogeochemical processes [Nianzhi Jiao, China]	Noted. Relevant text was deleted for FGD.
82869	9	15	9	16	I suggest to consider also listing moored instruments (not only buoys) and other autonomous instrumentation (e.g. gliders) in this overview. [Sebastian Gerland, Norway]	Taken into account. Section has been rewritten and links to chapter 1 for observational advances.
101691	9	17	9	17	Should be “Meridional Overturning Circulation” (not “Meridional Ocean Circ.”) [IAPSO ECS group review, United States of America]	Noted. Relevant text is deleted for FGD.
14681	9	17	9	17	Create separate sentence-long summaries of observational advances for sea ice and ice sheets. [Jeremy Fyke, Canada]	Noted. Relevant text is deleted for FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14683	9	17	9	17	Add separate sentence-long summaries of observational advances for glaciers and permafrost. [Jeremy Fyke, Canada]	Noted. Relevant text is deleted for FGD.
14685	9	17	9	17	For each new sentence described above, highlight 1-2 key measures which observations provide. This will mirror the ocean-specific previousl sentences. [Jeremy Fyke, Canada]	Taken into account. Section has been rewritten and the balance reflects the chapter content.
100151	9	17	9	18	Similarly, improved retrieval algorithms and satellite data have enhanced sea ice and ice sheet observations. [Carlye Peterson, United States of America]	Taken into account. Section has been rewritten and links to chapter 1 for observational advances.
15797	9	18	9	18	Replace "data and improved retrieval algorithms" with "platforms" [Olga Sergienko, United States of America]	rejected. 'Platform' is too vague for readers outside satellite community
129387	9	18	9	19	Note the improvements in GPS data processing and its role in the sea-level observation network. Sea level can be measured and geocentric sea level separated from local land motion using GPS (e.g., Larson et al., 2017: A 10-Year Comparison of Water Levels Measured with a Geodetic GPS Receiver versus a Conventional Tide Gauge). Since AR5, due to GPS data, there are more robust global sea-level reconstructions (Dangendorf et al., 2017: Reassessment of 20th century global mean sea level rise), and VLM explains regional sea-level deviations (e.g., Frederikse et al., 2019: The imprints of contemporary mass redistribution on local sea level and vertical land motion observations, but there are many more studies). [Trigg Talley, United States of America]	Noted. Although not referred to in the introduction advances from Dangendorf and Frederikse papers are referred to in 9.6.1
3289	9	18	9	19	It may be an idea to note the improvements in GPS data processing and their role in the sea-level observation network? We can now measure sea level and separate geocentric sea level from local land motion using GPS (For example Larson et al. 2017: A 10-Year Comparison of Water Levels Measured with a Geodetic GPS Receiver versus a Conventional Tide Gauge). Since AR5, due to GPS data, we now have more robust global sea-level reconstructions (Dangendorf et al. 2017: Reassessment of 20th century global mean sea level rise), and we now know that VLM explains regional sea-level deviations (for example Frederikse et al. 2019: The imprints of contemporary mass redistribution on local sea level and vertical land motion observations, but there are many more studies) [Thomas Frederikse, United States of America]	Noted. Although not referred to in the introduction advances from Dangendorf and Frederikse papers are referred to in 9.6.1
8985	9	19	9	19	The advances are described too shortly. We have new satellites, longer times series, more techniques and the signal has gotten larger. All these important elements are diluted. The observation record is not getting its fair share of its importance in this assessment and state of knowledge. [Eric Rignot, United States of America]	Taken into account. The introduction draws heavily on chapter 1 now which has more detailed discussion. The improvements due to observations are also highlighted in the sections
15799	9	19	9	19	Replace "data" with "instruments". [Olga Sergienko, United States of America]	Noted. Relevant text is deleted for FGD.
85211	9	20	9	20	Is it worth mentioning remaining observational limitations here, including errors in heat and salt surface fluxes and budgets and even basic T-S variations for poorly observed regions such as polar regions (especially in winter), the deep ocean and narrow boundary and frontal currents? Also for satellite observations many measurements are adversely affected by cloud and/or rain, which could be expected to result in sparse sampling and introduce aliasing for regions where cloud and rain occur much of the time, such as the Southern Ocean or boundary current regions and perhaps the storm tracks? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	noted, this detailed descriptions are given in the following sections
100157	9	21	9	27	Re-written: Understanding past ocean (e.g., sea-surface temperature, salinity, ocean heat content, thermohaline circulation), cryosphere (e.g., Arctic sea ice area, Antarctic ice shelves), and sea level (Annex II) changes prior to instrumental records is established with paleoclimate model simulations and reconstructions. Particularly relevant to this chapter are paleoclimate reconstructions from the Little Ice Age (LIA), the Last Millennium, mid-Holocene (MH), Last Glacial Maximum (LGM), Last Interglacial (LIG), Mid-Pliocene Warm Period (MPWP) and Early Eocene Climate Optimum (EECO) (Cross-Chapter Box 2.1). [Carlye Peterson, United States of America]	Noted. The text has been rewritten for FGD.
67189	9	21	9	51	here and elsewhere, there are far to many acronyms. Please spell out LIA, MH, LIG etc. While well-established in the discipline-specific communities, this report is for a broader audience and becomes unreadable with the excessive use of acronyms that pervails throughout this chapter and report. There is great potentatial to shorten through more concise and succinct formulation if space is an issue. [Regine Hock, United States of America]	accepted. Acronyms are avoided throughout chapter where possible
33073	9	22	9	22	Paratentz has been written twice [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. Text no longer in the chapter.
79509	9	22	9	22	Paratentz has been written twice (commrent by: darvishi.khatooni@gmail.com) [Hanieh Zargarlellahi, Iran]	Not applicable. Text no longer in the chapter.
32743	9	22	9	22	Paratentz has been written twice [sadegh zeyaeyan, Iran]	Not applicable. Text no longer in the chapter.
20981	9	22	9	22	Delete past Ocean,replace with ocean circulation systems and properties [Ladislaus Chang'a, United Republic of Tanzania]	Noted. Text has been revised for FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
151	9	23			Suggest adding land ice to the list, cryosphere is not just sea ice and ice shelves. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section draws on links to chapter 1 describing observational advances
20985	9	24	9	24	After sea level add variation [Ladislaus Chang'a, United Republic of Tanzania]	Noted. Text has been rewritten so no longer applicable.
30729	9	24	9	26	The uncertainty term for MHW *very high confidence" need to be supported by literatures. It is not provided in Cross Chapter Box 9.1. [Iskhaq Iskandar, Indonesia]	Noted. Box 9.2 assesses marine heatwaves
100153	9	24	9	27	particularly relevant to this chapter are paleoclimate reconstructions from the Little Ice Age (LIA), the Last Millennium, mid-Holocene (MH), Last Glacial Maximum (LGM), Last Interglacial (LIG), Mid-Pliocene Warm Period (MPWP) and Early Eocene Climate Optimum (EECO) (Cross-Chapter Box 2.1) [Carlye Peterson, United States of America]	Noted. Text has been rewritten for FGD.
100711	9	25	9	25	Add: "...(LIG), Miocene Climatic Optimum (MCO), Mid-Pliocene..." [Matthew Kohn, United States of America]	rejected, MCO is not included here, see details in following sections
88577	9	28	9	28	Graversen and Langen, 2019 - Reference not found. [Rosemary Vieira, Brazil]	Noted. References have been checked thoroughly
49971	9	28	9	29	Please include the citation to Gilford et al. (submitted in 2019, currently under minor revision), which details how improved LIG observational constraints could better inform future change in Antarctic mass loss and global mean sea-level rise: Daniel M. Gilford, Erica L. Ashe, Robert E. Kopp, Robert M. DeConto, David Pollard, Alessio Rovere, 2019: Can the Last Interglacial Constrain Projections of Future Antarctic Ice Mass Loss and Sea-level Rise? JGR-Earth Surface (in revision Feb. 2020). preprint: https://www.essoar.org/doi/10.1002/essoar.10501078.1 [Daniel Gilford, United States of America]	Noted. This is assessed in 9.4
88579	9	29	9	29	Smith et al., 2019 - Reference not found. [Rosemary Vieira, Brazil]	Noted. References have been checked thoroughly
101693	9	29	9	29	We find it confusing that there are "Cross-Chapter Boxes" and "Boxes" in the same chapter. Perhaps page numbers should be included in parenthetical notes referring to these (e.g., Box 9.2, p. 56) [IAPSO ECS group review, United States of America]	rejected, Cross-Chapter Box and Box are different types
49973	9	29	9	31	Please cite Gilford et al. (2019) and Edwards et al. (2019) whose results strongly indiciate that current observations of the LIG (and the even less certain Pleicone) are not very informative for Antarctic contributions to sea-level rise. [Daniel Gilford, United States of America]	Noted. This is assessed in 9.4
101695	9	31	9	34	"Models and geological reconstructions nonetheless consistently indicate that ice sheet equilibrium responses to warming are substantially greater (in terms of mass loss) than transient (decadal to centennial) changes, suggesting that the ice sheets are currently in disequilibrium with the warming climate." - From the current phrasing, it is not entirely clear why the models and geological reconstructions suggest that the ice sheets are currently in disequilibrium. Is it because the current rates of change exceed the transient changes of the past? [IAPSO ECS group review, United States of America]	Taken into account. Section has been revised and refers to the long timescales.
61505	9	31	9	34	This statement seems to be lacking a confidence statement [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	rejected, the introduction section does not include confidence statements
100159	9	31	9	35	Re-write: Yet models and geological reconstructions consistently indicate substantially greater ice sheet equilibrium responses to warming (i.e., mass loss) than to transient changes (decadal to centennial), which suggests ice sheets are currently in disequilibrium with the warming climate. Hence it will take thousands of years for the slower responding elements of the ocean and cryosphere to reach a new equilibrium state (Clark et al., 2016). [Carlye Peterson, United States of America]	Taken into account. Section has been revised and refers to the long timescales.
61489	9	34	11	35	Delete " including the slower responding elements of the ocean and cryosphere" or reword [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text has been rewritten for FGD
41451	9	37			Would it make sense to also highlight modelling advances regarding future SLR? Since AR5, e.g. process-based emulators like Kopp et al 2014 and Nauels et al 2017 have been developed to fill the gap between process-based modelling and semi-empirical approaches, for example facilitating the post-2100 SLR outlook. [Alexander Nauels, Germany]	Noted. Advances in the sea level methodology and coherency across the report are highlighted and discussed in more detail in 9.6.3
52039	9	38	9	41	The wording here is a bit inaccurate. For example the SSPs are explicitly mentioned as being part of DECK but ScenarioMIP is also mentioned in the list of additional MIPs. Also the MIP list does not include SIMIP. If the list is just about additional MIP experiments then it's ok to leave out SIMIP (but "experimnents" should be added), otherwise SIMIP should be included. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Section links to more detailed descriptions in chapter 1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
97969	9	38	9	41	The comments about the CMIP6 DECK, SSPs etc are a little garbled. CMIP6 is comprised of 22 activities (or activity_id's) in which the core/DECK simulations are part of CMIP6 CMIP activity, and the future scenarios e.g. ssp585 are part of the ScenarioMIP activity, see https://wcrp-cmip.github.io/CMIP6_CVs/docs/CMIP6_experiment_id.html for more details. This could be solved by "In addition to CMIP6 DECK (Eyring) experiments, we also leverage CMIP6 ScenarioMIP future scenarios (SSPs; O'Neill) along with simulations from the full suite of CMIP6 activities (HighResMIP, OMIP, FAFMIP, etc)" [Paul Durack, United States of America]	accepted. Section links to more detailed descriptions in chapter 1
100161	9	39	9	39	In general, use the word "use" rather than "utilizing/utilising" for brevity and reading ease. [Carlye Peterson, United States of America]	accepted. Text has been improved.
79659	9	39	10	13	Permafrost and Peatlands- Modelling studies have found that that areas where the peat production was initially hampered by permafrost and low plant productivity due the cold climate conditions would benefit from the precipitation rate and greater CO 2 levels. On the other hand, areas which experience reduced precipitation rates and without permafrost will experience moisture stress conditions and lose more carbon in the near future, particularly, peatlands located in the European region and between 45–55°N latitude. Drier conditions promote shrub expansion to the north affecting the plant litter composition and peat quality (Chaudhary etal. 2020 and 2017) Glob Change Biol. 2020; 00: 1– 15. https://doi.org/10.1111/gcb.15099; https://onlinelibrary.wiley.com/doi/abs/10.1111/gcb.15099 [Nitin Chaudhary, Sweden]	Noted. This point is on the role of permafrost in the global carbon cycle which is not treated in this chapter. The paper suggested by the reviewer is cited in Chapter 5 on biogeochemical cycle
5553	9	39			Need to define the coastal extremes. The coastal extremes depend of storm surges, tide, wave combined with the SLR, but also the river flood for the estuairies, tidal rivers and all coastal rivers [Benoit Laignel, France]	accepted. This is in box 9.1 and 9.6.4.
100163	9	40	9	41	It seems that Annex III only includes information on CMIP6 DECK, CORDEX, and ScenarioMIP, but not the other modeling efforts included in this sentence, such as HighResMIP, OMIP, FAFMIP, SIMIP, PMIP4, ISMIP6, and GlacierMIP. [Carlye Peterson, United States of America]	Noted. The text refers to chapter 1 and Annex II for modelling efforts.
19255	9	41	9	41	The table of MIPS is table 1.4 in chapter 1. Annex III documents the models. [Anne-Marie Treguier, France]	accepted. Chapter 1 referred to for MIPs.
100165	9	43	9	43	"modeling results" rather than "modeling outputs"? [Carlye Peterson, United States of America]	Noted. Text has been rewritten for FGD.
83539	9	43	9	43	It might be smarter to list the paleo-periods in age order like it is done in lines 25-26, i.e. refer to LGM prior to LIG. [Antje H. L. Voelker, Portugal]	rejected, the order is right
100167	9	44	9	51	Re-written: In this chapter, a novel advance is the enhanced ocean resolution of models in the HighResMIP and OMIP2 experiments (section 9.2). Most DECK experiments parameterize mesoscale eddies and use ocean resolutions of approximately 1 degree which does not resolve the Rossby radius (the length scale of the most energetic form of ocean internal variability, mesoscale eddies, and also the typical scale of boundary currents) (Hallberg, 2013; Hewitt et al., 2017). HighResMIP and OMIP2 include models with ocean resolutions of approximately 1/4 degree (where mesoscale eddies are present) and 1/10 degree (where simulations are rich with mesoscale eddies) (Griffies et al., 2015; Chassignet et al., submitted). [Carlye Peterson, United States of America]	Noted. Text has been rewritten.
97971	9	45	9	45	OMIP2 is an experiment which is part of the OMIP MIP/activity_id, whereas HighResMIP is a MIP/activity, so there is some ambiguity here [Paul Durack, United States of America]	Taken into account. We refer to chapter 1 for MIPs.
99931	9	46	9	48	"DECK experiments have ocean resolutions of approximately one degree which does not the resolve the Rossby radius (the length scale of the most energetic form of ocean internal variability, mesoscale eddies, and also the typical scale of boundary currents)." Would it be possible to include the actual size of the Rossby radius, which varies with latitude, but is approximately 10 km to 200 km, to give the reader a better grasp of the DECK experiments resolution? [Dan Helman, United States of America]	rejected, the textbook-like information is not suitable for the introduction which now focusses on advances
82871	9	46	9	50	I suggest to add also information in kilometers/kilometer ranges in addition to degrees (resolution of models). [Sebastian Gerland, Norway]	rejected, the textbook-like information is not suitable for the introduction which now focusses on advances
23995	9	51	9	51	write "... rich with mesoscale and submesoscale eddies ...", instead of "... rich with mesoscale eddies ..." [Moacyr Araujo, Brazil]	noted. Text has been rewritten for FGD.
68917	9	53	9	54	“Lowest since at least 1850” is an understatement. AR5 concluded that Arctic summer sea ice extent is the lowest in 1450 years and SROCC might have extended this further. In addition, the informative text about paleo sea ice extent in 9.3.1.1 suggests that the current condition is more unusual than is represented by the short instrumental record. Please revise this statement to reflect with is (and is not) known about paleo sea ice changes. [Darrell Kaufman, United States of America]	not applicable to 9.1. Sea ice is assessed in 9.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67881	9				Human activity has proved to be a cause of climate change since the mid-20th century. Some of the effects of human activity on marine ecosystems related to climate change include: rising sea levels caused by melting glaciers, decreasing pH, increasing sea water temperatures, and changing levels salinity. Human activities that affect changes in sea level are projected to contribute to more than 50% of the sea area by 2040. Given the high impact of these human activities, there is a need for more detailed explanation on which activities are most influential on the marine ecosystems in each region, because each region has different characteristics. This will help decision makers in identifying mitigation efforts to alleviate negative impacts of human activities. [Ruandha Agung Sugardiman, Indonesia]	Not Applicable. Text no longer included in Chapter.
67883	9				Global sea level rise was 0.19 m between the period 1900-2018, mainly caused by temperature rise and glacier melt. However, the impact can be felt in countries even located far from the melting glacier. Additional information and maps showing the locations impacted by rising sea levels is also necessary. [Ruandha Agung Sugardiman, Indonesia]	Noted. This information/maps is captured by Chapter 12 and the Atlas.
67885	9				Differences in sea conditions due to climate change in the tropics and non-tropics have been described in this document, which shows a trend of rising temperatures since 1950, except in the eastern tropical Pacific. Increased sea temperatures in the tropics in the long run are caused by anthropogenic factors. Indonesia has an important role in maintaining the global climate, because the oceans in the tropics are connected with the Indonesian Throughflow (ITF) or cross-country flows in Indonesia. This current allows the movement of water masses between the Pacific Ocean and Indian Ocean. Therefore, strengthening the ability of the mass movement of sea water is needed to stabilize sea surface temperature. [Ruandha Agung Sugardiman, Indonesia]	Noted. The Indonesian throughflow changes are assessed in 9.2.3
61517	10	1	10	6	the writing in the 'bubble' explaining coastal processes in the top graphic of Figure 9.1 is not readable [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted. Figure quality is improved.
20165	10	1	10	6	Figure 9.1 is packed with information. Please try to make the font in circular subplots larger. It is understood that the numbers indicate glacierized regions but what do they mean? do they refer to a list? Finally, the colour code "ocean(speed)" is enigmatic. [philippe waldteufel, France]	accepted. Figure quality is improved.
41453	10	3			This will be a very useful figure! Please revise legend colours as they are very hard to distinguish (ice sheet, snow, sea ice) and separate frozen water from the ocean as well as ocean currents because it is not clear atm that the green tone actually captures the speed. Please also magnify the two insets of the top schematic so that they are readable. [Alexander Nauels, Germany]	accepted. Figure quality is improved.
130615	10	9	10	22	This section could be removed. [Panmao Zhai, China]	Taken into account. Section 9.1 has been rewritten to focus on advances.
35061	10	10	10	10	Remove "The outline of" it is redundant [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text has been rewritten for FGD.
67191	10	10	10	25	This paragraph is a waste of space and should be deleted. Possibly a highly shortened version would fit much better in the introduction of this chapter. [Regine Hock, United States of America]	accepted, section has been rewritten to focus on advances since SROCC drawing also on chapter 1.
100169	10	10	10	26	Re-written: Chapter 9 focuses on understanding the processes driving changes in ocean, cryosphere, and sea level on timescales of decades to millennia (Sections 9.2 – 9.6). This chapter sits between the discussion of large-scale observed trends, human attribution, and future projections (Chapters 2 – 4), and regional perspectives (Chapters 10 – 12, Atlas). Ocean and cryosphere processes influence heat budget (Chapter 7), water cycle (Chapter 8), and carbon cycle (Chapter 5) changes. The Boxes explore themes within Chapter 9 and with other chapters, including: marine heatwaves (Cross-chapter Box 9.1), paleo insights to ice sheet processes (Box 9.2), closing energy and freshwater budgets (Cross-chapter Box 9.2). Large uncertainty in high-end sea level projections come from (1) appropriate conceptual models that describe relationships among key driving forces in a system; (2) the probability distributions used to represent uncertainty about key variables and parameters; and/or, (3) how to weigh and value desirable alternative outcomes (adapted from (Lempert et al., 2003; Abram et al., 2019; Marchau et al., 2019). Hence a storyline approach is adopted (Box 9.3) to assess climate and ice sheet processes linked to high- and low-end sea level projections linked to ice sheet processes. Section 9.7 highlights the statistical robustness and limitations to the assessment in this chapter. Other themes include the commitment to ongoing ocean, cryosphere, and sea level change due to the climate system’s long response times and changes expected at different warming levels. [Carlye Peterson, United States of America]	Taken into account. The section doesn't outline the structure of chapter-that is in figure 9.1
1709	10	17	10	23	It would be of value to discuss the measures necessary to decrease the uncertainty around high-end sea level projections that hinder the application of appropriate models. This information can be inserted after "processes." on line 23. [Michael Kennish, United States of America]	Noted. High end sea level is discussed in box 9.4

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20167	10	17	10	23	A closing parenthesis seems to be missing in this long sentence [philippe waldteufel, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
77781	10	17	10	23	This sentence is hard to digest because of the very long parethesis in it, which includes a list of bullet points! [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text has been rewritten
14687	10	18	10	22	These reasons for high uncertainty in high-end sea level projections miss the major reason that needs to be added, something like "coupled ice-sheet/Earth system interactions that cause ice sheets to lose mass are very complex and erquire sophisticated and/or very high resolution modelling that is not currently operational". Here, it is not so much that conceptual models aren't agreed upon - it's that these conceptual models are extremely hard to implement in practice. [Jeremy Fyke, Canada]	Noted. Box 9.4 and 9.7 focus on this and the problems with forcing for the models.
14689	10	18	10	22	I believe there is a missing closing bracket in this uncertainty-parsing statement - and in general, too many nested brackets [Jeremy Fyke, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
96881	10	18	10	22	The list of reasons for deep uncertainty given in the parenthesis is hard to read. Maybe drop enumeration and references since this is the chapter outline. [Nicole Wilke, Germany]	Noted. Text has been rewritten for clarity
16323	10	18	10	22	The bracket beginning with “i.e.” is very long and reads as a large digression. Consider moving that out as a sentence, possibly placed before “Given the deep uncertainty...” at line 17 (“i.e.” is then not needed). [Julian Mak, China]	Noted. Text has been rewritten for clarity
33413	10	18			Close parenthesis. [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
61479	10	22	10	22	Is it possible to add some brief explanation about 'storyline approach' since most readers may not be familiar with such method, (and the corresponding Box 9.3 is too far away). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Space is limited and this approach is introduced now in box 9.4
11155	10	22	10	22	Is it possible to add some brief explanation about 'storyline approach' since most readers may not be familiar with such method, (and the corresponding Box 9.3 is too far away). [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Space is limited and this approach is introduced now in box 9.4
23487	10	22	10	23	ice sheet processed linked to high and low-end sea level projections. delete - linked to ice sheet processes (after low-end sea level projections) [Saurabh Rathore, Australia]	Noted. Space is limited and this approach is introduced now in box 9.4
14691	10	26	10	27	"to the assessment in this 27 chapter. " -> "of assessments within this chapter" [Jeremy Fyke, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
100171	10	32	10	33	Figure 9.2 caption is a sentence fragment. Consider revising: Schematic outline of chapter 9 including links to relevant assessments in other chapters and common themes within this chapter. [Carlye Peterson, United States of America]	Noted. Figure captions have been revised
41455	10	32			That's a very helpful visual chapter guide, thanks! [Alexander Nauels, Germany]	Noted.
52031	10	38	12	24	I really enjoyed BOX9.1 on the key processes driving sea-level change. Generally speaking, there is much confusion about these processes and how they all fit together, so I think this a useful addition to AR6. My only comment is that perhaps it might be helpful to include some schematics. For example I found the small pictures in the Guardian article at https://www.theguardian.com/environment/ng-interactive/2018/sep/12/greenland-antarctic-ice-sheet-sea-level-rise-science-climate quite useful in this regard. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	[noted] Thank you very much for your compliment. With regard to the suggested graphics, the figure in this box (Figure 9.1) already captures these processes, so we have decided against adding separate graphics.
40965	10	41	10	41	The glossary for SROCC defined 'Local sea level change' as "Change in sea level relative to a datum (such as present-day mean sea level) at spatial scales smaller than 10 km." Regional sea level change was defined as "Change in sea level relative to a datum (such as present-day mean sea level) at spatial scales of about 100 km." Do you want to use these definitions in the WGI glossary? [TSU WGI, France]	[accepted] Yes.
1779	10	41	12	21	Let me stress at the outset that I see many strong elements in this chapter. Box 9.1 is an excellent example of this, one that I will likely use in my teaching once the report gets published. [Torbjorn Tornqvist, United States of America]	[noted] Thank you very much for your compliment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67193	10	41	12	22	again: there are far to many acronyms. Please spell out GrIS, AIS, SLC, ESL, VLM and many more. While well-established in the discipline-specific communities, this report targest a broader audience and becomes unreadable with the excessive use of acronyms that pervail throughout this chapter and report. There is great potentatial to shorten through more concise and succinct formulation if space is an issue. Often a short form can be used when context is clear, i.e. if entire subsection is about Greenland, 'ice sheet' is sufficient. [Regine Hock, United States of America]	[taken into account] Part of the purpose of this box is actually to define the acronyms that are used throughout the chapter, so we have maintained those definitions here. However, we have written out most of the acronyms in the sentences (where practical) so as to avoid excessive use of acronyms in this box, and we have removed the acronym SLC, as this is not used elsewhere.
285	10	43	10	43	This section could use the sentences referred to in my previous comment (re lines 5-9 on Page 9). It needs a few sentences on how it's all tied together. [THOMAS Wagner, United States of America]	[noted] The first sentences in this box serve a similar purpose as the sentences suggested by the reviewer. We have therefore not made any additional changes here.
65853	10	43	18	43	Suggest including a critical reference on the definition for marine heatwaves. Currently, the definition as listed cites the recent IPCC reports. However, it is important to include Hobday et al. 2016, which defines specifically that marine heatwaves are discrete, prolonged, anomalously warm water events. This reference has already been included later on in Box 9.1. Hobday, A. J., Alexander, L. V., Perkins, S. E., Smale, D. A., Straub, S. C., Oliver, E. C., ... & Holbrook, N. J. (2016). A hierarchical approach to defining marine heatwaves. Progress in Oceanography, 141, 227-238. [Kushla Munro, Australia]	Accepted. Reference included
88249	10	44	10	46	Considers isotstatic adjustments? [Sharon Smith, Canada]	[Rejected] This sentence is meant as a general introduction of the concept RSL, including the term isostatic adjustment would require the explanation of isostatic adjustment to be included here, which we don't think adds to the clarity and flow of the box.
32413	10	44			page 9-10 line 44 uses "solid earth", while 9-11 line 52 uses "solid Earth". Make consistence and check through documents. [Olaf Eisen, Germany]	[accepted] done.
129389	10	45	10	45	A more accurate definition of "RSL" is sea level relative to the underlying sea floor, or relative to the underlying solid Earth. That makes the definition easier for the open ocean when there's no land nearby. [Trigg Talley, United States of America]	[accepted] changed.
3291	10	45	10	45	A more accurate definition of 'RSL' is sea level relative to the underlying sea floor, or relative to the underlying solid Earth. That makes the definition easier for the open ocean when there's no land nearby. [Thomas Frederikse, United States of America]	[accepted] changed.
90453	10	45			would be more clear to say that RSL change is the change in local mean "sea surface height" (instead of "sea level") relative to the land? Or, define "sea level" earlier in the chapter? [Holly Kyeore Han, Canada]	[accepted] changed to sea surface height
114871	10	46	10	47	"This reference frame is most relevant when considering flood hazards" -- more general than this statement -- relative sea-level rise is the appropriate parameter for all impacts, hazards (e.g., flooding, erosion, salinisation, etc) and adaptation needs. And most relevant understates the point -- impacts and adaptation assessment require relative sea-level rise (e.g., Nicholls et al. 2014 and Nicholls et al. 2020 (accepted subject to revision) both WIRES CC). (e.g., doi: 10.1002/wcc.253) [Robert Nicholls, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] text changed to: This reference frame is used when considering coastal impacts, hazards and adaptation needs.
100055	10	47	10	47	"water column" should be "top of the water column" or "sea surface" [Glenn Milne, Canada]	[accepted] changed to sea surface height
100173	10	49	10	49	Re-written: Sea level change processes are summarized in this Box using standard terminology (Gregory et al., 2019). [Carlye Peterson, United States of America]	[taken into account] sentence changed to a more active form: This Box provides a brief summary of sea-level processes, using standard terminology
14693	10	55	11	1	Is it necessary to note that if sea level rise is large, one has to also take into account that the ocean surface area will expand (i.e. as coastal regions are flooded)? This is a mitigating factor that reduces the amount of sea level rise for a given unit of volume change - I'm frankly not aware of any calculation of this though. For that matter, are all high-end SLR predictions in this report assuming a constant ocean area? IS THIS A PERVASIVE PROBLEM IN SLC #s IN THIS CHAPTER? [Jeremy Fyke, Canada]	[Rejected] As noted in Gregory et al (2019): "For centennial timescales, we can assume the ocean surface area A is constant, with A=3.625 x10^14 m^2 (Cogley 2012). It is altered substantially by global-mean sea-level changes of many metres, such as on glacial–interglacial timescales or possibly over future millennia due to ice-sheet changes, and on geological timescales due to plate tectonics."
132543	10	55	11	10	I found this flow a bit confusing. Perhaps introduce steric sea level first (driven by changes in ocean density), then define thermosteric and halosteric, noting that both are important regionally and thermosteric is most important globally. [Kyle Armour, United States of America]	[accepted] changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12449	11	1	11	1	change to "through temperature and salinity changes" [Lijing Cheng, China]	[rejected] this sentence is about global mean SLR, where salinity is considered negligible (see Gregory et al, 2019, p1268)
40867	11	3	11	3	Suggest to define 'terrestrial water storage' in the glossasry [TSU WGI, France]	[noted] we have changed 'terrestrial water storage' to 'land water storage', for consistency with previous IPCC reports.
45273	11	5	11	10	It's a little bit confusing to title this paragraph as thermosteric sea level when both thermo- and halo- steric sea level rise were discussed. [Anson Cheung, United States of America]	[accepted] changed to 'steric sea level change'
107247	11	5		10	It says, "Thermosteric sea-level rise (or thermal expansion, Section 9.2) occurs as a result of ocean warming, which reduces the ocean density and increases the volume per unit of mass. On a local scale, ocean water density can also change due to salinity changes, called halosteric sea-level change: higher salinity leads to higher density and thus sea-level fall. While thermosteric sea-level rise is an important process on a global mean scale, halosteric change is only important on local scales. Steric sea-level change is the combination of thermosteric sea-level change and halosteric sea-level change." That is misleading. You need to add the following: "However, it should be noted that thermal expansion in the upper layer of the ocean (where most thermal expansion occurs) is local, because gravity balances mass, not volume. Such sea-level rise occurs only where the water warms. It does not affect sea-level elsewhere. Depending on where it occurs, such localized sea-level rise can affect satellite altimetry measurements of sea-level, without affecting coastal sea-levels, because it causes no net lateral flows of water. (Caveat: if the density of the water at the sea floor were to change, that would affect sea-level everywhere, much like raising or lowering the sea floor, itself, would do. However, in practice, there's hardly any of that. Most ocean warming is surface warming.)" [David Burton, United States of America]	[rejected] the reviewers' statement is not supported by literature. Density changes in the water column do lead to changes elsewhere (see e.g. Landerer et al., 2007 https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2006GL029106)
12451	11	9	11	9	halosteric is important at local scale and also important at short time scale. Reference: Wang, G., L. Cheng, T. Boyer, C. Li, 2017: Halosteric Sea Level Changes during the Argo Era. Water, 9(7), 484, http://dx.doi.org/10.3390/w9070484 [Lijing Cheng, China]	[noted] We have considered adding this, but the same goes for thermosteric change, so it doesn’t particularly set apart halosteric and thermosteric. We have therefore decided not to include the temporal scale here.
27565	11	9	11	9	About '[…] on local scale': we suggest 'regional to local scales.' [Eric Brun, France]	[accepted] changed.
61509	11	12	11	21	Should it be mentioned explicitly at the end of this paragraph that the impact of the AIS on SLC is unknown at present? Right now, it's implicitly suggested as only the impact of the GrIS on SLC is stated at the end of the paragraph (lines 19-21). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[taken into account] the statement on GrIS has been removed to focus more on processes and less on projections. Therefore, a statement on AIS did not need to be added here.
32409	11	12	11	38	While for Glaciers percolation and refreezing is mentioned, this is not the case for Greenland, although the effect is by far larger for the GrIS in total. Should be mentioned for GrIS, too. [Olaf Eisen, Germany]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115043	11	12		21	It says, "The Greenland and Antarctic ice sheets (GrIS and AIS; Section 9.4) are the largest reservoirs of frozen freshwater available on Earth, and therefore potentially the largest contributors to sea-level rise. The mass balance of an ice sheet is determined by changes in ice dynamical processes and changes in the surface mass balance (SMB), which is the balance between the accumulated snow and the melted or sublimated snow and ice. Adjacent to the AIS, ice shelves (which do not directly contribute to SLC) melt from below or disintegrate from surface meltwater penetrating crevasses and reduce buttressing of the grounded outlet glaciers (which do contribute to SLC) flowing onto the ice shelves. The GrIS is expected to mainly contribute to SLC due to SMB changes, with elevation and albedo feedbacks that amplify surface melting at the margins, while the dynamic contribution is expected to decrease due to landward retreat of marine-terminating glaciers." This is noticeably improved from the FOD -- at least you mention snowfall accumulation! However, there's still no mention of how a warming climate increases snowfall accumulation rates, potentially offsetting the processes that cause ice loss. The largest factor affecting ice mass balance for both the GIS and the AIS is snowfall accumulation. For the AIS it is approximately equal to the SUM of submarine melting, iceberg calving, and sublimation. The GIS loses ice most years (though not in the 2017-18 and 2018-19 glaciological years). But for the AIS ice accumulation & loss are in almost perfect balance. Some studies show it is gaining ice, some show it is losing ice, but all show that the net rate, whether positive or negative, is so tiny that it could cause less than 3 inches of sea-level change per century. Based on ICESat and ERS Zwally et al (2015) found Antarctica is gaining ice: http://sealevel.info/zwally2015.pdf https://www.nasa.gov/feature/goddard/nasa-study-mass-gains-of-antarctic-ice-sheet-greater-than-losses Based on CryoSat, McMillan (2014) found Antarctica is losing 79 to 241 Gt/yr of ice, though that was based on only 3 years of data. Based on GRACE, Shepherd (2012) concluded that Antarctica ice mass change since 1992 has averaged -71 ±83 Gt/yr, which means they couldn't tell whether it's actually gaining or losing ice mass. Based on ICESat, Zwally (2012) found that Antarctica is gaining ice mass: +27 to +59 Gt/yr (averaged over five years), or +70 to +170 Gt/yr (averaged over 19 years). The range from those various studies, with error bars, is from +170 Gt/yr to -241 Gt/yr, which is equivalent to just -0.47 to +0.67 mm/yr sea-level change, i.e., less than 3 inches of sea-level change per century. In other words, though we don't know with certainty whether Antarctica is gaining or losing ice, we do know that the rate, either way, is very slow, and has a much smaller effect on sea-level	[noted] Thanks for these suggestions. However, this box is about processes and definitions, so we have not added these numbers. The assessment for the ice sheets is in section 9.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107249	11	12		21	It says, "The Greenland and Antarctic ice sheets (GrIS and AIS; Section 9.4) are the largest reservoirs of frozen freshwater available on Earth, and therefore potentially the largest contributors to sea-level rise. The mass balance of an ice sheet is determined by changes in ice dynamical processes and changes in the surface mass balance (SMB), which is the balance between the accumulated snow and the melted or sublimated snow and ice. Adjacent to the AIS, ice shelves (which do not directly contribute to SLC) melt from below or disintegrate from surface meltwater penetrating crevasses and reduce buttressing of the grounded outlet glaciers (which do contribute to SLC) flowing onto the ice shelves. The GrIS is expected to mainly contribute to SLC due to SMB changes, with elevation and albedo feedbacks that amplify surface melting at the margins, while the dynamic contribution is expected to decrease due to landward retreat of marine-terminating glaciers." This is noticeably improved from the FOD -- at least you mention snowfall accumulation! However, there's still no mention of how a warming climate increases snowfall accumulation rates, potentially ofsetting the processes that cause ice loss. The largest factor affecting ice mass balance for both the GIS and the AIS is snowfall accumulation. For the AIS it is approximately equal to the SUM of submarine melting, iceberg calving, and sublimation. The GIS loses ice most years (though not in the 2017-18 and 2018-19 glaciological years). But for the AIS ice accumulation & loss are in almost perfect balance. Some studies show it is gaining ice, some show it is losing ice, but all show that the net rate, whether positive or negative, is so tiny that it could cause less than 3 inches of sea-level change per century. Based on ICESat and ERS Zwally et al (2015) found Antarctica is gaining ice: http://sealevel.info/zwally2015.pdf https://www.nasa.gov/feature/goddard/nasa-study-mass-gains-of-antarctic-ice-sheet-greater-than-losses Based on CryoSat, McMillan (2014) found Antarctica is losing 79 to 241 Gt/yr of ice, though that was based on only 3 years of data. Based on GRACE, Shepherd (2012) concluded that Antarctica ice mass change since 1992 has averaged -71 ±83 Gt/yr, which means they couldn't tell whether it's actually gaining or losing ice mass. Based on ICESat, Zwally (2012) found that Antarctica is gaining ice mass: +27 to +59 Gt/yr (averaged over five years), or +70 to +170 Gt/yr (averaged over 19 years). The range from those various studies, with error bars, is from +170 Gt/yr to -241 Gt/yr, which is equivalent to just -0.47 to +0.67 mm/yr sea-level change, i.e., less than 3 inches of sea-level change per century. In other words, though we don't know with certainty whether Antarctica is gaining or losing ice, we do know that the rate, either way, is very slow, and has a much smaller effect on sea-level	[noted] This box is about processes and definitions, so we have not added these numbers.
67195	11	13	11	15	SMB: the definition is correct following the mass balance glossary (Cogley et al., 2011), however this is not how the term is used later in the chapter, where SMB includes refreezing, a major component of the mass budget (and termed climatic mass balance in the glossary). As written a major component (refreezing) is missing. [Regine Hock, United States of America]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020).
103789	11	13	11	16	Basal melt also contributes to the mass balance of ice sheets, even if it a small contribution. [Philippe Tulkens, Belgium]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020).
80825	11	13	11	16	Basal melt also contributes to the mass balance of ice sheets, even if it a small contribution. [Louise Sandberg Sørensen, Denmark]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020).
15801	11	14	11	16	Reword "changes in ice dynamical processes and changes in the surface mass balance (SMB), which is the balance between the accumulated snow and the melted or sublimated snow and ice. " to something like "changes in ice dynamical processes and changes in the balance between the accumulated snow and the melted or sublimated snow and ice, termed here SMB". The term 'surface mass balance' is used in glaciology in a sence of the net surface ablation/accumulation, however, for a general audience "mass balance" is equivalent to "mass conservation", and it is not what's meant here by SMB. [Olga Sergienko, United States of America]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020).
129391	11	15	11	15	Shouldn't SMB with respect to sea-level changes be described as "precipitation - evaporation - runoff"? With that formulation, SMB only contributes to sea level after the molten snow, etc., has actually reached the ocean. [Trigg Talley, United States of America]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020).
3293	11	15	11	15	Shouldn't SMB with respect to sea-level changes be described as 'precipitation - evaporation - runoff'? With that formulation, SMB only contributes to sea level after the molten snow etc has actually reached the ocean. [Thomas Frederikse, United States of America]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020).
67197	11	15	11	17	something seems wrong with the sentence? [Regine Hock, United States of America]	[taken into account] the text has been rewritten using definitions from the IMBIE papers (2019, 2020).
68097	11	16	11	16	Technically, melting of an ice shelf will directly affect sealevel, due to the non-linearity of the equation of state for seawater,. Cf eg, Simms et al, QSR, 2019, Balancing the last glacial maximum (LGM) sea-level budget. [Lev Tarasov, Canada]	[taken into account] fair point. Sentence has been reworded and now no longer states that ice shelves don't directly contribute to SLR.
61493	11	16	11	16	Add *floating* ice shelves [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106631	11	16	11	16	which do not directly contribute to SLC: It is not entirely true to write that the melting of ice shelves do not contribute directly to SLC. Given the difference in density between melted fresh water and salty sea water, ice shelves do contribute to SLC (although this contribution is rather small). Indeed, although melting of ice shelves mostly affect their capacity to buttress the grounded ice sheet (and thus has an indirect contribution to SLC), the small contribution of ice shelves to sea-level rise cannot be totally neglected. [Kevin Bulthuis, United States of America]	[taken into account] fair point. Sentence has been reworded and now no longer states that ice shelves don't directly contribute to SLR.
89395	11	16	11	18	"[...] ice shelves (which do not contribute to SLC) [...]" - This is not entirely correct, since the density difference between freshwater and seawater can lead to (albeit very small) changes in sea-level after all. See Jenkins and Holland, 2007 (https://doi.org/10.1029/2007GL030784): "Contrary to popular belief, the melting of floating ice (in the form of ice shelves, icebergs and sea ice) may have a non-zero impact on sea level. This is because the melting process cools and dilutes the oceans on average, and unless these opposing effects exactly balance each other there will be a net change in the ocean density." [Ricarda Winkelmann, Germany]	[taken into account] fair point. Sentence has been reworded and now no longer states that ice shelves don't directly contribute to SLR.
89405	11	16	11	18	"[...] glaciers (which do contribute to SLC) flowing onto the ice shelves." -- change to "into the ice shelves" [Ricarda Winkelmann, Germany]	[not applicable] this sentence has been reworded
15803	11	16	11	18	Reword "Adjacent to the AIS, ice shelves (which do not directly contribute to SLC) melt from below or disintegrate from surface meltwater penetrating crevasses and reduce buttressing of the grounded outlet glaciers (which do contribute to SLC) flowing onto the ice shelves. " into something like "Ice shelves - floating extensions of ice sheets - modulate ice discharge from the grounded parts of ice sheets that determines ice-sheet contribution to SLC". [Olga Sergienko, United States of America]	[taken into account] the sentence has been reworded
54493	11	16	11	18	Consider adding "Floating on the ocean and" at the start of this sentence and also adding a comma after "crevasses". The modified sentence might look like this: "Floating on the ocean and adjacent to the AIS, ice shelves (which do not directly contribute to SLC) melt from below or disintegrate from surface meltwater penetrating crevasses, and reduce buttressing of the grounded outlet glaciers (which do contribute to SLC) flowing onto the ice shelves.". [Maria del Pilar Bueno Rubial, Argentina]	[taken into account] the sentence has been reworded
61491	11	17	9	17	Replace "grounded outlet glaciers" with "grounded ice" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[taken into account] the sentence has been reworded
67199	11	18	11	20	expected' is unclear. Is this something expected for the future? When? Studies indicate that this is the case already now. [Regine Hock, United States of America]	[noted] the statement on GrIS has been removed to focus more on processes and less on projections.
103791	11	18	11	21	This sentence describes a projection of what will happen to GrIS. By judging from the other sections in this box, it should only contain process description and not projection results. [Philippe Tulkens, Belgium]	[accepted] the statement on GrIS has been removed to focus more on processes and less on projections.
27567	11	18	11	21	For clarity, it could be specified if this statement is for past decades and / or future projections. For past decades, this statement can be somehow confusing as SMB and dynamic mass loss contributed almost equally to SLR (e.g. IMBIE team 2019, https://doi.org/10.1038/s41586-019-1855-2) [Eric Brun, France]	[noted] the statement on GrIS has been removed to focus more on processes and less on projections.
80827	11	18	11	21	This sentence describes a projection of what will happen to GrIS. By judging from the other sections in this box, it should only contain process description and not projection results. [Louise Sandberg Sørensen, Denmark]	[accepted] the statement on GrIS has been removed to focus more on processes and less on projections.
96883	11	19	11	19	Consider not using the abbreviations SLC and SMB but write out full words. Other wise, please provide a list of abbreviations to help the reader. [Nicole Wilke, Germany]	[accepted] abbreviations written out
61495	11	20	11	21	Basal melting and dynamic ice loss still likely plays a major role at shorter timescales? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] the statement on GrIS has been removed to focus more on processes and less on projections.
8987	11	21	11	21	This is a biased opinion given that half of the mass loss is from glaciers. The largest potential contributors to SLR are the marine basins of Greenland: Petermann/Humboldt, Zachariae/79north and Jakobshavn. To state that these glaciers do not matter because the others will lose contact with the ocean waters is misleading. These three basins hold 3 m sea level rise potential. The statement made here is a biased statement. [Eric Rignot, United States of America]	[noted] We apologize, it was not at all intended as a biased statement. The statement on GrIS has been removed to focus more on processes and less on projections in this box. Assessment of ice sheets is done in section 9.4.
14695	11	21	11	21	Main contributors to SLC from GrIS are noted. Add another equivalent sentence for AIS, i.e. "The AIS is mainly expected to contribute to SLC via increased ice discharge through outlet glaciers, countered by increases to surface mass balance via increased snow accumulation" [Jeremy Fyke, Canada]	[noted] the statement on GrIS has been removed to focus more on processes and less on projections.
106633	11	21	11	21	At the end of the paragraph, I would expect a sentence about the contribution of the AIS to SLC in addition to the sentence about the contribution of the GrIS to SLC. Maybe something like: "the AIS is expected to mainly contribute to SLC due to increased dynamical losses (driven by sub-shelf melting) while SMB changes (increased snowfall) is expected to partially compensate for dynamical mass losses. [Kevin Bulthuis, United States of America]	[noted] the statement on GrIS has been removed to focus more on processes and less on projections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49975	11	23	11	23	"change by changes" reads very odd, and should be rewritten. [Daniel Gilford, United States of America]	[taken into account] paragraph rewritten
67203	11	24	11	25	this is too simplistic and not correct. First 'changes' in line 25 should be 'increases' since a decrease (which is also a change) does not increase melting. Second melt is not only a function of temp and radiation [Regine Hock, United States of America]	[taken into account] paragraph rewritten
26369	11	25	11	25	"(for example)" -> Suggestion: use commas instead of () [María Santolaria-Otín, France]	[taken into account] paragraph rewritten
26489	11	25	11	25	I would suggest to replace "darkening of the snow surface" with "surface darkening due to increased exposure of bare ice". [Ward van Pelt, Sweden]	[taken into account] paragraph rewritten
67205	11	26	11	27	this process, though far more important for ice sheets is not mentioned in the preceeding ice sheet paragraph; [Regine Hock, United States of America]	[taken into account] both paragraph have been rewritten
67207	11	26	11	27	this is incomplete and biased: refreezing and proglacial lakes are only 2 (possibly minor) processes why melt water does not reach the oceans, i.e. there is storage in lakes that are not proglacial and endorheic basins, evaporation or any other hydrological processes of the melt water on its way to the ocean. [Regine Hock, United States of America]	[taken into account] paragraph rewritten
103793	11	26	11	28	The firn retention applies also to the section above on GrIS and AIS [Philippe Tulkens, Belgium]	[taken into account] both paragraph have been rewritten
26491	11	26	11	28	This sentence is currently not complete, so it would need to be reformulated or complemented. Melt water may also be stored in liquid form in glacier systems (e.g. firn aquifers, supra- or subglacial lakes) for shorter or longer periods of time. [Ward van Pelt, Sweden]	[taken into account] paragraph rewritten
27569	11	26	11	28	IGE : While mentioning that melt water coming from ice or snow does not flow directly to the ocean, it is important to mention that sometimes this melt water flows inside endorehic basins, without any access to oceans. This has larger impacts (delay, complexity of the processes) than storage in proglacial lakes, as it is stated. [Eric Brun, France]	[taken into account] paragraph rewritten
80829	11	26	11	28	The firn retention applies also to the section above on GrIS and AIS [Louise Sandberg Sørensen, Denmark]	[taken into account] both paragraph have been rewritten
77783	11	27	11	27	I think “drainage” is definitely an anthropogenic change. If it’s climatic, I would call it something else e.g. “drying out”. Similarly, “deforestation” means people cutting down trees; loss of forests due to altered climate requires some other expression. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] We have moved 'deforestation' to the sentence on anthropogenic effects, and have changed 'drainage' to 'changes in the amount of water'.
2051	11	28	11	29	"Glacier mass loss also occurs when ice flows to lower elevations": The flow of glacier mass to lower elevations does not contribute to glacier mass loss as such, it also occurs during periods of balance mass budget or mass gain. Does this refer to anomalous ice discharge, i.e. during surge activity? Then it should be stated [Matthias Huss, Switzerland]	[taken into account] paragraph rewritten
3093	11	28	11	29	The meaning of the sentence is unclear: is it meant to indicate that surface melt increases at lower elevations because of higher temperatures there? The issue is that ice flowing at lower elevation does not contribute to mass loss as such. [Daniel Farinotti, Switzerland]	[taken into account] paragraph rewritten
96885	11	28	11	29	"mass loss also occurs when ice flows to lower elevations…" is not entirely correct; the mass loss occurs when the ice at lower elevations is exposed to warmer temperatures (i.e., with a delay). [Nicole Wilke, Germany]	[taken into account] paragraph rewritten
26493	11	28	11	29	The statement "Glacier mass loss also occurs when ice flows to lower elevations" is rather odd, since it is not the ice flow that causes mass loss but rather the higher melt rates at lower elevations. It may be more relevant to mention that ice flows from the accumulation zone at high elevations to the ablation area lower down, and that any trends in flow speeds may affect melt rates and frontal position. [Ward van Pelt, Sweden]	[taken into account] paragraph rewritten
67201	11	28	11	29	This sentence makes little sense. Do you mean: Marine (tidewater glaciers)- or lake-terminating glaciers also lose mass through iceberg calving and subaqueous melt.. [Regine Hock, United States of America]	[taken into account] paragraph rewritten
32141	11	28			Endorheic basins should be mentioned. Glaciers, e.g. in part of High Mountain Asia, draining into these basins do not contribute to sea level rise. [Anja Wendt, Germany]	[accepted] changed.
8989	11	29	11	29	elevation feedback should be mentioned [Eric Rignot, United States of America]	[taken into account] paragraph rewritten
74085	11	29			into the ice shelves“ [Matthias Mengel, Germany]	[taken into account] paragraph rewritten
76707	11	31	11	31	Add water storage in the canopy? [Roelof Rietbroek, Germany]	[accepted] added
27571	11	36	11	37	Might be more specific ? e.g. dam constructions for land water impoundment ? [Eric Brun, France]	[rejected] This is already said in the sentence before.

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27573	11	40	11	40	About 'Regional': On short time-scales to multi-decadal time scales, changes in surges (including inverted barometer effects and changes in wind stress forcing) and in wave setup can also contribute to SL changes at the coast (e.g. Dangendorf et al. 2013; Melet et al. 2018; Dodet et al. 2019; Woodworth et al. 2019). However, these processes are only discussed for their (relatively larger) contribution to extreme sea levels here. - Dangendorf et al. 2013: 10.1007/s00382-013-1932-4 - Melet et al. 2018: 10.1038/s41558-018-0088-y - Dodet et al. 2019: 10.1007/s10712-019-09557-5 - Woodworth et al. 2019: 10.1007/s10712-019-09531-1 [Eric Brun, France]	[accepted] this has been added to the paragraph on ESL.
27575	11	40	11	40	About 'local': A review of forcing factors affecting sea level changes at the coast is provided by Woodworth et al. 2019. This study could therefore be relevant for this chapter. [Eric Brun, France]	[taken into account] thank you for this suggestion
77787	11	41	12	21	I think this box is pretty clear. You've done a good job! [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] Thank you very much for your compliment.
27577	11	42	11	42	About 'Ocean dynamic sea-level change': In the standardized terminology of Gregory et al. (2019), this is rather "sterodynamic sea-level changes" as it includes the steric + dynamic sea level changes (and is indeed IB corrected) [As it is mentioned on p10 l49 that this box adopts the Gregory et al. (2019) terminology] [Eric Brun, France]	[taken into account] We really meant 'ocean dynamic sea-level change' in this sentence, but have added and explained the term 'sterodynamic sea-level change' later in this paragraph.
129393	11	42	11	43	Minor detail, but dynamic sea level changes cannot be defined as changes relative to the geoid for two reasons. (1) Dynamic sea level changes actually can change the geoid itself. That is because ocean dynamics do affect ocean bottom pressure, for example on shallow shelves. That in turn causes the geoid to change. On shelves, that effect could actually be substantial (see Richter et al., 2013: Impact of self-attraction and loading effects induced by shelf mass loading on projected regional sea level rise). (2) Relative sea-level changes due to GRD effects from ice and TWS also partially consist of sea-level changes relative to the geoid. How about something like "Ocean dynamic sea-level change (Section 9.2) are sea-level changes associated with wind-driven and density-driven changes in the ocean."? [Trigg Talley, United States of America]	[rejected] we follow the definitions of Gregory et al, who state: "N13 Ocean dynamic sea-level change Df: The change in time-mean ocean dynamic sea level, i.e., the change in IB-corrected mean sea level relative to the geoid."
3295	11	42	11	43	Minor detail, but dynamic sea level changes cannot be defined as changes relative to the geoid for two reasons: 1. dynamic sea level changes actually can change the geoid itself. That is because ocean dynamics do affect ocean bottom pressure, for example on shallow shelves. That in turn causes the geoid to change. On shelves, that effect could actually be substantial (See Richter et al. 2013: Impact of self-attraction and loading effects induced by shelf mass loading on projected regional sea level rise). 2. relative sea-level changes due to GRD effects from ice and TWS also partially consist of sea-level changes relative to the geoid. How about something like 'Ocean dynamic sea-level change (Section 9.2) are sea-level changes associated with wind-driven and density-driven changes in the ocean.'? [Thomas Frederikse, United States of America]	[rejected] we follow the definitions of Gregory et al, who state: "N13 Ocean dynamic sea-level change Do: The change in time-mean ocean dynamic sea level, i.e., the change in IBM-corrected mean sea level relative to the geoid."
68557	11	42	11	48	As described, the distinction between dynamic sea level rise and steric sea level rise seems like it may be muddled. It is my understanding that dynamic sea level rise is associated with geostrophic sea-surface slope changes in near-surface currents, and that the global mean of dynamic sea level rise is roughly zero (see for example Yin et al. 2012, figure 2). However the description here does not make this point, and Figure 9.13 is depicting dynamic sea level changes that are positive almost everywhere. Is this chapter using a different definition of dynamic sea level rise than that which is common in the literature? [Robert Hallberg, United States of America]	[noted]The reviewer is correct, ocean dynamic sea level change has a zero global mean by definition. We follow the definition of Gregory et al (2019) for ocean dynamic sea-level change here. Figure 9.13 and caption has been changed.
20987	11	45	11	47	Delete Changes in ocean currents occur due to variations in heating and cooling, variability in winds and changes in seasonal to annual averaged air temperature and humidity. Replace with In regional scale changing of sea level height due to warming or cooling of the ocean may affect current circulation system and influence Ocean and atmospheric circulation patterns in the region (Nicholls et al 2003). In local scale variation of sea level which is mostly influenced by local weather systems related to atmospheric disturbances and storm surges (Flather & Khandker 1993) [Ladislaus Chang'a, United Republic of Tanzania]	[noted] Thanks for the suggestion, but we don't think that this further distinction is relevant in the context of this box.
77785	11	47	11	47	It sounds like a tautology to say that variation on 10-30-year timescales is dominated by decadal variation. Perhaps you mean "unforced" rather than "decadal". [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] decadal variations changed to internal climate variability
51971	11	47	11	48	There does not appear to be any justification for this statement in the cited section, 9.2.4. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	[rejected] please see the final paragraph of the cited section
96887	11	47	11	48	Isn't it obvious that on timescales of 10-30 years the variability is dominated by decadal variations? [Nicole Wilke, Germany]	[accepted] decadal variations changed to internal climate variability

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27579	11	47	11	48	About 'decadal variations and annular modes': "internal climate variability" might be a more generic term here [Eric Brun, France]	[accepted] decadal variations changed to internal climate variability
90451	11	48			capitalize "s" in "section" [Holly Kyeore Han, Canada]	[accepted] changed.
68099	11	50	11	54	Given statement is incorrect, as the collapse of the forebulge results in rising sealevel within the stated 2000 km range. This is evident in observations and detailed modelling for the East coast of North America from Newfoundland to Florida, cf Table 1 in : Estimated Future Contribution of Glacial Isostatic Adjustment in doi:10.1002/2016EF000363, Love et al, 2016, Earth's Future. [Lev Tarasov, Canada]	[taken into account] the statement referred to deals with contemporary GRD, not with GIA - we have added the word 'contemporary' to clarify this. The statement on GIA at the end of this paragraph disuses solid earth movement, not uplift alone. We have decided not to include further details such as the forebulge here, as this would be too specific for an overview box such as this.
90455	11	50			change "viscoelastic solid-Earth Deformation" to "Earth surface"? Changes in viscoelastic solid-Earth Deformation does not sound clear. [Holly Kyeore Han, Canada]	[rejected] this is the definition given in Gregory et al (2019) for GRD, which we follow here
100057	11	51	11	51	Since the sentence beginning "Terrestrial" considers the GRD of comtemporary processes, I suggest making this explicit: "Contemporary terrestrial mass loss…". [Glenn Milne, Canada]	[accepted] changed.
32411	11	52	11	52	page 9-10 line 44 uses "solid earth", while this line uses "solid Earth". Make consistence and check through documents. [Olaf Eisen, Germany]	[accepted] changed.
100059	11	52	11	53	Quantifying distances (2000, 7000 km) is problematic as these values depend strongly on the area of ice mass changes. Suggest being less quantitative and making the point that the spatial extent of the sea-level signal is governed by the area of ice mass change (e.g. glacier signals are commonly much more confined compared to ice sheet signals). [Glenn Milne, Canada]	[noted] we have changed the text to reflect that these distances only work for single sources, but that patterns from multiple sources may interfere. We have retained the numbers for the distances, as we think it is useful information: many readers will not know that these are the kind of spatial scales involved.
76709	11	53	11	53	:"RSL rises more than GMSL" This can potentially be misinterpreted. The GMSL here refers to the contribution of a single mass source only. Maybe say RSL rises more than the average contribution [Roelof Rietbroek, Germany]	[accepted] changed.
129395	11	55			Change to "... toward the source of the terrestrial mass loss impart long wavelength second-order ..." [Trigg Talley, United States of America]	[taken into account] changed the location of the word 'source', but think that adding 'long wavelength' would not help for the clarity of the box.
116823	11		11		Could Box 9.1 also provide estimates of levels of scientific understanding associated with the various processse? [Valerie Masson-Delmotte, France]	[accepted] Text has been added.
112485	12	1	12	1	It might be useful to mention also ocean deoxygenation and ocean acidification in this chapter, referring the reader to Chapter 5. [Pedro LLanillo del Rio, Germany]	Noted.
90457	12	1	12	2	how about changing the start of the sentence "GIA is the ongoing GRD response of the solid Earth and global gravity field to past changes…" to something like, "GIA is the ongoing GRD in response to past changes..." [Holly Kyeore Han, Canada]	[accepted] changed.
39183	12	3			"though" insteadof "thought" [Lourdes Tibig, Philippines]	[editorial] changed to 'through'
76711	12	4	12	4	causing solid Earth movement and geoid changes [Roelof Rietbroek, Germany]	[accepted] changed.
129397	12	4			Change "... causing solid Earth movement, which can result in ..." to "... causing solid Earth movement and internal mass change, which can result in ..." [Trigg Talley, United States of America]	[rejected] internal mass change is implicit in the earlier part of the sentence 'Earths mantle flows..'
82873	12	5	12	5	I suggest to consider writing "local to regional" instead of "local". [Sebastian Gerland, Norway]	[accepted] changed.
65855	12	5	19	6	Suggest including a more recent reference to the final sentence: 'These impacts are still evident today' - which currently references a paper from 2016. Suggest a more recent reference to reflect the long-term impacts caused by this marine heatwave. For example, see the impact and recovery of Western Australian fisheries in Caputi et al. 2019. Caputi, N., Kangas, M. I., Chandrapavan, A., Hart, A., Feng, M., Marin, M., & de Lestang, S. (2019). Factors affecting the recovery of invertebrate stocks from the 2011 Western Australian extreme marine heatwave. Frontiers in Marine Science, 6, 484. Alternatively, this final sentence could be removed. [Kushla Munro, Australia]	Accepted. Final sentence removed
103795	12	7	12	11	What is the VLM measured relative to? [Philippe Tulkens, Belgium]	[noted] it can be measured relative to the earths centre of mass (absolute) or with respect to the sea surface (relative). We don't think there is an added value of including this here, as the absolute vs relative reference frame is already explained earlier in the box.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80831	12	7	12	11	What is the VLM measured relative to? [Louise Sandberg Sørensen, Denmark]	[noted] it can be measured relative to the earths centre of mass (absolute) or with respect to the sea surface (relative). We don't think there is an added value of including this here, as the absolute vs relative reference frame is already explained earlier in the box.
129399	12	10			Change "… or drainage of peatlands. Tectonic …" to "… or drainage of peatlands. Coastal topography may change due to sediment deposition or removal and these may also have isostatic influences on VLM as well. Tectonic …" [Trigg Talley, United States of America]	[noted] we have here chosen to give a few examples but not an exhaustive list, and have therefore not included this proposed sentence.
27581	12	13	11	13	Note that "Extreme sea levels" is a deprecated term in Gregory et al. (2019) and the recommended replacement is "extreme coastal water levels" [Eric Brun, France]	[rejected] Extreme sea level is only a deprecated term when considering coastal impacts, but it can still be used to indicate the occurrence of exceptionally high local sea surface due to short term phenomena, as we intend in this box. (see Gregory et al, 2019, section 8) We have added the term 'extreme coastal water levels' too.
24485	12	13	12	21	The definition of extreme sea levels (ESLs) is vague for general readers. Use of total sea levels as described in BOX 9.1 is better to use for general purpose. Additionally, using the word of ESLs for different probabilities is also difficult to understand for readers. ESLs are used 1% of annual and 1/10 to 1/100 years return values depends on the subsections. For example, the section 9.6.4 uses ESLs combing RSL. The 1% annual ESL is generally wave swash and RSL change but 1/100 years ESL dominants storm surge. It is difficult to figure out target of phenomena using single word of "ESL". In page 48, Chapter 12, there is clear use of ESL as "1:100 yr ESL" which sounds appropriate and easy to understand for general readers. [Nobuhito Mori, Japan]	[taken into account] We disagree that the use of 'extreme sea level' can only be used exclusively for the 1-in-100 year return period, but we do agree that it needs to be clear what the term means. We have reworded this paragraph to reflect the potentially different uses of the term.
14585	12	13	12	21	This is a vey complete definition of ESL. Well done! But the way ESL is defined here allows extremes in both TWL and SWL (Storm tide?) to be referred to as ESL. I feel this could lead to confusion. Ch 12 for e.g. uses ESL exclusively to refer to a certain return period (e.g. 1:100 yr) TWL. Why not therefore have two different acronyms (e.g. ESL and ESWL) for extremes in TWL and SWL respectively. [Roshanka Ranasinghe, Netherlands]	[noted] We have worked with Ch12 to rewrite this paragraph
67209	12	14	12	14	can 'surge' be explained? [Regine Hock, United States of America]	[accepted] we have added 'storm' to clarify
77789	12	14	12	15	Gregory et al (2019) use SSH to mean the instantaneous height, including surge, tide and waves. I would say that these things are superimposed on mean sea level, not on SSH. I feel that “total water level” isn’t a good term because “total” is a word that is meaningful only in comparison to something else i.e. the parts, rather than stand-alone. Apparently you need a term for the difference of instantaneous SSH from MSL – is that right? Gregory et al. didn’t propose a term for that. Maybe you could call it “SSH anomaly”? That would be analogous to plenty of other “anomaly” terms, meaning difference of the instantaneous thing from its climatological value. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] Thanks, yes we agree that 'superimposed on the local ssh' was awkward phrasing, and we have changed this to reflect the definition in Gregory et al 2019. As we were not looking for a term to differentiate SSH from MSL we have not added the term 'ssh anomaly'.
49977	12	20	12	20	Add "and storm events" after "weather systems" [Daniel Gilford, United States of America]	[rejected] storm surges are already mentioned in this paragraph
76713	12	21	12	21	Such as the modification of coastlines or dredging [Roelof Rietbroek, Germany]	[noted] we have included dredging as an example
68567	12	27	40	40	I have read the entireity of section 9.2, and I would like to congratulate the authors on a thoughtful and mature Second Order Draft, particularly given the limitations of the available output from CMIP6 models. It would be great if all this material could be retained, but when the inevitable pressure comes to shorten this section, one area that I feel could be compressed without severely compromising the overall message of the chapter is the discussion of the different flavors of the Meridional Overturning Circulation in the Glacial and more distant past, from page 9-31, line 54 through page 9-32, line 25. Other areas that are also important for the overall IPCC assessment but might be somewhat compressed by leaning more heavily on SROCC include the changes in the Southern Ocean and Eastern Boundary Upwelling System, both of which had specific internal or cross-chapter boxes in SROCC. [Robert Hallberg, United States of America]	Noted. We thank the reviewer for the compliments and thoughtful advices
97975	12	31	13	37	This is a really nice intro section, you have outlined the state of large-scale (but sub-basin) features really well, with an overview as to why reported changes are consistent with our current understanding - if this is reflective of the whole chapter, very nice work indeed! [Paul Durack, United States of America]	Noted. Thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67211	12	33	12	33	Remove 'It is certain'. This is not IPCC uncertainty language. Or do you mean 'virtually certain'? [Regine Hock, United States of America]	Rejected. Certain is not IPCC language, it is language though. It is a factual statement rather than a probabilistic statement.
24207	12	33	12	34	How the statement is made with very high confidence based on one study only. Also provide recent reference [Md Arfan Uzzaman, Bangladesh]	Noted. It is now clarified that we actually reported AR5 assessment
99481	12	33	12	34	If this is indeed "certain" then suggest just starting with "Global seas-surface temperature ..." and remove the "(very high confidence)". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We report AR5 assessment with their own wording
22557	12	33	12	43	As is noted in chapter 2 there have been major innovations in our understanding of recent SST biases in in situ records. This updated knowledge needs to be reflected in a redraft of this paragraph with the reader pointed to chapter 2 for the in-depth assessment of implications for understanding of GSAT. The issue primarily relates to more properly handling the switch from primarily ship-based observations to primarily buoy based observations and the associated biases. Also, HadISST does not incorporate these adjustments and thus isn't a great estimator. HadISST2 (if available) would be better. It has been used as the lower boundary condition for several recent reanalysis products but a paper has never, to my knowledge, been produced. [Peter Thorne, Ireland]	Noted. We have updated the text accordingly
6747	12	33	12	44	The comment that this is placeholder text is appreciated, but the figure of 0.087ºC/decade is quoted on page 5 without reference to it being a placeholder value - see comment 209 above regarding its inconsistency with Table 2.4. Where did the placeholder value come from? It's disturbing that it is so far adrift from what is in Table 2.4, and it's also disturbing that the value quoted for AR5 (which should not be a placeholder value) is further adrift still. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Placeholder have all been replaced.
82609	12	33	12	44	I note that this paragraph flags the need to reconcile with Chapter 2. Chapter 2 does not use HadISST for global mean assessments because it does not incorporate the latest generation of homogenisation/bias adjustment - in particular with respect to the transition towards buoy measurements which is incorporated in the latest versions of HadSST, ERSST and COBE. HadISST does have value for applications where high spatial resolution and/or globally complete fields are required. [Blair Trewin, Australia]	Accepted. As noted in the SOD, that was a placeholder and the text is now reconciled with Chapter 2
793	12	33	13	37	Much of the new literature has not been cited and the text is dependent on results from a decade old literature. In addition, some aspects are discussed in SROCC that shoul dfurther be cited instead of the older literature [Baruch Rinkevich, Israel]	Noted. We revised our text and included SROCC starting point
39941	12	33		44	Apart from Hartmann2013, which should be made more obvious that it's AR5, this paragraphs lack traceability (i.e. no citation) [TSU WGI, France]	Noted. The paragraph is now better referenced
97973	12	34	12	41	The direct comparison AR6 to AR5 is really great, good work! [Paul Durack, United States of America]	Noted. Thanks. We changed the paragraph but kept that aspect.
96889	12	34	12	43	The discussion in Ch2 page 2-38 contains more data sets than just HADISST, and no justification is given why this data set has been selected. Other chapters use different data sets. Please provide consistent and coherent information across chapters, avoiding duplications. [Nicole Wilke, Germany]	Accepted. As noted in the SOD, that was a placeholder and the text is now reconciled with Chapter 2
79047	12	36	12	36	HadISST is now a rather old data set. Most notably, it does not have bias adjustments applied to it after 1942 and so is prone to the cool bias discussed in Chapter 2 in the section on surface temperatures. I would recommend that an alternative dataset be used in addition to HadISST, if not instead of it. ERSSTv5 and COBE-SST-2 are both globally complete and have adjustments throughout the record. Alternatively, the data sets used in Chapter 2. [John Kennedy, France]	Accepted. As noted in the SOD, that was a placeholder and the text is now reconciled with Chapter 2
61257	12	37	12	38	As you raise the improvement in the observational techniques especially after 1979 you should refer to Sect. 2.3.1.1.3 describing 'Temperature during the instrumental record', with focus on the marine area. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. A clear link is now made with Chapter 2 on observational improvements
100175	12	37	12	41	Re-written: Since 1979, more extensive satellite coverage measuring surface temperature has improved global mean and regional SST observations. The updated global mean SST warming rate from 1979 to 2018 is 0.087±0.007°C per decade (90% confidence interval) which is faster than the mean warming rate over the past century (AR5 estimate is 0.072 ± 0.024°C per decade from 1979 to 2012). [Carlye Peterson, United States of America]	Not applicable. Text entirely changed
1711	12	37	12	41	It would be informative to give the estimated global mean SST for each succeeding decade since 1979. These values can be inserted after "1979." on line 38. [Michael Kennish, United States of America]	Rejected. Global mean SST is discussed further in Chapter 2. Here our remit is to discuss regional pattern and processes underlying the global mean change
61353	12	38	12	38	A reference to support this statement would be welcome. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text entirely changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85215	12	38	12	38	Is 'robust' the right word to use here? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text entirely changed
85097	12	40	12	40	The estimated global mean SST has been updated from AR5 and new values are mentioned but I was wondering if a citation is needed for those numbers? 0.0087 plus minus 0.0007). [Aakash Sane, United States of America]	Accepted. As noted in the SOD, that was a placeholder and the text is now linking to Chapter 2 assessment
101697	12	43	12	43	It is unclear what this "placeholder" is referring to. Reported SST trends and their uncertainties must be consistent across chapters. [IAPSO ECS group review, United States of America]	Accepted. This is the point of having a placeholder! The text is now reconciled with Chapter 2
65857	12	44	20	7	Suggest clarifying this statement with Oliver et al. 2019. Permanent marine heatwaves are described as occurring when they last more than 360 days per year, however, is this meant to mean 365 days per year? Oliver et al. 2019 describes the emergence of a permanent marine heatwave in climate model projections and greenhouse gas emission scenarios. Are the findings in that study consistent with what is shown in Box 9-1, Figure 1, which is based on CMIP6 model analyses? Oliver, E. C., Burrows, M. T., Donat, M. G., Sen Gupta, A., Alexander, L. V., Perkins-Kirkpatrick, S. E., ... & Thomsen, M. S. (2019). Projected marine heatwaves in the 21st century and the potential for ecological impact. Frontiers in Marine Science, 6, 734. [Kushla Munro, Australia]	Accepted. We confirm we used a threshold at 360 days. The result of Oliver et al. are consistent with the box and we have now clarified by citing Oliver et al.
85219	12	45	12	46	For me, it seems important to make the key point about the impacts on Arctic SST and sea-ice of polar amplification clearly in this section, assuming that it is evident observations and projections of Arctic SSTs and/or sea-ice changes, as well as in near surface temperature changes over Arctic land? Because of the colour scales on the figs it was hard to tell (as detailed seperately in a comment on the figure). In particular, it seems important to include an expert statement (linked to detail in the sea-ice sections of this chapter) along the lines of that even with medium scenarios and waming of 2-3 deg C, many models suggest Arctic warming over 10 deg C in some months or in soma cases even in the annual mean change (with obvious implications for impacts and potential feedbacks on the cryosphere, ecosystems and other linked aspects of regional and global climate)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have now added a sentence to assess SST change in the Arctic
101699	12	46	12	47	"While a positive SST trend is evident globally, some regions have experienced quicker or slower warming than the global average" - The time period over which this statement holds should be mentioned. As shown in Fig. 9.3, some regions also experience cooling, which should be mentioned for accuracy. [IAPSO ECS group review, United States of America]	Accepted. Periods now mentioned, as well as regional cooling
82863	12	46	12	47	This sentence can be understood so that in all regions warming has happend, some faster, others slower. But a few lines further (page 13, line 1), also regional cooling is mentioned. I suggest to reword the statement here. [Sebastian Gerland, Norway]	Accepted. Text has been clarified to mention cooling
100177	12	46	12	51	Substance: I'm not sure if figure 9.3 demonstrates rates of temperature change for different regions but only global mean SST change and anomalies relative to 1970-1980. See my later comments on figure 9.3. [Carlye Peterson, United States of America]	Noted. Figure redrawn
100179	12	46	12	51	Editorial re-write: While the global SST trend is positive (warming), regional warming trends are more nuanced with some regions warming faster or slower than the global average (Figure 9.3). For instance, over the past century, the Western Boundary Currents of the subtropical gyres have warmed relative to the global mean (medium confidence) (Wu et al., 2012; Yang et al., 2016a) despite observational challenges, low agreement on changes in current position and gyre intensity, and uncertain underlying dynamical causes (low confidence) (Section 9.2.3.4). [Carlye Peterson, United States of America]	Not applicable. Text entirely changed
45277	12	46	13	19	There's also paleoclimate evidence showing warming in different parts of the ocean since the beginning of pre-industrial period, including Abram et al. 2016: https://doi.org/10.1038/nature19082; Jimenez et al. 2018: https://doi.org/10.1002/2017GL075323 [Anson Cheung, United States of America]	Noted
69075	12	47	12	51	Add reference to appropraite Chatper 2 section (Section 2.3.3.4.2) [Bernadette Sloyan, Australia]	Accepted. Reference added
80655	12	50	12	50	of' is missing between 'intensity' and 'change' [Helene Jacot Des Combes, Marshall Islands]	Accepted. Changed
85217	12	50	12	50	low agreement 'in model or observational products or both'? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified
12453	12	51	12	51	I'm wondering why 1950 is chosen here? Is that because of WWII? [Lijing Cheng, China]	Noted. This is due to the choice of the underlying publication that are assessed here
86431	12	51	12	55	Indian Ocean is warming faster than the rest of the tropical oceans, mainly due to atmospheric-ocean feedback processes and also contributed by regional ocean dynamics. Especially due to the shallow overturning circulation leading to patterns of heat redistribution in the Indian Ocean (Miyama et al., 2003; Swapna et al., 2017; Sreenivasu et al 2016). [Swapna Panickal, India]	Noted. The list of references have now been replaced by SROCC assessment in order to not duplicate assessment of same papers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132545	12	53	12	53	What is atmospheric ocean feedback"? [Kyle Armour, United States of America]	Accepted. We changed feedback in " coupling"
735	12	53	12	53	“due to atmosphere ocean feedback (medium confidence, thought there is...” replace “thought” with “though” [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text entirely changed
85221	12	53	12	53	should 'thought' be repleaced with 'although'? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text entirely changed
87995	12	53			change thought to though [Kathleen McInnes, Australia]	Not applicable. Text entirely changed
16325	12	55	12	55	Don’t really need the comma after “southern ocean” [Julian Mak, China]	Noted.
86813	12	55	13	1	There is a big difference between slower than average warming and cooling. Please consider splitting this sentence into two to make it easier to see which oceans are warming slowly, and which ones are cooling. [Oyvind Christophersen, Norway]	Noted. This is clarified in the next sentences
101701	12	55	13	1	"In contrast, the eastern Pacific Ocean, North Atlantic Ocean, and Southern Ocean, have warmed slowly or cooled due to regional ocean dynamics" - The general attribution of the slow warming or cooling to regional ocean dynamics for these regions requires references. Alternatively, the general attribution ("due to regional ocean dynamics") could be deleted, as the following sentences discuss these regions in more detail and include the corresponding references. [IAPSO ECS group review, United States of America]	Accepted. References provided
107437	12	55	13	19	This section begins with saying the eastern Pacific Ocean, North Atlantic Ocean, and Southern Ocean have warmed slowly or cooled due to regional ocean dynamics. This is discussed for the Pacific Ocean and Southern Ocean, but is not adequately explained for the North Atlantic. Section 9.2.3.1 is referenced but some further information seems warranted here [Jennifer Walker, United States of America]	Rejected. Because of text length constraint, we prefer referring to the relevant section where all details are provided
27583	13	4	13	4	About 'warming': could specify "surface" warming'. [Eric Brun, France]	Accepted. We now clarified we discuss surface ocean
88581	13	4	13	7	Suggestion of literature: (1) Tonelli, M.; Marcello, F., Ferrero, B., Wainer, I. Warm Deep Water Variability During the Last Millennium in the CESM–LME: Pre-Industrial Scenario versus Late 20th Century Changes. Geosciences 2019, 9, 346. (2) Sippel, S., Meinshausen, N., Fischer, E.M. et al. Climate change now detectable from any single day of weather at global scale. Nat. Clim. Chang. 10, 35–41 (2020). https://doi.org/10.1038/s41558-019-0666-7 [Rosemary Vieira, Brazil]	Noted
132547	13	5	13	5	Should be "since the 1950s". [Kyle Armour, United States of America]	Accepted. Time period has been clarified
96891	13	7	13	9	The finding of subpolar southern ocean cooling since the 1970s with the summary given in lines 4-6 which states lower than average warming. Please check [Nicole Wilke, Germany]	Noted. The time periods are different in the two sentences
101703	13	7	13	12	The references to the underlying causes of the Southern Ocean surface cooling over this period are here merged, without differentiating the very different conclusions provided by these studies on the underlying mechanisms. Some studies attribute the cooling to changes in ocean circulation, sea ice, glacial meltwater, etc. We suggest to be more precise on the suggested underlying mechanisms. [IAPSO ECS group review, United States of America]	Rejected. Because of text length constraint, we prefer not expanding too much on underlying mechanisms. We however now refer to Section 9.2.3.2 where more details are provided
61195	13	8	13	8	"the temperature around the subpolar Southern Ocean". I suppose this is related to the atmospheric warming mentionned in the previous line but it would be clearer to specify which temperature is concerned here, according to the litterature cited, e.g., surface air temperature. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Now clarified (sea surface temperature)
65859	13	9	33	9	Suggest editorial change to: "... new evidence suggests..." [Kushla Munro, Australia]	Noted. We are unsure what this comment refers to (p13, line 9 or page 33 line 9, or elsewhere), but we noted to emphasise new evidences as much as possible in our text
99483	13	10	13	10	Remove "cause and" [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Reworded
98335	13	10	13	12	The following reference is of relevance here: Olonscheck, D., M. Rugenstein, and J. Marotzke (2020), "Broad consistency between observed and simulated trends in sea surface temperature patterns", Geophysical Research Letters 47, 1-10, doi:10.1029/2019GL086773 [Dirk Olonscheck, Germany]	Accepted. Thank you. The paper has now been considered in our assessment
23489	13	12	13	14	clearer or less clear? we know only a few modes of internal variability (e.g. ENSO, AMO, NAO, IOD, SAM, NAM, and few others) not the complete envelop and their operating time-scales. So in my opinion the complete understanding of internal variability is still uncertain at this stage and that makes the projections less clearer. [Saurabh Rathore, Australia]	Rejected. We do not discuss projection here. But only note that we know clearer in these regions that internal variability is at play
81707	13	14	13	15	The link between North Atlantic SST and AMOC is not as clear - many papers discuss the role of the gyre circulations and external forcing (see below). [Laure Zanna, United States of America]	Accepted. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100181	13	14	13	15	Re-written: For instance, North Atlantic SST variability is related to the AMOC as discussed in Section 9.2.3.1. [Carlye Peterson, United States of America]	Not applicable. Text removed
3025	13	14	13	15	The Southern Ocean seems to get special attention here and other regions aren't covered in any detail even close to that of the Southern Ocean. Why is there no summary statement about the cooling in the subpolar North Atlantic south of Greenland? A declining overturning circulation leads to an increased north-south sea surface temperature gradient in the subpolar North Atlantic Ocean (Winton, 2003, in J. Clim.), which is typically what the cooling south of Greenland is attributed to. But the increased sea surface temperature gradient also leads to enhanced storm tracks and more storm-driven north-south moisture transport in the atmosphere (Woolings et al., 2012, in Nature Geosci.). Enhanced storm tracks result in increased downward vertical velocity anomalies equatorward of the jet and enhanced boundary layer inversion strength (Grise and Medeiros, 2016, in J. Clim.). The increased vertical stability of the lower troposphere and north-south moisture transport in the atmosphere reduces the entrainment of dry upper tropospheric air into the lower troposphere and increases low cloud coverage (Klein and Hartmann, 1993, in J. Clim.; and Wood and Bretherton, 2006, in J. Clim.). The low cloud coverage perturbation can ultimately cool the regional (e.g., south of Greenland) temperatures (Trossman et al., 2016, in Geophys. Res. Lett.). [David Trossman, United States of America]	Noted. We have revised the text toward a more balanced regional treatment, in particular giving more attention to the tropical oceans
107439	13	15	13	19	Sentence beginning with "Decadal-scale…" does not flow from previous sentence. Discussing Pacific cooling and Indian Ocean warming which should be included with this discussion earlier in paragraph [Jennifer Walker, United States of America]	Accepted. Here shorter time-scale are discussed. We have now clarified by separating the two paragraphs.
132549	13	17	13	17	Better to note the sign of the anomaly of the trade winds (strengthened). [Kyle Armour, United States of America]	accepted. Sign of change clarified
35317	13	21	14	15	The CMIP and HighResMIP models show biases compared to the observed SST (Figure 9.3). Could the authors comment on the potential impact of these biases for the projection of SST change? For example, does the cold bias in the South Pacific of the HigResMIP means that projection of warming in this region could be underestimated? And on the contrary, could the local warm biases of the model cause an overestimation of the projected warming? [Etienne Pauthenet, France]	Accepted. Better description of bias is now added to the text. The figure has now be entirely be redrawn so the comment cannot be address directly, but we did make efforts to better describe bias and difference between coarse and high resolution models
72085	13	22	13	37	The followings could be important recent references for mean state CMIP biases--- Samanta, D., Karnauskas, K. B., & Goodkin, N. F. (2019). Tropical Pacific SST and ITCZ biases in climate models: double trouble for future rainfall projections?. Geophysical Research Letters, 46(4), 2242-2252. --- Samanta, D., Karnauskas, K. B., Goodkin, N. F., Coats, S., Smerdon, J. E., & Zhang, L. (2018). Coupled model biases breed spurious low frequency variability in the tropical Pacific Ocean. Geophysical Research Letters, 45(19), 10-609. [Samanta Dhrubajyoti, Singapore]	Noted
737	13	23	13	24	“equatorial Pacific and Atlantic, Southern Ocean, Gulf stream” this list should have an and [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
3027	13	27	13	28	If the mechanism described in my previous comment is an artifact of the models used in the aforementioned studies, then model representation of ocean circulation-cloud interactions is a potential reason why biases still remain south of Greenland. [David Trossman, United States of America]	Rejected. We thank the reviewer for this insightful comment, but we must assess published literature
61243	13	30	13	31	No reference is given to the effects of a wrong representation of the thermocline depth, as compared to the other causes. Opotions are Linz et al. (2014, doi: 10.1002/2013JD021415) or Li and Xie (2012, doi:10.1029/2012GL053777). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Referenced now considered.
101705	13	31	13	31	"...errors in atmospheric model cloud-related short-wave." - We assume that the authors are referring to "short-wave radiation" here. Please add "radiation" or specify differently. [IAPSO ECS group review, United States of America]	Noted. Text clarified
101707	13	31	13	31	Specify if "circulation" referes to atmospheric and/or oceanic circulation. [IAPSO ECS group review, United States of America]	Noted. Text clarified
97977	13	32	13	35	It would be useful to clarify these statements with the results presented in Ch3, Fig 3.24. In summary, it seems that while (preliminary) assessments of CMIP6 vs CMIP5 show marginal improvements in equatorial SST biases, the zonal mean biases are of the same and sometimes larger (particularly the S Hemi) magnitude. (Preliminary) HighResMIP simulations don't show any significant improvement over CMIP6/5 with similar magnitude in the N Hemi and larger and opposite sign in the S Hemi (Ch3, Fig 3.24) [Paul Durack, United States of America]	Accepted. We have streamlined consistency with chap 3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65861	13	32	33	32	Suggest editorial change to: "This progress, along with …" [Kushla Munro, Australia]	Noted. We are unsure what this comment refers to, but in response to this comment we made strong efforts to emphasize on new evidences and progresses
85223	13	33	13	37	Is it worth mentioning here known errors in aerosol indirect and direct forcing both on SST biases and SST projections as well uncertainties in SST projections linked to uncertainties in climate sensitivities and climate forcings? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now clarify the link between sst and climate sensitivity
101709	13	34	13	34	Please provide a range of latitudes rather than "south of 50°N", since the equatorial and Southern Ocean biases remain high. [IAPSO ECS group review, United States of America]	accepted. We now clarified that we refer to Norther Hemisphere, south of 50°N.
98333	13	34	13	37	I suggest the following changes that carefully balance the evaluation of simulated SST trends in CMIP: "Despite some persisting regional biases, CMIP6 coupled climate models generally reproduce observed SST trends over the past century (Figure 9.4; Olonscheck et al., 2020). There is no agreement on whether the observed SST trends in some regions are an unusual realization of the Earth's possible behavior or the global climate models are systematically biased but that large internal variability covers that bias (Olonscheck et al., 2020)." Reference: Olonscheck, D., M. Rugenstein, and J. Marotzke (2020), "Broad consistency between observed and simulated trends in sea surface temperature patterns", Geophysical Research Letters 47, 1-10, doi:10.1029/2019GL086773 [Dirk Olonscheck, Germany]	Accepted. The text has been rewritten to include the balance suggested by the reviewer.
61245	13	35	13	35	Also refer to Fig. 3.24 which clearly shows meridional and zonal SST biases for CMIP6 and CMIP5 separatey. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted. Reference added
132551	13	35	13	36	Over what time period to CMIP6 models produce observed SST trends, and in what regions? Also note that you refer to the wrong figure here, Fig. 9.4 is something else. You refer to this incorrect figure in the following paragraphs as well, so check those. [Kyle Armour, United States of America]	Accepted. Time period is now clarified and a reference to a paper is added for the reader. The regional aspect is now further discuss in the paragraph. Figure number is corrected
61175	13	35	13	37	"Despite some persisting regional biases, CMIP6 coupled climate models generally reproduce observed SST trends over the past century (Figure 9.4), and there is high confidence in their ability to inform on future large-scale SST changes." Figure 9.4 shows trends in freshwater flux, surface heat flux and wind stress magnitude, but not SST trends. This seems to be more than a typo since there is no figure in Chapter 9 that shows SST trends. The closest is Figure 9.3 which shows SST changes over 1950-2014, but not trends per se (i.e., slopes of regression lines). [Patrick Cummins, Canada]	Accepted. Figure number corrected
61177	13	35	13	37	The assessment given in this statement is excessively positive and should be qualified. Based on the top part of Figure 9.3, the models generally reproduce the globally integrated change in SST over the past century. However, based on the lower half of Figure 9.3, they seem to have virtually no skill in reproducing regional SST changes, especially so in the case of the CMIP models. Mention of 'persisting regional biases' refers to biases in the climatological state, not to discrepancies in representing regional changes. Accordingly, this does not suffice as a qualification of the skill of the models to represent regional changes. [Patrick Cummins, Canada]	Noted. We have separated assessment of mean bias, with potential impact on climate sensitivity, and assessment of trends, which show better skill. We also removed the confidence assessment.
61247	13	36	13	36	Wrong figure reference. Reader should be pointed to Fig. 9.3. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Figure number corrected
100183	13	36	13	36	Figure 9.4 does not have SST, I think the authors meant fig. 9.3. [Carlye Peterson, United States of America]	Accepted. Figure number corrected
96893	13	36	13	36	Reference to wrong figure. Should probably be 9.3. [Nicole Wilke, Germany]	Accepted. Figure number corrected
82875	13	36	13	37	I suggest to specify for what time scale "future large-scale SST changes" is meant in this context. [Sebastian Gerland, Norway]	Accepted. Timescale are now clarified
41457	13	42			Please explain what causes the projections to jump in the 2090! Is this a bug? [Alexander Nauels, Germany]	Noted. This is an error in how the running mean was calculated. Now resolved.
6749	13	43	13	43	HadISST is not "observations". It is a spatially complete SST dataset formed by analysing observations using an optimal interpolation procedure. "observations" should be replaced by a phrase such as "observational analyses" or "observationally-based estmates". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Labelling revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65863	13	50	36	6	Suggest clarifying between the main part of the western boundary currents (WBC) and the eddy-rich extension regions. The text notes that many recent studies suggest accelerated warming poleward of subtropical WBCs, given a poleward shift in the wind stress zero-curl line. However, the text suggests there are limited observations for this poleward shift or enhancement of WBCs. Locations poleward of the WBCs are in the WBC extension regions but they are not distinguished as extension regions in-text. For example, Sloyan and O'Kane (2015) find that the East Australian Current's (EAC) transport is anti-correlated with the East Australian Current extension transport. In addition, the EAC extension region is associated with one of the fastest warming regions around Australia (Wijffels et al. 2018). Furthermore, there are in situ long-time series indicating warming trends for this region, which have been associated with the southward extension of the EAC (Ridgway et al. 2007). In addition, further north, findings based on models show a poleward shift in the bifurcation latitude for the Pacific South Equatorial Current and the North Equatorial Current (Zhai et al. 2014). References for these poleward shifting bifurcation latitudes have not been included in this section. Wijffels, S. E., Beggs, H., Griffin, C., Middleton, J. F., Cahill, M., King, E., ... & Sutton, P. (2018). A fine spatial-scale sea surface temperature atlas of the Australian regional seas (SSTAARS): Seasonal variability and trends around Australasia and New Zealand revisited. Journal of Marine systems, 187, 156-196. Zhai, F., Hu, D., Wang, Q., & Wang, F. (2014). Long-term trend of Pacific South Equatorial Current bifurcation over 1950–2010. Geophysical Research Letters, 41(9), 3172-3180. Ridgway, K. R. (2007). Long-term trend and decadal variability of the southward penetration of the East Australian Current. Geophysical Research Letters, 34(13). [Kushla Munro, Australia]	Noted. This level of detail is not accessible, but the writing of this section has been revised.
795	13	53	14	36	Same as above [Baruch Rinkevich, Israel]	Noted. We now strive to better cite SROCC, and cite new literature. Explicit example would have helped, but we have made our best to address the comment.
100185	13	54	13	54	"increase" not "increased" [Carlye Peterson, United States of America]	Not applicable. Text removed
132553	13	54	13	54	"Increased" should be "increase". [Kyle Armour, United States of America]	Not applicable. Text removed
39185	13	54	14	3	The differences in the amounts of very likely ranges of global mean SST increase vary becausedifferent scenarios were used- the SROCC used the RCPs and the findings for this used the SSPs. Is there a way we can even compare these? [Lourdes Tibig, Philippines]	Accepted. We do not compare and only now provide CMIP6 projections
22559	13	56	14	3	How does this chime or otherwise with the adopted chapter 4 approach which used emulators instead of the raw CMIP6 model output to arise estimates of 21st century GSAT changes. Does this risk arising an inconsistency between chapters. Suggest a discussion with chapter 4 on this issue. [Peter Thorne, Ireland]	Noted. This was discussed with Chap 4 but the emulator cannot produce Global mean SST. We provide CMIP6 future projection.
105979	13	56	14	3	There should be a reference to earlier material explaining the SSPs and RCPs, specifically Cross-Chapter Box 1.5. [William Gutowski, United States of America]	Not applicable. Text removed
116825	13		13		impact=> effect (impact has a special meaning in the IPCC context) [Valerie Masson-Delmotte, France]	Accepted. We removed the term impact
19257	14	2	14	2	Consistency with chapter 4? The global GSAT based on CMIP6 models is different from the emulator based "best estimate" og chapter 4. Can the impact of this CMIP6/emulator difference be estimated for SST? [Anne-Marie Treguier, France]	Noted. This was discussed with Chap 4 but the emulator cannot produce Global mean SST. We provide CMIP6 future projection.
27585	14	2	14	2	About 'SSP4-8.5': we suggest to check if it is not a typo for SSP5-8.5 [Eric Brun, France]	Not applicable. Text removed
29641	14	2	14	2	should ssp4-8.5 be SSP5-8.5? [Aixue Hu, United States of America]	Not applicable. Text removed
61249	14	3	14	3	Wrong figure reference. Reader should be pointed to Fig. 9.3. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected
101711	14	3	14	3	The reference to Figure 9.4 should be replaced by a reference to Figure 9.3 here (specifically the "top right panel") [IAPSO ECS group review, United States of America]	Accepted. Corrected
100187	14	3	14	3	I think this is supposed to be referring to figure 9.3, not 9.4. [Carlye Peterson, United States of America]	Accepted. Corrected
96895	14	3	14	3	Reference to wrong figure. Should probably be 9.3. [Nicole Wilke, Germany]	Accepted. Corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61197	14	3	14	6	This sentence could be cut in two by separating the second part on lines 6-7, e.g., "However, there is low confidence that this warming will emerge this century(...)". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Text revised.
101713	14	3	14	6	The findings by Bracegirdle et al. (2020) that update this statement for CMIP6 models should be discussed here. The current references refer to CMIP5 models. Bracegirdle, T. J., Krinner, G., Tonelli, M., et al . Twenty first century changes in Antarctic and Southern Ocean surface climate in CMIP6. Atmos Sci Lett. 2020; e984. https://doi.org/10.1002/asl.984 [IAPSO ECS group review, United States of America]	Accepted. Reference added
16327	14	5	14	5	Clarify "...eventually warming by more than the tropics..." sentence ("by more than" suggests a number?) [Julian Mak, China]	Noted, though we cannot quantify that aspect
16329	14	5	14	5	Consider "and" instead of "but", as "but" sounds like a caveat and potentially downplays the significance, whilst "and" is just a statement of fact. [Julian Mak, China]	accepted. Corrected
61251	14	6	14	6	The reference to Section 7.4.4.2 refers to 'Tropical sea-surface temperature gradients', which does not compy with the statement on the emergence timescale of southern ocean warming. The better cross-reference in Chapter 7 is 7.4.4.1.3 where no emergence is has been observed dring the historical record. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected
19259	14	6	14	13	Very long sentence. [Anne-Marie Treguier, France]	Noted. Revised
100189	14	6	14	13	Re-written for clarity, but I suggest using the same verb conjugation for the list of things that cause the tropical Pac. SST pattern to change, for example "a weakening of" could be simply "weakened". One comment for clarity in the text in square brackets. Similarly, the SST change pattern observed in the tropical Pacific ocean will eventually transition to a pattern similar to El Niño SST change on century timescales (medium confidence) due to weakened Walker circulation (Vecchi et al., 2008), weakened evaporative cooling in the western equatorial Pacific (Hartmann and Michelsen, 1993; Knutson and Manabe, 1995; Xie et al., 2010; Luo et al., 2015), [where's the verb? Have there always been zonal gradients in low cloud feedbacks, are the gradients enhanced or weakened?] zonal gradients in low cloud feedbacks (Meehl and Washington, 1996), and enhanced warming in extratropical subduction regions and subsequent upwelled waters (Barreiro and Philander, 2008; Fedorov et al., 2015) (Section 7.4.4.3). [Carlye Peterson, United States of America]	Accepted. Rephrased
1713	14	6	14	13	It would be useful to discuss the factors responsible for weakening of the Walker circulation (see lines 8 and 9, page 14). [Michael Kennish, United States of America]	Rejected. Beyond the mandate of our chapter
82649	14	6	14	13	This section, in effect, provides an assessment for the mean state of ENSO. No such assessment is made in Chapter 4 (which only considers ENSO variability); the question of where this assessment best sits should be reconsidered (observations of the mean state, as indicated by the SST gradient, and variability are assessed together in Chapter 2). At a minimum there should be cross-referencing where relevant. [Blair Trewin, Australia]	Accepted. We now clarified we mean altered variability not mean state
61199	14	7	14	8	Here it is my feeling that it would be useful to mention shortly what a transition to a pattern similar to ENSO SST pattern on century timescale would mean in terms of impacts on the regional-global climate. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Beyond the mandate of our chapter
23491	14	7	14	8	eventually transits [Saurabh Rathore, Australia]	Rejected. Phrase reworded
20989	14	8	14	8	after El Niño Delete SST [Ladislaus Chang'a, United Republic of Tanzania]	Rejected. We keep SST for accuracy since El Nino has diverse impact on different part of the climate system
61255	14	9	14	9	It might further be worthwile to include nother reference already invoked in Sect. 7.4.4.2.1: Cha, S. C., Moon, J. H., and Song, Y. T. (2018). A Recent Shift Toward an El Niño-Like Ocean State in the Tropical Pacific and the Resumption of Ocean Warming. Geophys. Res. Lett. 45, 11,885-11,894. doi:10.1029/2018GL080651. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This reference is about short term trends in the observational record, which we believe is not directly relevant to our assessment here.
82651	14	9	14	9	For consistency with Chapter 2 it would be better to quote their assessment finding of 3 +/- 1C (Cross-Chapter Box 2.4). [Blair Trewin, Australia]	Noted. However this comment does not seem to refer to the page and line indicated here.
61253	14	13	14	13	The reference to Section 7.4.4.3 seems misplaced. You better refer to Sect. 7.4.4.2 or more specifically to 7.4.4.2.1 where the weakening of the Walker circulation and the transition to an El-Niño-type warming patterns is described (chapter 7, page79, lines 45-53). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected
100191	14	13	14	15	Re-written: The North Atlantic subpolar gyre is projected to warm more slowly than surrounding regions as the Gulf Stream concurrently warms rapidly (Cheng et al., 2013) (Figure 9.4) and AMOC further declines under greenhouse gas forcing (Section 9.2.3.1). [Carlye Peterson, United States of America]	Noted. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23493	14	13	14	15	At what confidence level? Recent study shows that the warming of the Indian Ocean can strengthen the AMOC. "Indian Ocean warming can strengthen the Atlantic meridional overturning circulation" DOI- 10.1038/s41558-019-0566-x [Saurabh Rathore, Australia]	Rejected. This reference is about cause of AMOC change which is discuss in section 9.2.3.1
93547	14	13			Cross citing "Sea-ice free Arctic contributes to the projected warming minimum in North Atlantic" by Suo et al (2017) [Vivien How, Malaysia]	Accepted. Reference added
96899	14	14	14	14	Reference to Fig 9.4 is wrong. Perhaps Fig. 9.3? [Nicole Wilke, Germany]	Accepted. Corrected
101715	14	15	14	15	Use correct figure reference to Figure 9.3 rather than 9.4. [IAPSO ECS group review, United States of America]	Accepted. Corrected
14835	14	17	14	17	nice to have the MPWP as a comparison for present and future. Are there other past period reconstructions available? [Marie-France Loutre, Switzerland]	Not applicable. Text entirely changed
100193	14	17	14	18	I believe the authors are trying to communicate that large changes in SST have happened in the geologic past under slower changes in atmospheric CO2 concentration, but I'm not sure how effective it is to say "while the system approaches equilibrium" because the climate system is arguably rarely (if ever) in equilibrium. I think the point is that the CO2 changes were slower in the past, hence closer to equilibrium, while today carbon emissions are clearly outpacing equilibrium sequestration. I'm not sure how to reconcile this, but here is a suggested change that might be easier to read: Re-written: The geologic record illustrates large SST changes while the climate system approaches equilibrium. [Carlye Peterson, United States of America]	Not applicable. Text entirely changed
573	14	17	14	36	In the Atlantic Ocean, sea surface temperature (SST) variability increased (~+10%) at high latitudes and decreased (~-7%) at low and mid-latitudes during the Mid Pliocene Warm Period (MPWP). These changes were likely related to changes in the meridional SST gradient during this period. Full reference: Pontes, G.M.; Wainer, I.; Prado, L.F; Brierley, C. (2020). Reduced Atlantic variability in the Mid-Pliocene. Climatic Change, doi: 10.1007/s10584-020-02675-9. [Luciana Figueiredo Prado, Brazil]	Not applicable. Text entirely changed
45275	14	17	14	36	There are two Mid Pliocene papers that are currently in discussion, and should be relevant: https://doi.org/10.5194/cp-2019-145; https://doi.org/10.5194/cp-2019-161 [Anson Cheung, United States of America]	Not applicable. Text entirely changed
61373	14	18	14	18	Please place the '2'of 'CO2' as subscript [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text entirely changed
33415	14	18			Change: "…CO2…" by "…CO2…". [Guiomar Rotllant, Spain]	Not applicable. Text entirely changed
22561	14	19	14	19	Reference would be better to cross-chapter box 2.4 [Peter Thorne, Ireland]	Not applicable. Text entirely changed
61375	14	19	14	19	Please, as GMST is for the first time use in this chapter, consider giving the signification of this Acronym [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text entirely changed
68919	14	19			This GMST value for MPWP is from AR5. Please see (and call-out) CCB2.4, which is devoted the MPWP. [Darrell Kaufman, United States of America]	Not applicable. Text entirely changed
19267	14	20	14	20	The reference to chapter 2 could be more explicit/detailed. Cross-Chapter Box 2.4, Figure 1 is very useful to illustrate this paragraph. [Anne-Marie Treguier, France]	Not applicable. Text entirely changed
83541	14	20	14	20	Include here also the new SST compilation and review by McClymont et al. (submitted): McClymont, E. L., Ford, H. L., Ho, S. L., Tindall, J. C., Haywood, A. M., Alonso-Garcia, M., et al. (submitted). Lessons from a high CO2 world: an ocean view from ~ 3 million years ago. Clim. Past Discuss. (submitted). 41 doi:10.5194/cp-2019-161, who observe a GMST range of 2.3 to 3.2°C. [Antje H. L. Voelker, Portugal]	Not applicable. Text entirely changed
61201	14	22	14	22	"(-2 to +4)" I suppose these are degrees C with respect to pre-industrial period but the units should be added and mention to the period of comparison, even if done on line 19, would improve clarity. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text entirely changed
19261	14	22	14	24	This sentence is hard to follow. What is the main point? What are the air-sea thermostat mechanisms? [Anne-Marie Treguier, France]	Not applicable. Text entirely changed
101717	14	22	14	24	"In the tropics, MPWP warming was modest but variable (-2 to +4) (Dowsett et al., 2013) but significant in the western warm pool, refuting hypothesized air-sea thermostat mechanisms (O'Brien et al., 2014), debatably (Brierley et al., 2015)." - It is unclear what the clause "debatably" at the end of this sentence refers to [IAPSO ECS group review, United States of America]	Not applicable. Text entirely changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99485	14	22	14	24	This is a very confusing sentence, please clarify what the assessment is. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text entirely changed
132555	14	22	14	24	I had real trouble understanding this sentence. [Kyle Armour, United States of America]	Not applicable. Text entirely changed
68921	14	22	14	36	The topic of latitudinal and longitudinal temperature gradients during the Pliocene is also discussed in CH7 and in CCB2.4. Please be sure that they are consistent with each other and better yet, are complement to make best use of available words. [Darrell Kaufman, United States of America]	Not applicable. Text entirely changed
88583	14	23	14	23	"...but significant in the western warm pool," (Where? Pacific? Atlantic?) [Rosemary Vieira, Brazil]	Not applicable. Text entirely changed
61203	14	25	14	25	"(4-12C" please add "with respect to pre-industrial period". Perhaps use an accronym for it? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text entirely changed
83543	14	25	14	25	This temperature range is no longer in agreement with the re-evaluation and focus on the narrower KM5c time interval presented by McClymont et al. (submitted) (for complete reference see comment above). Their range is much narrower and the offsets smaller (2-6°C). [Antje H. L. Voelker, Portugal]	Not applicable. Text entirely changed
100713	14	25	14	25	Add: "...(Dowsett et al., 2013). Similarly, during the MCO, when atmospheric CO2 levels were comparable to expectations for mid- to late 21st century, GMST's were ~9 °C above pre-industrial, tropic temperatures also were similar to PI, while high southern and northern latitude sea-surface temperatures were 10-15 °C higher (Super et al., 2018, 2020; Sangiorgi et al., 2018; Burls et al., in review). Both findings support..." [Matthew Kohn, United States of America]	Not applicable. Text entirely changed
101719	14	26	14	26	The term "polar amplification" seems inaccurate given the reduced/delayed warming or slight cooling of the Southern Ocean surface. A more accurate description would be "Arctic amplification" [IAPSO ECS group review, United States of America]	Not applicable. Text entirely changed
19263	14	26	14	27	"over thousands of years, regions are capable of much more warming than projected for the 21st century": isn't this a well known fact in general (consider the EECO, for example)? Why make this statement in the present paragraph? [Anne-Marie Treguier, France]	Not applicable. Text entirely changed
14837	14	29	14	29	The changes in the ocean circulation over intermediate time (between the reecent and the MPWP) would be interesting (e.g. last 2000 years). [Marie-France Loutre, Switzerland]	Not applicable. Text entirely changed
101721	14	29	14	30	"reduction of AMOC recently and in near-term projections, opening the possibility that slowdown of AMOC may be a near-term transient response that will ultimately reverse (Jansen et al., 2018)." - This finding is also supported by Oschlies et al., 2019, who find that the AMOC initially slows down, but then recovers and on timescales of +1000 years even strengthens substantially compared to pre-industrial, before levelling off at strength just above pre-industrial. (Oschlies, A., Koeve, W., Landolfi, A. et al. Loss of fixed nitrogen causes net oxygen gain in a warmer future ocean. Nat Commun 10, 2805 (2019). https://doi.org/10.1038/s41467-019-10813-w) [IAPSO ECS group review, United States of America]	Not applicable. Text entirely changed
89323	14	29			"opening the possibility" is hardly the correct term here - it has been a well-established fact for decades (if I remember correctly, since the early Ron Stouffer simulations) that the AMOC weakening due to global warming is primarily a transient response, because you warm the surface ocean relative to the older deep water underneath. I don't see an obvious reason why in equilbrium, a warmer climate would have a weaker AMOC. It might be weaker or stronger, depending on surface forcing, but this is totally different from the obvious transient effect of surface warming and freshening which reduces density in the DWF sites relative to what it was earlier, making it harder for new deep water to replace the older deep water. [Stefan Rahmstorf, Germany]	Not applicable. Text entirely changed
132557	14	30	14	30	Cite Sigmond et al. 2020 (doi: 10.1038/s41558-020-0786-0) here as well. It is refernced quite a bit in Chapter 4. [Kyle Armour, United States of America]	Not applicable. Text entirely changed
67213	14	39	14	39	remove acronym SSS. The report becomes unreadable with the excessive use of acronyms. [Regine Hock, United States of America]	Accepted. Acronym removed
95929	14	41	14	41	"surface ocean salinity' [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text revised.
97979	14	41	14	44	It is true that Wust (1936) was the first to highlight the consistency between the E-P and SSS fields, however there have been considerable more recent efforts, using idealized climate model simulations to investigate these interactions. Some studies for reference include: Williams et al 2006 doi: 10.1007/s00382-006-0151-7; Williams et al 2007 doi: 10.1029/2007GL029275; Williams et al 2010 doi: 10.1016/j.dynatmoce.2009.02.001; Durack et al 2012 doi: 10.1126/science.1212222 (check the supp, figs S7, S8); Lago et al 2016 doi: 10.1175/JCLI-D-15-0519.1 [Paul Durack, United States of America]	Not applicable. Text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79909	14	41	15	22	Section 9.2.1.2. Sea surface salinity. There is no mention in the section to salinity changes in the North Atlantic apart from those in the subtropical Atlantic. In relation with increasing salinities in these areas, it might be interesting to note that: 'Increasing salinities and arrival of Atlantic water entering the subpolar gyre from the South compensated the salinity decrease expected in the subpolar North Atlantic from increase of Greenland meltwater flux since the early 1990s, and a salinification by 0.15–0.2 was observed in the region (Dukhovskoy et al. 2016). After the mid-2010s the trend has reversed, and a broad freshening, the largest in the last 120 years, is observed form mid- to high-latitudes of the North Atlantic (Holliday et al. 2020).' References: (1) D.S. Dukhovskoy, P.G. Myers, G. Platov, M-L. Timmermans, B. Curry, A. Proshutinsky, J. L. Bamber, E. Chassignet, X. Hu, C. M. Lee and R. Somavilla (2016). Greenland freshwater pathways in the sub-Arctic Seas from model experiments with passive tracers. doi: 10.1002/2015JC011290. (2) Holliday, P. et al. Ocean circulation causes the largest freshening event for 120 years in eastern subpolar North Atlantic. Nature Communications 11, (2020). [Somavilla Raquel, Spain]	Accepted. Thank you. The two papers has now been considered in our assessment
107441	14	41	15	22	Can this have more quantitative information about surface salinity changes? There are only two actual values, both for the Southern Ocean. Additional values or a more informative metric than "saltier" and "fresher" would be useful [Jennifer Walker, United States of America]	Not applicable. Text removed
79907	14	45	14	46	Perhaps these studies are worth to be cited unless it is just one new analysis and the remaining 'observation-based studies' are in preparation. In this case, the sentence could be changed to avoid indicating that they 'provide stronger evidence' (if these studies have not been published) or even removed. [Somavilla Raquel, Spain]	Not applicable. Text removed
101723	14	45	14	46	"Since AR5, new analysis and consistency between a growing number of observation-based studies provide stronger evidence for increased contrast between high- and low- sea surface salinity regions since the 1950s." - The mentioned "studies" need to be referenced here. [IAPSO ECS group review, United States of America]	Not applicable. Text removed
68565	14	47	14	47	SROCC and AR5 were adopted by the consensus fo the governments of the world. The authors of this chapter do not have the power to "revise" previous IPCC reports. This line should be revised appropriately. I would suggest changing "In response, we revise the AR5 reported confidence in such change from very likely to extremely likely ()." to something like "In response, the increasing contrast between high- and low- salinity regions is now assessed to be extremely likely ()." [Robert Hallberg, United States of America]	Accepted. The use of revise has been revised
101725	14	48	14	50	"At basin-scales, this report confirms the conclusion of AR5 and SROCC that it is very likely the Atlantic has become saltier and the Pacific and Southern oceans have freshened over the period of historical observational coverage" - Singh et al. (2016) point to the importance of changes in atmospheric moisture transport in these changes. They state that "robust changes in the aerial hydrologic cycle with warming, particularly the increase in the distance between moisture source and sink regions [...], amplify the Atlantic-Pacific interbasin salinity contrast." (Singh, H. K., Donohoe, A., Bitz, C. M., Nusbaumer, J., & Noone, D. C. (2016). Greater aerial moisture transport distances with warming amplify interbasin salinity contrasts. Geophysical Research Letters, 43(16), 8677-8684.) [IAPSO ECS group review, United States of America]	Rejected. We do not include that paper in our assessment since this is material relevant for chapter 8 rather than chapter 9.
12457	14	52	15	55	The pattern intensificiation (salty gets saltier and fresh gets fresher) described in previous paragraph is regional change, and pretty robust. So I don't think this sentence is correct. Remove "detecting" helps in the first sentence. But the time scale matters here, for example, trends over what time period？ For your consideration, here is a recent study examining the near-surface salinity changes in the Pacific Ocean: Li G., Y. Zhang, X. Jing, X. Song, J. Abraham, L. Cheng, J. Zhu, 2019: Examining the salinity change in the upper Pacific Ocean during the Argo Period. Climate Dynamics, 1-20, https://doi.org/10.1007/s00382-019-04912-z [Lijing Cheng, China]	Not applicable. Text removed
116827	14		14		This first paragraph is important related to state dependent feedbacks (see ch 7) and would deserve to have a summary statement reflected in the ES and the TS. [Valerie Masson-Delmotte, France]	Noted though we think chap 7 is better placed for providing this assessment in their ES.
116829	14		14		Thanks for including paleoclimate evidence. However, in many pplaces in chpater 9 (like here), there is duplication between the assessement done in this chapter and other parts of the AR6 WGI (for instance, the MPWP box in chapter 2). Please join forces, and seek integration options (more xChapter boxes for instance) to give a comprehensive view of specific past warm periods (improved characterisation, understanding, and implications for mechanisms of interest, or confidence in models, etc). Also, check use of GMST or GSAT related to reconstructions. [Valerie Masson-Delmotte, France]	Noted.

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24209	15	1	17	56	This study is (Sallée et al., submitted) cited several times. Neither it is accepted nor it is published. [Md Arfan Uzzaman, Bangladesh]	Noted. The reference is now published and cited
101727	15	2	15	10	A more detailed referencing to studies that describe the processes mentioned here would be desirable. A lot of the processes are described in more detail in section 9.2.2.2, which should at least be referenced here if not repeating some of the evidence provided by studies referenced in section 9.2.2.2. Progress made since SROCC (the only citation here) and AR5 should be explicitly mentioned. [IAPSO ECS group review, United States of America]	Noted. This section has now been merged with 9.2.2.2
82877	15	3	15	3	The number (0.0011 +/- ...) refers to SROCC, but there it again refers to a single reference (de Lavergne et al. 2014). I suggest to list that reference here as well. [Sebastian Gerland, Norway]	Not applicable. Text entirely changed
82879	15	3	15	3	Freshening is expressed with a negative trend in salinity, here without a minus sign. I wonder if this could be detailed more. [Sebastian Gerland, Norway]	Noted. Sign of change has been clarified
61205	15	12	15	12	The use of "fresh salinity anlomaly" seems rather confusing to me. Could it be replaced by "fresh water anomaly" or "negative salinity anomoly"? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
6751	15	12	15	12	Shouldn't "fresh salinity anomalies" be "low salinity anomalies"? Water can be anomalously fresh or anomalously saline, but salinity is either low or high. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
85225	15	12	15	12	could this statement be clarified - for example is this a direct link via rivers? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
12455	15	12	15	14	What's this mean? I suppose it is related to the following finding: "some local salinity changes can be used as a precusor of remote rainfall changes, because the evapuration over sea increases local sea salinity and the moisture is then transported to some remote land areas and support rainfall" (this is my suggested wording for this point). This is true and several cases were identified, but I won't say it is the "considerable advancement" as this process works only at short time scale and works only for a handfull of well-established atmospheric transportation routines. [Lijing Cheng, China]	Not applicable. Text removed
97981	15	12	15	14	The two studies cited (Li et al 2016 JCL and Liu et al 2018 GRL) are correctly cited, however Li shows that an SSS positive anomaly (salinity increase) suggests an increased (atmospheric) moisture flux divergence from the source region. The text "The direct link between near-surface fresh salinity anomalies.." is not an accurate descriptor for both studies and their conclusions. Liu suggests that increased P in the tropical Pacific leads to a dynamical ocean response, which leads to freshwater export from the tropics to extra tropics through Rossby wave excitation [Paul Durack, United States of America]	Not applicable. Text removed
23495	15	13	15	14	Please also mention the additional reference about the mechanistic link that exist between sea surface salinity and terrestrial precipitation during the climatic events such as ENSO and IOD. Moreover, the changes in the SSS start to happen before the peak of these events and could aid the prediction of terrestrial rainfall. "Near-Surface Salinity Reveals the Oceanic Sources of Moisture for Australian Precipitation Through Atmospheric Moisture Transport" "https://doi.org/10.1175/JCLI-D-19-0579.1" [Saurabh Rathore, Australia]	Not applicable. Text removed
22563	15	18	15	22	Can anything be said about behaviour in CMIP6 models which will be of great interest to policymakers? [Peter Thorne, Ireland]	Noted. Though we are unware of reference looking at this aspects from CMIP6 models
99487	15	19	15	19	Should it be C20th and/or CMIP6? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. It should be 20th century.
85099	15	27	15	27	The words in bracket "per whole earth area, not ocean area" are slightly confusing. Did you want to say 'per whole earth area, not just ocean area' or 'per whole earth area (land+ocean) and not just ocean area' [Aakash Sane, United States of America]	Accepted. Reworded
82881	15	27	15	27	I suggest to start with stating what specific fluxes are addressed in detail here. [Sebastian Gerland, Norway]	Accepted. Reworded.

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85227	15	27	15	27	Is it worth starting the air-sea flux section with a clear statement that despite some recent improvements considerable errors remain in air-sea flux products, which hinder robust assesment of heat and salt budgets and surface flux trends in many regions, as deatiled by several of the Ocean Obs 2019 papers, e.g. Cronin et al, 2019, Frontiers in Marine Science? For many satellite observations, even SST, the lack of observations during periods of high cloud-cover and rain introduces possible aliasing issues, particuarly for cloudy convective regions such as the Southern Ocean or boundary currents. Similarly it might be useful to include an overview summary statement that coupled models mostly have significant, and in some cases cancelling, errors in heat flux components, freshwater and momentum surface fluxes linked to errors in the representation of clouds, boundary layer charcateristics, dynamics and winds, surface exchange processes, etc often compounded by the impacts of SST bias feedbacks? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added
129401	15	27	15	55	The flux number for CMIP6 (0.XX±0.XX Wm-2) is not given. Check the net air-sea heat flux range in the Argo era (2005-2017) for accuracy. The 0.68±0.60 Wm-2 appears in error. The CMIP6 model estimate (0.68±0.02 Wm-2) is at the high end of the range, not closely resembling the observation as stated in the text. Furthermore, the passage states a high confidence in attributing the net increase to anthropogenic forcing without given a summary of evidences in Section 7.2 and 7.3. The attribution in the passage below on spatial variations and trends in net flux (lines 45-55) appears to implicate the basis on modeling. The hindcasting modeling, as said in the text however, underestimates the observed variations. Thus the model simulation itself may be questionable in providing the justification for high confidence in the anthropogenic attribution. [Trigg Talley, United States of America]	Noted, values removed.
12461	15	27	15	55	Recently, there is a new generation of surface flux data derived from TOA observations and atmoshperic reanalysis, which has much better accuracy than in situ based surface flux data for climate-relevant studies and widely used in literature. References (just FYI): (1) Trenberth KE, Fasullo JT (2018) Applications of an updated atmospheric energetics formulation. J Climate, 31:6263-6279. doi:10.1175/JCLI-D-17-0838. (2)Trenberth KE., et al (2017) Atlantic meridional heat transports computed from balancing Earth's energy locally. Geophys Res Lett 44:1919 1927 doi:10.1002/2016GL072475 (3) Trenberth KE, Fasullo JT, Kiehl J (2009) Earth's global energy budget. Bull. Am. Meteorol. Soc. 90:311–323 (4) TrenberthKE, Zhang Y, Fasullo JT, Cheng L (2019) Observation-Based Estimates of Global and Basin Ocean Meridional Heat Transport Time Series. J. Climate, 32 :4567-4583' https://doi.org/10.1175/JCLI-D-18-0872.1 (5) Mayer M, et al (2019) An improved estimate of the coupled arctic energy budget. J Climate 32:7915-7933. DOI: 10.1175/JCLI-D-19-0233.1 (6)Loeb NG, et al (2018a) Clouds and the Earth's Radiant Energy System (CERES) Energy Balanced and Filled (EBAF) Top-of-Atmosphere (TOA) Edition 4.0 Data Product. J Climate 31(2):895–918. https://doi.org/10.1175/JCLI-D-17-0208.1 (7) Liu C, Allan RP, Berrisford P, Mayer M, Hyder P, Loeb N, Smith D, Vidale P-L, Edwards JM (2015) Combining satellite observations and reanalysis energy transports to estimate global net surface energy fluxes 1985-2012. J Geophys Res Atmospheres. ISSN 2169-8996 doi: 10.1002/2015JD023264 (8) Liu C, Allan RP, Mayer M, Hyder P, Loeb NG, Roberts CD, Edwards JM, Vidale P-L (2017) Evaluation of satellite and reanalysis-based global net surface energy flux and uncertainty estimates. J Geophys Res Atmospheres 122(12):6250-6272. ISSN 2169-8996 doi: 10.1002/2017JD026616 [Lijing Cheng, China]	Accepted. References added
132559	15	29	15	29	Be sure to coordinate with Chapter 7 on these years and numbers, as they might have changed. [Kyle Armour, United States of America]	Accepted, link rather than numbers now used.
97983	15	30	15	30	Domingues et al 2008 assessed CMIP3 (not CMIP5) models, so not sure the correct citations are in place here [Paul Durack, United States of America]	Accepted. Clarified
79911	15	31	15	32	Reviewers must assume that some estimation exists since it is indicated that they are similar. Perhaps, for future IPCC reports, it might be better to include preliminary results indicating that they will be changed than not indicating anything [Somavilla Raquel, Spain]	Noted
101729	15	31	15	32	The statement "CMIP6 models show a similar range" cannot be evaluated due to missing information. Also, it is not specified if this heat flux change refers to the same period as the ARGO period. Please specify. [IAPSO ECS group review, United States of America]	Noted, text removed

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3029	15	31	15	32	It could be worth adding that Huber and Zanna (2017, in Geophys. Res. Lett.) have shown that the majority of the uncertainty associated with ocean heat uptake in climate simulations is associated with the air-sea fluxes that are not entirely due to errors in the representation of ocean circulation parameters, and this uncertainty is largest in the Southern Ocean. [David Trossman, United States of America]	Accepted. Reference added
129403	15	31	15	35	The statement that the increasing radiative forcing is directly attributed to anthropogenic forcing appears in several places. Such attribution should provide adequate evidence or be scientifically elaborated. The citation of Sections 7.2 and 7.3 should be checked for adequacy. [Trigg Talley, United States of America]	Accepted, now attribution assessment is deferred to section 3.5.1.3.
101731	15	37	15	38	This statement that flux trends have not yet emerged is not supported by Figure 9.4. There are clear patterns of flux trends that are similar to the simulated changes. However, due to the missing indication of statistical significance in the figure a robust statement on the emergence of these trends is difficult. [IAPSO ECS group review, United States of America]	Noted. This figure is not yet finalized and furthermore does not constitute an assessment of global trends. Regional trends are robust, as indicated in the text.
101733	15	37	16	7	Please specify which panels in Fig. 9.4 these paragraphs refer to. [IAPSO ECS group review, United States of America]	Noted. Figure subpanel labels have been added for this purpose.
12459	15	38	15	38	Zanna et al. is not a proper reference here, this study is not about surface flux [Lijing Cheng, China]	Rejected, the paragraph is about variability in heat content, which is the topic of that paper.
97985	15	38	15	38	It is worth noting that the uncertainties remain (as fluxes are almost impossible to measure in-situ, let alone over large regional areas accurately) with modern flux datasets having global mean net heat fluxes in the range of 10-30 Wm-2 indicative of major unresolved biases that remain in the latest generation products and two orders of magnitude larger than the best estimates of the flux associated with the current energy imbalance (see Josey et al 2013 doi: 10.1016/B978-0-12-391851-2.00005-2) [Paul Durack, United States of America]	Noted. The global fluxes are no longer quantified specifically here.
79979	15	38	15	39	Air-sea flux data as of AR5 were too noisy to detect even the mean fluxes directly, which remains the case'. This sentence is confusing. Which mean fluxes are being referred to? If it is the climatological mean state then the statement is incorrect as there has been broad agreement across different air-sea flux datasets since the 1970s regarding the climatological mean pattern of net heat exchnage abd its components. For example, all datasets agree that there is strong heat loss over the Gulf Stream, so they are certainly 'not too noisy to detect even the mean fluxes directly' . Please clarify the text here. [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified
34427	15	38	15	39	This assertion seems contradictory with respect to Fig. 9.4 which shows a signal (but not the error bars). [Claire Waelbroeck, France]	Noted. This figure is not yet finalized and furthermore does not constitute an assessment of global trends. Regional trends are robust, as indicated in the text.
79983	15	42	15	43	while noting uncertainties in atmospheric reanalysis-based fluxes, including global energy imbalances that exceed the observed ocean warming. ' This sentence implies that only atmospheric reanalysis-based fluxes have energy imbalance uncertainties that exceed the observed ocean warming. However, all flux products suffer from this problem so for accuracy the sentence needs to be changed to 'while noting uncertainties in all flux products, including global energy imbalances that exceed the observed ocean warming. ' [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified
101735	15	45	15	46	"Decadal trends in satellite observed global fluxes are not detectable, but regional changes are robust in some regions in both satellite observations and projections" - What do the authors mean by robust? Is this associated with some significance level? [IAPSO ECS group review, United States of America]	Accepted. Clarified
97987	15	45	16	9	This section may be an opportunity to highlight the progress between CORE2 and the OMIP2 (JRA55-do) focused ocean-only simulations, with JRA55-do (Tsujino et al 201 doi 10.1016/j.ocemod.2018.07.002) a step jump in improving the consistency of contemporary and continuously updated data used to force ocean-only simulations [Paul Durack, United States of America]	Accepted. Reference now considered
6753	15	50	15	51	It would be better to replace "for the satellite era (since July 1987, Figure 9.4)" by "since July 1987 (Figure 9.4)". The satellite era begins in 1979 for some, in 1972 for others, and so on. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised

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79985	15	51	15	52	These products agree with sparse buoy and ship observations (Bentamy et al., 2017)'. This statement needs some quantitative detail to be added in order to convey any useful meaning. As an author of the paper I can state that it would be more accurate to say 'A combined satellite and atmopsheric reanalysis product provides the best agreement with sparse buoy and ship observation based flux estimates, with RMS differences of the latent heat flux typically less than 25 Wm-2 (Bentamy et al., 2017)'. Please revise along these lines for accuracy. [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, revised
20543	15	52	15	52	Figure 9.4 does not show model fluxes. The legend is however not completely clear. As for the trends, when the text comments that " magnitudes are substantially weaker in models" perhaps it is somewhat understated, talking about discrepancies by factors of the order of 3. [philippe waldteufel, France]	Noted. Figure was not finalized.
19265	15	52	15	52	"While patterns agree between models and satellites in net fluxes": figure 9.4 does not show a comparison between model and satellite fluxes. Is there another figure, or a reference? [Anne-Marie Treguier, France]	Noted. Figure was not finalized.
101737	15	52	15	52	"While patterns agree between models and satellites in net fluxes" We do not understand what this is referring to since the CMIP6 mean fluxes are not represented. Is it the trend patterns that are compared? [IAPSO ECS group review, United States of America]	Noted. Figure was not finalized.
101739	15	52	15	53	"the trend magnitudes are substantially weaker in models": what seems to be compared here are observational trends over 1995-2014 (or 2001-2014), middle column, and CMIP6 trends over 2015-2100, right column, which does not make much sense to us to compare two different periods. The right column in Figure 9.4 doesn't look like what we would expect for 21st century trends under increased global warming and we suspect the text would make more sense if the represented CMIP6 trends were over the same period as observed and not 2015-2100 [IAPSO ECS group review, United States of America]	Noted. Figure was not finalized.
129405	15	52	15	55	This model underprediction of observed variation is also evident in land-based measurements -- for example, in model underestimation of precipitation extremes (Chang et al., 2016; Liang et al., 2020). This model inability on extremes should be explicitly considered in making conclusions from models themselves. There are sections in this and other AR6 cycle assessments, that rely the synthesis conclusions on model outputs exclusively. [Trigg Talley, United States of America]	Noted, not relevant to this chapter
132561	15	53	15	54	It could also reflect observational errors, right? [Kyle Armour, United States of America]	Noted, but sampling errors greatly exceed the (non-systematic) instrument errors.
61259	15	55	15	55	The phrase 'Where the patterns agree in models and observations' is a repetition with above about the genreal agreement in pattern. I would delete it as it holds no additional info and does not add to the rest of the sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reworded.
79913	15	55	16	1	Does it refer to heat gain by the ocean? If that is the case, it would be clearer indicating ' … warming (net heat gain)…' or something similar [Somavilla Raquel, Spain]	Accepted. Reworded.
61261	16	1	16	1	It confused me that you talk about consistency to changes in other water-masses. I first thought about volume change. Yet you clearly refer to heat content and temperature changes. to clarfy, I would reformulate to: '[...] consistent with the largest changes observed in related water-mass properties'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reworded.
101741	16	7	16	8	These statements on the freshwater flux trend patterns cannot be evaluated due to missing information [IAPSO ECS group review, United States of America]	Noted. Figure was not finalized.
99933	16	12	16	21	Figure 9.4 is too small to appreciate anything more than just a general idea of changes in the fluxes being discussed. Perhaps larger panels or a different design? [Dan Helman, United States of America]	Noted. Figure was not finalized.
34431	16	14	16	14	Fig. 9.4 left: the source of these data should be given: are they satellite data? What are the references? [Claire Waelbroeck, France]	Noted. Figure caption clarified.
61325	16	14	16	19	Fig. 9.4: In the bottom left panel on the wind stress magnitude, the reference period is missing. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Figure was not finalized.
61327	16	14	16	19	Fig. 9.4 Caption: Delete first '(left)'. Further on, when you describe the time periods for the observed trends, you refer to P-E, which is not plotted. Consider deleting this term. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Figure was not finalized.
129407	16	24	16	42	On wind variations and long-term changes, not only the extreme strong winds including high-speed gust wind are important. Calm wind and stagnant air flow are also characteristic of climate change and variability. See Abhishek et al. (2010). [Trigg Talley, United States of America]	Noted.
6755	16	25	16	25	Delete "of global synoptic measurements". Scatterometer measurements are not synoptic measurements. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, revised

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27587	16	26	16	26	Young and Ribal 2019 could also be cited here doi: 10.1126/science.aav9527 [Eric Brun, France]	Accepted. Reference added
79981	16	26	16	28	‘In the Southern Ocean we have high confidence that the zonal wind stress increased from the early 1980s to the 1990s’. AR5 Chapter 3 concluded with ‘medium confidence that Southern Ocean wind stress has strengthened since the early 1980s.’ No new studies are reported in the AR6 text here so how has the level of confidence been increased from medium to high? Note the cited paper of Jones et al. (2011) makes use of a combination of theory and idealizedfr numerical simulations so does not provide any observational basis for increased confidence in the zonal wind stress trend. I raised this point in my FOD review but it has not been addressed. At present the text states that there has been a significant increase between what was reported in AR5 and AR6 from 'medium' to 'high' confidence in Southern Ocean multidecadal wind stress trends without any supporting evidence to justify this change in confidence level. This needs to be rectified otherwise it looks like previous assessment results can be revsied without justification. [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted, corrected.
34429	16	27	16	28	Referring to Fig. 9.4 here is again strange since confidence levels not visible on Fig. 9.4. [Claire Waelbroeck, France]	Noted. Figure was not finalized.
82883	16	27	16	28	I wonder if "but" would fit better here than "although", and I suggest to add more detail to the trend remark for the Indian sector. [Sebastian Gerland, Norway]	Noted. Reworded.
39187	16	28	16	29	Projections also weaken? [Lourdes Tibig, Philippines]	Noted. Reworded.
45279	16	29	16	31	There's also indirect evidence from coral records: doi:10.1002/2014PA002683. [Anson Cheung, United States of America]	Accepted. Reference added
61263	16	29	16	33	This sentence holds some circular argumentation, concerning the Walker circulation. The latter passage on 'at odds with the expected slowdown of the Walker circulation under greenhouse gas forcing and the CMIP6 projections' is redundant. Consider removing this passage. Also consider referencing to Sect. 7.4.4.2.1. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reworded
1715	16	29	16	33	More discussion is needed on the dynamics of greenhouse gas forcing in the slowdown of the Walker circulation. [Michael Kennish, United States of America]	Rejected, inappropriate chapter for this discussion
22565	16	29	16	38	Is this characterisation consistent across 4-7-9? This would be worth checking. It feels consistent from my recollection but would require careful cross-checking I think. [Peter Thorne, Ireland]	Accepted. Checked
109081	16	33	16	38	coordination with chapter 6 on section referencing section 6.3.4? [Chaincy Kuo, United States of America]	Noted. Text in 6.3.4 doesn't cover wind stresses, mostly precipitation and circulation.
61265	16	33	16	38	In this passage, the sign of the SST trends is unclear. From Fig. 9.3, which should be referenced, I take it that you speak of a general warming trend. Consider indicating the sign. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. However, the changes described are not illustrated but drawn from the references cited.
100803	16	39	16	39	Here a reference to the Technical Annex VI.2 and VI.3 for NAM and SAM respectively, should be included [Corti Susanna, Italy]	Accepted. References added
101743	16	41	16	42	This difference between CMIP5 and CMIP6 trends and the here reported CMIP6 trends are missing a reference and evidence. Therefore, the statement is currently not supported [IAPSO ECS group review, United States of America]	Accepted. References added
54347	16	46	16	47	Also seen in E3SM CMIP6 / HighResMIP results (e.g. Caldwell et al 2019 [Luke Van Roekel, United States of America]	Accepted. References added
81709	16	47	16	47	Given the "very high confidence", can you clarify what is meant? Is it biases in the trends or over short timescales? I can see a scenarios on various timescales in which the model structure might still be a problem. [Laure Zanna, United States of America]	Accepted. Mean flux was intended, sensitivity and trend would require specific investigations that have not been carried out.
80601	16	48	16	48	Haarsma et al. (2016) is referenced here, but this is just a description of the HighResMIP protocol. You could use Bock et al. (2020, revised), as referenced and shown in Ch. 3. [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101745	16	48	16	53	"Oceanic variability outside of the tropics is traditionally thought to stem either from internal variability or in response to atmospheric weather (Hasselmann, 1976), but these high-resolution coupled models show that the atmosphere responds to ocean eddy variability on small scales (Bishop et al., 2017), exhibiting coupled modes with an impact on clouds, precipitation, and boundary layers in the atmosphere and ocean" - The meaning of this sentence and its content is unclear due to a mixture of multiple aspects. It seems obvious that "oceanic variability" stems from "internal variability" otherwise it would stem from the "external forcing", which would result in a trend rather than "variability". Despite "atmospheric weather" there is a large number of processes that could cause "oceanic variability", which would need to be specified. Where is the contrast in this sentence that justifies the "but"? This sentence is too long and requries substantial reformulation to make it understandable. [IAPSO ECS group review, United States of America]	Accepted. Clarified
45281	16	48	16	53	Small et al. (2019) also discussed the role of ocean in driving air-sea flux in high latitudes: https://doi.org/10.1175/JCLI-D-18-0576.1 [Anson Cheung, United States of America]	Accepted. Reference added
101747	16	53	17	3	The content and meaning of these sentences is difficult to understand and requires reformulation. [IAPSO ECS group review, United States of America]	Accepted, revised
79915	16	55	17	1	If they are systematically different one would expect higher confidence in this fact. [Somavilla Raquel, Spain]	Noted, but mean and sensitivity/variability are quite different limiting the scope of the statement
69079	17	6	18	2	This section needs to undertake a more thorough literature search and either add more supporting evidence or revise the assessments made on a very limited number of studies [Bernadette Sloyan, Australia]	Accepted. We entirely revised the assessments. And we believe we have done a thorough literature search.
101749	17	8	17	8	"Changes in surface temperature and salinity affect the upper-ocean stratification of the ocean..." - remove "of the ocean" or add "globally" after "stratification". [IAPSO ECS group review, United States of America]	Accepted. Reworded
12463	17	8	17	8	"changes in surface temperature and salinity affect the upper ocean stratification of the ocean". This is not a complete definition, subsurface changes also matter. Stratification is used to describe the density difference at different ocean vertical layers: generally ocean forms layers with lighter waters near the surface and denser waters depth, this difference is stratification, representing a stable configuration of ocean water. Therefore, the changes in both surface and subsurface temperature and salinity affect the upper ocean stratification. [Lijing Cheng, China]	Accepted. We now clarified that upper ocean stratification refers to density contrast between surface and deep ocean
101751	17	8	17	10	"Changes in surface temperature and salinity affect the upper-ocean stratification of the ocean, which alter the depth penetration of surface mixing, and has many important consequences on global heat and carbon cycles, as well as global ocean biogeochemistry and biology" - See comment regarding chapter 5, p. 59, lines 32-33, which cross-references section 9.2.3.3 instead of 9.2.1.4 in chapter 9 regarding that projected increased stratification will reduce nutrient supply to the upper ocean. There is however, no information about nutrient supply in 9.2.1.4, which then would need to be added if this cross-reference is to be corrected. First sentence of 9.2.1.4 briefly mentions effects for ocean biogeochemistry and biology - perhaps the authors can rephrase it to add info about nutrient supply? [IAPSO ECS group review, United States of America]	Noted. We do not assess nutrient supply in 9.2.1.4 since this beyond our remit, but we now note the link with nutrient
1717	17	8	17	10	Include some description of the biotic and biochemical consequences of increased upper ocean stratification. This can be added on line 10 after "Bindoff et al., 2019)." [Michael Kennish, United States of America]	Accepted. This is not the remit of our chapter, but we now provide non exhaustive impact: upper ocean nutrient supply and oxygenation
12475	17	8	17	28	The section can be better organized by separating two things: (1) The statement of ocean is stratifying, it is high confidence or virtually certain, given so many literature. (2) Quantification of stratification change, which is a bit diverge in literature. [Lijing Cheng, China]	Noted. We have revised the organisation to better reflect that aspect
12477	17	8	17	28	Including a figure is hepful here to show stratification change, as it represents different information compared with T/S: stratification is about the vertical gradient of T/S/density. [Lijing Cheng, China]	Noted. We agree but because of constraint on the length of the chapter we made the choice to not include such figure.
23281	17	8	17	28	Year-to-year to decadal variabilities of the upper ocean stratification, partly associated with major climate modes, have amplitudes comparable to the long-term trends over 50 years (Yamaguchi and Suga, 2019), which makes the estimation of the trends difficult based on shorter time series. This point may be better to be mentioned in this paragraph. [Toshio Suga, Japan]	Accepted. Now clarified in the text
101753	17	8	18	2	This entire section relies too heavily on the reference to Sallée et al. (submitted). A more diverse view on the literature of this topic and a more differentiated referencing would enhance the credibility and broadness of the assessment. [IAPSO ECS group review, United States of America]	Accepted. We reduce the weight on Sallée et al., and now mention to it as emerging evidence

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12465	17	10	17	11	This is also incorrect. AR5 and SROCC assess the stratification by using temperature difference between surface and 200m. This does not represent the upper 200m stratification. For example, temperature changes within 50-150m also cause stratification change, but it can't be represented by AR5/SROCC method. [Lijing Cheng, China]	Noted. Text revised.
89803	17	11	17	11	As for the previous comment above, the parameter (density) being used to define stratification is not included in the text. It is inferred later with discussion in the context of the pycnocline but it would help to have this information earlier. [Peter Croot, Ireland]	Accepted. This is now added in the first sentence of the section
85229	17	11	17	11	becoming more statified rather than stratifying? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
61207	17	12	17	12	This is either an editorial or substance comment: "since 1970 with a total stratification increase of 2.18%-2.42% of the mean stratification. from 1970 to 2017". This statement is not clear to me. Is this a total stratification increase of 2.28%-2.24% with respect to the 1970 to 2017 mean, or with respect to the 1970 value? I think it should be verified or rephrased. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Now clarified ("long term mean")
96903	17	12	17	12	Perhaps it would be reasonable to give a definition of the "increasing stratification in %". [Nicole Wilke, Germany]	Accepted. We have now clarified (in other words, the 2018 global ocean stratification is 2.18%–2.42% greater than the 1970-2017 mean)
12467	17	13	17	15	A comment: using individual profiles rather than interpolated data won't naturally give you better results. This is the same case in temperature and salinity, nobody used individual profiles to estimate ocean warming or salinity change, because sampling matters a lot. It is better to make it clear here, which gives a basis for the decision on which estimate is used as AR6 final estimate. [Lijing Cheng, China]	Noted. We clarified that the new studies computed stratification on individual profiles then globally gridded the change. We believe this is an advance. The difference with temperature and salinity is we here discuss vertical gradients which might be washed out or not properly conserved in a climatological interpolated product
98833	17	14	17	20	If both the Yamaguchi and Suga estimate of the change in spatially averaged stratification and the Sallée et al. estimate of the average percentage change in the summer pycnocline stratification are retained, this paragraph needs to be very clear that these are not comparable quantities mathematically, even setting aside physical difference. (I have carefully read the Sallée et al. preprint, and am pretty sure I have this right, based in particular on the text of p.3 of that paper.) To illustrate with mathematically convenient values, consider an average of 2 processes, where one goes from 0 to 2, the other from 98 to 100. The average percentage change compared with the time mean is 1/2(200% + 2%) = 101%, while the percentage change in the average is 2/50 = 4%. Choosing the average percentage change (as does the 5-20% per decade increase quoted here) both amplifies the change and heightens the sensitivity to inaccuracies in the measurements of the small values. [Robert Hallberg, United States of America]	Noted. We now make a much clearer point on the difference between the two metrics. In terms of the mathematical difference mentioned in taken % change, it is hard for us to discuss since there is no explanation of the SROCC calculation and it is based on no published results. But we note that the example taken by the reviewer is an extreme case, and when actually applied on Sallée et al. fields, the two way of taking the % change actually give very consistent and similar results.
79917	17	15	17	17	I might suggest to make to include some estra information in this sentence: 'Stratification estimated in the upper 200 m however cuts across many different mixing regimes (within or below the upper ocean mixed-layer depending on location and season) and and do not include information about the vertical distribution of the density difference between the surface and the deeper layers (Somavilla et al. 2017), so here we revise SROCC by providing a more physically consistent stratification change of the seasonal pycnocline (at the base of the surface mixed-layer).' As pointed by Somavilla et al. 2017, due to the facts mentioned now in the report, stratification estimated in the upper 200 ms should not be expected to necessarily reliably inform about changes in the vertical extent of mixing. I'm really happy to see that some of these drawbacks have been corrected in this report. Reference: R. Somavilla, C. González-Pola, and J. Fernández-Diaz (2017), The warmer the ocean surface, the shallower the mixed layer. How much of this is true?, J. Geophys. Res. Oceans, 122, 7698–7716, doi: 10.1002/2017JC013125 [Somavilla Raquel, Spain]	Noted and reference now included. Thank you for the support, we entirely agree with you !
51509	17	15	17	18	These lines explain the reasons for revising the SROCC stratification estimates and basing these on the more physically consistent stratification change of the seasonal pycnocline (base of the surface mixed layer). Would it be possible to include a range of depth estimates this revised measure corresponds to? This would help the reader put into context how the latest estimates compare to the previous SROCC estimate for the upper 200m. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have now revised to better put the new assessment in the context of AR5 and SROCC. In particular we keep the 0-200 m assessment and provide the additional new metric

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68643	17	17	17	17	SROCC and AR5 were adopted by the consensus fo the governments of the world. The authors of this chapter do not have the power to "revise" previous IPCC reports. This line should be revised appropriately. I would suggest changing "..., so here we revise SROCC by providing ... seasonal pycnocline ()" to something like "...; an alternate physically consistent metric is the change in the stratrification of the seasonal pycnocline ()." [Robert Hallberg, United States of America]	Accepted. We revised and dropped "revise"
101755	17	17	17	17	" ... so here we revise SROCC by providing a more.." - Rather than "revising" SROCC it would be better to formulate this as an alternative to the measure provided in SROCC or new measure, since a revision would refer to a similar estimate, whereas this estimate here is a completely different measure. [IAPSO ECS group review, United States of America]	Accepted. The section has been entirely redrafted to include that aspect better
12469	17	17	17	20	It is good to use pycnocline. But it is better to clarify here the difference between this method and SROCC/AR5 method. They are different and not comparable. I don't have access to the unpublished paper, but given the current description, using pycnocline only represents a certain/specific layer of ocean, many of ocean layers do not stratify at the rate of 5-20% per decade. So the quantification here does not represent "upper ocean stratification", this is very important and should be corrected throughout this chapter and also in SPM. [Lijing Cheng, China]	Accepted. We better describe the difference between the metrics; and actually now keep both metrics
98835	17	18	17	18	I agree that examining the stratification outside of the surface mixed layer is much more physically relevant than simply taking the average over a fixed depth. However, there are still arbitrary choies in this new metric of the stratification of the seasonal pycnocline, such as the choice to estimate stratification from a difference of density over 15 m (as opposed to 2 m or 30 m or 100 m), and the definition of the mixed layer as the depth at which sigma0 changes by 0.03 kg m-3 from the value at 10 m. If these choices have contributed to the large error bars in this estimate, that should noted; if not, it should be noted that this is a novel metric whose values depend on arbitrary but reasonable choices that have not been extensively examined in the peer reviewed literature. [Robert Hallberg, United States of America]	Accepted, we now note this is emerging evidence
61267	17	19	17	21	You first raise the 'very likely' stratification increase in the upper ocean 1970-2018 and then you focus on the 'pycnocline' that increased by 5-20% per decade. As these numbers are one magnitufe higher than stated before (p17 line12 & 15), I wonder if you refer here to the stratification increase or the pycnocline depth increase. Please clarify. If the former was true, consdier shortening the text. My suggestion: 'The stratification of the upper ocean pycnocline has very likely increased from 1970 to 2018, with high confidence for a 5–20% increase per decade in summer-time between 60°S and 60°N (Sallé et al., submitted)' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We revised and now clarify the difference between the different metrics, and keep both assessment for 0-200 and pycnocline
68537	17	19	17	21	The "high confidence" claim that "the summer pycnocline has increased by 5-20% per decade from from 1970 to 2018, between 60S and 60N" is imprecise, and supported by a single unaccepted citation. From the source paper, this is the zonal mean of the percentage change in the local stratification. From the same unpublished paper, had this been the percentage change in the spatial mean stratification, this percentage would have been about half as large. Over the 48 years of this analysis, there have been profound changes in instrumentation and data coverage (especially in the Southern hemisphere), opening up the possiblility that there are artefacts related to these changes. Had this analysis been restricted to the Argo / marine mammal period of the past 16 years the instrumental concerns would be less acute, but then it would have been harder to differentiate natural variablility from climate-change related trends. This is a bold claim, and one that is being highlighted in both the ES and the SPM, so it needs to be described precisely what is actually being assessed, and greater care should be taken in describing possible systematic analytic biases. [Robert Hallberg, United States of America]	Accepted, we now refer to limited emerging evidence. Note that the new version of the manuscript of the "source paper" include a investigation of the impact of instrumentation change showing no evidence of any impact on the produced estimate of change

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68539	17	19	17	21	The upper end of the 5-20% per decade rate of increase in pycnocline stratification seems very large to me compared with other published values or model output. Change at the rate of 20% per decade over 48 years would be an increase in stratification by a factor of 2.38. From the same paper, the rate of change in the zonal mean stratification (as opposed to the zonal mean of the local percentage change reported here) would only be half as large. The message that the upper ocean stratification is in fact increasing is profoundly important, but there should be care taken to make sure that these large values are well substantiated. Every author on this chapter is responsible for its contents, and I would urge that every author understand where these large numbers come from and can explain and justify them. If the author team can not reach that point, this language should be brought in line with what every member of the author team can support. With a thinly supported claim of such large changes, the chapter author should be very cautions to avoid a debaucle like the "Himalayan Glaciers will disappear by 2035" fiasco that was used to by adversaries to try dicredit the IPCC. [Robert Hallberg, United States of America]	Noted. The new estimate is now the global mean (more consistent with other estimates). We would like to reassure the reviewer that this assessment was not made by the first author (also LA) of the paper, but by the team of LA of chapter 9. In addition, we have now a more balanced and careful assessment, keeping the 0-200 m metric, and proposing the pycnocline change estimate as emerging evidence
101757	17	20	17	20	" There is very high confidence that increased stratification results from..." - A "high confidence" seems an overestimate given that this result substantially differs from SROCC and is based on a single study. Anything between "low" and "medium" confidence would seem more appropriate given the apparent large difference between studies. [IAPSO ECS group review, United States of America]	Accepted. Now referred to limited emerging evidence
96907	17	20	17	20	Suggestion to use deepened instead of increased. [Nicole Wilke, Germany]	Rejected but reworded for clarity
12471	17	20	17	20	I wonder how "high confidence" is reached here [Lijing Cheng, China]	Accepted. Now assessed as limited emerging evidence
12473	17	20	17	20	5-20% per decade increase means up to 100% increase in the past 50 years, this is not 10 times larger than SROCC/AR5 assessment, it is up to 100 times larger. Issues here: (1) they are not comparable. (2) The uncertainty range seems too large (5-20%) to be useful, so the reliability of this estimate is doubted. Physicial consistent does not guarantee it can work out and is the best assessment. It is also not clear how good this method is compared with other methods used in literature. These questions need to be addressed to back-up this extraordinary assessment result (3) the confidence level in this section should be carefully reconsidered, given only on study examined pycnocline changes. [Lijing Cheng, China]	Accepted. The section has been entirely revised to address those very true comments
101759	17	20	17	21	"high confidence that the summer pycnocline has increased by 5–20% per decade from 1970 to 2018" - It is not clear what an "increase of the pycnocline" means; We suggest to rephrase to clarify. [IAPSO ECS group review, United States of America]	Accepted. Text revised
101761	17	21	17	21	Missing "N" in 60ºN [IAPSO ECS group review, United States of America]	Not applicable. Text removed
95963	17	21	17	21	60N [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
8947	17	21	17	21	"60°S and 60°" should be "60°S and 60°N" [Paban Bhuyan, India]	Not applicable. Text removed
35319	17	21	17	21	missing N after 60 degree : "between 60S and 60N" [Etienne Pauthenet, France]	Not applicable. Text removed
34433	17	21	17	22	There is a syntax problem in this sentence, which makes its meaning unclear. [Claire Waelbroeck, France]	Not applicable. Text removed
101763	17	21	17	22	"increased stratification results from global ocean warming and high-latitude freshening being both surface intensified" - it is not entirely clear what "surface intensified" means here. Perhaps a rephrasing is needed? [IAPSO ECS group review, United States of America]	Not applicable. Text removed
23283	17	21	17	24	It is not clear if this statement is about the summer stratification or not. If this is about the upper stratification in general, Yamaguchi and Suga (2019) presented spatial distribution of warming contributions and freshening ones to the increased stratification and thus should be included in the references here. [Toshio Suga, Japan]	Accepted. Reference added
98677	17	22	17	22	Move "both" to before "being" [Sonya Legg, United States of America]	Not applicable. Text removed
63603	17	24	17	26	"There is high confidence that the stratification of the upper ocean winter pycnocline has increased, but shorter time-series in winter gives low confidence in the rates at global scale (Yamaguchi and Suga, 2019; Sallée et al., submitted)." On of the longest records of subsurface water properties is available from Station P in the subpolar North Pacific and, to date, it does not show a statistically meaningful trend in the strength of permanent (winter) pycnocline (Cummins & Ross, 2020, https://doi.org/10.1016/j.pocean.2020.102329). [Patrick Cummins, Canada]	Accepted. Reference added
89805	17	24	17	26	There is some evidence for winter time density stratification in high latitudes and the subtropics to being reduced due to increased wind forcing. E.g. Somavilla et al. 2017. This is potentially crucial to issues around nutrient supply and the timing and duration of subsequent spring phytoplankton blooms. The Sallee et al. (unpublished) paper cited in the document sees the same trend globally but suggests that it needs to be further validated. [Peter Croot, Ireland]	Accepted. We now add that (very important) reference in the context of change in mixed-layer

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1957	17	26	17	28	The sentence is very hard to folllow, in particular as the 'primary stratification' has not been introduced earlier and 'deep relative to shallow stratification' is not explicit enough, [Hugues Goosse, Belgium]	Not applicable. Text removed
101765	17	27	17	27	The meaning of the term "primary stratitification" is unclear. [IAPSO ECS group review, United States of America]	Not applicable. Text removed
81711	17	27		28	Hard to understand the meaning of this sentence. [Laure Zanna, United States of America]	Not applicable. Text removed
16331	17	28	17	28	There is a Marzocchi & Jansen (2019, Nature Geoscience) reference with links to stratification and global carbon storage that can go here. [Julian Mak, China]	Noted, text removed
99935	17	31	17	41	Figure 9.5 is too small to appreciate anything more than just a general idea of changes in the mixed layer depths being discussed. Perhaps larger panels or a different design? [Dan Helman, United States of America]	Noted, white space reduced
41459	17	33			Please increase figure label font size [Alexander Nauels, Germany]	Noted, white space reduced and labels resized
101767	17	44	17	44	The meaning of the term "climate-scale" is unclear [IAPSO ECS group review, United States of America]	Accepted. Reworded
2459	17	44	18	32	This seems to imply that stratification is increasing everywhere and I find this surprising. Certainly, in the northern regions, such as the Gulf of Alaska Line-P, observations clearly show near-surface warming and freshening which MUST imply increasing stratification. Of that there is no doubt. Plus we have verified the mechanisms thanks to Durack's excellent work. You quote him with salty areas get saltier and fresh areas get fresher. Surely this implies that in equatorial regions the oceans are warming and getting saltier. Also we note that the SST changes are expected to be smaller at low latitudes compared with high. This surely could lead even to a decreasing stratification in the high evaporation areas? [Howard J. Freeland, Canada]	Noted. The low latitude regions are warm, and as a result of the equation of state, salinity has little impact on density. The very large surface warming at low latitude induce a global scale increase of upper ocean stratification even in regions of increasing evaporation, as discussed in the papers references in the section
101769	17	47	17	48	It is unclear what the "overall increasing stratification" is caused by and referring to. Is this in the context of current observed changes or future projections? Please be more explicit. [IAPSO ECS group review, United States of America]	Accepted. We changed the wording
85231	17	48	17	48	that act towards? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text revised.
61269	17	49	17	49	In parenthesis, the reference to Sect. 9.2.1.4 is a self-reference to the section we are in. Consider deletion. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References deleted
34435	17	49	17	49	"(Section 9.2.1.4)" should be erased because this sentence is in section 9.2.1.4… [Claire Waelbroeck, France]	Accepted. References deleted
69077	17	49	17	52	Is it apporproate to assign a "medium confidence" assessment based on only one cited reference to a submitted manuscript. I suspect that there is more evidence in the literature to suppport this finding and request that the authors undertake a more thorough literature search to substantiate the assessment. [Bernadette Sloyan, Australia]	Accepted. We reconsidered "medium confidence". There however very little reference on that aspect to our knowledge. If the reviewer knew more, we would have been happy to consider.
61271	17	50	17	52	Consider referring to Fig. 9.5 in this sub-clause. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Fig 9.5 shows model trends
101771	17	52	17	53	"Such deepening in the summer mixed-layer depth has important consequences for surface ocean biological activity, and is consistent with observed global scale intensification of the surface winds" - This sentence states that the summer mixed layer deepening has important consequences for biological activity, but does not state what those are. Are there observed changes in biological activity or is this a speculative comment? [IAPSO ECS group review, United States of America]	Not applicable. Text removed
1719	17	52	17	56	Provide description of the surface ocean biotic consequences of the deepening of the summer mixed-layer depth. [Michael Kennish, United States of America]	Not applicable. Text removed
61273	17	55	17	55	Consider deleting 'associated with wind, waves and sub-mesoscales'. It holds some redundance, seems somewhat self-evident and does not add much. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79919	17	55	18	2	There is a previous study cited in the previous works cited in this section discussing changes in stratification and MLD in mid-latitudes regions of the global Ocean (subtropical gyres of the North Pacific and Atlantic and mid-latitudes of the Eastern North Atlantic) in agreement with deepening trends reported here by Sallée et al. (submitted). For these reasons, I might suggest to include this reference here. Specifically, I might suggest to change the last sentences as indicated: 'Given the shorter observational record in winter than in summer, there is low confidence in quantifying winter mixed-layer GLOBAL trends (Sallée et al., submitted). However, at mid-latitudes regions data from long-term monitoring programs in combination with Argo floats have shown that at these locations winter mixed-layer depths are deepening at trends between at rates over 10 m · decade-1 (5%) (Somavilla et al. 2017)'. Reference: R. Somavilla, C. González-Pola, and J. Fernández-Diaz (2017), The warmer the ocean surface, the shallower the mixed layer. How much of this is true?, J. Geophys. Res. Oceans, 122, 7698–7716, doi: 10.1002/2017JC013125 [Somavilla Raquel, Spain]	Accepted. We now include this study in our assessment
7835	17	56	18	2	This whole section seems to have very little (other than this sentence) on deep winter mixed layer depths (ie Atlantic/Arctic). Should either discuss here or explicitly say that it is dealt with later in the chapter [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, we lack observational time-series in winter…
81713	17				There is a submitted paper to GRL showing that the background stratification can serve as an emergent constraint for ocean heat uptake efficiency (I will share the link with the authors of Chap 9) [Laure Zanna, United States of America]	Noted.
20169	18	2	18	2	Surely "CFC" here is not a chlorofluorocarbon compound; then, what is it ? [philippe waldteufel, France]	Accepted. Reworded
65867	18	3	39	4	Suggest including Caputi et al. (2015) for how marine downscaling (no atmospheric coupling) of climate model projections to a regional-scale model influence projections for the Leeuwin Current, Australia's poleward eastern boundary current. Suggest including this reference, in part, because there are currently no sections in the chapter which include poleward (downwelling) eastern boundary currents. This reference illustrates how regional downscaling enables models to better capture shelf processes and also resolve currents more confined along the coasts and not represented in coarser-scale climate models. In addition, this reference relates the results of those marine downscaling projections to important fisheries and aquaculture industries for Western Australia. Caputi, N., Feng, M., Pearce, A., Benthuysen, J., Denham, A., Hetzel, Y., ... & Chandrapavan, A. (2015). Management implications of climate change effect on fisheries in Western Australia Part 1: Environmental change and risk assessment. FRDC Project, (2010/535). [Kushla Munro, Australia]	Noted. The reference was considered in our assessment, but not cited since we believe the implication on fisheries is not part of our remit in this chapter
96913	18	4	18	4	Which current observed trend is meant? [Nicole Wilke, Germany]	Noted. Text reworded
98679	18	4	18	32	This whole paragraph needs to be thoroughly proofread, to make verbs and nouns consistent (plural/singular). [Sonya Legg, United States of America]	Noted, text rewritten
65869	18	5	59	10	Recommend the executive summary increase emphasis on uncertainties in the current and likely future mass balance of the East Antarctic Ice Sheet, by including this conclusion in the Executive Summary: "Methodological limitations, linked in particular to estimates of firn compaction rates and corrections for glacial isostatic adjustment (Rignot et al., 2019) and snowfall variability (Palerme et al., 2017a), imply that there is only medium confidence that the East Antarctic Ice Sheet (EAIS) has been close to balance during this period (Bamber et al., 2018b; The IMBIE Team, 2018). There is high confidence that mass loss of the AIS has been dominated by high ice discharge rates over the West Antarctic Ice Sheet (WAIS) and the Antarctic Peninsula." [Kushla Munro, Australia]	Rejected. There is insufficient room for this level of detail in the ES.
99489	18	8	18	9	If there are regional biases, why is the global scale estimate robust? Please clarify. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text removed
12479	18	8	18	10	If authors want to push forward pycnocline method for stratification, they should calculate CMIP6 model result for comparison.The question is: do models replicate the observed changes and why? [Lijing Cheng, China]	Rejected. This would require a published work on that aspect which is lacking to our knowledge. However, we now report both 0-200 and pycnocline
739	18	9	18	9	“Regional biases in individual model” should be models [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Noted, text removed
96919	18	10	18	10	Odd wording 'MLD is filled with … small scale processes' - please revise. [Nicole Wilke, Germany]	Noted, text removed
34437	18	10	18	11	"filled with and" should be erased, it needlessly complicates the sentence. [Claire Waelbroeck, France]	Noted, text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
97989	18	10	18	12	In P15 L14-18 you highlight progress in observational platforms and programs that are resolving salinity variability, it may be useful to point to this when describing "small-scale processes" which are unresolved by ALL current generation models (some of the fresh water lenses are of order 100 m to 1 km, with timescales often sub- the timesteps of coupled atmos-ocean modelling systems) [Paul Durack, United States of America]	Noted, text removed
54349	18	12	18	12	The Langmuir intercomparison of Li et al 2019 could fit here too. But is one of a great many [Luke Van Roekel, United States of America]	Noted. referenced.
23497	18	12	18	13	The challenges in parameterizing the small-scale surface processes, [Saurabh Rathore, Australia]	Not applicable. Text removed and entirely rephrased
46607	18	15	18	16	I hate to be that person, but more relevant publications would be Heuzé et al. (2013), doi:10.1002/grl.50287, and Heuzé (2017), doi:10.5194/os-13-609-2017 (actually cited later in that chapter) [Céline Heuzé, Sweden]	Noted. Referenced now considered.
61209	18	16	18	16	"the current generation climate model represent". I would switch to "current generation climate models represent" for grammatical consistency and because I suppose this statement does not refer to a unique modell. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text revised
46609	18	16	18	18	No; in the Southern Ocean, open ocean deep convection is a common unrealistic feature in CMIP5 models (Heuzé et al. 2013), and in CMIP6 as well (Heuzé 2020, subm to Ocean Science Discussions). It badly affects their water mass properties and projected changes (Heuzé et al. 2015, doi:10.1175/JCLI-D-14-00381.1) [Céline Heuzé, Sweden]	Accepted. More balanced assessment in now provided
101773	18	16	18	18	"Despite these limitations, the current generation of climate model represent all the main features of historical mixed layer structure, giving high confidence in their first order water-mass formation mechanisms, and large-scale circulation features" - I am somewhat uncomfortable with taking the shortcut from a good simulation of mixed layers to a good simulation of water masses and large scale circulation. I suggest to phrase this a bit more carefully, e.g., "a good representation of ML is a prerequisite for a realistic simulation of water masses and ocean circulation" or so. [IAPSO ECS group review, United States of America]	Noted. Text revised
34439	18	17	18	18	This statement is contradictory with the plot of CMIP6 model biases in winter (Fig. 9.5), where one clearly sees that the data pattern is only partly captured by the models, with biases of more than 50% of the observed values (e.g. ~250 m bias with respect to ~400 m observed in the North Atlantic). [Claire Waelbroeck, France]	Accepted. Reworded to note qualitative agreement
85233	18	18	18	18	figure 9.5 is not very clear (its obviously hard to present clearly due to the huge spatial and seasonal variations of more than an order of magnitude in mixed layer depth and associated model biases)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure redrawn
61381	18	21	18	21	Please, as CFC is for the first time use in this chapter, consider giving the signification of this Acronym [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
97991	18	21	18	21	"..CFC biases" this is a little vague. A CFC "bias" would just suggest that the ocean circulation pathway doesn't align with the sparse observational network of CFC point measurements, which are extrapolated to provide the gridded fields [Paul Durack, United States of America]	Not applicable. Text removed
46611	18	23	18	32	First, reference to Heuzé et al. (subm to Nature Climate Change, JB Sallée received the draft) is missing, and in fact they show a relatively high confidence [Céline Heuzé, Sweden]	Rejected. This manuscript was not accepted by the acceptance cut-off deadline so cannot be included in WG1 AR6 report
61211	18	25	18	25	"challenges with mixed-layer with surface layer parameterisation" is not clear to me. Did you mean "challenges with mixed-layer models with surface-layer parameterisations"? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed and entirely rephrased
98681	18	25	18	25	Delete "with surface layer" (repetition, coming after "with mixed layer") [Sonya Legg, United States of America]	Not applicable. Text removed and entirely rephrased
34441	18	25	18	25	"with mixed-layer with surface layer parameterisation": only one of the two should be kept: either "with mixed-layer", or "with surface layer parameterisation". [Claire Waelbroeck, France]	Not applicable. Text removed and entirely rephrased
82885	18	25	18	25	I wonder if this should be "with mixed layer and surface layer", instead of "with mixed layer with surface layer". [Sebastian Gerland, Norway]	Not applicable. Text removed and entirely rephrased
741	18	25	18	25	“Challenges with mixed-layer with shallow layer parameterisation”… this doesn’t quite make sense to me, should it be “Challenges with mixed-layer and shallow layer parameterisation”? Consider restructuring for clarity [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed and entirely rephrased
35321	18	25	18	25	"with mixed layer with surface layer" which one is it surface or mixed? [Etienne Pauthenet, France]	Not applicable. Text removed and entirely rephrased
23499	18	26	18	27	as the increased stratification overcomes the increased surface turbulence. [Saurabh Rathore, Australia]	Not applicable. Text removed and entirely rephrased

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
207	18	27	18	30	Oops - looks like this sentence has been edited so many times that it's become a little jumbled. Could use a quick assist. [Patrick Orenstein, United States of America]	Accepted. It is now revised.
1721	18	27	18	32	Include information at the end of line 32 on the effects of mixed-layer shallowing and mixed-layer deepening on biotic productivity and other biotic processes. This information can be added at the end of line 32. [Michael Kennish, United States of America]	Rejected. This is not part of our mandate
12481	18	28	18	28	How does shallowing of the mixed layer correspond to stratification? Again, a clear definition of stratfication should be prescribed. [Lijing Cheng, China]	Accepted. Definition of stratification has now been clarified
65865	18	28	36	28	Suggest including Sun et al. (2012), regarding marine downscaling projections, as an important reference for how increased resolution improves the simulation of the strength and position of the East Australian Current. In addition, Sun et al. (2012) find the EACs main jet core and EAC extension strengthen in climate projections. The group of references listed here could be re-organized so that the corresponding reference follows after each region, as it is currently unclear which corresponds to the EAC. Sun, C., Feng, M., Matear, R. J., Chamberlain, M. A., Craig, P., Ridgway, K. R., & Schiller, A. (2012). Marine downscaling of a future climate scenario for Australian boundary currents. Journal of Climate, 25(8), 2947-2962. [Kushla Munro, Australia]	Noted. Referenced now considered and text revised and shortened
96921	18	29	18	29	Should be Arctic instead of subpolar ocean in general, please revise. [Nicole Wilke, Germany]	Accepted. Text revised
19269	18	29	18	30	Considering Fig 9.5, the region showing mixed layer deepening would be "polar ocean" rather than "subpolar ocean" [Anne-Marie Treguier, France]	Accepted. Text revised
101775	18	29	18	30	"subpolar ocean" should be specified to "subpolar Arctic ocean" since the reference Lique et al. 2018 only refers to the Arctic and following sentence states a shallowing for the Southern Ocean. [IAPSO ECS group review, United States of America]	Accepted. Text revised
105473	18	30	18	30	Based on existing observations they find similar results in Våge et al., 2018, https://doi.org/10.1038/s41467-018-03468-6): Ocean convection in the western Iceland Sea linked to the recent ice edge retreat along east Greenland. [Helene R. Langehaug, Norway]	Noted.
105471	18	30	18	30	There seems to be some confusion about the regions with projected deepening of the mixed layer: In Lique et al. (2018) the deepening is found in the southwestern part of the Nordic Seas and the high Arctic. It seems (based on Fig. 9.5) that the western Nordic Seas show a deepening of the mixed-layer in future projections. [Helene R. Langehaug, Norway]	Accepted. Text revised
100123	18	37	20	17	There are potential issues with defining MHWs relative to a fixed historical baseline. While the mean SST is rising, the variance of the SST may not change. So what might be called more intense and persistent MHWs in the future may represent nothing more than a higher mean (gobal warming). In fact, calling something a ‘permanent MHW’ is counterintuitive and potentially confusing to the public. One could instead talk about extreme heat as the combination of mean warming and MHWs (extremes relative to the trend). See Jacox (2019) for more discussion of this issue. These issues come up throughout the report. Jacox, M.G. (2019). Marine heatwaves in a changing climate. Nature, 571, 485-487. [Steven Bograd, United States of America]	Accepted. We have now made very clear what is our definition of future MHWs as the first sentence of the paragraph on future MHWs. The reference is a "news and views" piece that is not peer reviewed, so we refrain to cite it.
101777	18	37	20	17	There is no mention of the sub-surface structure of surface MHWs or MHWs occuring at depth. While the majority of publications have focused on SST there are a few recent studies out there that have assessed depth structures associated with MHWs in specific regions. If the authors feel that there is no space for additional citations, as a minimum it could be mentioned that little is known about the depth-structures but that these will be important to assess in the future in order to e.g. understand more about the driving processes and regional impacts. [IAPSO ECS group review, United States of America]	Accepted. We now clarified MHWs can occur at subsurface
2461	18	37	20	17	The section on Marine Heat Waves is good, but a couple of comments. 1) Don't you think it is time to retire the name "The Blob"? I know I am at fault in this usage. In a recent paper (see next comment) I had to use the name to keep referees happy. [Howard J. Freeland, Canada]	Accepted. Blob has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2463	18	37	20	17	The section on Marine Heat Waves is good, but a couple of comments. Please look at a paper of mine that shows, among other things, that the Marine Heat Wave that started at the beginning of 2014 has never really gone away. Even as I write, conditions in the Gulf of Alaska are profoundly different from anything pre-2014. 'The Blob' - or, how unusual were ocean temperatures in the Northeast Pacific during 2014-2018? Howard Freeland and Tetjana Ross, Deep-Sea Res. 150, 2019. https://doi.org/10.1016/j.dsr.2019.06.007 See Figure 5, I have to wonder if this could be the first example of a permanent MHW? You cite the event as 2014/15, I'd say it has a much longer duration. Look at our Fig 5 please. [Howard J. Freeland, Canada]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
68541	18	37	20	17	The discussion of Marine Heatwaves requires an explicit definition of Marine Heatwaves as anomalies relative to a fixed baseline (I think that 1981-2010 is often used), and not anomalies relative to an average over the preceeding 30 years (as might be assumed from the WMO definition of "climate" as a 30-year mean). Without this definition, the changes in statistics could easily be misinterpreted as resulting from changing variability, and not the simple result of a long-term trend being assessed relative to a fixed reference period. Human and natural systems do adapt to conditions they have experienced in recent years, so I think that the anomalies relative to a 30-year running mean would be more helpful for understanding the impacts of heat-waves, but in assessing the literature we are stuck with the definition that has been used in the publications. However that defintion needs to be explicitly given to help the readers understand what is being discussed.. [Robert Hallberg, United States of America]	Accepted. We agree with the reviewer. We now made the definitions explicit both for past and future MHWs
27589	18	39	18	41	Mentioning contributors at the beginning of this box may affect the readability of the text. Would it be possible to place it at the end of the box? At least it should be consistent with the other Boxes. [Eric Brun, France]	Taken into account. We slightly adjusted the structure of the box, but we prefer providing definitions and drivers early
67215	18	43	18	43	remove acronym MHW. The report becomes unreadable with the excessive use of acronyms. In most cases, as context is clear, it can be replaced by 'heat waves' [Regine Hock, United States of America]	Rejected. MHW is a standard acronym in assessed literature
101779	18	43	18	44	Consider mentioning that MHWs are generally defined based on a percentile threshold (90th or 99th). I assume that the authors want to avoid going into detail about different definitions. [IAPSO ECS group review, United States of America]	Taken into account. We clarified the relationship between the definition of MHW and the different categories based on the definition.
40601	18	43	18	50	Note thtat the SOD glossary definition for MHWs (coming from SROCC) is "A period of extreme warm near-sea surface temperature that persists for days to months and can extend up to thousands of kilometres." [TSU WGI, France]	Accepted. Definition clarified in the box
101783	18	43	20	17	Cross-chapter box 9.1: It is worth mentioning the prominent marine heatwaves in the southwestern Atlantic as well since most of the examples given are from the northern hemisphere (Manta, G., de Mello, S., Trinchin, R., Badagian, J., & Barreiro, M. (2018). The 2017 Record Marine Heatwave in the Southwestern Atlantic Shelf. Geophysical Research Letters, 45(22), 12-449. and Rodrigues, R. R., Taschetto, A. S., Gupta, A. S., & Foltz, G. R. (2019). Common cause for severe droughts in South America and marine heatwaves in the South Atlantic. Nature Geoscience, 12(8), 620-626.) [IAPSO ECS group review, United States of America]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
61239	18	46	18	47	The reference list is biased to the Pacific. Please add 'e.g' or add a reference to a global assessment of MHW as Fröhlicher et al. (2018, https://www.nature.com/articles/s41586-018-0383-9) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
27591	18	52	18	53	These events have occurred over the last two decades but the exact dates are not given. [Eric Brun, France]	Accepted. Dates clarified
7143	18	52	19	6	Please consider including the following reference, that connects extreme draught events to marine heatwves in SouthWestern Atlantic: "Rodrigues, R. R., Taschetto, A. S., Gupta, A. S., & Foltz, G. R. (2019). Common cause for severe droughts in South America and marine heatwaves in the South Atlantic. Nature Geoscience, 12(8), 620-626." [Guilherme Longo, Brazil]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
69081	18	52	19	55	Can we include an example in the Southern Hemisphere. I sugest citing marine heatwave in the Tasman Sea or Indain Ocean [Bernadette Sloyan, Australia]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
33077	18	53	18	53	generally, caspean sea should be included in consideration. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
32747	18	53	18	53	generally, caspean sea should be included in consideration. [sadegh zeyaeyan, Iran]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12

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19449	18	53	18	53	Generally, Caspian Sea should be included in consideration. [Mostafa Jafari, Iran]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
23997	18	54	18	54	write "... , southwest (Rodrigues et al., 2019) and northwest Atlantic (Mills et al., 2014; ...", instead of "... and northwest Atlantic (Mills et al., 2014; ..." [Moacyr Araujo, Brazil]	Not applicable. Text has been removed
88585	18	55	18	55	Which is the correct one: Laufkötter or Laufkotter ? [Rosemary Vieira, Brazil]	Accepted. Corrected
23999	19	1	19	1	write "... persisting impacts on the climate of the surrounding continental regions, and marine ecosystems ranging ...", instead of of "... persisting impacts on marine ecosystems ranging ..." [Moacyr Araujo, Brazil]	Rejected. The sentence and references therein focus on impacts on marine ecosystems.
65873	19	1	91	20	Suggest strengthening the explanation of "Structured Expert Judgment (SEJ)" or refer to its explanation in another chapter. The SEJ is a concept not well understood even in the scientific community, and may be read as "educated guess" if not clearly explained. [Kushla Munro, Australia]	Rejected, not applied to Cross-Chapter Box 9.1.
65877	19	1	190	11	Suggest further explanation regarding the colour scale. It shows a continuous range of colours but the subplots appear to show discrete colours. The continuous range of colours in the colour scale makes it difficult to see what the corresponding values are in the subplots. Suggest clarifying how the contours are defined in b and c. [Kushla Munro, Australia]	Accepted. Figure revised
65879	19	1	223	1	Suggest consistency. The compound adjective is interchangeably presented as 'sea-level' and 'sea level' e.g. Page 10 - Line 43 'sea-level change'; Page 10 - Line 11 'sea level change'. Both are common in the literature but need to be consistent in this report. Suggest also being consistent with: 'sea ice'. [Kushla Munro, Australia]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
29645	19	4	19	5	what is "hundreds of km's of kelp"? Should it be "hundreds of km2's of kelp"? Or something else. [Aixue Hu, United States of America]	Taken into account, slightly revised the sentence.
61241	19	5	19	6	The reference to 'These impacts' is unclear. Wernberger et al. (2016) generally speak of impacts on the 'biodiversity pattern'. I suggest the following reformulation: Biodiversity changes are still evident today (Wernberger et al., 2016). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, slightly revised the sentence.
101781	19	8	19	13	It could be mentioned here that the MHW definition uses a fixed baseline. [IAPSO ECS group review, United States of America]	Accepted. Definition clarified
61181	19	8	19	26	In this paragraph dealing with the frequency of MHW, it would be worthwhile noting that the 2014/15 MHW in the northeast Pacific (the largest of all known MHWs) was estimated using observational records to have a return time of 100 years, i.e., it is a once-per-century event; see Freeland and Ross, 2019, Deep-Sea Research, https://doi.org/10.1016/j.dsr.2019.06.007. [Patrick Cummins, Canada]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
99491	19	10	19	10	Suggest liaising with the Atlas to use the Interactive Atlas to visualise these results (like those shown in the box figure). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Extensive discussions with the Atlas about which SST variables and their statistics could be reasonably included were carried out. The SST, but not the extreme statistics, were included.
88587	19	10	19	11	Which is the correct one: Laufkötter or Laufkotter ? [Rosemary Vieira, Brazil]	Accepted. Corrected
99493	19	16	19	21	Have the calculations of the %ages been derived from detrended data, would be useful to state (and if not what the influence might be on the numbers). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We now provide reference for the calculation providing traceability of the method
80657	19	21	19	22	MHW" is missing between 'IV)' and 'during on line 22 [Helene Jacot Des Combes, Marshall Islands]	Accepted. Text corrected.
65871	19	22	62	25	Suggest the following point be included in the Executive Summary given its importance for projections of sea level rise: "[There is] limited scenario dependence of projected mass losses of Antarctica. As shown in (Seroussi et al., submitted), this is in part due to the complex interplay between mass gain from increased snowfall and mass loss due to ice shelf basal melting (and consequent speed up of glacier flow)." [Kushla Munro, Australia]	Rejected, not applied to Cross-Chapter Box 9.1.
22567	19	24	19	24	Is 'the blob' a scientific enough term for an IPCC report? [Peter Thorne, Ireland]	Accepted. We removed the term
68105	19	24	19	25	I agree on the difficulties of attributing this regional behavior to anthropogenic forcing and because this fact is important to decision makers and experts in calculating the flooding and inundation levels on the coast and estuarine systems where the sea level rise could be higher because of morphology and river run off in the Eastern Boundary Upwelling Systems region where the cold-water mass is replaced by a warm water one during months. [Hernan Moreano, Ecuador]	Rejected, not applied to Cross-Chapter Box 9.1.
68107	19	24	19	25	Reference: Moreano H.R., Zambrano E., Suescum R., Paredes N., (1986). El Nino 1982-83, su Formación, su Desarrollo y sus Manifestaciones en Aguas Ecuatorianas. Acta Oceanográfica del Pacífico 3, 01-23. Paper available at www.inocar.mil.ec/publicaciones [Hernan Moreano, Ecuador]	Rejected. We appreciate the suggestion, however, the paper is on ENSO not MHW.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81443	19	24	19	25	Towards the end of 2019, a new MHW has again emerged in the N Pacific arising concerns that an event similar to the "Blob" is reappearing. This 2019 warm expanse ranks as the 2nd largest marine heatwave in terms of area in the northern Pacific Ocean in the last 40 years, after the "Blob". [Carlos Garcia-Soto, Spain]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
52519	19	24	19	25	I would recommend to include in Cross-Chapter Box 9.1 Marine Heatwaves, in page 19 line 24, after (Hobday et al., 2016). If the affected area is huge, so is the kelvin wave high with considerable impact on sea level (Moreano et al.,1986). [Hernan Moreano, Ecuador]	Rejected. Thank you for the suggestion. However, the reference is outside the scope of this box.
24001	19	26	19	26	write "... (2016). In 2013/14 eastern South America experienced one of its worst droughts. The drought was linked to suppression of the South Atlantic convergence zone and its associated rainfall, which led to water shortages in Brazil and impacted food supplies globally. The increased shortwave radiation due to reduced cloud cover and reduced ocean heat loss from weaker winds are the main contributors to the establishment of marine heatwaves in the region. Atmospheric blocking triggered by tropical convection in the Indian and Pacific oceans can cause persistent anticyclonic circulation that not only leads to severe drought but also generates marine heatwaves in the adjacent ocean. This mechanism, which involves droughts, extreme air temperature over land and atmospheric blocking explains approximately 60% of the marine heatwave events in the western South Atlantic (Rodrigues et al., 2019). ...", instead of "... (2016.)" [Moacyr Araujo, Brazil]	Rejected. For space constraints we cannot comment on specific events. Regional aspects are treated in Chapter 12
86815	19	28	19	29	It would be interesting with a sentence or two elaborating on exactly how the anthropogenic global warming is altering the probability of MHW occurrences, and which factors are most important in determining where the MHWs will most likely occur in the future. [Oyvind Christophersen, Norway]	Accepted. We have clarified change of probability of occurrence
5613	19	28	19	29	The NAO generates also a reponse in the water cycle (streamflow, groundwater) and water level in the sea. Cf ref : Fritier et al., 2012 CR Geoscience; Chevalier et al., 2014 Hydrological Sc. J.; Laignel et al., 2010 IAHS publ, Massei et al., 2017 Journal of Hydrology; Turki et al.,2019 Oceanologia [Benoit Laignel, France]	Rejected. Thank you for the suggestion. However, the references are outside the scope of this box.
82887	19	28	19	30	I wonder if a confidence level could be added to this statement. [Sebastian Gerland, Norway]	Taken into account. We have added very likely range
61183	19	28	19	30	The SROCC is careful to state that attribution studies of MHWs are based on simulations with climate models. In particular, on page 6-29 of the SROCC is it stated: "On a global scale and at present day (2006–2015), climate models suggest that 84% to 90% (very likely range) of all globally occurring MHWs are attributable to the temperature increase since 1850–1900 (Fischer and Knutti, 2015; Frölicher et al., 2018)." [Emphasis added.] A similar qualification should be made here. [Patrick Cummins, Canada]	Rejected. We reference the SROCC and their probability and confidence assessment. All references can be found in the SROCC
99495	19	28	19	32	Suggest including this paragaph before the text on modes of variability. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. text on mode of variability has been removed
33417	19	28			Do not start a sentence by a number: "84-90% of all…". [Guiomar Rotllant, Spain]	Taken into account. Text revised.
65875	19	30	115	33	Suggest clarification. Suggest changing to: "Substantial global mitigation endeavours could lead to an emissions trajectory that follows emissions scenarios, leading to GSAT stabilizing around 1.5°C-2°C above pre-industrial levels by 2100, and physical uncertainties associated with the different contributions to GMSL rise could all break toward the low end of their likely ranges." [Kushla Munro, Australia]	Rejected, not applied to Cross-Chapter Box 9.1.
88589	19	32	19	32	Which is the correct one: Laufkötter or Laufkotter ? [Rosemary Vieira, Brazil]	Accepted. Corrected
6757	19	40	19	40	Change "of satellite observations" to "from satellite oibservations". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. text revised
20171	19	43	19	44	The hatched areas are hardly visible [philippe waldteufel, France]	Accepted. Figure revised
20545	19	50	19	51	Figure CCB9.1-1 shows nothing about spatial extent nor intensity [philippe waldteufel, France]	Rejected. Not all can be shown in the figure, but references support our statements and assessments
85235	19	56	20	1	Is it definietly accurate that seasonal and year to year variations are small in the Acrtic, as the sentence in its current format implies? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text has been removed
39189	20	1	20	2	"medium confidence" should be italicized if it is an uncertainty language. [Lourdes Tibig, Philippines]	Accepted. Text italicised
20547	20	2	20	6	This possible evolution toward a state of permanent heatwave suggests that heatwaves might then be just a way to characterize ocean global warming. However, it is said (page 18 line 43) that MHW are discrete events. This does not seem compatible with permanency [philippe waldteufel, France]	Accepted. Definition adjusted
1723	20	2	20	6	What ecosystem and fisheries changes are projected for a near-permanent MHW state in many parts of the ocean by the late 21st century? If these potential changes have been investigated in the literature, they should be included in this part of the chapter. [Michael Kennish, United States of America]	Rejected. Impact on ecosystems and fisheries is beyond the scope of chapter 9. For assessment by WGII

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101785	20	2	20	7	It is important to clarify that these projections are the case under a fixed basline. Since there are discussions whether using a moving baseline will be more appropriate, depending on adaptions of e.g. ecosystems in specific regions. [IAPSO ECS group review, United States of America]	Accepted, MHW definition clarified.
39191	20	2	20	11	No calibrated language in the projections for MHWs? [Lourdes Tibig, Philippines]	Accepted. Very likely range now provided
15203	20	4	20	11	The MHW projections here and elsewhere require context on what is being used as a baseline. This is important so that readers, especially those not familiar with the technical definitions and code for being used by most authors calculating MHW, understand that longer and more frequent MHWs does not necessarily mean "extremes" above the future baseline (e.g. what would feel like an extreme in 2081-2100 SSTs, given the mean SSTs for that period), but rather that the secular trend is leading to more days of SSTs exceeding the current baseline. The distinction is important ecologically, as some organisms and systems may acclimatise. For example, see the Logan et al. (2014) Global Change Biology work that looked at the impact of a rolling baseline on heat stress projections for coral reefs. [Simon Donner, Canada]	Accepted, MHW definition clarified.
88591	20	11	20	11	Which is the correct one: Laufkötter or Laufkotter ? [Rosemary Vieira, Brazil]	Accepted. Corrected
27593	20	13	20	13	What are the threats posed by these changes ? We feel that there is a lack of development about the impacts. [Eric Brun, France]	Rejected. Impact is beyond the scope of chapter 9. For assessment by WGII
101787	20	14	20	16	"While the physical understanding of MHWs and their ecological consequences is rapidly emerging, the science community is challenged by the ability to predict and monitor MHWs given the increasing pace at which they are occurring worldwide" - and the manifold involved processes that can act as a driver of MHWs [IAPSO ECS group review, United States of America]	Not applicable. Text has been removed
85237	20	17	20	17	I'm not sure about its review but there was a submitted paper showing considerable dependence of the representation of marine heatwaves off New Zealand on ocean model resolution by Erik Behrens (NIWA-Erik.Behrens@niwa.co.nz) [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted
85027	20	22	24	47	No comments [Katrine Husum, Norway]	Noted
107451	20	22			Suggest different title for Section 9.2.2, considering this section includes change in heat and salinity, but also includes water masses. As it is now, Section 9.2.2.3 on Water Masses does not seem to fit in Section 9.2.2 titled Change in Heat and Salinity [Jennifer Walker, United States of America]	Rejected. Water mass change are directly related to change in heat and salinity
101797	20	24	24	34	We find section 9.2.2.1 (OHC) long and somewhat convoluted; if you wish to shorten the report text, 9.2.2.1 could be streamlined/shortened. [IAPSO ECS group review, United States of America]	Noted. We tried streamlined by better introducing SROCC AR5 and previous chapters
85239	20	26	20	26	For heat content I always find the very large number is joules hard to grasp. In contrast, I find changes in volume-mean temperature easier to get a grasp of? Similarly for heat content trends in PW I find equivalent area mean surface fluxes Wm-2 easier to get a grasp of? It is worth considering presenting both? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We present mean ocean temperature change where relevant, esp. For paleo and committed change
7837	20	26	20	47	There are a number of grammatical issues/typos in this paragraph [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Entirely reworded

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12485	20	26	20	55	I strongly oppose to use Zanna et al. 2019 as "observation" here to be compared with model. The reasons are listed here: (1) Zanna et al. is not based on observations of ocean subsurface temperature which are OHC observations, instead, it is based on SST. (2).It is a reconstruction based on a single physical process: it assumes subsurface changes are due to surface transportation, and this vertical transportation does not change over time. This assumption is aparently not true, and over-simplified, which neglects many other important processes (changes in ocean circulation etc.). This assumption works not bad over very long time scale, so it was used by Zanna et al. to reconstruct OHC since late-1800s. So the long-term change can be used for some sort, but not for shorter periods less than ~50 years. (3). The vertical heat transport information used in Zanna et al reconstruction comes from an ocean reanalysis product, ECCO, which spans from 1990s to present. The reanalyses did poor job in representing OHC changes, see Palmer et al. 2018 Climate Dynamic paper. ECCO is one of the worest product representing OHC changes compared with real observations, which indicates that this reanalysis data contain huge biases in the deeper ocean. (4). The method of Zanna et al is based on an additional assumption for the driver of OHC: OHC is driven by SST. However this is again an oversimplified assumption. We know local SST change is driven by air-sea heat flux and ocean processes including vertical/horizonal advection and mixing in the mixed layer. However, local OHC change is a balance of heat flux and heat convergence/divergence within in an ocean volume. Their controling processes has fundermental difference, so SST drives OHC framework is a over-simplified one. (5) Given all these issues above, it is more proper to label Zanna et al. as a reconstruction, which is based on simplfied processes, rather than "observation". And it should not be used to benchmark models, especially models have more or less complete physical processes, so it is not an apple-to-apple comparison. (6). An assessment of Zanna et al. time series is emergently needed: Is this reconstruction superior than observational estimates given some recent new developments (Ishii et al. 2017, Cheng et al. 2017, update of Levitus et al. 2012 and other Argo-based products)? A simple test is to compare Zanna et al. estimate with other observational datasets in the Argo perids since 2005: Zanna et al. is 2/3 smaller than all other products (chapter-2 fig.2.25), and at least 1/3 smaller than other estimates since 1990s. (7) Last, if this method is superior than all other real observational based estimates, why should we put so many efforts and money to Argo and other subsurface observation system?? [Lijing Cheng, China]	Accepted. Zanna is not used for observations. We use Zanna in addition to observation, but mentioned this is a "hybrid" reconstruction not only based on observations
2465	20	28	20	28	This line says "There is high confidence that this ocean heat content increase represents over 90% of the total excess heat.." This is a very important number, and I'm surprised that we get "over 90%" whereas AR5 expressed greater confidence and put the number at 93%. This might suggest that we have lost a little confidence in this number. Can one quote a best estimate? Would you still agree with the AR5 figure. I note that the SPM of AR5 did say "over 90%" as you do, but chapter 3 was more specific. [Howard J. Freeland, Canada]	Noted. We report chapter 7 assessment.
61275	20	30	20	33	As this sentence holds no distinct confidence qualifier, consider dropping the repetative referenc to Section 2.3.3.1; Section 7.2.2.2; Cross chapter Box 9.2, just raised before. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text entirely rewritten
101789	20	30	20	34	"Confidence in this assessment is strengthened since AR5 and SROCC by longer observational time-series, further bias correction and understanding of uncertainty, consistent closure of both global sea level and energy budgets for the period 1971-present, and increasing lines of evidence from novel estimates of ocean warming largely independent of subsurface observations" - The progress mentioned here since AR5 and SROCC needs to be documented with the respective references to publications that report this progress. [IAPSO ECS group review, United States of America]	Accepted. It is now clearer that we refer to assessment made in previous chapters
108119	20	31	20	31	Instead of the term “bias correction” it might be more appropriate to use the term “bias adjustment”, which is explained in Chapter 10 Section 10.3.1.4.2 and used in Chapter 2, 8, 10 and 12. [Claas Teichmann, Germany]	Taken into account. We avoided using the term bias correction in this section
99497	20	34	20	34	Should "evolved" be "improved" here? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, we prefer the term evolve which is more neutral
12483	20	34	20	36	Besides of observation itself. Techniques to process data and fill data gaps matter a lot. For example, OHC time seris in AR5 are calculated based on enssentially the same database (WOD, plus <1% additional data), but the results from different groups show with huge difference. The progress in data processing is worthy a discussion (bias correction, gap-filling, quality control etc.) [Lijing Cheng, China]	Noted, though this is chapter 2, and cross-chapter Box 9.2 matters. Here we report conclusions from previous chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35063	20	34	20	36	This sentece is over-long and hard to interpret. Suggest replace with "The ocean observing system has evolved over time through the incorporation of new technologies. This results in data sets with varying depth and spatial coverage and from a range of sensors. Thus, calculations of global temperature or of heat content change by depth range have accuracies that are time-period and depth-range dependant. [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded
30731	20	36	20	38	This part should be sycronized with the Chapter 3 Page 46 Lines 25-32 [Iskhaq Iskandar, Indonesia]	Accepted. Consistency with Chapter 2, 3, 7 is now ensured on the vertical decomposition
86817	20	36	20	38	This is new information since AR5 and the SR 1.5. Please consider to mention this fact in the sentence, as it is interesting to put emphasis on this updated information. [Oyvind Christophersen, Norway]	Accepted. The new or updated assessment are now clearly highlighted
61277	20	36	20	38	The given relative estimates of OHC increase between 1970-2018 are not consistent with Table 2.7. The Table rather suggests the following split: 70%, 25% and 5% for layers 0-700m, 700-2000m and >2000m respectively. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Consistency with Chapter 2, 3, 7 is now ensured on the vertical decomposition
6759	20	36	20	38	This is a strange expression of high confidence. How can one have high confidence that the various percentages are exactly 60, 30 and 10? Shouldn't these three percentages be quoted as ranges of values? Or at least specify ranges for the 0-700m and 700-2000m layers. The percentage below 2000m does not need to be quoted as it can be simply calculated by the reader from the other two percentages. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable, text removed
97993	20	36	20	38	Ch3 Fig 3.25 currently breaks the modelled (CMIP5) warming down into the 3 layers you note, with 65% 0-700m, 20% 700-2000m and 15% > 2000m listed in Ch3 Ex Summ and Ch 3.5.1.3. We will need to synthesize these estimates across obs (Ch2) and models (Ch3) [Paul Durack, United States of America]	Not applicable, text removed
77529	20	37	20	47	Multiple typos in paragraphs that require correction. [Emer Griffin, Ireland]	Accepted. Text entirely revised and proof read
33419	20	38			I would suggest to separate units (m) from number in the following sentence: "0-700m layer, 30% in the 700-2000m layer and 10% in the 2000-6000m layer…" and all over the chapter. [Guiomar Rotllant, Spain]	Not applicable, text removed
6761	20	39	20	39	"reservoirs" should be "reservoir". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected
82889	20	39	20	39	I wonder if this should be "reservoir" or "one of the major … reservoirs". [Sebastian Gerland, Norway]	Accepted. Corrected
132563	20	39	20	39	"Southern Oceans" should be "Southern Ocean". [Kyle Armour, United States of America]	Accepted. Corrected
743	20	39	20	39	"the Southern Oceans is the major anthropogenic excess heat reservoirs" inconsistent pluralisation, should it be "are" rather than "is" or should some of the plurals be changed [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected
97995	20	39	20	42	It is worth noting that there is considerable literature that has highlighted the poor S. Hemi coverage as a potential blindspot for Earth's ernergy imbalance estimations, these include Gregory et al 2004 doi: 10.1029/2004GL020258; Gouretski & Koltermann 2007 doi: 10.1029/2006GL027834; Gille 2008 doi: 10.1175/2008JCLI2131.1; Durack et al 2014 doi: 10.1038/nclimate2389; amongst numerous others. The recent estimate from Argo with 67-98% in the S. Hemi extratropical ocean 2006-2013 (Roemmich et al 2015 doi: 10.1038/nclimate2513) is a motivator to further attempt to quantify what may have been missed due to incredibly poor S. Hemi data coverage [Paul Durack, United States of America]	Accepted. We have added a sentence to describe that aspect
209	20	39	20	45	Language needs some general cleaning up. [Patrick Orenstein, United States of America]	Accepted. Text rewritten and proof-read
6763	20	41	20	41	"where" should be "when". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected
12487	20	42	20	42	NCEI has some caveats in Southern Hemisphere because it is drifted to climatology (Durack et al. 2014, Nature Climate Change; Cheng et al. 2014, GRL). (1). Cheng L., and J. Zhu, 2014: Artifacts in variations of ocean heat content induced by the observation system changes, Geophysical Research Letters, 41(20), 7276-7283, http://dx.doi.org/10.1002/2014GL061881 (2) Durack, P.J., P.J. Gleckler, F.W. Landerer, and K.E. Taylor. 2014. Quantifying underestimates of long-term upper-ocean warming. Nature Climate Change 4(11):999–1,005, https://doi.org/10.1038/nclimate2389. [Lijing Cheng, China]	Not applicable, text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23501	20	42	20	45	Please discuss about the asymmetrical global ocean warming in the 0-2000 m observed during 2005-2015. The observed global ocean warming of 0-2000 m depth over the period of 2005-2015 is consistent with the multi-model mean of the climate model simulations (CMIP5). However, the contrast of heat gain between the hemispheres is due to the asymmetric internal climate variation which enhanced southern hemisphere warming and reduced northern hemisphere warming. "Recent hemispheric asymmetry in global ocean warming induced by climate change and internal variability" "https://doi.org/10.1038/s41467-020-15754-3" [Saurabh Rathore, Australia]	Accepted. This is now discussed and the findings of the mentioned paper are assessed
132565	20	43	20	43	Should be "have stored a large amount" [Kyle Armour, United States of America]	Taken into account. We revised the sentence by including "a" before large amount
85241	20	43	20	43	NCEI - what is this dataset, could you write this in full? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Removed
88593	20	44	20	45	Gao et al., 2017. In the Reference (page 134, Line 12) the year is 2018, not 2017. [Rosemary Vieira, Brazil]	Not applicable. Removed
88595	20	45	20	45	Kolod et al., 2019 - Reference not found. In the Page 145, Line 41 is writen Kolodziejczyk. [Rosemary Vieira, Brazil]	Accepted. Reference corrected
7159	20	45	20	45	Kolodziecjzyk et al., 2019 (not Kolod et al.) [Nicolas Kolodziejczyk, France]	Accepted. Sorry !
16333	20	45	20	46	"...assessment in sparseLY OBSERVED areaS"? [Julian Mak, China]	Not applicable. Text Removed
101793	20	45	20	47	"More generally, global ocean temperature change reflects water mass formation processes and ocean circulation, which provides confidence in assessment in sparse observation area" - The last sentence of the paragraph appears out of place to me. [IAPSO ECS group review, United States of America]	Not applicable. Text removed
98683	20	46	20	47	Change "in sparse observation area" to "in areas of sparse observations" [Sonya Legg, United States of America]	Not applicable. Text Removed
27595	20	47	20	47	The figure does not seems explicitly related to the Desbruyeres reference? Is it? [Eric Brun, France]	Accepted. Reference to the figure revised
79921	20	49	20	51	Where in Fig. 9.6 can be seen the pronounced warming in the Atlantic sector in observations? [Somavilla Raquel, Spain]	Accepted. Reference to the figure revised
101791	20	49	21	2	The accuracy of the statements made here cannot be reviewed due to missing figures in Figure 9.6. [IAPSO ECS group review, United States of America]	Accepted. Reference to the figure revised
107447	20	49	21	13	In lines 49-50, says the pattern in the 0-700 m layer is characterized by widespread warming across all ocean basins. But then in lines 11-13, says there is recent cooling in several regions, presumably in the 0-2000 m layer. So there is cooling below 700 m, but warming above? Or is it because of different time periods, but we don't know what 'recent' is. Unclear as written [Jennifer Walker, United States of America]	Accepted. Text is now clarified in that regard
34443	20	51	21	2	This is not visible on Fig. 9.3 and 9.6. It would be worth referring to a figure zooming on the Southern Ocean (one from the SROCC maybe?). [Claire Waelbroeck, France]	Accepted. Figures are now updated and of greater quality
101795	20	52	21	2	This pattern of OHC in the Southern Ocean high latitude is described in detail with an interpretation of the underlying mechanisms by Haumann et al. (2020), which should be included in this discussion. The other references provided here only discuss the OHC changes in the ACC and north of it, but not those south of it referred to here. [IAPSO ECS group review, United States of America]	Accepted. Reference now considered in our assessment
107443	20	54	20	54	The acronym ACC has not been used before this point and should be written out [Jennifer Walker, United States of America]	Accepted. Acronym spelt out at first use
82891	20	54	20	54	To me it is not fully clear whether "high latitude" is specified here to address the fact that the Southern Ocean is at high latitudes, or to refer to the southernmost part of the Southern Ocean. I suggest to clarify/reword. [Sebastian Gerland, Norway]	Taken into account. We now discuss the high latitude in opposition to northern Southern Ocean region.
61279	20	54	21	2	Circular formulation. Consider condensing. Suggestion: 'This high latitudinal surface cooling contrasts with heat uptake and subtacted warming in the high-latitude Southern Ocean over the ACC and directly north of it in the 0-700m layer and deeper (Gille, 2008; Frölicher et al., 2015; Swart 1 et al., 2018) (medium confidence).' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reworded
33421	20	54			Describe abbreviation when first cited: ACC. [Guiomar Rotllant, Spain]	Accepted. Acronym spelt out at first use
61281	21	1	21	1	Consider deleting 'in the 0-700m layer and deeper' or replacing it by 'in the entire water column' or by 'primarily within the first 700m'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reworded

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16335	21	1	21	1	“warming” is not the thing being “subducted” as implied by current usage here. Not convinced “subducted” is absolutely required (which would mean the use of “cooling” is balanced precisely by the use of “warming” as might be expected). [Julian Mak, China]	Accepted. We now mention heat being subducted.
132567	21	1	21	1	You may consider citing Armour et al. 2016 (doi: 10.1038/NGEO2731) here as well, which explains this mechanism. [Kyle Armour, United States of America]	Accepted. Reference cited
65881	21	1	223	1	Suggest correction of the text citation that forms the structural part of sentence to e.g. "Author Group (2018) and Author (2019) state that". If the sentence is not structural, it can be corrected to e.g. "sea-level rise will continue past 2050 (Author and Writer, 2018) and past 2100 (Author 2019)". Suggest consistency through the entire report. [Kushla Munro, Australia]	Noted.
97997	21	4	21	5	The recent Argo insight from Roemmich et al 2019 doi: 10.3389/fmars.2019.00439 maybe a useful updated citation [Paul Durack, United States of America]	Not applicable. This part has been removed.
101799	21	4	21	13	There is increasing evidence that the Indian Ocean has warmed more than the other ocean basins since 2005 (Li et al., 2018), possibly due to changes to the Indonesian Throughflow (Zhang et al., 2018; Lee et al., 2015). This may be worth mentioning. (Full references: Li, Y., Han, W., Hu, A., Meehl, G. A., Wang, F., Li, Y., … Wang, F. (2018). Multidecadal Changes of the Upper Indian Ocean Heat Content during 1965-2016. Journal of Climate, JCLI-D-18-0116.1. https://doi.org/10.1175/JCLI-D-18-0116.1; Zhang, Y., Feng, M., Du, Y., Phillips, H. E., Bindoff, N. L., & Mcphaden, M. J. (2018). Strengthened Indonesian Through flow Drives Decadal Warming in the Southern Indian Ocean. Geophysical Research Letters, 45, 1–9. https://doi.org/10.1029/2018GL078265; Lee, S.-K., Park, W., Baringer, M. O., Gordon, A. L., Huber, B., & Liu, Y. (2015). Pacific origin of the abrupt increase in Indian Ocean heat content during the warming hiatus. Nature Geoscience, 8(May), 445–449. https://doi.org/10.1038/ngeo2438) [IAPSO ECS group review, United States of America]	Accepted. Reference now considered in our assessment
44967	21	4	21	13	Studies have found that the southern subtropical ocean warming is mainly due to the wind stress change (ie., Llovel and Terray 2016 ; Volkov et al 2017). Adding some discussion on the causes of such decadal ocean warming change especially located at the center of the subtropical gyres could improve significantly this specific section. [WILLIAM LLOVEL, France]	Not applicable. SROCC assessed those processes, so we now refer to SROCC assessment
79923	21	7	21	9	According to Fig. 9.6, it is difficult to see it. Conversely, from Fig. 6 , an homogenous distribution of OHC between 50ºS and 50ºN approximately is observed. [Somavilla Raquel, Spain]	Noted. The SOD figure was a placeholder. It is now updated
27597	21	9	21	9	About '[…] (Figure 9.6, high confidence). ': The mention "high confidence" does not seem clear here. Does it refer to the whole statement or just the figure 9.6? If it refers to the whole statement, is it coherent to talk about "high confidence" while taking into account natural climate variability? [Eric Brun, France]	Accepted. We now clarified. Change can be of high confidence even in case of natural climate variability. We only assess change here, not underlying cause
2467	21	10	21	10	The period of global Argo coverage is, however, relatively short... I'd suggest you be specific and say something like "2006 to the present time". I think Dean Roemmich would likely claim global coverage in 2006. [Howard J. Freeland, Canada]	Accepted. The period is now clearly stated
86819	21	10	21	11	How long does a time series of data need to be in order to start discussing long-term climate changes? You say that the Argo coverage is relatively short - it would be interesting with a sentence discussing the time-scales necessary in order to distinquish climate changes from natural fluctuations. [Oyvind Christophersen, Norway]	Accepted. We now discuss how time-scale impact change due to heat redistribution
110541	21	10	21	11	The statement on background natural climate variability and trend detection is incomplete. It should be expanded with examples, as it is unclear which variability sources the authors are referring, and how substantial their impact on trends could be expected to be. [Keven Roy, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now expanded on internal variability
23503	21	10	21	13	Recent study shows that the global ocean warming over the period of 2005-2015 for the depth of 0-2000 m is consistent with the multi-model mean of the CMIP5 models simulation. Moreover the hemispheric asymmetry which is observed during 2005-2015 in the global ocean warming is due to the asymmetric internal climate variation that transfers the heat from the northern hemisphere to the southern hemisphere. Due to this there is an excess heat gain in the southern hemisphere and no or little heat gain the northern hemisphere. "Recent hemispheric asymmetry in global ocean warming induced by climate change and internal variability" "https://doi.org/10.1038/s41467-020-15754-3" [Saurabh Rathore, Australia]	Agreed. Reference now assessed

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129409	21	10			This section could benefit from mention of the much higher S/N ratio of OHC/sea level versus other fields such as surface temperature. See Cheng et al. (2017, EOS). [Trigg Talley, United States of America]	Noted. We understand the comment refer to hemispheric asymmetry. We now discuss that aspect.
107445	21	11	21	11	What time period is 'recent' cooling? [Jennifer Walker, United States of America]	Accepted. The period is now clearly stated
61391	21	11	21	13	A reference to support this statement would be welcome. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text Removed
27599	21	22	21	22	About "(e)": It's not a question of the graph (e) but of the graph (c). [Eric Brun, France]	Accepted - figure revised
27601	21	25	21	25	About "(d)": It's not a question of the graph (d) but of the graph (g). [Eric Brun, France]	Accepted - figure revised
27603	21	25	21	25	About "(g)":It's not a question of the graph (g) but of the graph (d). [Eric Brun, France]	Accepted - figure revised
27605	21	26	21	26	Why take the reference period 2091-2100, when for the previous charts the reference period is 2081-2100? [Eric Brun, France]	Accepted - figure revised
69083	21	31	21	33	Change " Direct observations of ocean temperature change below 2000 m are limited to ship-based, high-quality deep ocean temperature measurements, with improved sampling from 1990 to the present due to the World Ocean Circulation Experiment (WOCE) and GO-SHIP program." to "Direct observations of ocean temperature change below 2000 m are limited to ship-based, high-quality deep ocean temperature measurements, with improved sampling during the 1990's by World Ocean 33 Circulation Experiment (WOCE) and continued to the present by GO-SHIP (Talley, et al., 2016, Sloyan et al., 2019)." Reference (1) LD Talley, RA Feely, BM Sloyan, R Wanninkhof, MO Baringer, JL Bullister, CA Carlson, SC Doney, RA Fine, E Firing, N Gruber, DA Hansell, M Ishii, GC Johnson, K Katsumata, RM Key, M Kramp, C Langdon, AM Macdonald, JT Mathis, EL McDonagh, S Mecking, FJ Millero, CW Mordy, T Nakano, CL Sabine, WM Smethie, JH Swift, T Tanhua, AM Thurnherr, MJ Warner, J-Z Zhang, 2016. Changes in Ocean Heat, Carbon Content, and Ventilation: Review of the First Decade of Global Repeat Hydrography (GO-SHIP). Annual Review of Marine Science, 8, 19.1-19.31, 10.1146/annurev-marine-052915-100829 (2) Sloyan BM, Wanninkhof R, Kramp M, Johnson GC, Talley LD, Tanhua T, McDonagh E, Cusack C, O'Rourke E, McGovern E, Katsumata K, Diggs S, Hummon J, Ishii M, Azetsu-Scott K, Boss E, Ansorge I, Perez FF, Mercier H, Williams MJM, Anderson L, Lee JH, Murata A, Kouketsu S, Jeansson E, Hoppema M and Campos E (2019) The Global Ocean Ship-Based Hydrographic Investigations Program (GO-SHIP): A Platform for Integrated Multidisciplinary Ocean Science. Front. Mar. Sci. 6:445. doi: 10.3389/fmars.2019.00445 [Bernadette Sloyan, Australia]	Accepted. Text reworded
2469	21	31	21	46	A good discussion. I'm sorry if I sound like I am asking for a cite, but the first observation of warming in the abyssal oceans was made in a paper of which I am a co-author:- Fukasawa, M., Freeland, H., Perkin, R. et al. Bottom water warming in the North Pacific Ocean. Nature 427, 825–827 (2004). https://doi.org/10.1038/nature02337 This was a revisit in 1999 of the WOCE Hydrographic Program Line P01, an effort now part of what is called GO-SHIP. Of course, following this paper Purkey and Johnson published a large number of papers showing that what we saw was in fact globally typical. Great work by them, but Fukasawa et al did get there first. [Howard J. Freeland, Canada]	Rejected. We refrain from doing a review. We start from previous report assessment and add additional evidences
34939	21	31	21	50	The SOD admits that ocean temperature data below 2000m is very sparse. The SOD implies that the earth system warming is a matter of concern. See general comment #5 above. [Jim O'Brien, Ireland]	Noted. We are unsure what is the concern or question of the reviewer.
41885	21	31	21	50	Here may be a good place to include the published work Garry et al. 2019 which estimates the uncertainties in deep ocean heat content observation below 2000 m, finding that trends over the period 1990-2010 (as per Purkey and Johnson) may be biased due to uncertainties in temporal or spatial sampling. Regionally these biases can be up to 0.1 W/m2 across an ocean basin, and in the model used, only 82% of the global warming trend deeper than 2,000 m was captured by hydrographic section-style sampling. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JC014225. The full reference is: Garry FK, McDonagh EL, Blaker AT, Roberts CD, Desbruyeres DG, Frajka-Williams E, King BA (2019) Model derived uncertainties in deep ocean temperature trends between 1990-2010. Journal of Geophysical Research: Oceans. 124, 1155–1169. https://doi.org/10.1029/2018JC014225. [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference now added and assessed
97999	21	33	21	33	The recent GO-SHIP insight from Sloyan et al 2019 doi: 10.3389/fmars.2019.00445 maybe a useful citation [Paul Durack, United States of America]	Accepted. Reference added to our assessment
10235	21	33	21	33	Just like WOCE being World Ocean Circulation Experiment, GO-SHIP is an acronym for Global Ocean Ship-based Hydrography Investigation Program (see https://www.go-ship.org). [Katsumata Katsuro, Japan]	Accepted. Acronym spelt out at first use

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3031	21	34	21	35	It seems relevant to add the reference to Garry et al. (2019, in J. Geophys. Res.-Oceans) here too because, using a model, they quantified the expected biases associated with hydrographic-style sampling of the ocean below 2000 meters depth, which they found to be largest in the Indian and Southern Oceans. [David Trossman, United States of America]	Accepted. We now discuss that aspect
61283	21	38	21	39	I do not understand what a spatially coarse ocean basin is. Consider deleting this sentence. The paragraph reads fine without it. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text deleted
61285	21	39	21	40	You state here that deep ocean temperatures are consistent with trends. This formulation seems inaccurate. I suggest the following reformulation: 'Trends in deep-ocean (>2000m) temperatures are consitent before and after 2000 […]' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable, text removed
77531	21	39			insert "trends" between "ocean temperature" and "before the 2000s" [Emer Griffin, Ireland]	Not applicable, text removed
16337	21	43	21	43	Units, ZJ is used elsewhere when taking about ocean heat content so for consistency probably want to use it here too. [Julian Mak, China]	Not applicable. Text removed
85243	21	43	21	43	I'd be very interested to know what is the (fractional) contribution of observed heat content trends below 2000m to the global mean surface energy budget in terms of Wm-2? Also how does this compare to equivalent estimates from models, given their known issues with spurious convection forming Antarctic bottom water that fills much of the ocean below 2000m, e.g. Heuze at el, 2013 & 2015 - https://agupubs.onlinelibrary.wiley.com/doi/10.1002/grl.50287? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Though we do not assess fractional warming by depth range, which is assessed in Chap 3, we report the inconsistencies and discuss causes
101801	21	43	21	44	"The abyssal layer (4000-6000 m) shows the largest rate of warming of the deep layer, dominated by trends in the Southern Ocean consistent with.." - "abyssal" is used for the first time here and it may be confusing for a non-oceanographer how it relates to "deep" (which is used in the same sentence); We suggest to introduce here or earlier "deep" and "abyssal" ocean; on p27 line 1 there is some sort of definition: "deep waters ventilate the mid depth and the abyss" but perhaps a better definition is needed ? [IAPSO ECS group review, United States of America]	Accepted, we now clarify depth range when first introducing deep and abyssal
77527	21	45	21	46	Inconsistency in the statement "abyssal layer overall contributes to one third of the total ocean heat uptake below 200m" and text on page 20 lines 36-38 suggesting a 10% distribution of OHC in the 2000m-6000m layer [Emer Griffin, Ireland]	Accepted. We checked for consistency
10695	21	47	21	49	This is a good example of the confusion caused by using ill-defined terms like "Medieval Warm Period" and "Little Ice Age". The "LIA" period stated is different to elsewhere in this report. No where in Gebbie and Huybers (2019) are the periods of "Medieval Warm Period" and "Little Ice Age" actually defined. This is a really vague statement. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
101803	21	47	21	50	The last sentence of the paragraph on regional trends is long/somewhat out of place to me; I suggest to split it into two sentences or try to shorten, e.g., "Regional trends such as in the XX have been suggested to result from the ocean memory [or recovery] of past climate events such as the Medieval Warm Period or Little Ice Age (low confidence)". [IAPSO ECS group review, United States of America]	Not applicable. Text removed
16339	21	48	21	48	Consider moving "by inverse modelling" to after "(low confidence)". [Julian Mak, China]	Not applicable. Text removed
61287	21	49	21	49	Here, I see the first time that 'CE' is appended to the year. A quick check indicated that it is not often used in Ch.9 and neither in the Executive summary. I would drop it. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
101805	21	52	21	54	"The regional pattern in ocean heat content can be decomposed into a component coming from extra heat entering the ocean as if it were a passive tracer ('excess' heat), and a component related to ocean transport of background temperature field as if it were unperturbed ('redistributed' heat)" - This decomposition is also presented and investigated in a number of other papers, e.g. Banks and Gregory 2006 (doi: 10.1029/2005GL025352), Xie and Vallis 2012 (doi: 10.1007/s00382-011-1063-8), Marshall et al 2015 (doi: 10.1007/s00382-014-2308-0), Garuba and Klinger 2016 (doi: 10.1175/JCLI-D-16-0138.1) Garuba and Klinger 2018 (doi: 10.1175/JCLI-D-17-0452.1), Todd et al 2020 (doi: 10.1002/essoar.10501557.1) [IAPSO ECS group review, United States of America]	Noted. Though we do not aim here at providing an exhaustive review of papers using this decomposition.
34447	21	52	21	55	2) Another, more precise, definition of "excess heat" and "redistributed heat" is given in Fig. 9.8 caption of the same terms: "total heat uptake can be decomposed into passive uptake of added heat ("excess heat"), and pure redistribution of unperturbed heat ("redistributed heat")". I suggest that the latter definition is given here, rather than in a figure caption. [Claire Waelbroeck, France]	Accepted. We now changed to the definition that was in the mentioned caption

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34445	21	53	21	54	1) "… and a component related to ocean transport of background temperature field as if it were unperturbed (‘redistributed’ heat)": "it" is ambiguous in this sentence. I'd recommend to replace by "ocean transport" or "the transport". [Claire Waelbroeck, France]	Accepted. Reworded
34449	21	54	21	54	3) Also, the term "background" should be defined here. It is used a lot in the next paragraphs and it is unclear which time period it refers to. For instance, p. 23, l.10-11 could be part of such a definition. [Claire Waelbroeck, France]	Accepted. Reworded
101807	21	55	21	56	"making an assumption of minimum water mass transformation" - this is unclear ("minimum change of wmt"?), please rephrase. [IAPSO ECS group review, United States of America]	Accepted. Reworded
23505	21	55	22	3	Please also mention this study "Recent hemispheric asymmetry in global ocean warming induced by climate change and internal variability" "https://doi.org/10.1038/s41467-020-15754-3" [Saurabh Rathore, Australia]	Accepted. Reference now considered in our assessment
107449	21	56	21	56	The acronym OHC has not been used before this point and should be written out [Jennifer Walker, United States of America]	Accepted. Acronym spelt out at first use
61387	21	56	21	56	Please, as OHC is for the first time use in this chapter, consider giving the signification of this Acronym [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Acronym spelt out at first use
81715	21	56			What is the "minum water mass transformation assumption"? [Laure Zanna, United States of America]	Not applicable. Text Removed
33423	21	56			Describe abbreviation when first cited: OHC. [Guiomar Rotllant, Spain]	Accepted. Acronym spelt out at first use
116833	21		21		Please reconsider the use of "transition" for variations of the last centuries; please link changes to understanding of drivers. The recurrence of volcanic eruptions has been stressed as a key driver of centennial variations in other chapters (esp ch 1, 2, 3). [Valerie Masson-Delmotte, France]	Accepted. Transition is not used anymore in this context
12489	22	1	22	4	It should be around ~100m, check fig.3 in Roemmich et al. 2011 GRL, and Fig.9 in Cheng et al. 2019, this is a new study investigating the ocean heat change associated with ENSO: the heat redistribution within the ocean and ocean-air heat exchanges are both significant during ENSO, many processes are involved. Reference: Cheng L., K. E. Trenberth, J. T. Fasullo, M. Mayer, M. Balmaseda, J. Zhu, 2019: Evolution of ocean heat content related to ENSO. Journal of Climate, 32, 3529–3556, https://doi.org/10.1175/JCLI-D-18-0607.1. [Lijing Cheng, China]	Not applicable. Text Removed
81717	22	1		13	This paragraph might not be a good reflection of our current understanding and knowledge: 1) Changes in gyre circulations also contribute a great deal to changes in heat content on interannual to decadal timescales probably more than the AMOC (See papers by Lozier, Ric Williams, or Piecuch). 2) The focus on 2006-2016 seems mainly to be around the Zika reference, which is fine, but I would suggest including other studies such as Roemmich et al which discuss the South Pacific OHC changes and the role of the wind-driven gyre; I would also suggest including a discussion of excess and redistribution in the context of longer trends (Bronsealer & Zanna, 2020) which is more relevant for detecting the anthropogenic signature; 3) Finally, while Zanna et al 2019 showed that most of the heat uptake can be described as passive, they also showed that this is not true in the tropical and North Atlantic and they attributed about 1/2 of heat content pattern (and thermosteric sea level rise) to redistrbution (e.g., gyre, AMOC changes) between 1970 to present. [Laure Zanna, United States of America]	Accepted. The text has now be clarified to reflect better this literature.
101809	22	2	22	2	It is unclear what is meant by "mode of variability". Are the authors generally referring to "natural variablity" here? If so, it might be good to specify this also in terms of the decadal variations. [IAPSO ECS group review, United States of America]	Accepted. Clarified. We now refer to decadal variability and internal ocean circulation variability
132569	22	2	22	3	Should be "models of variability and/or decadal changes in ocean currents". [Kyle Armour, United States of America]	Accepted. Clarified. We now refer to decadal variability and internal ocean circulation variability
96923	22	3	22	4	The sentence remains unclear, please modify. [Nicole Wilke, Germany]	Not applicable. Text Removed
129411	22	4	22	8	Here the recent cooling of the subpolar North Atlantic since 2006 is interpreted largely in terms of changes in the Atlantic meridional overturning circulation. This is potentially only one part of the story, and ignores an entire literature on the possible influence of changes in wind forcing and the horizontal gyre circulation in driving subpolar cooling during this period since 2006. See, for example, Piecuch et al. (2017) and references in their introduction: Piecuch, C. G., R. M. Ponte, C. M. Little, M. W. Buckley, and I. Fukumori (2017), Mechanisms underlying recent decadal changes in subpolar North Atlantic Ocean heat content, J. Geophys. Res. Oceans, 122, 7181-7197, doi:10.1002/2017JC012845. [Trigg Talley, United States of America]	Accepted. We now discuss gyre circulation variability

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23507	22	4	22	8	Please also consider to mention "Recent hemispheric asymmetry in global ocean warming induced by climate change and internal variability" "https://doi.org/10.1038/s41467-020-15754-3" [Saurabh Rathore, Australia]	Accepted. Reference assessed
61213	22	7	22	7	"and regional anomalously cold atmospheric conditions (excess)". What is the meaning of the word excess in parentheses here? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text Removed
101811	22	7	22	7	We suggest to delete "(excess)", or else rephrase to clarify, e.g., to "and a contribution of excess heat due to regional anomalously cold atmospheric conditions", if I understand correctly. [IAPSO ECS group review, United States of America]	Not applicable. Text Removed
83545	22	8	22	8	You could cite here in addition: Zunino, P., Lherminier, P., Mercier, H., Daniault, N., García-Ibáñez, M.I., Pérez, F.F., 2017. The GEOVIDE cruise in May–June 2014 reveals an intense Meridional Overturning Circulation over a cold and fresh subpolar North Atlantic. Biogeosciences 14, 5323-5342, doi: 10.5194/bg-14-5323-2017. [Antje H. L. Voelker, Portugal]	Not applicable. Text Removed
7839	22	8	22	10	I don't understand this sentence. I suggest rewriting [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The entire paragraph has been reworded
132571	22	10	22	13	I'm not sure we can say that the changes over recent decades is dominated by redistribution since the Zika et al. study only looked at a short time period. Also, this explanation is a bit too technical. I think it would be better to say "In contrast to the observed changes on short timescales, which is dominated by redistribution, on multidecadal and longer timescales ocean warming can be reconstructed by anomalous surface heat uptake being carried into the interior by time-invariant circulation processes, with the notable exception of the North Atlantic where AMOC changes are projected to dominate (Zanna et al. 2019, Marshall et al. 2015). [Kyle Armour, United States of America]	Not applicable. Text Removed
35065	22	10	22	13	Another poorly constructed and ambiguous sentence. Suggest replace with "Observed changes during recent decades are dominated by redistribution of heat. By contrast, full-depth warming over centennial timescales can be essentially reconstructedas sea surface temperature anomalies carried into the ocean interior by time-invariant circulation processes." [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text Removed
10237	22	10	22	13	Is this sentence missing a word or two? ("assuming" between "by" and 'sea surface temperature"?) [Katsumata Katsuro, Japan]	Not applicable. Text Removed
95931	22	12	22	12	"sea surafce temeperature anomalies that are…" [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text Removed
20173	22	12	22	12	Last word of the line: replace "are" by "being" [philippe waldteufel, France]	Not applicable. Text Removed
61215	22	12	22	13	"by sea surface anomalies are carried" . Did you mean sea surface anomalies THAT are carried? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text Removed
34451	22	12	22	13	Is the word "which" lacking between "sea surface temperature anomalies" and "are carried into the ocean interior"? [Claire Waelbroeck, France]	Not applicable. Text Removed
745	22	12	22	13	“full depth warming can be essentially reconstructed by sea surface temperature anomalies are carried into the ocean interior by time-invariant circulation processes” Doesn’t make sense, should this be “anomalies that are carried”? Restructure for clarity [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text Removed
79925	22	18	22	21	Would not be useful to have comparison for observed and modelled trends and not only of mean values? [Somavilla Raquel, Spain]	Rejected. We agree it would be but we cannot for size and clarity of the figure
2471	22	19	22	19	Please, not "ARGO" but "Argo" it is a name not an acronym so should never be written in all capitals. Please search all the text, it happens more than once, and change "ARGO" to "Argo". https://en.wikipedia.org/wiki/Argo_(oceanography) [Howard J. Freeland, Canada]	Accepted - Argo is now written in lower case
61299	22	20	22	20	Figure captionL To which CMIP ensemble do you refer here. In the accompanying text (p23, line 30) you specify CMIP5 and refer to this figures. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Figure title clarified (this is CMIP6)
16341	22	26	22	26	Don’t really need the “an” [Julian Mak, China]	Not applicable, text removed
747	22	26	22	26	“the ocean observing system and the climate modelling”, use of two “the” sounds clunky, especially before climate modelling [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "the" removed
85245	22	26	22	26	What does 'concomitant' mean, is there a simpler word that could be used? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Concomitant is an English word

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2473	22	26	22	41	The writing style seems to have taken a nose-dive here. [Howard J. Freeland, Canada]	Noted.
23509	22	26	22	41	The recent study shows that on a shorter ocean record 2005-2015 for the depth range of 0-2000 m the internal climate variation can redistributes the ocean heat gain to generate the hemispheric asymmetry in global ocean warming. This study also suggest that the role of the natural variability such as "aerosols" is of second order in the OHC redistribution. "Recent hemispheric asymmetry in global ocean warming induced by climate change and internal variability" "https://doi.org/10.1038/s41467-020-15754-3" and also please refer to this study "Anthropogenic Aerosols, Greenhouse Gases, and the Uptake, Transport, and Storage of Excess Heat in the Climate System" "https://doi.org/10.1029/ 2019GL082015" This study shows that in the northern hemisphere during 1861-2005, Anthropogenic Aerosols canceling the effects of Green House Gas. However, in future the projected decline in anthropogenic aerosol would lead to a much more symmetrical uptake of excess heat between the hemispheres and a consequent reduction in the northward transport of excess heat. So, in my opinion the heat redistribution from the internal variability should be a subject for meticulous investigation for near term prediction for sea level rise due to ocean warming. [Saurabh Rathore, Australia]	Accepted. Both studies are now assessed
85247	22	26	22	41	I'd be interested to know what is the (fractional) contribution of observed heat content trends below 2000m to the global mean surface energy budget in terms of Wm-2? Also how does this compare to equivalent estimates from models, given their known issues with spurious convection forming Antacrtic Bottom water that fills much of the ocean below 200m, e.g. Heuze at el, 2013 & 2015 - https://agupubs.onlinelibrary.wiley.com/doi/10.1002/grl.50287? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Because of length constraint we cannot describe this aspect
98685	22	28	22	28	Change "volcanoes eruptions" to "volcanic eruptions" [Sonya Legg, United States of America]	Accepted. We now use volcanic eruptions
101813	22	28	22	28	We suggest to start a new sentence before "despite" (as there are many points in this one sentence). [IAPSO ECS group review, United States of America]	Accepted. Sentence split
77791	22	28	22	28	Volcanoes → volcanic [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now use volcanic eruptions
61289	22	29	22	29	Sect. 3.5.1.2 points to 'Tropical Sea Surface Temperature Evaluation'. From my understanding your should rather point to Sec.t 3.5.1.3, which sheds light on 'Ocean Heat Content Change Attribution'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected
89689	22	29	22	31	New work on the resolution dependence of vertical heat transport should be added here, e.g., Chassignet et al. 2020. Potentially, a comparison between highresmip and regular CMIP models could be formulated within chapter, but that is unlikely to be conclusive. [Baylor Fox-Kemper, United States of America]	Accepted. Reference assessed
112479	22	29	22	31	I would suggest to change to " ... suggest that models have too large diffusivity" instead of "... suggest models are too diffuse". [Pedro LLanillo del Rio, Germany]	Accepted. Reworded
34453	22	29	22	31	Fig. 9.7 only shows data from 2005-2014 (ARGO), it is thus not relevant and should not be referred to in this sentence. [Claire Waelbroeck, France]	Accepted. We removed the citation
61291	22	31	22	31	I did not find any comment on an overestimated diffusivity in Sect. 3.1.5.2 nor in Sect. 3.5.1.3. Please remove section reference. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We remove the reference

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41887	22	31	22	31	Here may be a good place to include the published work Garry et al. 2019 which estimates the uncertainties in deep ocean heat content observation below 2000 m, finding that trends over the period 1990-2010 (as per Purkey and Johnson) may be biased due to uncertainties in temporal or spatial sampling. Regionally these biases can be up to 0.1 W/m2 across an ocean basin, and in the model used, only 82% of the global warming trend deeper than 2,000 m was captured by hydrographic section-style sampling. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JC014225. The full reference is: Garry FK, McDonagh EL, Blaker AT, Roberts CD, Desbruyeres DG, Frajka-Williams E, King BA (2019) Model derived uncertainties in deep ocean temperature trends between 1990-2010. Journal of Geophysical Research: Oceans. 124, 1155–1169. https://doi.org/10.1029/2018JC014225. The submitted work Garry et al. submitted is still under review. [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added to our assessment
41889	22	32	22	32	Specifically... 'In addition, historical model runs simulate different phasing of natural variability than observations (Garry et al., submitted).' The citation here points to an old submission. The paper is currently under review and the citation should be: Garry, F. K., Roberts, C. D., Blaker, A. T., McDonagh, E. L., Frajka-Williams, E., and King, B. A. Increasing importance of deep ocean heat storage in 21st century climate projections. Scientific Reports (submitted). [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Noted, tough the paper did not make the cut-off deadline for accepted papers. So we cannot cite
1725	22	32	22	35	Relevant references should be cited at the end of line 34. [Michael Kennish, United States of America]	Rejected. Assessment and associated references are in the Section pointed. We do not redo the assessment
132599	22	33	22	35	This seems inconsistent with the statement in Chapter 4 (page 64, lines 52-53) that it is only very likely that anthropogenic forcing has made a substantial contribution to the ocean heat content increase. Chapter 3, page 47 says that it is extremely likely, but this too seems too weak. I suspect your statment of virtually certain is correct, but you should check for consistency with Chapters 3 and 4. [Kyle Armour, United States of America]	Noted. We checked for consistency across the report
61293	22	35	22	35	Sect. 3.5.1.2 points to 'Tropical Sea Surface Temperature Evaluation'. From my understanding your should rather point to Sec.t 3.5.1.3, which sheds light on 'Ocean Heat Content Change Attribution'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We changed the reference to the correct section
82893	22	35	22	37	I suggest to list actual reference(s) supporting the attribution statement. [Sebastian Gerland, Norway]	Rejected. Assessment and associated references are in the Section pointed. We do not redo the assessment
16343	22	37	22	37	Don't need the comma after "While" [Julian Mak, China]	Noted. Editorial.
101815	22	37	22	39	"While, explosive volcanic eruption disturbs the earth radiative budget for only a few years, the ocean integrates the radiative cooling in its subsurface, typically in the upper 500 m (Bilbao et al., 2015; Gupta and Marshall, 2018)," - the authors could also cite Tokarska et al 2019 (their section 3.3, doi: 10.1088/1748-9326/ab23c1) [IAPSO ECS group review, United States of America]	Rejected. Not relevant for ocean heat content discussion
78057	22	38	22	38	I think Bilbao et al. (2019) is more relevant here than the 2015 paper. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We note cite Bilbao et al. 2019
61337	22	43	22	43	Please reconcile the 'very high confidence' statement here with the probability assessment in the Executive summary (page 5, line 18) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We checked for consistency across the report
85249	22	45	22	45	Define inertia? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Removed
61295	22	47	22	47	Is the warming by 900±345 ZJ for another scenario? Please amend. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Scenario associated to projected OHC change has been clarified
61393	22	47	22	47	Please, as ZJ is for the first time use in this chapter, consider giving the signification of this Acronym [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. ZJ is explained when it first appears
27607	22	47	22	47	Why is the temperature rise not expressed in degrees? This could be detrimental to the understanding. [Eric Brun, France]	Noted. Ocean heat content is expressed as energy unit, in Joules. For easing understanding in response to this comment, we also expressed in multiplication factor change compared the historical period
72001	22	47			I do not undersand this senternce - something seems to be missing. [John Church, Australia]	Accepted. Reworded
77533	22	47			Clarify which scenario the "900+-345 ZJ" projection refers to [Emer Griffin, Ireland]	Accepted. Corrected
54503	22	49	22	59	Here we revise those estimates from CMIP6 projection estimating that heat content in the 0–2000 m layer will increase by [PLACEHOLDER COMPUTED FROM ONLY ONE MODEL; ADDITIONAL CMIP6 50 MODELS WILL BE USED TO UPDATE AND PROVIDE CONFIDENCE INTERVAL] [Maria del Pilar Bueno Rubial, Argentina]	Noted. We guess the comment request for replacing placeholder, which is now done.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89249	22	55	22	56	It might worth noting that the projected ocean warming patterns, at least in the Southern Ocean, are consistent with the observed patterns. For example, see figure 1 of this recent paper https://doi.org/10.1175/JCLI-D-19-0478.1 comparing CMIP5 multi-model simulations with observations. Both also share very similar mechanisms as far as I read from this report. Thus we should have a good confidence with Southern Ocean waming and its patterns/mechanisms/future projections. [Kewei Lyu, Australia]	Accepted. Reference added to our assessment
101817	22	55	23	3	Maybe another study to consider: Garuba and Klinger (2018) (doi: 10.1175/JCLI-D-17-0452.1) also show that most of the uptake of heat is done passively (although they don't have a focus on the Southern Ocean) [IAPSO ECS group review, United States of America]	Accepted. Reference added to our assessment
81721	22	55	23	11	You might again want to add more recent papers (Bronselaer & Zanna, 2020) which discuss redistribution vs. excess heat in all basins in CMIP models. [Laure Zanna, United States of America]	Accepted. Reference now considered in our assessment
116835	22		22		The role of volcanic eruptions on heat content can be relevant for the CCB on the pre industrial reference (it is focused on temperature, but ocean heat content matters, so the recurrence of eruptions prior to the defined reference period too). [Valerie Masson-Delmotte, France]	Accepted. We how now established links with the Cross-Chapter Box
88597	23	3	23	3	Sallée et al., 2013. Which is the correct one: Sallée et al., 2013a or Sallée et al., 2013b? [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88599	23	3	23	3	Garry et al., 2018. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88601	23	4	23	4	Couldrey et al., 2020. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
101819	23	4	23	4	It appears that what Fig 9.8, i.e., the models, show is inconsistent with the statement in the earlier paragraph of this section on the observed OHC, p. 21 line 52ff. In the paragraph on the observations, the conclusion was that the redistributed heat dominates the spatial pattern (at least on these time scales); Fig 9.8 shows a dominance of excess heat (panel b) rather than a dominance of redistributive heat (panel c); to me it appears the discussion of excess vs redistributive heat is still an open question (some papers argue temperature changes are mostly passive, some argue that the redistributed part is substantial, e.g., Winton et al (2013), https://doi.org/10.1175/JCLI-D-12-00296.1, Morrison et al (2016), https://doi.org/10.1175/JCLI-D-15-0579.1, Chen et al (2019) https://doi.org/10.1029/2018GL080961); please clarify. It is important to be transparent about what the models can (not) do; it may be also a matter of time scales. [IAPSO ECS group review, United States of America]	Accepted. We now clarify better the time-scale (we assess that it is a matter of time-scale)
89251	23	4	23	5	Note that the varying magnitude of westerly wind changes across models is a main cause for the uncertainties in the projected Southern Ocean warming magnitude (see figure 9 of this recent paper https://doi.org/10.1175/JCLI-D-19-0478.1) [Kewei Lyu, Australia]	Accepted. Reference added to our assessment
101821	23	4	23	5	"In addition, strengthening of the Southern Ocean westerlies (Section 9.2.1.3) causes a northward and downward redistribution of the background heat content" - Based on the recent findings by Bracegirdle et al. (2020) this statement on the strengthening westerlies needs refinement. The low emission scenario actually exhibits a weakening of the westerlies over the 21st century due to the recovery of stratospheric ozone. Bracegirdle, T. J., Krinner, G., Tonelli, M., et al . Twenty-first century changes in Antarctic and Southern Ocean surface climate in CMIP6. Atmos Sci Lett. 2020; e984. https://doi.org/10.1002/asl.984 [IAPSO ECS group review, United States of America]	Accepted. Point taken on board
61297	23	6	23	6	Please also refer to Sect. 4.3.2.3. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. we refer to the Chapter's AMOC section
3033	23	7	23	8	Add a reference to Garuba and Klinger (2016, in J. Clim.) and maybe Trossman et al. (2016, in Geophys. Res. Lett.) as well [David Trossman, United States of America]	Accepted. Garuba and Klinker is now assessed
101823	23	10	23	11	"Redistribution of background heat content makes zero contribution to global ocean heat uptake, which therefore equals the global integral of excess heat." We suggest to move this to where the reports starts to discuss redistributed/excess heat in paragraph p21 l52ff. [IAPSO ECS group review, United States of America]	Accepted. Statement moved to earlier paragraph where we mention redistribution
23511	23	10	23	11	Recent hemispheric asymmetry in global ocean warming induced by climate change and internal variability "https://doi.org/10.1038/s41467-020-15754-3" [Saurabh Rathore, Australia]	Noted. Reference added to our assessment
81719	23	10		11	This is a true statement (about redistribution integrating to 0) but it probably does not belong to this paragraph; it might be more appropriate in the paragraph above in which you talk about redistribution for the first time. [Laure Zanna, United States of America]	Accepted. Statement moved to earlier paragraph where we mention redistribution
81723	23	10			Todd et al 2020 also shows the same [Laure Zanna, United States of America]	Accepted. Reference added to our assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61323	23	16	23	22	Fig. 9.8. Caption: For the top right panel (d) it remains unclear what these 'gyre' values refer to. What is their relevance? A few extra words in the caption might help. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Caption revised
88603	23	20	23	20	Which is the correct one: Couldrey et al. submitted or Couldrey et al., 2020?. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
85251	23	27	23	27	I'd be in terested to know what is the (fractional) contribution of observed heat content trends below 2000m to the global mean surface energy budget in terms of Wm-2? Also how does this compare to equivalent estimates from models, given their known issues with spurious convection forming Antacrtic Bottom water that fills much of the ocean below 200m, e.g. Heuze at el, 2013 & 2015 - https://agupubs.onlinelibrary.wiley.com/doi/10.1002/grl.50287? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Because of length constraint we cannot describe this aspect
79927	23	27	23	28	The message of this sentence is not clear. Another two things about this sentence: 1. It is not clear whether it is talking about ocean observations or modelling. This is a common problem in all this page. It would be beneficial to make small corrections in such a way that this point is clear as in other sections in the report ; 2. Observations do not support this conclusion. During the late-2000s and early-2010s there have been an effective heat injection from upper to deeper layers in the subpolar gyre (Sullivan, 2016; Yashayaev and Loder (2016), ...) This comment do not pretend that this information or reference will be included, just to highlight the importance of indicating whether the sentences refers to observations or modelling because they can differ significantly. References: Sullivan (2016) . The North Atlantic Ocean's Missing Heat Is Found in Its Depths. Sullivan, C. (2016), Eos, 97, doi: 10.1029/2016EO047009. Yashayaev, I., and Loder, J. W. (2017), Further intensification of deep convection in the Labrador Sea in 2016, Geophys. Res. Lett., 44, 1429– 1438, doi:10.1002/2016GL071668. [Somavilla Raquel, Spain]	Accepted. We now clarified that we discuss future change represented by climate models
111429	23	27	23	28	Reverse the order to make clearer sense: "...excess heat signal downward, but with decreasing efficiency due to decreasing convective activity" [James Renwick, New Zealand]	Not applicable. Text changed
18027	23	27	23	28	This seems to be an incomplete sentence. Or at best it is awkward. [Lisa Levin, United States of America]	Accepted. Revised
7841	23	27	23	28	I don't understand this sentence. I suggest rewriting [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised
16345	23	27	23	28	Move “with decreasing efficiency” to after “...downard, but” (“downward, but WITH DECREASING EFFICIENCY due to decreasing convective activity (reference)” (avoids splitting a sentence and saves a comma). [Julian Mak, China]	Accepted. Revised
749	23	27	23	28	“, but due to decreasing convective activity” The comma should come after the but [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised
72003	23	27		29	I usually think of convectin transporting heat upwards? [John Church, Australia]	Accepted, we revised to efficiently affect deep ocean temperature characteristics.
54351	23	29	23	30	Note that a recent paper by Hu et al (2020) J. Climate lends some credence to the assumption that overturning has an impact on the deep warming signal [Luke Van Roekel, United States of America]	Noted
61217	23	30	23	31	which suggestS models are too diffuse". This isn't clear to me. Are the results of model ensemble simulations too diffuse, or are models too diffusive? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text changed
61301	23	33	23	33	The reference to Sect. 4.5.2.1 is inappropriate at this position. In Sect. 4.5.2.1, the authros make no refrence to the NADW or AABW, nor is there a regional breakdown of warming below 2000m. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Removed
34455	23	33	23	33	Referring to "Figure 9.8" is not relevant here: deep waters are not visible on Figure 9.8. [Claire Waelbroeck, France]	Accepted. Removed
96925	23	33	23	33	Reference to wrong figure. Should probably be 9.7. [Nicole Wilke, Germany]	Accepted. Removed
77793	23	33	23	33	Rugenstein et al 2016 10.1002/2016GL070907 is relevant. They show relatively small warming in equilibrium under various scenarios. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Reference now considered
46613	23	36	23	36	Reference to the role of polynyas on abyssal warming should be added, e.g. Zanowski et al 82015), doi:10.1175/JPO-D-15-0109.1 [Céline Heuzé, Sweden]	Noted. Reference now considered
96927	23	39	23	46	It is not clear what the key aspects of ocean heat transports are rated as high confidence, and the text in line 40-46 are rated as low confidence in line 46. [Nicole Wilke, Germany]	Accepted. Text has been revised.
61303	23	41	23	41	Instead of referencing an entire chapter, specify Sect. 7.2.2.4, which discusses Fig. 7.5. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Sentence is removed..

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89253	23	41	23	47	Actually the warming patterns/mechanisms in the Southern Ocean are very robust in both observations and model simulation/projections in many literatures listed in this report. Are you giving a "low confidence" because we don't have a good understanding of eddy contribuiton and climate models don't resolve eddies? Note Morrison et al. 2016 analysed an eddy-resolving climate model projections and quantified edded contributions - I think their results are consistent with other studies. So I think we should have at least "medium confidence" with Southern Ocean changes, but manybe low confidence with magnitudes because of eddy compensation. [Kewei Lyu, Australia]	Not applicable. This part has been removed..
80659	23	44	23	44	in the previous paragraphs, the depth is expressed in m, not in km. It would be better to be consistent [Helene Jacot Des Combes, Marshall Islands]	Not applicable. This part has been removed.
34457	23	44	23	44	Either "as shown in Figure 9.7" or "sudden" should be erased from the sentence: nothing in Fig. 9.7 indicates that the simulated OHC increase is sudden. [Claire Waelbroeck, France]	Accepted. Sudden has been removed
132573	23	44	23	44	Perhaps drop the word "sudden" here since this makes it sound like you are referring to a rapid warming, while I think you mean that the warming is localized? [Kyle Armour, United States of America]	Accepted. Sudden has been removed
85253	23	46	23	46	Is it worth mentioning that eddies are parameterised in most CMIP models, which has implications for the fidelity of subtle residual balance between compensating wind and eddy driven overturning in the Southern Ocean, and its future changes, which impacts of on many aspects of Southern Ocean projections, which one would expect to include heat uptake, e.g. eddy compensation and eddy saturation to wind strength changes are known to depend considerably on ocean model resolution? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed..
132575	23	46	23	47	This sentence is confusing since the previous sentence refers to time-mean currents carrying anomalous heat to where it is subducted (rather than to changes in wind-driven circulations). Perhaps expand this to note that there is only medium confidence (I don't think it is low) because changes in winds can also affect the surface flow, but to an uncertain degree because this wind-driven change in circulation are partly counteracted by eddies. [Kyle Armour, United States of America]	Accepted. Text has been revised to clarify this point.
81725	23	46		47	Why the low confidence because of eddies is not clear. [Laure Zanna, United States of America]	Not applicable. This part has been removed.
79929	23	47	23	51	As mentioned in previous comment, the text will benefit from indicating whether we are talking about model and observations since observations do not show necessarily what is indicated here. [Somavilla Raquel, Spain]	Accepted. Text has been revised to clarify this point.
61305	23	49	23	49	The reference to Sect. 4.3.2 (Cryosphere, Ocean, and Biosphere) is rather general. As you speak of AMOC weakening refer to 4.3.2.3. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Sentence has been removed.
61219	23	54	23	54	"most models project AN increase in oceanic heat transport" and parentheses missing around "medium condifence". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Sentence has been removed.
77795	23	54	23	54	medium confidence in () [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence has been removed.
80603	23	56	23	56	Roberts et al (submitted) does not talk about changes in overturning in the Nordic Seas in quite this way. There is a shift to warmer (less dense) inflow but not necessarily a stronger overturning, more that the balance between Nordic Seas and subpolar gyre -driven overturning changes. Jackson et al. (submitted) goes into more detail for one model. [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised according to reviewer's remark.
41765	24	5	24	9	The references are strongly based on modelling results, although Muilwjk et al. (2018) make use of observations (see my later comment). There are interesting observational and theoretical studies looking into what may happen to vertical mixing and deep water formation in the Arctic Ocean in the future. For example: Rudels et al. (2016, doi:10.1002/2015JC011045; 2012, doi:10.5194/os-8-261-2012). [Benjamin Rabe, Germany]	Rejected. The topic is here not the Arctic Ocean but meridional heat transport.
41767	24	5	24	9	Muilwijk et al. (2018) use a questionable approach to heat transports, based on reference temperature and transport through a section with significant net volume transport (see also Schauer and Beszczynska-Möller, doi:10.5194/os-5-487-2009). Although Muilwijk et al. do reference the Schauer paper, they do not, in consequence, look for a more sophisticated approach, dealing with the reference termperature issue. [Benjamin Rabe, Germany]	Noted. We can only reference a paper that explicitly criticises Muilwijk. Even if the method of Muilwijk can be questioned, the conclusion that trends are still obscured by IV remains.
23513	24	8	24	9	Also consider this study "Recent hemispheric asymmetry in global ocean warming induced by climate change and internal variability" "https://doi.org/10.1038/s41467-020-15754-3" [Saurabh Rathore, Australia]	Rejected. While this paper could be relevant for a discussion on ocean heat uptake and changes on ocean heat content (discussed elsewhere) in 9.2.2, it is not relevant for a discussion on ocean heat transport changes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77797	24	11	24	11	Illustrates → illustrate [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
61221	24	11	24	16	Very very long sentence: it is possible to cut it for clarity? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Sentence revised
77799	24	11	24	34	I wonder if this paragraph could be shortened a bit, in order to exhibit the policy-relevant conclusions more clearly. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Paragraph shortened and link to other lines of evidences has been clarified
65883	24	12	5	12	Suggest changing to: 'continues', rather than 'continue'. [Kushla Munro, Australia]	Accepted. Text revised
19273	24	13	24	13	"abrupt cooling" : an indication of time scale would be useful here. [Anne-Marie Treguier, France]	Not applicable, text removed
61223	24	16	24	19	Not clear. Write "rather than in the Southern Ocean only, as previously assumed"? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable, text removed
98687	24	16	24	19	A reference is needed for this statement [Sonya Legg, United States of America]	Not applicable, text removed
16347	24	17	24	17	Units, consider converting these to Zjs [Julian Mak, China]	Rejected. We changed units to YJ.
98689	24	19	24	24	Are all these statements based on a single model study? Which model? How much can be generalized from this? [Sonya Legg, United States of America]	Not applicable, text removed
26371	24	20	24	20	(Pedro et al., 2018).Timescales -> space before Timescales [María Santolaria-Otín, France]	Accepted. Text revised
42911	24	22			It would be relevant here to also incude the LIG ocean heat content measurements from ice cores of Shackleton, S., et al. (2020), Global ocean heat content in the Last Interglacial, Nature Geoscience, 13(1), 77-81, doi:10.1038/s41561-019-0498-0. I also proposed this in chapter 2, where this is treated again. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added
16349	24	24	24	24	Units, consider converting these to Zjs [Julian Mak, China]	Rejected. We changed units to YJ.
96929	24	29	24	29	Please change GMST to GMSST. [Nicole Wilke, Germany]	Not applicable, text removed
101825	24	29	24	29	The methodology of Figure 9.9 is discussed in Clark et al (2016), but the methods are only mentioned in this report in the parenthetical "(ensemble of four intermediate complexity models)". Since this result is highlighted in a figure (Figure 9.9), more text about the models (particularly in relation to the CMIP ensemble) and the relationship between the four given future emissions values and the SSP scenarios would help with interpretation of this section and Figure 9.9 [IAPSO ECS group review, United States of America]	Rejected. For text length constraints we cannot expend on the methodology. But there is a perfect traceability to the methodology by referring to Clark et al. 2016 in the caption
129413	24	30	24	30	GMST is not defined before. [Trigg Talley, United States of America]	Not applicable, text removed
96931	24	30	24	30	Please check numbers. Fig 9.9 does not give future emission of 0 Gt, but 5120 Gt is missing in text. [Nicole Wilke, Germany]	Accepted. 5120 has been added in the text
16351	24	30	24	30	Units, consider converting these to Zjs [Julian Mak, China]	Rejected. We changed units to YJ.
80435	24	33	24	34	As a corrolary to this particular aspect, Gebbie & Huybers, 2019 (Science) showed that the deep Pacific temperature field was still ajdusting to the global cooling associated with the Little Ice Age (1300-1860), whereas temperature trends in the surface ocean and deep Atlantic refltect modern warming. This observation has implications for determining the net heat gain in the deep Indo-Pacific. [Samuel Jaccard, Switzerland]	Noted. Reference included
34459	24	37	24	46	The simulated warming in Clark et al., 2016 is larger than plotted here: 5120 Gt projection peaks around 7°C and stabilizes around 6°C. [Claire Waelbroeck, France]	Accepted - figure revised
83547	24	39	24	43	Throughout the AR6 chapters there is a mixed use of kyr/ka (and Ma/Myr) as I have pointed out for several of the other chapters. I think this is confusing for readers, especially those not familiar with these "units". I think a consensus should be reached for the complete AR6 with strong input from the Chapter 2 authors. Correctly, since they refer to fixed dates, it should be ka and not kyr here. [Antje H. L. Voelker, Portugal]	Rejected. Those are not the same. Myr will refer to a period of 1 million years while Ma will refer to a date i.e. 1 million year ago.
61321	24	39	24	44	Fig. 9.9: The inset title 'Long-term projection and historical changes in ocean temperature' is sdistracting and biased to temperatures. The figure also shows OHC. Consider removing the inset title. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	The title has been revised
61185	24	49	24	49	The title of the section 'Ocean Salt Content' is poorly chosen. In contrast to ocean heat content, there is no net change in the salt content of the oceans over the time scales of interest in this report; such changes occur only on geological time scales. [Patrick Cummins, Canada]	Accepted. Changed to Ocean salinity
34461	24	52	24	55	As such, the sentence is unclear. This sentence is probably uncomplete. Otherwise, the portion of the sentence "has considerably expanded salinity measurement towards global coverage for the 0 – 2000m ocean since 1999 (Argo program (Riser et al., 2016)), and process-understanding" should be rephrased and clarified. [Claire Waelbroeck, France]	Agreed. Reworded
12491	24	54	24	54	The start time of Argo should be consistent across AR6 chapters, some refers to 2005, 2007, here is 1999. [Lijing Cheng, China]	Noted. We now not mention start date of the Argo program to avoid confusion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23515	24	55	24	56	Also consider this study "Near-Surface Salinity Reveals the Oceanic Sources of Moisture for Australian Precipitation Through Atmospheric Moisture Transport" "https://doi.org/10.1175/JCLI-D-19-0579.1" [Saurabh Rathore, Australia]	Noted. Though this section is about subsurface salinity change
116837	24		24		There is a need for coordination of this aspect (last deglaciation, phase lags etc) with the assessment done in chapter 5. The section needs a summary statement where you bring together what is learnt from the diversity of lines of evidence assessed. [Valerie Masson-Delmotte, France]	Accepted. We revised the paragraph and bring it better together with other lines of evidences
44969	24		25		Why don't you mention any work done on the global salt budget ? Because we have now more an more in situ salinity measurments, we can assess the global fresh water budget and therefore investigate its link with the global mean barystatic sea level and the continental ice melting. See for exemple Munk et al (2003); Wadhams et al (2004) and more recently Llovel et al (2019). [WILLIAM LLOVEL, France]	Accepted. Now assessed
12493	25	1	25	49	Adding a figure would greatly facilitate the discussion: showing both observational and model results of 0-2000m mean ocean salinity trend since 1960, which is backuped by the new study since AR5 and never been shown before. [Lijing Cheng, China]	Noted. Though we had to make choices on figure. The figure of salinity is in chap 3, so we decided not to include here
23517	25	3	25	6	confusing sentence, could be simplify. [Saurabh Rathore, Australia]	Not applicable. Removed
85255	25	6	25	6	Does this correspond to a strengthening of the global hydrological cycle - if it does is it worth stating it? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Removed. Actually replaced by a paragraph summarizing chapter 2,3 finding, and including links to hydrological cycle
98001	25	7	25	19	The broad/basin-scale results presented in Durack and Wijffels 2010 and previous and subsequent papers all show the same pattern of fresh getting fresher and salty getting saltier. The statement "most observational studies.." doesn't reflect the absolute consensus of all relevant studies. If "most" was accurate, then cite the descenting studies, and reduce the certainty statement in the exec summ from "virtually certain" to something that shows lesser agreement/certainty [Paul Durack, United States of America]	Not applicable. Text removed (the assessment is done in chap 2)
88605	25	14	25	14	Cheng et al., submitted. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
61187	25	16	25	18	Strong evidence for freshening of the surface mixed layer in the eastern subpolar North Pacific is provided by the analyses of Station P data given by Freeland (2013, Atmosphere-Ocean, https://doi.org/10.1080/07055900.2012.754330) and Cummins & Ross (2020, Prog. Oceanography, https://doi.org/10.1016/j.pocean.2020.102329) [Patrick Cummins, Canada]	Accepted. Reference now added and assessed
61189	25	16	25	18	The statement on freshening of the intermediate water in the North Pacific is too sweeping and should be qualified. In fact, long observational records show that there has been warming and compensating increases in salinity on isopycnal surfaces associated with North Pacific Intermediate Water. This is the case both in the western subpolar North Pacific (Nakanowatari et al., 2007, Geophysical Research Letters, https://doi.org/10.1029/2006GL028243) and at Station P in the northeast Pacific (Cummins & Ross, 2020, Prog. Oceanography, https://doi.org/10.1016/j.pocean.2020.102329). [Patrick Cummins, Canada]	Noted. Reference now considered in our assessment
69975	25	18	25	18	Not only Greenland Sea, but also the upper Labrador and Irminger seas are freshening. The latter effect is more important, as the seas of the Subpolar Gyre have a larger impact on the AMOC (Dukhovskoy D.S., I. Yashayaev, A. Proshutinsky, J.L. Bamber, I.L. Bashmachnikov, E.P. Chassignet, C.M. Lee, and A.J. Tedstone, 2019. Greenland Freshwater Flux Anomaly as a Possible Driver of the Recent Freshening in the Subpolar North Atlantic, Journal of Geophysical Research: Oceans, 124 (5), 3333-3360, doi: 10.1029/2018JC014686) [Dmitry Kovalevsky, Germany]	Noted. Reference now considered in our assessment
101827	25	18	25	18	"These report a multidecadal freshening in ...Greenland Seas (Friedman et al., 2017)..." - The reference Friedman et al., 2017 does not report on the Greenland Sea as stated in the text. There is only one small area in the Nordic Seas which is reported on, and this is not freshening. The northern North Atlantic (south of the Nordic Seas) has a negative (freshening) trend. The Greenland Sea has experienced increased salinities during the last three decades (Brakstad, A., Våge, K., Håvik, L., and Moore, G.: Water mass transformation in the Greenland Sea during the period 1986–2016, Journal of Physical Oceanography, 49, 121–140, DOI: 10.1175/JPO-D-17-0273.1, 2019) [IAPSO ECS group review, United States of America]	Agreed. We have now corrected the text
82895	25	18	25	18	I assume the Greenland Sea (without s at the end) is meant here. [Sebastian Gerland, Norway]	Not applicable. Removed
16353	25	19	25	19	Reference surname is “de Lavergne et al. (2014)” (no capital letter for consistency since the other Casimir de Lavergne publications have no capital in “de”) [Julian Mak, China]	Not applicable. Removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22569	25	22	25	22	To break up what is a very long paragraph I would add a paragraph break when starting to talk about numerical modelling here. [Peter Thorne, Ireland]	Noted. This paragraph has been entirely revised
29933	25	22	25	22	suggest adding new work on the recent freshening of the Subpolar North Atlantic after '..(Friedman et al., 2017)' and before 'Numerical mode'. The suggested is "The subpolar North Atlantic has in the past years experienced a significant freshening (Holliday et al., 2020). This is the largest and most rapid freshening experience since 1900." Reference: doi: 10.1038/s41467-020-14474-y [Léon Chafik, Sweden]	Accepted. Reference now considered in our assessment
88607	25	24	25	24	Cheng et al., submitted. Reference not found. [Rosemary Vieira, Brazil]	Noted. Citation is now fully referenced
27609	25	24	25	24	Please specify that E-P means Evaporation minus Precipitation. [Eric Brun, France]	Accepted. We removed the mention to E-P in this section
16355	25	25	25	25	Put “Cheng et a, submitted” to the back for choronogical ordering (cf. Line 32) [Julian Mak, China]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
19275	25	26	25	26	What does "free running" means when refering to the historical CMIP5 simulations? [Anne-Marie Treguier, France]	Accepted. This was unclear and has now been clarified
88609	25	28	25	28	Silvy et al., 2019. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
82897	25	28	25	28	I wonder if Silvy et al. (submitted) is meant here; the reference list does not include a Silvy et al. 2019 reference. [Sebastian Gerland, Norway]	Accepted. Corrected
19277	25	29	25	29	Do you mean "the strengthened AR5 attribution statements" or strengthened AR6 statements relative to AR5? [Anne-Marie Treguier, France]	Not applicable. Removed
101829	25	30	25	32	"There is medium confidence in the absolute rate of salinity changes through time, however agreement exists between independent estimates when error margins are considered" - If by "salinity changes" authors mean the global amplification pattern of SSS (judged by the studies cited), then the paper by Durack et al. 2012 (doi: 10.1126/science.1212222) should probably be cited as well when talking about different estimates of the rate of salinity field/water cycle amplification [IAPSO ECS group review, United States of America]	Not applicable. Removed
88611	25	32	25	32	Cheng et al., submitted. Reference not found. [Rosemary Vieira, Brazil]	Noted. Citation is now fully referenced
98003	25	33	25	35	Ch3 Fig 3.22 assesses subsurface biases in CMIP6 compared to WOA18 (contrasted to the CMIP5 results from Flato et al 2013, AR5 Ch9, global zonal mean) with some discussion of this in 3.5.2.1 [Paul Durack, United States of America]	Accepted, references were added
85257	25	33	25	36	Is it worth making this statement about biases before the projections stating their main causes, cloud, flux, sea-ice errors etc etc, and their potential implications for the fidelity of implications of salinity projections? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Mention of bias is now added
1727	25	37	25	39	Insert the main mechanisms in parentheses after "mechanisms" on line 39. [Michael Kennish, United States of America]	Not applicable. Removed
34463	25	42	25	44	This sentence is unclear: what does "only" mean here? It reads as if the observation is not consistent with any other simulations than CMIP5 simulations. [Claire Waelbroeck, France]	Not applicable. Removed
69977	25	43	25	43	In the subpolar North Atlantic (the region important from climatic point of view, since it directly affects AMOC) salinity is decreasing, which is parly due to the increasing the freshwater & sea-ice flux from the Arctic, partly due to the accelerating Greenland glaciers melt. The role of the latter constantly increases in time (Bamber, J., van den Broeke, M., Ettema, J., Lenaerts, J., & Rignot, E. (2012). Recent large increases in freshwater fluxes from Greenland into the North Atlantic. Geophysical Research Letters, 39, L19501. https://doi.org/10.1029/2012GL052552; Selyuzhenok, V., Bashmachnikov, I, Ricker, R., Vesman, A., Bobylev, L., 2020. Sea ice volume variability and water temperature in the Greenland Sea. The Cryosphere, 14 (2), 477-495, https://doi.org/10.5194/tc-14-477-2020; Haine, T. W. N., Curry, B., Gerdes, R., Hansen, E., Karcher, M., Lee, C., et al. (2015). Arctic freshwater export: Status, mechanisms, and prospects. Global and Planetary Change, 125, 13–35. https://doi.org/10.1016/j.gloplacha.2014.11.013) [Dmitry Kovalevsky, Germany]	Taken into account. Discussion of AMOC is in another section. We however now present more clearly change in subpolar North Atlantic, and assess new literature showing the link with glacial discharge.
23519	25	43	25	43	contrasting freshening in the Pacific basin [Saurabh Rathore, Australia]	Not applicable. Removed
86821	25	47	25	49	It would be very useful with a table with an overview of all different projections: salinity, temperature, sea level rise etc. in different oceans. Please consider to include this. [Oyvind Christophersen, Norway]	Rejected. Some of this information can be found in the interactive Atlas and cannot be treated here because of text length constraints
61307	25	47	25	49	In this passage, you forward to confidence statements wihtout providing a reference. The uncertainty guidance note, yauthors are requested to: 'Provide a traceable account describing your evaluation of evidence and agreement in the text of your chapter.' Please add your lines of evidence by pointing to another section or literature. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Evidences have now been clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82899	25	47	25	49	I suggest to consider listing reference(s) to support this statement. [Sebastian Gerland, Norway]	Accepted. Evidences have now been clarified
85259	25	49	25	49	I am surprised there are not salinity figures to go with this section (perhaps due to low confidence)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Some salinity figure can be found in chapter 3
93549	25	49			Medium confidence? High confidence? Cross citing "The role of the subtropical North Atlantic water cycle in recent US extreme precipitation events" by Li (2017) [Vivien How, Malaysia]	Noted, though we believe the proposed reference is not relevant to this particular assessment
99499	25	52	25	52	This whole subsection contains a lot of interesting material but it is not clear what the assessment findings are and how much of the material is required to support these findings. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have now streamline the text
40737	25	52	28	17	section 9.2.2.3: progress since AR5 not clear in this section. [TSU WGI, France]	Accepted. We revised and made progress sinceAR5 and SROCC clearer
95947	25	52	28	17	I believe that a paragraph that describes "paleo" water masses can be helpful, as done for example for the OHC. [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We unfortunately cannot expand the text more than it is, and we believe the paleo assessment here would be less relevant than in other places
67217	25	52	31	55	This section (and beyond) is extremely difficult to grasp for non-specialists due to the excessive number of acronyms. Please reduce and spell out as many as possible. [Regine Hock, United States of America]	Accepted. We tried to reduce use of acronyms
85261	25	54	25	54	In this section, or later at the start of modelling water masses paragraphs, is it worth introducing the idea that forming and maintaining interior water mass properties for their very long-lifecycles is challenging for ocean models due to spurious numerical mixing and poor representation of the myriad of mixing and ventilation processes, including both convection and dense overflows? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. In the interest of space, we do not include this general mention here, however, wherever relevant we indicated limiting processes for climate model in the individual water-masses paragraphs.
20175	25	54	26	2	Some words (formed, subducted, ventilate) are given in quotation marks. What does it mean? An explanation ought to be provided, possibly using the glossary. [philippe waldteufel, France]	Accepted. Quotes removed
40605	25	54	26	4	Note thtat the SOD glossary definition for water mass (coming from AR5) is "A body of ocean water with identifiable properties (temperature, salinity, density, chemical tracers) resulting from its unique formation process. Water masses are often identified through a vertical or horizontal extremum of a property such as salinity. North Pacific Intermediate Water (NPIW) and Antarctic Intermediate Water (AAIW) are examples of water masses." [TSU WGI, France]	Noted
2487	25	54			Some water masses are also formed by mixind of other water masses. CDW as a mix of NADW, AABW; PDW and IDW as the return flows of water masses in the deep Pacific/Indian Oceans [Thomas Ronge, Germany]	Noted.
77535	25	55			"convected" could be added or used to replace "subducted" [Emer Griffin, Ireland]	Rejected. We find subducted is clearer description of the physical process
81727	25		26		Section 9.2.2.3 - There are a lot of quotes "subducted" "ventilate" etc not sure why. [Laure Zanna, United States of America]	Accepted. Quotes removed
12495	26	1	27	55	A very informative and useful section. An additional figure related to water masses will be great (showing where the water masses are, their definitions, locations and changes). A plot like Fig.3 in Cheng et al. 2020 would be a candidate, but instead using density surface to label/identify water masses (happy to help if needed). Cheng, L.*, J. Abraham, J. Zhu, K. E. Trenberth, J. Fasullo, T. Boyer, R. Locarnini, B. Zhang, F. Yu, L. Wan, X. Chen, X. Song, Y. Liu, and M. E. Mann, 2020: Record-setting ocean warmth continued in 2019. Adv. Atmos. Sci., 37(2), https://doi.org/10.1007/s00376-020-9283-7. [Lijing Cheng, China]	Noted. Thank you.
12497	26	1	27	55	Is that possible to harmonize the unit used in this section? Some unit such as Wm-2 is area relevent, it is not helpful because it depends on how the water masses were defined. [Lijing Cheng, China]	Noted. We tried to harmonise as much as possible
61309	26	3	26	3	It might be worth to emphasise that 'non-surface' water masses provide higher signal-to-noise ratios. Please consider adding this adjective. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, we reworded the sentence to clarify we mean higher signal to noise ratio in subsurface waters
61399	26	13	26	13	Please consider to Add uper case letter to the 'W'estern 'B'oundary 'C'urrent [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The acronym is not used anymore

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101831	26	13	26	14	The warming of western boundary current extensions is stated here, and the reader is referred to section 9.2.2.1. However, section 9.2.2.1 does not explicitly mention the enhanced warming in WBC extensions. Recommend adding WBC extension warming to section 9.2.2.1. Possible citations: Rouault, M., Penven, P., & Pohl, B. (2009). Warming in the Agulhas Current system since the 1980 ' s. Geophysical Research Letters, 36(June), 2–6. https://doi.org/10.1029/2009GL037987, Wu, L., Cai, W., Zhang, L., Nakamura, H., Timmermann, A., Joyce, T., … Giese, B. (2012). Enhanced warming over the global subtropical western boundary currents. Nature Climate Change, 2(3), 161–166. https://doi.org/10.1038/nclimate1353, Yang, H., Lohmann, G., Wei, W., Dima, M., Ionita, M., & Liu, J. (2016). Intensification and poleward shift of subtropical western boundary currents in a warming climate. Journal of Geophsical Research: Oceans, 1–14. https://doi.org/10.1002/2015JC011486. [IAPSO ECS group review, United States of America]	Accepted. It was a typo. The correct section to reference here is 9.2.1.1 where warming in WBC is assessed
61225	26	14	26	17	The sentence that spans these 4 lines is very long and complex. It would help clarity if it were cut or if punctuation (:, ;) was used . [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence length has been reduced
77537	26	14	26	43	Significant number of English corrections required. [Emer Griffin, Ireland]	Noted. Editorial.
61311	26	16	26	16	I do not understand to what layer you refer to when speaking about thinning Please explain. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable, text removed
751	26	17	26	17	"associated to" change to "associated with" [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable, text removed
34465	26	20	26	20	I'd recommend to add "increase" after "about 1.5-2.0 °C of GSAT". [Claire Waelbroeck, France]	Not applicable, text removed
65889	26	21	10	22	Suggest avoiding using parentheses within parentheses. [Kushla Munro, Australia]	Noted. Editorial.
69085	26	22	26	22	Change " Sub Antarctic Mode Water (SAMW)" to "SubAntarctic Mode Water (SAMW)" [Bernadette Sloyan, Australia]	Accepted. Text revised
65887	26	25	219	3	Suggest clarification that this report contains different baseline period to previous reports. Suggest also including the implications for directly comparing estimates from this report to previous reports, i.e. are the estimates in AR6 for SLR higher or lower than in AR5, SROCC etc.? [Kushla Munro, Australia]	Noted. Consistent baselines and side-by-side comparisons now made in tables throughout.
20177	26	33	26	36	3 mistakes (at least) to be corrected please [philippe waldteufel, France]	Noted. Editorial.
24003	26	34	26	34	write "... SAMW appears as one of the strongest ...", instead of "... SAMW appears has one of the strongest ..." [Moacyr Araujo, Brazil]	Not applicable, text removed
753	26	35	26	35	"have already emerge" change to "have already emerged" [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
16357	26	38	26	38	Seems a bit weird having a short paragraph that does not say anything substatial in terms of being comprehensive, ground breaking or things with high confidence. Expand with quantitative details for balance reasons or get rid of it (!!!) [Julian Mak, China]	Accepted. Water-mass removed
83549	26	40	26	40	You could add here the following reference: García-Ibáñez, M.I., Pardo, P.C., Carracedo, L.I., Mercier, H., Lherminier, P., Ríos, A.F., Pérez, F.F., 2015. Structure, transports and transformations of the water masses in the Atlantic Subpolar Gyre. Progress In Oceanography 135, 18-36, doi: http://dx.doi.org/10.1016/j.pocean.2015.03.009. [Antje H. L. Voelker, Portugal]	Not applicable, text removed
65885	26	42	5	42	Suggest including a comma between 'scenario' and 'ranging'. [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
85263	26	50	26	50	CMIP5 model biases are getting 'smaller (not 'weaker')? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable, text removed
105477	27	4	27	17	Please update this part so that it is consistent with the discussion in section 9.2.3.1. [Helene R. Langehaug, Norway]	Accepted. The entire paragraph has been reworded
19279	27	4	27	52	Both NADW and AABW volumes would decrese in the future. In the north Atlantic, does this mean that the subpolar mode water volume would inflate? Is there some indication of this in CMIP models? [Anne-Marie Treguier, France]	Noted. This is an interesting point but it has not been assessed by anyone as far as we know.
101837	27	6	27	6	"...formed by deep overflows entraining water as they flow over sills between Scotland and Greenland" - Perhaps rephrase to "formed in the Nordic Seas and Arctic Ocean, and supplied to the North Atlantic by deep overflows over the sills between Greenland and Scotland" or similar, to emphasize what goes on north of the overflows [IAPSO ECS group review, United States of America]	Accepted. Text has been revised according to reviewer's remark.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96933	27	6	27	8	Here the most recent literature is missing, for instance Loder and Yashayev (2016) presenting the variability in the Labrador Sea convection area from the 1940s to 2016 from the surface to 2800m depth, and Rhein et al (2017), Ventilation variability of Labrador Sea Water and its impact on oxygen and anthropogenic carbon, Phil. Trans. R. Soc. A, doi:10.1098/rsta.2016.0321. [Nicole Wilke, Germany]	Not applicable, text removed
83839	27	8	27	8	plsease add brackets to the "high confidence" [Nianzhi Jiao, China]	Not applicable, text removed
61227	27	9	27	9	Missing parentheses around "high confidence". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable, text removed
96935	27	9	27	10	The LNADW time series in the papers cited ended around 2010, please add more recent references. [Nicole Wilke, Germany]	Accepted, references were added and text has been revised.
101839	27	10	27	11	"Lower NADW displays a density-compensated cooling and freshening trend since 1955, associated with similar density-compensated changes in deep overflows (Cunningham and Alderson, 2007; Mauritzen et al., 2012) medium confidence." - Recent studies find no volume/transport changes in the overflows, and a warming (density compensated by increased salinities) in one of the overflows (FBC), so not sure the confidence on the freshening is medium here (should maybe be lower). Østerhus et al., 2019 (Østerhus, S., Woodgate, R., Valdimarsson, H., Turrell, B., de Steur, L., Quad- fasel, D., Olsen, S. M., Moritz, M., Lee, C. M., Larsen, K. M. H., et al.: Arctic Mediterranean exchanges: a consistent volume budget and trends in transports from two decades of observations, 2019. https://doi.org/10.5194/os-15-379-2019) and Hansen et al., 2016 (Hansen, B., Húsgar Larsen, K. M., Hátún, H., and Øster- hus, S.: A stable Faroe Bank Channel overflow 1995–2015, Ocean Science, 12, 1205–1220, doi:10.5194/os-12-1205-2016, URL https://www.ocean-sci.net/12/1205/2016/, 2016) [IAPSO ECS group review, United States of America]	Accepted. References included and text revised.
101833	27	13	27	15	"CMIP5 models have large biases in NADW characteristics (Heuzé, 2017). Despite the large range in model bias in NADW water mass characteristics, the projected decrease in NADW formation is consistent across models" - If shortening is needed: Can be shortened to one sentence. [IAPSO ECS group review, United States of America]	Noted.
105475	27	15	27	17	Please provide references for this statement. Recent literature is suggesting a more stable Atlantic thermohaline circulation in the Arctic Medterranean than previously thought (Østerhus et al., 2019, https://doi.org/10.5194/os-15-379-2019; Lambert et al., 2016, https://doi.org/10.3402/tellusa.v68.31051). Østerhus et al. (2019) find in observations a stable Nordic Seas overflow transport, and Lambert et al. (2016) find that a more localized freshwater input is needed to cause large changes in the thermohaline circulation. [Helene R. Langehaug, Norway]	Accepted. References included and text revised.
101841	27	15	27	17	"It is concluded that it is likely that NADW will decrease in volume in the future, associated with a decrease in density in the northern North Atlantic and Arctic basins, where freshening of the upper 1400m dominates warming in terms of density decrease." - Observations from the Greenland Sea, one area where pre-Lower NADW is formed, show increased salinities in the upper 1500 m the last three decades (Brakstad et al. 2019), Brakstad, A., Våge, K., Håvik, L., and Moore, G.: Water mass transformation in the Greenland Sea during the period 1986–2016, Journal of Physical Oceanography, 49, 121–140, DOI: 10.1175/JPO-D-17-0273.1, 2019). Therefore, I am not sure there is basis for this conclusion. [IAPSO ECS group review, United States of America]	Noted. Reference included and text reworded but we disagree that the projected trend becomes unlikely if the signal is not present in the last 30-years of (some) observations.
69087	27	20	27	20	Change "which forms as admixture of NADW" to "which forms as a mixture of NADW" [Bernadette Sloyan, Australia]	Not applicable, text removed
101835	27	20	27	21	Also PDW/IDW are part of CDW ->"a mixture of NADW, PDW/IDW and Antarctic Bottom Waters (AABW) (Talley, 2013)." [IAPSO ECS group review, United States of America]	Rejected. For clarity we prefer not entering in too much water-masses details, which would not help the description and understanding of CDW change
101843	27	23	27	23	"The age of CDW" change to "The high age of CDW" [IAPSO ECS group review, United States of America]	Not applicable, text removed
61313	27	23	27	24	I have problmes understanding the first part of this sentence. I think there is a qualifier/adjective missing for the age of the CDW water masses. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable, text removed
96937	27	23	27	24	Do you mean 'old age of CDW'? Otherwise the sentence remains unclear. [Nicole Wilke, Germany]	Not applicable, text removed
99937	27	23	27	25	Southern Ocean confusion. Possibly re-write? [Dan Helman, United States of America]	Accepted. Text revised
112481	27	28	27	29	Spence et al 2014 (doi: 10.1002/ 2014GL060613) showed a plausible mechanism (weakening of the antarctic slope front) by which increased and poleward migrated westerlies can induce a larger presence of UCDW in the shelf waters of Antarctica. [Pedro LLanillo del Rio, Germany]	Accepted. This paper was (in the SOD) and is assessed (in the FGD)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46533	27	29	27	34	The work of Jeong et al. (2020; submitted 2019) could be mentioned here in terms of work that shows how the process of sub-ice shelf melting, absent from most ESMs, increases S. Ocean stratification, leading to warming of intermediate waters at depth. This is based on E3SM's novel capability to simulate sub-ice shelf circulation (and heat and freshwater exchange) within a fully coupled Earth system model (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, accepted). [Stephen Price, United States of America]	Accepted. Reference assessed
29241	27	33			Role of CDW varies from region to region but upwelling seems to be highly favored in areas where deep-waters are channeled in canyons at least in the Ross Sea, which could be more sensitive to upwelled (warm) CDW as compensating factor for channeling deep waters (Morrison et al., 2020, Science Advance) [Francesca Sangiorgi, Netherlands]	Noted. However, this is not so relevant when discussing CDW impact on cavity, since the increased upwelling is transformed in these regions by sea-ice formation (which feed dense water formation).
101845	27	35	27	35	Try to find a better word for intrude. I would associate an intrusion with a current or water mass but not the term 'warming'. Maybe say something like: '... Antarctica, and impacts the continental shelf in different regions.' [IAPSO ECS group review, United States of America]	Accepted. Word removed
96939	27	37	27	40	Please add recent and pertinent literature: Danabasoglu et al., North Atlantic simulations in coordinated ocean-ice reference phase II (Core II), Part II: interannual to decadal variability. (2016), Ocean Modelling 97, page 65-90, especially Figure 15 is pertinent to the statement made here. [Nicole Wilke, Germany]	Noted. Reference now considered
82901	27	39	27	39	I assume this should be Amundsen-Bellingshausen Seas with s at the end. [Sebastian Gerland, Norway]	Rejected. This is the name of one sea
101847	27	39	27	39	Reword 'driving CDW heat delivery'. For example: '...role of winds in transporting heat associated with CDW to the Amundsen-Bellingshausen Sea...' [IAPSO ECS group review, United States of America]	Accepted. Reworded
101849	27	41	27	41	"..caused a slight mean change of the local winds from 1920-2018, facilitating CDW heat intrusion on the Amundsen-Bellingshausen continental shelf and ice-shelf melt (Holland et al., 2019)" - The model study by Morrison et al, 2020, "Warm Circumpolar Deep Water transport toward Antarctica driven by local dense water export in canyons", https://doi.org/10.1126/sciadv.aav2516, may be a reference to be included somewhere here. [IAPSO ECS group review, United States of America]	Noted. However, this is not so relevant when discussing CDW impact on cavity and change, since the increased upwelling in these regions is transformed in these regions by sea-ice formation (which feed dense water formation).
36433	27	42	27	45	Additional evidence supporting the sensitivity of CDW access to the shelf to wind patterns is provided by Nost et al. (2011, JGR-O), Stewart and Thompson (2015, GRL) and Spence et al. (2017, Nat. Clim. Change). [Andrew Stewart, United States of America]	Rejected. We here only assess new literature since the SROCC.
101851	27	42	27	45	" In the future, there is high confidence that a change of wind pattern (Spence et al., 2014), and reduction in sea-ice production (Timmermann and Hellmer, 2013) are processes that can facilitate access of CDW in the sub iceshelf cavities, but there is low confidence on the quantification and importance of each of these processes." - add 'over the continental shelf' [IAPSO ECS group review, United States of America]	Rejected. Sub-ice shelf cavities are "over the continental shelf" so we prefer not being redundant
73847	27	43	27	44	Another study with different ocean model show the importance of sea ice production as Timmermann and Hellmer 2013. Obase et al. (2017), Responses of basal melting of Antarctic ice shelves to the climatic forcing of the Last Glacial Maximum and CO2 doubling, Journal of Climate, 30(10), 3473-3497 [Takashi Obase, Japan]	Accepted. Reference added to our assessment
61315	27	45	27	45	Iceshelf --> ice-shelf (correct terminology)/ [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Sent to copy editing for consistent spelling
36431	27	46	27	49	The thresholds for these shifts are quantified in a regional model of the Weddell Sea by Hazel and Stewart (2020, JGR-O). [Andrew Stewart, United States of America]	Accepted. Reference added to our assessment
101853	27	46	27	49	All three studies show that these changes occur due to stratification/density changes on the continental shelf. I think this would be worth mentioning as it does not contradict that there is low-confidence in the driving processes or thresholds. [IAPSO ECS group review, United States of America]	Rejected. We believe there is still uncertainty in processes due to low agreement, so prefer not discussing further specific processes
34467	27	51	27	51	It'd be better to be consistent in the use of the plural or singular for water masses. Both appear in the text for the moment. [Claire Waelbroeck, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
101855	27	51	27	54	A recent study in the Weddell Sea based on observations however shows a stabilization of AABW export (Abrahamsen et al., 2019,Stabilization of dense Antarctic water supply to the Atlantic Ocean overturning circulation, https://doi.org/10.1038/s41558-019-0561-2 - it is cited in the report already at another point) [IAPSO ECS group review, United States of America]	Accepted. Reference added to our assessment

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69089	27	51	28	17	Purkey, S. G., Johnson, G. C., Talley, L. D., Sloyan, B. M., Wijffels, S. E., Smethie, W., et al. (2019).Unabated bottom water warming and freshening in the South Pacific Ocean. Journal of Geophysical Research: Oceans, 124, 1778– 1794. https://doi.org/10.1029/2018JC014775. Update this increases, for he South Pacific Ocean. Purkey at al 2019 find that warming rates from the 2000s to the 2010s in the Southwest Pacific Basin have accelerated relative to the rates from the 1990s to the 2000s and that this acceleration is in contrast to the Southwest Atlantic, where near-bottom warming in the Argentine Basin, strong from 1989 to 2005, was reduced or absent from 2005 to 2014, although warming continued in the abyssal Brazil Basin (Johnson et al., 2014). Does the spatially variabillity variability of Southern Ocean AABW need to be commented on? [Bernadette Sloyan, Australia]	Accepted. Reference added to our assessment
65891	27	54	14	3	Suggest clarification of comparisons of estimates between AR6 and SROCC, since there are different baseline periods. Is it possible to compare 1995-2014 model means in from the CMIP 5 baseline/start of projections to the CMIP 6 models? If not, at least some indication of whether AR6 SSTs are expected to change more or less than SROCC SSTs. e.g. RCP 8.5 was 1.64 C - 3.51 C in SROCC but 2.2 - 4.0 in AR6. However presumably the 1995-2014 model mean is warmer than the 1986-2005 model mean so this comparison might be underdoing how much warmer SSTs are with this new modelling. Also why are AR6 SST estimates warmer than SROCC? [Kushla Munro, Australia]	Noted. Consistent baselines are not the major issue here--CMIP6 ensemble has much higher sensitivity discussed in many locations throughout the report
85265	27	55	27	55	Is it worth including a stronger statement along the lines of 'given the fact that Antarctic Bottom waters are formed in the models by spurious open ocean convection, a completely different mechanism to those forming Antarctic botttom waters in reality, i.e. brine rejection in polynias, water mass modification under-ice shelves, and dense cascaids, there is extremely low confidence in projections of Antartic Bottoms and related aspects of climate such as the lower cell of the MOC, heat uptake below 2000m and its impacts on the global energy budget and sea-level, etc? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We now report SROCC assessment, and update with improvements since then, which although notable improvements are noted, confirm SROCC. Our assessment is "low confidence"
85267	27	55	27	55	Is it worth discussing the issue of very poorly represented dense overlfows in most models and their implications for deeper water masses both in the Southern Ocean and North Atlantic. [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is discussed in the Southern Ocean section
16359	28	2	28	2	Reference surname is “de Lavergne et al. (2014)” (no capital letter for consistency since the other Casimir de Lavergne publications have no capital in “de”) [Julian Mak, China]	Editorial - copyedit to be completed prior to publication
24005	28	3	28	3	write "... large-scale circulation including ...", instead of "... large-scale circulation (including ..." [Moacyr Araujo, Brazil]	Not applicable, text removed
65895	28	4	19	4	Suggest using a different term rather than: 'wiped out' [Kushla Munro, Australia]	Accepted. The sentence was removed entirely during the revision of the chapter.
95933	28	12	28	17	There are now many studies suggesting that increased ice sheet discharge is the main driver of AABW changes, in particular freshening. Among the studies are: Jacobs Giulivi jclim 2011, Jacobs et al Science 2002, Nakayama et al 2014 grl, Nakayama et al Cryosphere submitted, Jullion et al. Jclim 2013. So I agree that we still do not know this well, but the body of literature is important. I would mention this in the report. [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These are assessed in SROCC. We report SROCC assessment consistent with the reviewer's suggestion
101857	28	16	28	16	"..a warming of 0.19±0.07°C, a salinification of 0.02±0.01 g kg-1, and a lightening of..." - where does the salinification come from? I would instead have thought that it is freshening due to a decrease in ice production, thus salt flux into the ocean over the continental shelves. Since in these cold temperature salinity dominates density it also does not seem to be consistent with a lightening. But maybe I am missing a key component. Heuze et al (2015) mention in their abstract that models do not agree on the sign of salinity changes. [IAPSO ECS group review, United States of America]	Not applicable, text removed (instead we report SROCC assessment which assessed both Heuze et al 2015 and Sallée et al., 2013)
85029	28	20	32	26	No comments [Katrine Husum, Norway]	Noted.
19289	28	22	28	22	In this section on AMOC, it could be interesting to have a link with section 9.2.3.4 on gyres. In the subpolar Atlantic, the AMOC in density coordinates is strongly dependent on the gyre component. What is the effect of abrupt changes of the subpolar gyre (p36l14) on the AMOC? [Anne-Marie Treguier, France]	Noted. Discussing the link to gyres is too much review instead of assessment for AR6. Abrupt change in the Subpolar Gyre is different from an AMOC collapse as discussed in Sgubin et al. 2017.
82903	28	22	28	35	I suggest to include somewhere here a cross reference to section 2.3.3.4.1 in chapter 2. [Sebastian Gerland, Norway]	Accepted. Reference included.
40739	28	22	32	25	section 9.2.3.1: improvements since latest reports not necessarily clear [TSU WGI, France]	Accepted. Text has been revised to clarify this point.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129415	28	22	32	25	The role of AMOC and its future change, along with others (e.g., circulation in Southern Ocean), can significantly modulate heat flux and climate on the continental climate systems. Such an importance is cited for further discussed in Chapter 11 and other chapters. Section 9.2.3.1 would benefit from more complete review and analysis under separate titles for three categories of evidence: measurements, modeling, and palaeoclimate studies. It would also be helpful to place the subject in one single location so as to avoid repitition and fragmented discussions. [Trigg Talley, United States of America]	Accepted. Text has been restructured according the second part of comment. For the first part of the comment, we decided to focus on new insights and delete more review-type text that can be found in earlier reports.
61407	28	22			Section 9.2.3.1 is very poorly written and hard to follow. In particularthe parts prior to the paragraph on impacts of AMOC on temperatures (Page 31, line 27). I have given some specific comments to improve things, but I think a thoughtful restructuring would be useful. In particular try to highlight the main message of each paragraph at the start, rather than leaving the reader guessing till the end. I believe the problem with this section is a result of text coming from multiple authors, which has not been carefully condensed and combined. In addition the section is too long, and probably can be summarized to be shorter - look at the formatting of the other sub sections under the section of "Regional Ocean Circulation" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been thoroughly restructured.
29935	28	24	28	24	replace "salty" by "saline" [Léon Chafik, Sweden]	Noted. Text has been rewritten
79527	28	24	28	26	Language could be improved and would aid clarity. I suggest: "The Atlantic Meridional Overturning Circulation (AMOC) is characterised by warm, salty waters flowing northward in the upper 1000 m. As they cool their bouyancy decreases and these waters start to sink, creating a return flow of North Atlantic Deep Water between 1000 and 3000 m depth (Buckley and Marshall, 2016).". In addition, 'decreasing salinity' could be added as a cause for the loss of bouyancy. Note that 'poleward' is bit ambiguous since that could mean south pole too, better to change to 'northward´. [Flor Vermassen, Sweden]	Noted. Text has been revised but also shortened and review-type parts have been cut.
40599	28	24	28	35	Note thtat the SOD glossary definition for AMOC (coming from AR5) is "The main current system in the South and North Atlantic Oceans. AMOC transports warm upper-ocean water northwards, and cold, deep water southwards, as part of the global ocean circulation system. Changes in the strength of AMOC can affect other components of the climate system." [TSU WGI, France]	Noted. However, we do not repeat the glossary in the chapter text.
3451	28	25	28	25	Why is the Arctic Ocean excluded here? The northernmost loop of the thermohaline circulation occurs in the Nordic Seas and the Arctic Ocean. Please rephrase to "...sinking at subpolar and polar latitudes..." [Georgi Laukert, Germany]	Noted. Sentence has been removed.
32909	28	25	28	26	note that much of the return NADW flow of the AMOC is between 3000 and 5000m rather than simply 1000 to 3000m e.g. McCarthy et al (2012), Bryden et al. (2005) [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Sentence has been removed.
29937	28	26	28	28	While I think this text "There is very high confidence that the AMOC is linked..." is OK, it is important to recognize the new emerging view that it is the seas between Greenland and Scotland as well as the Nordic Seas which are key to the MOC (Lozier et al., 2019, Chafik and Rossby 2019, Zou et al., 2020) and not the Labrador Sea. I know that this mentioned later in the text but needs to be done here as well. [Léon Chafik, Sweden]	Accepted. Text has been thoroughly restructured.
32911	28	27	28	28	In light of Lozier et al. (2019) we cannot have high confidence that the AMOC is linked to convective overturing in the Labrardor Sea. It is in many models, but may not be in the real world. Especially as many recently observed changes in the AMOC at 26˚N occur in the 3000 to 5000m depth range, which are not linked to the Labrador Sea. [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been thoroughly restructured.
61401	28	28	28	35	The structure of these lines is confusing because the authors have jumped back and forth between different ideas. I propose the following restructuring: "There is very high confidence that the AMOC is linked to convective overturning in the Labrador-Irminger Seas and Nordic Sea, and to the overflows between the Nordic Seas and North Atlantic Subpolar Gyre (Buckley and Marshall, 2016). The sinking branch itself is likely localized in the boundary current flowing cyclonically along the perimeter of the Nordic Seas and North Atlantic Subpolar gyre, which are also regions of intense surface exchange (Spall and Pickart, 2001; Katsman et al., 2018). At annual to decadal time scales the air-sea-land exchange of heat and salinity influences the convection and overflow rates, while at longer time scales (decadal to longer) the northward transport of heat and salinity by the AMOC itself can feedback onto the convection and overflow rates (Buckley and Marshall, 2016)". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Text has been thoroughly restructured.
101859	28	29	28	29	Remove "Iceland Basin" as the Iceland Basin is in the Nordic Seas (or change to "the Iceland and Greenland Basins in the Nordic Seas") [IAPSO ECS group review, United States of America]	Accepted. Text has been changed accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99501	28	30	28	35	It is not clear what this adds to the assessment so suggest clarifying or removing. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been thoroughly restructured.
7843	28	32	28	34	This is a very awkward sentence. I suggest rewriting [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This part has been removed.
77539	28	32	28	35	Very confusing sentence that should be rephrased. [Emer Griffin, Ireland]	Noted. This part has been removed.
61403	28	33	28	35	The authors invoke a sense of time scales by saying "…different time-lags, and at even large time scales …". The time scales need to be explicitly spelled out, in the current state "different time-lags" could imply hours to days and "large time scales" could imply monthly. However, I believe that the auhors probably meant to talk about longer timescales. I have suggested some probable timescales in the restructuring I suggest above, but I think these should be verified by the expert authors. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This part has been removed.
85269	28	35	28	35	Is it worth also introducing here the issue (discussed on the next page) of very poorly represented dense overflows in most models and their implications for MOC characterestics in models, e.g. that together with convection and other T-S biases they contribute to too shallow North Atlantic MOC in most models? I am not an AMOC expert so I obviously would trust expert judgment more my my thoughts on this but my personal non-expert concern is that perhaps the myriad of known model process representation errors and biases that impact on the representation of the AMOC could be emphasized more in a summary statement at the start of the modelling section, together with their implications for confidence in projections of AMOC changes? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text has been thoroughly restructured, but overflow problems are mentioned
61317	28	40	28	49	Figure 9.10. Caption: For the top row panels, it is unclear with respect to what time period the anomalies are computed. Please amend. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Now reference periods are explicit.
61319	28	40	28	49	Figure 9.10: Titel of bottom left panel is redundant with y-axis label. Please shorten the title. I suggest: 'Period: 1950-2050' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Now title makes reference period clear.
41461	28	40			Could you add a secondary y-axis to the top right CMIP5/CMIP6 panel showing percentage of AMOC change [Alexander Nauels, Germany]	Noted, but data not readily available.
7845	28	43	28	43	The compilation (done by me) is not from Jackson and Wood (2018) or any paper. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Caption text reference was misleading to suggest that the compilation might be found in that reference. The caption has been corrected by removing the reference in this location as part of the chapter corrigendum.
34469	28	49	28	49	"Supplementary material" is mentioned in several figure captions but I could not find any… [Claire Waelbroeck, France]	Accepted. Reference to Appendix corrected.
65893	28	52	18	55	Suggest including some Australian examples, e.g.: - Oliver et al. (2017) - https://www.nature.com/articles/ncomms16101/ - Benthuysen et al. (2018) - https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017JC013326 [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
46617	28	54	28	54	Add reference to e.g. Lozier et al. (217), doi: 10.1175/BAMS-D-16-0057.1, for OSNAP [Céline Heuzé, Sweden]	Accepted. Reference included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61405	28	54	29	17	All this text here is meant to explain the state of estimates from observations. However, the text is written in a very confusing way, and reads like a list of facts pieced together, rather than a synthesis. I suggest that the authors work hard to fix this. As expected by this report, please give your interepretation of the combined understanding from the different studies. Also, first try to highlight what you are more certain of (example long term trends) and later get into the more uncertain aspects. Here is a suggestive text "Changes in the strength of the AMOC based on observations are highly uncertain, but studies show a general weakening over time scales longer than a few decades. Long timeseries based on AMOC temperature-fingerprints indicate a reduction of approximately 3 Sv since the mid-20th century (Dima and Lohmann, 2010; Rahmstorf et al., 2015; Caesar et al., 2018), and paleo-proxies suggest that the twentieth-century AMOC is weaker than in the previous 1500 years (Thornalley et al., 2018). The estimates of variability at subdecadal time scales are only available since the late 80s (check??), and highly dependent on the estimation techniques. Indirect estimates of AMOC strength based on ocean reanalyses (Robson et al 2016) and North Atlantic Subpolar Temperature records (Jackson et al 2016) agree in the early 90s and suggest an increase in the strength of the AMOC, but disagree past the mid 90s to XX (do they reagree later??). The strength of the AMOC has been measured directly since 2004 using the RAPID array (Smeed et al., 2018) (Section 2.3.3.4). RAPID based estimates shows a large amount of variability compared to CMIP models (Roberts et al., 2014) (Figure 9.10), as can be expected because the CMIP models do not resolve mesoscale variability. This makes it is hard to estimate a long term trend from short record, regardless Smeed et al 2018 argue that between 2007 and 2011 the AMOC shifted to a state of reduced overturning; decresing from 18.8 Sv between 2004-2008 to 16.1 Sv after 2008. This estimate has been challenged by Trenberth et al 2019, who used a residual of atmospheric energy budget estimate, to argue that the the decreasing ocean heat transport trend is weaker than that suggested by RAPID. It is probably fair to say that the varying and contradicting messages from different studies are a result of large natural variability at annual to decadal scales, and also the different indirect observational estimates having different sensitivity to different processes. For example, it is possible that the temperature or salinity signature based estimates are recording not only the signature of AMOC, but also picking up signals of NAO like atmospheric variability." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been thoroughly restructured.
20549	28	54	32	25	This long discussion about AMOC and multiple conflicting evidences is frustrating, and all the more so since not many leads to progress are apparent. Correcting biases? [philippe waldteufel, France]	Accepted. Text has been thoroughly restructured.
7847	28	55	29	3	The way this is written implies that there is a contradiction. The AMOC can have shifted to a weaker state as part of variability. I suggest rephrasing [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised.
96941	29	1	29	1	Fig. 9.10 only gives AMOC time series from CMIP5 and CMIP6 models, we don't see any RAPID data and therefore cannot compare the observed red variability to modelled data. Please check. [Nicole Wilke, Germany]	Accepted. RAPID has been added along with changing latitude to 26N.
29647	29	1	29	55	somewhere in this page, the new AMOC paper can be cited which assess the AMOC in CMIP6 and CMIP5. Weijer, W., W. Cheng, O. A. Garuba, A. Hu, B. T. Nadiga, 2020, CMIP6 Models Predict Significant 21st Century Decline of the Atlantic Meridional Overturning Circulation, Geophys. Res. Lett., published online. Maybe also Hu, A., L. V. Roekel, W. Weijer, O. A. Garuba, W. Cheng, B. T. Nadiga, 2020, Role of AMOC in transient climate response to greenhouse gas forcing in two coupled models, J. Climate, Published online April 17, 2020 which studies the influence of AMOC on transient climate change. It is found that AMOC mean state may affect the mean climate in PI control and AMOC can modulate the rate of global climate response to greenhouse gas forcing. [Aixue Hu, United States of America]	Accepted. First Reference included.
32907	29	2	29	3	should say "Smeed et al. (2018) argue that the AMOC shifted to a state of reduced overturning after 2008" [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised accordingly.
65917	29	4	29	5	Suggest remove the parenthesis for clarity: 'The amount of decrease has been challenged by Trenberth et al. (2019).' [Kushla Munro, Australia]	Not applicable. This part has been removed.
33425	29	4	29	5	Change: "The amount of decrease has been challenged by (Trenberth et al., 2019),..." by "The amount of decrease has been challenged by Trenberth et al. (2019),...". [Guiomar Rotllant, Spain]	Not applicable. This part has been removed.
24007	29	4	29	5	write "... challenged by Trenberth et al. (2019), ...", instead of "... challenged by (Trenberth et al., 2019), ..." [Moacyr Araujo, Brazil]	Not applicable. This part has been removed.
65897	29	5	19	5	Suggest the full word is used, i.e. 'kilometres/kilometres' rather than "km's". [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3455	29	5	29	6	Where does 'their northward transport refers to is not clear'. Are these estimates by Trenberth et al. (2019)? [Petteri Uotila, Finland]	Not applicable. This part has been removed.
98691	29	5	29	6	"Their northward ocean heat transport.." - not clear who "Their" refers to. [Sonya Legg, United States of America]	Not applicable. This part has been removed.
61409	29	5	29	7	Since in this paragraph a comparison is being done between different estimates from observations, it is important to mention how Trenberth et al 2019 got their estimates. Specially since you refer to reanalyses in the second part - left me confused if the Trenberth et al 2019 was a reanalyses too or was it also from RAPID using a different method. Also the use of "Their" at the end of line 5 is ambiguous. I will suggest you change it to "Trenberth et al 2019 northward ocean heat transport estimate (dominated by AMOC), based on a residual estimate of atmospheric energy budget, shows a ..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
61411	29	6	29	6	You mention that Trenberth et al 2019 shows a "weaker trend", what direction is the trend in? Do they show that the AMOC increased or decreased? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
53539	29	7	29	9	or that it was driven by a subtle balance between the response to anthropogenic GHG vs aerosol forcings? [Hervé Douville, France]	Not applicable. This part has been restructured.
7849	29	7	29	12	This is confusing changes in the subtropical and subpolar AMOC. These can vary separately during variablity. Theres a new paper (Jackson et al 2019) showing that in this reanalysis (and others), the subpolar gyre weakened from the mid 90s, as in forced ocean models, while the subtropical AMOC showed a strengthening to around 2004 and then a weakening. See also Danabasoglu et al showing different responses in subtropics and subpolar gyre. Jackson et al (2019) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JC015210 Danabasoglu et al (2016) https://www.sciencedirect.com/science/article/pii/S1463500315002231?via%3Dihub [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Paragraph has been rewritten.
65899	29	9	19	10	Suggest clarification. Is the final 'high confidence' referring to the fragment "associated with long-term ocean warming trend" or "larger spatial extent"? Suggest including a confidence statement. [Kushla Munro, Australia]	Accepted. Paragraph has been rewritten.
19281	29	9	29	12	"In contrast": this sentence is difficult to understand. Is there really a discrepancy? The paper by Desbruyeres et al 2019 (https://www.ocean-sci.net/15/809/2019/) explains the apparent contrast between SPG heat content decrease and AMOC increase in 2012-2017 by just a 5 years lag. [Anne-Marie Treguier, France]	Noted. Text has been thoroughly restructured.
129417	29	9	29	12	This sentence interprets recent decadal changes in subpolar North Atlantic Ocean heat content entirely in terms of changes to the Atlantic meridional overturning circulation. It is not appropriate to state this interpretation so confidently, since the drivers of those decadal temperature changes has been subject to considerable debate. There is an entire literature that explores the possible role of wind-driven changes to the gyre circulation in effecting those decadal changes in heat content. This literature on wind forcing and gyre circulation should also be acknowledged. [Trigg Talley, United States of America]	Accepted. Text has been revised to clarify this point.
93557	29	13	29	15	Thornalley et al., 2018 suggests a weaker AMOC based mostly on slowing down of the DWBC backed up by the calculation of fingerprints and this is since 1850 (this may need correcting as it's not strictly the 20th century). There are other plenty reconstructions tackling different components of the surface and deep AMOC that do not show this shift (for all regional high resolution proxy data for the North Atlantic region see Moffa-Sanchez et al., 2019_AGU AMOC Special Issue - Deep AMOC watermasses in Fig 7 (2) proxy reconstructions of surface and subsurface temperatures in the North Atlantic spanning the last 2000 years do not show any clear E-W pattern (Zhang et al., 2008 AMOC T sub fingerprint) from 1850 compared to pre-industrial. See Figure 10 in Moffa-Sanchez et al., 2019 https://doi.org/10.1029/2018PA003508 which represents this clearly. This extra detail about the large discrepancies in the proxy data and the use of terrestrial data for AMOC fingerprints such as the SST from the subpolar gyre (e.g. Rahmstorf et al., 2015) perhaps reinforces the low confidence. [Paola Moffa-Sanchez, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Paragraph has been rewritten.
79529	29	14	19	14	In fact, only one paleo-proxy is used in the Thornalley study (the sortable silt proxy), so it is better to change 'paleo-proxies' to 'paleo-proxy'. [Flor Vermassen, Sweden]	Accepted. Text has been changed accordingly.
80437	29	14	29	15	Recent high-resolution paleoceanographic evidence suggests that the AMOC has remained remarkably stable throughout the Holocene (even when affected by large-scale, transient freshwater perturbations), further emphasizing that the recent (potential) decline is a pretty unique feature (Hoffmann et al., 2018 (GRL); Lippold et al., 2019 (GRL)). [Samuel Jaccard, Switzerland]	Not applicable. This part has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7851	29	15	29	15	contradicting messages on when the weakening occurred? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text has been thoroughly restructured.
29939	29	15	29	15	Suggest adding new work on the MOC after '(Thornalley et al., 2018)'. The suggested text is "A recent dynamic reconstruction of the Atlantic inflow to the Nordic Seas suggest a strong multidecadal variability synchronizing sea-surface temperatures in the North Atlantic and no slowdown over the past 70-100 years (Rossby et al., 2020)". Reference: https://doi.org/10.1029/2020GL087456 [Léon Chafik, Sweden]	Accepted. Reference included.
14839	29	15	29	15	how is the AMOC variability (past and present) taken into account here? Did it change much between past and present? How does it compare to the model simulated variability? [Marie-France Loutre, Switzerland]	Noted. Good point, but this has not been assessed in the literature.
3457	29	15	29	17	This sentence 'The various AMOC reconstructions ...' seems to miss a reference in particular the NAO effects. [Petteri Uotila, Finland]	Noted. Text has been thoroughly restructured.
111747	29	15	29	17	This interpretation is supported by a recent paper by Jackson & Wood (J. Clim accepted 2020), which we will send to the authors. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference included.
34471	29	15	29	17	Unclear sentence which could be replaced by "The various contradicting AMOC reconstructions might indicate that various AMOC-temperature and salinity footprints are timescale-dependent, or, affected by other processes such as the NAO." [Claire Waelbroeck, France]	Noted. Text has been thoroughly restructured.
89361	29	15			"various AMOC reconstructions contain contradicting messages" - I am not sure what is meant here. Those I know all contain the same first-order message, a 20th Century AMOC decline that is unprecedented in at least a millennium. There is some differences in timing, in particular with Thornalley's sorted silt proxies, but note that this is an entirely local flow speed proxy indicative of two spots, not of a larger region, and is thus not likely a good proxy for total AMOC transport. In any case it is not good practice to claim "contradictions" without any evidence or reference and without discussing whether these really are contradictions. To do justice to its assessment role, the AR6 authors should include a graph with a compilation of the proxy series listed above, all of which show a highly consistent picture. An example of such a graph was presented in August 2019 at the International Conference on Paleoceanography and is found here: https://twitter.com/rahmstorf/status/1176873455473614848?s=20 A more comprehensive intercomparison is presented in a manuscript by Caesar et al., currently in review at Nature Geoscience. I propose you contact Levke Caesar; the reconstructions shown in her compilation are in the published literature and can thus be shown in the AR6. Such a figure would provide a rational basis for discussing points of agreement and disagreement between different reconstructions. [Stefan Rahmstorf, Germany]	Noted. We also cite literature that contradicts and/or questions these messages. Also, CMIP models are not confirming these trends. The paragraph is rewritten to give a more balanced view of this problem.
89325	29	15			This is a very incomplete mention of proxy evidence for a twentieth-century AMOC that is weaker than during the previous millennium or more. For example the following studies have reconstructed the AMOC for the last millennium or more: Rahmstorf S, Box J E, Feulner G, Mann M E, Robinson A, Rutherford S and Schaffernicht E J 2015 Exceptional twentieth-century slowdown in Atlantic Ocean overturning circulation Nature Climate Change 5 475-80 Sherwood O A, Lehmann M F, Schubert C J, Scott D B and McCarthy M D 2011 Nutrient regime shift in the western North Atlantic indicated by compound-specific delta15N of deep-sea gorgonian corals Proc Natl Acad Sci U S A 108 1011-5 Spooner P T, Thornalley D J R, Oppo D W, Fox A D, Radionovskaya S, Rose N L, Mallett R, Cooper E and Roberts J M submitted Exceptional 20th century ocean circulation in the Northeast Atlantic Geophysical Research Letters Thibodeau B, Not C, Zhu J, Schmittner A, Noone D, Tabor C, Zhang J and Liu Z 2018 Last Century Warming Over the Canadian Atlantic Shelves Linked to Weak Atlantic Meridional Overturning Circulation Geophysical Research Letters 45 12,376-12,85 Thornalley D J R, Oppo D W, Ortega P, Robson J I, Brierley C M, Davis R, Hall I R, Moffa-Sanchez P, Rose N L, Spooner P T, Yashayaev I and Keigwin L D 2018 Anomalously weak Labrador Sea convection and Atlantic overturning during the past 150 years Nature 556 227-30 The latter with two independent proxies, making together six different reconstructions all finding a 20th Century AMOC decline that is unprecedented in at least a millennium. [Stefan Rahmstorf, Germany]	Noted. We focus however on new insights since SROCC. The paragraph is rewritten to hopefully give a more balanced view of the problem. We still have that CMIP models do not confirm this trend and that some reconstructions challenge this evidence. See remark above.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100805	29	17	29	17	NAO mentioned without reference to technical annex VI.2. The reference should be included. [Corti Susanna, Italy]	Noted. The NAO is discussed in many places before it is mentioned here (3rd time in Ch. 9, for instance). We decided not to include the reference each time the NAO is discussed, but leave that to where it is introduced first (Chapter 1 and 2).
19283	29	17	29	19	A weak negative trend between -0 and -2 Sv for CMIP5 does not show up in the figure 9.10, the CMIP5 AMOC seems unchanged. Quoting Menary et al (submitted): "the multi-model mean AMOC in CMIP6 increases by ~10% from 1850 to the 1980s, in contrast to the CMIP5 multi-model mean, which shows little change over this time period." [Anne-Marie Treguier, France]	Accepted. Text has been revised accordingly.
61413	29	18	29	18	Figure 9.10 is referenced to indicate a -2SV trend, however no such trend is visible in the figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised accordingly.
3459	29	19	29	20	Should stochastic weather forcing be listed here a a potential cause for the 60 year cycle in addition to aerosols? [Petteri Uotila, Finland]	Not applicable. This part has been removed.
109079	29	19	29	23	coordination with chapter 6 on section referencing section 6.3.4? [Chaincy Kuo, United States of America]	Not Applicable. This section is about the AMOC. There is no reference to section 6.3.4 in the text nor in any other part of Chapter 9, and no relation with anything written in Chapter 6. Maybe the reviewer has confused pagenumbers. This comment does not seem to apply to Chapter 9 page 29, lines 19-23.
89327	29	20			"CMIP6 models show no, or even a slight positive trend over the historical period with a larger AMOC increase between 1850 and 1985-1995". As you will know, many of the CMIP6 models show a climate sensitivity that is far too high, resulting in far too much aerosol cooling roughly 1940-1980 followed by a far too large warming trend until the present. Therefore this AMOC response is not surprising; the AMOC would strengthen - with some lag - in response to aerosol cooling and then weaken when the warming takes over. The previous sentence talks about the aerosol effect. [Stefan Rahmstorf, Germany]	Noted. While personally I tend to agree with your assessment there is unfortunately not an objective ground to articulate it here.
82905	29	21	29	21	I wonder if it should be here "only" instead of "even". [Sebastian Gerland, Norway]	Not applicable. This part has been restructured.
7853	29	21	29	22	I believe the Menary et al (submitted) paper shows this increasing trend is from aerosol forcing - should be mentioned [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised accordingly.
1729	29	23	29	26	What steps are necessary to increase the confidence that the AMOC has been declining since the mid-twentieth century and that the presently observed AMOC reduction is anthropogenically forced? This information should be added at the end of line 26. [Michael Kennish, United States of America]	Not applicable. Such a remark is beyond IPCC's remit.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89329	29	23	29	26	These lines contain three unjustified "low confidence" statements that can only be explained by ignoring a lot of evidence. In addition to the six independent proxy records, for the 20th Century there is further relevant papers providing evidence: Caesar L, Rahmstorf S, Robinson A, Feulner G and Saba V 2018 Observed fingerprint of a weakening Atlantic Ocean overturning circulation Nature 556 191-6 Cheng L, Trenberth K E, Fasullo J, Boyer T, Abraham J and Zhu J 2017 Improved estimates of ocean heat content from 1960 to 2015 Science Advances 3 e1601545 Osman M B, Das S B, Trusel L D, Evans M J, Fischer H, Grieman M M, Kipfstuhl S, McConnell J R and Saltzman E S 2019 Industrial-era decline in subarctic Atlantic productivity Nature 569 551 A key piece of evidence is the marked cooling in the subpolar Atlantic, shown in previous IPCC reports to be the only region on Earth with significant cooling. This cooling has been predicted by climate models as a response to anthropogenic warming and shown to be linked to an AMOC slowdown, no credible alternative explanation for this northern Atlantic "warming hole" has been advanced, and it has been shown to be of anthropogenic origin: Chemke R, Zanna L and Polvani L M 2020 Identifying a human signal in the North Atlantic warming hole Nature Communications 11 1540 The fact that this cooling is just what models predict in response to an AMOC slowdown, the associated observed strong warming along the US east coast as additional part of the "fingerprint" of an AMOC slowdown, and the lack of an alternative explanation for these observed features should alone give us at least medium confidence for the reality of an AMOC slowdown, even without the multitude of proxy evidence listed above. I think a reasonable discussion could be held whether the consistent results with a variaty of independent proxy methods, models and attribution studies provide "high" or "medium" confidence about an unprecedented 20th Century AMOC weakening, but "low confidence" is not reasonable. Perhaps it is	Noted. See discussion above. Here we do not re-assess (paleo) observations, which is done in Chapter 2, but focus on processes and mechanisms. We support the problems noted in Chapter 2 and contradiction with CMIP models but hopefully give a more balanced view. Because of a lack of agreement we have both low confidence in reconstructions showing these trends as in models not showing these trends. Our assessment is that we do not know what has been going on with the AMOC and more research is needed to resolve the issue why reconstructions are in such disagreement with CMIP models (or vice versa).
61415	29	24	29	24	"These results" Are you referring to the CMIP5/6 results? Or the Obs results? Or both? Please explicitly clarify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised to clarify this point.
3037	29	28	29	49	This section mentions Lozier et al. (2019) in passing, but that study deserves more attention. The OSNAP array isn't even mentioned, whereas there is a length discussion about the RAPID array. Lozier et al. (2019) found that the AMOC variability from regions (Irminger and Iceland basins) east of Greenland actually dominates that from the region (Labrador Sea) west of Greenland. Some caution could be used because it's only the first 21 months of data from OSNAP, but it could be more clear in the text that at least sometimes, the variability in the subpolar North Atlantic is derived from waters to the east of Greenland. [David Trossman, United States of America]	Accepted. Text has been thoroughly restructured.
82907	29	30	29	30	I wonder if "many (climate models)" could be more specified. [Sebastian Gerland, Norway]	Rejected. We can't mention which of the ca 30 climate models exhibit a certain bias and which not.
46619	29	30	29	31	"many climate models exhibit ocean convection that is too deep, over too large an area, too often and too far south" is word for word sentence from Heuzé (2017). Cite accordingly! [Céline Heuzé, Sweden]	Accepted. Reference included.
65901	29	31	21	33	Suggest including 'and' in list, e.g. between 'ship-based' and 'high-quality'. [Kushla Munro, Australia]	rejected. We added a comma instead
96943	29	33	29	33	CMIP3 or typo? [Nicole Wilke, Germany]	Not applicable. This part has been removed.
65921	29	36	29	36	Suggest rephrase for clarity: 'the AMOC has been declining SIGNIFICANTLY since the mid-twentieth century.' [Kushla Munro, Australia]	Not applicable. This part has been restructured.
98693	29	36	29	36	"significant" should be "significantly" [Sonya Legg, United States of America]	Not applicable. This part has been restructured.
80605	29	36	29	36	Roberts et al does not show an improvement in subpolar convection at higher resolution, if anything it becomes too strong (though there is low model diversity), though not explicitly analysed there. Koenigk et al (submitted) does look at mixed layer depths in HighResMIP with a mixed picture with resolution. Koenigk et al, Deep water formation in the North Atlantic Ocean in high resolution global coupled climate models. Submitted to Ocean Science. https://www.ocean-sci-discuss.net/os-2020-41/ [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised to clarify this point.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81729	29	37	29	47	1) You give high confidence to the relationship between convection and AMOC in proxies but not in the trends (the paragraph above) - is that a contradiction? 2) The relationship from Lozier (based on OSNAP) stems from a short timeseries so can you directly compare this results with that from other studies? [Laure Zanna, United States of America]	Accepted. Text has been thoroughly restructured.
111755	29	37	29	47	Correlation doesn't imply causation (even when there's a lag) and I think there is an implicit assumption here that the deepst convection drives the AMOC. Lozier et al show that this may not be the case as the main density transformations take place upstream of teh Labrador Sea. So I don't think there's a contradiction here (especially as the submitted paper cited shows consistency between one model and the OSNAP observations). More research needed for sure but I'm not convinced that the models get the variability wrong. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been thoroughly restructured.
129419	29	40	29	41	The Lozier result should be mentioned in the introductory paragraph for 9.2.3.1 as it challenges some fundamental assumptions and understanding of the AMOC. [Trigg Talley, United States of America]	Accepted. Text has been thoroughly restructured.
16361	29	40	29	41	Unnecessary splitting of sentence, consider instead “However, this relation is somewhat challenged by recent observations (Lozier et al 2019) where the impact of Labrador Sea convection of the AMOC appears small”. (No commas now, and puts the “this” closer to when it was previously used.) [Julian Mak, China]	Noted. Text has been thoroughly restructured.
7855	29	41	29	41	The Jackson et al (submitted) shows a good agreement in both the eddy-permitting (0.25) and non-eddy resolving (1 degree) climate models. This sentence suggests that it is eddy-resolving which achieves the weak overturning. There is also another paper which is submitted (Menary, Jackson and Lozier: Reconciling the role of the Labrador Sea in OSNAP and climate models) which I believe has been sent in which shows that these models agree with OSNAP, but also show a strong relationship between convection in the Lab Sea nad the AMOC and attempts to reconcile these ideas. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised to clarify this point.
29941	29	41	29	41	Suggest adding these references after Lozier et al., 2019. The suggested text is "…appears small (Lozier et al., 2019, Chafik and Rossby, 2019, Zou et al., 2020)". References: https://doi.org/10.1029/2019GL082110 and https://doi.org/10.1038/s41561-019-0517-1 [Léon Chafik, Sweden]	Not applicable. This part has been restructured. References are included.
88615	29	41	29	43	The sentence is not clear: "An eddy-permitting ocean resolution coupled model showed similar weak overturning across the southern section of the Labrador Sea as was found in (Lozier et al., 2019) (Jackson et al., submitted) but this result...." [Rosemary Vieira, Brazil]	Accepted. Paragraph has been rewritten.
61417	29	42	29	42	"eddy permitting ocean resolution coupled model" I believe you mean "A coupled model where the ocean component had eddy permitting resolution". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised to clarify this point.
61419	29	42	29	42	"Weak overturning" Do you mean weak overturning or weak impact on overturning? I am confused because the result from Lozier et al you are comparing is to about impact on overturning and not the overturning strength itself. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Lozier et al do calculate the overturning. Note, however, that the paragraph has been rewritten.
54353	29	43	29	43	remove parenthesis around Lozier et al 2019 [Luke Van Roekel, United States of America]	Rejected. Parenthesis occur automatically conform prescribed style.
33427	29	43			Change: “…section of the Labrador Sea as was found in (Lozier et al., 2019) (Jackson et al., submitted) but this result…” by “…section of the Labrador Sea as was found in Lozier et al. (2019) and Jackson et al. (submitted) but this result…”. [Guiomar Rotllant, Spain]	Rejected. Parenthesis occur automatically conform prescribed style.
16363	29	45	29	45	“...were found TO BE instrument in...” [Julian Mak, China]	Not applicable. This part has been removed.
3461	29	47	29	49	This concluding sentence is not linked to what is explained in the paragraph. Anthropogenic forcing is not mentioned in the paragraph, but is mentioned that recent high resolution models are more realistic in terms of reproducing deep convection. Would be better to change the sentence to 'Based on this we have low confidence that CMIP models are able to correctly simulate internal variations in AMOC, deep convection, and simulate long-term trends forced by large-scale changes in forcing. [Petteri Uotila, Finland]	Accepted. Paragraph has been rewritten.
129421	29	47	29	49	[CONFIDENCE] Based on the evidence compiled here, it is not clear how a "medium confidence" level for the simulated long-term changes is derived, given the "low confidence" and mentioned errors for the model simulations overall. [Trigg Talley, United States of America]	Accepted. Paragraph has been rewritten.
1731	29	47	29	49	Specify the long-term anthropogenic trends noted in line 49. These trends can be added at the end of line 49. [Michael Kennish, United States of America]	Not applicable. This part has been removed.
99503	29	48	29	48	It is not clear what the evidence is for the "medium confidence" statement, please clarify or add relevant references. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised to clarify this point.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111757	29	48	29	49	The statement about medium confidence in projecting anthropogenic trends is made twice (also at p30 l 14-15). I don't disagree, but it's an important statement and I think needs some justification since it is contrasted both times with a statement of low confidence in other aspects of the models.. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised accordingly.
46621	29	49	29	49	Add reference to Koenigk et al. (subm), doi:10.5194/os-2020-4, for CMIP6 / HighresMIP [Céline Heuzé, Sweden]	Accepted. Reference included.
29943	29	51	29	51	suggest adding a reference after 'Nordic Seas'. The suggested addition is "…Nordic Seas (Chafik and Rossby, 2019)". Reference: https://doi.org/10.1029/2019GL082110 [Léon Chafik, Sweden]	Accepted. Reference included.
51511	29	51	29	52	Potential citation not included but relevant here: Østerhus, S., Woodgate, R., Turrell, B., Quadfasel, D., Moritz, M., Lee, C. M., ... & Cunningham, S. (2019). Arctic Mediterranean exchanges: a consistent volume budget and trends in transports from two decades of observations. Ocean Science, 15(2), 379-399. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This section discusses the AMOC, not changes in the Arctic Ocean.
93559	29	51	29	55	There is some interesting paleoceanographic data showing short multidecadal (https://doi.org/10.1002/2016GL068227;https://doi.org/10.1016/j.quascirev.2015.06.007) and long-term changes in the overflows (https://doi.org/10.1016/j.quascirev.2015.06.007; https://doi.org/10.5194/cp-9-2073-2013;) and even compensation of the two overflows ISOW and DSOW on multicentennial timescales (https://doi.org/10.1002/2014PA002737). Or perhaps this comment could complement pg. 32 Line 1-13 [Paola Moffa-Sanchez, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We focus on new studies since SROCC.
33429	29	53			Change: "…et al., 2017) The representation…" by "…et al., 2017). The representation…". [Guiomar Rotllant, Spain]	Not applicable. This part has been removed.
61389	29	54	20	54	Please, as ACC is for the first time use in this chapter, consider giving the signification of this Acronym [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We spelt out ACC at first use
65903	29	54	24	55	Suggest avoiding use of double round parentheses if possible. e.g. (Argo program; Riser et al., 2016). [Kushla Munro, Australia]	Not applicable. This part has been removed.
88613	29	54	29	54	The references Wang et al., 2015b and Wang et al., 2015c are the same. [Rosemary Vieira, Brazil]	Accepted and corrected.
99505	30	1	30	1	Suggest "driven" in place of "sourced" [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
61421	30	1	30	1	Please indicate confidence of the statement that the source of nortward AMOC transport is from Agulhas. I doubt that this is a settled question. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This part has been removed.
19285	30	1	30	2	It is true that the Agulhas leakage is the main source, but a recent paper points out that the "cold route" is non negligeable (Ruhs et al 2019, https://www.ocean-sci.net/15/489/2019/). [Anne-Marie Treguier, France]	Not applicable. This part has been removed.
96945	30	1	30	15	The paragraph about Agulhas leakage appears a bit out of context and since there is only low confidence about linkage to AMOC it might need a clearer connection to the paragraphs above. [Nicole Wilke, Germany]	Noted. This part has been removed.
101861	30	2	30	2	There is evidence that approximately 50% of Agulhas Leakage takes place outside of Agulhas Rings (Cheng, Y., Putrasahan, D., Beal, L., & Kirtman, B. (2016). Quantifying Agulhas leakage in a high-resolution climate model. Journal of Climate, 29(19), 6881–6892. https://doi.org/10.1175/JCLI-D-15-0568.1). Suggest editing to "large eddies (Agulhas Rings), as well as cyclones and filaments" [IAPSO ECS group review, United States of America]	Not applicable. This part has been removed.
61423	30	2	30	3	"Numerical model experiments" Please be more specific here. I assume this is not reference to idealized experiments, but probably some data assimilative results, since you are suggesting seeing actual changes in circulation since 1960s. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
7857	30	6	30	7	The AMV is not normally defined as cross-hemispheric so I don't know how relevant this is. I suspect that the impact is on the SSTs in the south Atlantic rather than the key pattern in the N Atlantic [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
16365	30	7	30	7	AMV is defined here but not used until page 31, consider moving "(AMV)" to page 31 line 36 [Julian Mak, China]	Not applicable. This part has been removed.
34473	30	7	30	9	The sentence is too vague. I suggest to replace "a multidecadal proxy" by "a multidecadal satellite-derived proxy". [Claire Waelbroeck, France]	Not applicable. This part has been removed.
65905	30	9	26	9	Suggest including either a hyphen or space in 'Subtropical'. [Kushla Munro, Australia]	rejected. Subtropical is a word
82909	30	9	30	13	I wonder if both CMIP5 and CMIP6 models are addressed here. I suggest to specify. [Sebastian Gerland, Norway]	Not applicable. This part has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39797	30	9		10	"Most CMIPmodels" which generation? CMIP5? CMIP6? Both? [TSU WGI, France]	Not applicable. This part has been removed.
99507	30	11	30	11	Re use of "potentially", is this just conjecture (in which case remove) or is there evidence (in which case state) [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
93551	30	11			Cited statement "high-resolution models potentially give better projections of changes in Adulhas leakage" with reference "Quantifying Agulhas Leakage in a high-resolution climate model" by Cheng et al., (2016) [Vivien How, Malaysia]	Not applicable. This part has been removed.
79931	30	12	30	15	This sentence is repeated. It appears in the previous page 29 in line 47 [Somavilla Raquel, Spain]	Not applicable. This part has been removed.
3463	30	12	30	15	This sentence is identical to the last sentence of the previous paragraph. It needs to be rewritten. [Petteri Uotila, Finland]	Not applicable. This part has been removed.
34475	30	12	30	15	This sentence is a repetition of l. 47-49 of p. 29. It would be better to discard these lines p. 29. [Claire Waelbroeck, France]	Not applicable. This part has been removed.
61425	30	12	30	15	This last sentence is an exact copy of the sentence on page 29, line 47-49. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
46631	30	15	30	15	e.g. there or anywhere in that section, a discussion of flow in the other direction, from North Atlantic to Nordic Seas (and from Nordic Seas to Arctic) is missing. See Heuzé and Årthun (2019), doi:10.1525/elementa.354, for CMIP5, and references therein to Berx, to Gelderloos, to Årthun, to Furevik etc for observations [Céline Heuzé, Sweden]	Noted. This part has been removed.
99509	30	17	30	17	Given how poorly many aspects of the AMOC are simulated (see 9.2.3.1) is "very likely" justified here? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The assessment has changed.
7899	30	17	30	17	Very likely' contradicts the earlier summary of likely in P5 L37. Chapter 4 also states 'likely' [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The assessment has changed.
132577	30	17	30	34	You should probably cite Sigmond et al. 2020 (doi: 10.1038/s41558-020-0786-0) here, or discuss it in the paragraphs below. [Kyle Armour, United States of America]	Accepted. Reference included.
7859	30	18	30	18	AMOC can recover after longer timescale forcings so this isn't necessarily true. See chapter 4, page 90, lines5-12 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised to clarify this point.
3465	30	19	30	19	'Previous cycles’ means ‘previous CMIP cycles’, right? Then better mention it explicitly. [Petteri Uotila, Finland]	Accepted. Text has been revised accordingly.
34477	30	19	30	19	What does mean "cycles" here? previous Assessment Reports? I suggest to replace "than in previous cycles" by "than previously estimated". [Claire Waelbroeck, France]	Accepted. Text has been revised accordingly.
96947	30	19	30	19	What is meant by 'previous cycles'? [Nicole Wilke, Germany]	Accepted. Text has been revised accordingly.
77801	30	19	30	19	What are “cycles”? Maybe you mean “phases of CMIP”? Or do you mean some cyclical variation of AMOC? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised accordingly.
61427	30	19	30	19	"cycles" What do you mean? Do you some natural variablity cycles? Or previous intercomparison studies? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised accordingly.
39805	30	19			"previous cycle" is ambiguous: do you mean IPCC cycles? if so, it's better to use "assessment reports" or specify which ones (e.g. AR5) [TSU WGI, France]	Accepted. Text has been revised accordingly.
34479	30	20	30	20	There is a typo in the name of the scenarios which should be SSP… [Claire Waelbroeck, France]	Accepted and corrected.
29649	30	20	30	20	change "SPS" to "SSP" [Aixue Hu, United States of America]	Accepted and corrected.
29651	30	20	30	22	Gokhan analyzed CORE forced ocean runs with multimodels. He also found that in the forced ocean-sea ice runs, there is a strengthening trend of AMOC during second half of 20th century. Here we may need to cite his paper. [Aixue Hu, United States of America]	Noted. We focus however on new research since SROCC.
88617	30	21	30	21	Weijer et al., submitted. Reference not found. [Rosemary Vieira, Brazil]	Noted. Reference is updated.
54355	30	21	30	21	Weijer et al is accepted (2020) [Luke Van Roekel, United States of America]	Noted. Reference is updated.
7861	30	21	30	22	This is also addressed in more detail by Jackson et al (submitted) which I sent in. Note that we find a greater weakening in models with higher resolution, but think this is because the higher resolution models have different mean states with stronger AMOC and that the different mean state affects the AMOC weakening. One major caveat (and also for Robserts et al) is that all these models are based on NEMO [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference included.
3467	30	22	30	25	Description of historical simulation results belongs to the earlier paragraph in page 29, lines 1-26. Here it appears repetive. [Petteri Uotila, Finland]	Accepted. Text has been thoroughly restructured.
7863	30	22	30	30	This is talking about the historical runs so should be moved/merged with the first paragraph in page 29 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been thoroughly restructured.
61429	30	23	30	24	"a second increase" Is this a feature that the reader is expected to see in figure9.10? I can not find any such signal in the figure. Or is this from a certain referene, if so please indicate here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Text has been rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129423	30	24	30	24	The use of the term "early nineties" should be "the 1990s" for consistency. [Trigg Talley, United States of America]	Accepted and corrected.
61431	30	26	30	26	"1900" Is this a typo? Because you seem to be talking about the "nineties" in this sentence. Or is the sentence supposed to be talking about the "nineteenth century"? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been rewritten.
3469	30	27	30	27	Multi-model mean contains only the forced AMOC changes if he multi-model mean entirely captures the internal variability, which it most surely does not. This claim also contradicts what is written in the end of the paragraph of the role of internal variability. [Petteri Uotila, Finland]	Noted. Text has been rewritten.
3471	30	28	30	30	I do not understand this conclusion. If the multi model mean does not contain internal variations but only changes due to forcings, e.g. upward AMOC is due to aerosols, then why did this change not occur in the real world? I would expect rather that internal variations in simulations generate changes that do not occur in the real world. [Petteri Uotila, Finland]	Noted. Text has been rewritten. However, in the real world internal variations occur that are largely eliminated in an ensemble mean. So the ensemble mean can only follow the forced changes occurring in the real world, never the whole signal.
65907	30	33	26	33	Suggest removing ';' after RCP8.5 for clarity. [Kushla Munro, Australia]	Not applicable. This part has been removed.
16367	30	33	30	34	"...coming century there will STILL be decades when increases [IN WHAT?] occur" [Julian Mak, China]	Not applicable. This part has been removed.
82911	30	34	30	34	I suggest to specify here what is addressed when writing about increase. [Sebastian Gerland, Norway]	Not applicable. This part has been removed.
29653	30	36	30	42	Here you may also cite Hu, A., G.A. Meehl, W. Han, J. Yin, B. Wu, M. Kimoto, 2013, Influence of continental ice retreat on future global climate, J. Climate,26, 3087-3111, doi:10.1175/JCLI-D-12-00102.1 and Stouffer, R. J., D. Seidov, and B. J. Haupt, 2007: Climate response to external sources of freshwater: North Atlantic versus the Southern Ocean. J. Climate, 20, 436–448. In both papers, the seesaw like changes of AABW and NADW have been studied using CCSM3 or GFDL model. [Aixue Hu, United States of America]	Not applicable. This part has been removed.
3473	30	37	30	42	This is a horribly long sentence in a brief paragraph, and therefore difficult to understand. Split to at least three sentences for readability. [Petteri Uotila, Finland]	Not applicable. This part has been removed.
96949	30	44	31	13	We understood from this subsection that the melting of the Greenland ice sheet (GrIS) could have a major impact on the AMOC. It would be very helpful to understand what the influence might be, if the projections would account for the melting at least in a general manner. Would the melting further weaken the AMOC? Please add some clarity here. [Nicole Wilke, Germany]	Accepted. Text has been revised to clarify this point.
111753	30	44			Are you sure? I think AR5 and SROCCC both assessed this as 'very unlikely'. If the assessment is now 'unlikely' this needs to be discussed. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text has been revised including additional arguments.
65925	30	45	30	45	Suggest rephrase for accuracy: 'the vast majority of models up until now…' [Kushla Munro, Australia]	Not applicable. This part has been rewritten.
26373	30	47	30	47	the previous A5 assessment->AR5 [María Santolaria-Otín, France]	Accepted and corrected.
54357	30	47	30	47	A5 -> AR5 [Luke Van Roekel, United States of America]	Accepted and corrected.
3475	30	47	30	47	Would probably make more sense to cite the more recent IPCC SROCC assessment report here instead of AR5. This would also better fit to comparisons with SROCC elsewhere in this chapter. [Petteri Uotila, Finland]	Accepted. Text has been revised accordingly.
82913	30	47	30	47	I assume it should be AR5 instead of A5. [Sebastian Gerland, Norway]	Accepted and corrected.
16369	30	47	30	47	Typo, "AR5 assessement" not "A5" [Julian Mak, China]	Accepted and corrected.
10239	30	47	30	47	A5 → AR5 [Katsumata Katsuro, Japan]	Accepted and corrected.
77541	30	47			Should read "AR5" not "A5" [Emer Griffin, Ireland]	Accepted and corrected.
7865	30	48	30	49	This point is out of order. It is discussed in more detail on the following page so should be removed here [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been restructured.
96951	30	50	30	53	Yang et al, 2016 showed a relation between LSW thickness and freshwater, indicating that increased freshwater reduced the LSW convection. The time series in the paper ended before the next onset of deep convection in the Labrador Sea. Since winter 2013/2014, the two time series are inversely related, i.e. despite the increasing freshwater the convection intensified. It is still the air-sea fluxes that dominate convection in the Labrador Sea. Rhein et al (2018) could show that the additional freshwater from Greenland has not intruded in the LSW convection area yet- Please add a reference to Rhein et al., (2018), Greenland submarine melt water observed in the Labrador and Irminger Sea. Geophys. Res. Lett., 45, 10,570-10,578, doi:10.1029/2018GL079110. [Nicole Wilke, Germany]	Not applicable. This part has been removed.
99511	30	51	30	53	Please add ranges and likelihoods to the two numbers quoted. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
16371	30	52	30	52	The use of "50%" are not accompanied by error bars which seems oddly precise as written (given error bars or use of degree of confidence are pervalent in the document) [Julian Mak, China]	Not applicable. This part has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61433	30	53	30	53	What do you mean by Labrador Sea Water "thickness"? Are you talking about the volume? Or change in stratification? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
82915	30	53	30	54	If the link discussed is only such that GrIS melting affects Labrador Sea water volume, this could be specified. Using the term "linked" alone could suggest effects in both directions. [Sebastian Gerland, Norway]	Not applicable. This part has been removed.
93553	30	53			Cited statement "there is medium confidence that GrIS melting and the volume of Labrador Sea Water are linked" with reference "Sensitivity of Labrador Sea Water Formation to Changes in Model Resolution, Atmospheric Forcing, and Freshwater Input" by Garcia-Quintana et al., 2019 [Vivien How, Malaysia]	Not applicable. This part has been removed.
7867	30	54	30	55	This is based on one model study. Also the fluxes used to force the model after 2010 were extrapolated from previous time series, according to the paper. If this is the only evidence for this statement then it should be at least downgraded to low confidence, though I don't think the evidence is strong enough to support this statement. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
69979	30	55	30	55	The effect of GrIS on the North Atlantic upper ocean salinity is also thoroughly discussed in a more recent study by Dukhovskoy D.S., I. Yashayaev, A. Proshutinsky, J.L. Bamber, I.L. Bashmachnikov, E.P. Chassignet, C.M. Lee, and A.J. Tedstone, 2019. Greenland Freshwater Flux Anomaly as a Possible Driver of the Recent Freshening in the Subpolar North Atlantic, Journal of Geophysical Research: Oceans, 124 (5), 3333-3360, doi: 10.1029/2018JC014686; [Dmitry Kovalevsky, Germany]	Not applicable. This part has been removed.
83551	30	55	30	55	The lack of influence is also corroborated by the deep Labrador Sea mixing reported in Zunino, P., Mercier, H., Thierry, V., 2020. Why did deep convection persist over four consecutive winters (2015–2018) southeast of Cape Farewell? Ocean Sci. 16, 99-113, doi: 10.5194/os-16-99-2020. [Antje H. L. Voelker, Portugal]	Not applicable. This part has been removed.
89331	30	55	31	2	"low confidence that this melting has already affected the evolution of the AMOC over the 20th century, as claimed by e.g. (Rahmstorf 1 et al., 2015; Yang et al., 2016b). Interestingly this "low confidence" claim is based on one model simulation, which started adding meltwater from Greenland only from 1990, with very slow ramp-up, leaving hardly enough time for the AMOC to respond. Estimates for Greenland mass loss (Box and Colgan 2013) suggest that ice loss started in the 1930s and already by 1990 had added more freshwater than in the entire Böning model experiment.from 1990-2020 - thus adding much more freshwater, and much earlier, than assumed by Böning. Also, Böning et al. concluded that the meltwater impact would soon emerge, hence the title of their paper "Emerging impact of Greenland meltwater on deepwater formation in the North Atlantic Ocean". A critical assessment of their model results would therefore conclude that had they used the full meltwater history of Greenland, as reconstructed by Box and Coltan, their model would have supported the conclusion of Rahmstorf et al. 2015, which reads: "It is thus plausible that the accumulated freshwater input from Greenland may have made a significant contribution to the observed freshening trend." I see the Böning et al. model simulation as strengthening this conclusion. Also consider the discussion that follows in the chapter, finding that most models likely have a too stable AMOC. [Stefan Rahmstorf, Germany]	Not applicable. This part has been removed.
116843	30		30		cross chapter coordination is needed on the issue of "CMIP models are too stable due to tuning and model biases" especially with ch 3, 4, 8 [Valerie Masson-Delmotte, France]	Noted. Coordination has taken place.
29655	31	1	31	13	You may cite Hu, A., G.A. Meehl, W. Han, J. Yin, B. Wu, M. Kimoto, 2013, Influence of continental ice retreat on future global climate, J. Climate,26, 3087-3111, doi:10.1175/JCLI-D-12-00102.1 and Hu, A., G. A. Meehl, W. Han and J. Yin, 2011, Effect of the Potential Melting of the Greenland Ice Sheet on the Meridional Overturning Circulation and Global Climate in the Future, Deep Sea Res. Part II, 58, doi: 10.1016/j.dsr2.2010.10.069, 1914-1926. In these two papers, idealized runoff from GrIS and Antarctic IS has been systematically studied. [Aixue Hu, United States of America]	Not applicable. This part has been removed.
65913	31	3	28	5	Suggest clarification. This text is missing a closed parentheses ")". Suggest breaking this sentence into 2 sentences or replacing the open parentheses "(" with a comma. [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
85271	31	4	31	4	models 'which' neglected [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
7869	31	4	31	4	that have neglected this effect [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
20179	31	4	31	4	Verb missing; alternatively replace "neglected" by "neglecting" [philippe waldteufel, France]	Not applicable. This part has been removed.
755	31	4	31	4	“simulated by the CMIP models neglected this effect” needs a that: “simulated by the CMIP models that neglected this effect” [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61435	31	4	31	4	"decreases … effect" needs to be rephrased, probably should read "CMIP models that neglected this effect" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
111751	31	4	31	9	With apologies for self-citing, Wood et al 2019 https://doi.org/10.1007/s00382-019-04956-1 (Fig 6) show that even if Mov contains information about the bistability of the current state, there is little relationship between Mov and the distance from a threshold for abrupt change. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
7871	31	6	31	9	I think it's worth pointing out that this bias has improved in cmip5 (and likely cmip6) [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
99513	31	9	31	9	The "high confidence" here seems at odds with the "very likely" on P30 L17 [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
1733	31	9	31	10	Discuss the ramifications of existing model biases that limit the accuracy of the models displaying the correct sensitivity of the AMOC response to global warming. [Michael Kennish, United States of America]	Not applicable. This part has been removed.
96953	31	9	31	11	Maybe consider rephrasing 'High confidence in model biases and limited accuracy of AMOC response in models' to a negative statement. [Nicole Wilke, Germany]	Not applicable. This part has been removed.
85273	31	9	31	13	Could these sentences be clarified? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
129425	31	9	31	13	[CONFIDENCE] Isnt this "high confidence" in the model biases and model skill -- and thus model limitations -- contradicting the "very likely" AMOC decrease statement on page 30, line 17? This needs to be reconciled. [Trigg Talley, United States of America]	Not applicable. This part has been removed.
129427	31	11	31	13	Grammar issue. However and "on the other hand" are used in one sentence. [Trigg Talley, United States of America]	Not applicable. This part has been removed.
2475	31	11	31	13	The sentence starting with "However..." I find to be rather obscure. I understand every word, but cannot find the meaning in the sentence. [Howard J. Freeland, Canada]	Not applicable. This part has been removed.
89335	31	11			"there is on the other hand not sufficient support that without these biases the likelihood of an abrupt AMOC collapse before year 2100 is significantly increased" The Liu et al. 2017 reference provides that evidence. [Stefan Rahmstorf, Germany]	Not applicable. This part has been removed.
3477	31	13	31	13	Do you mean increased from CMIP3 to CMIP6? [Petteri Uotila, Finland]	Not applicable. This part has been removed.
3489	31	16	31	16	Meltwater fluxes are already affecting the AMOC. Change to '… and Antarctica increasingly affect the evolution ...' [Petteri Uotila, Finland]	Not applicable. This part has been removed.
77803	31	17	31	17	You can't say "With an emulator" unless you make Bakker et al the subject of the sentence. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
65931	31	19	31	19	Suggest there's a word missing: '...to decline IN an RCP8.5 scenario' [Kushla Munro, Australia]	Not applicable. This part has been removed.
61437	31	19	31	19	"with 74% [] by 2290-2300". What is the 74% figure referring to? Also there is a typo, should be "in" RCP8.5. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
29657	31	21	31	21	Cite: Hu, A., G.A. Meehl, W. Han, J. Yin, B. Wu, M. Kimoto, 2013, Influence of continental ice retreat on future global climate, J. Climate,26, 3087-3111, doi:10.1175/JCLI-D-12-00102.1 and Stouffer, R. J., D. Seidov, and B. J. Haupt, 2007: Climate response to external sources of freshwater: North Atlantic versus the Southern Ocean. J. Climate, 20, 436–448. [Aixue Hu, United States of America]	Not applicable. This part has been removed.
7873	31	21	31	22	This needs rephrasing - as it is it suggests that the AMOC does not respond much to additional freshwater which is incorrect. Maybe 'It should be noted, however, that there is large model uncertainty around the amount of freshwater required to collapse the AMOC' [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
7901	31	21	31	25	Very little is said here about potential for collapse/hysteresis. More discussion around AMOC response and model differences is needed. In particular, chapter 4 deals with abrupt change/hysteresis in general, and points to this section for more background. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
69981	31	23	31	23	The principal possibility of the bi-stable AMOC is also obtained analytically, using a simplified 2-box dynamic model (Kovalevsky D.V., Bashmachnikov, I.L. 2020. An analytical model of open-ocean deep convection with multiple steady states, Ocean Modelling, accepted). According to this study, the parameters which affect the state of the system are the mid-latitude upper ocean temperature and salinity, as well as the intensity of the heat release to the atmosphere in the subpolar basin. [Dmitry Kovalevsky, Germany]	Not applicable. This part has been removed.
65915	31	30	28	35	Suggest breaking this sentence into two sentences, for clarity. [Kushla Munro, Australia]	Not applicable. This part has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22571	31	30	31	34	Likelihood language usage implies medium or high confidence yet low confidence is given. Either the confidence is wrong or likelihood language should not be used. My feeling is it is the latter - that use of likelihood language here is an issue? [Peter Thorne, Ireland]	Not applicable. This part has been removed.
29945	31	36	31	37	Nothing is said about a dipole in the Knight et al. (2005). I suggest to redefine the AMV following that used in Ruprich-Robert et al. (2017). The suggested text is "A major impact of the AMOC on climate is through its link with the AMV, a large-scale SST horse-shoe pattern in the North Atlantic (Knight et al., 2005) with a maximum loading over the subpolar gyre (Ruprich-Robert et al., 2017) and a timescale of .... " Reference: https://doi.org/10.1175/JCLI-D-16-0127.1 [Léon Chafik, Sweden]	Accepted. Text has been revised accordingly.
81731	31	36	31	52	I think this paragraph again might not reflect the recent advances in understanding the AMV: the AMOC/AMV low-frequency view has been challenged in several studies. The AMOC/AMV is also strongly model dependent. The role of forcing has been explored in observations, models, and paleo-proxies, and the role of the wind-driven circulation as well. Here are some papers that should probably be assessed, even if you give them low confidence: for the gyre/wind driven components: Williams et al. 2014, 2015; Piecuch et al. 2017. For forcing: Bellomo et al. (2018), Clement et al. 2015. For all internal and external sources in proxies and models: O'Reilly et al 2019. [Laure Zanna, United States of America]	Accepted. Text has been revised to clarify this point.
129429	31	36	31	52	An interesting result of this study is that freshwater forcing can result in a weakening of the AMOC whether or not the freshwater propagates down the water column. When the freshwater does mix downwards, the increased buoyancy of the freshened region leads to AMOC weakening. While freshwater that does not mix downwards collects to form a barrier to surface fluxes, causing the development of a temperature anomaly below, this in turn reduces the buoyancy and weakens the AMOC. The formation of a surface cap does not qualitatively alter the rate of AMOC weakening, and the AMOC weakening was not found to be sensitive to whether the weakening is dominated by salinity or temperature changes. Citation: Haskins et al. 2020: https://link.springer.com/article/10.1007/s00382-019-04998-5 Temperature domination of AMOC weakening due to freshwater hosing in two GCMs. [Trigg Talley, United States of America]	Not applicable. This remark seems to refer to an earlier paragraph that has been removed.
41895	31	36	31	52	Our paper (Garry et al., in review, title: Climate models may underestimate pre-industrial North Atlantic Ocean summertime multidecadal variability) in second stage of review at Climate Dynamics is very relevant to this paragraph, as it compares the variability of north east Atlantic temperatures from a high resolution multi-centennial proxy to CMIP5 models, finding that CMIP5 models (and higher resolution shelf model) are likely to underestimate multidecadal variability. The paper also shows the relationship between the proxy site and the north Atlantic, inferring that multidecadal variability across the Atlantic is likely to be underestimated. Please get in touch with freya.garry@metoffice.gov.uk if you would like to see the draft manuscript. The abstract follows: "Multidecadal variability is a prominent feature of the North Atlantic ocean, but this variability is not well understood due to the relatively short nature and anthropogenic 'contamination' of instrumental records. This restricted knowledge of multidecadal variability limits our ability to evaluate climate prediction capability for regions around the North Atlantic. Multi-centennial annually resolved and absolutely dated marine oxygen isotope (d18Oshell) data from bivalves uniquely provide absolutely dated, high resolution insights into preindustrial summertime marine variability. The d18Oshell variability represents the combined influence of summertime seawater salinity and temperature. We compare preindustrial d18Oshell marine variability at a site west of Scotland (Tiree Passage, Hebridean Shelf) to the variability in summertime model data at the same location in a hierarchy of models, including CMIP5 global climate models and a high-resolution shelf sea model. On annual timescales, d18Oshell variability from the bivalves and that derived from instrumental observations are consistent with the range of model simulations, providing confidence in the proxy estimate. On multidecadal timescales the models generally underestimate pre-industrial multidecadal variability as recorded by the bivalves. Multidecadal variability in last millennium climate model simulations with external forcing (solar and volcanic) is only half that recorded in the bivalve record on 20-year timescales. Models and observations indicate that variability at the Tiree Passage is representative of wider change across parts of the North Atlantic. Our results therefore imply that models do not adequately represent the wider North Atlantic processes that result in Atlantic multidecadal variability." [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79533	31	36	31	52	It seems like this section mainly discusses the view that the AMOC influences or is responsible for the AMV (i.e. importance of ’internal ocean variability’). However, it is also possible that the AMV can be explained solely by external forcing and there are quite a lot of papers suggesting this. I think this section should at least mention the latter view as well. For example, I think the authors could add a reference to the study of Haustein et al. (2019), who show that the AMV can be explained by external forcing alone, if the effect of aerosols is taken into account. Link:https://journals.ametsoc.org/doi/10.1175/JCLI-D-18-0555.1 [Flor Vermassen, Sweden]	Accepted. Text has been revised to clarify this point.
3035	31	36	31	52	The study by Garuba et al. (2018, in Geophys. Res. Lett.) is relevant to this discussion as well because they found that the ocean-forced variability associated with the AMV weakens when the ocean and atmosphere are coupled and this damps the ocean-forced SST variability and ultimately the AMV. [David Trossman, United States of America]	Noted. This paper, however, discusses a different point than what is assessed here.
99515	31	37	31	37	Do we really have enough observations to be able to be sure such a long period "oscillation" really exists? Could model-generated modes be artifacts? Need some evidence of the former and some discussion of the latter. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We do think there is general consensus on the existence of a 60-80 year AMV in the observations.
79531	31	39	31	41	To me it is a little unclear what ‘projects on’ means, perhaps better is ‘reflects’ or ‘influences’?The sentence structure could also be improved. I propose: ‘The AMV strongly influences/reflects global mean surface temperature, although it is underestimated in models. As a result, low-frequency variations in global mean temperature are underestimated as well (high confidence) (Zhang et al., 2019b).’ [Flor Vermassen, Sweden]	Accepted. Text has been revised to clarify this point.
65909	31	43	26	43	Suggest changing 'Waters (AAIW)' to either 'Waters have' or 'Water has'. [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
1735	31	45	31	46	Discuss the ramifications of the limited ability of models to simulate realistic AMV and other AMOC variability. [Michael Kennish, United States of America]	Accepted. Text has been revised accordingly.
111749	31	47	31	49	This apparent contradiction has recently been shown to be largely due to different authors using different definitions of the AMV. See Wills et al. https://doi.org/10.1175/JCLI-D-18-0269.1 [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference included.
132579	31	47	31	49	I think the relative roles of stochastic atmospheric variability versus AMOC in the AMV is more subtle than stated here. Many recent papers show that the AMV is impacted by both simultaneously, for instance see the discussion in Chapter 4 (page 46, Lines 46-52) and in Wills et al. 2019 (doi: 10.1175/JCLI-D-18-0269.1). [Kyle Armour, United States of America]	Accepted. Text has been revised to clarify this point.
65911	31	51	28	2	Suggest shortening this very long sentence, for clarity. [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
105479	31	51	31	52	It seems important to connect this part with section 4.4.3.5, which discusses AMV skill in decadal predictions. [Helene R. Langehaug, Norway]	Noted. This aspect however is not discussed here.
82917	31	52	31	52	I suggest to consider writing "multi-decadal" instead of "low-frequency". [Sebastian Gerland, Norway]	Accepted. Text has been changed accordingly.
89333	31	54			The entire discussion of DO events is confused and not well supported by evidence; it falls well behind the discussion in the AR4. E.g. it ignores lots of evidence for the linkage between DO events and AMOC changes from deep sea sediments as well as the bipolar see-saw. It basically gets DO events backwards when it attributes stadial-interstadial switches to "abrupt AMOC-decreases by freshwater release" - all the evidence, including the abrupt warming in Greenland, points to them being abrupt AMOC increases due to convection starting in the Nordic Seas. The freshwater release story applies to Heinrich events, and they need to CO2 or other trigger. I may recommend my Nature review paper here, although it is not very fresh - but older evidence should not be forgotten after all. Rahmstorf, S. Ocean circulation and climate during the past 120,000 years. Nature 419, 207-214 (2002). doi:10.1038/nature01090 [Stefan Rahmstorf, Germany]	Accepted. Text has been restructured, but this part has been removed.
116845	31		31		On the evaluation of the skill of models for internal variability, please coordinate the assessment with ch 3. This is very important and would deserve to also be reflected in the TS. [Valerie Masson-Delmotte, France]	Noted. Coordination has taken place.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83553	32	1	32	5	There is now evidence beyond the Younger Dryas and Heinrich Stadial 1 as demonstrated by the Pa/Th records for the last glacial cycle published by 1) Böhm, E., Lippold, J., Gutjahr, M., Frank, M., Blaser, P., Antz, B., Fohlmeister, J., Frank, N., Andersen, M.B., Deininger, M., 2015. Strong and deep Atlantic meridional overturning circulation during the last glacial cycle. Nature 517, 73-76, doi: 10.1038/nature14059 and 2) Henry, L.G., McManus, J.F., Curry, W.B., Roberts, N.L., Piotrowski, A.M., Keigwin, L.D., 2016. North Atlantic ocean circulation and abrupt climate change during the last glaciation. Science 353, 470-474, doi: 10.1126/science.aaf5529. [Antje H. L. Voelker, Portugal]	Rejected. We focus on new studies since SROCC.
34481	32	1	32	5	New observational studies have demonstrated the occurence of large changes in AMOC across all D-O, i.e. not only across the Younger Dryas and the last Heinrich stadial HS1. Here are the references of these recent studies: Gottschalk et al. (2015) Nature Geoscience 8: 950-954, doi: 10.1038/ngeo2558; Henry et al. (2016) Science 353(6298): 470-474, doi: 10.1126/science.aaf5529; Waelbroeck et al. (2018) Clim. Past 14: 1315-1330, doi: 10.5194/cp-14-1315-2018. Therefore, the sentence should be modified into the following 3 sentences: "A strong link between AMOC variability and D-O events has been found in modelling studies (Lynch-Stieglitz, 2017). Moreover, new observational studies (Gottschalk et al., 2015; Henry et al., 2016; Waelbroeck et al., 2018) have demonstrated the occurence of large changes in AMOC, not only across the Younger Dryas and the last Heinrich stadial HS1, but across all D-O events of the last glacial period. We thus have high confidence that switches between different modes of the AMOC occurred and were essential in explaining the concurrent change in temperature." [Claire Waelbroeck, France]	Accepted. Reference included.
23471	32	2	32	3	The reference Lynch-Stieglitz (2017) should be placed after "scarce", as this paper is a review of the observational evidence not modelling studies. [Jean Lynch-Stieglitz, United States of America]	Not applicable. This part has been rewritten.
77545	32	6	32	7	Not clear where it states "evidence includes the onset of a basin wide AMOC" whether this means a strong or weak AMOC. [Emer Griffin, Ireland]	Not applicable. Text removed
61463	32	7	32	8	Gap: there is no mention of sea ice ocean interactions and their feedback mechanisms linked to water mass transformation - e.g.Abenathy et al 2016, and recent work by Haumann et al 2020, who show subsurface warming occuring which may be retarding global surface warming [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This suggestion seems out of the context of this paragraph
2489	32	9			Although there is no PMOC that is an analogue to the modern AMOC, we do have an active meridional circulation in the Pacific today, ventilating it from the south. [Thomas Ronge, Germany]	Not applicable. Text removed
83555	32	10	32	10	There is paleo-evidence for AMOC/NADW formation at least back to 4.7 Ma. A reference for this, building on the combination of several time series, would be for example Bell, D.B., Jung, S.J.A., Kroon, D., Hodell, D.A., Lourens, L.J., Raymo, M.E., 2015. Atlantic Deep-water Response to the Early Pliocene Shoaling of the Central American Seaway. Scientific Reports, 5, 12252, doi 10.1038/srep12252. [Antje H. L. Voelker, Portugal]	Noted. However, this is not a review, but assesses recent paleo-evidence that directly supports assessment on future changes.
93561	32	13	32	14	Some model studies and data have also suggested that climate centennial variability across the last millennium (and perhaps applicable to other centennial variability) around the North Atlantic could be explained by changes in the subpolar gyre strength https://doi.org/10.1007/s00382-016-3106-7 (model study) https://doi.org/10.1038/ngeo2094 (data-model study) and would not have required strong changes in the AMOC agreeing with Lippold et al., 2019. [Paola Moffa-Sanchez, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
80439	32	13	32	25	High-resolution paleoceanographic evidence suggests that the AMOC became instable during peak interglacials of the past, notably when temperatures were warmer than today (e.g. MIS 5e, MIS 7e, MIS 9e, MIS 11c - Galaasen et al., 2014 (Science); Galaasen et al., 2020 (Science)) possibly associated with the bippoar seasaw (Hayes et al., 2014 (Science); Thomas et al., 2020 (QSR)). I would recommend briefly assessing these points, which I feel are important in better predicting the long-term behaviour of the AMOC in the face of accelerated warming. [Samuel Jaccard, Switzerland]	Accepted. Reference included.
61455	32	16	32	16	what about submesoscale eddy processes? importance of parameterised mesoscale and submesoscale? eddy processes [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Unclear what the reviewer means and how eddies would affect our view on AMOC mode switches in the paleo-record.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85275	32	28	32	28	General comment: From my personal perspective the statements of confidence and uncertainty did not seem fully consistent between this section and other regional and topic sections? For example, this section states reasonable confidence in several qualitative aspects model projections for a region which, although obviously critical for global climate, has huge known climate model biases, and cancelling errors, which spread through the closely coupled atmosphere-ocean-ice system, linked with many deficiencies in process-representation. I would expect these errors to have implications for the fidelity of and uncertainties in many aspects of projections (e.g. see Hyder et al, 2018). Also in-situ observations from ships are almost entirely limited to summer months and the year round observations we do have from from drifting buoys, and a few few moorings, are still only a few years in duration. Are the confidence statements in this section therefore fully consistent with those of the Meredith et al, 2019, Special Report on the Ocean and Cryosphere in a Changing Climate, which I perhaps wrongly understood to suggest low confidence in many aspcts of Southern Ocean projections, given the known prevalent model errors? By contrast, for example, the tropics section stated low confidence in projections, given the many tropica biases. [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. In the SOD, we tried to differentiate quantitative and qualitative assessment, with greater confidence in the latter. But we have now reconsidered this approach, and stick to the SROCC approach. We however complement their assessment from new line of evidences including paleo which can strengthen our confidence for some aspect of our assessment. Nevertheless, we believe we responded to this comment by carefully reconsidering confidence level to lower confidence
40741	32	28	34	46	section 9.2.3.2: there seems to be a lack of traceability in numerous statements of the sections (e.g. p32 L53-55), please check. [TSU WGI, France]	Accepted. We have now clarified reference when referring to the SROCC assessment
153	32	28	34	46	This section should explicitly comment on Circumpolar Deep Water and its relationship to the southern westerly winds and the ACC. The CDW is important for delivering heat to the western coast of Antarctica (Rignot et al., 2019). Authors should comment on how changes in the southern westerly winds are influencing this important current, and evaluate the strength and confidence in the relationship. Line 50 on P.32 comments on the souterhn westely winds, but that ACC transport is only weakly sensitive. How does this relate to the changes in the CDW mentioned later on in the report? Also, when mentioning the SWW, it would be useful to also discuss the ozone hole and how this affects the winds (cf. Rintoul et al. 2018). [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Circumpolar Deep Water are carefully assessed in a dedicated section 9.2.2.3, and here there associated circulation (overturning cell) is also carefully assessed. Change in winds and their relation to ozone hole are also assessed.
85277	32	30	32	30	This paragraph seems a very long sentence so could it perhaps be made clearer as two sentences? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
85333	32	30	32	30	Is it perhaps worth stating that the myraid of Southern Ocean model errors, and observational unknowns linked to data sparsity particuarly in winter (where prior to ARGO there were virtually no observations), also have important implications for uncertainties in global-scale aspects of climate change because of the Southern Ocean's key role in global climate? For example, see summary in Hyder et al, 2018 introduction "The Southern Ocean plays an important role in global ocean heat and anthropogenic carbon uptake (refs) [yet it is poorly represented in climate models]. For example, recent climate model experiments suggest that 75 ± 22% of ocean heat uptake and 43 ± 3% of ocean anthropogenic carbon uptake over the historical period occurs south of 30°S (ref). The Southern Ocean also influences climate sensitivity (ref), the Meridional Overturning Circulation (MOC) (ref), water mass formation (ref), sea level through basal melt of ice shelves (ref), nutrient cycling (ref) and the Inter-tropical Convergence Zone (ITCZ) position (ref). It has a persistent dynamical connection from the stratosphere to the deep ocean (ref), involving clouds, air-temperatures, winds, surface heat fluxes, sea surface temperatures (SST), and ocean thermal structure leading to interrelated coupled model biases in all of these quantities (refs)". See also more detailed expert discussion by Rintoul, S. in Science Diplomacy: Antarctica, Science and the Governance of International Spaces (eds Berkman P. A., Lang M. A., Walton D. W. H. & Young O. R.) 175–187; Rintoul, S. R. et al. The global influence of localised dynamics in the Southern Ocean. Nature 558, 209–218 (2018); Rintoul, S. R. et al. Choosing the future of Antarctica. Nature 558, 233–241 (2018). [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	This is assessed in the SROCC and we report their assessment here. The importance of data sparsity for assessing OHC is assessed in 9.2.2.1
65919	32	31	29	31	Suggest clarification to the statement: 'too deep, over too large an area, too often and too far south'. What is happening too often? Or is there a misplaced comma or missing/extra word? [Kushla Munro, Australia]	Accepted. Rephrased
20181	32	31	32	31	"approximately" would be consistent with a much less detailed numerical value, such as 170 Sv [philippe waldteufel, France]	Not applicable. Text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101863	32	31	32	31	"The circulation of the Southern Ocean is composed of a strong eastward flowing current, the Antarctic Circumpolar Current (ACC), which transports approximately 173.3 ± 10.7 106 m3 s–1 (Donohue et al., 2016) of water eastward connecting the world's three main ocean basins..." - There is a substantial difference between this one estimate provided by Donohue et al. (2016) and earlier estimates found in literature. The argument that only the difference in baroclinic and barotropic transport explains this difference seems not to hold given that also model reanalysis estimates have a substantially lower transport. Therefore, we would provide more caution in reporting only a single value here and rather refer to a range of values, including earlier estimates and explaining a potential cause for the range. [IAPSO ECS group review, United States of America]	Not applicable. Text removed
16373	32	31	32	31	Units, Sv is used elsewhere so maybe use Sv here too [Julian Mak, China]	Noted. Though here this is not applicable since the text has now been removed
61457	32	32	32	34	the sentence seems a bit laborious. Suggestion to change to 'provide us with lines of evidence allowing us ot reach medium to high confidence in the qualitative assesment ' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
16375	32	33	32	33	Units, Sv is used elsewhere so maybe use Sv here too [Julian Mak, China]	Noted. Though here this is not applicable since the text has now been removed
77543	32	33			Stick with either Sverdrup or m3/s and don't alternate between the two units. [Emer Griffin, Ireland]	Noted. Though here this is not applicable since the text has now been removed
46623	32	34	32	46	too long and complex a sentence, hard to follow [Céline Heuzé, Sweden]	Accepted. Rewritten and simplified
22573	32	37	32	39	This should cross-reference to chapter 5 where an in-depth assessment of this aspect of the carbon cycle was performed if I remember correctly. [Peter Thorne, Ireland]	Accepted. Reference added
82919	32	38	32	38	I wonder if it is on purpose that the confidence statement is placed here between the number and uncertainty range (different from point i in line 35-37). [Sebastian Gerland, Norway]	Not applicable. Text removed
61453	32	39	32	39	suggest wording change: ocean-ice shelves interactions - ocean ice shelf interactions [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
69687	32	39	32	39	"ocean-ice shelves" should be "ocean-ice shelf" [Matthew Hoffman, United States of America]	Not applicable. Text removed
61461	32	39	32	40	Add new work by Wahlin et al provided a mechanism by which baroclinic CDW waters flow in basal channels (https://doi.org/10.1038/s41586-020-2014-5) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Not relevant for our assessment here
101865	32	39	32	46	If shortening is needed: we suggest to condense point (iii) [IAPSO ECS group review, United States of America]	Accepted. Rewritten and simplified
69689	32	40	32	40	"Antarctic Ice Shelves" should be "Antarctic ice shelves" [Matthew Hoffman, United States of America]	Not applicable. Text removed
46535	32	42	32	44	The work of Jeong et al. (2020; submitted 2019) should be mentioned here. As noted above, that work shows how the process of sub-ice shelf melting, absent from most ESMs, increases S. Ocean stratification. This is based on E3SM's novel capability to simulate sub-ice shelf circulation (and heat and freshwater exchange) within a fully coupled Earth system model (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). [Stephen Price, United States of America]	Rejected, not relevant here. Though we now consider in our assessment later in this section
101867	32	42	32	46	"..which impacts the stability of the Antarctic Ice Sheet (Section 9.4.2) and in return the melt of the ice-shelves can feedback onto the large-scale circulation through widespread increase of the upper-ocean stratification (high confidence) or through facilitating heat transport toward the ice-shelf base (low confidence)..." - The recent study by Moorman et al. (2020) shows that this widespread increase in upper-ocean stratification and heat transport to the ice shelves might be much more complex than previously thought and that it does not necessarily hold for all regions of the Antarctic coast. We suggest a higher level of caution and a more differentiated view of this process here. Also the cause of the increase upper ocean stratification in Southern Ocean high-latitudes is still being debated with other possible explanations being changes in precipitation and sea ice. Therefore, the "high confidence" stated here seems not justified. (Moorman, R., A.K. Morrison, and A.M. Hogg (2020): Thermal responses to Antarctic ice shelf melt in an eddy rich global ocean–sea-ice model. J. Climate, https://doi.org/10.1175/JCLI-D-19-0846.1) [IAPSO ECS group review, United States of America]	Not applicable here. Though Mororman (was and) is included in our assessment. Plus, we believe the range of evidences including clear theoretical understanding, provide high confidence increased stratification driven by basal melt (even if there can be other additional causes)
65197	32	43	32	44	I think it is important to note that current models do not represent giant tabular icebergs at all (which are estimated to account for ~40% of the freshwater deposited into the Southern Ocean from the Antarctic ice sheet). I think this is an important limitation in understanding where the location in which the freshwater is deposited in the Southern Ocean, and the associated effects, e.g. Lago and England 2019. [Mark England, United States of America]	Accepted. We added calving as a limitation, in addition to basal melt

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61459	32	44	32	47	is this sentence incomplete? Difficult to follow [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
46625	32	45	32	46	Add reference to Wåhlin et al. (2020), doi:10.1038/s41586-020-2014-5, for observed role of ice shelf shape itself in controlling oceanic heat delivery [Céline Heuzé, Sweden]	Not applicable. Text removed
69691	32	46	32	46	Additional reference: Asay-Davis, X.S., Jourdain, N.C., Nakayama, Y., 2017. Developments in Simulating and Parameterizing Interactions Between the Southern Ocean and the Antarctic Ice Sheet. Curr. Clim. Chang. Reports 3, 316–329. doi:10.1007/s40641-017-0071-0 [Matthew Hoffman, United States of America]	Accepted. Now included in our assessment
69693	32	46	32	46	Additional reference: Dinniman, M.S., Asay-Davis, X.S., Galton-Fenzi, B.K., Holland, P.R., Jenkins, A., Timmermann, R., 2016. Modeling ice shelf/ocean interaction in Antarctica: A review. Oceanography 29, 144–153. doi:https://doi.org/10.5670/oceanog.2016.106 [Matthew Hoffman, United States of America]	Rejected. We prefer not including reviews, where possible
85281	32	48	32	48	Is it worth saying 'The SROCC "and references therein"' to make it clear why no references are given for the statements in this paragraph? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, though we have now clarified that all assessment of the paragraph are from the SROCC
80457	32	49	32	51	Paleoceanographic evdence suggests that ACC flow through Drake Passage was consistently stronger during warm intervals of the past (both during interstadials and interglacials) (Lamy et al., 2015, PNAS). A strong, persistant ACC has been associated with a southward shift of the westerly wind belt during warmer intervals of the past [Samuel Jaccard, Switzerland]	Accepted. Very useful. We have now added a paragraph assessing paleo evidences
20551	32	49	42	51	This sentence suggests that changes in wind might matter. But is there a place in this report where one might find synthetic information about such possible changes, and the projected impact of anthropogenic activity on winds? [philippe waldteufel, France]	Accepted. We have now included projected future changes in winds in this section
20183	32	53	32	53	"increases" [philippe waldteufel, France]	Not applicable. Text removed
16377	32	53	32	53	"...and observed increaseS of..." [Julian Mak, China]	Not applicable. Text removed
82921	32	53	32	55	I wonder if there is reference(s) to support this statement. [Sebastian Gerland, Norway]	This is assessed in the SROCC. This is now clarified
20553	32	53	33	1	This is a beautiful fluid dynamics hypothesis. Are they recent references to theory, high resolution ocean modelling, observations on eddy strengths, laboratory physical simulations? The answer, unfortunately, will not be found in the single reference given here, that is to a few lines in the SROCC report. [philippe waldteufel, France]	The assessment is done in the SROCC, which we link to, and papers are cited in the SROCC. We cannot redo the assessment here or review the literature on this aspect
16379	32	55	32	55	A reference seems appropriate after "eddy saturation" or after "intensity of ACC" (page 33) unless that is to be under the umbrella of Meredith et al 2019. Sample references could be Marshall et al (2017, Geophys Res Lett) or Munday et al (2013, J. Phys. Oceanogr.) [Julian Mak, China]	Not applicable. Text removed
116847	32		32		another ex of elements which could be linked to the Pliocene cross chapter box. What about the 8.2 ka event in this context? [Valerie Masson-Delmotte, France]	Not applicable. Text removed
65929	33	1	31	2	Suggest reformatting the parentheses to: "as claimed by, for example, Rahmstorf et al. (2015) and Yang et al. (2016b)." [Kushla Munro, Australia]	Not applicable. The comment is not consistent with the text at this line and page number.
96955	33	4	33	4	What is meant by 'bottom ocean overturning rate'? [Nicole Wilke, Germany]	Accepted. Now clarified that there are two overturning cell
80459	33	4	33	13	Given the relatively short interval covered by robust observations related to AABW dynamics, it would perhaps be helpful to consider observations gleaned througt the testimony of marine sediments. Indeed, large fluctuations in AABW ventilation have been documented across the last glacial termination, with implications for air-sea gas exchange. Specifically, these observations suggest that AABW formation/ventilation was vulerable to freshwater fluxes during past interglacials (Hayes et al., 2014 (Science), Huang et al., 2020 (NComms), Turney et al., 2020 (PNAS)) and that AABW formation was strongly reduced (Skinner et al., 2010, Gottschalk et al., 2016 (NComms), Jaccard et al., 2016 (Nature)) or possibly totally curtailed (Huang et al., 2020,(NComms) during the LGM and transient cold intervals of MIS 2 & 3. AABW ventilaton increased at the onset of the last glacial termination, promoting the release of previously sequestered CO2 to the atmosphere on centennial to millennial timescales (Bauska et al., 2016 (PNAS), Jaccard et al., 2016 (Nature), Rae et al., 2018 (Nature)) concomitant with a southward shift (and intensification?) of the SH westerly wind belt (e.g. Denton et al., 2010 (Science), Jaccard et al., 2016 (Nature)) and reduced sea-ice cover (Ferrari et al., 2014 (PNAS), Stein et al., 2020 (PNAS)). [Samuel Jaccard, Switzerland]	Accepted. Very useful. We have now added a paragraph assessing paleo evidences
95935	33	7	33	13	There are some new studies that show recevery in AABW formation in recent years (Abrahamsen et al 2019 (Nat Clim Change) Castagno et al 2019 (Nat Comm), and Silvano et al., (submitted)). I think it is important to mention these new results. (The dacadal signal is not clear from observations.) [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now fully considered

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85283	33	15	33	15	It is worth also stating the Southern Ocean climate system is strongly coupled from stratosphere to deep ocean and within the ocean, ice and atmospheric components so that model errors propagate throughout the coupled model component models making it hard to determine causality, e.g. Hyder et al, 2018? It could also be worth stating that as well as the challenges for ocean models, there are also considerable Southern Ocean challenges for atmospheric models in this region, including the prevalence of super-cooled liquid cloud (in part due to low aerosol levels) which tends to be mis-represented as ice in most coupled and atmosphere-only models (see Hyder et al, 2018 and refs therein); errors in the strength and latitude of the maximum of the Southern Ocean near surface wind stresses; errors in the strom track representation; etc? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now included some aspect of atmospheric jet assessment
82923	33	16	33	16	I wonder if the placement of the confidence statement is meant to be where it is now (after importance), or if it should be moved, for example to the following comma or the end of the sentence. [Sebastian Gerland, Norway]	Rejected. We keep here to clarify that the confidence apply to the entire sentence
69695	33	21	33	21	I recommend changing "Antarctic shelves" to "Antarctic continental shelves" to avoid any ambiguity with ice shelves [Matthew Hoffman, United States of America]	Accepted. Reworded as suggested
3479	33	24	33	24	'... simulations is lower than CMIP3 and CMIP5 but consistent ...' [Petteri Uotila, Finland]	Rejected. This is not what we mean
69697	33	25	33	25	"envelops" should be "envelopes" [Matthew Hoffman, United States of America]	Accepted. Typo corrected
20555	33	25	33	26	Against 173 Sv mentioned above [philippe waldteufel, France]	Noted.
69699	33	32	33	32	"progresses" should be "progress" [Matthew Hoffman, United States of America]	Not applicable. Text removed
98695	33	32	33	32	Change first phrase to "This progress, along with a number of recent important improvements in process understanding," [Sonya Legg, United States of America]	Not applicable. Text removed
88251	33	32	33	32	Would "These advances...." be better wording? [Sharon Smith, Canada]	Not applicable. Text removed
85285	33	32	33	34	I am slgihtly surprised by the medium to high confidence, expressed even for qualitative changes, here given the myriad of known biases and process deficiencies? Is this consistent with the statements by the SROC? In particular the Southern Ocean overturning system, which is linked to heat uptake, the ACC, etc, is a relative small resdiual circulation resulting from opposing wind and eddy driven cells, and hence its means state and expected future changes are both expected to be sensitive to small fractional errors in the representation of either winds or eddies (Marshall and Speer, 2008 - https://www.nature.com/articles/ngeo1391). Most CMIP models are low resolution model which parameterise eddies so there is considerable doubt whether the existing paramterisations can adequately simulate the eddy responses to expected future wind stress changes. For example, in idealised experiments, there are considerable differences in ACC and MOC responses to wind stress changes in low resolution IPCC style ocean-models compared to the much weaker responses in higher resolution eddy permitting or eddy-resolving models. [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. In the SOD, we tried to differentiate quantitative and qualitative assessment, with greater confidence in the latter. But we have now reconsidered this approach, and stick to the SROCC approach. We however complemented their assessment from new line of evidences including paleo which can strengthen our confidence for some aspect of our assessment. Nevertheless, we responded to this comment by carefully reconsidering confidence level to lower confidence
99517	33	32	33	37	Suggest merging some of the text in the paragraph with those immediately above and below as appropriate. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
77547	33	32			Sentence uses plural for "progresses" and should be rephrased. [Emer Griffin, Ireland]	Not applicable. Text removed
69701	33	35	33	35	"model" should be "models" [Matthew Hoffman, United States of America]	Not applicable. Text removed
69703	33	35	33	35	"suffer" should be "suffers" [Matthew Hoffman, United States of America]	Not applicable. Text removed
20185	33	35	33	37	Add an s to the end of line 35, a comma after "above" in line 36, replace "is" by "are" in line 37 [philippe waldteufel, France]	Not applicable. Text removed
69705	33	36	33	36	"the number" should be "a number" [Matthew Hoffman, United States of America]	Not applicable. Text removed
3481	33	36	33	36	delete 'most' [Petteri Uotila, Finland]	Not applicable. Text removed
8991	33	36	33	37	This assessment is critical and indicates that the skills of models in the southern ocean. Yet when the report talks about the ice sheet modeling results for Antartica, the issue is dropped. I have a fundamental problem with that. My conjecture is that the Antarctic ice sheet models are not correctly constrained at the ocean boundary because the ocean models do not work close to the coastline and even over miuch of the southern ocean. By glossing over that you attribute false confidence to the model and remove the most important forcing on the Antarctic ice sheet, as document by 40 years of observations. Models show growth, data show shrinkage. We have been stuck in this situation and the descrption of the model results does not give fair exposure of their fundamental limitations. Same comments applies at the end of page 34: the models do not work, but we use some sort of alternative forced by the same models anyways ... This is bad, bad. [Eric Rignot, United States of America]	Noted. The impact of using low confidence model as a forcing for ice sheet model is assessed in Section 9.4, and has now been revised. We cross reference here to relevant sections in 9.4
3483	33	39	33	39	'Both observational and CMIP6 climate model studies ...' [Petteri Uotila, Finland]	Not applicable. Text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88253	33	39	33	39	revision suggested "...and climate modelling studies...." [Sharon Smith, Canada]	Not applicable. Text removed
80461	33	39	33	41	Paleoceanographic evdence suggests that ACC flow through Drake Passage was consistently stronger during warm intervals of the past (both during interstadials and interglacials) (Lamy et al., 2015, PNAS). A strong, persistant ACC has been associated with a southward shift of the westerly wind belt during warmer intervals of the past. In other words, under past climatic conditions ACC transport shows a strong sensitivity to SH temperatures and the strnght and position of the SH westerly wind field. [Samuel Jaccard, Switzerland]	Accepted. Very useful. We have now added a paragraph assessing paleo evidences
99519	33	39	33	41	Suggest replacing the two instances of "predicted" with "likely" or similar as appropriate. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
19287	33	39	33	50	This paragraph is somewhat difficult to follow...Considering the three statements: "high confidence that the ACC position and transport will remain insensitive..." but "CMIP5 models show a wide variety of ACC responses " and "CMIP5 models show consistently a southward shift of the northern boundary of the ACC", the first statement does not seem supported by the last two. Is the whole paragraph consistent? [Anne-Marie Treguier, France]	Accepted. We have now clarified by reporting the SROCC assessment
101869	33	42	33	42	"...the predicted increase in westerly winds will lead to increased eddying activity as suggested by theory and observation" - When changes in eddy activity are discussed, we expected Fig 9.14 (variability of SSH and its change under climate change) to be mentioned (instead, this figure is referred to only in the sea level change section) [IAPSO ECS group review, United States of America]	Not applicable. Text removed. We now report what was assessed in the SROCC assessment
65923	33	43	29	43	Suggest removing the parentheses to "(Lozier et al., 2019) (Jackson et al., submitted)" since this is a structural part of the sentence. [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
99521	33	43	33	50	Suggest including or replacing with results from CMIP6. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now did wherever published papers were available
95937	33	44	33	44	"and inter model variability"? [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
65927	33	47	30	47	Suggest correcting 'A5' to 'AR5'. [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
3485	33	47	33	47	'In contrast to CMIP6 models, CMIP5 models show ...' [Petteri Uotila, Finland]	Not applicable. Text removed
89255	33	47	33	50	Note the poleward shift of the Southern Hemisphere subtrorpical gyres might be overestimated due to the equatorward biases in the climatological westerly winds and subtropical gyres. See figure 10 of this recent paper https://doi.org/10.1175/JCLI-D-19-0478.1. However, this equatorward bias is largely reduced in CMIP6 (https://doi.org/10.1175/JCLI-D-19-1029.1) so we should also expect reduced bias in projections. [Kewei Lyu, Australia]	Accepted. The assessment of atmospheric jet is now added
132581	33	52	33	52	Be sure to check for consistency with Chapter 4, who show that future westerly wind trends depends on the emissions scenario consiodred owing to the competing effects of greenhouse-gas-induced warming and ozone recovery. Not all predict intensification. [Kyle Armour, United States of America]	Accepted. This is now clarified
36437	33	52	34	34	How should we expect the lower cell to respond to changes in the westerly winds? There is evidence (Hogg et al., JPO 2017) that a strengthening and southward shifting of the westerlies can increase polynya formation by increasing cyclonic circulationin the subpolar gyres, and intensify the abyssal overturning. [Andrew Stewart, United States of America]	Noted. Though we have limited evidence only and therefore low confidence. The reference is considered nevertheless
16381	33	55	33	55	Bishop et al (2016) and Poulsen, Jochum & Nuterman (2018, Ocena Modelling) should/could be in here for high res modelling looking at eddy compensation. There is also Mak et al. (2018, J. Phys Oceanogr.) but that could be considered covered by Munday et al (2013) already if the intention here is to only include eddy permitting/resolving calculations. [Julian Mak, China]	Not applicable, these are assessed in the SROCC, and we now report what was assessed in the SROCC assessment
98697	33	55	34	5	This sentence is very long and convoluted. [Sonya Legg, United States of America]	Accepted. Rewritten
116839	33		33		I suggest to have a coordination with ch 2, 3, 4, 5 on the issue of the southern ocean : understanding recent trends and attribution, model evaluation, confidence on models, related to sea ice, ocean ice sheet interplay, and carbon fluxes. The chapter ES could include this aspect given the low state of understanding and confidence in models and the global relevance (also including limits to the state of knowledge and confidence in projections). [Valerie Masson-Delmotte, France]	Accepted. We have now coordinated and elevated to the ES
101871	34	1	34	1	"eddies are poorly represented" - In other paragraphs the report calls on results from HighResMIP - if available, it would be interesting to include HighResMIP results here. [IAPSO ECS group review, United States of America]	Noted. Unfortunately there is no study we can rely on to assess this aspect.
99523	34	1	34	5	Suggest adding CMIP6 results and revising confidence assessment (if appropriate). [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now did wherever published papers were available

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36439	34	6	34	34	Changes in eddy kinetic energy may also translate to changes in lee wave generation (e.g. Yang et al., 2018, JPO), and thus to abyssal mixing rates in the ACC. This latter point is hinted at in the current text, but the link to lee waves is not made explicitly. [Andrew Stewart, United States of America]	Rejected. It is not clear enough from the literature what we learn about future climate change through a change in lee wave generation
36441	34	6	34	34	I am not sure that the Newsom et al. 2016 study supports the statement that "predicted changes of the lower cell intensity from surface forcing are expected to overwhelm any changes of the lower cell through interior mixing" because the model used by Newsom et al. does not permit a realistic response of the internal wave-driven mixing rates to changes in the mean flow or eddy field in the ACC. [Andrew Stewart, United States of America]	Accepted. Removed
85287	34	7	34	7	associated 'with' air-sea? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
80441	34	7	34	10	Paleoceanographic evdence suggests that AABW formation may have been severly curtailed as a result of enhanced meltwater forcing (Hayes et al., 2014 (Science), Huang et al., 2020 (NComms), Turney et al., 2020 (PNAS)) during peak interglacial conditions. Specifically, sedimentary reconstructions show a transient reduction in AABW ventilation in the Atlantic sector of the Southern Ocean during MIS5e, which is beleived to have been warmer than modern climate. Please consider adding this specific point to the discussion related to AABW stability in the future (Thomas et al., 2020 (QSR)). [Samuel Jaccard, Switzerland]	Accepted. Very useful. We have now added a paragraph assessing paleo evidences
80463	34	7	34	11	I certainly agree with this statement. However, long (multi-centennial/millennial) model runs under higher-than-preindustrial CO2 concentrations show that after 500-1000yrs, ventilation in the SO resumes (and even possibly overshoots). Specifically, enhanced convection in the Weddell and Ross seas leads to enhanced bottom water ventilation globally (Yamamoto et al., 2015 (GBC); Frölicher et al., in revision (GBC). [Samuel Jaccard, Switzerland]	Accepted. Very useful. We have now added a paragraph assessing paleo evidences
95939	34	7	34	11	"high confidence" seems a bit too much. AABW is not well reproduced in climate models (mostly forms through open ocean convection, while we know it is not the case in recent decades). Observations are now showing a recovery in AABW formation. Finally, paleo reconstructions do not support a rapid shutdown of AABW formation. I believe the AABW paragraph needs to be revisited in order to better reflect current knowldege. [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have now clarified our assessment including paleo evidences
83285	34	7	34	34	This long paragraph is quite hard to follow. Also, there is lack of clarity on the different characteristics and role of Antarctic coastal and open pcean polynyas - see Barber, D.G., and R.A. Massom 2007. The Role of Sea Ice in Arctic and Antarctic Polynyas. In W.O. Smith and D.G. Barber (Editors), Polynyas: Windows to the World's Oceans, pp. 1-54. Elsevier, Amsterdam. [Robert Massom, Australia]	Accepted. Entirely rewritten
16383	34	10	34	10	Reference surname is "de Lavergne et al. (2014)" (no capital letter for consistency since the other Casimir de Lavergne publications have no capital in "de") [Julian Mak, China]	Not applicable. Text removed
77805	34	10	34	12	I don't think there is high confidence that any given model's projection of forced dynamic SLC is correct, though, because of the large and still unexplained spread among models (Gregory et al 2016, Couldrey et al submitted). That's a systematic error. Maybe this statement is referring just to statistical error: if you have a large enough ensemble, you can quantify the forced part with confidence. I think this bit should be rephrased. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
85289	34	12	34	12	2Sv reduction 'compared to a mean of ?' ? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
101873	34	12	34	12	The report provides numbers on the projected changes of the Southern Ocean overturning cells; I suggest to add the absolute numbers of the overturning to put these changes into perspective, even if uncertain, e.g., from Talley, 2013. [IAPSO ECS group review, United States of America]	Not applicable. Text removed
69707	34	17	34	17	"climate model" -> "climate models" [Matthew Hoffman, United States of America]	Not applicable. Text removed
757	34	17	34	17	"There is some indications" should be "there are some indications" or "there is some indication" [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
759	34	17	34	17	"that climate model" should be plural "that climate models" [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
3487	34	18	34	18	Drop 'projected by'. [Petteri Uotila, Finland]	Not applicable. Text removed
69709	34	19	34	19	"climate model" -> "climate models" [Matthew Hoffman, United States of America]	Not applicable. Text removed
761	34	19	34	19	"Coarse-resolution climate model" should be plural "Coarse-resolution climate models" [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
16385	34	20	34	20	The surname is "Naveira Garabato" [Julian Mak, China]	Not applicable. Text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
95941	34	23	34	23	why should interior mixing increase? Because of eddies and ultimately winds? If so, please add few words to explain. [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
34483	34	23	34	26	The link between the last part of the sentence "further reducing the amount of well-ventilated water reaching the world ocean basins to the north (low confidence)." and what precedes is unclear: in contrast to what is written, it seems that increased mixing of bottom water with lighter layers could result in a better ventilation of bottom waters. [Claire Waelbroeck, France]	Not applicable. Text removed
89619	34	26	34	26	when introducing polynyas it would be helpful if a short definition was included (sea ice -- or lack of it -- isn't mentioned) [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
83283	34	26	34	26	Change "Open ocvean polynya, which are rarely observed" to "Open ocean sensible-keat polynbyas, which are rarely observed (Swart et al., 2018)". ADD NEW REFERENCE: Swart, S., E.C. Campbell, C.H. Heuzé, K. Johnson, J.L. Lieser, Massom, R.A., M. Mazloff, M. Meredith, P. Reid, J.-B. Sallée and S. Stammerjohn. 2018. Return of the Maud Rise Polynya: Climate litmus or sea ice anomaly? In: State of the Climate in 2017. Bulletin of the American Meteorological Society 99(8), S188–S189, doi:10.1175 /2018BAMSStateoftheClimate.1. [Robert Massom, Australia]	Not applicable. Text removed
16387	34	27	34	27	Reference surname is "de Lavergne et al. (2014)" (no capital letter for consistency since the other Casimir de Lavergne publications have no capital in "de") [Julian Mak, China]	Not applicable. Text removed
82925	34	27	34	27	I would consider writing "common" without "extremely". [Sebastian Gerland, Norway]	Not applicable. Text removed
98699	34	29	34	34	Zanowski and Hallberg 2015 (https://doi.org/10.1175/JPO-D-15-0109.1) and Zanowski et al (2017) (https://doi.org/10.1175/JPO-D-15-0109.1) are pertinent modeling studies examining the impact of Polynyas on abyssal warming, which should be referenced here. [Sonya Legg, United States of America]	Accepted. References now assessed
46627	34	31	34	34	Add discussion of papers by Zanowski, by Dufour, and by Martin/Latif on the opposite of what is written here [Céline Heuzé, Sweden]	Accepted. References now assessed
69711	34	33	34	34	"the Antarctic ice shelf" -> "Antarctic ice shelves" [Matthew Hoffman, United States of America]	Not applicable. Text removed
112483	34	33	34	34	Khazendar et al. 2012 "Observed thinning of Totten Glacier is linked to coastal polynya variability" doi: 10.1038/ncomms3857 [Pedro LLanillo del Rio, Germany]	Rejected. We discuss here open ocean polynya not coastal polynya. This is now made clearer.
82927	34	33	34	34	From what I can see adressed Naughten et al. (2019) specifically the Filchner-Ronne Ice shelf (one of the Antarctic ice shelves), I suggest to specify this here. [Sebastian Gerland, Norway]	Not applicable. Text removed
763	34	33	34	34	"polynya can have affect rate of melt..." should be "can affect the" or "can have an effect on the" [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
95943	34	34	34	34	"Antarctic ice shelves' [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
69713	34	36	34	36	"temperature of Antarctic shelves" -> "ocean temperature in the cavities beneath Antarctic ice shelves" [Matthew Hoffman, United States of America]	Accepted. We now clarified that we mean Antarctic continental shelf
155	34	36	34	36	"projecting changes in the temperature of Antarctic ice shelves" - surface temperature? Basal temperature? Mean temperature? Meaning is unclear here. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now clarified that we mean Antarctic continental shelf
85291	34	36	34	46	Is it worth stating something along the lines that 'Modelling the Antarctic shelf T-S characteristics is extremely challenging, with serious implicatons for confidence in ice-shelf model projections? For example, there are considerable biases in the near coastal and offshore wind stresses, eddies, coastal and slope currents, larger scale Southern Ocean T and S characteristics, sea-ice characteristics, etc, all of which significantly adversely impact on Antarctic shelf T-S characteristics.' For example, large temperature biases near the West Antarctic Ice Shelves in the Hadley Centre N216-1/4 HadGEM3 GC3.1 model currently make it impossible to even use our new active ice shelf model to make projections. [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now assessed the importance of bias in the Antarctic continental shelf. Note though that we need to base our assessment on published literature
36435	34	36	34	46	In this paragraph I was surprised not to see a discussion of future changes in the zonal winds around Antarctica, as the role of changing winds on CDW access to the continental shelf is discussed earlier on p. 27 (see above comments). The projected future southward shift and strengthening of the westerlies may be expected to increase upwelling of CDW onto the Antarctic continental shelf. However, despite enhancement of the SAM in the past few decades, the easterly winds around the Antarctic coastline have not weakened (Hazel and Stewart, 2019, J. Climate), I am unsure as to how much confidence to place in such a projection. [Andrew Stewart, United States of America]	Accepted. We added that assessment
83287	34	36	34	46	What is meant here by Antarctic shelves/shelf? [Robert Massom, Australia]	Accepted. We now clarified that we mean Antarctic continental shelf
46495	34	36	34	46	As written, this section is a little confusing because with the use of "shelves" alone it is not clear if you are referring to ice shelves or continental shelves (I think the former, but it would be good to be explicit). [Stephen Price, United States of America]	Accepted. We now clarified that we mean Antarctic continental shelf

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
95945	34	36	34	46	It might be good to mention iceberg calving given its impact, for example, on AABW formation (Snow et al., 2018, grl) [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Calving is now mentioned
46537	34	36	34	46	In this discussion, it seems relevant to mention that some Earth system models are starting to account for these processes explicitly (another option relative to pursuing these stuides in only regional scale models, as suggested on lines 44-45). This includes, for example, the work of Jeong et al. (2020; submitted 2019), who explore the impacts of sub-ice shelf melting in a fully coupled Earth system model (E3SM) that explictily resolves ocean circulation (and heat and freshwater exchange) within Antarctic ice shelf cavities (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). [Stephen Price, United States of America]	Rejected. We only discuss CMIP6 here. Jeong is cited elsewhere where relevant
39845	34	36			"temperature of Antarctic ice shelves" is ambiguous. Do you mean ice shelf cavity (i.e. temperature of the water)? [TSU WGI, France]	Accepted. We now clarified that we mean Antarctic continental shelf
61497	34	40	34	40	Also Marsh et al., 2016, GRL, provides the only in ground-based evidence for basal channels. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Reference considered
61499	34	40	34	40	In addition to basal channels and the role of eddies and tides, small scae processes occuring in the ice-ocean boundary layer and high values of melt rates occuring in narrow grounding zones are also reasons for the lack of adequate treatment of ice shelves in mdels [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted.
46629	34	44	34	44	Here is also a good place to add reference to Wåhlin et al. (2020), doi:10.1038/s41586-020-2014-5, for observed role of ice shelf shape itself in controlling oceanic heat delivery [Céline Heuzé, Sweden]	Rejected. Not relevant for our assessment here
69715	34	44	34	44	"the Antarctic shelf" -> "Antarctic ice shelves" [Matthew Hoffman, United States of America]	Not applicable. Text removed
46493	34	44	34	45	It is not entirely true that future projections of oceanographic changes on Antarctic shelves requires regional models. Some research groups (e.g., E3SM) are focussing on high and/or variable (enhanced regional) resolution simulations with a focus on S. Ocean processes and ice shelf / ocean interactions. [Stephen Price, United States of America]	Accepted. Rewritten
78305	34	49	35	27	We support the focus on tropical oceans. Overall, we note 2019 was a neutral year for Singapore for the El Niño Southern Oscillation (ENSO). However, Singapore also saw the development of one of the strongest, positive Indian Ocean Dipole (IOD) events since the 1960s. The development of this IOD event since the middle of 2019 contributed significantly to the below-average rainfall and higher temperatures observed in the rest of 2019. [Leonie Lee, Singapore]	Modes of variability are not treated in our chapter. We have now clarified and indicated reference where to find the assessment in the AR6
40781	34	49	39	13	sections 9.2.3.3-9.2.3.5: improvements since latest report not necessarily clear [TSU WGI, France]	Accepted. We have now clarified by reporting the SROCC assessment and streamlined the text
107453	34	49			Section 9.2.3 is about regional ocean circulation, but Section 9.2.3.3 on tropical oceans has a focus on temperature rather than circulation, especially in the first paragraph. What is the link to circulation? Page 35, line 2 references changes in oceanic circulation from warming tropics but does not explain [Jennifer Walker, United States of America]	Accepted. Entirely rewritten
132583	34	53	34	54	Which atmospheric circulation is this referrring to, and over which time period? I think it would be more accurate to say "These changes are concurrent with a weakened Walker Circulation (trade winds), partcularly since the 1970s". [Kyle Armour, United States of America]	Not applicable. Text removed
45283	35	10	35	12	Additional reference, Lee et al. (2015): DOI: 10.1038/NGEO2438 [Anson Cheung, United States of America]	Not applicable. Text removed
86433	35	10	35	13	Better to mention that strengthened ITF transport have acted to cool the Pacific and warm the southern Indian Ocean (Zhang et al. 2018,Dong and McPhaden,2016,Lee et al.2015) [Swapna Panickal, India]	This is treated in 9.2.1 has now clarified here
33431	35	10			Add space between parenthesis: "…(Cai et al., 2019)(very high confidence)…". [Guiomar Rotllant, Spain]	Not applicable. Text removed
39193	35	12	35	15	What is the uncertainty language for the part of the statement which says most models project a decline on the centennial scale? What could be the reason for this change in the trends? [Lourdes Tibig, Philippines]	Not applicable. Text removed
101875	35	13	35	15	"...total water transport from the Pacific to the Indian Ocean via the Indonesian Throughflow has increased (Collins et al., 2019), although most models project a decline on the centennial timescale (Figure 9.11) as winds change" - This statement should be more specific about the nature of the wind change that causes the centennial decline of the ITF volume transport. [IAPSO ECS group review, United States of America]	This is now treated in 9.2.3.4 and clarified
101877	35	13	35	15	What is the projected time period for the ITF volume transport decline? [IAPSO ECS group review, United States of America]	This is now treated in 9.2.3.4 and clarified

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86435	35	14	35	15	Sen Gupta et al (2016) is still under review or published? [Swapna Panickal, India]	Not applicable. Text removed
129431	35	15	35	15	Gupta et al., 2016 (submitted) should not be stated as "submitted". The same issue is widespread. It appears the report chapter is assembled from various previous reviews/synthesis work. This is also reflected in the use of "We" in many places. [Trigg Talley, United States of America]	Not applicable. Text removed
99525	35	15	35	15	Should the reference be 2016 or submitted or both? [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
129433	35	15			The Gupta et al. (2016, submitted) paper is referenced several times, but can't find evidence that it has been published. Several important statements in the chapter are supported by this seemingly unpublished study, so this needs to be checked. [Trigg Talley, United States of America]	Not applicable. Text removed
129435	35	17	35	26	[CONFIDENCE] This is a typical example of the problem with qualitative confidence designations. The passage states that separation of the multi-decadal variability from anthropogenic warming is difficult. If so, how can any of the conclusions or inferences be assigned high confidence in preceding sections? [Trigg Talley, United States of America]	Not applicable. Text removed
45285	35	21	35	23	McGregor et al. (2018) also highlighted the effects of mean state bias on simulating long term variability in the tropical oceans: https://doi.org/10.1038/s41558-018-0163-4 [Anson Cheung, United States of America]	Not applicable. Text removed
1737	35	21	35	26	More detail is needed on how CMIP5 model biases impact modeling projections. [Michael Kennish, United States of America]	Not applicable. Text removed
65933	35	22	31	23	Suggest correcting "(Figure 9.10 bottom right panel)" to e.g. Figure 9.10b rather than "bottom left panel". [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
67569	35	24	35	26	Add the reference Tian and Dong (2020) for the cold-tongue and double-ITCZ bias in models. Tian, B., & Dong, X. (2020), The Double-ITCZ Bias in CMIP3, CMIP5, and CMIP6 Models Based on Annual Mean Precipitation, Geophys. Res. Lett., 47(8), e2020GL087232, https://doi.org/10.1029/2020gl087232 [Baijun Tian, United States of America]	Not applicable. Text removed
54359	35	25	35	26	The double ITCZ statement is unclear to me. Are you implying SST biases are causing (at least in part) the double ITCZ bias? This seems odd as the double itcz has been observed in AMIP style simulations for decades. [Luke Van Roekel, United States of America]	Not applicable. Text removed
98837	35	28	35	28	The phrase "Despite the biases" does not make sense here and should be removed. It is a simple statement of fact that most models project these changes to ENSO. Perhaps it is despite the biases noted in the previous paragraph, or maybe it is because of them. Whether one chooses to pay attention to these projections despite the biases is a different matter, but that is a personal judgment call. [Robert Hallberg, United States of America]	Not applicable. Text removed
20991	35	28	35	28	Delete El Niño and La Niña, Replace with ENSO [Ladislaus Chang'a, United Republic of Tanzania]	Not applicable. Text removed
45287	35	28	35	29	Cai et al. (2018) highlighted increase in extreme EP El Nino in particular: https://doi.org/10.1038/s41586-018-0776-9 [Anson Cheung, United States of America]	Not applicable. Text removed
132585	35	28	35	29	Chapter 4 (page 34) assess low condience in changes in ENSO variability, and state that the amplitude of ENSO does not robustly change in CMIP simulations. Perhaps by "frequency of ENSO events" you mean the periodicity of ENSO will change, rather than the amplitude? In any case, be sure your projected ENSO assessment is consistent with that in Chapter 4. [Kyle Armour, United States of America]	Not applicable. Text removed
1739	35	28	35	31	How accurate are the CMIP5 projections if biases exist in the models? This should be discussed as well. [Michael Kennish, United States of America]	Not applicable. Text removed
61467	35	29	35	39	much enhanced to enhanced [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
100807	35	30	35	30	For the IOD the reference to technical Annex VI.5 should be included. [Corti Susanna, Italy]	Accepted. Cross-reference added
100809	35	31	35	31	Slight inconsistency here with the assessment about IOD changes in frequency with global warming. Here it is reported a low confidence on an increase frequency of positive events under RCP4.5 and RCP8.5 scenarios. On the other hand CH4 (page 68 line 26) reports no robust changes in IOD frequency and only a slight increase of frequency for scenario RCP4.5. [Corti Susanna, Italy]	Not applicable. Text removed
61439	35	34	35	37	This information is an exact repeat of the information in lines 12-15 on this page (page 35). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed
82929	35	37	35	37	I wonder how "observations in the past two decades have been variable" is meant here, whether this should express that the observations are not in agreement with modelling results,which are addressed in the first part of the sentence. I suggest to clarify/reword. [Sebastian Gerland, Norway]	Not applicable. Text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129437	35	38	35	53	There are a number of evaluations that should be included: Gehne, M., T. M. Hamill, G. N. Kiladis and K. E. Trenberth, 2016: Comparison of global precipitation estimates across a range of temporal and spatial scales. J. Climate, 29, 7773-7795, doi:10.1175/JCLI-D-15-0618. Trenberth, K. E., Y. Zhang and M. Gehne, 2017: Intermittency in precipitation: duration, frequency, intensity, and amounts using hourly data. J. Hydrometeor. 18, 1393-1412, Doi: 10.1175/JHM-D-16-0263. Covey, C., C. Doutriaux, P. J. Gleckler, K. E. Taylor, K. E. Trenberth, Y. Zhang, 2018: High frequency intermittency in observed and model-simulated precipitation. Geophys. Res. Lett., 45, 12,514-12,522, doi:10.1002/2018GL078926. [Trigg Talley, United States of America]	Not applicable - These comments do not seem to be related to Chapter 9.
39919	35	40	35	40	A glossary definition for 'western boundary currents' would be welcome, as you see fit. [TSU WGI, France]	Accepted, the term "WBCs" is added in Glossary.
61193	35	40	35	48	Although it is stated that "transports and water masses are sensitive to decadal variability and secular changes in the atmospheric circulation", Section 9.2.3.4 on Western Boundary Currents and Gyres has very little to say on observed variability and trends in gyre transports in the Pacific. Such trends have, in fact, been documented. In particular, Hristova et al. (2019, Journal of Geophysical Research, https://doi.org/10.1029/2019JC015231) showed that there has been an increasing trend in the transport of the Alaska Gyre in the northeast Pacific over the 25 year period 1993-2017. In addition, variability of Alaska Gyre has been linked to the Pacific Decadal Oscillation (see Cummins & Masson, 2018, Prog. Oceanogr. https://doi.org/10.1016/j.pocean.2018.09.014 and Hristova et al., 2019, https://doi.org/10.1029/2019JC015231). [Patrick Cummins, Canada]	Accepted. Text has been revised accordingly, and references included.
101879	35	42	35	43	Fig 9.11 is not easy to read/interpret for a bit more general audience. We suggest to include information in the caption to make its purpose more clear; when referring to it, it would help the reader if the report said clearly what the reader is supposed to focus on (for instance here in this case for the WBC); again it would help also to refer to specific panels. [IAPSO ECS group review, United States of America]	Accepted, and Figure 9.11 was re-plotted.
7875	35	44	35	44	40N is too far south for subpolar in the North Atlantic [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
100195	35	46	35	48	Re-written: Both gyres and overturning cells are sensitive to decadal variability and secular changes in atmospheric circulation which can modulate the transport of heat, freshwater and carbon in mode and intermediate waters (Section 9.2.3). [Carlye Peterson, United States of America]	Accepted. Rewritten
61191	35	51	35	52	"Systematic changes in wind stress curl over both hemispheres (Figure 9.4), in particular, the poleward shift of the zero-curl line the North Pacific …" Figure 9.4 shows only changes in wind stress magnitude, not wind stress curl. It is not possible to discern shifts in the zero curl line from the results shown in Figure 9.4. [Patrick Cummins, Canada]	Accepted, text revised.
100197	35	51	35	55	Re-written: Systematic changes in wind stress curl over both hemispheres (Figure 9.4), in particular, the poleward shift of the zero-curl line the North Pacific, the North Atlantic and the South Atlantic Oceans, may strengthen subtropical gyres or shift them poleward (Figure 9.11). Such poleward shifts in subtropical gyres could accelerate warming of the subtropical WBCs (e.g., Cai, 2006; Roemmich et al., 2007; Chen and Wu, 2012; Wu et al., 2012; Zhai et al., 2014; Hu et al., 2015a; Yang et al., 2016a). [Carlye Peterson, United States of America]	Accepted, text revised.
89259	35	53	35	53	Note that in the North Pacific the subtropical gyre and Kuroshio is projected to intensify in the upper ocean but weaken below the main thermocline. See these two studies https://link.springer.com/article/10.1007/s00382-013-1902-x and https://doi.org/10.1175/JCLI-D-18-0895.1 [Kewei Lyu, Australia]	Accepted, text revised.
7877	35	53	35	53	The figure 9.11 does not appear to show this. See also later comment [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and Figure 9.11 was re-plotted.
98839	35	54	36	1	There is no reason to have an undifferentiated cluster of 7 citations to make a point, especially when many predate AR5. Adding so many citatations detracts from the readability of the chapter and unnecessarily lengthens it. When the authors are pressed to shorten the report, eliminating excess citations (3 citations should usually be more than sufficient) can be a good way to abreviate the report without losing scientific value. Please remember that this is an assessment, not a review paper, and that the primary audience is the policy-makers of the world and not your fellow scientists hoping to be cited. [Robert Hallberg, United States of America]	Accepted, text revised.
116849	35		35		For extreme El Nino, please refer to SROCC and also chapter 11. [Valerie Masson-Delmotte, France]	Not applicable. Text removed

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100199	36	1	36	9	Re-written: However, observations from the past 20 years do not support such enhancement or poleward shift of WBCs including the Kuroshio (Wang et al., 2016d; Wang and Wu, 2018), Gulf Stream (McCarthy et al., 2018; Dong et al., 2019), Agulhas Current (Beal and Elipot, 2016; Elipot and Beal, 2018), and East Australian Current (Sloyan and O'Kane, 2015). Instead, the strength and position of WBCs vary on multidecadal timescales (McCarthy et al., 2018) because observed changes combine anthropogenic trends and decadal-interdecadal variability (Feng et al., 2010; Zhang and Church, 2012). Intensifying winds increase the boundary current mesoscale eddy field, rather than their mean flow (medium confidence) particularly in the Kuroshio, East Australian, and Agulhas Currents (Cetina-Heredia et al., 2014; Yan and Sun, 2015; Beal and Elipot, 2016). [Carlye Peterson, United States of America]	Accepted, text revised.
61441	36	5	36	5	You note that the observed changes at decadal time scales combine anthropogenic and natural variability. Is there any indication that these changes are a signal of anything more than natural variablity, or are you speculating? Also in the sentence previous to this you say that the WBCs have not showed the response that might be expected based on models with secular forcing changes. Please clarify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, text revised.
26363	36	6	37	6	area by 2040. the ocean area by 2040. (medium confidence) ->check ponctuation [María Santolaria-Otín, France]	Noted. Editorial.
88619	36	9	36	11	The sentence has double interpretation (past and future). Driven by an expansion of the Hadley cell and poleward shift and enhancement of extratropical wind stress, global WBCs estimated from simple ocean data assimilation (SODA) tend to intensify and stronger warming tends to occur in WBC regions during the past century (Wu et al., 2012). [Rosemary Vieira, Brazil]	Accepted, text revised.
100201	36	10	36	12	Re-written: Observed changes in gyre circulation are likely dominated by interannual and decadal modes of variability globally (Chen and Wu, 2012; Qiu and Chen, 2012; Melzer and Subrahmanyam, 2017; McCarthy et al., 2018). [Carlye Peterson, United States of America]	Accepted. Rewritten
33433	36	12			Ass a space at the begging of the sentence: "…al., 2018).The North…". [Guiomar Rotllant, Spain]	Accepted, text revised.
93563	36	14	36	15	That's not the only positive feedback that has been found for the Subpolar Gyre. North East Atlantic blocking patterns have been repeatedly proposed as a mechanism by which you could lock the atmosphere-ocean into a strong versus weaker gyre strength and also with forcing of freshwater input into the North Atlantic (see https://doi.org/10.1007/s00382-016-3106-7 (model study and other work by E. Moreno-Chamarro) https://doi.org/10.1038/ngeo2094 (data-model study). [Paola Moffa-Sanchez, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text revised.
7879	36	15	36	16	Over what period is the Beaufort gyre increase observed? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text revised.
82931	36	16	36	16	In this context, I suggest considering results from a newer study by Armitage et al. (2020, Nature comm., https://doi.org/10.1038/s41467-020-14449-z). [Sebastian Gerland, Norway]	Accepted, text revised.
100203	36	17	36	17	Re-written: reduced ice thickness, changes in ice pack morphology, [Carlye Peterson, United States of America]	Accepted, text revised.
34485	36	17	36	17	"changes morphology of ice pack" should be replaced by "changed morphology of ice pack" or "changes in the morphology of ice pack". [Claire Waelbroeck, France]	Accepted, text revised.
16389	36	22	36	22	"WBCs" already defined in the previous paragraph so may not need "western boundary currents" again here [Julian Mak, China]	Accepted, text revised.
85293	36	22	36	22	Is it worth stating something along the lines of 'All CMIP5 models and nearly all CMIP6 models are low resolution models with parameterisaed eddies which do not adequately represent WBC or frontal currents, leading to substantial biases particuarly in their extenstion regions, e.g. SST biases in the Gulf stream northward turn region which can exceed 5 degrees.'? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text revised.
1741	36	22	36	24	Include the high resolution models that provide the best representation of western boundary currents and why. [Michael Kennish, United States of America]	Noted. References are now consider where relevant. For instance we now assess on that topic (among others), Sasaki et al., 2004; Chassignet and Marshall, 2008; Delworth et al., 2012; Yu et al., 2012; Small et al., 2014a; Haarsma et al., 2016; Chassignet et al., 2017, 2020; Hewitt et al., 2020
99527	36	22	36	43	Please clarify the policy relevance of the assessment here. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted,the policy relevance was clarifed here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54361	36	23	36	24	again Caldwell et al (2019) and Petersen et al (2019) would seem to be appropriate here too [Luke Van Roekel, United States of America]	Accepted. Text has been revised accordingly, and references included.
34487	36	26	36	26	Fig. 9.3: the model bias does not seem larger for CMIP5 than CMIP6 models. Fig. 9.11: CMIP5 models are not shown. [Claire Waelbroeck, France]	Both Figure 9.3 and 9.11 were re-plotted with new model data.
101881	36	26	36	28	"Recent ..."; this is partly repeating content of the first sentence of the paragraph and could be shortened. [IAPSO ECS group review, United States of America]	Accepted, text revised.
96957	36	26	36	33	Most cited papers here have been published before AR5, please shorten the text, refer to AR5 and only keep the new findings. [Nicole Wilke, Germany]	Accepted, old references removed and new references included.
100205	36	28	36	28	In general, for this section, try to be consistent about how these WBCs are named, i.e., whether they need "Current" in their name, because deciding not to use Current in the name will slightly reduce the length. For example, in this line "the Kuroshio Current" has also been referred to as "the Kuroshio". [Carlye Peterson, United States of America]	Accepted, text revised.
87997	36	28		31	also seen in Zhang, X., Church, J.A., Monselesan, D. and McInnes, K.L. 2017: High Resolution Sea Level Projections for Australian Coasts in the 21st Century. Geophys. Res. Lett., 44, doi:10.1002/2017GL074176 for the EAC [Kathleen McInnes, Australia]	Accepted. Text has been revised accordingly, and references included.
799	36	33	36	34	I would add the references to the underestimation by models and satellite-derived models. These two regarding western boundariies:https://doi.org/10.1016/j.rse.2018.03.040 and 10.1016/j.rse.2018.10.029 [Michael Hart-Davis, Germany]	Accepted. References included.
65935	36	34	31	46	Suggest breaking this sentence into two sentences. [Kushla Munro, Australia]	Accepted, text revised.
98701	36	36	36	36	Another reason for overly energetic mesoscale eddies in these models is the need to represent energy transfer to lee-waves (Nikurashin and Ferrari, 2011 https://doi.org/10.1029/2011GL046576) and the associated lee-wave drag (Trossman et al, 2013 https://doi.org/10.1016/j.ocemod.2013.08.006) [Sonya Legg, United States of America]	Accepted. References included.
19291	36	37	36	38	"High-resolution coupled ocean-atmosphere models have substantially weaker eddies than high-resolution ocean-only models": such a statement cannot be made in general, without some explanation or further details. A protocol for forcing ocean-only models has been proposed for CORE (Large and Yeager 2004, Griffies et al, Ocean Modelling 2009, formula in appendix C1) and this protocol advocated a wind stress calculation based on the difference (wind-current). When such a bulk formula is used in an ocean only model, it has weaker eddies than a corresponding coupled model (contradicting chapter 9's statement). For example, in Renault et al 2016, EXP3 (fully coupled) has more eddy kinetic energy than EXP2 (ocean-only forced simulation using a bulk formula including the surface currents). The reverse is true only when the wind stress calculation ignores the current speed (stress function of wind only, not of the wind-current difference). [Anne-Marie Treguier, France]	Accepted. Text revised.
101885	36	38	36	43	This sentence is lengthy and hard to read. We recommend rephrasing it for clarity. It should be separated into two points 1) "As eddies play a role in determining the strength of the gyre ….coarser resolution models (low confidence) and 2) how there are some aspects of the fundamental balances that are unaffected by resolution. It would be good to expand on 2) about why coarser resolution models are still utilized due to the computational costs of running higher resolution models. [IAPSO ECS group review, United States of America]	Accepted. Text revised.
101883	36	41	36	41	"Although some aspects…": vague, we suggest to delete or be more specific. [IAPSO ECS group review, United States of America]	Accepted. Text revised.
20187	36	46	37	1	One wonders about the colour code for subplot (e), which provides for negative values of surface speed [philippe waldteufel, France]	Accepted, and Figure 9.11 was re-plotted.
27611	36	49	36	49	About "(d)": It's not a question of the graph (d) but of the graph (e). [Eric Brun, France]	Accepted, and Figure 9.11 was re-plotted.
100207	37	4	37	6	Re-written: Subtropical gyres generally strengthen, and all expand southward in the Southern Hemisphere (Sen Gupta et al., 2009b, submitted; Hu et al., 2015a) under RCP4.5 and RCP8.5 scenarios as well as in CMIP6 simulations under SSP5-8.5 scenario (Figure 9.11). [Carlye Peterson, United States of America]	Accepted. Rewritten
7881	37	4	37	6	From Fig 9.11 it looks like: Indian STG weakens; N Pac STG stays same/weakens; S Pac STG stays same; N Atl STG weakens; S Atl STG shifts north/strengthens. This doesn't agree with what is written [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure 9.11 was re-plotted with new model data, and text was also rewritten accordingly.
765	37	4	37	6	“which are also observed” should this be a plural? Seems clunky/ confusing, it is referring to two effects but restructuring could make this clearer [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99529	37	4	37	17	Please clarify the policy relevance of the assessment here. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted,the policy relevance was clarifed here.
89261	37	5	37	5	Also seen in CMIP3 multi-model projections https://link.springer.com/article/10.1007/s00382-013-1902-x [Kewei Lyu, Australia]	Accepted. References included.
100209	37	7	37	7	Re-written: anthropogenic CO2 emissions [Carlye Peterson, United States of America]	Accepted, text revised.
100211	37	9	37	11	Re-written: Driven by an expansion and poleward shift of the Hadley cell and enhancement of extratropical wind stress, global WBCs estimated from the simple ocean data assimilation (SODA) tend to intensify and (strongly) warm over the past century (Wu et al., 2012). [Carlye Peterson, United States of America]	Accepted, text revised.
88621	37	12	37	12	Which is the correct: Yang et al., 2016a or Yang et al., 2016b?. [Rosemary Vieira, Brazil]	Accepted, duplicated references deleted.
34489	37	12	37	17	Lines 12-16 of p. 37 is a repetition of lines 36-43 of p. 36. I suggest to drop lines 12-17 from p. 37 and add 16-17 of p. 37 after lines 36-43 of p. 36 on the same topic. [Claire Waelbroeck, France]	Accepted, text revised.
89263	37	16	37	16	At least the poleward intensification of the Southern Hemisphere subtropical gyres is consistent in observations and future projections. [Kewei Lyu, Australia]	Accepted, text revised.
54505	37	16	37	17	Thus, these projections should be supported by direct observations (which at present is not the case) and higher resolution models. [PLACEHOLDER FOR CMIP6 RESULTS] [Maria del Pilar Bueno Rubial, Argentina]	Accepted, text revised.
40743	37	20			section 9.2.3.5 : what's the point of bringing 9.2.3.5.1 and 9.2.3.5.2 under the same section? Aren't both points quite different anyway and therefore could belong to separate sections? [TSU WGI, France]	Accepted, sections split
39917	37	22	37	22	A glossary definition for 'eastern boundary upwelling systems' would be welcome, as you see fit. [TSU WGI, France]	Noted
98841	37	22	38	26	There was a box on Eastern Boundary Upwelling systems (Box 5.3) in the 2019 IPCC SROCC that should be cited here, and some common material could be omitted to help shorten this chapter. [Robert Hallberg, United States of America]	Accepted. Link to SROCC is now clarified
40745	37	22			section 9.2.3.5.1 This section currently reads more as a review of the literature rather than an assessment of our current understanding of the literature. [TSU WGI, France]	Accepted. Text streamlined
100129	37	23	38	16	Note that Jacox et al (2018) describes the development of a refined upwelling index that provides estimates of vertical nitrate fluxes at the base of the mixed layer, using vertical velocities from output of a data-assimilative regional ocean model. This is a more direct metric of upwelling than standard upwelling indices, and thus more biologically relevant. Also see Bakun et al (2015) for a review of potential climate-change impacts on the dynamics and ecosystem responses of EBUS. Bakun, A., B.A. Black, S.J. Bograd, M. Garcia-Reyes, A.J. Miller, R.R. Rykaczewski, W.J. Sydeman, 2015. Anticipated effects of climate change on coastal upwelling ecosystems. Current Climate Change Reports, doi:10.1007/s40641-015-0008-4. [Steven Bograd, United States of America]	Noted. Reference considered
101887	37	25	37	25	"Coastal upwelling depends…": Toggweiler et al, 2019a, Upwelling in the Ocean Basins North of the ACC: 2. How Cool Subantarctic Water Reaches the Surface in the Tropics, https://doi.org/10.1029/2018JC014795, may be a reference to be included here [IAPSO ECS group review, United States of America]	Noted
55085	37	30	9	30	Add additional citation after "Ndoye et al., 2017": Saldias and Allen 2020 {clear quanitative description of impact of canyons on upwelling character} Saldias and Allen 2020 The influence of a submarine canyon on the circulation and cross-shore exchanges around an upwelling front. Journal of Physical Oceanagraphy https://journals.ametsoc.org/doi/abs/10.1175/JPO-D-19-0130.1 [Nancy Hamzawi, Canada]	Noted. Reference considered but they are not directly relevant to the assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55087	37	30	37	31	Suggest add: "; remote isopycnal heave due to coastally trapped waves (Engida et al. 2016)." to "...interaction of flow with bathymetry to modulate upwelling (...Ndoye et al., 2017)." {in some stretches of eastern boundary currents,I32 remotely forced upwelling is critical to regional scale responses - and may cause significant changes in character of upwelling under future climate wind regimes} Engida, Z., A. Monahan, D. Ianson, and R. E. Thomson (2016), Remote forcing of subsurface currents and temperatures near the northern limit of the California current system, J. Geophys. Res. Oceans, 121, 7244–7262, doi:10.1002/2016JC011880. [Nancy Hamzawi, Canada]	Not applicable. Text removed
100125	37	31	37	32	Useful to note Jacox et al (2015), which uses a data-assimilative regional ocean model to evaluate the relative contributions of wind, surface heat flux, and basin-scale (remote) climate forcing to upwelled nitrate supply in the California Current. Differentiating these processes is important for differentiating transient variability from secular (climate) trends. Jacox, M., S.J. Bograd, E.L. Hazen, J. Fiechter, 2015. Sensitivity of the California Current nutrient supply to wind, heat, and remote ocean forcing. Geophysical Research Letters, 42(14), 5950-5957, doi:10.1002/2015GL065147. [Steven Bograd, United States of America]	Noted
20557	37	34	37	36	Very elusive sentence [philippe waldteufel, France]	Noted. Text revised
85295	37	35	37	35	sufficient horizontal 'and vertical' resolution? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text removed
101889	37	35	37	36	"...involve fine spatial scales which can only be properly represented with sufficient horizontal resolution in the ocean and the atmosphere" - Suggestion to put a number for what is defined as "fine spatial scales", for example <4km resolution. [IAPSO ECS group review, United States of America]	Not applicable. Text removed
100127	37	38	37	52	See also Sydeman et al (2014) for a global meta-analysis on trends in wind-driven coastal upwelling in EBUS. In this test of the, This meta-analysis found that evidence for the Bakun (1990) hypothesis of climate-driven increases in coastal upwelling was equivocal, being dedpendent on EBUS, latitude, time period of historical data, and data source from observations vs models. Studies from three of the EBUS found some evidence for an increasing trend in coastal upwelling, for at least some part of the system. Rykaczewski et al (2015) provided a more thorough test of the Bakun hypothesis from an ensemble of climate models, and found the latitudinal variation in projected upwelling trends, as mentioned here, with increases in the poleward portions of the EBUS and no change or decreases at the lower latitudes. Sydeman, W.J., M. Garcia-Reyes, D. Schoeman, R. Rykaczewski, S.A. Thompson, B.A. Black, and S.J. Bograd, 2014. Climate change and wind intensification in coastal upwelling ecosystems. Science, 345(6192), doi:10.1126/science.1251635. [Steven Bograd, United States of America]	Noted. Reference considered
88623	37	46	37	46	Which is the correct: Wang et al., 2015 (a, b or c) ? [Rosemary Vieira, Brazil]	Reference clarified
22577	37	46	37	47	I'm not sure this is really needed, at least here. The confidence statement should come after and not before the associated evidence has been laid out [Peter Thorne, Ireland]	Accepted. Text revised.
77549	37	49	37	49	The term "heat low" is not clear and should be explained or text altered. [Emer Griffin, Ireland]	Accepted. Text revised.
116851	37		37		How does the assessment related to upwelling systems differ from SROCC? Explicit statements help. [Valerie Masson-Delmotte, France]	Accepted. Text clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26125	37		38		The comment concerning the trends of the East Boundary Upwelling Systems (EBUS). That is true that there are some contradictions between different assessments of the EBUS trends (see chapter 9, pp.9-37, 9-38). However, the majority of recent published results show that the tendency of EBUS intensification prevails at least for the last ~30 years. This tendency is not significant everywhere within the EBUS, but the absolute majority of the significant trends are positive (e.g., A. B. Polonskii and A. N. Serebrennikov. Intensification of Eastern Boundary Upwelling Systems in the Atlantic and Pacific Oceans. Russian Meteorology and Hydrology, 2020, v.45, Is.5). In general, the intensification of the alongshore wind is the principal cause of that. However, the Ekman pumping tendencies play an important role in such trends, too. This is especially true for the certain upwelling regions. The published results show (see, e.g., M. Garcia-Reyes, W. J. Sydeman, D. S. Schoeman, R. R. Rykaczewski, B. A. Black, A. J. Smite, and S. J. Bograd, "Under Pressure: Climate Change, Upwelling, and Eastern Boundary Upwelling Ecosystems," Front. Mar. Sci., 2, 2015; T. Lamont, M. Garcia-Reyes, S. J. Bograd, C. D. van der Lingen, and W. J. Sydeman, "Upwelling Indices for Comparative Ecosystem Studies: Variability in the Benguela Upwelling System," J. Mar. Systems, 188, 2018; A. B. Polonskii and A. N. Serebrennikov. 2020. "Intensification of Eastern Boundary Upwelling Systems in the Atlantic and Pacific Oceans". Russian Meteorology and Hydrology, v.45, Is.5 and many others) that uncertainty in the estimation of trends in the EBUS intensity is caused by two major reasons. Firstly, there are no long-term hydrometeorological data with required resolution for the upwelling regions of the World Ocean, which are sufficient to distinguish the statistically significant trends caused just by the global warming itself. The second problem stems from the low quality of the spatially-inhomogeneous wind field over the EBUS and importance of the change of the wind field vorticity in the vicinity of the some EBUS regions for the total EBUS trends. [Alexander Polonsky, Russian Federation]	Noted. We have clarified those limitations in the EBUS sections
101891	38	2	38	2	"These patterns are most apparent in summer." - Please define which hemisphere summer. [IAPSO ECS group review, United States of America]	Accepted. Text revised.
61443	38	16	38	16	I doubt that anything can be said with "high confidence" about upwelling velocities. While increases in upper ocean stratification would definitely change the modal structure and maybe confine velocities to more near the surface, the conclusion can not be drawn based on present day evidence with high confidence that this would lead to a reduced upwelling. This is an area of active research. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. There is high confidence in the theoretical arguments, as expressed in the referenced paper
85297	38	16	38	16	Is it worth discussing historical and expected future dissolved oxygen changes in some upwelling systems or is this discussed elsewhere? Also is it worth mentioning that often upwelling systems are co-located with strato-cumulus cloud regions, which are known to be subject to large atmospheric model cloud and boundary layer errors? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is not assessed in our chapter
34491	38	21	38	21	Figure 9.12 a to d seem to be exactly identical... are they dummy figures? [Claire Waelbroeck, France]	Not applicable. Figure removed
129439	38	27	38	27	Use SSP or RCP consistently through the document for emission scenarios. [Trigg Talley, United States of America]	Accepted. SSP or RCP are used depending on whether we refer to CMIP5 or CMIP6.
129441	38	31	38	31	SSH is not defined anywhere. Assume it refers to Sea Surface Height. [Trigg Talley, United States of America]	Accepted. Rewritten
27613	38	31	38	39	The paragraph is very reduced despite the importance of marginal seas in assessing climate change impacts on human activities. The paragraph could be further developped from the numerous references cited to provide details concerning change in water masses, shelf ocean exchanges. Concerning the Mediterranean Sea, recent papers related to regional THC may be includes such as Soto-Navarro et al 2020 (doi.org/10.1007/s00382-019-05105-4), Somot et al. 2018 (https://doi.org/10.1007/s00382-016-3295-0) [Eric Brun, France]	Accepted. References included.
1743	38	31	39	10	Considering the importance of coastal systems and marginal seas, the services they provide, and the significant impacts of climate change on them, the two paragraphs comprising Section 9.2.3.5.2 should be expanded. The amount of coverage in this section is not in balance with the amount of coverage in other sections of the chapter. [Michael Kennish, United States of America]	Rejected, not enough space. Leaving this task to WGII, Chp 12, Atlas.
40747	38	31			section 9.2.3.5.2: what's the impact of the lack of coupling? It not really clear from the text? [TSU WGI, France]	Accepted, text revised.

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5555	38	31			The section about the coastal systems concerns only the coasatl ocean, but it is important to introduce the coastline or shoreline.The coast can range from a few hundred meters to several kilometers on either side of the land-sea interface, and can include the shelf, the nearshore zone, the coastline and river mouths including estuaries and deltas. These environments are affected by large variations of water level, in connection with many hydro-meteo-marine phenomena : offshore currents, wind-driven shelf circulation and waves, tides, storm surges, sea level rise and inputs from streamflow and groundwater for estuaries [Benoit Laignel, France]	Accepted, shoreline now mentioned.
35073	38	33	38	33	Shelf seas, marginal seas, and the coastal zone is where we interact with the ocean (where our fisheries are, oil and gas extraction, where our cities are, etc). I wonder if this point warrents a sentence. [Jonathan Tinker Tinker, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, WG2 content
85299	38	41	38	41	Is it worth also mentioning that ekman drainage in the bottom boundary layer can also be important in shelf-deep ocean exchanges which require terrain following vertical coordinates to adequately represent (the majority of IPPC models do not use terrain following coordinates)? See review by Holt et al, Prospects for improving the representation of coastal and shelf seas in global ocean models, Geoscientific Model Development 10(1):499-523, 2017. [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, too detailed.
61469	38	45	38	45	Wahlin et al 2020 work on explaining ice front barrier to barotropic flow https://doi.org/10.1038/s41586-020-2014-5 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References included.
101893	38	47	38	48	"fjord dynamics linked to glacial outflows (Straneo and Cenedese, 2015)" - here also fjord dynamics regarding a potentiailly large carbon sink (Smith et al., 2015; Cui et al., 2016) has to be mentioned with a cross reference to Chapter 5 (Global Carbon and biogeochem cycles and feedbacks). This potentially large carbon sink in coastal regions (including those with upwelling) as well as fine-scaled shelf processes (e.g. water-sediment interactions, tides, fronts) makes it difficult for coarse models to estimate the effect of climate change for biological carbon pump (Schrum et al., 2016; Holt et al., 2016). This information is crucial as coastal regions are one of the most productive in the world and it is important to know how climate change will affect coastal ecosystems and their ecosystem services. Full references: Smith, R. W., Bianchi, T. S., Allison, M., Savage, C., & Galy, V. (2015). High rates of organic carbon burial in fjord sediments globally. Nature Geoscience, 8(6), 450-453; Cui, X., Bianchi, T. S., Savage, C., & Smith, R. W. (2016). Organic carbon burial in fjords: Terrestrial versus marine inputs. Earth and Planetary Science Letters, 451, 41-50; Holt, J., Schrum, C., Cannaby, H., Daewel, U., Allen, I., Artioli, Y., ... & Pushpadas, D. (2016). Potential impacts of climate change on the primary production of regional seas: A comparative analysis of five European seas. Progress in Oceanography, 140, 91-115; Schrum, C., Lowe, J., Meier, H. M., Grabemann, I., Holt, J., Mathis, M., ... & Wakelin, S. (2016). Projected change—North sea. In North Sea region climate change assessment (pp. 175-217). Springer, Cham. [IAPSO ECS group review, United States of America]	Accepted, link to Section 5.2.2.4 added.
82933	38	47	38	48	Another example under this topic (fjord dynamics and glacial outflow) which I suggest to consider here is a study by Torsvik et al. (2019, Estuarine, Coastal and Shelf Science, see https://doi.org/10.1016/j.ecss.2019.02.005). [Sebastian Gerland, Norway]	Accepted. References included.
35067	38	48	38	48	Carbon export from shelf seas to the deep ocean is another example. e.g. Holt J, Wakelin S, Huthnance J (2009) Down-welling circulation of the northwest European continental shelf: A driving mechanism for the continental shelf carbon pump. Geophys Res Lett 36:. doi: L14602 10.1029/2009gl038997 [Jonathan Tinker Tinker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, link to Section 5.2.2.4 added.
77551	38	49	38	53	The two sentences are very unclear/incomplete and should be rephrased. [Emer Griffin, Ireland]	Accepted, text revised.
767	38	51	38	53	“or nesting and local mesh refinement” this sentence doesn’t seem to end or needs a verb or something added to it [Jonathan Rosser, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten
101895	38	51	38	55	The part of sentence is missing here: "Due to the complexity of shelf-deep ocean exchanges and their small scale, linking coastal ocean changes to global ocean changes with high resolution modelling or nesting and local mesh refinement" - do you mean "is needed" or something else? [IAPSO ECS group review, United States of America]	Accepted. Rewritten
18029	38	51	38	55	This seems to be an incomplete sentence. Or at best it is awkward. [Lisa Levin, United States of America]	Accepted. Rewritten
129443	38	51	38	55	Grammar. Incomplete sentence. [Trigg Talley, United States of America]	Accepted. Rewritten
99531	38	51	38	55	This sentence does not make sense, please rephrase. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1745	38	51	38	55	"Due to the complexity…Zhang et al., 2016b)." This is not a sentence. [Michael Kennish, United States of America]	Accepted. Rewritten
20189	38	51	38	55	One guesses what this sentence is about; still one would like it to have a verb [philippe waldteufel, France]	Accepted. Rewritten
27615	38	51	38	55	The sentence is not complete. [Eric Brun, France]	Accepted. Rewritten
35071	38	51	38	55	incomplete sentence [Jonathan Tinker Tinker, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten
99533	39	4	39	4	Add "models" after "cryosphere". [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten
101897	39	4	39	6	"Coarse climate models and even the HighResMIP models at near 10 km ocean resolution do not represent some coastal phenomena such as cross-shelf exchanges and sub-mesoscale eddies..." - See comment above regarding p 38 lines 47-48: other fine scaled processes such as water-sediment interactions, tides and fronts are also difficult for coarse climate models to resolve in coastal regions (Schrum et al., 2016; Holt et al., 2016) [IAPSO ECS group review, United States of America]	Noted, but enough example processes are already covered and one of these references is already included.
35069	39	7	39	7	Tides are absent from most current (CMIP5/CMIP6) GCM, and are essential to simulate many shelf seas regions (e.g. Tinker et al 2015). (I'm sure there are more relevant references). Tinker J, Lowe J, Holt J, et al (2015) Validation of an ensemble modelling system for climate projections for the northwest European shelf seas. Prog Oceanogr 138:211–237. doi: 10.1016/j.pocean.2015.07.002 [Jonathan Tinker Tinker, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Addition of tides is already mentioned. Reference added.
20559	39	8	39	10	This issue might deserve to be mentioned in a "knowledge gap" section, if there was one [philippe waldteufel, France]	Noted.
132587	39	13	39	13	It seems that a key point to make here is that while global sea level change comes from multiple sources, regional observed sea level change has largely mirrored the thermosteric component over recent decades, linking sea level to pattern of ocean warming you discuss above. [Kyle Armour, United States of America]	Accepted. Text has been revised accordingly.
14697	39	13	40	21	In this section titled "impact of ocean changes on sea level", it would be very appropriate to include a significant summary of ocean-driven marine ice sheet (i.e. Antarctic Ice Sheet) contributions to sea level, via warmer sub-shelf temperatures -> increased sub-shelf melt -> reduced buttressing -> increased ice flow. Indeed, especially over long periods, the impact of warming ocean temperatures on Antarctica (and to lesser extent Greenland) will very likely be the largest "impact of ocean changes on sea level", that will overwhelm steric/dynamic sea level ocean impacts. In lieu of example text here - please let me (Jeremy Fyke, fykej@ae.ca) know if you want me to generate this text. [Jeremy Fyke, Canada]	Rejected. This is discussed in 9.4
14699	39	13	40	21	Is section 9.2.4 "impact of ocean changes on sea level" actually needed? Or is it duplicating information that is also captured in Section 9.6? At the least, provide prominent references to section 9.6 and relevant subsections. Currently I do not see any such references. [Jeremy Fyke, Canada]	Noted. Title has changed to more precisely explain what we discuss: Processes not discussed in 9.6.
96959	39	13	40	21	Chapter 9.2.4 seems a bit redundant with 9.6 in general. Please streamline. [Nicole Wilke, Germany]	Noted. Title has changed to more precisely explain what we discuss: Processes not discussed in 9.6.
29643	39	13	40	22	Here you may cite: Little, C. M., A. Hu, C. W. Hughes, G. D. McCarthy, C. G. Piecuch, R. M. Ponte, M. D. Thomas, 2019, The Relationship between United States East Coast Sea Level and the Atlantic Meridional Overturning Circulation: a Review, JGR-Ocean, 124, doi: 10.1029/2019JC015152. This is review paper on sea level along US east coast and AMOC. You may also cite: Hu, A. and S. C. Bates, 2018: Internal climate variability and projected future regional steric and dynamic sea level rise. Nature Communications, 9, 1068, doi:10.1038/s41467-018-03474-8 for influence of internal variability on regional sea level change. [Aixue Hu, United States of America]	Accepted. The reference to Little is included, the reference to Hu & Bates seemed less appropriate in the text of this section.
801	39	13	40	42	I think it would be important to mention the impact that tide gauges have on regional understanding in the trends of sea level rise. It is such a vast dataset that has been collected for decades that could have great impact on sea level trends (maybe this is mentioned in another chapter). Tide gauges have also been vital in validating sea level trends from satellite altimetry. [Michael Hart-Davis, Germany]	Accepted. This point is included in Box 9.1
803	39	13	40	42	Furthermore, maybe studies on vertical land motion should be mentioned to put sea level rise into greater context. [Michael Hart-Davis, Germany]	Rejected. This is not the topic of this paragraph.
40749	39	13			section 9.2.4: improvements since AR5 not really clear [TSU WGI, France]	Accepted. Text has been revised accordingly.
90459	39	15	39	15	What is the differences between "sea level height" and "sea level"? [Holly Kyeore Han, Canada]	Accepted. Text has been revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129445	39	17	39	17	Ocean dynamics are not just density effects, but also affect ocean bottom pressure. Some examples: Hughes et al. 2017: A window on the deep ocean: The special value of ocean bottom pressure for monitoring the large-scale, deep-ocean circulation. Fukumori et al. 2015: A near-uniform fluctuation of ocean bottom pressure and sea level across the deep ocean basins of the Arctic Ocean and the Nordic Seas. [Trigg Talley, United States of America]	Accepted. Text revised according to reviewer's remark.
3297	39	17	39	17	Similar to comment No 10: ocean dynamics are not just density effects, but also affect ocean bottom pressure. Some examples: Hughes et al. 2017: A window on the deep ocean: The special value of ocean bottom pressure for monitoring the large-scale, deep-ocean circulation and Fukumori et al. 2015: A near-uniform fluctuation of ocean bottom pressure and sea level across the deep ocean basins of the Arctic Ocean and the Nordic Seas [Thomas Frederikse, United States of America]	Accepted. Text revised according to reviewer's remark.
27617	39	17	39	20	Forget and Ponte (2015) could be a relevant citation here. 10.1016/j.pocean.2015.06.002 [Eric Brun, France]	Rejected. We focus on new insights from recent literature.
62155	39	19	39	20	Figure 9.11 does not show that changes in barotropic circulation affects local and regional sea level [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Incorrect figure referencing corrected.
96961	39	20	39	20	No regional sea level in Figure 9.11, please check referencing figure number. [Nicole Wilke, Germany]	Accepted. Incorrect figure referencing corrected.
90461	39	22	39	22	"Changes" => "An increase"? (Since talking about thermal "expansion" of ocean) [Holly Kyeore Han, Canada]	Not applicable. This part has been removed.
96963	39	23	39	25	Please mention, if these numbers compatible with the SROCC statements. [Nicole Wilke, Germany]	Not applicable. This part has been removed.
61331	39	24	39	24	In the executive summary, a probability quantifier 'likely' is raised for the 1971-2015 thermosteric sea-level change (p5, line 14). This is not the case here. Please clarify. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
61341	39	25	39	28	The estimated thermosteric contribution to future sea-level rise is given here without a confidence or probability assessment. Yet the exectuive summary shows one (page 5, line 21). Please be consistent. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been removed.
99535	39	29	39	39	Need justification for the statement "even though … model errors". Please add references or remove. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been removed.
90463	39	30	39	30	"shows" => "show" [Holly Kyeore Han, Canada]	Not applicable. This part has been removed.
84035	39	31	39	31	This is the first time SSH appears on Chapter 9 and the meaning of this abreviation is not present. [Marco Tulio Cabral, Brazil]	Noted. Term SSH no longer used.
107455	39	31	39	31	The acronym SSH has not been used before this point and should be written out [Jennifer Walker, United States of America]	Noted. Term SSH no longer used.
27619	39	33	39	34	Please note that satellite altimetry measure geocentric sea-level, not relative sea-level. Relative sea-level should thus be changed to geocentric sea-level in this paragraph. [Eric Brun, France]	Accepted. Text revised according to reviewer's remark.
129447	39	33	39	35	[PRECISION] The figure referenced does not appear to support the statement. Figure 9.14 does not show regional trends from the satellite altimeters. [Trigg Talley, United States of America]	Accepted. Incorrect figure referencing corrected.
129449	39	33	39	35	[PRECISION] Tide gauges have already been used to assess regional variations in sea-level trends before the altimetry era (e.g., Gornitz et al., 1982: Global Sea Level Trend in the Past Century). Also, satellites don't measure RSL but geocentric sea level. [Trigg Talley, United States of America]	Accepted. Text has been revised accordingly.
129451	39	33	39	35	[PRECISION] In this sentence, it is stated that, since the 1990s, due to the advent of satellite altimetry, spatial patterns of relative sea level change can be described and characterized. This phrasing is odd for two reasons. First, it's unclear why relative sea level is being spoken of, since altimeters observe not relative sea level but rather absolute or geocentric sea level. Second, patterns of relative sea level change can, to some extent, be described and characterized for periods going back much further than the 1990s, based on available tide gauge records and paleo proxies. One of two things need to happen here. Either the sentence needs to be changed to refer to absolute or geocentric sea level. Or, if the insistence is on relative sea level, then it should be clarified that other observing systems (tide gauges, proxies) allow (albeit more limited) characterization of the spatial structure of relative sea level change going back much farther than the last ~3 decades. [Trigg Talley, United States of America]	Accepted. Text revised according to reviewer's remark.
3299	39	33	39	35	Tide gauges have already been used to assess regional variations in sea-level trends before the altimetry era (e.g. Gornitz et al 1982: Global Sea Level Trend in the Past Century. Also, satellites don't measure RSL but geocentric sea level. [Thomas Frederikse, United States of America]	Accepted. Text revised according to reviewer's remark.
1781	39	33	39	35	I'm not sure why this is referred to as RSL; the text suggests that this concerns the climatic (geocentric) component of sea level. Also, Fig. 9.14 shows SSH, not RSL. [Torbjorn Tornqvist, United States of America]	Accepted. Text has been revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20561	39	33	39	44	In summary, the high resolution simulations reproduce the observed standard deviations remarkably well over the historical period. This offers good hope of extracting the anthropogenic signal as soon as it becomes detectable [philippe waldteufel, France]	Noted.
44971	39	33	39	44	The variability described in Llovel et al (2018) for the regional sea level trends is due to chaotic nature of the oceans. This variability is defined as intrinsic ocean variability. This variability is just a fraction of the total internal variability of the climate system. Llovel et al 2018 do not discuss the intrinsic atmospheric variability nor the coupled variability between oceans and atmosphere. I suggest to clarify this section accordingly. [WILLIAM LLOVEL, France]	Accepted. Text revised according to reviewer's remark.
1783	39	34	39	35	The figure suggests the largest changes in temperate basins, so it is not clear what tropical/subtropical refers to. [Torbjorn Tornqvist, United States of America]	Accepted. Text has been revised accordingly.
27621	39	35	39	35	This might be removed as different groups are providing satellite altimetry datasets (Univ of Colorado, Copernicus Marine Service distributing formerly AVISO sea level products, JPL, ..) [Eric Brun, France]	Accepted. Text has been revised accordingly.
129453	39	35	39	37	This statement is very confusing, particularly the reference to annual to decadal timescales. From one year to the next, changes in regional sea level can be much, much larger than GMSL. 10-year regional trends can also be much larger than what is seen in GMSL. It seems as though the intent here is that during the full altimeter record, regional trends can deviate by 100% from GMSL trend. This statement is not true when including shorter timescales. [Trigg Talley, United States of America]	Accepted. Text revised according to reviewer's remark.
27623	39	36	39	36	About '[...] ocean dynamic sea-level trends [...]': satellite measurements measure the total sea level trend, not only the trend in sterodynamic sea level (although the spatial heterogeneity in SL trends is mostly due to sterodynamic trends). We suggest rephrasing this sentence. [Eric Brun, France]	Accepted. Text has been revised accordingly.
129455	39	36	39	37	On annual time scales, the differences can be much larger than 100%. 100% is more or less the regional variation in trends over the full 1993-2018 altimetry record (e.g., Hamlington et al., 2020: Investigating the Acceleration of Regional Sea-level Rise During the Satellite Altimeter Era, Figure 3). [Trigg Talley, United States of America]	Accepted. Text has been revised accordingly.
3301	39	36	39	37	On annual time scales, the differences can be much larger than 100%. 100% is more or less the regional variation in trends over the full 1993-2018 altimetry record (For example Hamlington et al. 2020: Investigating the Acceleration of Regional Sea-level Rise During the Satellite Altimeter Era figure 3) [Thomas Frederikse, United States of America]	Accepted. Text has been revised accordingly.
29947	39	38	39	42	Chafik et al. (2019) looked exactly at this. Suggest adding a reference after Han et al. 2017. The suggested text is "RSL change (Hamlington et al., 2014; Han et al., 2017; Chafik et al., 2019)" Reference: https://doi.org/10.1038/s41598-018-37603-6 [Léon Chafik, Sweden]	Not applicable. This part has been revised and this point is no longer discussed.
27625	39	42	39	42	About 'RSL': We recommend to replace by geocentric sea level. [Eric Brun, France]	Accepted. Text has been revised accordingly.
3341	39	42	39	44	Another study that might be relevant for this stetement is Hamlington et al (2020): Investigating the Acceleration of Regional Sea-level Rise During the Satellite Altimeter Era. This paper assesses regional trends and accelerations from altimetry to conlude that many trends are outside the range of natual variability, but regional accelerations are amost everywhere not outside the range. [Thomas Frederikse, United States of America]	Accepted. Reference included.
129457	39	42	39	44	Another study that might be relevant for this statement is Hamlington et al. (2020, Investigating the Acceleration of Regional Sea-level Rise During the Satellite Altimeter Era). This paper assesses regional trends and accelerations from altimetry to conlude that many trends are outside the range of natual variability, but regional accelerations are almost everywhere not outside the range. [Trigg Talley, United States of America]	Accepted. Reference included.
72005	39	42			Suggest add Zhang and Church 2012 [John Church, Australia]	Rejected. We focus on new insights from recent literature.
89257	39	43	39	43	in certain regions, e.g., the Southern Ocean [Kewei Lyu, Australia]	Not applicable. This part has been revised and the sentence has disappeared.
90465	39	46	40	4	In its current status, this paragraph does not seem to have a clear idea on what it is about - is it about improvements in modelling or about uncertainties in models and spread in model results? [Holly Kyeore Han, Canada]	Accepted. Paragraph has been rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129459	39	46	40	4	The phrases "thermosteric and ocean dynamic sea-level change" and "ocean dynamic and thermosteric sea-level change" are used several times. This phrasing needs to be changed, since it is very confusing. The authors likely intend to say something like "GLOBAL-MEAN thermosteric and ocean dynamic sea-level change". Or, as Gregory et al. (2019, Surveys in Geophysics) would call it, "sterodynamic sea level change." The "global-mean" qualifier before "thermosteric" is important. As it's currently written, this sentence would seem to suggest that "ocean dynamic sea level" is not steric in nature. This would imply that all ocean-dynamic sea level changes are reflected in changes in ocean bottom pressure, and mostly indicative of barotropic processes. This is, of course, not true, since (especially on long climate time scales) most dynamic sea level changes are baroclinic, and manifested in density and steric sea level changes. Phrasing like this is a pervasive problem. Change this phrasing to either "global-mean thermosteric and dynamic sea level" (in the sense of ZOSTOGA and ZOS variables from CMIP models) or call it "sterodynamic" as Gregory et al. advocate. [Trigg Talley, United States of America]	Accepted. Text has been revised accordingly.
89353	39	48	39	53	Watanabe et al. (2020, ERL) found that uncertainty in the vertical diffusivity (model parameterization) leads to uncertainty in sea level rise. I think it would be good to refer this to discuss the source of the uncertainty of sea level rise. https://doi.org/10.1088/1748-9326/ab8ca7 [Michio Watanabe, Japan]	Not applicable. This part has been revised and this point is no longer discussed.
89265	39	51	39	52	A recent paper analysing dynamic sea level change in the CMIP5 and CMIP6 models suggests that a large part of the uncertainties in dynamic sea level projections is due to uncertainties in model mean state simulations. https://doi.org/10.1175/JCLI-D-19-1029.1 [Kewei Lyu, Australia]	Accepted. Reference included.
27627	39	51	39	52	About '[...] these are the primary sources of uncertainty in thermosteric and ocean dynamic sea-level change.': at local to regional scales. At global scale, the spread in CMIP5 global mean thermosteric sea level rise is largely explained by the model spread in climate feedback parameter (notably related to cloud feedbacks) and global heat uptake efficiency (Melet and Meyssignac 2015) [Eric Brun, France]	Not applicable. This part has been revised and this point is no longer discussed.
65937	39	52	31	52	Suggest changing to: "Evidence since AR5 shows…" [Kushla Munro, Australia]	Not applicable. This part has been revised and the sentence has disappeared.
89267	39	52	39	53	More importantly, the inter-model spread in dynamic sea level projections remains similarly large in CMIP6 as in CMIP5. https://doi.org/10.1175/JCLI-D-19-1029.1 [Kewei Lyu, Australia]	Accepted. Reference included.
61445	39	52	40	4	It seems to me that the information in these lines is repeated/overlaps with the information the next paragraph that starts with "Future Projections of RSL …". I think you should try to collect the ideas coherently and only mention what is important. Here you talk about changes associated with AMOC slowing down, and that is metioned again at the end of the next paragraph. If you are talking about two different processes, then please make that clear. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text has been revised accordingly.
89269	39	53	39	54	In the North Atlantic and Arctic the large uncertainties in dynamic sea level proejctions are mainly due to uncertainties in AMOC projections. https://doi.org/10.1175/JCLI-D-19-1029.1 [Kewei Lyu, Australia]	Accepted. Reference included.
61449	40	1	40	1	Wanting for a reference. Suggestion review by Little et al 2019 (https://doi.org/10.1029/2019JC015152) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference included.
107457	40	1	40	1	Seems a reference needed at the end of this sentence [Jennifer Walker, United States of America]	Accepted. Reference included. Note however, that the text has been completely revised.
96965	40	3	40	3	Wrong figure number, probably 9.14 instead. [Nicole Wilke, Germany]	Accepted. Incorrect figure referencing corrected.
129461	40	10	40	12	[CONFIDENCE] It is hard to see how this statement is attributable to the Fasullo and Nerem (2018) study, much less that high confidence is given to this statement based on that one study. Possible that there are additional references, but these lines are weakly supported (at best) by the referenced paper. [Trigg Talley, United States of America]	Accepted. Text has been thoroughly revised.
89271	40	10	40	12	This doesn't make sense at all - a large ensemble can account for internal variability but doesn't mean the model can simulate dynamic sea level change patterns "correctly". It's even strange to see this statement is labled as "high confidence". [Kewei Lyu, Australia]	Accepted. Text has been thoroughly revised.
99537	40	10	40	12	Sentence reads as if confidence comes from ensemble size which I do not think is what is meant. Please rephrase. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised.
96967	40	12	40	14	Please explain why the CMIP5/6 model ensembles do not separate internal variability and model uncertainty. [Nicole Wilke, Germany]	Not applicable. This part has been revised and the sentence has disappeared.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22579	40	14	40	14	You really do not want to be using a trigger phrase such as 'special technique' this will be pounced on by those wishing to discredit thye report. Instead describe the technique scientifically here. [Peter Thorne, Ireland]	Not applicable. This part has been revised and this point is no longer discussed.
98703	40	14	40	14	"using a special technique" - This is too vague and mysterious. Give details. [Sonya Legg, United States of America]	Not applicable. This part has been revised and this point is no longer discussed.
129463	40	14	40	14	This sentence references a "special technique" used by Marcos and Amores (2014). This is totally unacceptable. Why use such a vague and magical-sounding phrase? Say what Marcos and Amores did. They used, in their own words, "a signal-to-noise maximizing empirical orthogonal function technique". Say that. [Trigg Talley, United States of America]	Not applicable. This part has been revised and the sentence has disappeared.
107459	40	14	40	14	What is a 'special technique'? [Jennifer Walker, United States of America]	Not applicable. This part has been revised and the sentence has disappeared.
61451	40	14	40	15	using a special technique a common pattern' - suggestion to change this to be more specific. E.g. applying a signal to noise EOF which eliminates climate noise, a forced repsonse can be illicited CMIP5/6 that matches with the observed trends'. Perhaps add reference to Venzke, S., M. R. Allen, R. T. Sutton, and D. P. Rowell (1999), The atmospheric response over the North Atlantic to decadal changes in sea surface temperature, J. Clim., 12, 2562–2584 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part has been revised and this point is no longer discussed.
82935	40	18	40	19	I wonder if there are reference(s) which could be cited here, in addition to the cross reference to 9.2.3.4, to support this statement. [Sebastian Gerland, Norway]	Not applicable. This part has been revised and the sentence has disappeared.
61465	40	20	40	20	Here high confidence is given to sea level rise on the western north atlantic associated with a decreasing AMOC. In section 9.2.3.4 low confidence is attributed to a currently decreasing trend in the AMOC (page 29,line 26), although future predictions show a decrease with high confidence until the end of the 21st centruy where predictions diverge again. Perhaps this should be made clearer here so that conflicting messages are not portrayed. Moreover, as WBC are not well represented in models, a further uncertainty could be introduced. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text expanded to better explain this point.
35141	40	20	40	22	Also worth adding Harning et al. (2019) to the citations for the North Icelandic Shelf as it uses a similar IP25-based approach to show that in addition to increasing sea ice extent, sea ice was also likely thickest during the LIA. REFERENCE: Harning, D.J., Andrews, J.T., Belt, S.T., Cabedo-Sanz, P., Geirsdóttir, Á., Dildar, N., Miller, G.H., Sepúlveda, J., 2019b. Sea ice control on winter subsurface temperatures of the North Iceland Shelf during the Little Ice Age: A TEX86 calibration case study. Paleoceanog. Paleoclimatol. 34, 1006-1021. [David Harning, United States of America]	Rejected. This is not the topic of this paragraph.
98705	40	27	40	27	Delete "the" in "using the a method" [Sonya Legg, United States of America]	Noted. Caption revised
85031	40	32	44	25	No comments [Katrine Husum, Norway]	Noted.
129465	40	35	40	38	For Figure 9.14, it is unclear if the std of the CMIP dynamic SSH is calculated over a model average. If so, it would underestimate and average out variability, thereby making it an apples-to-oranges comparison against the observations (AVISO record). Needs to be clarified and possibly rectified. [Trigg Talley, United States of America]	Noted. Caption revised. The std is calculated on 5 day averages model-by-model so as to be comparable to the AVISO data.
287	40	43	45	4	Why isn't there any discussion of changes in Arctic sea ice age from motion tracking by satellite? [THOMAS Wagner, United States of America]	Accepted. We now mention that this is covered in SROCC
109067	40	43	48	24	Section 9.3 has a minimum of 1 "low confidence" statements per page, and a few pages have 5-6 "low confidence" statements. While the assessment of the diversity of sea-ice research is informative, can section 9.3 more succinctly summarize what is considered more substantial knowledge gained by the community? [Chaincy Kuo, United States of America]	Noted. We have carefully examined our usage of "low confidence" and now restrict it to policy-relevant statements

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98337	40	43	48	24	I totally miss an assessment of what we know about sea ice variability, its historical and projected change and the driving mechanisms. Although the understanding of drivers and future changes is still poor, this information is highly relevant for economic activity in the Arctic and of interest not only for Arctic communities. We have learned a lot since AR5. I suggest a separate paragraph that includes the following references (among others): 1) Miles, M. W. et al. (2014), "A signal of persistent atlantic multidecadal variability in arctic sea ice." Geophys. Res. Lett. 41, 463–469, https://doi.org/10.1002/2013GL058084; 2) Zhang R. (2015), "Mechanisms for low-frequency variability of summer arctic sea ice extent", Proc. Natl Acad. Sci. USA 112, 4570–4575, https://doi.org/10.1073/pnas.1422296112; 3) Wernli, H. & Papritz, L. (2018), "Role of polar anticyclones and mid-latitude cyclones for Arctic summertime sea-ice melting", Nat. Geosci. 11, 108–113, https://doi.org/10.1038/s41561-017-0041-0; 4) Olonscheck D. et al. (2019), "Arctic sea-ice variability is primarily driven by atmospheric temperature fluctuations", Nat. Geoscience 12, 430-434, https://doi.org/10.1038/s41561-019-0363-1; 5) Charles F. K. & Yulaeva, E. (2020), "Influence of Arctic sea-ice variability on Pacific trade winds, Proc. Natl Acad. Sci. USA 117 (6) 2824-2834, https://doi.org/10.1073/pnas.1717707117; 6) Halloran, P.R. et al. (2020), "Natural drivers of multidecadal Arctic sea ice variability over the last millennium", Sci. Rep. 10, 688, https://doi.org/10.1038/s41598-020-57472-2; 7) Brennan, M. K. et al. (2020).Arctic sea-ice variability during the instrumental era. Geophysical Research Letters 47, e2019GL086843. https://doi.org/10.1029/2019GL086843 [Dirk Olonscheck, Germany]	Accepted. We have revised and strengthened our treatment of internal variability and now cite all the suggested references.
55089	40	43			Other sections for cryosphere variables are organized using sub-sections focused on (1) observations (2) model evaluation and (3) model projections. This approach is not used in the sea ice section, which negatively affects the consistency of the chapter structure, and the organization of the sea ice section itself. [Nancy Hamzawi, Canada]	Noted. For each topic within the sea-ice section, we follow the organisational structure suggested here. However, owing to the granularity of the sea-ice section, this is not made explicit through 5th level sub-section headings.
27629	40	45	40	50	The use of introductory paragraphs is not widespread. However, it is very useful for understanding the text. Another introductory paragraph is to be found in point 9.5.2. Their use could be generalised. [Eric Brun, France]	Noted.
88255	40	47	40	47	Are you referring to sea ice extent/area as a key indicator? [Sharon Smith, Canada]	Accepted. This has been clarified to now read "sea-ice area"
67219	40	49	40	50	If this is generally true for all sections this should go in the intro and does not need to be here. [Regine Hock, United States of America]	Accepted. This statement has now been removed, and the specific findings from SROCC are spelled out explicitly throughout the text.
6765	40	55	40	55	This sentence is not entirely clear. It is referring to a net decrease since 1979 for all months of the year. For some months in years after 1979 sea-ice coverage is higher than in 1979. So the sentence could be amended to something like "Sea-ice coverage in the Arctic has decreased from 1979 to the present in every month of the year". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The original wording was kept for the citation of the SROCC finding, while the suggested new wording has been used for the findings on sea-ice area introduced here.
65189	40	55	40	56	Should also cite Onarheim et al 2018 here as well. [Mark England, United States of America]	Noted. The relevant sentence has been replaced by a citation of SROCC that cite Onarheim et al 2018.
105783	40		48		Due to limitation of resources and time caused by COVID-19 for past several months, I was only able to read the summary and sea ice section 9.3 (my expertise) with a bird's eye view. I apologize that I could not read the full Chapter-9 critically. I find that the section 9.3 is mostly well-written covering the most recent developments, research findings, and model outputs in sea ice for both poles. It does contain recent publications that justify the statements. In Figure 9.15, sea ice thickness observations retrieved from satellites could also be shown in addition to sea ice concentration. Several satellite-based sea ice thickness products are currently available on daily basis since 2010 onwards. Sea ice concentration is a good indicator of the spatial view, but sea ice thickness can be more useful for estimating the total sea ice volume in the polar regions. [Mukesh Gupta, Belgium]	Not applicable. Sea-ice is not discussed here.
61481	41	4	41	5	recommend two papers: (1) Alekseeva, Tatiana, et al. "Comparison of Arctic Sea Ice Concentrations from the NASA Team, ASI, and VASIA2 Algorithms with Summer and Winter Ship Data." Remote Sensing 11.21 (2019): 2481. (2) Ye, Y., Shokr, M., Aaboe, S., Aldenhoff, W., Eriksson, L. E. B., Heygster, G., … Girard-Ardhuin, F. (2019). Inter-comparison and evaluation of sea ice type concentration algorithms. doi:10.5194/tc-2019-200 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Partly accepted. The paper by Alekseeva et al is now cited. The paper by Ye et al was not accepted for publication and cannot be cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11157	41	4	41	5	recommend two papers: (1) Alekseeva, Tatiana, et al. "Comparison of Arctic Sea Ice Concentrations from the NASA Team, ASI, and VASIA2 Algorithms with Summer and Winter Ship Data." Remote Sensing 11.21 (2019): 2481. (2) Ye, Y., Shokr, M., Aaboe, S., Aldenhoff, W., Eriksson, L. E. B., Heygster, G., … Girard-Ardhuin, F. (2019). Inter-comparison and evaluation of sea ice type concentration algorithms. doi:10.5194/tc-2019-200 [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	Partly accepted. The paper by Alekseeva et al is now cited. The paper by Ye et al was not accepted for publication and cannot be cited.
103797	41	5	41	8	Are the references to back up these statements the ones listed in lines 4-5? [Philippe Tulkens, Belgium]	Accepted. The text has been revised to clarify which citations relate to which statement
80835	41	5	41	8	Are the references to back up these statements the ones listed in lines 4-5? [Louise Sandberg Sørensen, Denmark]	Accepted. The text has been revised to clarify which citations relate to which statement
103799	41	6	41	6	ice loss -> ice losses [Philippe Tulkens, Belgium]	Accepted, the wording has been changed as suggested.
80833	41	6	41	6	ice loss -> ice losses [Louise Sandberg Sørensen, Denmark]	Accepted, the wording has been changed as suggested.
105981	41	8	41	9	The distinction between SIA and SIE is not covered in the SOD 2.3.2.1, nor the reason to favor one over the other. In fact, both SIE and SIA are used frequently in that subsection. You should perhaps should refer to the Glossary and definitions given there, which could support this choice of emphasis. [William Gutowski, United States of America]	Accepted. This distinction is now made explicit in the opening paragraph of section 9.3.1.1
55091	41	9	41	9	"However, these uncertainties are far smaller than the observed sea-ice loss." Could specify that the uncertainties are small than the sea ice loss in each month of the year. [Nancy Hamzawi, Canada]	Accepted, this suggested wording has now been added.
45347	41	11	41	30	Good figure! [Clara Burgard, Germany]	Noted.
52041	41	12	41	29	Figure 9.15: I was confused when looking at the 4th column (SSP2-4.5, 2045-2054) because the plot is of number of models and not of concentration. Changing the colour bar for the 4th colomun - so different from cols #1&2, would help to make it clear that something different is being plotted. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure has been redrawn with a different colorbar for the fourth column.
109867	41	12	41	44	Figure 9.15 and regional analysis ignores the important seasonal ice regions of the Labrador-Newfoiundland shelf and the Gulf of St Lawrence, where models project dramatic winter ice loss. [Donald Forbes, Canada]	Noted. However, both owing to space constraints and lack of robustness, we do not include a detailed analysis of regional projections.
99939	41	14	41	29	Figure 9.15. The colored anomoly image is usable as is, but the observations shown in the right panel are not viewable at the small size as shown. Suggest larger panels for both left and right in Figure 9.15. [Dan Helman, United States of America]	Accepted. We have redrawn the figure for clarity.
55093	41	14	41	41	Be precise with terminology: suggest not using terms like 'sea ice coverage' but rather extent, area, or concentration as appropriate. [Nancy Hamzawi, Canada]	Accepted. The text has been revised to use specific terms suggested by the reviewer as much as possible. Occasional usage of "coverage" has remained to refer to vague assessments of the sea-ice cover of the Arctic Ocean for example from paleo records.
27631	41	24	41	24	About '2055' : The diagram indicates 2054. [Eric Brun, France]	Accepted. Corrected to 2054.
12167	41	25	41	27	The observational datasets are listed / cited twice. Maybe only the first time is enough. [Thomas Lavergne, Norway]	Accepted, the caption has been revised to now only include the data sources once.
52043	41	32	41	33	The wording "it is most pronounced during winter in the Barents Sea and the Sea of Okhotsk" is a little misleading as the suggestion is that the most pronounced sea ice loss anywhere in the Arctic throughout the year is winter Barents/Okhotsk, when September shows a more pronounced loss. Reordering this to "during winter it is most pronounced in the Barents Sea and the Sea of Okhotsk" would fix this. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, this has been revised as suggested
2979	41	32	41	33	Ice loss in the Baltic Sea has also been very pronounced: "fact that the 1985–2015 Baltic Sea ice extent distribution differs from any other preceding 30 winter period since 1720 with a high confidence" (Uotila et al. 2015). Reference: Uotila, P., Vihma, T., & Haapala, J. (2015). Atmospheric and oceanic conditions and the extremely low Bothnian Bay sea ice extent in 2014/2015. Geophysical Research Letters, 42(18), 7740–7749. https://doi.org/10.1002/2015GL064901. [Petteri Uotila, Finland]	Accepted. This paper is now cited for the discussion of long-term sea-ice changes.
61513	41	32	41	44	This occurs here but also in other paragraphs of this section 9.3.1.1. Specific seas are named but for non specialists, it is difficult to pinpoint where these Arctic seas are as they are less well known that e.g. the North Sea/Baltic Sea etc. I wonder if it might be possible to include an extra figure with all the seas labelled for the Arctic regions at least. I wouldn't suggest adding this information on e.g. Figure 9.1 as it is already very busy and extra information would make it very cluttered. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Owing to space constraints, we cannot provide a map that includes names of individual seas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83289	41	32	41	44	Include 2 additional key references here re change in Arctic sea ice seasonality: (1) (9) Stammerjohn, S., R. Massom, D. Rind and D. Martinson. 2012. Regions of rapid sea ice change: An inter-hemispheric seasonal comparison. Geophysical Research Letters, 39, L06501, doi:10.1029/2012GL050874; (2) (4) Maksym, T. 2019. Arctic and Antarctic sea ice change: Contrasts, commonalities, and causes. Annu. Rev. Mar. Sci., 11, 187-213. [Robert Massom, Australia]	Accepted in part: The recent paper has been added, while the 2012 reference is considered as outdated relative to the studies cited now.
109865	41	37	41	37	"pan-Arctic wide" is redundant. [Donald Forbes, Canada]	Accepted, this now reads "pan-Arctic"
24155	41	38	41	38	I don't think this necessarily means the absolute seasonal ice area will hve increased, it could just be a relative increase? [Alek Petty, United States of America]	Rejected. If the summer sea-ice area decreases faster than the winter sea-ice area, the absolute area of seasonal sea ice must increase (Seasonal SIA = SIA March - SIA September; if SIA September decreases more than SIA March, Seasonal SIA increases)
24157	41	39	41	39	Change 'consistently; to something more physical - i.e. over 'decadal time-scales'. It probably hasn't declined every year. [Alek Petty, United States of America]	Accepted partly. This has been revised to clarify that the term consistently is here meant to link the previous sentence to the finding of this sentence.
103801	41	39	41	40	It seems like this reference would be appropriate here: https://doi.org/10.1088/1748-9326/aae3ec [Philippe Tulkens, Belgium]	Accepted, this has been added
80837	41	39	41	40	It seems like this reference would be appropriate here: https://doi.org/10.1088/1748-9326/aae3ec [Louise Sandberg Sørensen, Denmark]	Accepted, this has been added
82959	41	41	41	42	I suggest to consider referring here also to findings from a new study for two Svalbard fjords by Johansson et al. (2020, Annals of Glaciology, https://doi.org/10.1017/aog.2019.52). [Sebastian Gerland, Norway]	Accepted, this has been added
24159	41	42	41	42	Was Bering Sea the only region analyzed or that exhibited this increasing trend? If so make that point. [Alek Petty, United States of America]	Accepted, this now reads "only in the Bering Sea"
45349	41	49	41	49	The following reference could be added here: Smith, A. and Jahn, A.: Definition differences and internal variability affect the simulated Arctic sea ice melt season, The Cryosphere, 13, 1–20, https://doi.org/10.5194/tc-13-1-2019, 2019. [Clara Burgard, Germany]	Accepted, this has been added
35873	41	51	41	53	Suggest adding reference to the recent study of Brennan et al. (2020), who reached the same conclusion using a data assimilation-based reconstruction of sea ice spanning 1850 to present. Reference: Brennan, M.K., Hakim, G.J. and Blanchard-Wrigglesworth, E., 2020. Arctic Sea-Ice Variability During the Instrumental Era. Geophysical Research Letters, 47(7), e2019GL086843. [Mitch Bushuk, United States of America]	Accepted, this has been added
83625	41	51			P9-41 line 51 “Reconstructions of Arctic sea-ice coverage put the satellite period changes into centennial context. Direct 52 observational data coverage warrants high confidence that the low Arctic sea-ice area of summer 2012 is 53 unprecedented since 1850” Only appears unprecedented because the author has not included relevant literature. See comment above relating to page 9-5. [michael asten, Australia]	Rejected. The reviewer does not provide relevant literature that assesses pan-Arctic sea-ice area or extent. Fluctuations on a regional scale analysed in some of these studies are covered in the current text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83627	41	51			P9-41 line 51 “Reconstructions of Arctic sea-ice area before 1850 remain sparse, but as in AR5 and SROCC, there remains medium confidence that the current pan-Arctic 1 sea-ice loss is unique during the past 1450 years (Kinnard et al., 2011; De Vernal et al., 2013b).” I conclude that No, the seaice loss is not unique in past 1ka; see my comments at page 9-5 above. The reconstructions from Cabedo-Sanz and Humlum make this clear. See also my figure attached at comment page 9-5. Cabedo-Sanz, P., Belt, S. T., Jennings, A. E., Andrews, J. T., and Geirsdóttir, Á. (2016). Variability in drift ice export from the Arctic Ocean to the North Icelandic Shelf over the last 8000 years: A multi-proxy evaluation. Quat. Sci. Rev. 146, 99–115. doi:10.1016/j.quascirev.2016.06.012. Humlum, O. , Jan-Erik Solheim , Kjell Stordahl, 2011, Identifying natural contributions to late Holocene climate change, Global and Planetary Change 79 (2011) 145–156 Lüdecke H-J and , C.O.Weiss, 2017, Harmonic Analysis of Worldwide Temperature Proxies for 2000 Years. The Open Atmospheric Science Journal, 11, 44 -53. [michael asten, Australia]	Partly accepted. None of these papers makes an attempt to reconstruct pan-Arctic sea-ice coverage. The paper by Cabedo-Sanz is cited several times regarding its results on regional sea-ice fluctuations. Note also that only "medium confidence" is attached to the finding that the sea-ice loss is unique in the past 1 ka owing to the small number of relevant studies. Our treatment of internal variability has been expanded.
52045	41	54	41	56	The wording here (i.e., "lowest March ... which is usually the maximum" implies that in these years March was very low but in isolation - so that either February or April became the minimum. Perhaps better to flip this sentence to say something more like: "Since 1953, the years 2015 to 2018 had the four lowest recorded winter maximum values of Arctic sea-ice areal coverage, which usually occurs in March. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the wording has been changed as suggested.
109047	41	54	41	56	The wording "..which usually is..." seems to imply that March is no longer the highest ice coverage. As such, over 2015-2018, does March have the lowest ice coverage compared to a historical period March, or is March no longer the annual high in this period, where it was the annual high historically? Some wording adjustment can help clarify this sentence. [Chaincy Kuo, United States of America]	Accepted, this sentence has been re-written
61519	42	4	42	10	Here the authors describe what kind of paleo-proxy reconstructions they take into account to make inferrences about sea-ice dynamics beyond the observational record. They mention IP25, bromine, and dinocyst assemblage. However in the paragraph below (page 42 line 12-27) they also refer to studies that reconstruct past sea-ice extent based on driftwood occurrence (i.e. Funder et al. 2011). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, driftwood has been added.
103803	42	4	42	10	Relevant paper : https://doi.org/10.1016/j.quascirev.2017.12.016 [Philippe Tulkens, Belgium]	Accepted, this paper has been added.
105125	42	4	42	10	A comparison of PMIP4-CMIP6 models vs these reconstructions is provided in Kageyama et al.Climate of the Past, 2020 (in revision). This paper shows the potential for better characterising sea ice cover in the Arctic during the LIG, because there is a relationship between the response of Arctic sea ice during the LIG and near term future Arctic sea ice [Masa KAGEYAMA, France]	Noted. However, owing to space constraints we can unfortunately not add at this point this recent progress in paleo modelling of Arctic sea ice.
80839	42	4	42	10	Relevant paper : https://doi.org/10.1016/j.quascirev.2017.12.016 [Louise Sandberg Sørensen, Denmark]	Accepted. This paper is now cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41903	42	4	42	27	This paper adds information on preindustrial model performance: 'Natural drivers of multidecadal Arctic sea ice variability over the last millennium' Halloran et al. 2020. ABSTRACT 'The climate varies due to human activity, natural climate cycles, and natural events external to the climate system. Understanding the different roles played by these drivers of variability is fundamental to predicting near-term climate change and changing extremes, and to attributing observed change to anthropogenic or natural factors. Natural drivers such as large explosive volcanic eruptions or multidecadal cycles in ocean circulation occur infrequently and are therefore poorly represented within the observational record. Here we turn to the first high-latitude annually-resolved and absolutely dated marine record spanning the last millennium, and the Paleoclimate Modelling Intercomparison Project (PMIP) Phase 3 Last Millennium climate model ensemble spanning the same time period, to examine the influence of natural climate drivers on Arctic sea ice. We show that bivalve oxygen isotope data are recording multidecadal Arctic sea ice variability and through the climate model ensemble demonstrate that external natural drivers explain up to third of this variability. Natural external forcing causes changes in sea-ice mediated export of freshwater into areas of active deep convection, affecting the strength of the Atlantic Meridional Overturning Circulation (AMOC) and thereby northward heat transport to the Arctic. This in turn leads to sustained anomalies in sea ice extent. The models capture these positive feedbacks, giving us improved confidence in their ability to simulate future sea ice in in a rapidly evolving Arctic.' https://www.nature.com/articles/s41598-020-57472-2 [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This paper is now cited both in this paragraph and in a new paragraph on internal sea-ice fluctuations.
61521	42	7	42	10	In this sentence there is a confidence statement but the remaining information is quite vague (e.g. some regions, sea-ice responds to climate forcing). Is there a way to re-write this sentence and make it more specific, so that it is easier for the reader to pick up on what the authors are hinting at? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The relationship between sea-ice fluctuations , internal variability and climate forcing is now made explicit in a dedicated paragraph.
42913	42	7			"other paleo proxies including bromine". The Br ice core proxy remains heavily debated, I suggest "other proposed palaeo proxies including bromine in ice cores". [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the sentence has been changed as suggested.
65941	42	8	33	8	Suggest reviewing and confirming the statement 'We here confirm SROCC assessment'. [Kushla Munro, Australia]	Rejected. There is no SROCC assessment of paleo sea-ice fluctuations as far as we can tell.
45351	42	10	42	10	"indicating that sea-ice responds to climate forcing" is very general. It is not clear to me to what kind of "climate forcing" this is referring to and to what timescales. [Clara Burgard, Germany]	Accepted. The relationship between sea-ice fluctuations , internal variability and climate forcing is now made explicit in a dedicated paragraph.
67375	42	12	42	27	Add the Jansen et al. (2020) paper to this chapter. In this paper, Jansen et al. conclude that recent temperature changes in the Arctic (as seen in reanalyses) are as abrupt as temperature changes related to Dansgaard/Oeschger events in the palaeo record. [Martin Stendel, Denmark]	Rejected. This reference is not appropriate for the sea-ice section of this report
55095	42	12	42	27	Improve readability of this paragraph - it contains an impenetrable sequence of regions and citations. [Nancy Hamzawi, Canada]	Noted. Readability can be improved through formatting in the final typesetting.
61515	42	12	42	27	Same comments as just above: Specific seas are named but for non specialists, it is difficult to pinpoint where these Arctic seas are as they are less well known that e.g. the North Sea/Baltic Sea etc. I wonder if it might be possible to include an extra figure with all the seas labelled for the Arctic regions at least. I wouldn't suggest adding this information on e.g. Figure 9.1 as it is already very busy and extra information would make it very cluttered. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Owing to space constraints, we cannot provide a map that includes names of individual seas.
61523	42	12	42	27	Many of these studies extend further back than the YD, demonstrating millenial-scale sea-ice variability across the entire last deglaciation (e.g. Belt et al. 2015, Meheust et al. 2018, Horner et al. 2016). Additionally, millenial-scale sea-ice variability has also been observed across DO events in the North Atlantic (Hoff et al. 2016 Nature Communications, Sadatzki et al. 2019 Science Advances). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. These references are now included, as is some coverage of sea-ice fluctuations before the YD.
24161	42	13	42	13	What relative changes? Do you mean covariability between northern hemisphere temperature and sea ice? At millennial time-scales? [Alek Petty, United States of America]	Accepted. This has been re-written to now include the term "covariability"
61525	42	19	42	23	increasing sea-ice cover during the middle and late Holocene is also observed in northeast Greenland, around Ellesmere Island, and in the CAA (Funder et al. 2011, England et al. 2008 Geophysical Research Letters, Belt et al. 2010 Quat Sci Rev, Vare et al. 2009 Quat Sci Rev) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted partly. The information is now included, but only related to the Funder et al., 2011 reference. This is because of our focus on more recent literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10697	42	20	42	23	Do all these references show "peaks" at the same time, or is the claim that they peak at different times in some 400 year period? This sort of statement could be used to imply that the "Little Ice Age" was more coherently cold than it actually was. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A re-read of these references showed that too few of these references include findings on sea ice during the Little Ice Age, which is why this statement has been removed.
109869	42	21	42	21	Add "central Canadian Arctic Archipelago (St-Hilaire-Gravel et al., 2010)," before West Greenland. St-Hilaire-Gravel, D., Bell, T., Forbes, D.L. 2010. Raised gravel beaches as proxy indicators of past sea-ice and wave conditions, Lowther Island, Canadian Arctic Archipelago. Arctic, 63(2), 213-226, doi:10.14430/arctic976 [Donald Forbes, Canada]	Rejected. We focus on recent literature in this assessment.
65943	42	24	33	24	Suggest clarification regarding "the spread in CMIP6 ACC transport simulations is lower but". What does "is lower" refer to? Lower than what? [Kushla Munro, Australia]	Accepted. Text revised.
83557	42	24	42	24	Verify the age used here for the LIG. The given age falls towards the upper end of the LIG interval (129-116 ka; Annex II, Chapter 2), but the single age used throughout AR6 for the LIG is 125 ka (see for example p. 10 in Chapter 2). [Antje H. L. Voelker, Portugal]	Accepted. This now reads "around 125 ka"
6767	42	29	42	32	Arctic sea-ice cover has gone down for all months of the year and GMST has gone up for all months of the year, over the past four decades. This indeed suggests a negative correlation. But does much correlation remain after the linear trend is removed from each time series? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The correlation holds on decadal and longer scales. This has now been added.
109871	42	29	43	25	This discussion focused on climate forcing of sea-ice loss omits consideration of positive-feedback effects such as from loss of multiyear ice and reduction of mean ice thickness. [Donald Forbes, Canada]	Partly accepted. The relationship between sea-ice fluctuations , internal variability and climate forcing is now made explicit in a dedicated paragraph. We had already before made explicit that self-acceleration driven by feedbacks does not play a major role in the observed and projected sea-ice evolution.
65939	42	30	32	46	Suggest including the westward Antarctic Coastal Current, particularly given the Hellmer et al., 2012 reference. [Kushla Munro, Australia]	Not applicable to the page and line referred to. If the comment is misplaced we cannot identify what this refers to.
12169	42	38	42	38	Maybe change “Because” to “Based on” or “On the basis of” [Thomas Lavergne, Norway]	Accepted. This has been changed to "Based on"
1747	42	38	42	41	If more than half of the observed Arctic sea-ice loss in summer is attributed to anthropogenic factors, how much sea-ice loss is attributed to other external factors during summer? More detail is needed regarding the external and internal factors responsible for Arctic sea-ice loss. [Michael Kennish, United States of America]	Rejected. This is covered in detail in section 3.4.1
65945	42	41	33	42	Suggest clarification that the eddy component is also very (and regionally) important. Suggest citing: Foppert, A., K. A. Donohue, D. Randolph Watts, and K. L. Tracey (2017), Eddy heat flux across the Antarctic Circumpolar Current estimated from sea surface height standard deviation, J. Geophys. Res.Oceans, 122, 6947–6964, doi:10.1002/2017JC012837. [Kushla Munro, Australia]	Noted. Though we do not feel this clarification would help our assessment here
33435	42	41			Change: “…sensitivity of sea-ice loss to CO2 emissions,…” by “…sensitivity of sea-ice loss to CO2 emissions,…”. [Guiomar Rotllant, Spain]	Accepted. This now reads "sea-ice loss" instead of "sea-ice area"
103805	42	43	42	43	What is meant by internal variability here? [Philippe Tulkens, Belgium]	Rejected. Internal variability is defined in the glossary and is a standard term in climate science.
80841	42	43	42	43	What is meant by internal variability here? [Louise Sandberg Sørensen, Denmark]	Rejected. Internal variability is defined in the glossary and is a standard term in climate science.
88625	42	45	42	45	Castruccio et al., 2018. Reference not found. [Rosemary Vieira, Brazil]	Accepted. The reference list has been corrected to now include this paper.
65191	42	47	42	48	In agreement with England et al 2019 (doi: 10.1175/JCLI-D-18-0864.1) which examined CESM large ensemble and CMIP5 models. [Mark England, United States of America]	Accepted. This paper has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35875	42	50	42	53	In Figures 9.16d and 9.16h, it is unclear why both the x and y axes were cast as sensitivities to CO2. Since both axes are sensitivities with respect to CO2, the dCO2 terms “cancel out” in this plot, and what remains is a scatterplot of sea ice trends versus surface air temperature trends. I suggest plotting Figs 9.16d,h simply as scatterplots of trends, which would provide a simpler and more transparent presentation. Also, this approach offers a conceptual advantage, since casting sea ice changes as sensitivities to CO2-alone neglects the important confounding effects of other forcing agents on sea ice trends. In particular, other forcing agents, such as non-CO2 greenhouse gases (Polvani et al., 2020) and aerosols (Rosenblum and Eisenman, 2016), have been shown to exert an important influence on simulated sea ice trends. Sea ice and temperature have a well-established physical link, whereas the sea ice/CO2 link relies upon other forcing agents being relatively constant or relatively small over the analysis period. For example, sea ice and cumulative CO2 emissions are strongly correlated over the satellite period, since aerosol forcing has been relatively constant over this period, but this link may break down if aerosol forcing changes in the future (this fact is noted on Page 43, lines 43-46). This suggests that the sea ice/temperature connection is a more fundamental relationship to focus on in this figure. References: Polvani, L.M., Previdi, M., England, M.R., Chiodo, G. and Smith, K.L., 2020. Substantial twentieth-century Arctic warming caused by ozone-depleting substances. Nature Climate Change, 10(2), pp.130-133. Rosenblum, E. and Eisenman, I., 2016. Faster Arctic sea ice retreat in CMIP5 than in CMIP3 due to volcanoes. Journal of Climate, 29(24), pp.9179-9188. [Mitch Bushuk, United States of America]	Noted. The plot is presented as trends with respect to CO2 because (a) CO2 is the overall driver of most of the observed changes and (b) the trends are near-linear versus CO2 but not versus time. Hence, it is less clear what should be plotted if the reference to CO2 is removed. The limitations of this approach are discussed in section 5.5 for the approach of a carbon budget to achieve a specific temperature goal. This section is now explicitly referred to.
26375	42	51	42	51	CO2 ->2 as subindex [María Santolaria-Otín, France]	Accepted. Text revised.
30701	42	53	42	53	In a number of places there appears …. 'submitted, submitted' [Ian Simmonds, Australia]	Accepted, this has now been corrected throughout,
55097	42	54	42	55	I realize this may be difficult for the pre-industrial period, but can the estimates of internal variability from CMIP6 models be placed in any kind of observational context? [Nancy Hamzawi, Canada]	Accepted. The CMIP6 estimate is now put into context with lower estimates from reanalyses and observational estimates from Brennan et al., 2020.
16391	42	55	42	55	Is sigma defined somewhere (standard deviation presumably)? [Julian Mak, China]	Accepted. Sigma is now defined explicitly as standard deviation of September sea-ice area
26377	43	1	43	1	submitted, submitted -> twice [María Santolaria-Otín, France]	Accepted, this has been corrected
98339	43	3	43	13	The following reference is of relevance here and may be added after Mydryk et al., 2018: Olonscheck D. et al. (2019), "Arctic sea-ice variability is primarily driven by atmospheric temperature fluctuations", Nat. Geoscience 12, 430-434, https://doi.org/10.1038/s41561-019-0363-1 [Dirk Olonscheck, Germany]	Accepted, this reference has been added.
82937	43	4	43	5	Another (new) regional study connecting air temperature and (regional) sea ice changes, which I suggest to consider here, is the one by Dahlke et al. (2020, International Journal of Climatology, DOI: 10.1002/joc.6517). [Sebastian Gerland, Norway]	Accepted. This paper has now been added.
24163	43	4	43	11	I don't see the scientific value of this paragraph and think it should be dropped or substantially modified. We know that terms in the near surface atmosphere modulate the surface energy balance (e.g. surface air temperature) and thus affect sea ice evolution. What added value do these studies provide? It's also a mix direct (temperature) and indirect (cloud) parameters which is confusing. is this just making up for the fact we have low confidence in the relative contribution of each - the sentences that follow? [Alek Petty, United States of America]	Noted. The paragraph has been kept as it outlines which physical drivers contribute to the observed evolution of Arctic sea ice, which constitutes fundamental understanding of the observed sea-ice evolution.
55099	43	11	43	11	The focus of this paragraph is physical drivers of regional sea ice changes. How does disagreement among CMIP5 models limit understanding of drivers during the observational era? [Nancy Hamzawi, Canada]	Noted. If CMIP5 models were to agree with each other and with observational records, we would have better process understanding of the contribution of individual drivers.
24165	43	15	43	16	Correlation is one thing but what we really want to know is the sensitivity of ice loss to global warming and how that might change? I currently think there is a bit too much focus on the global temperature correlations. [Alek Petty, United States of America]	Accepted. Correlation between sea-ice area and global mean temperature is considered as another way of phrasing the sensitivity of sea-ice loss to global warming. To clarify this, the latter framing has now been added.
24167	43	16	43	16	Change 'ice has vanished' to 'summer ice has vanished' [Alek Petty, United States of America]	Accepted, this has been changed as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65193	43	17	43	25	Some of the uncertainty in the date of ice-free arctic arises purely due to internal variability (Jahn et al 2016 [doi: 10.1002/2016GL070067] [Mark England, United States of America]	Noted. This paragraph does not talk about the specific date of an ice-free Arctic, but this reference is cited in one of the following paragraphs that discuss internal variability of sea ice in more detail.
52047	43	19	43	46	The terminology used for an "ice-free" Arctic needs to be consistent because there is often confusion with the lay community (media etc.). On P43L19 you say that below 1 million sq km is "effectively ice-free", on P43L24 you use "becomes sea-ice free", on p43L39 is it "practically ice-free", and on P43L42 you use "near-ice free"! I recommend to change all of these to "practically ice-free" for consistency - including with the terminology used in the SIMIP paper and in the ch9 exec summary. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now referred to as "practically sea-ice free" throughout.
109049	43	23	43	25	Can this statement simply say that at this time, CMIP5 and CMIP6 models are not sensitive enough to determine the specific level of global warming for the onset of Arctic Sept ice-free? Or does Chp 9 have low-confidence in models in general? [Chaincy Kuo, United States of America]	Noted. The current wording implies the meaning suggested by the reviewer and has been left unchanged.
26379	43	25	43	25	submitted, submitted -> twice [María Santolaria-Otín, France]	Accepted. This has been corrected.
52049	43	28	43	36	Fig 9.16: the observations (black dot) and plausible scenarios - grey shading based on standard deviations - are not described in the figure caption. Also I find the use of grey for CMIP3 confusing given the grey "plausibility" shading and recommend use of a 4th colour here (maybe the yellow used int he other panels?). [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The caption now explains the dot and the shading. The colour for CMIP3 now differs from the grey shading.
45353	43	29	43	34	The caption does not explain the dark square in in d) and h) of Fig. 9.16 [Clara Burgard, Germany]	Accepted. The caption now explains the dot and the shading.
61527	43	30	43	34	state in the figure caption of Figure 9.16 what the black dot and grey shading represent in panel d and h [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The caption now explains the dot and the shading.
85303	43	39	43	39	I didn't noticed too much detail, although perhaps I missed it, on IPCC coupled model Arctic seasonal sea-ice biases and associated process errors, including known errors in northward Atlantic heat transport in low resolution ocean models? Also is it worth mentioning the implifications for sea-ice of polar amplification and temperature changes in excess of 10 deg C in some months for the Arctic even for global mean warming levels of 2-3 deg C (as discussed in a gernal editorial comment)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There is discussion of the possible physical drivers of sea-ice changes, including a statement on the disagreement of CMIP models on the relative roles of these drivers. This specifically includes oceanic heat transport. We also specifically discuss the possible loss of winter sea ice in higher warming scenarios.
24169	43	39	43	39	Shouldn't this idea have a confidence attached? [Alek Petty, United States of America]	Accepted. The confidence level has been added.
52051	43	39	43	40	From the CMIP6 analysis in the SIMIP paper we cannot say "throughout September" here - only September mean. I suspect that having 30th September ice-free would happen a fair bit later than the September mean . [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This now reads September mean.
132589	43	39	43	41	I find it strange that Arctic sea ice is the only variable discussed as a function of cumulative carbon emissions. I get that there are publications making this point, but then anything in the system that scales with global temperature could also be linked to cumulative carbon in this way. Why not discuss sea ice as a function of global temperature like you do with other system components? The link to cumulative carbon depends on TCRE, which is being updated in this report (or have you accounted for that update in your values?). [Kyle Armour, United States of America]	Noted. The link to cumulative carbon emissions is made here because several studies have used this metric. Some of them are directly based on observed sensitivity of sea ice to carbon emissions, which agrees well with the sensitivity of sea-ice to carbon emissions in CMIP6 simulations.
85035	43	41	43	41	The references reads "(SIMIP Community, submitted, submitted)" [Katrine Husum, Norway]	Accepted. This has been corrected.
33437	43	41			Change: ”(SIMIP Community, submitted, submitted)” by “(SIMIP Community, submitted)”. [Guiomar Rotllant, Spain]	Accepted. This has been corrected.
24171	43	43	43	46	I think this aerosol idea needs to be expanded: e.g. please state how exactly would reduce aerosol load impact future ice loss. How constrained is this? [Alek Petty, United States of America]	Accepted. This now reads "future reduction in atmospheric aerosol load that would cause additional warming". Additional details can be found in the two references cited here.
109077	43	45	43	45	coordination with chapter 6 on section referencing section 6.3? [Chaincy Kuo, United States of America]	Noted.
129467	43	48	43	54	[ENSEMBLES] According to the passage, there appears a difference between CMIP5 and CMIP6 on method for future projections. The same is inferred for other climate impacts discussed earlier. This difference raises a question as to what causes are responsible for this apparent model difference between CMIP6 and CMIP5. In addition, by recognizing the model ensemble difference, should AR6 provides opinions on which is better in model projections? Having both CMIP5 and CMIP6 in one place with differing conclusions is really confusing. [Trigg Talley, United States of America]	Accepted. This has been clarified now: This paragraph focuses on CMIP5 models because no regional assessments of sea ice in CMIP6 models exist yet.
1749	43	48	43	54	It would be informative to make recommendations at the end of line 54 on the steps necessary to increase the confidence level in CMIP5 model projections for Arctic regional sea-ice evolution. [Michael Kennish, United States of America]	Noted. However, both owing to space constraints and lack of understanding, no such recommendations can be included here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109053	43	48	43	54	Will regional Arctic sea-ice models be addressed for regional forecasts of sea-ice concentrations? Regional sea-ice models are used for shipping route forecasts, aren't they? [Chaincy Kuo, United States of America]	Noted. Unfortunately, we have been unable to find published literature on this topic and can hence not assess projections from regional models here.
109051	43	48	44	24	CMIP6 models will be assessed, now that there are more models to analyze? [Chaincy Kuo, United States of America]	Accepted. This now covers the available studies on CMIP6 models.
55101	43	50	43	54	Edit for readability [Nancy Hamzawi, Canada]	Accepted. This sentence has been re-written and shortened.
116855	43		43		The notion of "correlation" seems misleading (Artic sea ice / global temperature). Co-vary? (no lag, responding both to cumulative emissions)? [Valerie Masson-Delmotte, France]	Accepted in part: It has now been made explicit that the correlation only exists on decadal and longer time scales.
2981	44	1	44	3	Use correct grammar for ‘sea ice’ or ‘sea-ice’, should be ‘Arctic sea ice’ but ‘Arctic sea-ice extent’ etc. [Petteri Uotila, Finland]	Accepted. This has been corrected.
68923	44	14	44	15	This “no tipping point” comment directly contradicts Table 4.10, which lists Arctic sea ice as a tipping element in the climate system. I suspect that there are other inconsistencies between CH 9 and 2 [Darrell Kaufman, United States of America]	Accepted. We have now coordinated with chapter 4 to state in both chapters that sea ice is not a tipping element and that all changes are reversible
132591	44	14	44	24	Good assessment here. However, it disagrees with what is reported in Chapter 4 (see their Table 4.10 and associated discussion, where they label sea ice as having a tipping point but reversible, somehow). We need to decide on a consistent definition of tipping points and reversibility to use across the report. I prefer the definition you use here, which is consistent with the literature I know of. [Kyle Armour, United States of America]	Accepted. We have now coordinated with chapter 4 to state in both chapters that sea ice is not a tipping element and that all changes are reversible
109055	44	21	44	21	"..in some, but not all, CMIP5 projections…". Can a specific fraction or percentage of CMIP5 and CMIP6 models be quoted here, for which this acceleration is estimated? [Chaincy Kuo, United States of America]	Accepted in principle. This sentence has been revised to now make the statement that in CMIP5 and CMIP6 models the ice loss in winter accelerates once the ice retreats from the continental margins.
100849	44	21	44	23	A recent analysis by Meccia et al. 2020 on CMIP5 RCP8.5 ensemble simulations with the EC-Earth model in two different configurations (base and with Stochastic Physics scheme activated in the atmosphere) shows an abrupt transition in the winter Arctic sea ice loss when GSAT crosses a given threshold in the model (as for many models documented in in Bathiany et al. 2016). However in the ensemble integrations with stochastic physics the GSAT warming is slower and the abrupt transition occurs about a decade or so later. Meccia, V. L., Fabiano, F., Davini P., & Corti S. 2020. Stochastic parameterizations and the climate response to external forcing: An experiment with EC-Earth. Geophysical Research Letters, 47, e2019GL085951. https://doi.org/10.1029/2019GL085951 [Corti Susanna, Italy]	Accepted. This reference has now been added.
52053	44	21	44	24	Re. March/winter decline for high warming levels: would this confidence level be upgraded now that several of the CMIP6 models show winter decline post 2080? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised this sentence to now discuss (with high confidence) the acceleration of winter sea-ice loss in CMIP5 and CMIP6 models once the ice cover retreats from the continental margins.
83291	44	27	45	2	Include discussion of snow cover thickness on Arctic sea ice here too. Reference Webster et al. 2018 and Sturm and Massom (2017) - (1) Webster, M., S. Gerland, M. Holland, E. Hunke, R. Kwok, O. Lecomte, R. Massom, D. Perovich and M. Sturm. 2018. Snow in the changing sea-ice systems. Nature Climate Change, 8, 946-953. doi.org/10.1038/s41558-018-0286-7; and (2) Sturm, M. and R.A. Massom. 2017. Snow in the Sea Ice System: Friend or Foe? In D. Thomas (editor): Sea Ice. 3rd Edition, Wiley-Blackwell, New York (USA) & Oxford (UK), pp. 65-109. DOI:10.1002/9781118778371.ch3. [Robert Massom, Australia]	Noted. Snow on sea ice is discussed in the atlas, as stated on p. 9-44, l.45
24173	44	29	44	29	I thought it was strange to start with a very confident statement 'decreased in all months' then to mention that the quantitative estimates are low confidence. Maybe reorder this paragraph, lead with perhaps medium confidence in the recent satellite observations for assessing interannual variability but that there is still high confidence that in this long time period the volume has decreased in all months! [Alek Petty, United States of America]	Rejected. We find it more important to transport the message that we know for sure that sea-ice volume has decreased. We therefore start with this statement.
109057	44	29	44	36	section 9.3.1.2 describes the difficulty in quantifying the sea-ice thickness, with present remote sensing and historically. So, perhaps the paragraph can start with a short physical process explanation describing why there is the expectation that the ice should become thinner with global warming. Just a statement to constrast against decrease in ice area, for the policymakers? [Chaincy Kuo, United States of America]	Accepted. A short factual statement has now been added to describe that sea ice melts both vertically and horizontally in a warming climate.
26381	44	34	44	34	Current best estimates from "reanalysis " or " renalyses"? [María Santolaria-Otín, France]	Accepted. This has been changed to "reanalyses".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35877	44	34	44	36	I suggest also providing an estimate (or multiple estimates) of the average Arctic sea ice volume over this period, in order to place these volume loss estimates in appropriate context. [Mitch Bushuk, United States of America]	Accepted. We now only report percentage change in sea-ice volume.
27633	44	36	44	36	About ' […] 1979 to 2010 (Schweiger et al., 2019).': We recommend checking to make sure that it is not rather '1978 to 2018'. [Eric Brun, France]	Noted. The original time period is correct, but an updated estimate has been included now for the period 1979-2016.
61475	44	38	44	38	Lack of literature in the end of the sentence "For the more recent past, direct ice-thickness estimates from satellites exist". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The literature has been moved from the following sentence to this one.
52055	44	38	44	38	"direct ice-thickness estimates from satellites exist" This is not exactly true. Radar altimeter (e.g. CryoSat-2) observations are of freeboard (or more specifcially "radar freeboard") and laser altimeter (e.g. ICESat) observations are of snow freeboard. Thickness then has to be inferred by making assumptions about the snow depth ("loading") and the relative densities of snow, ice and ocean (using a hydrostatic approximation). Therefore I would not include the word "direct" here and would say something more like "sea ice thickness estimates inferred from satellite measurements". I'd also remove the "direct" from the 2nd sentence too. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Both sentences have been changed to reflect this notion.
82939	44	40	44	44	A new publication by Petty et al. (2020, JGR-O, https://doi.org/10.1029/2019JC015764) could be relevant for this discussion. [Sebastian Gerland, Norway]	Accepted. This reference has now been added.
28635	44	44		45	The reference to Atlas 5.10.1.3 does not contain a descripition of the role of snow for satellite retrievals. I think the reference should be to Atlas 5.10.3, but this section also does not contain a description of the role of snow for satellite retrievals, only something on the distribution of snow, snow-ice formation, radiative fluxes, ecology and ice dynamics. [Isolde Glissenaar, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This now only reads "detailed discussion of snow on sea ice" with no direct reference to remote sensing.
28633	44	44			Reference to Atlas 5.10.1.3 should be Atlas 5.10.3 [Isolde Glissenaar, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected.
1751	44	47	44	51	Explain in this paragraph the greater thinning of sea ice in the central Arctic Ocean over the past 60 years. [Michael Kennish, United States of America]	Rejected. The thinning of central Arctic sea ice is here not put into any comparative context, which is why we do not explain some "greater" thinning there.
46633	44	47	45	2	Missing: discussion on FYI/MYI, or more specifically the disappearance of MYI. Could also refer to the 2018 North Greenland Polynya (e.g. Ludwig et al 2019) [Céline Heuzé, Sweden]	Noted. The disappearance of Multi-year ice is discussed in SROCC, which is cited in section 9.3.1.1
109059	44	50	44	50	"...thinning around 2m...". The word 'thinning' to me, is a rate. Perhaps reword to '...thinning to around 2 m..."? [Chaincy Kuo, United States of America]	Accepted. "Thinning" is now changed to "decrease in ice thickness"
2983	44	51	44	51	Use correct grammar: 'data are' instead of 'data is'. [Petteri Uotila, Finland]	Accepted, changed as suggested.
82957	44	53	44	53	In a new publication by Spreen et al. (2020/in press, JGR-O, https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019JC016039), Fram Strait sea ice thickness observations from 1990 up to 2014 are published. I suggest to consider including this here. [Sebastian Gerland, Norway]	Accepted. This reference has now been added.
109061	44	54	45	2	CMIP6 models will be assessed, now that there are more models to analyze? [Chaincy Kuo, United States of America]	Accepted. This now covers the available studies on CMIP6 models.
116857	44		44		What about responses to reduced emissions ? (see discussion on reversibility in chapter 4 too). I think that there is overlap and consistency needs to be found. [Valerie Masson-Delmotte, France]	Accepted. We have now coordinated with chapter 4 to state in both chapters that sea ice is not a tipping element and that all changes are reversible
99941	44				This section of Chapter 9 discusses the loss of area and change in thickness or volume of ice in the Arctic. It may only be anecodatal, but the "quality" of ice has also changed. The older, very hard and long-lived ice has been replaced by softer, slushier ice in many cases. I've not seen any studies that address this quality of ice issue but perhaps it's been included in some other way in the Arctic sea ice discussion. [Dan Helman, United States of America]	Noted. Unfortunately, we have been unable to find published literature on this topic and can hence not assess these changes here.
100025	45	3	45	44	Figure 9.17. The colored anomoly image is usable as is, but the observations shown in the right panel are not viewable at the small size as shown. Suggest larger panels for both left and right in Figure 9.17. [Dan Helman, United States of America]	Accepted. The panels have been increased.
103807	45	5	48	24	Suggest to include as reference also this paper: https://doi.org/10.1073/pnas.1906556116 [Philippe Tulkens, Belgium]	Accepted. This paper has been added.
80843	45	5	48	24	Suggest to include as reference also this paper: https://doi.org/10.1073/pnas.1906556116 [Louise Sandberg Sørensen, Denmark]	Accepted. This paper is now included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20191	45	7	45	10	We have here a quasi-perfect duplication of page 40 lines 47-49. This ought to be avoided as much as possible [philippe waldteufel, France]	Noted. We felt it was useful to introduce both sections with a similar paragraphs for readers who only read one of them.
83293	45	10	45	11	Change "The findings differ from previous assessments in particular because of the observed, substantial decline in Antarctic sea-ice area in all months since 2016" to "The findings differ from previous assessments in particular because of the observed substantial decline in overall Antarctic sea-ice area since 2016 preceded by record high coverage in 2012 to 2014." [Robert Massom, Australia]	Accepted in principle. This sentence has been removed, as such detailed description is given in the next paragraph.
101899	45	11	45	11	"...substantial decline in Antarctic sea-ice area in all months since 2016." - It would be good to refer to the studies by Parkinson (2019; https://doi.org/10.1073/pnas.1906556116) and the one by Maksym (2019, https://doi.org/10.1146/annurev-marine-010816-060610) that describe this decline in sea ice since 2016 here. One might also refer to Figure 9.17 left here. [IAPSO ECS group review, United States of America]	Accepted in principle. This sentence has been removed, as such detailed description is given in the next paragraph, which cites these papers and refers to Fig. 9.17
8993	45	11	45	11	The spectacular, pronounced, decrease in sea ice since 2016 is totally downplayed in this report. I do not see the paper by Parkinson PNAS 2019. [Eric Rignot, United States of America]	Accepted in principle. The paper by Parkinson is now cited. Note that Antarctic sea-ice area has increased in all months since 2017 and has been near the long-term average for several months at the time of writing
52057	45	11	45	11	"decline in Antarctic sea-ice area in all months since 2016" It was very late in 2016 (November?) so this could be considered misleading. Maybe best to say "late 2016" or "November 2016"? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted in principle. This sentence has been removed
21107	45	11	45	11	add reference for decline in Antarctic ses ice (Parkinson et al., 2019), Parkinson, C. L., 2019: A 40-y record reveals gradual Antarctic sea ice increases followed by decreases at rates far exceeding the rates seen in the Arctic. Proceedings of the National Academy of Sciences of the United States of America, 116, 14 414–14 423, https://doi.org/10.1073/pnas.1906556116. [AMNA JRRAR, Jordan]	Accepted. This paper is now included.
12171	45	14	45	24	At the start of the section on Arctic sea ice, a note was made about the irreductible uncertainties of satellite-based sea-ice concentration/area/extent information. Since these uncertainties are also valid in the Antarctic (and actually show more impact between the datasets, e.g. NasaTeam), a new comment at the beginning of the section on Antarctic sea ice could be appropriate. [Thomas Lavergne, Norway]	Accepted. We now include observational uncertainty among the list of reasons for the unknown significance of the long-term trend in Antarctic sea-ice area
96969	45	14	47	55	We suggest to shorten these regional details needed, since there is ample information provided in Ch3, Ch2 and in the SROCC. [Nicole Wilke, Germany]	Accepted. The regional description has been shortened.
34941	45	16	45	39	The SOD admits to no trend in Antarctic sea ice since 1979. Please see general comment #9 above. [Jim O'Brien, Ireland]	Accepted. This still holds for the FGD.
88257	45	17	45	18	Could you just say "...which includes record-high coverage in.." since you indicate in previous sentences that satellite retrievals are used. [Sharon Smith, Canada]	Accepted, we have dropped "satellite-retrieved" when re-writing this statement
83295	45	17	45	19	Add - (1) Kusahara, K., Reid, P., Williams, G.D., Massom, R., and Hasumi, H. 2018. An ocean-sea ice model study of the unprecedented Antarctic sea ice minimum in 2016. Environmental Research Letters, 13(8), DOI:10.1088/1748-9326/aad624. (2) Reid P. & Massom R.A. 2015. Successive Antarctic sea ice extent records during 2012, 2013 & 2014, In: State of the Climate in 2014. Bulletin of the American Meteorological Society, 96 (7), S163-S164. [Robert Massom, Australia]	Accepted in principle. The paper by Kusahara is now included further below where the forcing of the sea-ice reduction is discussed. The Reid & Massom paper was not cited as we focus on the most recent references for space constraints.
21109	45	19	45	19	(Parkinson et al., 2019) is more inclusive reference. [AMNA JRRAR, Jordan]	Accepted. This paper is now included.
105449	45	25	45	25	ADD, the new paper,"Twenty first century changes in Antartic and Southern Ocean surface climate in CMIP, publish on 28 May 2020, by Thomas Bracegirdle et.al. Abou the changes of the temperatura change between 1995-2014. [Elizabeth SILVESTRE, Peru]	Rejected. The relationship of the findings of this paper to sea-ice evolution is not clear.
26383	45	32	45	32	, Fluctuations -> error un ponctuation or in capital [María Santolaria-Otín, France]	Accepted. This has been corrected
33439	45	32			Change comma by a dot: "...Kimura, 2016), Fluctuations...". [Guiomar Rotllant, Spain]	Accepted. This has been corrected
129469	45	34	45	37	See also comment for Executive Summary, page 6, lines 3-7. Clarify statements related to Antarctic sea ice cover and statements made in SROCC. The SROCC Summary for Policymakers also notes high confidence related to the topic of Antarctica and sea ice: "Antarctic sea ice extent overall has had no statistically significant trend (1979-2018) due to contrasting regional signals and large interannual variability (high confidence)." [Trigg Talley, United States of America]	Accepted. The "medium confidence" statement that only referred to some of the small regional trends has been removed to avoid any confusion.
52059	45	42	46	3	Figure 9.17: I was confused when looking at the 4th column (SSP2-4.5, 2045-2054) because the plot is of number of models and not of concentration. Changing the colour bar for the 4th colomun - so different from cols #1&2, would help to make it clear that something different is being plotted. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The last column is now plotted with a different colour bar.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65947	45	44	34	44	Suggest clarification. Sudden ice shelf disintegration can also be influenced by long period ocean swell in the absence of a protective sea ice barrier. Suggest citing: Massom et al. 2018: Antarctic Ice shelf disintegration triggered by sea ice loss and ocean swell. Nature. 558, 383-389, doi:10.1038/s41586-018-0212-1. [Kushla Munro, Australia]	Taken into account. This is now referred to in the text.
54475	45	44	45	56	In the caption of Figure 9.17 the following sentence: "The observational records are based on the average sea-ice coverage of OSISAF/CCI (OSI-450 for 1979-2015, OSI-430b for 2016-2018)(Lavergne et al., 2019), NASA Team (version 1, 1979-2018)(Cavalieri et al., 1996) and Bootstrap (version 3, 1979-2018)(Comiso, 2017)", is repeated two times (near the beginning and also at the end). I suggest removing one of both in order to avoid redundancy. [Maria del Pilar Bueno Rubial, Argentina]	Accepted, the caption has been revised.
27635	45	54	45	54	About '2055' : The diagram indicates 2054. [Eric Brun, France]	Accepted. Corrected to 2054.
29661	46	6	46	17	should cite Meehl et al., 2016 (Meehl, G.A., J.M. Arblaster, C. Bitz, C.T.Y. Chung, and H. Teng, 2016: Antarctic sea ice expansion between 2000-2014 driven by tropical Pacific decadal climate variability. Nature Geoscience, DOI: 10.1038/NGEO2751. [Aixue Hu, United States of America]	Noted. This paper was already cited.
65195	46	6	47	30	England et al 2016 [doi: 10.1002/2016GL070055] show that stratospheric ozone depletion causes a robust deepening of the Amundsen Sea low, however this does not seem to be consistent with Antarctic sea ice trends according to Landrum et al 2017 [doi: 10.1002/2017GL075618]. This is another strange piece of the puzzle. [Mark England, United States of America]	Accepted, both papers are cited now.
29239	46	6			Comment as above: remark on 9.3.2.1 sea ice also [Francesca Sangiorgi, Netherlands]	Accepted. This paper is now included, but this does not affect our overall assessment
109063	46	7	46	7	Switching the order of the phrases …"different contribution is difficult among others..." to "…different contibutions among others is difficult…" would help with readability. [Chaincy Kuo, United States of America]	Accepted. This has been corrected
31465	46	11	46	11	Add references to effect of cyclones on sea ice drift: 1) Alberello, Alberto, et al. "Drift of pancake ice floes in the winter Antarctic marginal ice zone during polar cyclones." Journal of Geophysical Research: Oceans 125.3 (2020): e2019JC015418.; 2) Vichi, Marcello, et al. "Effects of an explosive polar cyclone crossing the Antarctic marginal ice zone." Geophysical Research Letters 46.11 (2019): 5948-5958. [Alberto Alberello, Australia]	Accepted. Both papers are cited now.
65949	46	12	42	12	Suggest consistency in the term 'Northern-hemisphere' with other instances (e.g. line 16 & line 33 same page). Suggest 'Northern Hemisphere'. [Kushla Munro, Australia]	Accepted. This has been corrected throughout.
21111	46	12	46	13	The strengthening of the Amundsen Sea Low will lead to increasing sea ice coverage in the Ross Sea. [The Ross Sea SIE seasonal trends are positive throughout the year, but largest in spring. The stronger meridional low over the Ross Sea has been driven by a deepening of the Amundsen Sea Low (ASL).(Turner et.a, 2016)], Through these wind-driven changes, the deepening of the ASL thus partially explains the increase in sea ice extent (equatorward ice motion) in the Ross Sea and decreases in the Amundsen and Bellingshausen Seas (ice compaction toward the coast). Raphael et al., 2016] [AMNA JRRAR, Jordan]	Accepted. This is corrected now.
39767	46	13			"increasing sea-ice coverage in the Amundsen Sea" this sounds a bit contradictory to p45 L34-36: The evolution of mean Antarctic sea-ice area is the result of opposing regional trends, with slightly decreasing sea-ice cover during the period 1979 to 2018 in the Amundsen Sea [TSU WGI, France]	Accepted. This has been corrected to now read "increasing sea-ice cover in the Ross Sea"
102275	46	14	46	17	In this paragraph, it is indicated that “Multi-decadal variations in the tropical Pacific strengthen the Amundsen Sea low”. However, several recent publications show that the Multi-decadal variations in the tropical Atlantic also significantly strengthen the Amundsen Sea low: Li, X., Holland, D. M., Gerber, E. P. & Yoo, C. Impacts of the north and tropical Atlantic Ocean on the Antarctic Peninsula and sea ice. Nature 505, 538-542 (2014) Simpkins, G. R., Peings, Y. & Magnusdottir, G. Pacific influences on tropical Atlantic teleconnections to the Southern Hemisphere high latitudes. Journal of Climate 29, 6425-6444 (2016). So I suggest to modify the sentence to “Multi-decadal variations in the tropical Pacific and Atlantic strengthen the Amundsen Sea low” and add the above references. [Xichen Li, China]	Accepted. Both papers are now cited and the sentence has been modified as suggested.
8995	46	19	46	19	So the sea ice cover is affected by northward transport but has nothing to do with the changes in westerly winds. Have you ever heard about Ekman transport? Increased northward advection is clearly a result of the strengthening of the winds .. I do not understand how this paragraph can have so many mutually exclusive statements. [Eric Rignot, United States of America]	Noted. Sea ice moves typically at an angle of about 20 % to the wind direction, rather than perpendicular to it.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29663	46	19	46	37	Maybe also cite Meehl et al., 2019 (Meehl, G.A., J.M. Arblaster, C.T.Y. Chung, M. M. Holland, A. DuVivier, L. Thompson, D. Yang, and C.M. Bitz, 2019: Recent sudden Antarctic sea ice retreat caused by connections to the tropics and sustained ocean changes around Antarctica, Nature Comms., 10:14, https://doi.org/10.1038/s41467-018-07865-9) [Aixue Hu, United States of America]	Noted. This paper was already cited in the following paragraph.
52061	46	20	46	20	SAM = Southern Annular Mode [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected
46539	46	21	46	28	The work of Jeong et al. should be discussed here as well as the conclusions in this discussion are supported by that work. Important, that work is based on a fully coupled Earth system model (E3SM) that includes the impacts of ice-ocean coupling via the explict representation of ocean circulation (and heat and freshwater exchange) within Antarctic ice shelf cavities (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, accepted). [Stephen Price, United States of America]	Accepted. This is now included.
112451	46	26	46	26	"Haumann et al., submitted , submitted" - There is only one Haumann et al. (submitted) paper in the reference list, so I am wondering if this is typo that should just be a reference to just that paper, or if there was supposed to also be a reference to the similiar name Hausmann et al. (submitted) in the reference list? [Inga Jane Smith, New Zealand]	Accepted. This has been corrected.
54477	46	26	46	26	The word "submitted" is repeated two times. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. This has been corrected.
26385	46	26	46	27	submitted, submitted -> twice [María Santolaria-Otín, France]	Accepted. This has been corrected
112449	46	26	46	27	There is an important paper that could be added to this list (although it is cited on the next page): Bronselaer, B., Winton, M., Griffies, S.M. et al. Change in future climate due to Antarctic meltwater. Nature 564, 53–58 (2018). https://doi.org/10.1038/s41586-018-0712-z [Inga Jane Smith, New Zealand]	Accepted. This paper is now included.
54479	46	27	46	27	The word "submitted" is repeated two times. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. This has been corrected.
65951	46	31	42	31	Suggest inserting a space after 0.81 [Kushla Munro, Australia]	Accepted. Space inserted.
31467	46	32	46	34	Wave induced break up of the Antarctic sea ice is the dominant process during the sea ice retreat in austral spring and summer. During the austral winter sea ice expansion is driven by the pancake ice cycle. Wave activity keeps the small pancake floes (1–10 m) separated (Alberello, Alberto, et al. "Brief communication: Pancake ice floe size distribution during the winter expansion of the Antarctic marginal ice zone." Cryosphere 13.1 (2019). and references therein), and only when wave energy is dissipated floes can thermodynamically weld together to form continuous ice cover.Small pancake ice floes, shorter that the characteristic length of ocean waves, are unaffected by wave induced break up. [Alberto Alberello, Australia]	Accepted. This has been reformulated to now make the factual statement that Southern Ocean sea ice is affected by wave induced break up.
73849	46	34	46	34	Another study suggests the importance of variability in thermal conditions. Kusahara et al. (2017) Spatiotemporal dependence of Antarctic sea ice variability to dynamic and thermodynamic forcing: a coupled ocean–sea ice model study, Climate Dynamics. [Takashi Obase, Japan]	Accepted. This paper is now included.
46541	46	34	46	37	The work of Jeong et al. could also be mentioned here as it shows support that the explicit resolution of ice shelf melting in Earth system models (relative to ice shelf melting and its impacts being absent) impacts S. Ocean sea ice concentration and thickness in ways that are consistent with observations (i.e., increased sub-ice shelf melting could explain some of the observed S. Ocean sea ice expansion in recent decades). This is based on simulations using a fully coupled Earth system model (E3SM) that includes the impacts of ice-ocean coupling via the explict representation of ocean circulation (and heat and freshwater exchange) within Antarctic ice shelf cavities (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, accepted). [Stephen Price, United States of America]	Accepted. This paper is now included.
16393	46	39	46	41	Confusing sentences. Maybe something along the lines of “There is low confidence in the precise physical drivers causing the delice in Antarctic sea ice since 2016, as both change in atmospheric circulation (references) and subsurface ocean heat flux (references) contribute.” [Julian Mak, China]	Accepted. This sentence has been re-written largely as suggested.
46637	46	39	46	43	At least mention the Weddell / Maud Rise polynya [Céline Heuzé, Sweden]	Accepted. The polynya is now mentioned in this paragraph with respective literature and a link to section 9.2.3.2
101901	46	40	46	42	"Change in atmospheric circulation…. and in subsurface ocean heat flux…. contribute." - For completeness the study by Purich and England (2019; https://doi.org/10.1029/2019GL082671) should be listed here as well. (Purich, A., & England, M. H. (2019). Tropical teleconnections to Antarctic sea ice during austral spring 2016 in coupled pacemaker experiments. Geophysical Research Letters, 46, 6848– 6858. https://doi.org/10.1029/2019GL082671) [IAPSO ECS group review, United States of America]	Accepted. This paper is now included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101903	46	40	46	42	"Change in atmospheric circulation…. and in subsurface ocean heat flux…. contribute." - It might be good to briefly discuss the possible influence of tropical variability on the 2016 sea ice decline as suggested by Stuecker et al. (2017, https://doi.org/10.1002/2017GL074691), Meehl et al. (2019, https://doi.org/10.1038/s41467-018-07865-9), Wang et al. (2019, https://doi.org/10.1038/s41467-018-07689-7), and Purich and England (2019, https://doi.org/10.1029/2019GL082671).. [IAPSO ECS group review, United States of America]	Accepted. The teleconnections to the tropics are now explicitly mentioned.
83297	46	41	46	42	Add - Kusahara, K., Reid, P., Williams, G.D., Massom, R., and Hasumi, H. 2018. An ocean-sea ice model study of the unprecedented Antarctic sea ice minimum in 2016. Environmental Research Letters, 13(8), DOI:10.1088/1748-9326/aad624. [Robert Massom, Australia]	Accepted. This is now included.
83299	46	45	46	45	Change "Before satellites" to "Before the modern continuous satellite passive-microwave time series (i.e., prior to 1978)" [Robert Massom, Australia]	Accepted. This has been revised.
42915	46	45	46	53	There is evidence (albeit low confidence) about ice extent over recent centuries from ice cores and marine cores, and I'm surprised it isn't even mentioned. Summarised in Thomas, E. R., C. S. Allen, J. Etourneau, A. C. F. King, M. Severi, V. H. L. Winton, J. Mueller, X. Crosta, and V. L. Peck (2019), Antarctic Sea Ice Proxies from Marine and Ice Core Archives Suitable for Reconstructing Sea Ice over the past 2000 Years, Geosciences, 9(12), doi:10.3390/geosciences9120506, and more genarally in Abram, N. J., E. W. Wolff, and M. A. J. Curran (2013), A review of sea ice proxy information from polar ice cores, Quat. Sci. Rev., 79, 168-183, doi:10.1016/j.quascirev.2013.01.011. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have now included the Thomas et al. (2019) reference in our discussion of sea-ice proxies from ice cores
245	46	46	11	11	For Schemm et al. (2018), I suggest to mention also the role of anticyclones and blocks and that the link is valid across seasons (typically from growth to retreat season and vice versa) [Sebastian Schemm, Switzerland]	Noted. This level of details is not warranted owing to space constraints.
247	46	46	19	21	Regionally, there is a clear trends between March-April-May (austral fall) sea-ice extend and cyclones and blocks during the same and the preceeding season, depending on the exact region of interest (compare Fig1b with Fig4a,b and Fig5a in Schemm et al. 2018: doi: 10.1029/2018GL079109). [Sebastian Schemm, Switzerland]	Accepted. This information is now added.
101905	46	46	46	46	"...large-scale decadal fluctuations in sea ice coverage caused by large-scale temperature and wind forcing." - A brief discussion of the re-occurrence of the Maud Rise polynya and its relation to multi-decadal variability as well as the 2016 sea ice decline should be included (Campbell et al. 2019, https://doi.org/10.1038/s41586-019-1294-0; Jena et al. 2019, https://doi.org/10.1029/2018GL081482) even though the detailed connections are not yet clear. Other parts of this chapter refer to this event and might be cross-referenced here. Campbell, E.C., Wilson, E.A., Moore, G.W.K. et al. Antarctic offshore polynyas linked to Southern Hemisphere climate anomalies. Nature 570, 319–325 (2019). https://doi.org/10.1038/s41586-019-1294-0. Jena, B., Ravichandran, M., & Turner, J. (2019). Recent reoccurrence of large open-ocean polynya on the Maud Rise seamount. Geophysical Research Letters, 46, 4320– 4329. https://doi.org/10.1029/2018GL081482 [IAPSO ECS group review, United States of America]	Accepted. These references are now included here, as is a link to section 9.2.3.2
65953	46	49	42	49	Suggest consistency in the term 'sea ice' with line 39 [Kushla Munro, Australia]	Noted. Sea ice is hyphenated when it refers to a third noun, otherwise it is not hyphenated. We have checked the consistent application of this rule.
39725	46	49			"Antarctic ocean" do you mean the Southern ocean? [TSU WGI, France]	Accepted. This has been corrected.
83559	46	51	46	52	There is a new circum-Antarctic study for the LIG by Chadwick, M., Allen, C.S., Sime, L.C., Hillenbrand, C.D., 2020. Analysing the timing of peak warming and minimum winter sea-ice extent in the Southern Ocean during MIS 5e. Quaternary Science Reviews 229, 106134, doi: https://doi.org/10.1016/j.quascirev.2019.106134 that could be added to the references here. [Antje H. L. Voelker, Portugal]	Accepted. This reference has now been added.
65955	46	52	42	53	Suggest deleting repeated word and parentheses in: (Figure 9.16h; (SIMIP Community, submitted, submitted)). Suggest reviewing this page for additional instances of this. [Kushla Munro, Australia]	Accepted. This has been corrected.
116859	46		46		redundancy with the assessment of Antarctic sea ice also in ch 2 and 3. Cross references needed. I am not always convinced by the logical flow of information in sections (paleo last and references to earlier reports last). [Valerie Masson-Delmotte, France]	Accepted. Cross references are now added, and the logical flow has been made more apparent
54481	47	1	47	1	The word "submitted" is repeated two times. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. This has been corrected.
26387	47	1	47	18	(Roach et al., submitted, submitted)->submitted twice [María Santolaria-Otín, France]	Accepted. This has been corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35325	47	1	47	18	Could the authors explain more the potential explanation behind the sudden loss of antarctic sea ice since 2015/2016? Maybe citing the recent paper by Turner et al. 2020 (Turner, J., Guarino, M. V., Arnatt, J., Jena, B., Marshall, G. J., Phillips, T., ... & Murphy, E. J. Recent Decrease of Summer Sea Ice in the Weddell Sea, Antarctica. Geophysical Research Letters, e2020GL087127.) [Etienne Pauthenet, France]	Accepted. This paper has now been added.
35327	47	1	47	18	Could the authors maybe add a figure showing the difference of antarctic sea ice coverage between CMIP6 and observations? Like the figure 4 of Roach et al 2020? It could help to understand better the discrepencies the authors are describing in this paragraph. [Etienne Pauthenet, France]	Rejected. Owing to the lack of space, we can unfortunately not include such figure.
1753	47	1	47	51	What are the conclusions/recommendations regarding future use of CMIP5 model applications to simulate Antarctic sea-ice coverage, thickness (Section 9.3.2.2, page 48), volume, and long-term decline? The poor performance of CMIP5 models to realistically simulate regional patterns and evolution of Antarctic sea ice relative to CMIP6 models would suggest limited utility for such applications. Consider adding recommendations at the end of line 51 on the viability and future use of CMIP5 models in Antarctic sea-ice assessment. [Michael Kennish, United States of America]	Noted. We unfortunately have insufficient understanding of possible routes forward to make such recommendations as part of AR6
54483	47	4	47	4	The word "submitted" is repeated two times. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. This has been corrected.
65957	47	5	48	24	Suggest rewording this section (9.3.2.1) to improve flow. For example: - page 45, line 26 starts off talking about large-scale variability, but does not say what the variability is. - Page 46, line 39 talks about a decline in sea ice since 2016 while page 47, line 32 talks about a positive trend in over the period 1979-2015. Suggest these periods be treated in chronological order and have an overarching introductory sentence or two. [Kushla Munro, Australia]	Noted. We have re-written this section to improve the logical flow. Rather than moving from one period to the next, we now more explicitly examine in turn the observational record, the model simulations, and the attribution of the observed changes
54485	47	8	47	8	The word "submitted" is repeated two times. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. This has been corrected.
16395	47	10	47	10	Comma after reference. [Julian Mak, China]	Accepted. This has been corrected.
54487	47	18	47	18	The word "submitted" is repeated two times. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. This has been corrected.
85301	47	20	47	20	Presumably sea-ice seasonal mean state biases are also strongly influenced by large scale SST biases caused by cloud errors? For example, see Hyder et al, 2008 & Bracegirdle et al, 2018, CMIP5 uncertainty in projected 21C change in southern westerlies significantly related to simulated historical sea ice area; strong link to strengthening and weak link to shift. J. Clim. 31, 195–211? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This paragraph now focuses on the mis-representation of the time evolution rather than the mean state.
46543	47	20	47	30	Should ice sheet / ocean interactions be discussed explicitly here? The work of Jeong et al. shows support that the explicit resolution of ice shelf melting in Earth system models (relative to ice shelf melting and its impacts being absent) impacts S. Ocean sea ice concentration and thickness in ways that are consistent with observations (i.e., increased sub-ice shelf melting could explain some of the observed S. Ocean sea ice expansion in recent decades). This is based on simulations using a fully coupled Earth system model (E3SM) that includes the impacts of ice-ocean coupling via the explict representation of ocean circulation (and heat and freshwater exchange) within Antarctic ice shelf cavities (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). [Stephen Price, United States of America]	Accepted. This process and the related paper are now included.
54489	47	27	47	28	Please, consider modifying the sentence "Also the low resolution of CMIP5 models and their lack of ability in representing eddies might cause shortcomings in the Southern Ocean,..." adding "processes representation" before the comma. Or alternatively: "Also the low resolution of CMIP5 models and their lack of ability in representing eddies might cause shortcomings in the understanding/representation of the Southern Ocean,...". [Maria del Pilar Bueno Rubial, Argentina]	Accepted. This sentence has been improved as suggested.
8997	47	27	47	29	The fundamentla limitations of models are clearly stated here. Great. But these limitations are NOT emphasized in the summary assessment. IPCC should state: the evoltuion of antartic sea ice is not captured by models for the following reasons. The fundamental physics that govern the change are ... We fail because. This goes for sea ice AND simulation of Antarctic ice mass balance. So I am asking the authors to properly reflect the state of knowledge in the overall assessment. [Eric Rignot, United States of America]	Noted. We emphasize in the executive summary that models currently do not capture the evolution because they do not simulate the regional processes well. For space constraints, we cannot be more explicit in the ES.
52063	47	27	47	30	I would be careful saying that increasing resolution from CMIP5 would likely improve the SO. Certainly the opposite is true for Met Office models where the 1/4degree CMIP6 model has much higher SO SST biases than the 1 degree CMIP5 model. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We are carefully phrasing this as "higher resolution might help"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54491	47	34	47	34	Consider replacing "and" by a comma in "and proxy data...". [Maria del Pilar Bueno Rubial, Argentina]	Rejected. We kept this to read "reconstructions from early observations and proxy data" as we feel this is clearer
132593	47	35	47	35	Prehaps cite Zhange et al. 2019 (doi: 10.1038/s41558-018-0350-3) here as well. They aregue that the Antarctic sea ice increase (and pattern of changes) could be a response to convection changes coming out of the 1970s. [Kyle Armour, United States of America]	Accepted. This paper is now included.
101907	47	40	47	45	"These centennial trends are consistent with simulations from CMIP5 models .... Nonetheless, without accurate simulations of observed changes possible contribution of anthropogenic forcing to the regional changes remains unclear ...and there is low confidence in the attribution to internal variability ..." - It would be good to refer to the study by Zhang et al. (2019) that argue for a strong influence of internal, multi-decadal variability on the sea-ice trends. This includes multi-decadal variations in open-ocean deep convection associated with the Maud Rise polynya. (Zhang, L., Delworth, T.L., Cooke, W. et al. Natural variability of Southern Ocean convection as a driver of observed climate trends. Nature Clim Change 9, 59–65 (2019). https://doi.org/10.1038/s41558-018-0350-3) [IAPSO ECS group review, United States of America]	Accepted. This paper is now included.
21113	47	47	47	48	I do not understand this paragraph, is it supposed to be CMIP6 instead of CMIP5? [AMNA JRRAR, Jordan]	Accepted. This has been updated to now refer to CMIP6
67221	47	47	47	48	This is one of the many examples in this chapter where references is made to AR5 but SROCC is ignored as if it did not exist. What is new/different here since SROCC? [Regine Hock, United States of America]	Accepted. SROCC findings are now specifically discussed.
82941	47	50	47	50	I suggest specifying this by writing 21st century or the ongoing (or present) century. [Sebastian Gerland, Norway]	Noted. This part of the sentence has been removed.
116861	47		47		why not refer to SROCC here? [Valerie Masson-Delmotte, France]	Accepted. The reference to SROCC is now included.
83303	48	9	48	10	Add: Sturm, M. and R.A. Massom. 2017. Snow in the Sea Ice System: Friend or Foe? In D. Thomas (editor): Sea Ice. 3rd Edition, Wiley-Blackwell, New York (USA) & Oxford (UK), pp. 65-109. DOI:10.1002/9781118778371.ch3. [Robert Massom, Australia]	Noted. This reference is less specific than the two references cited already, so we did not include it.
83301	48	11	48	11	Add: Massom, R.A., Stammerjohn, S.E., Smith, R.C., Pook, M.J., Iannuzzi, R.A., Adams, N., Martinson, D.G., Vernet, M., Fraser, W.R., Quetin, L.B., Ross, R.M., Massom, Y. and Krouse, H.R. 2006. Extreme anomalous atmospheric circulation in the West Antarctic Peninsula region in austral spring and summer 2001/2, and its profound impact on sea ice and biota. Journal of Climate, 19, 3544-3571. [Robert Massom, Australia]	Accepted. This paper is now included.
83305	48	11	48	11	After "(Massom et al., 2006; Williams et al., 2015)", add - "Regarding snow thickness, Antarctic observations are even more sparse in space and time than in the Arctic, and there is currently no large-scale baseline dataset against which to gauge whether snow thickness is changing across Southern Ocean sea ice (Webster et al., 2018). ADD: Webster, M., S. Gerland, M. Holland, E. Hunke, R. Kwok, O. Lecomte, R. Massom, D. Perovich and M. Sturm. 2018. Snow in the changing sea-ice systems. Nature Climate Change, 8, 946-953. doi.org/10.1038/s41558-018-0286-7. [Robert Massom, Australia]	Accepted. This information is now added.
67237	48	13	48	13	Something is missing here: the direction, magnitude, trend ??? There will always be some evolution so this needs to be specified what variable the low confidence refers to. [Regine Hock, United States of America]	Accepted. This now reads "long-term trend"
109065	48	13	48	16	Is it not clear with the way the sentence is written, how observational data on sea-ice thickness (ship-board and remote sensing), is biased low, when it is "consistent" with reanalysis and AV obs. [Chaincy Kuo, United States of America]	Accepted. This now reads "compared to" rather than "consistent"
83307	48	19	48	20	ADD: (1) Kusahara, K., Williams, G.D., Massom, R., Reid, P. and Hasumi, H. 2019. Spatiotemporal dependence of Antarctic sea ice variability to dynamic and thermodynamic forcing: A coupled ocean-sea ice model study. Climate Dynamics, 52(7-8), 3791-3807, doi.org/10.1007/s00382-018-4348-3. and (2) Schroeter, S., W. Hobbs, N.L. Bindoff, R. Massom and R. Matear. 2018. Drivers of Antarctic sea ice volume change in CMIP5 models. Journal of Geophysical Research – Oceans, 123(11), 7914-7938 https://doi.org/10.1029/2018JC014177. [Robert Massom, Australia]	Noted. The first paper does not discuss the long-term trend discussed here. The second paper has already been cited.
2987	48	19	48	22	I recommend removing these conclusions due to the large regional uncertaintly of the sea-ice volume simulated by ocean reanalyses. A more recent study found that an extensive data set based on nine ocean reanalyses is not sufficient to provide trustful reconstructions and estimations of past Antarctic sea-ice thickness. Reference: Uotila, P., Goosse, H., Haines, K., Chevallier, M., Barthélemy, A., Bricaud, C., et al. (2019). An assessment of ten ocean reanalyses in the polar regions. Climate Dynamics, 52(3–4), 1613–1650. https://doi.org/10.1007/s00382-018-4242-z. [Petteri Uotila, Finland]	Accepted in principle. Rather than removing these conclusions, we have added this reference and discussed its findings.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82943	48	24	48	24	I suggest considering to mention new findings for Antarctic fast ice thickness (9 year time series), published by Arndt et al. (2019, TCD, https://doi.org/10.5194/tc-2019-293). [Sebastian Gerland, Norway]	Rejected. These findings are too regional to be assessed here.
83315	48	27	67	40	This Ice Sheet secion is very long compared to other sections. Ensure that there's no repetition. [Robert Massom, Australia]	accepted, Removed unnecessary duplication with SROCC.
73843	48	27	73	34	very good and comprehensive description in the state of the art [Veijo Pohjola, Sweden]	Noted. Thanks for this from the author team.
91093	48	27	74	34	This is a suggestion which can be taken or ignored. The figures of mass trends for the ice sheets and GIC are all shown as cumulative trends. That's OK for some purposes but it i) hides the inter annual variabilty, ii) makes interpreting trends very hard (e.g. the deceleration in mass loss from the GrIS after 2012 which is almost impossible to see in Fig 9.18) and iii) makes comparing estimates of limited value because it's an integral. Integrals smooth everything, differentials do the opposite. If you plotted (in a separate figure say) the annual trends for GIC and the ice sheets, it would make it clear to the eye/reader how large inter annual varability is and how meaningful a trend is given that variability. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	accepted, additional panels for figure 9.16 in response to this comments and a table with decadal trends for period 1992-2020 added
103809	48	29	48	32	Should this statement not have an associated confidence level ? [Philippe Tulkens, Belgium]	Not applicable, text removed
80845	48	29	48	32	Should this statement not have an associated confidence level ? [Louise Sandberg Sørensen, Denmark]	Not applicable, text removed
82105	48	29	48	37	Needs to me some context here of internal variability rather than implying that acceleration is a permanent state. Holland et al 2019 make a stong point of internal variability Particularly of the warm watersd of the Ammundsen Sea. The 'observations' of mass loss need too be put in context. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised, this section is removed and text in subsection discuss variability
88259	48	29	48	37	Reference could be made to chapter 2 for observed trends in ice sheet mass balance. [Sharon Smith, Canada]	Accepted, references to Ch2 added reference
35785	48	29	48	37	Place likelihood or confidence language at the end of the opening sentence of this paragraph which presents a firm, clear assesssment of ice sheet loss rates and their acceleration. [Michael Oppenheimer, United States of America]	Not applicable, text removed
61637	48	29	49	4	There are many duplications in the references that are used in this part of the text. See my specific comments. These should be carefully checked and updated before the publication. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable, text removed
15805	48	30	48	30	Replace "due to atmospheric, oceanic, and dynamic processes," with "due to atmospheric, oceanic forcings and internal, ice dynamic processes" [Olga Sergienko, United States of America]	Not applicable, text removed
115455	48	30	48	30	"dynamic processes" can be related to atmoshpere, ocean, ice sheets, etc. More specificity is needed. [Robert DeConto, United States of America]	Not applicable, text removed
90469	48	31	48	31	"sea level rise" => "sea-level rise"? [Holly Kyeore Han, Canada]	Not applicable, text removed, consistency in use of - will be ensured
61611	48	31	48	31	You cite Bamber et al. (2018a) here, which is the dataset. I would rather cite the article, e.g. Bamber et al. (2018b), which is more relevant here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
61613	48	31	48	32	I recommend adding Hanna et al. (2020) in the references here. This is a review of recent observational estimates of ice-sheet and glacier mass balance since AR5. Hanna, E. et al. (2020). Mass balance of the ice sheets and glaciers – Progress since AR5 and challenges. Earth-Science Reviews, https://doi.org/10.1016/j.earscirev.2019.102976. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
61501	48	32	48	32	Add Smith et al., 2020, Science, Pervasive ice sheet mass loss reflects competing ocean and atmosphere processes [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
1755	48	32	48	34	It would be informative to add on line 37 the main local and global climate responses resulting from freshwater input to the oceans from ice sheet melting. [Michael Kennish, United States of America]	not applicable, text removed
69717	48	32	48	37	The first part of this sentence implies there is confidence that climate responses occur in reality, wherea the second part of this sentence suggests this may be merely a model phenomenon, which casts doubt on the expression of confidence. [Matthew Hoffman, United States of America]	not applicable, text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61679	48	33	48	34	In the text it's indicated that freshwater input to the oceans from ice sheet melting triggers local climate responses, using Figure 9.10 as an example. Firstly it takes time to find the good part of figure 9.10 corresponding to the local impact of freshwater. It will be usefull to precise bottom right on figure 9.10 citation. Second, the term local to speak about AMOC, which is one of Earth's major ocean circulation systems, seems ambiguous to me. What's exactly the definition/size of local? To avoid ambiguity in the substance of this introduction, it could be appropriated to move Figure 9.10 citation with the authors' list citation after "Global climate responses". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
90471	48	33	48	37	There seems to be a number of missing commas between sentences. [Holly Kyeore Han, Canada]	not applicable, text removed
115457	48	35	48	35	also, in review: Rogstad, S., Condron, A., DeConto, R. & Pollard, D. Future climate response to Antarctic ice sheet melt caused by anthropogenic warming. Science Advances Preprint at http://arxiv.org/abs/2005.09731 (2020). [Robert DeConto, United States of America]	Accepted, reference included in assessment
61535	48	35	48	36	These papers reference processes, but most consider the magnitude (and the change in magnitude) of the freshwater input to be of great importance to the results. Incorporating this into the sentence would be more true to these situations and less vague than just the "processes" captured. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
64367	48	35	48	36	ambiguous formulation the magnitude is always depending on what you put in a model? Unclear what you want to say, please rephrase [roderik van de wal, Netherlands]	not applicable, text removed
46497	48	35	48	36	I think it would be more clear here if it "... may depend on processes incorporated in these models" was written as " ... may depend on processes incorporated in, approximated by, or missing from these models" (to make it clear that many of the refs previously mentioned (e.g., Bronselaer et al., 2018) are still only accounting for important processes in very crude ways). [Stephen Price, United States of America]	not applicable, text removed
32415	48	42	48	46	use of hyphens in value ranges inconsistent (long vs short) [Olaf Eisen, Germany]	Editorial. Copyediting of report makes hyphens consistent.
61533	48	45	48	45	The IMBIE Team paper refers the period from 1992-2017, not through 2018. The Bamber et al. paper refers to the period from 1992-2016, so this should be explicitly stated, just as in the previous sentence the different time periods are differentiated for the two studies. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	noted, periods clarified and made consistent
61681	48	50	48	52	I found the literature justifying this sentence incomplete. Goelzer et al., 2018 present outputs from a large range of Greenland ice sheet model, including different ways of representing ice-dynamics processes only. Pattyn's and Turner et al., 2017 papers present impacts of the environmental processes on the Antarctic ice sheet. It would be fair to select as much paper from Antarctica as from Greenland to avoid reader think only Antarctic ice sheet is sensitive to environmental processes.There are many papers about Greenland ice sheet interactions with environmental processes such as Huybrechts et al., 2002; Alley and Joughin, 2012; Edwards et al., 2014; Vizcaíno et al., 2015; Fyke et al., 2018; Le clec'h et al., 2019. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
46499	48	50	48	54	The references chosen to support this statement seem rather arbitrary. One of them (Goelzer et al., 2017) seems to be to a model intercomparison description and one of them is to a review paper on ice sheet models (Pattyn et al., 2017), which both seem at least partly appropriate. Pattyn (2007) however is a reference to a single ice sheet model, and one that is not particularly representative of the ice sheet modeling communities best efforts. Similarly, Tuner et al. (2017) does not even seem to be a paper that explicilty includes or discusses ice sheet modeling. I would suggest that an author in this chapter who is more familiar with review and or community-effort-based papers choose more appropriate references to summarize the efforts of the ice sheet modeling community (and similarly, where gaps still remain). One possibly relevant review paper not mentioned here is Fyke et al. (Rev. Geophys., 56, 2018; on p. 133, line 59 of Ch. 9). [Stephen Price, United States of America]	not applicable, Text deleted. Cited Fyke et al. in model evaluation section.
15207	48	50	49	4	The issue of ice sheet model ability to represent dynamic processes is vital to interpretation of IPCC GMSL likelihood statements. Given that there is widespread agreement in the modelling community that the models are not capable of representing all key processes, why not give the statement about model limitations a confidence assessment? Can't we say that there is very high or high confidence that models underestimate future GMSL due to their limitations? Saying this would be a clear way of expressing what the community knows, and help governments choose whether to use model-based estimates in planning, or to plan for the long-tail risk of higher GMSL. [Simon Donner, Canada]	not applicable, text removed, model assessment section included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82107	48	50	49	4	If the models have limitations then the deficiencies in Expert Judgement exercises need also to be expressed. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	not applicable, text removed
88627	48	52	48	52	The references Goelzer et al., 2017, Goelzer et al., 2018a and Goelzer et al., 2018b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88629	48	52	48	52	Which is the correct: Turner et al., 2017a or Turner et al., 2017b?. [Rosemary Vieira, Brazil]	not applicable, text removed
61537	48	52	48	52	Goelzer et al. 2018 has two identical citations in the bibliography at the end of the chapter. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61539	48	52	48	52	The Turner reference has two options in the bibliography. I think 2017a is the paper meant to be cited here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
64371	48	52	48	52	I think Goelzer 2018 should be H Goelzer, A Robinson, H. Serroussi, R S W van de Wal, Recent progress in Greenland ice sheet modelling, Current Climate Change Reports, https://doi.org/10.1007/s40641-017-0073-y. [roderik van de wal, Netherlands]	not applicable, text removed
90491	48	52	48	52	add a label ("a" or "b") on the reference Goelzer et al., 2018 [Holly Kyeore Han, Canada]	not applicable, text removed
61615	48	52	48	52	There is an error in the References: Goelzer et al. (2018) is repeated three times (2017, 2018a and 2018b), see page 135 L45-53. Same for Goelzer et al. (2016a, b), which are similar, see page 135 L38-41. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61617	48	52	48	52	Instead of Goelzer et al. (2018), who focus on the ISMIP6 intercomparison, I would rather cite Goelzer et al. (2017), which is a review paper about the recent progress in Greenland Ice Sheet modeling. Goelzer, H. et al. (2017). Recent progress in Greenland Ice Sheet modelling, Current Climate Change Reports, https://doi.org/10.1007/s40641-017-0073-y. This is more appropriate in the context of this sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
61619	48	52	48	52	Is it Turner et al. (2017a) or Turner et al. (2017b)? I guess 2017a is the right answer. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
61541	48	52	48	53	This paper doesn't only refer to simplifications of physics, numerical schemes, or spatial resolution, but also variation of "model parameters, input forcing, initialization techniques, and physical processes." Yes, limitations in physics, numerical schemes, and spatial resolution do introduce errors in these models, but it is also the differences in parameters, forcing, initialization, and physics that cause you to look at intercomparison studies and not at each model result individually for the IPCC report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
14703	48	52	48	53	Note that "limitations or simplifications in terms of physics, numerical schemes, or spatial resolution, still persist" is a problem also common to models of all other climate components (ocean, atmosphere, land, sea ice, etc.). E.g. see ocean and sea ice components of Chapter 9 for analagous disclaimers. If there is a cross-cutting text that generally describes climate and climate component model limitations somewhere in WG1 text, cite this text here (and conversely, ensure ice sheet modelling challenges are noted there, as appropriate). [Jeremy Fyke, Canada]	not applicable, text removed
61621	48	53	48	54	Seroussi et al. (2019) is about the impact of ice-sheet model initialization on the Antarctic Ice Sheet dynamic contribution to sea-level rise. I think that this reference is too specific relative to the broader context of this sentence. I would rather make references to Pattyn et al. (2017) for the Antarctic Ice Sheet and Goelzer et al. (2017) for the Greenland Ice Sheet. The former reference is already cited earlier in the sentence, so I would move it to the end instead. The latter reference is Goelzer, H. et al. (2017). Recent progress in Greenland Ice Sheet modelling, Current Climate Change Reports, https://doi.org/10.1007/s40641-017-0073-y. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
88631	49	1	49	1	The references Goelzer et al., 2017, Goelzer et al., 2018a and Goelzer et al., 2018b are the same. [Rosemary Vieira, Brazil]	not applicable, text removed
61623	49	1	49	1	Nowicki et al. (2016) is not in Discussion anymore, see https://doi.org/10.5194/gmd-9-4521-2016. The reference should be updated, see page 158 L11-13. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61625	49	1	49	1	Goelzer et al. (2018a) should be Goelzer et al. (2018) as there is a duplication in the reference list. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61627	49	1	49	1	Seroussi et al. (2019) is not in Discussion anymore, see https://doi.org/10.5194/tc-13-1441-2019. The reference should be updated, see page 167 L27-28. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61629	49	1	49	2	Goelzer et al. (submitted) should be Goelzer et al. (2020), available in The Cryosphere Discussions. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115459	49	2	49	2	Why is Levermann et al., (submitted) listed here, if LARMIP2 is not being used in the final analysis? While not perfect, and SMB is not considered in LARMIP2, I'm not sure I see enough justification here not to incorporate LARMIP somehow. That's a more general comment. [Robert DeConto, United States of America]	Noted. Text removed here but LARMIP2 included in assessment
89407	49	2	49	2	Change to: Levermann et al., 2020, (+ update in references: A. Levermann, R. Winkelmann, T. Albrecht, J. van Breedam, H. Goelzer, N.R. Golledge, R. Greve, J. Jordan, G. Leguy, D. Martin, M. Morlighem, F. Pattyn, D. Pollard, A. Quiquet, C. Rodehacke, H. Seroussi, J. Sutter, T. Zhang, R. DeConto, Ch. Dumas, S. Fuzuki, J. Garbe, H. Gudmundsson, M. Hoffman, A. Humbert, P. Huybrechts, T. Kleiner, W. Lipscomb, E. Ng, M. Perego, S.F. Price, N.-J. Schlegel, S. Sun, R.S.W. van de Wal: Projecting Antarctica's contribution to future sea level rise from basal ice-shelf melt using linear response functions of 16 ice sheet models (LARMIP-2), Earth System Dynamics (2020), DOI: 10.5194/esd-11-35-2020.) [Ricarda Winkelmann, Germany]	Editorial - copyedit to be completed prior to publication
61631	49	2	49	2	There is an error in the References: Levermann et al. (submitted) is repeated twice (a and b), see page 148 L42-47. So, it should be Levermann et al. (submitted) in the main text. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61633	49	2	49	2	Nowicki et al. (submitted, b) should be updated into Nowicki et al. (2020) as it is now in The Cryosphere Discussions. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61635	49	2	49	3	Seroussi et al. (submitted) should be updated into Seroussi et al. (2020) as it is now in The Cryosphere Discussions. By the way, this reference appears twice in the reference list (see page 167 L16-17 and L24-26). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
129471	49	3			Structured expert judgment exercises are not physically based exercises, and really should not be included in AR6. [Trigg Talley, United States of America]	noted, references are made to SEJ in assessment for projections beyond 2100
34943	49	7	49	49	The SOD indicates Greenland Ice Sheet loss of 3800Gt between 1992 and 2018; however it fails to state that this amounts to less than 1% of its total ice mass, hence the figure must be regarded as falling within natural variability. See general comment #8 above. [Jim O'Brien, Ireland]	noted. The fraction of change is a separate to the issue of attribution to natural variability or human causes. And aside from this, the combined sea level changes outlined in this chapter (both observed and projected) are sufficient to alter regional sea level and coastal flooding so remain relevant for scientific study and informing policymakers.
115463	49	7	51	2	Some of this section on Greenland observations jumps around in time. Consider improving the tempral flow. [Robert DeConto, United States of America]	taken into account, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129473	49	7	110	31	Measurements made by the GRACE mission are relevant to Section 9.4, Ice Sheets. For both the Greenland and the Antarctic Ice Sheets, satellite measurements of changes in the ice mass near the edges of the sheets are being used to understand the processes leading to the ice sheet melting that is a main contribution to sea level rise. The satellite gravimetry measurements made by GRACE are one of the three types of measurements discussed in Section 9.4 for determining mass changes in the ice sheets, and a number of the papers listed in the references for that section discuss the GRACE measurements. Since GRACE ended its operations in 2017, a GRACE Follow-On Mission was launched jointly by NASA and the German agency DLR. In addition to the microwave link between the two satellites, GRACE Follow-On has a laser interferometric link, which was included as a demonstration of the much improved accuracy that could be achieved for measuring changes in the satellite separation in this way. Since changes in the satellite separation are the main observable for determining changes in the geopotential height along the orbit, the excellent results that have been obtained by the laser interferometry are substantially improving the measurements of the very short wavelength mass changes with time at the glacier edges. At longer wavelengths the results unfortunately are limited by noise in the accelerometers aboard the satellite that measure the effect of non-gravitational forces on the satellites. This noise source also could be strongly reduced in future earth gravity missions by flying the satellites in a very drag-free mode, but the success of that approach wasn't demonstrated early enough to be included in the GRACE Follow-On mission. The expected improvement in measuring ice sheet mass changes with GRACE Follow-On are mentioned in a couple of the papers cited in Section 9.4. However, none of them was written recently enough to mention the results now being obtained. Thus, suggest adding a subsection containing several references to this new mission. In particular, a paper by Kornfeld et al. that gives a quite complete description of the design, launch, and early operation of GRACE Follow-On should be included. And a paper by Abich et al. describing the much improved accuracy of the early laser interferometry results from the mission would be of strong interest. In addition, by the time that the WGI AR6 literature cutoff date of 31 January 2021, there will be a few new papers published that discuss mass changes at the Greenland and Antarctic Ice Sheet edges that include improved results from the GRACE Follow-On Mission. Citations: Kornfeld, R. P., et al., (2019), ""GRACE-FO: the gravity recovery and climate experiment follow-on mission"", J. Spacecraft Rockets 56: 931-951.	taken into account, suggested references added to assessment
67263	49	9	49	9	Remove 'and model evaluation and make it 9,9.1.2 later (page 52, line 4). The section is long and observations and models are distinctly different topics. Same later for Antarctica [Regine Hock, United States of America]	accepted, text revised
96971	49	9	50	46	A shorter version of this is found in Ch2 2.3.2.4.2, please streamline. [Nicole Wilke, Germany]	accepted, coordination with Ch2 improved consistency between chapters
96973	49	9	50	46	Most of these findings have already be reported in the SROCC, please clarify what is new or provide references to the SROCC. [Nicole Wilke, Germany]	accepted, Removed unnecessary duplication with SROCC and clarified advances since SROCC.
40751	49	9			section 9.4.1.1: this section currently reads more as a review of the literature rather than an assessment of our current understanding of the literature. [TSU WGI, France]	accepted, text revised
22581	49	11	49	49	This is a very long paragraph and mixes several things. Could it be split into several smaller paragraphs to make for an easier read? [Peter Thorne, Ireland]	accepted, text revised
83309	49	11	49	49	This paragraph is far too long and difficult to follow. [Robert Massom, Australia]	accepted, text revised
67239	49	11	49	49	long paragraph! Can it be broken up? [Regine Hock, United States of America]	accepted, text revised
67243	49	11	49	49	This is framed essentially entrely around what has changed since AR5 as if SROCC did not exist. Shouldn't this be framed around what has changed / been added since SROCC. As mentioend earlier this is a problem with the entire chapter. [Regine Hock, United States of America]	Accepted, Removed unnecessary duplication with SROCC and clarified advances since SROCC.
82111	49	11	49	49	An over emphasis on figures of mass loss repeating those in Ch 2. The mechanisms of mass loss variability should instead be emphasised (NAO) as in Bevis et al. 2019 & Sherman et al. 2020 (and others associated with NAO induced cloud changes). The point needs to be simply made that one cannot just extrapolate a trend, or at least not assume continued acceleration, since more NAO+ will have a negative feedback on melt. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised and sections on drivers added
107463	49	11	49	49	Extremely long first paragraph for this section. Suggest splitting to have an introductory first paragraph, and then continue with further detail [Jennifer Walker, United States of America]	accepted, text revised
77807	49	11	49	49	This paragraph would be more digestible if split into two or more. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
69627	49	11	52	2	this section is really nicely written and easy to read, but isn't quite IPCC-style 'assessment' - could it be tightened up? [Nicholas Golledge, New Zealand]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107267	49	11			this is a long, dense paragraph that is hard going to read. Suggest hat it is broken up into easier chunks. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
106635	49	12	49	12	over the past 25 years: I find a bit weird to use this phrase here. As there is a difference of 6 years between AR5 and SROCC, it makes little sense to talk over the past 25 years (what is the reference date from which are the pas 25 years considered). [Kevin Bulthuis, United States of America]	accepted, Edited to more closely reflect SROCC assessment.
67261	49	12	49	18	SROCC assessed the literature since AR5, i.e. this is reduncdant. What is new since SROCC? This is one of many example throughout the entirer chapter. [Regine Hock, United States of America]	accepted, Removed unnecessary duplication with SROCC and clarified advances since SROCC.
64373	49	13	49	13	outdated statement to repeat AR5 whereas SROCC has a more up to date statement on the increased rates don't jump back to AR5 if that is not needed [roderik van de wal, Netherlands]	Accepted, Removed unnecessary duplication with SROCC and clarified advances since SROCC.
61639	49	13	49	13	In the SROCC, the Greenland Ice Sheet mass loss over the same time period is 263 Gt/yr (so 22% higher than the AR5 estimate) and comes from the recent paper by Bamber et al. (2018). Shouldn't you use the updated figure, which is supposed to be more precise due to the new datasets available? If you don't replace the AR5 figure by the SROCC figure, I think it is necessary to also add the SROCC figure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised, numbers replaced new IMBIE (2019) results.
61641	49	13	49	13	According to the SROCC, the increase in Greenland Ice Sheet mass loss between 1992-2001 and 2002-2011 is 33-fold (263 Gt/yr in 2002-2011 divided by 8 Gt/yr in 1992-2001). Due to the relatively high interannual variability in the mass loss of the 1992-2001 period (expressed by the standard deviation), is it really necessary to emphasize the 6-fold increase, which contains a lot of uncertainty? I suggest to replace '6-fold' by 'substantially'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
98543	49	13	49	13	6-fold -> six-fold [Miren Vizcaino, Netherlands]	accepted, text revised
32417	49	13	49	14	Chapter 2 of what, AR5, SROCC, AR6? [Olaf Eisen, Germany]	accepted, text Clarified.
90473	49	14	49	14	a comma after "In Chapter 2 (Section 2.3.2.4)" ? [Holly Kyeore Han, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
103811	49	14	49	14	It could be good with a very short recap here of what's in Sect 2.3.2.4. [Philippe Tulkens, Belgium]	taken into account, reference to Ch2 made
80847	49	14	49	14	It could be good with a very short recap here of what's in Sect 2.3.2.4. [Louise Sandberg Sørensen, Denmark]	taken into account, reference to Ch2 made
45345	49	18	49	18	change 1984 to 1987 instead [Kristian Kjelden, Denmark]	noted, text revised
61543	49	18	49	18	I'm not sure what "modelling and combination of methods" refers to. If it means synthesizing data from these different methods, then this is redundant since this is mentioned on line 20. If it means something else (related to modeling) this should be explicitly stated. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
33441	49	19			Describe abbreviation when first cited: GNSS. [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
49979	49	20	49	23	Here and elsewhere, please include the citation of Hamlington et al. (2020, submitted in 2019) which is a NASA sea-levelchange team review paper placing modern sea-level changes in the context of existing observations: Hamlington, B. D., Gardner, A. S., Ivins, E., Lenaerts, J. T. M., Reager, J. T., Trossman, D. S., et al (2020). Understanding of Contemporary Regional Sea-level Change and the Implications for the Future. Reviews of Geophysics, 58, e2019RG000672. https://doi.org/10.1029/2019RG000672 [Daniel Gilford, United States of America]	accepted, suggested reference added to assessment
103813	49	20	49	23	Relevant paper to include: https://doi.org/10.3390/rs11121407 [Philippe Tulkens, Belgium]	accepted, suggested reference added to assessment
80849	49	20	49	23	Relevant paper to include: https://doi.org/10.3390/rs11121407 [Louise Sandberg Sørensen, Denmark]	accepted, suggested reference added to assessment
15807	49	25	49	25	Replace "10 models of surface mass balance" with "10 models of surface processs" [Olga Sergienko, United States of America]	not applicable, text removed
88017	49	26	49	27	You may considerremoving or rephrasing the sentence in view of the more explicit statement of the same kind in lines 42-44, same page [Georg Kaser, Austria]	accepted, text removed
22585	49	27	49	27	Is this confidence statement necessary? Also, where does the confidence arise from? There is no obvious traceable basis that underlies this assessment. [Peter Thorne, Ireland]	accepted, text removed
107261	49	27			year to year variability is large - need to quanitty this statement perhaps as fraction of accumulated trend since 1990s [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
106637	49	28	48	28	high melt rates: I assume the authors refer to surface melt rates. Maybe this could be written more explicitly. [Kevin Bulthuis, United States of America]	accepted, text revised
103815	49	28	49	28	97% is provided without incertainties. [Philippe Tulkens, Belgium]	accepted, text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80851	49	28	49	28	97% is provided without incertainties. [Louise Sandberg Sørensen, Denmark]	accepted, text removed
15809	49	28	49	29	Replace "high melt rates" with "high surface melt rates" [Olga Sergienko, United States of America]	accepted, text revised
15811	49	30	49	30	Replace "satellite data" with "satellite observations" [Olga Sergienko, United States of America]	accepted, text revised
61685	49	30	49	32	It is indicated that the peak mass loss in 2012 is at 324 ± 70 Gt then in the next sentence, the summer melt of 2012 led to a mass loss of at 627 ± 89 Gt, which is twice bigger than the first peak value. It will be less ambiguous to specified that the 324 ± 70 Gt mass loss in 2012 is an annual mean while the 627 ± 89 Gt is a peak in the same year. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
15813	49	30	49	49	Replace "melt" with "surface melt" [Olga Sergienko, United States of America]	accepted, text revised
64375	49	31	49	31	leave out the value for 2011 it confuses as you have focus on 2010 and 2012 [roderik van de wal, Netherlands]	accepted, text revised
32419	49	31	49	31	PLACEHOLDER? Unclear which values to use now. Will there be a third order draft with so many placeholders? [Olaf Eisen, Germany]	noted. Many papers were still being finalised at the time of writing. The assessments have now been updated.
61683	49	31	49	31	Is there any reason why the peak mass loss in 2011 given in this chapter (335 ± 62 Gt yr−1) is different from the 345 ± 66 Gt yr−1 explicitly written in the IMBIE Team, 2019 paper? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text revised and numbers updated
80481	49	31	49	35	It is difficult to understand how the number of 627 Gt matches up with the 324 Gt specified just before for 2012. If the 627 was surface melt, then I could understand that part of it is refrozen and compensated by accumulation. But here 627 Gt is given as summer mass loss, that I would interpret as runoff. [Heiko Goelzer, Belgium]	accepted, text revised
64377	49	31	50	6	seems largely repeating content given in SROCC, please shorten [roderik van de wal, Netherlands]	accepted, removed unnecessary duplication with SROCC and clarified advances since SROCC.
84865	49	32	49	32	The number 627 ± 89 Gt for summer mass change is provided cited from Tedesco et al. (2013), but the reference mentions 628 ± 96 Gt (see Table 3 in Tedesco et al, 2013) [Jan Wuite, Austria]	accepted, text revised
82109	49	32	49	32	A summer 'mass loss' of 672Gt appears to be contradictory. If the annual mean loss was reduced due to refreeze, then this figure is not actually mass lost from the ice sheet. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
18111	49	32	49	33	9.4.1. It is not clearly stated if you are talking about melt or mass loss during summer 2012 (627Gt), since in the next sentence, the refreezing of much of the melt is mentioned. Did the 627Gt discharge to the ocean? [Thorsten Seehaus, Germany]	accepted, text revised
45333	49	32	49	34	This line is misleading since it implies that all snow that melted equates lost mass and then goes on to state that some of the mass loss was regained through refreezing. Should be rephrased. [Nanna B Karlsson, Denmark]	accepted, text revised
107269	49	32			not certain why so much discussion of 2012 is required. Reads like a review rather than an assessent. Hos does this discussion move the assessment forward? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
107263	49	32			discussion of 2012 confused mass loss of 627 implies that mass has indeed been lost so how can refreezing then reduce this loss so that the annual value is lower? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
82119	49	34	49	36	Is the impact of internal melt layers on radar altimetry actually relevant here? Remove or move to Ch2. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text removed
103817	49	35	49	36	It does not seem that using RA or LA has become more complicated. There are ways to handle this signal in the time series. But it would be correct to state that 'The 2012 event resulted in melt-generated ice layers in the firn that could be observed as a jump in elevation by radar altimeters.' [Philippe Tulkens, Belgium]	accepted, text removed
80853	49	35	49	36	I wouldn't say the using RA or LA has become more complicated. There are ways to handle this signal in the time series. But it would be correct to state that 'The 2012 event resulted in melt-generated ice layers in the firn that could be observed as a jump in elevation by radar altimeters.' [Louise Sandberg Sørensen, Denmark]	accepted, text removed
46465	49	36	49	36	The 2012 event also generated ice labs decreasing the ice sheet meltwater retention capacity according to MacFerrin, M., Machguth, H., van As, D., Charalampidis, C., Stevens, C., Heilig, A., Vandecrux, B., Langen, P., Mottram, R., Fettweis, X., van den Broeke, M., Pfeffer, W., Moussavi, M., & Abdalati, W. (2019). Rapid expansion of Greenland’s lowpermeability ice slabs. Nature, 573. [Xavier Fettweis, Belgium]	accepted, suggested reference added to assessment

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46467	49	36	49	36	Update (Tedesco and fettweis 2019) to Tedesco, M. and Fettweis, X.: Unprecedented atmospheric conditions (1948–2019) drive the 2019 exceptional melting season over the Greenland ice sheet, The Cryosphere, 14, 1209–1223, https://doi.org/10.5194/tc-14-1209-2020, 2020. [Xavier Fettweis, Belgium]	Editorial - copyedit to be completed prior to publication
103819	49	36	49	36	Suggested papers to include : 10.5194/tc-10-2953-2016 and 10.1016/j.rse.2016.12.012 [Philippe Tulkens, Belgium]	noted, text removed, suggested papers are per-SROCC
80855	49	36	49	36	Suggested papers to include : 10.5194/tc-10-2953-2016 and 10.1016/j.rse.2016.12.012 [Louise Sandberg Sørensen, Denmark]	noted, text removed, suggested papers are per-SROCC
61687	49	36	49	38	It will be usefull to provide the mean mass loss over the 2013-2018 in order to evaluate the meaning of "lower" from this sentence: "Mass losses between 2013 and 2018 were lower than the preceding few years". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
80483	49	38	49	41	The notion of "enhanced melt-albedo feedback" is taken out of context compared to the original information in Tedesco and Fettweis, 2019, now 2020), see below. What they describe is that the melt–albedo feedback is enhanced by the anticyclonic conditions and low albedo. "The summer of 2019 was characterized by an exceptional persistence of anticyclonic conditions that, in conjunction with low albedo associated with reduced snowfall in summer, enhanced the melt–albedo feedback by promoting the absorption of solar radiation and favored advection of warm, moist air along the western portion of the ice sheet towards the north, where the surface melt has been the highest since 1948" [Heiko Goelzer, Belgium]	accepted, text revised
61571	49	40	49	41	In my opinion "bringing the amount of melt to pre-2013 values" can be misleading because no lower limit is given for a variable which shows an increase in time. I would suggest "2010-2012" instead of "pre-2013". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
84867	49	41	49	41	The number 3800 ± 339 Gt for Greenland ice loss likely stems from a preprint of the IMBIE paper. The final published version mentions 3902 ± 342 Gt. [Jan Wuite, Austria]	accepted, numbers updated
67241	49	41	49	41	perhaps better presented as an average rate for comparability with other related numbers? [Regine Hock, United States of America]	accepted, numbers updated and made comparable with other related numbers
129475	49	41	49	41	Add a reference to the 2018/2019 GrIS melt statement: Velicogna, I., Mohajerani, Y., A, G., Landerer, F., Mouginot, J., Noel, B., et al. (2020). Continuity of ice sheet mass loss in Greenland and Antarctica from the GRACE and GRACE Follow-On missions. Geophysical Research Letters, 47, e2020GL087291. https://doi.org/10.1029/2020GL087291 [Trigg Talley, United States of America]	accepted, suggested reference added to assessment
61643	49	41	49	41	According to The IMBIE Team (2019), the Greenland Ice Sheet mass loss between 1992 and 2018 is 3902 +/- 342 Gt. This is a relatively small difference, but is there an update or an error somewhere? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, numbers revised to newest available.
84869	49	42	49	42	The number 10.6 ± 0.9 mm for sea level rise contribution from Greenland between 1992-2018 likely stems from a preprint of the IMBIE paper. The final published version mentions 10.8 ± 0.9 mm (see also comment no 1). [Jan Wuite, Austria]	accepted, text revised
106639	49	42	49	42	Greenland ice sheet -> The Greenland ice sheet [Kevin Bulthuis, United States of America]	accepted, text revised
61645	49	42	49	42	According to The IMBIE Team (2019), the Greenland Ice Sheet sea-level contribution between 1992 and 2018 is 10.8 +/- 0.9 mm. This is a very small difference, but is there an update or an error somewhere? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, numbers updated
80487	49	42	49	43	This statement is incorrect. Maybe: "The Greenland ice sheet was likely close to a state of balance ..." Compare statement l 26 to avoid repetition and contradiction. [Heiko Goelzer, Belgium]	accepted, text revised
61647	49	43	49	43	According to The IMBIE Team (2019), the Greenland Ice Sheet mass balance between 1992 and 1997 is -26 +/- 27 Gt/year. This is a relatively small difference, but is there an update or an error somewhere? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, numbers revised to newest available.
91073	49	43	49	55	I think it is important to discuss here or somewhere that the fact that the mass balance was ~0 in the 1990s does not indicate that the ice sheet was in equilbrium. This is to avoid confusion for the reader elsewhere. In Kjeldsen 2015 they estimate ~75 Gt/yr for the 20th C due as a response at the end of the LIA. This is ongoing , including the 1990s, but wa compensated from ~1980-1995 by an above average (for the 20th C) SMB. See e.g. Fettweis et al 2017 and Box et al 2013, pt II, J Climate of somewhere between 50-100 Gt/yr. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised

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3931	49	44	49	44	the loss was indicated with -270 … I would assume a loss should be noted with positive values, otherwise it is a double negation and comes to a plus in the end (eg l 41 correctly performed) [Sabine Baumann, Germany]	accepted, text revised
84871	49	44	49	44	The number -270 ± 28 Gt yr-1 for Greenland mass loss between 2007-2012 likely stems from a preprint of the IMBIE paper. The final published version mentions 275 ± 13 Gt yr-1. [Jan Wuite, Austria]	accepted, numbers revised to newest available.
84873	49	44	49	44	For clarity please try to be consistent about the sign of mass loss, it is here provided as a negative number (-270 ± 28 Gt yr-1), but for instance in line 30-33 it is provided as a positive number. [Jan Wuite, Austria]	accepted, text revised and numbers presented consistently with same sign
61649	49	44	49	44	According to The IMBIE Team (2019), the Greenland Ice Sheet mass balance between 2007 and 2012 is -275 +/- 28 Gt/year. This is a relatively small difference, but is there an update or an error somewhere? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, numbers revised to newest available.
61651	49	45	49	46	The Greenland Ice Sheet mass balance of -270 +/- 28 Gt/yr for the period 2007-2012 does not agree within uncertainties with Bamber et al. (2018), who report a mass balance of -320 +/- 10 Gt/yr over 2007-2011 in their Table 2 and of -341 Gt/yr over 2007-2012 based on their Table S6. I think you should rephrase the sentence. But the nice thing is that the total mass loss over the 1992-2018 period is very similar between The IMBIE Team (2019) and Bamber et al. (2018), with 3902 Gt for the former and 3947 Gt for the latter. I think this should be emphasized. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised and this point made
107265	49	45			this is an ideal place to make a confidence statement based on a wide range of studies agreeing [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
111325	49	46	49	47	Needs additional references on mass loss from west and SE Greenland. What about the recent mass loss signals in NW Greenland? I would recommend referencing the observations from the Promice station literature as well. This will provide greater context on observational records in addition to the GNSS observations. [Samiah Moustafa, United States of America]	accepted, text revised and regional assessment added
61689	49	46	49	47	Only the Figure 9.18 justify the larger mass loss rate along the west coast and in southeast Greenland. I suggest to cut the sentence like this: "These estimates (from previous cited Figures) agree within uncertainties with other post-AR5 reviews (Bamber et al., 2018b; Cazenave et al., 2018; Mouginot et al., 2019). Figure 9.18 shows that the largest mass losses are along the west coast and in southeast Greenland." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised and regional assessment added
45335	49	47	49	47	Add that the mass loss was concentrated at a few major outlet glaciers [Nanna B Karlsson, Denmark]	accepted, text revised and regional assessment added
69719	49	51	49	51	The previous paragraph describes mass change values during the 20th century. I think what is meant in this sentence is "covering" or "spanning" the 20th century. [Matthew Hoffman, United States of America]	accepted text revised
80489	49	51	49	51	More precision needed: The IMBIE estimates above are also in part for the 20th century. Maybe specify "before the advent of wide-scale satellite observations" [Heiko Goelzer, Belgium]	accepted, text revised
157	49	51	49	51	Missing a reference here. Carrivick et al. 2019 mapped the extent of mountain glaciers in Greenland in the LIA and reconstructed volume change. https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2018GL081383 [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	accepted, suggested reference added to assessment
10699	49	51	49	53	Admittedly the "Little Ice Age" is inconsistently and poorly defined, but no one thinks it lasted into the 20th century. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
3933	49	51	50	6	Is this section about ice sheet development in the 20th century? I would assume this from the first sentence. Hence, there are three sentences that describe the behaviour in the 21st century. I would exlcude them here and merge them together afterwards. [Sabine Baumann, Germany]	accepted, section restructured.
107271	49	51			good to discuss logr term but this paragraph seems out of place as immediately return to 1990s afterwards [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
80491	49	53	49	53	Specify approximate time bounds for the little ice age and note that it is not in the 20th century, contradicting the introductory sentence. [Heiko Goelzer, Belgium]	accepted, text revised
14709	49	53	49	53	"Little Ice Age" -> "Little Ice Age (LIA)" [Jeremy Fyke, Canada]	rejected, use of acronyms reduced as possible from paragraph.

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61691	49	54	50	1	It is specified that the average mass change during the period 1900-1983 is equal to -75 ± 29 Gt yr-1 and is following by an increased rate after 2000. I found these arguments confusing with the lower value (-18 ± 28 Gt yr-1) between 1992-1997 (line 43 P49) and with the Figure 9.18 which both suggest a Greenland ice sheet close to mass balance during the 1990s. By reading the text, I found difficult to well represent the ice mass loss timeline. I suggest adding information about the Greenland ice sheet state close to mass balance during the 1990s in line 55 Page 9-49, before the statement about the increase at the end of the 1990s. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
77697	49		61		Consistency in formats of sections Greenland (9.4.1.1, pg 49) and Antarctica (9.4.2.1, pg 58) - 9.4.1.1 has Model evaluation sub heading (pg 52 lin 4), 9.4.2.1 does not. Last 2 paragraphs 9.4.2.1 deal with model performance evaluation - could be under Model evaluation sub heading? [Emer Griffin, Ireland]	taken into account, consistency in sections of Greenland and Antarctica improved
80497	50	1	50	1	Specify "marine-terminating" outlet glaciers if that is meant here contrasting the next sentence about land-terminating glaciers. Consider starting the senence with "Many marine-terminating outlet glaciers in Greenlend underwent..." [Heiko Goelzer, Belgium]	taken into account, deleted in this location but term added elsewhere.
80495	50	2	50	2	LIA needs to be defined. [Heiko Goelzer, Belgium]	noted, use of acronyms reduced as possible
61545	50	2	50	3	The Andresen reference is only for Helheim glacier, but the wording here implies that this was a widespread phenomenon. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
79535	50	3	50	3	The study of Vermassen et al. (2020) would be a great addition to support the evidence for the loss of floating tongues in the early 20th century. Link: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL085954 [Flor Vermassen, Sweden]	accepted, suggested reference added to assessment
61547	50	3	50	4	The wording of this sentence could be improved, e.g. “small changes” in what? Terminus position? And does this connect to the previous clause? Did the loss of floating ice tongues increase calving but not cause major retreat? And then what does this have to do with the asynchronous retreat mentioned at the beginning? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
82113	50	3	50	6	Related to the point above, the regional climate shift and warming around 1930 is associated with internal variability this point can be condensed with the above. What is internal variability component and what is forced (timescales) [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	noted, text revised and new references added
45327	50	4	50	6	Start the sentence with "In the Southeast, land-terminating ...", because Bjørk et al, only looked at glaciers in the SE [Kristian Kjelden, Denmark]	accepted, text revised
65959	50	6	46	6	Suggest clarification. Suggest changing "sea-ice area" to "sea-ice cover" since sea-ice area is a specific variable. [Kushla Munro, Australia]	taken into account sea-ice cover used in chapter
46469	50	6	50	6	"Box, J. E.: Greenland ice sheet mass balance reconstruction, Part II: Surface mass balance (1840–2010), J. Climate, 26, 6974–6989, doi:10.1175/JCLI-D-12-00518.1, 2013." could be cited here [Xavier Fettweis, Belgium]	not applicable, text removed
46471	50	6	50	6	"Fettweis, X., Box, J. E., Agosta, C., Amory, C., Kittel, C., Lang, C., van As, D., Machguth, H., and Gallée, H.: Reconstructions of the 1900–2015 Greenland ice sheet surface mass balance using the regional climate MAR model, The Cryosphere, 11, 1015–1033, https://doi.org/10.5194/tc-11-1015-2017, 2017." could be cited here. Fettweis et al. 2017 mentionned an likely significant increase of snowfall over 1900-1950. [Xavier Fettweis, Belgium]	not applicable, text removed
22587	50	8	50	9	Is this sentence adding anything? [Peter Thorne, Ireland]	accepted, text removed
96975	50	8	50	21	The paragraph is hard to follow because of the many different periods, maybe a simple summary at the end of the shifted importance between SMB and glacier dynamics would be helpful. [Nicole Wilke, Germany]	accepted, text revised
107277	50	8			mixing several term here submarine melt, discharge, dyanmics. Better to introduce definitions that allow tyou to discuss more clearly (eg AR5). [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
3935	50	9	50	9	include here a short explanation what is surface mass balance and what is glacier dynamics and their differences? Afterwards, a lot of emphasis is put in the different development of the two processes concerning mass loss. [Sabine Baumann, Germany]	accepted, text revised
67245	50	9	50	9	glacier dynamics is not a mass loss; better 'ice discharge'? [Regine Hock, United States of America]	accepted, text revised
805	50	9	50	10	while glacier dynamics may lead to increased calving, mass loss from glacier dynamics is largely the result of (i.e. is triggered by) increased submarine melt and/or calving of icebergs. The cause and effect are mixed up in this statement. [Michael Wood, United States of America]	accepted, text revised

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62319	50	9	50	10	I suggest changing the statement, "the latter [glacier dynamics] results in calving of icebergs and submarine melting" to the following: "glacier dynamics result in calving of icebergs and can expose more ice to the ocean through grounding line retreat, increasing submarine melting." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
80499	50	9	50	10	This sentence needs reformulation. It is not correct that glacier dynamics results in submarine melting. [Heiko Goelzer, Belgium]	accepted, text revised
80501	50	9	50	10	"The mass losses can be partitioned between surface mass balance and glacier dynamics" Iis not correct. Maybe "The mass losses can be partitioned into contributions caused by changes in surface mass balance and those caused by ice dynamics" [Heiko Goelzer, Belgium]	accepted, text revised
73859	50	9	50	10	The mass losses is partitioned into two components, surface mass balance and glacir dynamics. However, the next sententene tells that the latter relates to another `mass balance’ (submarine melting). It is better to explain, the direct influence of submarine melting is small but indirect influence through glacier dynamics is comparable to the former mass balance. [Fuyuki SAITO, Japan]	accepted, text revised
67247	50	9	50	10	inconsistent with the earlier definition of surface mass balance which excludes refreezing. Either use the correct term 'climatic mass balance' which includes the surface mass fluxes and internal mass changes such as refreezing, or at least make clear how you defined surface mass balance here. [Regine Hock, United States of America]	accepted, text revised
61573	50	10	50	12	I would suggest to have the GrIS mass loss instead of the GrIS mass as subject of the sentence : "For the period 1972-2018, the GrIS mass loss was controlled at 66 ± 8% by glacier dynamics and 34 ± 8% by surface mass balance (Mouginot et al., 2019).". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
80507	50	10	50	13	I think the two studies (Mouginot and IMBIE) disagree on the partitioning over the common period. That is why the construction "This ratio changes to ..." can not be made without further explenation. Also, Mouginot is part of the IMBIE results, which does not become clear here. [Heiko Goelzer, Belgium]	accepted, text revised
61549	50	11	50	11	This could be taken out of context to read “The GrIS lost 66 % of its mass” so I would suggest re-phrasing to say “66 percent of its mass loss was due to ice dynamics…” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
80503	50	11	50	11	Needs reformulation. I hope the ice sheet has not lost 66% of its mass. [Heiko Goelzer, Belgium]	accepted, text revised
20193	50	11	50	11	The way it is written, GrIS lost 100% of its mass! A minimum correction would be to add "loss" after "mass", but CH09 authors can undoubtedly do better [philippe waldteufel, France]	accepted, text revised
80505	50	11	50	12	Add "changes" before "surface mass balance". [Heiko Goelzer, Belgium]	accepted, text revised
103821	50	11	50	12	This sentence reads wrong. It sounds like GrIS lost 66% of its mass which is not the case: 'the GrIS lost 66 ± 8% of its mass due to ice dynamics and 34 ± 8% by surface mass balance (Mouginot et al., 2019).' [Philippe Tulkens, Belgium]	accepted, text revised
80857	50	11	50	12	This sentence reads wrong. It sounds like GrIS lost 66% of its mass which is not the case: 'the GrIS lost 66 ± 8% of its mass due to ice dynamics and 34 ± 8% by surface mass balance (Mouginot et al., 2019).' [Louise Sandberg Sørensen, Denmark]	accepted text revised
98545	50	11	50	12	rephrase to "66% of the GrIS mass loss was due to (…)" [Miren Vizcaino, Netherlands]	accepted text revised
84875	50	12	50	12	The ratios 48% for glacier dynamics and 52% for surface melt from IMBIE is provided. In the final version of the paper, however, the numbers 50.3% respectively 49.7% are provided. [Jan Wuite, Austria]	accepted text revised
103823	50	12	50	12	52% surface melt or 52% SMB? [Philippe Tulkens, Belgium]	accepted text revised
80859	50	12	50	12	52% surface melt or 52% SMB? [Louise Sandberg Sørensen, Denmark]	accepted text revised
62321	50	13	50	13	I suggest replacing "a more detailed study" with "a study that partitioned losses in different periods" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted text revised
80509	50	13	50	13	Please clarify, what is "more detailed" about the Enderlin study. Is it specifying results for shorter time periods (which the others could do if they wanted) or is the methodology that is producing the results more detailed? If the first, are the results in agreement with the updated IMBIE results? Are Enderlin results maybe included in the IMBIE results? [Heiko Goelzer, Belgium]	noted, text revised and clarified, updated imbie results included
71803	50	15	50	15	I would subtitute these numbers by newer more precise values from Mouginot et al. 2019. It would also make the comparison with period 1972-2018 in previous sentence clearer. The contribution to mass loss for the periods 2000-2005, 2005-2009, 2009 and 2012 as estimated by Mouginot et al. 2019 are 57%, 40% and 33%, respectively. [Jeremie Mouginot, France]	accepted, text revised

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71805	50	15	50	15	The point is made here that the contribution of dynamic losses is decreased relatively to the loss from surface mass balance, but if we continue this comparison to more recent periods it is not true anymore. I would add the period 2012-2018 or 2014-2018 where contribution to mass loss from ice dynamic was 53% and 57%, respectively. I believe that it shows if SMB can vary extremely rapidly and lead to important mass loss over short time periods (e.g. 2009-2012), SMB losses can also decrease rapidly as during 2014-2018 and then contribution from dynamic becomes important again. My point is that both mechanisms matter and would like to sure that we do not give the false impression that SMB only matters. [Jeremie Mouginot, France]	accepted, text revised to reflect this
15815	50	15	50	16	Replace "The surface mass balance is observed to vary on a shorter time scale than the glacier dynamics" with "Mass loss associated with changes in surface processes is observed to vary on shorter time scales than mass loss due to ice dynamics processes" [Olga Sergienko, United States of America]	accepted text revised in response to many comments
67251	50	16	50	16	"than the mass loss due to ice dynamics" [Regine Hock, United States of America]	accepted, Now deleted.
91075	50	16	50	16	poor/wrong choice of reference. Use Fettweis 2013 or VdB 2017 here which examine directly SMB [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	accepted, now deleted text revised and references updated
22589	50	16	50	20	Either give values for all periods or no values but giving just the final value means the reader has no context to assess it against. On balanace I would give all the numbers but the worst situation is giving one number alone here I think. [Peter Thorne, Ireland]	accepted, text revised
61693	50	16	58	18	I suggest to add the 1986-2000 ice discharge value 430 Gt yr-1 (Mankoff et al., 2019) to provide change rate between the two steady-state zones: "A study with near-complete spatial coverage (using 276 flux gates) reports approximately steady solid ice discharge of about 430 Gt yr-1 from 1986 to 2000, then a sharp increase to over 500 Gt yr-1 during 2000-2005, followed by approximately steady ice discharge of 500 ± 50 Gt yr-1" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted text revised, updated Mankoff and King discharges added to assessment
107273	50	16			shorter time scale - quanitify please [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
67249	50	17	50	17	flux gates is not an easily understood term for this audience: is it 276 outlet glaciers? [Regine Hock, United States of America]	not applicable, text removed
15817	50	17	50	17	Either remove "(using 276 flux gates) " or explain what flux gates mean [Olga Sergienko, United States of America]	not applicable, text removed
8999	50	17	50	21	This is not a correct statement. Ice discharge has been continuously increasing in Greenland. There is no leveling off in the Mouginot et al. PNAS 2019 paper. Mankoff et al 2019 suffers from severe limitations on how they constrained ice thickness at the grounding line, they took BedMachine Version 3 totally blindly even though it is clearly state in BedMachine that some of these glaciers are not constrained by data. This mistake was not made in Mouginot et al. 2019. [Eric Rignot, United States of America]	accepted, updated Mankoff, King and Mouginot discharges are included in assessment and the point about bedrock topography made
71807	50	20	50	20	add Mouginot et al. 2019 along with Mankoff et al. 2019. [Jeremie Mouginot, France]	accepted, updated Mankoff, King and Mouginot discharges are included in assessment
14711	50	21	50	21	If the relevant Lead Authors agree, I suggest adding a summary statement at end of this paragraph to effect that "Changing ratios of ice loss between glacier dynamics and surface melt over these different time periods suggests a general transition towards a GrIS ice loss regime dominated by surface melting, even in light of surface mass balance and ice discharge interannual variability". [Jeremy Fyke, Canada]	taken into account, confidence statement about discharge vs SMB is revised
103825	50	21	50	21	Relevant reference also: https://doi.org/10.34194/GEUSB-201943-02-01 [Philippe Tulkens, Belgium]	accepted, suggested reference added to assessment
80861	50	21	50	21	Relevant reference also: https://doi.org/10.34194/GEUSB-201943-02-01 [Louise Sandberg Sørensen, Denmark]	accepted, suggested reference added to assessment
107275	50	21			case for a confidence statement that ice discharge increased 200-2005 but has been steady since? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	noted, confidence statement about discharge revised
14713	50	23	50	23	"...energy balance at the surface...": very specifically, energy balance at the surface, when this surface is at 0C and excess energy contributes to melting vs. ice/snow warming. Perhaps worth noting technicality in text. [Jeremy Fyke, Canada]	not applicable, text removed
15819	50	23	50	23	"Surface melt depends on the energy balance at the surface" is incorrect, it is a part of the energy balance. Either rephrase or remove this sentece. [Olga Sergienko, United States of America]	accepted, text revised

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30051	50	23	50	24	In the sentence "Surface melt depends on the energy balance at the surface (Bennartz et al., 2013; Van Tricht et al., 2016; Hofer et al., 2017).", please consider to indicate the papers by Niwano et al. (2015) that present the observed snow surface melt energy at a study site on the northwest Greenland ice sheet during the 2012 record surface melt event was higher under cloudy-sky conditions than clear-sky conditions, and Niwano et al. (2019) on the cloud radiative effects and its indirect feedback processes over the Greenland ice sheet like: "Surface melt depends on the energy balance at the surface (Bennartz et al., 2013; Van Tricht et al., 2016; Hofer et al., 2017; Niwano et al., 2015, 2019).". [References] Niwano, M., Aoki, T., Matoba, S., Yamaguchi, S., Tanikawa, T., Kuchiki, K., and Motoyama, H. (2015). Numerical simulation of extreme snowmelt observed at the SIGMA-A site, northwest Greenland, during summer 2012. Cryosph., 9, 971-988, doi:10.5194/tc-9-971-2015. Niwano, M., Hashimoto, A., and Aoki, T. (2019). Cloud-driven modulations of Greenland ice sheet surface melt. Sci. Rep., 9, 10380, doi:10.1038/s41598-019-46152-5. [Masashi Niwano, Japan]	accepted, suggested reference added to assessment
64379	50	23	50	24	text book information no references needed, SMB-elevation feedback for instance known for 30 years or so not just since VDB2017 [roderik van de wal, Netherlands]	accepted, text revised
67253	50	23	50	24	Are these seemingly randomly chosen references out of probably hundreds/thousands on this topic really necessary. Seems like a basic physics statement which is a simple fact. [Regine Hock, United States of America]	accepted, text revised
41877	50	23	50	36	Add the reference; Niwano, M., Hashimoto, A. & Aoki, T. Cloud-driven modulations of Greenland ice sheet surface melt. Sci Rep 9, 10380 (2019). https://doi.org/10.1038/s41598-019-46152-5. This article is strongly related to the surface melt and cloud effect to the surface radiation balance. [Rigen Shimada, Japan]	accepted, suggested reference added to assessment
64385	50	23	50	46	a somewhat incoherent summation of things which are important [roderik van de wal, Netherlands]	accepted, text revised to more clearly explain post-SROCC advances in understanding.
67255	50	23	50	46	This seems long and reads a bit like a textbook rather than an assessment [Regine Hock, United States of America]	accepted, text revised to more clearly explain post-SROCC advances in understanding.
15821	50	23	50	46	Replace "melt" with "surface melt" [Olga Sergienko, United States of America]	accepted text revised
77809	50	23	51	44	These two paragraphs are also uncomfortably long. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised to more clearly explain post-SROCC advances in understanding.
80511	50	24	50	24	Under what conditions do positive feedback mechanisms amplify the melting? In a cooling climate the positive feedbacks dampen the melting! [Heiko Goelzer, Belgium]	accepted, text revised and clarified
82121	50	25	50	25	Is this really worth giving a 'confidence' to? Is it a model or observational basis for the confidence? If the latter then has the full mass and energy budget been done? [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text removed
22591	50	25	50	26	There is no trace directly associated with this very high confidence statement. This comes back to the general chaprter wide issue of embedding confidence statements in the text rather than having concluding assessment findings per all other chapters up to this point. [Peter Thorne, Ireland]	accepted, text revised to more clearly explain post-SROCC advances in understanding and confidence statements add end of each paragraph
46473	50	25	50	26	"Le clec'h, S., Charbit, S., Quiquet, A., Fettweis, X., Dumas, C., Kageyama, M., Wyard, C., and Ritz, C.: Assessment of the Greenland ice sheet–atmosphere feedbacks for the next century with a regional atmospheric model coupled to an ice sheet model, The Cryosphere, 13, 373–395, https://doi.org/10.5194/tc-13-373-2019, 2019." could be cited as reference. [Xavier Fettweis, Belgium]	noted, suggested reference cited in model evaluation section.
62323	50	25	50	27	I think what these two sentences are trying to say is that there is very high confidence that the SMB-elevation feedback is actually happening but the way it is written makes it seem like the confidence statement has to do with surface melt lowering elevation. I suggest rewording these two sentences to: "Increased melt at the ice sheet margins decreases the surface elevation, which increases the local air temperature and further increases the melt. This is called the SMB-elevation feedback (van den Broeke et al., 2017) and there is very high confidence that this feedback is occuring on the ice sheet." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
103827	50	26	50	26	air temperature -> surface air temperature [Philippe Tulkens, Belgium]	not applicable, text removed
80863	50	26	50	26	air temperature -> surface air temperature [Louise Sandberg Sørensen, Denmark]	not applicable, text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115461	50	26	50	27	SMB- elevation feedback as a concept dates back at least to Weertman, 1961, Oerlemans, too (early 1980s), probably even earlier. [Robert DeConto, United States of America]	accepted, text removed
69629	50	26	50	27	link to instabilities paragraph in Box 9.2 and also to Ch.7 feedbacks section [Nicholas Golledge, New Zealand]	not applicable, text removed
82123	50	27	50	27	Is it really worth putting in a gratuitous 'confidence' to the albedo-temeperature feedback? What is such a 'confidence' actually telling us that is new? This should instead be a high level statement of confidence from observations that climate change is causing the decline in Greenland ice sheet (rather than internal variability). [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
61653	50	27	50	27	van den Broeke et al. (2017) does not really mention the SMB-elevation feedback. I would rather cite Edwards et al. (2014) here: Edwards, T.L. et al. (2014). Probabilistic parameterisation of the surface mass balance–elevation feedback in regional climate model simulations of the Greenland ice sheet. The Cryosphere, https://doi.org/10.5194/tc-8-181-2014. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised to more clearly explain post-SROCC advances in understanding.
109069	50	27	50	30	Can the surface darkening statements include black carbon deposition on the cryosphere(see Section 6.3.1.4, and page 9-80, lines 5-6)? [Chaincy Kuo, United States of America]	accepted, reference to 6.3.1.4 included
109073	50	27	50	30	Is there room to list some of the key natural and anthropogenic forcers, rather than just the references? [Chaincy Kuo, United States of America]	accepted, text revised
41879	50	27	50	33	Add the reference; Shimada R, Takeuchi N and Aoki T (2016) Inter-Annual and Geographical Variations in the Extent of Bare Ice and Dark Ice on the Greenland Ice Sheet Derived from MODIS Satellite Images. Front. Earth Sci. 4:43. doi: 10.3389/feart.2016.00043. This article showed the recent dark ice extent variation at the first time. [Rigen Shimada, Japan]	accepted, suggested reference added to assessment
41881	50	27	50	46	The albedo reduction is strongly related to the microbial activity. And there is a large uncertainly because of the biological activity. Takeuchi et al. 2018 mentioned to the cryoconites formed the hole on the glacial surface and it changed the surface albedo. I recommend to add the refecence; Takeuchi, N., Sakaki, R., Uetake, J., Nagatsuka, N., Shimada, R., Niwano, M., & Aoki, T. (2018). Temporal variations of cryoconite holes and cryoconite coverage on the ablation ice surface of Qaanaaq Glacier in northwest Greenland. Annals of Glaciology, 59(77), 21-30. doi:10.1017/aog.2018.19 [Rigen Shimada, Japan]	noted, not possible to discuss details, but text revised to reflect and reference in earlier comment added
111327	50	29	50	30	Several bodies of literature surrounding darkening of the Greenland Ice Sheet and albedo studies are missing here. For instance, Moustafa et al. (2015) in The Cryosphere characterizes albedo from observations and space over space and time in the lower ablation zone of SouthWest Greenland. Dr. Patrick Alexander also has several publications supporting the lowering of surface albedo with the progression of the melt season and transition of surface types. [Samiah Moustafa, United States of America]	noted, not possible to discuss all references, but text revised to emphasize post-SROCC advances in understanding
64381	50	30	50	30	feedback via darkening from algae needs mentioning here e.g. I G M Wientjes, R S W van de Wal, M Schwikoski, A Zapf, S Fahrni, L Wacker. Carbonaceous particles indicate Holocene dust as cause for the dark region in the western ablation zone of the Greenland ice sheet. Journal of Glaciology, 58(210) doi:10.3189/2012JoGG11J165 [roderik van de wal, Netherlands]	noted, not possible to discuss all references, but text revised to emphasize post-SROCC advances in understanding
1757	50	30	50	33	The recent increasing trend in the loss of Greenland ice sheet mass is coupled in part to the darkening of ice surfaces by particulate matter derived from natural and anthropogenic sources. This infers that there has been increasing deposition of dark particulate matter on ice sheet surfaces in recent years that decrease albedo. However, no estimates are given on the deposition rates of the particulate matter on ice sheet surfaces. [Michael Kennish, United States of America]	taken into account, text revised and a confidence statement about amplification of melt by positive albedo feedback
82115	50	31	50	31	Why would 'dark particulate matter' be increasing as a feedback or is it relic soot enbedded in the melting snow? [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text removed
22593	50	33	50	33	Why not start a new paragraph when starting to discuss clouds here? [Peter Thorne, Ireland]	accepted, text revised and paragraph structure substantially revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30053	50	33	50	36	Niwano et al. (2015) clearly show that the surface melt at a study site on the northwest Greenland ice sheet was accelerated under cloud-sky conditions during the 2012 record surface melt event, which can be cited here. Also, I would like to suggest that the latest study regarding the cloud radiative effects on the Greenland ice sheet surface melt by Niwano et al. (2019), which utilizes a state-of-the-art polar regional climate model, should be refered here. Niwano et al. (2019) also highlight that clouds did not enhance the Greenland ice sheet surface mass loss during the period 2011-2014, which may seem to be contradictory to the result by Niwano et al. (2015). My current understanding is that clouds can accelerate surface melt in a several days ~ a week time scale; however, in a climatic time scale (more than a year), clouds do not accelerate the Greenland ice sheet surface melt and its resultant surface mass loss. Overall, I would like to suggest that the sentence on the cloud effects can be updated as follows: "Cloud cover has various effects on melt; decreasing cloud cover can increase melt (Hofer et al., 2017) although clouds can accelerate melt in a short event time scale (Niwano et al., 2015), indirect time-integrated feedbacks of cloud radiative effects control the ice sheet surface energy balance (Niwano et al., 2019), low level liquid clouds increase melt (Bennartz et al., 2013), and clouds can reduce meltwater refreezing, thereby accelerating bare-ice exposure and enhance meltwater runoff (Van Tricht et al., 2016)." [References] Niwano, M., Aoki, T., Matoba, S., Yamaguchi, S., Tanikawa, T., Kuchiki, K., and Motoyama, H. (2015). Numerical simulation of extreme snowmelt observed at the SIGMA-A site, northwest Greenland, during summer 2012. Cryosph., 9, 971-988, doi:10.5194/tc-9-971-2015. Niwano, M., Hashimoto, A., and Aoki, T. (2019). Cloud-driven modulations of Greenland ice sheet surface melt. Sci. Rep., 9, 10380, doi:10.1038/s41598-019-46152-5. [Masashi Niwano, Japan]	taken into account, text revised and suggested references added to assessment
46475	50	33	50	36	I suggest to add reference to the accumulation/ablation zone as suggested below: Cloud cover has various effects on melt; decreasing cloud cover can increase melt "in the ablation zone" (Hofer et al., 2017), low level liquid clouds increase melt (Bennartz et al., 2013), and clouds can reduce meltwater refreezing "in the accumulation zone", thereby accelerating bare-ice exposure and enhance meltwater runoff (Van Tricht et al., 2016). [Xavier Fettweis, Belgium]	accepted, text edited and revised due to several comments
82117	50	33	50	46	Poorly structured discussion on cloud feedback. Explain at start of section elements of cloud feedback to to SW and LW at the moment LW forcing is mentioned on line38 after some hand-waving comments on clouds. Melt water refressing has been alluded to before in the Para and on page 49 but a description of it is given only towards end of paragraph Discussion of interal variability of water vapour can be moved to an earlier paragraph on internal variability - see comments 4 and 5. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, text revised and improved
46477	50	36	50	39	This sentence is only true over the accumulation zone. Over the ablation zone, it is the opposite according to: "Hahn, L. C., Storelvmo, T., Hofer, S., Parfitt, R., & Ummenhofer, C. C. (2020). Importance of orography for Greenland cloud and melt response to atmospheric blocking,. Journal of Climate, https://doi.org/10.1175/JCLI-D-19-0527.1, (2020)." [Xavier Fettweis, Belgium]	accepted, text revised and improved, suggested reference added to assessment
15823	50	37	50	37	What does "total column water" mean? [Olga Sergienko, United States of America]	accepted, text revised
64383	50	39	50	40	the longwave versus shortwave point was already made by Ambach 50? Years ago not really worh mentioning and it does not fit in the list of feedback mechanisms discussed here [roderik van de wal, Netherlands]	accepted, text revised and assessment improved
69721	50	39	50	46	There should be some mention of the recent formation of extensive refrozen lenses decreasing the ability of the firn to accept more meltwater for refreezing in the future. [Matthew Hoffman, United States of America]	accepted, FGD includes assessment on meltwater storage and refreezing
9001	50	43	50	46	This is a placeholder for aquifer, but there is no evidence that this aquifer has changed in time and has any relevance to the overall mass budget. So how can you say that this aquifer is potentially important with no reference or quantification. [Eric Rignot, United States of America]	accepted, placeholder deleted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22595	50	43	50	46	A policy maker is likely to want to know whether these are emergent features, whether they are growing etc. etc. Is there anything you can assess on such points? Also, how might they impact ice sheet processes? This seems like an area which, even if you are making very low confidence statements should be expanded upon in at least a fairly substantive paragraph in its own right? [Peter Thorne, Ireland]	accepted, text revised and assessment statements improved
67257	50	44	50	44	can the % of total ice sheet area be added to put the 70,000 km2 into perspective [Regine Hock, United States of America]	not applicable, text removed
45337	50	45	50	46	The recent publication by Chu et al (Chu, W., Schroeder, D. M., & Siegfried, M. R. (2018). Retrieval of englacial firn aquifer thickness from ice-penetrating radar sounding in southeastern Greenland. Geophysical Research Letters, 45, 11,770–11,778. https://doi.org/10.1029/2018GL079751) found that firn aquifers vary interannually in response to surface mass balance. This implies that it might not be a robust buffer. The study also found that the firn aquifer zone might be increasing so that the amount of water that can be stored in the firn will increase. [Nanna B Karlsson, Denmark]	taken into account, text revised in response to several comments and assessment improved
67903	50	48	50	48	Fig 9.19 has problems: the paleo-extents are one realization of a model; there is very large uncertainty associated with this. Top right has an unphysical break of slope at current time, which indicates that models are problematic (too conservative?). Related to that, the bottom right figure shows positive elevation change over much of interior Greenland; an unlikely occurrence under warming climates. [Martin Truffer, United States of America]	noted, figure has been revised and in response to the comments
42917	50	53			Here and in other parts of this section of the chapter (but not in the rest of the chapter or in chapter 2) the horrible word "interglaciation" is used. This reflects a lingstanding argument as to whether the correct term is interglacial or interglaciation. Common terminology through almost the entire community is interglacial. A small pocket of authors insist that interglacial is an adjective while the dictionary agrees it became a noun some decades ago, and mst of us consider interglaciation to be a process like deglaciation) and not a state. You need to be consistent and I suggest a global edit to "interglacial" is essential. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	accepted text revised
88633	50	55	50	55	The references Goelzer et al., 2017, Goelzer et al., 2018a and Goelzer et al., 2018b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
116863	50		50		for feedbacks, please check coherency with chapter 6 (SLCF) which has a specific related section. [Valerie Masson-Delmotte, France]	noted, a reference to section 6.3.1.4 made in relevant paragraph
116865	50		50		Please check that chapter 7 box on polar amplification correctly reflects recent knowledge related to the role of liquid clouds for Arctic surface warming [Valerie Masson-Delmotte, France]	noted, chapter 7 box on polar amplification not checked but we trust that recent knowledge about role of liquid clouds for Arctic surface warming is included in the appropriate places
9007	51	4	51	4	Parametrization of uncercutting is published in 2016 E. Rignot, X. Yun, L. An, M. van den Broeke, C. Cai, I. Fenty, X. Li, D. Menemenlis, J. Mouginot, M. Morlighem, Modeling of ocean-induced ice melt rates of five Greenland glaciers over the past two decades, Geophys. Res. Lett. 43(12) 6374-6382. and employed in Wood et al 2018; M Morlighem, M Wood, H Seroussi, Y Choi, E Rignot - Modeling the response of northwest Greenland to enhanced ocean thermal forcing and subglacial discharge. Cryosphere, 2019 [Eric Rignot, United States of America]	noted, text revised in response to several comments and assessment improved
27637	51	6	51	6	About '(2061-2100)' : The diagram indicates 2093-2100. [Eric Brun, France]	accepted, figure revised
807	51	8	51	9	"when accelerated retreat was observed in southeast and northwest" should have a citation. I suggest Murray, T., et al. "Extensive retreat of Greenland tidewater glaciers, 2000–2010." Arctic, antarctic, and alpine research 47.3 (2015): 427-447. [Michael Wood, United States of America]	noted, figure revised
20563	51	12	51	13	Would this mass loss through discharge from tidewater glaciers be part of the loss due to ice dynamics fraction mentioned on page 50 line 11? [philippe waldteufel, France]	accepted, text clarified.
67259	51	12	51	13	odd and awkward sentence [Regine Hock, United States of America]	accepted, text edited.
15825	51	12	51	13	Replace "are responsible for the mass loss through discharge, which increased by" with "increased their discharge by" [Olga Sergienko, United States of America]	accepted, text deleted.
61655	51	12	51	13	A confidence level is missing for this important statement about the tidewater glaciers contributing to the Greenland Ice Sheet mass loss. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised and assessment improved
107279	51	12			again rodering is jumbled and runs from discssion of ice dynamics (p50 8) to SMN (p58 23) and now back to dynamics [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text restructured and flow improved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107281	51	12			this is a long, dense paragraph that is hard going to read. Suggest that it is broken up into easier chunks. Also repeads some material from earlier paragraph on p50 [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text restructured.
14715	51	13	51	15	How is it possible for ice grounded ABOVE sea level to contribute to ice discharge? I.e., shouldn't 100% of ice discharge come from the 8% of the ice sheet periphery that is grounded below sea level, instead of 88%? In reading Morlighem et al 2014, I'm not sure the relevant study statement was interpreted correctly into this AR6 text. Suggest confirming with Morlighem et al., the details here. [Jeremy Fyke, Canada]	accepted, text deleted and section improved
62317	51	14	51	15	I suggest removing the statement "this small fraction controls 88% of the ice discharge from Greenland (Morlighem et al., 2014)." This statement comes directly from the Morlighem reference but I spoke with Morlighem and he explained that this is more of a statement about our best knowledge of the bed topography (BedMachine, v3) and not a statement about true Greenland discharge. It doesn't make sense that 12% of discharge occurs where bed > 0 and this is probably because the bed topography is incorrect and not actually because this much discharge is happening where the bed is above sea level. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text deleted.
39697	51	14			". Effects of CDR methods on climate are also assessed" doesn't it contradict p88, L44? [TSU WGI, France]	Noted. Wrong chapter so we cannot identify what this comment refers to.
46501	51	15	51	17	McFadden et al. (2011) should be added to this list of references as it was one of the early papers on this topic (McFadden, E. M., Howat, I. M., Joughin, I., Smith, B. E., & Ahn, Y. (2011). Changes in the dynamics of marine terminating outlet glaciers in west Greenland (2000–2009). Journal of Geophysical Research, 116(F2). [Stephen Price, United States of America]	noted, text substantially rewritten with post-SROCC references, the suggested reference not included as it is pre-AR5
45329	51	16	51	17	Motyka et al,2017, should be included as it examines this very aspect- doi: 10.1017/jog.2016.138 [Kristian Kjelden, Denmark]	accepted, suggested reference added to assessment
62325	51	17	51	19	I suggest adding the word "increased" to this statement "There is high confidence that increased submarine melt and iceberg calving contributed to the acceleration of mass loss from Greenland, …" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text edited.
61657	51	17	51	19	Since you say that there is 'high confidence' for submarine melt and calving to contribute to the Greenland Ice Sheet mass loss acceleration, you should cite some references here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text edited.
84877	51	19	51	19	While certainly a fast flowing glacier, Jakoshavn Isbrae is currently not the fastest glacier on earth, for instance Helheim and Kangerlussuaq Glacier, both in Greenland) are currently much faster. Maybe this should be clarified. [Jan Wuite, Austria]	accepted, text revised
91077	51	19	51	22	JI was also thickening in the 1990s, see Abdalati et al 2001, JGR and Krabill (various). This is important because it shows variable behaviour over decadal time scales and that prior to the accel in discharge the catchment was likely gaining mass. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
809	51	22	51	22	as the driving mechanisms are still an area of active debate, I think Joughin et al 2020 (submitted in 2019) should also be cited with Khazendar et al 2019b - the citation is Joughin, Ian, et al. "A decade of variability on Jakobshavn Isbræ: ocean temperatures pace speed through influence on mélange rigidity." The Cryosphere 14.1 (2020): 211-227. [Michael Wood, United States of America]	accepted, suggested reference added to assessment
61529	51	24	51	24	Khan et al. 2014 do not state a speedup of Petermann glacier. It is Ruckamp et al. (2019) (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018JF004775) that first state the 10% speedup of Petermann glacier [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
71811	51	25	51	25	should read Zachariae and not Zachariæ. Do not follow Mouginot et al. 2015 as they mistakenly use the wrong spelling (Bjørk et al. 2015). REF: Bjørk, A. A., Kruse, L. M., and Michaelsen, P. B.: Brief communication: Getting Greenland's glaciers right – a new data set of all official Greenlandic glacier names, The Cryosphere, 9, 2215–2218, https://doi.org/10.5194/tc-9-2215-2015, 2015. [Jeremie Mouginot, France]	accepted, text revised, Zachariae deleted
61695	51	25	51	25	I don't think providing volume of the Zachariæ Isstrøm glacier is relevant. As it is not specified for the others cited glaciers, it's not possible to evaluate how big is it. I suggest to remove the value or to add values for all the others cited glaciers (Jakobshavn Isbræ, Petermann, Humboldt and Nioghalvfjerdsfjorden and North East Greenland Ice stream). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32143	51	26			Check the statement "from late 2012 the velocity tripled". The abstract of Mouginot et al (2015) says: "acceleration RATE of its ice velocity tripled", not the velocity itself; Fig. 2 a shows the data: velocities less than doubled from 2001 to 2015; the observed time span 2012-2015 for the triplication of the rate should be stated as well. In Khan et al (2014) Fig 4 shows velocity increase in comparison to 2000/2001, reaching 600 m/yr increase at the very front, where absolute velocity is above 1000 m/yr in Fig 2. [Anja Wendt, Germany]	accepted, text deleted.
45339	51	29	51	29	Add: Hagen Bræ in northern Greenland lost its floating tongue in 2008 causing a 15 km retreat (Hill et al., 2017) but subsequent ice flow variability is likely due to internal dynamics causing a glacier surge triggered by surface meltwater (Solgaard et al., 2020). Refs: Hill,E.A. ,Carr,J.R., Stokes,C.R.(2017).A review of recent changes in major marine-terminating outlet glaciers in Northern Greenland. Frontiers in Earth Science, 4, 1–23. https://doi.org/10.3389/feart.2016.00111. Solgaard et al., (2020). Hagen Bræ: A surging glacier in North Greenland—35 years of observations. Geophysical Research Letters, 47, e2019GL085802. https://doi.org/10.1029/2019GL085802 [Nanna B Karlsson, Denmark]	taken into account, one of the suggested reference (post-SROCC) added to assessment
71809	51	29	51	29	I recommend to add Mouginot et al. 2015 along with Khan et al. 2014, as it provides a more detailed study on NEGIS speed-up, grounding line retreat, ice-shelf thinning of Nioghalvfjerdfjorden and Zachariae Isstrom. As Storstrømmen is also part of NEGIS and not included in Khan et al. 2014 nor Mouginot et al. 2015, reference to Mouginot et al. 2018 could be eventually added. REF: Mouginot, J., Rignot, E., Scheuchl, B., Fenty, I., Khazendar, A., Morlighem, M., et al. (2015). Fast retreat of Zachariae Isstrøm, northeast Greenland. Science 350, 1357–1361. doi:10.1126/science.aac7111. Mouginot, J., Bjørk, A. A., Millan, R., Scheuchl, B., & Rignot, E. (2018). Insights on the surge behavior of Storstrømmen and L. Bistrup Bræ, northeast Greenland, over the last century. Geophysical Research Letters, 45, 11,197– 11,205. https://doi.org/10.1029/2018GL079052 [Jeremie Mouginot, France]	taken into account, text revised and suggested reference added to assessment
9003	51	33	51	37	Several studies of undercutting have been published about undercutting of glaciers by the ocean but there is absolutely no mention of it. That's a big deal becasue the rate of undercutting is 2-3 times larger than the average rate of subaqueous melt by the ocean. Please mention these studies and their impact, this is a crucial point: 2015 E. Rignot, I. Fenty, C. Kemp, X. Yun, C. Cai, Undercutting of Greenland marine-terminating glaciers in deep glacial fjords., Geophys. Res. Lett., 42 (14), 5909-5917. 2016 E. Rignot, X. Yun, L. An, M. van den Broeke, C. Cai, I. Fenty, X. Li, D. Menemenlis, J. Mouginot, M. Morlighem, Modeling of ocean-induced ice melt rates of five Greenland glaciers over the past two decades, Geophys. Res. Lett. 43(12) 6374-6382. Slater, D. A., P. W. Nienow,D. N. Goldberg, T. R. Cowton, andA. J. Sole (2017), A model for tidewaterglacier undercutting by submarinemelting, Geophys. Res.Lett., 44, 2360–2368, [Eric Rignot, United States of America]	accepted, text revised and suggested references added to assessment
82125	51	34	51	34	Include 'Summer' before 'surface melting'. Although this is slightly redundant it emphasises the seasonality of the process, and as a consequence the glacier advance in winter. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, text revised due to several comments and improved
15827	51	35	51	35	Remove "enhance ocean melt rates", it's redundant [Olga Sergienko, United States of America]	accepted, text revised
45341	51	35	51	37	A study by Beckmann et al. (2019) showed that basal discharge (which is largely composed of surface meltwater) is as important as rising temperature for outlet glaciers implying that increased surface melt will also trigger increased solid ice discharge making. Ref:Beckmann et al., 2019. Modeling the response of Greenland outlet glaciers to global warming using a coupled flow line–plume model, The Cryosphere, 13, 2281–2301, 2019 https://doi.org/10.5194/tc-13-2281-2019 [Nanna B Karlsson, Denmark]	accepted, text revised and suggested reference added to assessment
64387	51	36	51	37	unclear what is meant with though maybe you need to omit even though ocean warming to get a consistent sentence [roderik van de wal, Netherlands]	accepted, text revised
79537	51	37	51	37	I would avoid the use of the word 'subtropical', even tough that term has been used in some high-profile papers. While it is true that the the warm waters in contact with the GrIS are ultimately derived from the subtropics, they are not actually that warm or salty. A better term would be 'modified Atlantic water'/'Atlantic-derived water', as used in many other papers. The deep waters in these fjords are at max ca. 4 C, while SSTs in the subtropics are easily >20 C. [Flor Vermassen, Sweden]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101909	51	37	51	40	"Migration of subtropical water masses around the coast of Greenland ... and its occupation of coastal fjords ... will affect retreat and calving rates ..., but there are gaps in understanding of the processes by which warmer Atlantic waters are driven toward the ice sheet" - The authors may want to consider citing a recent study (Schaffer et al., 2020, Bathymetry constrains ocean heat supply to Greenland's largest glacier tongue, Nature Geoscience, https://doi.org/10.1038/s41561-019-0529-x) presenting impressive observations at Nioghalvfjerdsfjorden (79 North Glacier) and assessing the role of bathymetry for the supply of warm Atlantic water into the cavity. [IAPSO ECS group review, United States of America]	accepted, text revised and suggested reference added to assessment
69631	51	40	51	42	this statement needs citations and confidence assessment [Nicholas Golledge, New Zealand]	accepted, text revised and confidence statements improved
14717	51	41	51	41	"reflects the geography of" -> "reflects the spatial pattern of" [Jeremy Fyke, Canada]	accepted, text revised
107283	51	46			good to summarise obs improveemts (although I think this has also been mentioned earlier in section) but needs statement on how this imapcts the assessment. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text edited to more clearly explain post-SROCC advances in understanding.
82127	51	51	51	51	In addition mention IceSAT-2 [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	not applicable, text removed
84879	51	52	51	52	The increasing availability of satellite observations is mentioned. In this respect, perhaps one of the main recent developments in satellite Earth Observation of the Polar regions (and Greenland in particular) was made possible by the dedicated polar acquisition plan of Sentinel-1 with continuous coverage of the Greenland Ice Sheet margin, permitting the production of continuous time-series of ice velocity of the entire Greenland Ice Sheet margin complemented with annual ice sheet wide campaigns. Although Sentinel-1 is mentioned in this sentence the paper to cite for this is: Nagler, T.; Rott, H.; Hetzenecker, M.; Wuite, J.; Potin, P. The Sentinel-1 Mission: New Opportunities for Ice Sheet Observations. Remote Sens. 2015, 7, 9371-9389. https://doi.org/10.3390/rs70709371 [Jan Wuite, Austria]	accepted, text edited to highlight post-SROCC advances, not post-AR5.
103829	51	52	51	52	Also relevant reference : https://doi.org/10.3390/rs11121407 [Philippe Tulkens, Belgium]	accepted, text revised and suggested references added to assessment
80865	51	52	51	52	Also relevant reference : https://doi.org/10.3390/rs11121407 [Louise Sandberg Sørensen, Denmark]	accepted, text revised and suggested references added to assessment
71831	51	54	51	54	Morlighem et al. 2017 is mentioned as a new source for bed topography completely occulting large efforts that have been made (and are or will be included in Morlighem's reconstruction). Large campaigns of bathymetry through the NASA's OMG project (Willis et al. 2018; Wood et al. 2018) and gravimetric surveys (Millan et al. 2018, An et al. 2019a, b) have been made and will definitively help constrain the evolution of Greenland REF: R. Millan, E. Rignot, J. Mouginot, M. Wood, A.A Bjørk, and M. Morlighem (2018). Vulnerability of Southeast Greenland glaciers to warm Atlantic Water from Operation IceBridge and Ocean Melting Greenland data. Geophys. Res. Lett., 45. doi:10.1002/2017GL076561. http://dx.doi.org/10.1002/2017GL076561. An, L., Rignot, E., Chauche, N., Holland, D., Holland, D., Jakobsson, M. et al. (2019a). Bathymetry of southeast Greenland from Oceans Melting Greenland (OMG) data. Geophysical Research Letters, 46. https://doi.org/10.1029/2019GL083953. An L, Rignot E, Millan R, Tinto K, Willis J. Bathymetry of Northwest Greenland Using "Ocean Melting Greenland" (OMG) High-Resolution Airborne Gravity and Other Data. Remote Sensing. 2019b; 11(2):131. https://doi.org/10.3390/rs11020131. Willis, J.K., D. Carroll, I. Fenty, G. Kohli, A. Khazendar, M. Rutherford, N. Trenholm, and M. Morlighem. 2018. Ocean-ice interactions in Inglefield Gulf: Early results from NASA's Oceans Melting Greenland mission. Oceanography 31(2). https://doi.org/10.5670/oceanog.2018.211. Wood, M., Rignot, E., Fenty, I., Menemenlis, D., Millan, R., Morlighem, M., et al. (2018). Ocean-induced melt triggers glacier retreat in Northwest Greenland. Geophysical Research Letters, 45. https://doi.org/10.1029/2018GL078024 [Jeremie Mouginot, France]	accepted, text revised and suggested references added to assessment

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89621	52	1	52	2	Bathymetry / fjord geometry is poorly resolved in many places -- whilst not strictly ice sheet observations, knowledge of fjord geometry (up to and including the outlet glacier fronts) is important for modelling outlet glaciers and ice ocean interactions. In some of the studies mentioned later on (page 53) calving laws require thermal forcing input, which is dependent on the access of open ocean waters to the ice front via fjords. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised and discussion of bathymetry included
32421	52	1	52	2	Not only poor coverage, there are also cases where past ice thickness observations were wrong by hundreds of meters, incl. Morlighem's (e.g. Franke et al., in press, https://doi.org/10.1017/aog.2020.12). This is important for fast-streaming areas and assimilation. [Olaf Eisen, Germany]	accepted, text revised to reflect this
3937	52	2	52	2	examples or references missing for last sentence (sorry, I dont have any in mind) [Sabine Baumann, Germany]	accepted, text revised and assessment improved
67265	52	4	53	23	This appears largely a review of models in general. Much of it is probably not unique to Greenland but also valid for Antarctic ice sheet modeling. Perhaps this can be restructured? [Regine Hock, United States of America]	accepted, text revised
67267	52	4	53	23	Structure: It is not clear what the purpose of this sectin is. If it is about general status of modling past and future then it is odd that there is no section on past modeling but one on future modeling (9.4.1.2). Are the examples / results given only past modeling? If they are future, shouldn't they be in 9.4.1.2? [Regine Hock, United States of America]	accepted structure improved, model evaluation is a separate section in FGD
22601	52	6	52	34	How much of this is unique to GrIS as opposed to generic applying also to the Antarctic Ice Sheet? Should it be split into a discussion of generic challenges of modelling ice sheets and then a specific discussion of issues particular to the GrIS? This would then avoid any need for repetition when discussing Antarctic Ice Sheet because the reader could then be referred to the generic discussion here? [Peter Thorne, Ireland]	accepted, text revised and structure improved
69633	52	6	53	23	this section is really nicely written and easy to read, but isn't quite IPCC-style 'assessment' - could it be tightened up? [Nicholas Golledge, New Zealand]	accepted, text edited to more clearly explain post-SROCC advances in understanding.
31605	52	6	53	23	This is a very informative section, but one additional detail would be useful for impact and adaptation chapters: in CMIP5, there was a factor 2 between greenland mass losses estimates as computed by CMIP5 GCMs and the observations (e.g., Hanna et al 2018; Delhasse et al 2018). Is it still the same for CMIP6 models outcome, and does this gap mean that high-end scenarios should consider the greenland contribution of sea-level rise as well (and not only Antarctica). [Gonéri Le Cozannet, France]	accepted, text revised
107285	52	6			different types of models …impies that models used for projection and reconstruction are different - they are not [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
15829	52	6			"SMB" is a glaciological jargon, replace it with "surface processes" [Olga Sergienko, United States of America]	accepted, text revised
61659	52	7	52	7	The bit 'ice dynamic models’ should be replaced by ‘ice sheet models for the representation of ice dynamics’. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text edited.
77695	52	7			Paragraphs 3, 4, 5 of model evaluation describe each type of ice model. Ice dynamic models line 7 are (standalone) ice sheet models, correct? Perhaps state as ice sheet models or ice flow models, consistent with 'ice flow models' described in Paragraph 3? [Emer Griffin, Ireland]	accepted, text edited.
61661	52	8	52	8	I would add ‘fully’: ‘These have not yet been fully coupled together...’ There exist some coupling methods between ice sheet and RCM models, e.g. Edwards et al. (2014). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
14723	52	8	52	9	"These have not yet been coupled together to include all feedbacks" -"These have not yet been coupled together to include all feedbacks, within global Earth system models, although this is a current priority." [Jeremy Fyke, Canada]	not applicable, text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45343	52	10	52	34	It is important to include that accumulation rates derived from ice-penetrating radar (representing average values over the past few hundred years or more) consistently show that RCMs are underestimating accumulation rates in central and northern Greenland. Especially northeastern Greenland. This questions whether the distribution of mass is correct in the model and thereby their ability to project future trajectories (refs: Karlsson et al., 2016, Accumulation Rates during 1311–2011 CE in North-Central Greenland Derived from Air-Borne Radar Data, Front. Earth Sci. 4:97. doi: 10.3389/feart.2016.00097. Lewis et al., 2017, Regional Greenland accumulation variability from Operation IceBridge airborne accumulation radar, The Cryosphere, 11, 773–788, 2017. doi:10.5194/tc-11-773-2017, Karlsson et al., 2020. Surface accumulation in Northern Central Greenland during the last 300 years. Annals of Glaciology 1–11. https://doi.org/10.1017/aog.2020.30). Ice-penetrating radar studies have also shown that RCMs underestimate accumulation rates in Southeast Greenland during the past decade 2009-2017 (ref: Montgomery L, Koenig L, Lenaerts JTM, Kuipers Munneke P (2020). Accumulation rates (2009–2017) in Southeast Greenland derived from airborne snow radar and comparison with regional climate models. Annals of Glaciology 1–9. https://doi.org/10.1017/aog.2020.8) [Nanna B Karlsson, Denmark]	accepted, suggested reference added to assessment in Ch9 and Atlas Arctic section
46479	52	11	52	14	"Fettweis, X., Hofer, S., Krebs-Kanzow, U., Amory, C., Aoki, T., Berends, C. J., Born, A., Box, J. E., Delhasse, A., Fujita, K., Gierz, P., Goelzer, H., Hanna, E., Hashimoto, A., Huybrechts, P., Kapsch, M.-L., King, M. D., Kittel, C., Lang, C., Langen, P. L., Lenaerts, J. T. M., Liston, G. E., Lohmann, G., Mernild, S. H., Mikolajewicz, U., Modali, K., Mottram, R. H., Niwano, M., Noël, B., Ryan, J. C., Smith, A., Streffing, J., Tedesco, M., van de Berg, W. J., van den Broeke, M., van de Wal, R. S. W., van Kampenhout, L., Wilton, D., Wouters, B., Ziemen, F., and Zolles, T.: GrSMBMIP: Intercomparison of the modelled 1980–2012 surface mass balance over the Greenland Ice sheet, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-321, in review, 2020." could be cited here [Xavier Fettweis, Belgium]	accepted, the suggested reference added to draft
69723	52	12	52	12	Observations of what quantities? [Matthew Hoffman, United States of America]	accepted, text revised
46481	52	14	52	14	The availability of new reanalysis like ERA5 improves also the model results over current climate according to : "Delhasse, A., Kittel, C., Amory, C., Hofer, S., van As, D., S. Fausto, R., and Fettweis, X.: Brief communication: Evaluation of the near-surface climate in ERA5 over the Greenland Ice Sheet, The Cryosphere, 14, 957–965, https://doi.org/10.5194/tc-14-957-2020, 2020." [Xavier Fettweis, Belgium]	Noted, suggested reference not included, but text substantially revised and assessment improved
30055	52	14	52	17	Recently, Niwano et al. (2018) succeeded in developing and presenting a state-of-the-art high-resolution (5 km) polar non-hydrostatic regional climate model NHM-SMAP. This is introduced by Pattyn et al. (2018, Nature Climate Change, cited in this chapter) as follows “Recent work uses high-resolution, non-hydrostatic atmospheric and detailed SMB models to better represent surface physical processes at scales finer than 10 km.” Therefore, I would like to suggest including the paper by Niwano et al. (2018) as follows: “Reconstructions of SMB evolution extend further back in time with models now covering all or a large part of the 20th century (Hanna et al., 2011; Box and Colgan, 2013; Kjeldsen et al., 2015; Noël et al., 2016; Fettweis et al., 2017; Wilton et al., 2017) with a spatial resolution of up to 1 km (Noël et al., 2016; Wilton et al., 2017; Niwano et al., 2018).” [References] Niwano, M., Aoki, T., Hashimoto, A., Matoba, S., Yamaguchi, S., Tanikawa, T., Fujita, K., Tsushima, A., Iizuka, Y., Shimada, R., and Hori, M. (2018). NHM–SMAP: spatially and temporally high-resolution nonhydrostatic atmospheric model coupled with detailed snow process model for Greenland Ice Sheet. Cryosph., 12, 635-655, doi:10.5194/tc-12-635-2018. [Masashi Niwano, Japan]	accepted, the suggested reference added to assessment
91079	52	17	52	19	I don't fully agree with this statement. There are multiple lines of evidence since AR5 in particular to indicate that recent runoff, and likely SMB changes are unprecedented on longer time scales. The most relevant is Trussel et al 2018, Nature, which is not cited but should be. It demonstrates, fairly convincingly, that runoff is greater than has been for at least 350 yrs but possibly over last 7k yrs. Although this study doesn't attribute a percentage or value to recent mass balance or runoff trends to AGW it provides compelling evidence for an anthropogenic origin for late 20th and 21st C trends. This is something that is discussed WRT Arctic sea ice but not to ice sheets at all. While the evidence for WAIS is weak for the GrIS it is stronger and should be discussed somewhere in this section. It is surely one of the most important issues pertaining to recent ice sheet behaviour and for the AR6 to consider. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text considerably revised, suggested reference is assessed in SROCC and referred to here

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107291	52	18			not certain why this paragraph is here and does not follow from discussion of SMB models on previous page [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised and assessment improved
109085	52	19	52	19	A comma is needed between "remain" and "relating". [Chaincy Kuo, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
22599	52	19	52	19	Perhaps split the paragraph when starting to discuss projections for readability as the paragraph is fairly long and dense and this feels like a separate(ish) topic? [Peter Thorne, Ireland]	accepted, text revised and structure improved
46483	52	20	52	20	I suggest add: ... included or poorly represented in RCMs "and in their forcing" as the not representation of the blocking event (cited line 23) are due to a problem in the RCMs forcing [Xavier Fettweis, Belgium]	accepted, text revised
82129	52	20	52	25	Should this list of supposed model failings (most are specific to particular models), lead on to a requirement for in situ observations/process studies? [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	noted, requirement list not made, but assessment of models improved
30057	52	22	52	22	I would like to suggest that the sentence "These include the presentation of clouds (Van Tricht et al., 2016; Hofer et al., 2019), " can be updated as follows: "These include the presentation of clouds (Van Tricht et al., 2016; Hofer et al., 2019; Niwano et al., 2019)," The state-of-the-art high-resolution polar regional climate model employed by Niwano et al. (2019) is the one and only non-hydrostatic model coupled with a detailed physical snowpack model. This implies that the representation of clouds by the model is very realistic. [References] Niwano, M., Hashimoto, A., and Aoki, T. (2019). Cloud-driven modulations of Greenland ice sheet surface melt. Sci. Rep., 9, 10380, doi:10.1038/s41598-019-46152-5. [Masashi Niwano, Japan]	accepted, text revised and suggested reference added to assessment
46503	52	22	52	23	"blocking index" over Greenland does not seem correct here. Surely you just mean that representations of the actual process of "blocking" should be / be better represented? This could be corrected by changing "blocking index over Greenland" to "blocking over Greenland" (as a side note, you could better clarify the jargon of "blocking" by calling it "high pressure blocking") [Stephen Price, United States of America]	accepted, text revised
72007	52	22			representation [John Church, Australia]	Accepted. Corrected.
16397	52	23	52	23	Inconsistency with reference in line 21 (van den vs Van Den) [Julian Mak, China]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
88635	52	23	52	23	Which is the correct: Van Den Broeke et al., 2016 or van Den Broeke et al., 2016 ? [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
88475	52	23	52	23	change Van Den Broeke to van den Broeke [Bert Wouters, Netherlands]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
90691	52	23	52	25	Regarding observed mass loss of GIS, I suggest these recent works based on satellite data such as Microwave radiometer, CryoSAT, & GRACE: Liang, L., Li, X., and Zheng, F., 2019, Spatio-Temporal Analysis of Ice Sheet Snowmelt in Antarctica and Greenland Using Microwave Radiometer Data, Remote Sensing, 11(16), 1838; https://doi.org/10.3390/rs11161838 Luthcke et al., 2013, Antarctica, Greenland and Gulf of Alaska landice evolution from an iterated GRACE global mascon solution, J. Glaciol., 59, 613–631. Sasgen,et al., 2013, Antarctic ice-mass balance 2003 to 2012: regional reanalysis of GRACE satellite gravimetry measurements with improved estimate of glacial-isostatic adjustment based on GPS uplift rates, The Cryosphere, 7, 1499–1512, https://doi.org/10.5194/tc-7-1499-2013 Schrama et al., 2014, A mascon approach to assess ice sheet and glacier mass balances and their uncertainties from GRACE data, J. Geophys. Res.-Solid Earth, 119, 6048–6066. [Thian Yew Gan, Canada]	taken into account, suggested references are pre-SROCC, text is revised with post-SROCC references

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30059	52	26	52	27	I would like to suggest that the sentence "--- and the retention and lateral and heterogeneous vertical transport of meltwater in the ice (Machguth et al., 2016a; Steger et al., 2017; MacFerrin et al., 2019)." should be updates as follow: "--- and the retention and lateral and heterogeneous vertical transport of meltwater in the ice (Machguth et al., 2016a; Steger et al., 2017; Niwano et al., 2018; MacFerrin et al., 2019)." The reason is that Niwano et al. (2018) present the sensitivity of the Greenland ice sheet surface mass balance to the choice of vertical water movement calculation schemes, which include a simple so-called bucket scheme and a very detailed the Richards equation scheme. [References] Niwano, M., Aoki, T., Hashimoto, A., Matoba, S., Yamaguchi, S., Tanikawa, T., Fujita, K., Tsushima, A., Iizuka, Y., Shimada, R., and Hori, M. (2018). NHM–SMAP: spatially and temporally high-resolution nonhydrostatic atmospheric model coupled with detailed snow process model for Greenland Ice Sheet. Cryosph., 12, 635-655, doi:10.5194/tc-12-635-2018. [Masashi Niwano, Japan]	taken into account, text revised and suggested reference added to assessment
61575	52	30	52	30	"submitted" after "van Kampehout et al, 2017" has to be deleted. It is probably due to a mix with "van Kampenhout et al, submitted" at line 33. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
98547	52	30			remove submitted after 2017 [Miren Vizcaino, Netherlands]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
46485	52	33	52	33	Cite the GrSMBMIP paper here (Fettweis et al., 2020) [Xavier Fettweis, Belgium]	accepted, suggested reference added to assessment
77811	52	33	52	33	Gregory et al. (submitted https://doi.org/10.5194/tc-2020-89) also used elevation classes in a GCM to obtain a reasonable simulation of GrIS SMB. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised and suggested reference added to assessment
82133	52	33	52	34	This statement only makes sense if there has been an earlier statement of confidence in the o0bservations, to pretend that the observations are truth when various methods are not actually consolidated as Shephard at al 2014 shows dispite the title. Can the mass budget actually balance from individual satellite 'observations' (LW, SW, Turbulent)? CF attempts to close the sea level budget [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
98549	52	33			submitted -> published [Miren Vizcaino, Netherlands]	accepted, text revised
46507	52	36	52	55	In the context of arguing and/or quantitatively demonstrating an improvement in predictive skill of (Greenland) ice sheet models from AR5 to AR6, the work of Price et al. (2018) may be relevent to note here (Price, S. F., Hoffman, M. J., Bonin, J. A., Howat, I. M., Neumann, T., Saba, J., … Nowicki, S. M. J. (2017). An ice sheet model validation framework for the Greenland ice sheet. Geoscientific Model Development, 10(1), 255–270. https://doi.org/10.5194/gmd-10-255-2017). [Stephen Price, United States of America]	not applicable, text removed
107287	52	36			repeats many of the points made in paragraph starting p52, 46 [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text restructured.
82131	52	37	52	39	Actually the model initialisation techniques have moved on surprisingly little after the pioneering work of Arthern & Gudmundsson (2010) DOI: 10.3189/002214310792447699 [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text edited to more clearly explain post-SROCC advances in understanding.
61697	52	38	52	39	I suggest to update the citation list of data assimilation method used to initialise the Greenland ice sheet model with Le clec'h et al., 2019 (in GMD) which also resume different data assimilation methods. Le clec'h, S., Quiquet, A., Charbit, S., Dumas, C., Kageyama, M., and Ritz, C.: A rapidly converging initialisation method to simulate the present-day Greenland ice sheet using the GRISLI ice sheet model (version 1.3), Geosci. Model Dev., 12, 2481–2499, https://doi.org/10.5194/gmd-12-2481-2019, 2019. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised and suggested reference added to assessment
129477	52	40			Regarding the use of increasingly large ensembles and Bayesian inference methods to study model sensitivity and parameter uncertainty (Saito et al, 2016; Aschwanden et al, 2019; Schlegel et al, 2018; Schlegel et al 2016; Schlegel et al, 2015; Schlegel et al, 2013; Larour et al, 2012), there is an entire body of literature using Bayesian inference methods missing, leveraging Monte Carlo Sampling methods and that are not ensemble based. [Trigg Talley, United States of America]	accepted, text edited to more clearly explain post-SROCC advances in understanding.

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15831	52	46	52	49	The sentence "In particular, the improved bedrock topography map of Greenland (Morlighem et al., 2017) has provided greater detail for models to capture the complex flow pattern of outlet glaciers (Aschwanden et al., 2016). There is, however, still insufficiently accurate observations of subglacial topography in southeast Greenland, which results in poor agreement between modelled and observed surface velocity in this area (Aschwanden et al., 2016). " is almost identicl to two last sentences of the previous paragraph p.51 line 54 - p.52 line 2. [Olga Sergienko, United States of America]	accepted, text revised.
15851	52	46	52	49	The sentence "In particular, the improved bedrock topography map of Greenland (Morlighem et al., 2017) ..." is almost identicl to two last sentences of the previous paragraph p.51 line 54 - p.52 line 2. [Olga Sergienko, United States of America]	accepted, text edited.
22603	52	47	52	49	What is the reason for this paucity of information? To be actionable (remediable) information a policy maker will need to know why. Is it resource, accessibility, complexity? [Peter Thorne, Ireland]	taken into account, text revised
32423	52	48	52	48	See previous comment line 1/2: Not only poor coverage, there are also cases where past ice thickness observations were wrong by hundreds of meters (e.g. Franke et al., in press, https://doi.org/10.1017/aog.2020.12). ice thicknesses are also partly wrong, which makes assimimilation difficult. [Olaf Eisen, Germany]	accepted, the text now deleted.
111329	52	48	52	49	There is a body of literature evidencing that subglacial topography data have higher uncertainty in marginal regions of West Greenland. A discussion of this and its importance for determining and validating surface runoff observations is important considering the low correspondence between observed and regional climate model runoff. [Samiah Moustafa, United States of America]	accepted, text revised
46505	52	49	52	54	"Models that assimilate … mismatches with observed present day observed velocity and geometry of the ice sheets." This statement is pointing out the contradiction and tradeoffs between standard "spin-up" methods vs. data assimilation methods. It reads as if there has not been progress on rectifying the two approches when in fact the Perego et al. (2014) approach (referenced above) is targetting, and shows progress towards exactly that (Perego, M., Price, S., & Stadler, G. (2014). Journal of Geophysical Research : Earth Surface Optimal initial conditions for coupling ice sheet models to Earth system models. Journal of Geophysical Research: Earth Surface, 119, 1894–1917. doi:10.1002/2014JF003181). [Stephen Price, United States of America]	taken into account, text edited to more clearly explain post-SROCC advances in understanding.
88637	52	54	52	54	The references Goelzer et al., 2017, Goelzer et al., 2018a and Goelzer et al., 2018b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
69635	52	54	52	55	Is this confidence statement related to the preceding sentences? I think the reason(s) for assigning medium confidence here need to be more explicitly stated. [Nicholas Golledge, New Zealand]	taken into account, text revised to more clearly explain post-SROCC advances in understanding and assessments improved
107289	52	54			this is a misleading statement since the uncertainties on ice dyanmics are not dominated by the bathymetric boundary conditions but by process understanding of calving. Read at face value implies that SMB uncertainty dominates dynamics. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text clarified.
14725	52	55	52	55	" uncertainties in future SMB" -> "uncertainties in future SMB related to uncertainty in future climate forcing". Cite Fyke et al. 2014 if justification needed on this statement (https://link.springer.com/article/10.1007/s00382-014-2050-7) [Jeremy Fyke, Canada]	accepted, text revised
9005	52	55	53	1	I completely disagree with the statement that uncertainties in SMB are greater than uncertainties in ice dynamics. Uncertainties in SMB are large but well bounded. Uncertainties in ice dynamics are larger and the limits are much higher. The marine based sector of Greenland holds a 3 m global sea level rise. You will never get that from SMB within less than several centuries. Secondly the statement says "arising from bathymetry" yet bathymetry is only part of the problem, you also need to have the correct ocean forcing at the boundary. The latter is even more difficult. [Eric Rignot, United States of America]	accepted, text revised and assessment improved
116867	52		52		There are aspects of model evaluation and bias correction in this chapter which could be better integrated with the approaches of ch 3 and ch 10. The section on model evaluation needs summary statements. [Valerie Masson-Delmotte, France]	accepted, text revised
80519	53	1	53	1	"uncertainties in ice dynamics arising from bathymetric boundary conditions". Uncertainty in ice dynamics arise from much more than only bathymetric boundary conditions. Is the intention to seprate this particular uncertainty? If not this should be formulated more generally. [Heiko Goelzer, Belgium]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85305	53	3	53	3	Is it worth stating something along the lines that 'Modelling the Antarctic shelf near-coastal T-S characteristics to drive ice-shelf models is extremely challenging, with serious implicatons for confidence in ice-shelf model projections? For example, there are considerable biases in the near coastal and offshore wind stresses, eddies, coastal and slope currents, larger scale Southern Ocean T and S characteristics, sea-ice characteristics, etc, all of which significiantly adversely impact on Antarctic shelf T-S characteristics.' For example, large temperature biases near the west antarctic ice shelves in the Hadley Centre N216-1/4 HadGEM3 GC3.1 coupled climate model currently make it impossible to even use our new active ice shelf model to make projections. In fact, presumably the same applies to Greenland ice-shelves, where models certaintly do not adequately capture T-S characteristics in narrow fjords and I suspect would not do even with downscaling efforts given the issues with biases in the larger scale boundary conditions from coupled models? Given these issues and the huge complexities of ice-sheet dynamics are we confident that we can really robustly provide upper limits via expert judgement on sea-level rise due to ice-shelf melt (or might it be better to say we cannot provide such estimates with sufficient confidence or to add qualification to very clearly explain our confidence in these estimates)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, the upper limit and uncertainty in projected ice sheet sea level contribution is discussed in box 9.4 high-end sea level projections for 2100
811	53	7	53	9	Choi et al 2017 should also be included here - Choi, Y., et al. "Modeling the response of Nioghalvfjerdsfjorden and Zachariae Isstrøm Glaciers, Greenland, to ocean forcing over the next century." Geophysical Research Letters 44.21 (2017): 11-071. [Michael Wood, United States of America]	accepted, suggested reference added to assessment
90477	53	19	53	19	are parenthesis needed in "(Muntjewerf et al, submitted, b)"? [Holly Kyeore Han, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
33443	53	19	54	20	Change the reference format: “Muntjewerf et al., submitted, b” by “Muntjewerf et al., submitted b”. [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
32425	53	19			remove some of the () surounding Muntjewerf citation. [Olaf Eisen, Germany]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
65961	53	21	46	23	Suggest clarification of what "to the increase in sea-ice coverage" refers to. Is this the increase from 1979-2015 that is mentioned on the following page (page 47)? [Kushla Munro, Australia]	Accepted. SECTION 4.3, text revised to clarify
14727	53	22	53	22	"and still require extensive development" -> "and are still undergoing extensive development" [Jeremy Fyke, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
77813	53	23	53	23	Gregory et al. (submitted https://doi.org/10.5194/tc-2020-89) have used a GCM with elevation classes and improved SMB simulation coupled to a dynamical ice-sheet model to study the future of GrIS and irreversibility of ice loss. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, the suggested reference added to assessment
69673	53	24			Regarding "representation of blowing snow process", Sugiura and Ohata (2008) focused attention on blowing snow process and globally estimated the amount of sublimation in hydrological-cycle estimates. Sugiura, K., and Ohata, T. (2008). Large-scale characteristics of the distribution of blowing-snow sublimation. Annals of Glaciology 49, 11-16. [Konosuke Sugiura, Japan]	noted, text is revised to more clearly explain post-SROCC advances in understanding.
40753	53	26			section 9.4.1.2: isn't it weird to have 5-95 percentiles (i.e. very likely range) here but in the table the likely range? [TSU WGI, France]	Accepted. Table and text are now consistent in how numbers are presented
22605	53	26			The section felt a little unbalanced towards centennial scale projections. Given the scoping of the SYR to consider much longer term commitments I would suggest taking a far more substantive effort at the 2300 timeframe and also committed changes in the GrIS and the timescales. There is some brief discussion at the end but I think this should be substantively increased and also these much longer term aspects must be clearly pulled through to the chapter ES. [Peter Thorne, Ireland]	Noted. In revising the text, we have attempted to be less unbalanced towards centennial scale projection and expand on longer time scales. The issue is that most new work since SROCC target end of century projection, hence why we may still in the end be a little unbalanced.
14843	53	26			The box says that there is low confidence on the mode to simulate LIG. Why would be the confidence higher for simulating the future? [Marie-France Loutre, Switzerland]	Not Applicable. Comment is no longer applicable due to text revisions, which no longer include this confidence statement

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61663	53	28	53	28	I would replace 'unchanged' into 'relatively similar' as the SROCC figures are a little different: 4-12cm for RCP2.6 and 8-27cm for RCP8.5 (SROCC Table 4.2 in Ch. 4). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text revised accordingly.
61699	53	28	53	33	This sentence is very long with lot of references citations and values in parenthesis. The link between the first line providing AR5 GMSL values and the next sentence (starting by 'based on…') is not clear. I suggest to end the first line after the AR5 references citations. The second part of the sentence is not complete. Probably because of the dot instead of the coma after the expert elicitation study citation. I suggest to change the second part of the sentence like this: Based on the recent modelling results using multiple climate and ice sheet models (Edwards et al., 2014; Fürst et al., 2015; Calov et al., 2018; Rückamp et al., 2018; Aschwanden et al., 2019; Golledge et al., 2019), and taking into account a recent expert elicitation study (Bamber et al., 2019), Figure 9.19 and Table 9.1 show the projected GrIS contribution to GMSL by 2100 under selected scenarios. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The text has been revised taking into account this comment. The sentence has been split into multiple smaller sentences and some of the original text has been removed
88639	53	29	53	29	Which is the correct: Churchr et al., 2013a or Church et al., 2013b?. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
32145	53	29			give numbers in m instead of cm matching the numbers cited further down the page [Anja Wendt, Germany]	Accepted. All numbers are now in m
61701	53	30	53	30	What does means "based on the good agreement with more recent modelling results with multiple climate and ice sheet models "? Does it mean "based on the recent modelling results using multiple climate and ice sheet models"? If yes I suggest correcting the sentence. If not, I suggest adding information about WHAT is in good agreement. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. Comment is no longer applicable due to text revisions, which no longer include this sentence
96977	53	33	53	33	The reference to Fig. 9.19 seems to be wrong. [Nicole Wilke, Germany]	Accepted. All figures references have been checked and modified when needed.
129479	53	35	53	39	Unclear sentence. What are the four numbers "respectively" referred to? [Trigg Talley, United States of America]	Accepted. Text has been reworded.
72009	53	37		50	I take it this is the 5-95% range of the models. But is this really the very likely range. Are there other uncertainties that go beyond the model results available - for example ice sheet instabilities not simulated, greater surface darkening, a wider range of global climate projections? [John Church, Australia]	Accepted. We have clarified this section and now discuss in greater details uncertainties that go beyond the ISMIP6 models
61665	53	38	53	39	This part of the sentence is confusing. I would add explicitly that 0.031m is for RCP2.6, 0.089m is for RCP8.5, 0.057m is for SSP1-2.6, and 0.153m is for SSP5-8.5. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Sentence has been clarified. Comment similar to C129479
61667	53	38	53	42	I understand that the lower figures of ISMIP6 (Goelzer et al., 2020) for the sea-level contribution compared to AR5 are partly due to a different period (relative to 2015 in this report vs. 2000 for AR5) and the committed mass change. But these figures are still almost twice lower than AR5 (0.031m vs. 0.07m (0.04-0.1) for RCP2.6 and 0.089m vs. 0.14m (0.07-0.21)) for RCP8.5)… This is a large change. Was there an error detected in the AR5 numbers? Or does the committed change explain a large part of this difference? Is there an impact of the models used and/or the methodology used? By the way, isn't it a strong assumption not to include the committed mass change in Goelzer et al. (2020), although the uncertainty is probably high? I think this relatively large change from AR5 to AR6 needs more explanation, as the people reading the report will check the figures and compare them to AR5 for sure. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We have clarified this section and now discuss in greater details uncertainties that go beyond the ISMIP6 models
33445	53	38	55	6	Change the reference format: "(Nowicki et al., submitted, a)" by "Nowicki et al., submitted a)". [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
65963	53	39	46	43	Suggest clarification noting that the net Antarctic sea-ice cover has been consistently well-below average, and at record low values from September 2016 until February 2020. This suggests that a change in the underlying ocean conditions may have taken place during 2016, rather than being a response only to transient atmospheric conditions. Suggest citing: Reid, P., S. Stammerjohn, R. A. Massom, S. Barreira, T. Scambos, and J. L. Lieser, 2019: Sea ice extent, concentration, and seasonality [in "State of the Climate in 2018"]. Bull. Amer. Meteor. Soc., 100 (9), S178-S18. This citation states: " The persistence of well-below-average total SIE from mid-September 2016 onwards suggests that transient weather systems and/or atmospheric climate modes alone are not responsible for the abrupt switch from record-breaking high to record-low conditions at that time." [Kushla Munro, Australia]	Noted. This comment seems "out of place" and belong to "sea ice". It was therefore passed to sea ice section authors

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82135	53	39	53	42	This is an irrelevant statement, and should be removed.The FUTURE simulations start in 2015 becaue that is when the IPCC scenarios start. Nothing to do with any of the reasons here. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have altered the text. We agree that in ScenarioMIP the future start in 2015, and that our original sentence try to include too many items. However, we do need to keep a statement that indicates that the time range for these new projections are 2015 to 2100 for the reader that is not familiar with the time considered by ScenarioMIP.
103831	53	40	53	40	What is meant by 'additional mass change' in this context? Additional to what? [Philippe Tulkens, Belgium]	Accepted. The text has changed substantially and the original sentence is no longer present. We have however clarified what the additional mass change refers to in Box 9.3.
80867	53	40	53	40	What is meant by 'additional mass change' in this context? Additional to what? [Louise Sandberg Sørensen, Denmark]	Accepted. Same comment to 103831 Text clarified.
88641	53	41	53	41	The references Goelzer et al., 2017, Goelzer et al., 2018a and Goelzer et al., 2018b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
46487	53	42	53	42	In addition to the listed uncertainties, I suggest to add that these projections consider that no change in general circulation in summer (as the ones currently observed) will occur in future. If such changes will occur, the melt increase should be multiplied by a factor 2 according to Delhasse et al. 2018 [Xavier Fettweis, Belgium]	Not Applicable. Comment refers to a sentence that is no longer part of the report
80521	53	43	53	43	"represent a range of possible future climates". Not just a range. The models were specifically chosen to maximise the spead within the CMIP5 ensemble. [Heiko Goelzer, Belgium]	Accepted. The text has been revised in the manner suggested, however it is now in Box 9.3
69637	53	46	53	48	for consistency should we be using likelihood ranges rather than percentiles? [Nicholas Golledge, New Zealand]	Accepted. All numbers are now consistent in the manner in which they are presented here and in the table
91081	53	46	53	49	I couldn't tally the numbers here with those in Table 9.A.1 esp. for the emulator values/ [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have checked that the numbers in the text tally with the ones shown in the table
98571	53	48			It is a coupled ESM-ice sheet not including ocean forcing (ice sheet meltwater forces the ocean though), but it reads as if only the atmosphere and ice sheet components were active.Maybe replace by "coupled ESM-ice sheet" and mention ocean forcing is not included. [Miren Vizcaino, Netherlands]	Accepted. We have modified the text to better reflect the description of the coupled ESM-ice model CESM2-CISM
98551	53	49			submitted-> 2020 (published in GRL) [Miren Vizcaino, Netherlands]	Editorial - copyedit to be completed prior to publication
116869	53		53		Given the importance of ISMIP6 as a novel element compared to SROCC and AR5, could it be possible to have a dedicated table or box, and provide more details including on the choice of CMIP5 models, and what they imply compared to the multi model asesssment done in chapter 3? [Valerie Masson-Delmotte, France]	accepted, table in appendix added for choice of CMIP models and text in box 9.3 discusses ISMIP
109251	54	1	54	11	The negative contributions of the Greenland Ice Sheet in Edwards et al. seem to be inconsistent with current observations and current process understanding. Some discussion of these numbers should happen in the main text. [Maria Zeitz, Germany]	Noted. The text includes the revised numbers from Edwards et al.
33447	54	3	54	20	Add dots before Table: “(Oppenheimer et al., 2019) Table 4.4 for 2081-2100 relative to 1986-2005, ** denotes contribution in 2100 relative to 2000 reported in SROCC Table 4.2, unless noted.”, change by ““(Oppenheimer et al., 2019). Table 4.4 for 2081-2100 relative to 1986-2005, ** denotes contribution in 2100 relative to 2000 reported in SROCC. Table 4.2, unless noted.”. [Guiomar Rotllant, Spain]	Noted. The comment is no longer applicable due to revisions made to clarify and simplify the table.
67269	54	4	54	4	Terminology: the terms dynamic contribution and ice discharge are used in this section. I assume there are the same? Using just one (ice discharge?) consistent with the Box 9.1 would make it less confusing for a reader. [Regine Hock, United States of America]	Noted. Dynamic contribution is no longer used here , as this text was for pre-SROCC studies which is no longer included in the text. However, we have checked for consistency with Box 1 in other places.
80523	54	4	54	14	Replace 'melt' by 'surface meltwater runoff'. Melt can refreeze and not contribute to mass loss. Runoff does. [Heiko Goelzer, Belgium]	Accepted. The text has been revised in the manner suggested
77815	54	8	54	8	You note that Goelzer et al exclude change already committed at 2015. That would be dynamical change, I think, since SMB response is very quick. For fair comparison, you might comment on AR5 DYN. I think it's unclear whether committed change was included in AR5. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Noted. In the revised text we provide a greater discussion on how the work from Goelzer et al compares to AR5 and SROCC
61551	54	9	54	9	The ranges represented for each RCP are not consistently low to high or high to low. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The ranges presented in the table are now consistent
31603	54	9	54	9	In the table, the value for the maximum GCM in Fuerst et al 2016 is 0,16m (not 0,016m) [Gonéri Le Cozannet, France]	Not Applicable. The text revisions no longer include post SROCC studies in the Table

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61577	54	9	54	10	Table 9.1 : add points in SSP1-26, SSP2-45 and SSP5-85 (2.6, 4.5 and 8.5) for consistency with the text. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Points in SSPs have been added for consistency with the text
61579	54	9	54	10	Table 9.1 : I find 0.109m and not 0.104m in Muntjewerf et al, submitted, a (0.023m by the mid-21st century in addition to 0.086m for the second half of the 21st century). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Numbers in Muntjewerf et al. have been checked and corrected in table
32147	54	9	54	10	in line (Aschwanden et al., 2019): correct value for RCP2.6: 0.05 to 0.19, transcription error from paper, delete superfluous zero, I have not checked the other numbers [Anja Wendt, Germany]	Not Applicable. The text revisions no longer include post SROCC studies in the Table
67271	54	9	54	12	is this past/future? Which time periods? [Regine Hock, United States of America]	Accepted. Table 9.1 has been modified so that it is easier to follow
35787	54	9	55	10	Table 9.1 will be extremely difficult for most readers to follow because of the mixture of differing base periods and the labeling of the rows which mixes values of other studies reported in SROCC with values SROCC developed based on those studies. Recognizing that this is a complex matter, I hope a clearer way to present the material can be developed. [Michael Oppenheimer, United States of America]	Accepted. Table 9.1 has been modified so that it is easier to follow
98553	54	9		10	In table: Vizcaino et al 2015 shows a 3-simulation ensemble for RCP8.5, maybe include range of projection? [Miren Vizcaino, Netherlands]	Not Applicable. Comment no longer applicable due to text revisions, which no longer include pre SROCC studies in the Table
98555	54	9		10	In table: remove "relative to 2015" in notes for CESM2-CISM2 and replace for *** for consistency [Miren Vizcaino, Netherlands]	Noted. The comment is no longer applicable due to revisions made to clarify and simplify the table. However, entries in the table are now more consistent
33449	54	13			Change: "…Muntjewerf et al., submitted, a, submitted, b)…" by "…Muntjewerf et al., submitted a, b)…". [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
15833	54	14	54	14	Replace "due to increased melt resulting in decreasing SMB" with "increased surface melt" [Olga Sergienko, United States of America]	Accepted. Text now uses "surface meltwater runoff", a suggestion from comment 80523
77821	54	14	55	21	I feel that this paragraph could be shortened a bit, to emphasise the policy-relevant conclusions. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The paragraph has been rewritten with a focus on policy relevant conclusions.
98561	54	14		16	Given the importance of SMB on future mass loss, the report misses detail on SMB-related processes in the projections (to pair with the part about observations, where detail is given on the role of clouds, turbulent fluxes, etc,…). Here only a general "concurrent with rising temperatures" is given. For instance: Vizcaino et al, 2013 (CESM1) Muntjewerf et al , submitted, b (CESM2-CISM2) and Sellevold and Vizcaino, submitted to JAMES,(CESM2) analyze the role of surface energy fluxes on melt rates, and of the different SMB components (precipitation, refreezing) on the SMB change. These three studies find a negative contribution of incoming solar radiation to future melt, as a result of enhanced cloudiness/reduced transmissivity. The CESM2 studies found an acceleration in those as a result of albedo and turbulent fluxes feedbacks as the ablation area quickly expands for a global warming of around 3 K above preindustrial. Before this acceleration, most of the melt energy is provided by enhanced incoming longwave radiation. After acceleration, the partitioning of melt sources among solar, thermal and turbulent changes substantially. SV and Mb project future decreases in refreezing capacity as a fraction of melt and rain water and increases as a fraction of snowfall up to a 90% limit (then, slight decrease). Other (non-ESM) Energy Balance-based SMB studies can also be used to expand more on SMB-processes [Miren Vizcaino, Netherlands]	Accepted. The text has been revised to expand on SMB-related processes for projections, while at the same time taking into account comment C77821 (which asks for the paragraph to be shortened)
89623	54	16	54	17	There is quite a bit of detail about other processes affecting SMB and dynamics in this section, but there isn't discussion about the interdependence of SMB and outlet glacier dynamics. E.g. dynamic thinning --> surface lowering --> higher ablation [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised to expand on interdependence of SMB and outlet glacier dynamics, while at the same time taking into account comment C77821 (which asks for the paragraph to be shortened)
82137	54	16	54	17	This statement is inconsistent with the previous and subsequent, both of which provide an explanation of the process. There is none here and so the reader is unable to assess the mechanism describing the attribution of confidence. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Comment is similar to other comments that this sentence needs an explanation of the process, which we have now done.
77817	54	17	54	17	Please describe the nature of the dependence, or at least the sign of the correlation. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now describe the nature of the dependence.
90479	54	18	54	18	double space between "discharge" and "will". [Holly Kyeore Han, Canada]	Accepted. The text has been revised in the manner suggested
11159	54	18	54	18	there are two blanks between 'discharge' and 'will' [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised in the manner suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77819	54	18	54	19	You could delete "as marginal thinning and retreat become self-limiting" because you have clearly explained what this means after the references in (). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised in the manner suggested
115465	54	18	54	20	Check grammar. [Robert DeConto, United States of America]	Accepted. The grammar has been checked.
90443	54	18			there is a double-spacing between the words "ice discharge" and "will" [Holly Kyeore Han, Canada]	Accepted. The text has been revised in the manner suggested
98567	54	20	55	2	because (…) and because thinning (e.g., from surface runoff) reduces ice discharge. Also, the retreat can be caused by surface runoff, not solely enhanced ice discharge [Miren Vizcaino, Netherlands]	Accepted. The text has been revised in the manner suggested
107297	54	38			mixing units from cm (at start of paragraph) to m makes things more compliacted than necessary [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Units are now consistent
107295	54	39			this is a key sentence and needs to be unpacked more. There are too many dues tos. Do studies used in SROCC (at start of paragraph) have same issue? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have expanded this sentence. Note: page number is wrong: it should have been p 53.
65967	55	1	47	18	Suggest removing repetition in "(Roach et al., submitted, submitted)". [Kushla Munro, Australia]	Editorial - copyedit to be completed prior to publication
69639	55	3	55	3	'limited understanding' of what? Needs clarification I think. [Nicholas Golledge, New Zealand]	Accepted. We have clarified that the limited understanding referred to ice-ocean interactions
80525	55	3	55	8	The distinction between open and standard experiments has not been analysed in detail in Goelzer et al. (2020) and is largely irrelavant for the Greenland uncertainty discussion. I would suggest to remove it: In ISMIP6, uncertainty in ice-ocean interaction was explored by taking three distinct timelines of tidewater glacier retreat per climate model into account to investigate ice sheet sensitivity to ocean variability (Slater et al., 2019a, 2019b; Nowicki et al., submitted, b). [Heiko Goelzer, Belgium]	Accepted. The text has been revised in the manner suggested
61703	55	7	55	8	Saying, without any more details, that the open approaches show smaller mass loss than the standard approaches doesn't bring any relevant information. To clarify and simplify the message, I suggest removing this sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The text has been revised in the manner suggested
32429	55	11	55	11	The term "emulated" should be introduced, what does mean, how does it differ from the other approaches. [Olaf Eisen, Germany]	Accepted. Emulator is now introduced in Box 9.3
82139	55	13	55	15	Poor sentence structure. The subject is 'uncertainly in basal sliding parameterisation', not Acshwanden et al. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has be rewritten.
32427	55	13	55	17	Citations in text should be without () [Olaf Eisen, Germany]	Accepted. Editorial comment fixed.
33451	55	13			Change: “(Aschwanden et al., 2019) ascribe… by « Aschwanden et al. (2019) ascribe…”. [Guiomar Rotllant, Spain]	Accepted. Editorial comment fixed.
61553	55	15	55	16	The way this sentence is written can be interpreted that the change from basal vs. surface + dynamic changes goes from 50/50 to 86/14, rather than basal + surface vs. dynamic changes [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable. The reasons are explained in answer to comment 98565
39747	55	15		16	"Furst2015 ….3/14% by 2100" this is confusing as basal mass balance encompasses basal melting in contact with the ocean, which is linked to dynamic discharge (through a loss of buttressing). [TSU WGI, France]	Not Applicable. The reasons are explained in answer to comment 98565
98565	55	15			partitioning - of what [Miren Vizcaino, Netherlands]	Not Applicable. The comment refers to a sentence that is no longer included as the text has been revised to focus on new work since SROCC as well as in response to comment 77821 which suggest that paragraph should be rewritten with a focus on policy relevant question
33453	55	15			Change: (Fürst et al., 2015)… by « Fürst et al. (2015)…”. [Guiomar Rotllant, Spain]	Not Applicable. The reasons are explained in answer to comment 98565
33455	55	16			Erase dot: “…2100. (Golledge et…” [Guiomar Rotllant, Spain]	Not Applicable. The reasons are explained in answer to comment 98565
98569	55	17	55	18	Muntjewerf et al do not include ocean forcing, citation should be removed [Miren Vizcaino, Netherlands]	Accepted. The citation has been removed
33457	55	17			Change the reference format: “Muntjewerf et al., submitted, a” by “Muntjewerf et al., submitted a”. [Guiomar Rotllant, Spain]	Accepted. Text has been revised accordingly.
67273	55	19	55	19	more' than what? Or do you mean 'increasingly' [Regine Hock, United States of America]	Accepted. Text has been revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9009	55	19	55	21	Over the past 40 years, 60% of the mass loss has been driven by glaciers and it is very likely that most of this loss was driven by ocean forcing on the tidewater glaciers. On what premise can the authors state that in the future, only SMB will matter? I know this is based on models, but these models do not include the ocean and do not agree with 40 years of observations!! if you do not point out this discrepancy, who will? [Eric Rignot, United States of America]	Noted. The text has been revised to highlight that ocean triggered mass loss maybe more important in the future than what many are models currently suggesting. The text now also includes new studies that show the importance of ocean driven change.
71813	55	19	55	21	I strongly disagree with the "high confidence" used here. If models show that the future of Greenland is dominated by SMB, it is because they can properly model ice-ocean interaction (as stated in the same paragraph) and because of our limited knowledge of the bed topography in the ice-sheet interior that currently rapidly disconnect glacier from ocean as they retreat. There are nothing in current records (about 30-50 years) of the evolution of mass changes in Greenland indicating that mass loss is or will be dominated by SMB. Therefore I would recommend to use a milder statement than "high confidence". [Jeremie Mouginot, France]	Noted. Comment similar to 9009. The high confidence has been changed to medium confidence
82945	55	20	55	20	I suggest to write "meaning" instead of "and". I understand the last part of the sentence as an explanation for what is written in the first part of the same sentence. [Sebastian Gerland, Norway]	Accepted. Editorial comment fixed.
67905	55	23	55	23	note that Aschwanden et al., 2019 explicitly separate the SMB-elevation feedback [Martin Truffer, United States of America]	Not Applicable. Comment refers to a section that is no longer part of the report
357	55	23	55	46	Melt processes and feedbacks are discussed here but not the trend in actual melt values. How did they evolve? What about a recent decline in ice sheet-wide melt, NAO related? More results would be welcome here, except if they are provided elsewhere. [Etienne Berthier, France]	Not Applicable. Comment refers to a section that is no longer part of the report
90481	55	24	55	26	The sentence "The feedback…. may diverge from coupled model results….". It is not very clear what it means for feedbacks to diverge. Could write that the outputs of the paramarmetrized models diverge from those from coupled models? (If that is what is originally intended to mean). [Holly Kyeore Han, Canada]	Not Applicable. Comment refers to a section that is no longer part of the report
65965	55	25	46	27	Suggest removing repetition in "Haumann et al., submitted , submitted" and "Mackie et al., submitted, submitted". [Kushla Munro, Australia]	Not Applicable. Comment refers to a section that is no longer part of the report
82141	55	26	55	27	Why would it deviate from coupled models? This is because of elevation induced atmospheric circulation changes, but this needs to be said. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Comment refers to a section that is no longer part of the report
90483	55	27	55	28	change "it" to "these" or "those" since "estimates" is plural? [Holly Kyeore Han, Canada]	Not Applicable. Comment refers to a section that is no longer part of the report
90485	55	27	55	28	"Estimates" has been "estimated" sounds redundant. [Holly Kyeore Han, Canada]	Not Applicable. Comment refers to a section that is no longer part of the report
98557	55	29			11% in Vizcaino et al. 2015 [Miren Vizcaino, Netherlands]	Not Applicable. Comment refers to a section that is no longer part of the report
77823	55	31	55	31	Gregory et al. (submitted https://doi.org/10.5194/tc-2020-89) find the SMB-elevation feedback increases the negative SMB perturbation by 20% on average in the second and third centuries of prolonged warm climate (like late 21st century), but on the longer term it is much less important than the contraction of area and the effects on regional climate of the changing topography, which give strong negative feedbacks on mass loss. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Comment refers to a section that is no longer part of the report
82143	55	33	55	33	.. as suggested by Zwally et al 2002 (DOI: 10.1126/science.1072708).. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Comment no longer applicable due to text revisions, which no longer include pre SROCC studies
67275	55	33	55	34	This was doubted by many already before. Perhaps reword so it does not come across as something entirely new. [Regine Hock, United States of America]	Not Applicable. Comment no longer applicable due to text revisions, which no longer include pre SROCC studies
14729	55	35	55	35	Include Hoffman et al. 2016 (https://www.nature.com/articles/ncomms13903) in list of 'nuanced understanding of influence of basal meltwater' references [Jeremy Fyke, Canada]	Not Applicable. Comment no longer applicable due to text revisions, which no longer include pre SROCC studies
16399	55	36	55	36	Chronological order in referencing. [Julian Mak, China]	Not Applicable. Comment no longer applicable due to text revisions, which no longer include pre SROCC studies
61705	55	40	55	41	This sentence does not provide helpful information and makes the text more complex. I suggest removing this sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence has been removed as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107299	55	43			needs a summary state saying what the projection is and what confidence exists (eg increased confidence if number of independent studies agree). [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now provide a summary and confidence statement
107301	55	43			long-term is sfficiently important to warrant its own subsection. Lost here. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised, new subsection 9.4.1.4 Projections beyond 2100 created
82145	55	44	55	44	The original study on commitment by Gregory at al (2004, DOI: 10.1038/428616a) is still relevant in this context, so it sould not be stated that such studies are absed on one model. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised
16401	55	44	55	45	"...found A highly non-linear response..." or "...found highly non-linear responseS..." [Julian Mak, China]	Accepted. Editorial comment fixed.
18109	55	44	56	3	"Large and irreversible future decline of the Greenland ice-sheet ", Gregory, George and Smith (2020, ms. under review at The Cryosphere www.the-cryosphere-discuss.net/tc-2020-89) is very relevant here and uses a much more complex model than Robinson et al. (2012) [Robin Smith, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested publication is included in FGD
72011	55	44			Note there is a new publication by Gregory et al. that specifically addresses this issue. [John Church, Australia]	Accepted. Suggested publication is included in FGD
65969	55	45	50	46	Suggest adding an apostrophe to "ice sheets' mass". [Kushla Munro, Australia]	Accepted. Editorial comment fixed.
99539	55	45	55	47	This seems like an important result so suggest including in the Executive Summary. Also, please clarify what global warming level the 1.8 deg regional summer warming equates to. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Warming level statements for Greenland Ice sheet included in ES
98559	55	47		48	remove "for a decline of the GrIS" or otherwise add "irreversible" [Miren Vizcaino, Netherlands]	Accepted. The text has been revised
89397	55	50	56	3	"These estimates rely upon a single, relatively low-complexity model" -- Not sure if this is true? (According to the supplementary information, SICOPOLIS+REMBO (GIS), PISM (AIS), and Marzeion et al. 2012 glacier model were used.) [Ricarda Winkelmann, Germany]	Noted. The text has been revised
98563	55	50		53	I do not understand this part and how it links to the following sentence [Miren Vizcaino, Netherlands]	Noted. The text has been revised
80527	56	5	56	6	There are a couple of issues with the experimental setup of the Aschwanden study that make it problematic to state the results without further discussion. The main reason for the large sensitivity and fast response in their study are the very high positive degree day factors sampled in the upper range of their parameter space. Any attempt tp properly initialise an ice sheet model with those PDD factors would fail, becasue the ice sheet would simply not be sustainable for the resulting forcing. Another issue that leads to an overestimation of the retreat is that the temperature forcing is taken as spatially constant from an average over a large area over and around Greenland that leads to a warm bias compared to spatially resolved forcing. These biases should be mentioned when comparing results to other studies. [Heiko Goelzer, Belgium]	Noted. The text has been revised and now includes estimate of when Aschwanden et al 2019 simulation reaches the 4m SLE threshold and discussion of these results earlier in the text
32431	56	6			What is meant with "84th percentile"? Unclear. [Olaf Eisen, Germany]	Noted. The text has been revised
109253	56	7	56	8	Differences for the timeframe of Greenland Ice Sheet loss could also be explained by different forcing temperatures. In Aschwanden et al. (2019) the maximal local temperature of approx 12 degrees is reached within 1000 years, while Huybrechts reaches similar local temperatures only after 3000 years. The statement "medium confidence" should be re-evaluated in light of the different forcings. [Maria Zeitz, Germany]	Accepted. Text revised, the statement in SOD now removed
77825	56	8	56	8	This threshold isn't really for irreversible loss, I would say. In those studies, the ice sheet is lost if a climate warmer than that threshold is maintained indefinitely, whereas if the climate is lower than the threshold, the ice sheet is retained, perhaps in a somewhat reduced state. Irreversibility refers to whether it can be regrown if a cooler climate is restored e.g. Ridley et al. (2010). The threshold for irreversibility is a different quantity, which refers to the size or shape of the ice sheet, not the climate. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text revised
77827	56	8	56	8	Gregory et al. (submitted https://doi.org/10.5194/tc-2020-89) find that there is no threshold for loss of the ice sheet. Instead, there is a continuous and fairly linear relationship between the initial SMB perturbation and the final steady state under a warmer climate. Under the recent climate, indefinitely maintained, GMSLR is about 0.5-2.5 m in equilibrium; under the warmest climates considered (with $4xCO_2$, more extreme than RCP8.5 at 2100), the rate of loss is initially large, about 3 mm yr^{-1} SLE, but it slows down and equilibrium is reached in ~10 kyr with almost complete loss i.e. GMSLR of about 7 m. There is a threshold for irreversibility; states of less than about 4 m SLE do not regrow to the full size, and about 2 m GMSLR consequently becomes irreversible. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Publication included in FGD and discussion of the threshold for irreversibility included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14731	56	8	56	8	PLACEHOLDER FOR FURTHER ASSESSMENT WITH NEW LITERATURE - if it makes sense to Authors, add Fyke et al. 2014 (https://link.springer.com/article/10.1007/s00382-014-2050-7) in discussion of uncertainty of GrIS response related to climate uncertainty (specifically, polar amplification and equilibrium climate sensivity). This study is independent to Robinson, 2012. [Jeremy Fyke, Canada]	Noted. Suggested reference not included because the focus of the text has changed
29247	56	11	58	46	While Box 9.2 is very helpful, it seems like its utility could be significantly enhanced by including some kind of visual with the box. In particular, I think that a timeline of some sort to illustrate the timing of the various insights from paleo evidence that are discussed, and the confidence levels we have for current or future ice sheet evolution from these lines of paleo evidence, would be really useful to readers. [Andra Garner, United States of America]	Rejected. There is no space for additional figures, and it's not clear how such a figure as described could be produced in a way that would be meaningful, given the large uncertainties in both proxy and model insights.
100715	56	13	56	13	Note: I would add some text at the beginning of this section to say _what_ the range of rates of change in Greenland and Antarctic ice sheet have been. This is implicit in the subsequent discussion of forcings, but some of these are truly startling. For example, for the Miocene Climatic Optimum, changes in sealevel (Miller et al., 2020) indicate that 2/3 of the Antarctic ice sheet (40 m sealevel rise) could have melted off in c. 5 -10 kyr. Yes, boundary conditions were quite different. But this points to a major difference between short-term vs. long-term responses to climate change. [Matthew Kohn, United States of America]	Accepted. Where possible we have identified similarities in rates-of-change between past and future periods, but uncertainties in deep-time reconstructions preclude firm statements.
67285	56	13	58	43	Very nice box, complementing and not repeating SROCC ! Most subsections have a concluding summary statement. Can those be added to the Atmosp and ocean forcing sections? [Regine Hock, United States of America]	Accepted. Thank you for the very nice comment. In rewriting the text it has been possible to add summary statements to the end of nearly all paragraphs.
68925	56	13	58	44	Box 9.2 is among the most in-depth and extensive treatments of paleoclimate information anywhere in the WG1 report. I learned a lot from it, but I had trouble relating it to our policy-relevant paleo key messages. Can it be revised to deliberately address one or more of the paleo key messages, and to summarize the main points in the CH9 Executive Summary so that they can be used in support of the findings in the Technical Summary? Please summarize the information in a few sentences to address the paleo key message about (1) how well models with paleoclimate forcings simulate large-scale Earth system changes, and (2) the long-term effects of sustained warming. In addition, this box would seem to be an appropriate place to highlight the evidence for abrupt transitions in the ocean-cryosphere system including D-O events. [Darrell Kaufman, United States of America]	Accepted. The text has now been significantly revised to ensure a clearer pathway from assessment to Executive Summary statements.
72013	56	13	58	44	Suggest add a section comparing the forcing of the two ice sheets today with rates of forcing in earlier periods. [John Church, Australia]	Accepted. Although there isn't enough space to add a whole new section, the revised text now makes clear links between rates or magnitudes of past forcing with those projected for future scenarios, as far as possible.
107465	56	13			First section for Box 9.2 on atmospheric forcing includes information about Greenland and Antarctic ice sheets. Nothing else is mentioned about Greenland Ice Sheet after this section. Can anything more be said about Greenland for the other sections? [Jennifer Walker, United States of America]	Accepted. Text revised throughout to include more information on Greenland and other ice sheets.
88019	56	13			boxes should stand indipendently from the main text, thus, acronyms should be avoided or explained. [Georg Kaser, Austria]	Noted. The text is now a section of the Chapter, rather than a Box, so this is no longer an issue.
67907	56	17	56	17	what's MPWP? [Martin Truffer, United States of America]	Accepted. This acronym is explained elsewhere in the Report and so is not expanded here.
2989	56	17	56	17	Perhaps write out the acronym MPWP (Mid-Pliocene Warm Period) for reader friendliness. [Petteri Uotila, Finland]	Accepted. This acronym is explained.
67277	56	17	56	17	What is MPWP? Avoid acronyms (not only this one in this box), esp in stand-alone boxes. [Regine Hock, United States of America]	Accepted. This acronym is explained.
15835	56	17	56	17	What does "MPWP" stand for? [Olga Sergienko, United States of America]	Accepted. This acronym is explained.
82147	56	17	56	17	Box should be self contained, hence need to spell out MPWP [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This acronym is explained.
67279	56	17	56	19	not clear: it took 1000 to 2000 years after when/what? [Regine Hock, United States of America]	Accepted. The revised text no longer has this ambiguous statement.
109873	56	17	56	31	This may be confusing to some readers. Speaks of reaching a minimum in the MPWP, then of there being no ice sheet at the Pliocene-Pleistocene transiton. This could be clarified by (1) reminding the reader of the age of the MPWP, (2) briefly stating the known history of the GRiS in the late Pliocene, and (3) being explicit about starting the Pleistocene with no GRiS. [Donald Forbes, Canada]	Accepted. The text has now been revised in such a way that this apparent conflict no longer exists.
115467	56	17	56	55	Based on the title of this box, I was surprised this jumps right into model results rather than observations. The box sort of jumps around between model results and observations, without clarity on what's what. Perhaps some rearanging would be helpful, and/or include 'modelling' in the title of the box. [Robert DeConto, United States of America]	Accepted. The revised text is now structured chronologically rather than by process, and the distinction between empirical and model studies more clearly identified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39799	56	17			"MPWP" as the box can be read on it's own it's maybe good to spell out the acronym [TSU WGI, France]	Accepted. This acronym is explained.
82149	56	18	56	18	High Confidence'??? Given the poor knowledge of the forcing, the use of standalone ice sheet models (much discussion in this chaper about how unreliable these are for multi-century simularions), I would say Low confidence. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The revised text no longer has this confidence statement.
67281	56	20	56	20	Pliocene: perhaps add when that was? [Regine Hock, United States of America]	Accepted. A table now defines the periods here.
82151	56	21	56	21	This is a very simplistic interpretation of the impacts of CO2 which changes winds, stormtracks, ocean mixing,heat exchange between ocean and atmosphere etc. To state theat the impacts on the ice sheet are atmosphere driven and not ocean is misleading, particularly for a limited region. [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable because the text was deleted.
67283	56	22	56	22	changes': better 'increases' to indicate the direction [Regine Hock, United States of America]	Not applicable because the text was deleted.
100213	56	29	56	55	Re-write: Model-data reconstructions during the Pliocene-Pleistocene transition (3-2.5 Ma) indicate GrIS accumulation is mainly controlled by declining atmospheric CO2 concentrations (medium confidence) rather than insolation forcing where perennial ice begins to accumulate at atmospheric CO2 levels below ~280-320 ppm (Tan et al., 2018) and the ice sheet is established by ~2.5 Ma (Bierman et al., 2016). This potentially supports a threshold for long-term GrIS loss below current CO2 levels (~400 ppm) (Section 9.4.1.2). The GrIS may have sustained greater ice loss during Marine Isotope Stage 11 (MIS 11: ~395-424 ka) than the shorter-duration Last Interglaciation (LIG, MIS 5e: ~116-129 ka) despite smaller insolation forcing (Robinson et al., 2017) which suggests (medium confidence) GrIS ice loss is approximately proportional to the magnitude of time-integrated atmospheric warming. However, poor agreement between LIG simulations and proxy-based estimates could be attributed to either increased ice sheet model sensitivity to summer insolation and warming (e.g. Goelzer et al., 2016; Robinson and Goelzer, 2014; Van De Berg et al., 2011; Yau et al., 2016) in fully-coupled surface energy balance forcing in climate-ice models compared to models using albedo-melt feedbacks via positive-degree-day schemes or over-estimated proxy-based temperature reconstructions. Hence GrIS LIG studies provide low confidence constraints on future ice sheet projections. In Antarctica, the LIG climate remained too cold(cool?) for widespread surface melting (Deconto and Pollard, 2016; Goelzer et al., 2016a). Therefore, it is unlikely (low confidence) that large-scale Antarctic ice shelf collapse triggered marine ice cliff instability during the LIG. As a consequence of orbitally-induced higher summer insolation (Dutton et al., 2015b; Goelzer et al., 2016a), LIG warming occurred despite lower-than-present atmospheric CO2 levels. Therefore, the LIG is not a direct analog for future warming, in which elevated CO2 will result in year-round atmospheric warming. However, the LIG may yet be a useful analog for assessing regional responses to warmer climates, particularly in the Arctic and Greenland. [Carlye Peterson, United States of America]	Noted. The revised text is structured differently, so that these different time periods are considered separately and the way that the processes mentioned are assessed has changed.
109875	56	31	56	31	Large volume changes means rapid growth – It would be better to say so explicitly. [Donald Forbes, Canada]	Not applicable because the text was deleted.
90445	56	34	56	36	"~395-424ka" => "424-395 ka", "~116-129 ka" => "129-116 ka". If want to keep "~", also put one on Line 29 ("3-2.5 Ma")? [Holly Kyeore Han, Canada]	Accepted. Text modified.
88643	56	43	56	43	The references Goelzer et al., 2016a and Goelzer et al., 2016b are the same. [Rosemary Vieira, Brazil]	Accepted. Text modified.
90447	56	43			add a label ("a" or "b") on the reference Goelzer et al., 2016 [Holly Kyeore Han, Canada]	Accepted. Text modified.
14841	56	45	56	46	Instead of suggesting that studying LIG is useless, which could be understood as 'no funding anymore for such a research'. I suggest the authors to propose ways to reconcile models and data. Which constraints does the SLR (discussed in section 9.6.2. reconstruction provide? [Marie-France Loutre, Switzerland]	Accepted. Text modified to highlight the shortcomings of the existing research, which may be a reason to justify further work in this area.
29243	56	46			BOX 9.2 Wilson et al. 2018 Nature seem to find evidence of EAIS ice loss during Quaternary interglacials [Francesca Sangiorgi, Netherlands]	Accepted. The suggested reference has been assessed and incorporated into the revised text.
88645	56	47	56	47	Deconto and Pollard, 2016. The correct is one "DeConto". [Rosemary Vieira, Brazil]	Accepted. Text modified.
88647	56	47	56	47	The references Goelzer et al., 2016a and Goelzer et al., 2016b are the same. [Rosemary Vieira, Brazil]	Accepted. Text modified.
30703	56	47	56	47	Goelzer et al., 2016a' should be 'Goelzer et al., 2016' (there is NO '2016b' paper. Same error on a number of places, including p 56, l 52; p 57, l 27; p 60, l 10; …) [Ian Simmonds, Australia]	Accepted. Text modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61713	56	48	56	49	Given the discussion of uncertainties surrounding the marine ice cliff instability mechanism later in the chapter (Box 9.3) relating to not just widespread surface melt requirements but also collapse rates required for this mechanism to operate, I think there should at minimum be a reference to Box 9.3 here. Consider mention of other reasons (localized nature of ice cliff collapse vs. collapse rates necessary to sustain entire ice shelf collapse) that lead to a low confidence in this statement. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text modified. There is now a much fuller consideration of the complexities involved in assigning confidence to this mechanism, and a link to the high-end sea level box is made (See Section 9.2.4.2).
88649	56	52	56	52	The references Goelzer et al., 2016a and Goelzer et al., 2016b are the same. [Rosemary Vieira, Brazil]	Accepted. Text modified.
116871	56		56		I am puzzled by the current box 9.2. There is some overlap with the assessments related to the Pliocene and the related box in chapter 2. There is a need to have a more comprehensive assessment of insights from LIG, across chapters, including model data mismatches for Antarctic warming (see also work by Holloway, Sime and others). I suggest to replace the box by a table, which would refer to the box on Pliocene, and other chapters (including polar amplification in chapter 7), and focus on describing, for each past period, the characteristics of environmental drivers of ice sheet change, the available paleo evidence (reconstructions, model results), and the insights. At the moment, the box is hard to follow and link with the rest of the text. [Valerie Masson-Delmotte, France]	Accepted. This section has now be substantially modified to address each of these concerns. Specifically, the LIG is considered in as much detail as permitted by the available space, and incorporates new text describing the environmental proxy reconstructions, as well as model simulations of ice loss. It is beyond the scope of this section to fully assess the state of LIG climate; rather, the focus has been to highlight the areas of research relevant to improving our understanding of ice sheet responses to LIG forcing. Other paleo periods are now much more fully considered, and in all cases the insights have been presented in terms of response to GWLs, for better connectivity across chapters.
82947	57	3	57	3	I wonder if it should be circa or ca. instead of c. [Sebastian Gerland, Norway]	Accepted. Text modified.
77829	57	4	57	4	hanges -> changes [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified.
61555	57	4	57	4	Should read “changes” not “hanges” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text modified.
32149	57	4			correct "hanges" into "changes" [Anja Wendt, Germany]	Accepted. Text modified.
33459	57	4			Do you mean changes? Check the sentence: “Based on evidence of ice-proximal glaciomarine sedimentation as well as hanges in…”. [Guiomar Rotllant, Spain]	Accepted. Text modified.
109877	57	5	57	5	hanges' should be 'changes' [Donald Forbes, Canada]	Accepted. Text modified.
61557	57	5	57	5	The term “dynamic” in this sentence does not have a clear meaning. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text modified.
73839	57	9	57	11	There is evidence for ice-rafted debris being supplied to the Nordic Seas through the Pliocene (from Greenland), which doesn't align with this statement that marine terminating glaciations "only developed" after 2.7 Ma (e.g. refs: reviewed in Mudelsee and Raymo 2005 Paleoceanography, https://doi.org/10.1029/2005PA001153, 2005 ; plus Bachem et al. 2017 Climate of the Past https://doi.org/10.5194/cp-13-1153-2017). Should this statement be for development of a 'marine terminating ice sheet' rather than glaciations i.e. to make the distinction between tidewater valley glaciers (Pliocene) and the larger scale ice sheet development at the Pliocene/Pleistocene transition? Or instead use the terminology of the Blake-Mizen paper that these Eirik drift sites reflect "Southern Greenland" glaciation rather than the whole ice sheet. (Blake-Mizen 2019 state "Based on elevated IRD deposition at this site from ~3 Ma (Fig. 2c and d), we can infer that at least isolated iceberg-calving glaciers occupied coastal northeastern and eastern Greenland on orbital timescales following the end of the mid-Piacenzian warm period (mPWP, 3.264e3.025 Ma"). [McClymont Erin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised to more clearly explain the mid-Pliocene onset of NH glaciation and late-Pliocene expansion.
109879	57	10	57	11	How could marine-terminating margins of the GRiS develop after 2.7 Ma if the onset of the ice sheet was ~2.5 Ma (p. 56, lines 32-33)? Technically this is 'after', but does depend on there being an ice sheet for marine margins to develop. [Donald Forbes, Canada]	Accepted. Text revised to more clearly explain the mid-Pliocene onset of NH glaciation and late-Pliocene expansion.
65971	57	19	53	19	Suggest deleting the open parentheses after "e.g.,". [Kushla Munro, Australia]	Accepted. Text modified.
2481	57	20			You are talking about the roles MIS31 and MIS5 (LIG) played. I think it would be warranted also to discuss MIS11. During this time GSL might have been up to 20m above current levels (Hearty et al., 1999; Olson et al., 2009, van Hengstum et al., 2009). If that was the case, the East Antarctic Ice Sheet (EAIS) must have significantly contributed as well. However, Raymo and Mitrovica (2012= estimate a GSL of 6-12m above modern values. This GSL rise would only require a minor contributin from EAIS and can be explained with a collapse of Greenland and the WAIS. [Thomas Ronge, Germany]	Accepted. Text modified. Note however that in revising the text to incorporate an assessment of MIS11 (as well as MIS5e and MIS31) we have used the most recent literature sources available. All of those suggested by the reviewer are pre-AR5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6769	57	21	57	22	"Ocean temperatures" are constrained by physical processes, not by data records. "Estimates of ocean temperatures" are what is "unconstrained by paleo-environmental proxy records". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
34493	57	21	57	22	This sentence is unclear. I suggest to replace it by "Because of the lack of LIG paleo-environmental proxy records south of 57ºS (Capron et al., 2014), ocean temperatures during the LIG are effectively unconstrained at high southern latitudes." [Claire Waelbroeck, France]	Accepted. We have now incorporated a fuller assessment of high latitude temperatures and have added additional references.
2483	57	21			I don't agree that there is no information on temperatures South of 57°S. Although still sparse you can find LIG temperature reconstructions in Bianchi and Gersonde (2002) and Esper and Gersonde (2014). [Thomas Ronge, Germany]	Accepted. We have now incorporated a fuller assessment of high latitude temperatures and have added additional references.
83561	57	22	57	22	If you want to add a newer reference, you could cite also the new Southern Ocean compliation by Chadwick, M., Allen, C.S., Sime, L.C., Hillenbrand, C.D., 2020. Analysing the timing of peak warming and minimum winter sea-ice extent in the Southern Ocean during MIS 5e. Quaternary Science Reviews 229, 106134, doi: https://doi.org/10.1016/j.quascirev.2019.106134 [Antje H. L. Voelker, Portugal]	Accepted. We have now incorporated a fuller assessment of high latitude temperatures and have added additional references.
14733	57	23	57	23	"additional" -> "arbitrary" [Jeremy Fyke, Canada]	Noted. Text has been modified and clarified on this point.
88651	57	24	57	24	Deconto and Pollard, 2016. The correct is "DeConto". [Rosemary Vieira, Brazil]	Accepted. Text modified.
90489	57	25	57	26	I may be misunderstanding the intended meaning of the sentence, but Clark et al. (Nature, 2020) show that more than 85% of GIS and AIS mass loss during the Penultimate Glacial Maximum can be explained by oceanic forcing alone, hence leaning towards "more mass loss estimates" rather than "less mass loss estimates"? [Holly Kyeore Han, Canada]	Noted. The original text referred to the different ways in which models were forced, and how these differences led to higher or lower mass loss. The revised text has been clarified to avoid this confusion.
73851	57	25	57	26	It should be noted that main point of Clark et al. (2020) is that Atlantic Meridional Overturning Circulation is a proposed cause of Antarctic warming. [Takashi Obase, Japan]	Not applicable because the text was deleted.
88653	57	26	57	26	The references Goelzer et al., 2017, Goelzer et al., 2018a and Goelzer et al., 2018b are the same. [Rosemary Vieira, Brazil]	Accepted. Text modified.
89807	57	26	57	26	See below, this citation is now published [Peter Croot, Ireland]	Accepted. Text modified.
61715	57	26	57	27	Goelzer et al., 2016a state in their abstract: "Retreat of the Antarctic ice sheet at the onset of the LIG is mainly forced by rising sea level and to a lesser extent by reduced ice shelf viscosity as the surface temperature increases". In box 9.2, page 9-57, line 27, these results are summarized as "It has also been proposed that LIG Antarctic ice loss was primarily driven by sea-level and ice viscosity changes, rather than ocean heat". I recommend adding "to a lesser extent" before "ice viscosity changes" as in the cited reference because the current wording makes the two factors (sea-level and ice viscosity changes) sound equally responsible [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable because the text was deleted.
88655	57	27	57	27	The references Goelzer et al., 2016a and Goelzer et al., 2016b are the same. [Rosemary Vieira, Brazil]	Accepted. Text modified.
61559	57	27	57	27	These references point mostly to sea-sea-level-driven ice loss, with viscosity changes as a secondary driver. The way this sentence is written the two seem like equally important drivers. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable because the text was deleted.
90497	57	28	57	28	including more references would be useful to establish that there is infact a disagreement between studies (currently the cited studies are done by only two different author groups) [Holly Kyeore Han, Canada]	Accepted. Many more (and newer) references have been included, and (dis)agreement between studies more clearly identified.
14735	57	28	57	28	"whilst we have" -> "while there is" [Jeremy Fyke, Canada]	Accepted. Text modified.
90499	57	28	57	30	This whole sentence could sound more convincing if more references and discussion were given about the "disagreement". Also, include ref(s) on atmospheric forcing being a dominating factor? [Holly Kyeore Han, Canada]	Accepted. Many more (and newer) references have been included, and (dis)agreement between studies more clearly identified.
74087	57	29			„at this time" which time is meant here? [Matthias Mengel, Germany]	Noted. It is clear in the SOD text that this paragraph was talking about the LIG. Nonetheless, the text has been revised such that this ambiguity should no longer be a problem.
74091	57	32	57	39	This paragraph is not fully consistent with the statement in the paragraph above, where the authors state „LIG Antarctic ice loss was primarily driven by sea-level and ice viscosity changes, rather than ocean heat." The confidence statements in l28 and l29 as summary of our understanding would be better placed after the paragraph that starts at l32. [Matthias Mengel, Germany]	Accepted. Text modified.
74089	57	33			„in one modelling" missing word? [Matthias Mengel, Germany]	Accepted. Text modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73853	57	37	57	37	I propose to add recent Antarctic ice sheet modeling studies over glacial cycles. Quiquet et al. (2018) The GRISLI ice sheet model (version 2.0): calibration and validation for multi-millennial changes of the Antarctic ice sheet, GMD; Sutter et al. (2019) Modelling the Antarctic Ice Sheet across the mid-Pleistocene transition – implications for Oldest Ice, The Cryosphere; Tigchelaar et al. (2019) : Nonlinear response of the Antarctic Ice Sheet to late Quaternary sea level and climate forcing, The Cryosphere; Albrechts et al. (2019): Glacial-cycle simulations of the Antarctic Ice Sheet with the Parallel Ice Sheet Model (PISM) – Part 1: Boundary conditions and climatic forcing, The Cryosphere. [Takashi Obase, Japan]	Accepted. Text modified to include an assessment of these new references.
115469	57	42	57	42	Sea ice can impact ice shelves, but I don't see how the coincidence of continental ice-sheet expansion and sea ice growth (both presumably occuring in a cooling climate) is proof of cause and effect. It seems to me, the offshore climatic influence of an expanded ice sheet can also influence sea-ice formation, rather than just the other way around. [Robert DeConto, United States of America]	Noted. Text has been modified and clarified on this point.
35789	57	42	57	49	Say a few words about the mechanism for sea-ice-driven reductions in grounded ice loss. [Michael Oppenheimer, United States of America]	Noted. There is at present no clear single mechanism. Our revised text nonetheless more thoroughly assesses the pertinent literature on this subject and mentions mechanisms where possible, but also highlights the uncertainties related to process understanding in this area.
90501	57	43	57	43	"14-15 Ma" => "15-14 Ma" ? Just to be consistent with usages from above. [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
83311	57	46	57	49	ADD 2 REFERENCES: (1) Massom, R.A., A.B. Giles, H.A. Fricker, R.C. Warner, B. Legresy, G. Hyland, N. Young and A.D. Fraser. 2010. Examining the interaction between multi-year landfast sea ice and the Mertz Glacier Tongue, East Antarctica: Another factor in ice sheet stability? Journal of Geophysical Research. 115, C12027, doi:10.1029/2009JC006083. and (2) Massom, R.A., Scambos, T.A., Bennetts, L.G., Reid, P., Squire, V.A. and Stammerjohn, S.E. 2018. Antarctic ice shelf disintegration triggered by sea ice loss and ocean swell. Article in Nature 558, 383-389. [Robert Massom, Australia]	Accepted. Text modified to include an assessment of these new references.
84881	57	46	57	49	A recent and detailed study clearly showing a strong link between slowdown of calving velocities and reduced calving fluxes in the Antarctic Peninsula on the one hand and the precence of ice mélange and sea ice cover on the other hand is Rott et al, 2018. This study should in my opinion be mentioned. The Antarctic Peninsula is a region of particular importance since the collapse of several ice shelves signifcantly changed the boundary conditions of former tributary glaciers (which are now effictively marine terminating), leading to a significant dynamic speed up. Reference: Rott, H., Abdel Jaber, W., Wuite, J., Scheiblauer, S., Floricioiu, D., van Wessem, J. M., Nagler, T., Miranda, N., and van den Broeke, M. R.: Changing pattern of ice flow and mass balance for glaciers discharging into the Larsen A and B embayments, Antarctic Peninsula, 2011 to 2016, The Cryosphere, 12, 1273–1291, https://doi.org/10.5194/tc-12-1273-2018, 2018. [Jan Wuite, Austria]	Accepted. Text modified to include an assessment of these new references.
95949	57	47	57	48	Add Massom et al 2018 (Nature) as a reference for sea ice buttressing [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified to include an assessment of these new references.
83313	57	48	57	49	Change "that sea ice plays a role in controlling the behaviour of floating and grounded ice" to "that sea ice (in the form of both both stationary coastal fast ice and moving pack ice) plays a role in controlling the behaviour of floating and grounded ice and in iceberg calving". [Robert Massom, Australia]	Accepted. Text modified to make this point more clearly.
14739	57	48	57	49	It is not clear that correlation between AIS expansion and perennial sea ice is a strong enough basis for "high confidence that sea ice plays a role in controlling the behaviour of floating and grounded ice". Recommend literature search for model-based sensitivity studies to corraborate this statement. [Jeremy Fyke, Canada]	Noted. Text has been modified and clarified on this point.
9011	57	54	57	55	This is quite a schock to read this. Ice streams control ice discharge but their role on ice evolution is not clear. Not clear to who? How do you generate Melt water pulse 1a? How do you generate 4 m sea level rise per century? Through SMB? This entire paragraph is completely misleading, biased and frankly wrong. [Eric Rignot, United States of America]	Not applicable because the text was deleted.
90503	57	55	57	55	"analyses" => "analysis"? [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
89625	57	55	58	1	The meaning of this sentence is unclear… what is a mature ice stream? A large ice stream? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable because the text was deleted.

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90507	57	55	58	1	I think the wording should be changed to clarify this sentence further: "...large ice sheets … as they grow larger" doesn't sound quite correct in the context of what Robel and Tziperman (2016) suggests. Also, the word "should" is not very necessary in the sentence, and it is not very clear what a "mature" ice stream should mean. Perhaps one could write something like "A modelling work shows that ice streams get fully formed in large ice sheets, making the ice sheets more sensitive to the same amount of environmental forcing compared to small ice sheets with small or no ice streams (Robel & Tziperman, 2016)." ? [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
109255	57	55	58	1	What does "ice streams become more mature" mean? [Maria Zeitz, Germany]	Not applicable because the text was deleted.
74093	58	1			„because ice streams become more mature." What does this mean exactly? [Matthias Mengel, Germany]	Not applicable because the text was deleted.
90505	58	4	58	4	missing comma between "British-Irish Ice Sheet" and "one..." [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
61733	58	4	58	7	I think it might be helpful to add that, although the findings of Bradwell et al. (2019) are consistent with those of Jones et al. (2015), the latter was a study on a smaller glacier whilst the former was on ice streams, the main topic of this paragraph. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text modified such that this issue no longer arises.
73857	58	5			It is not clear what `more resistant bedrock' means. Does is relate to rather small-scale roughness? Or to large-scale topography shape? [Fuyuki SAITO, Japan]	Not applicable because the text was deleted.
129481	58	6			The paper by Jones et al. may isolate an MISI on a small and not very significant mass expulsion. A much better recent study of this follows: Whitehouse, P. L., M. J. Bentley, A. Vieli, S. S. R. Jamieson, A. S. Hein, and D. E. Sugden (2017), Controls on Last Glacial Maximum ice extent in the Weddell Sea embayment, Antarctica, J. Geophys. Res. Earth Surf., 122, 371-397, doi:10.1002/2016JF004121. [Trigg Talley, United States of America]	Noted.
35791	58	7	58	10	Add some words to make clear that this rather sweeping conclusion refers only to ice sheets and ice streams in earlier eras and that modern observations and modeling provide greater insight into the relationship of ice stream activity to stability of individual ice sheet drainages and sectors. Otherwise, this statement could be seen as in conflict with p.60, lines 1-3 for example. [Michael Oppenheimer, United States of America]	Not applicable because the text was deleted.
35917	58	8	59	12	It says that "mass loss of the AIS has been dominated by high ice discharge rates over the West Antarctic Ice Sheet 10 (WAIS) and the Antarctic Peninsula (Mouginot et al., 2014; Sutterley et al., 2014; Wouters et al., 2015), 11 leading to substantial observed surface height changes (McMillan et al., 2014; Bamber et al., 2018b; 12 Schröder et al., 2019; Shepherd et al., 2019), particularly in coastal regions". There are some evidences demonstrating that in somes zones as King George Island (Antarctic Peninsula), the mass loss can be more in ice sheet-ocean interaction than coastal region. [IBETH CELIA ROJAS MACEDO, Peru]	At the spatial scales we refer to here (see Figure 9.18 in the final draft), the region where ice sheets interact with oceans is identical to coastal regions. There is no need to distinguish here between these regions. The revised text provides a clear link to the figure which shows this.
90509	58	9	58	9	"only low confidence" => inserting "only" doesn't seem necessary. [Holly Kyeore Han, Canada]	Accepted. Text has been revised to remove 'low confidence' statements.
111395	58	12	58	36	There is also evidence for icebergs calving directly off a grounded ice cliff in the Bear Island trough during the last deglaciation of the Barents Sea Ice Sheet (Piasecka et al., Mar. Geol. 402, 153–164, 2018). Brendryen et al. (2020) reconstructed the timescale of ice sheet retreat in Bear Island trough and found that the deglaciation of the Bear Island trough was very rapid. [Jo Brendryen, Norway]	Accepted. These references have now been assessed and included.
61731	58	12	58	36	Add a reference to Box 9.3 where the details of MICI are more fully explained (including the evidence for and arguments against this proposed mechanism) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text modified. There is now a much fuller consideration of the complexities involved in assigning confidence to this mechanism, and a link to Box 9.3 is made.
74095	58	12	58	36	This paragraph should be better balanced. The mechanisms of MISI and MICI are not explained, what I would expect from the paragraph. MICI is an established concept, and there is a large body of literature on it, but more than half of the paragraph is on MICI and why we can't trust it. [Matthias Mengel, Germany]	Accepted. Text modified. There are now separate paragraphs for the two mechanisms (MISI and MICI), with individual assessments for each.
106643	58	12	58	36	While the all paragraph is about instability mechanisms is interesting, the paragraph is definitely focused on the MICI mechanism (which is more hypothetical). I think that this paragraph could benefit from further discussions about the MISI mechanism. [Kevin Bulthuis, United States of America]	Accepted. Text modified. There are now separate paragraphs for the two mechanisms (MISI and MICI), with individual assessments for each.

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15837	58	12	58	36	"Instability mechanisms" paragraph does not belong to a paleo-climate section. Both are theoretical and can be applied to a mathematical steady-state configuration, i.e. constant environmental conditions. These mechanims cannot be directly observed on realistic ice sheets that experience(d) changes in atmospheric or oceanic conditions. [Olga Sergienko, United States of America]	Disagree. There is important information to be gained for how ice sheets responded to environmental forcing in the past, and how well models capture those changes. In our revised sections (See Section 9.2.4.2 and 9.6.2) we assess both bodies of literature and draw insights from both as far as is possible, whilst acknowledging that considerable uncertainty remains.
90511	58	14	58	14	positive feedbacks between what? Could write ".... suggests that there is a positive feedback between X and Y"? [Holly Kyeore Han, Canada]	Accepted. Text modified.
106641	58	15	58	15	The MISI and marine ice-cliff instability (MICI): I would add a least a reference for both mechanisms (e.g. Schoof 2007, Marice ice-sheet dynamics. Part 1. The case of rapid sliding or Ice sheet grounding line dynamics: Steady states, stability and hysteresis for MISI and D. Pollard D., R. M. De Conto, and R. B. Alley, 2015, Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure for MICI). [Kevin Bulthuis, United States of America]	Noted. The revised text is no longer a 'process-focused' Box but a Chapter section that assesses paleo literature relevant to ice sheet processes. To that end, the Schoof reference is not especially relevant (MISI is well-described in SROCC and AR5 and elsewhere in Ch. 9) and Pollard et al 2015 is out-dated because the same scheme is published in DeConto & Pollard 2016 (which we cite) with updated parameterisation.
90513	58	15	58	16	include references? [Holly Kyeore Han, Canada]	Accepted. The statement for which extra references are requested is actually addressed in the following sentence, where new references have now been added.
49981	58	17	58	19	Although well-understood, this sentence needs citations to justify it. I suggest e.g. Dutton et al. (2015). [Daniel Gilford, United States of America]	Accepted. New text and additional references address this point more fully now.
90515	58	19	58	19	"only poorly constrained" => inserting "only" doesn't seem necessary [Holly Kyeore Han, Canada]	Accepted. Text modified.
90517	58	19	58	19	define "long-term"? [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
129483	58	23			After Martin et al. (2019), add Larour et al. (2019). Already in the reference list. [Trigg Talley, United States of America]	Accepted. Text modified.
49983	58	26	58	36	It should be added that improvements in LIG sea levels have the potential to inform these instability mechanisms in more detail (Giford et al. 2019) especially with regards to the role of MICI. Gilford et al. (2019) show that while there is a limit to how effective the LIG is for constraining instability processes (and the associated future AIS contributions to GMSL), reductions in LIG uncertainties have potential to constrain and inform instability processes. Using the best available LIG costraints (Dutton et al. 2015) and an improved version of the DeConto and Pollard model, Gilford et al. found that MICI likely played some rolein AIS losses during the LIG, and hence MICI could be more likely in the future. Furthermore, although not currently very informative for 2100 sea levels, LIG observations become more informative on AIS contributions to GMSL over time, especially into the 22nd century. [Daniel Gilford, United States of America]	Accepted. Text modified.
90519	58	27	58	27	include a ref after "MICI is currently incorporated in only one ice sheet model" [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
115471	58	27	58	32	Meltwater feedback isn't necessarily always a possitive feedback. Subsurface ocean warming through stratification can certainly increase basal melt rates, but atmospheric cooling with expanded sea ice can reduce surface melt, possibly slowing/stopping hydrofracturing (producing a strong negative feedback; DeConto et al., in review). [Robert DeConto, United States of America]	Accepted. Text modified.
88657	58	29	58	29	The references Edwards et al., 2019a and Edwards et al., 2019b are the same. [Rosemary Vieira, Brazil]	Accepted. Text modified.
90521	58	29	58	29	The sentence "However, it is not necessarily required (Edwards et al., 2019b)…" could be clarified by writing something like "However, MICI is not necessarily requred to produce an agreement between modelling results and paleo sea-level records" [Holly Kyeore Han, Canada]	Accepted. Text modified.
90523	58	29	58	30	and better agreement 30 might also be achieved with models that do not include MICI [Holly Kyeore Han, Canada]	Accepted. Text modified.
90525	58	29	58	30	"...and better agreement might also be achieved with models that do not include MICI" - what is this based on? Or is it trying to mean that better agreement might be achieved "by" imposing ocean greater ocean warming or meltwater-ocean feedbacks? [Holly Kyeore Han, Canada]	Accepted. Text modified.

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89273	58	30	58	36	Dolan et al., (2018, Nat Comms, 9, 2799) showed that a wide range of ice sheet and climate models do not produce a good fit with recent MPWP sea level reconstructions. Suggest changing sentence to highlight that ice sheet models that do not include MICI also need additional processes to produce a better match to sea level data, such as the subgrid grounding line melt scheme of Golledge et al., (2017, Climate of the Past). The statement about robust sea level estimates should be updated to reflect recent publications by Dumitru et al., (2019, Nature) and Grant et al., (2019, Nature). [Edward Gasson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable because the text was deleted.
90527	58	32	58	32	(Raymo et al., 2018) => Raymo et al. (2018) ? [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
90529	58	32	58	33	write that uncertainties in sea level proxies and dynamic topography are larger than X (e.g. uncertainties in model results?) , rather than just saying that they are "so large". [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
90531	58	32	58	35	The sentence could be shorten and clarified by writing something like, "Furthermore, Raymo et al. (2018) suggests that uncertainties in Pliocene ice volume and sea level proxies are larger than uncertainties in modelled-Pliocene sea level, preventing a robust estimate of AIS contribution to Pliocene sea level through modelling studies."? [Holly Kyeore Han, Canada]	Not applicable because the text was deleted.
32433	58	32			() for Raymo citation wrong. [Olaf Eisen, Germany]	Not applicable because the text was deleted.
33461	58	32			Change: “Furthermore, (Raymo et al., 2018) consider…” by “Furthermore, Raymo et al. (2018) consider…”. [Guiomar Rotllant, Spain]	Not applicable because the text was deleted.
49985	58	35	58	35	"in the existence of" is too strong, especially as there remains uncertainty in paleo sea level changes and MICI modeling. Whether MICI contributing during past warm climates *strongly* depends on the modeling techniques and paleoclimate observations and storylines/interpretations considered (Kopp et al. 2017, DeConto et al. 2016, Gilford et al. 2019, Edwards et al. 2019). Instead, "in whether or not" would be more scientificaly rigorous and appropriate, and should be used in place of "in the existence of". [Daniel Gilford, United States of America]	Not applicable because the text was deleted.
109881	58	35	58	36	But it cannot be excluded. [Donald Forbes, Canada]	Accepted. Text now modified to more clearly identify the reasons for uncertainty and the fact that this uncertainty prevents firm statements being made.
109883	58	39	58	39	Be explicit that this paragraph refers to marine margins. This doesn't appear until the end (line 43). [Donald Forbes, Canada]	Not applicable because the text was deleted.
110733	58	39	58	44	Related to solid Earth is the regional sea-level, which can have strong effects on ice sheet stability and glacial cycle dynamics. https://doi.org/10.5194/tc-14-599-2020 or https://doi.org/10.5194/tc-13-2615-2019 [Torsten Albrecht, Germany]	Accepted, new references added.
88659	58	41	58	41	Gomez et al., 2018. Reference not found. [Rosemary Vieira, Brazil]	Accepted. Reference added.
115473	58	41	58	44	Consider adding the caveat that uplift/sea level feedback doesn't seem to be very impactful on short (decadal to a few centuries) timescales, as shown in the Pollard and Larour studies cited here. [Robert DeConto, United States of America]	Accepted, text modified to include mention of relevant timescale.
61741	58	42	58	44	We were instructed to mention seminal papers that we think may have been left out. On the topic of initiating a readvance following a halt in ice sheet retreat, I think the observations of Bradley et al. (2015) could be referenced here. Reference: Bradley, S.L., Hindmarsh, R.C.A., Whitehouse, P.L., Bentley, M.J., King, M.A., 2015. Low post glacial rebound rates in the Weddell Sea due to Late Holocene ice-sheet readvance. Earth and Planet. Sci. Lett. 413, 79-89. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, new reference added.
46509	58	42	58	44	Another relevant reference w.r.t. solid-earth feedbacks mitigating the MISI in the ASE of the WAIS is Kachuk et al. (in press) (Kachuck, S.B., D.F. Martin, J.N. Bassis, S.F. Price. Rapid viscoelastic deformation slows marine ice sheet instability at Pine Island Glacier. Geophys. Res. Lett., submitted 2019). [Stephen Price, United States of America]	Accepted, new reference added.
61483	58	43	58	44	recommend one paper: Adhikari, Surendra, et al. "Future Antarctic bed topography and its implications for ice sheet dynamics." Solid Earth 5.1 (2014): 569-584. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Suggested reference is much older than SROCC and so newer citations have been used instead.
11161	58	43	58	44	recommend one paper: Adhikari, Surendra, et al. "Future Antarctic bed topography and its implications for ice sheet dynamics." Solid Earth 5.1 (2014): 569-584. [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Suggested reference is much older than SROCC and so newer citations have been used instead.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71835	58	49	61	47	The paragraph about new observations in Antarctica is lacking a crucial component : bathymetry and bed topography in Antarctica. Without knowledge of these, every effort in trying to predict ice-ocean interaction or predicting the future of the Antarctic ice-sheet is limited. One would say that efforts have been made to fill this gap with new bed topography recontruction using mass conservation (Morlighem et al. 2019, Nias et al. 2018), new gravimetric inversion (Millan et al. 2017, Greenbaum et al., 2015, Eagles et al. 2018) or bathymetric measurement (Arndt et al. 2013). Nevertheless large regions remains poorly constrained limiting our ability to model the ocean forcing from the ocean on the ice-sheet (especially under ice shelves) (Millan et al. 2020). REF: Morlighem, M., Rignot, E., Binder, T. et al. Deep glacial troughs and stabilizing ridges unveiled beneath the margins of the Antarctic ice sheet. Nat. Geosci. 13, 132–137 (2020). https://doi.org/10.1038/s41561-019-0510-8 Nias, I. J., Cornford, S. L. & Payne, A. J. New mass-conserving bedrock topography for Pine Island Glacier impacts simulated decadal rates of mass loss. Geophys. Res. Lett. 45, 3173–3181 (2018). Millan, R., Rignot, E., Bernier, V., Morlighem, M. & Dutrieux, P. Bathymetry of the Amundsen Sea Embayment sector of West Antarctica from operation icebridge gravity and other data. Geophys. Res. Lett. 44, 1360–1368 (2017). Eagles, G. et al. Erosion at extended continental margins: insights from new aerogeophysical data in eastern Dronning Maud Land. Gondwana Res. 63, 105–116 (2018). Greenbaum, J., Blankenship, D., Young, D. et al. Ocean access to a cavity beneath Totten Glacier in East Antarctica. Nature Geosci 8, 294–298 (2015). https://doi.org/10.1038/ngeo2388 Millan, R., St-Laurent, P., Rignot, E., Morlighem, M., Mouginot, J., & Scheuchl, B.. (2020). Constraining an ocean model under Getz Ice Shelf, Antarctica, using a gravity-derived bathymetry. Geophysical Research Letters, 47, e2019GL086522. https://doi.org/10.1029/2019GL086522 Arndt, J. E., Schenke, H. W., Jakobsson, M., Nitsche, F. O., Buys, G., Goleby, B., … Wigley, R. (2013). The International Bathymetric Chart of the Southern Ocean (IBCSO) Version 1.0-A new bathymetric compilation covering circum-Antarctic waters. Geophysical Research Letters, 40(12), 3111–3117.	Taken into account. The improved bedrock topography and its positive impact on the simulated recent ice sheet evolution are now mentioned in section 9.4.2.2
77831	58	49	67	40	I feel that 9.4.2 is unnecessarily long. Although it's important, it's probably too detailed, and there is some repetition. For instance (as you have pointed out with a reference) both 9.4.2.1 and 9.4.2.2 discuss whether MISI is underway and which sectors are vulnerable. With some rearrangement, I suspect that 9.4.2.2 could be condensed considerably. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We did our best to shorten this section, and to keep it in particular within the allocated word limit. We also aimed at gaining sharpness by clarifying the SROCC starting point for each specific issue.
99703	58	51	61	47	This section is very disjointed and poorly organized. [Peter Clark, United States of America]	Noted. We tried to make this section clearer by reordering along an axis going from large to small scale and by types of processes.
40755	58	51			section 9.4.2.1: this section doesn't really highlight the improvements since the previous reports. [TSU WGI, France]	Taken into account. SROCC starting points were given were appropriate.
88261	58	51			Section 9.4.2.1. - Reference could be made to chapter 2 for observed trends [Sharon Smith, Canada]	Accepted. We signpost Chapter 2 at the very beginning of this section.
15839	58	53	58	53	Add "part" or "portion" after "grounded" [Olga Sergienko, United States of America]	Noted. The IMBIE ice loss numbers concern the entire ice sheet, not only the grounded parts. Sea-level contributions of course only take into account ice above flotation.
104423	58	53	58	55	MB. Statement in Ch9 "For the period 1992 to 2016-2017, the average grounded Antarctic Ice Sheet (AIS) mass loss was likely about 100 ± 50 Gt yr-1, amounting to about 2500 ± 500 Gt over this period (see Figure 9.18) (Bamber et al., 2018b; The IMBIE Team, 2018), equivalent to 6.9 ± 1.4 mm sea level rise.". In Atlas.5.9.1.1 (page 97, lines #36-38) there is a slightly different value of 2,720 ± 1,390 billion tonnes of ice between 1992 and 2017, which is consistent with the IMBIE Team, 2018 [citing IMBIE2018 abstract: "Here we combine satellite observations of its changing volume, flow and gravitational attraction with modelling of its surface mass balance to show that it lost 2,720 ± 1,390 billion tonnes of ice between 1992 and 2017, which corresponds to an increase in mean sea level of 7.6 ± 3.9 millimetres (errors are one standard deviation)]. [Irina Gorodetskaya, Portugal]	Taken into account. We now give the IMBIE numbers, to be consistent across the report

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106645	58	53	61	47	The authors might consider adding the following reference (also of interest of Greenland and glaciers): E. Hanna et al. (2020), Mass balance of the ice sheets and glaciers - Progress since AR5 and challenges, Earth-Science Reviews. [Kevin Bulthuis, United States of America]	Accepted. This paper is now cited in this section.
81555	58	54	58	54	This number (2500 plusmin 500) is a too rounded number that does not correspond with recents estimates from Mottram 2020 or the IMBIE2 report. It seems to be directly pulled from a figure and does not match the citations (Bamber, IMBIE) afterwards. [Melchior van Wessem, Netherlands]	Taken into account. Cited IMBIE2 directly. Mottram et al. in review is SMB only.
84883	58	54	58	54	The number 100 ± 50 Gt yr -1 for Antarctic mass loss between 1992-2016/17 is provided, which probably comes from the IMBIE paper for Antarctica. However, the the paper mentions 109 ± 56 Gt yr -1 (see table 1 in the IMBIE paper for Antarctica) [Jan Wuite, Austria]	Accepted. SOD text gave an appropriately rounded average of IMBIE and Bamber, as stated in that text. We only refer to IMBIE in the FGD.
69641	58	55	58	55	where does the 6.9+/- 1.4 mm come from, and why is this promoted to the Summary Statements? I can't find these numbers in either Rignot et al 2019 or IMBIE 2018. Also differs from IMBIE numbers in Atlas, p97 l37, "7.6 ± 3.9 mm between 1992 and 2017". [Nicholas Golledge, New Zealand]	Accepted. Now corrected and updated.
71815	58	55	58	55	I recommend to add Rignot et al. 2019 along with Bamber et al. 2019 and The IMBIE team, 2019. [Jeremie Mouginot, France]	Accepted. Cited now along with SROCC, IMBIE, Bamber, Smith and Velicogna. However, we note that the Rignot study is actually already in the SROCC (as is IMBIE).
62301	59	2	59	2	Because a reference is provided for the input-output method, similar references should be provided for satellite altimetry and gravity anomaly methods. Alternatively, the reference for the input-output method may be removed. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Recent refs to altimetry and gravimetry studies are now given at the same place.
71819	59	2	59	2	I recommend to add Velicogna et al. 2004, 2020 as reference for gravity anomalies (GRACE). [Jeremie Mouginot, France]	Accepted. Velicogna 2020 is cited now.
39109	59	2	59	2	Add reference Smith et al for satellite altimetry. Add reference Velicogna et al for GRACE. Smith, Ben, et al. "Pervasive ice sheet mass loss reflects competing ocean and atmosphere processes." Science (2020)., Velicogna, Isabella, et al. "Continuity of Ice Sheet Mass Loss in Greenland and Antarctica From the GRACE and GRACE Follow-On Missions." Geophysical Research Letters 47.8 (2020): e2020GL087291. [Ola Kalen, Sweden]	Accepted. See replies to #71819, 62301.
71817	59	5	59	5	add Velicogna et al. 2020 along with Rignot et al. 2019. Ref : Velicogna, I., Mohajerani, Y., A, G., Landerer, F., Mouginot, J., Noel, B., et al. (2020). Continuity of ice sheet mass loss in Greenland and Antarctica from the GRACE and GRACE Follow-On missions. Geophysical Research Letters, 47, e2020GL087291. https://doi.org/10.1029/2020GL087291 as reference for GRACE. [Jeremie Mouginot, France]	Accepted. See replies to #71819, 62301.
67287	59	8	59	8	The same statement is made in SROCC with very high confidence (see SPM A1.1) [Regine Hock, United States of America]	Taken into account. The SROCC statement is mentioned and confirmed in the rewritten paragraph.
14741	59	8	59	12	Suggest updating references associated with AIS mass changes to include more recent publications regarding Antarctic ice discharge rates - I'm assuming more recent publications exist? [Jeremy Fyke, Canada]	Taken into account. We now very systematically start from SROCC and cite known incremental changes since then (e.g. Gudmundsson et al., 2019)
35919	59	8	59	46	From line 8-12, it affirms that there is more mass loss in coastal areas; however, from line 42-48 It seems to support the idea that there more mass loss for basal melting. [IBETH CELIA ROJAS MACEDO, Peru]	Taken into account. The section has been rewritten and this issue has been clarified.
103833	59	9	59	9	mass loss -> the mass loss [Philippe Tulkens, Belgium]	Editorial, taken into account in the rewritten text
80869	59	9	59	9	mass loss -> the mass loss [Louise Sandberg Sørensen, Denmark]	Editorial, taken into account in the rewritten text
110711	59	11	59	12	Please add letters to each panel in Fig. 9.20 and reference accordingly. [Torsten Albrecht, Germany]	Noted, each figure panel label is now consistent with other figures in the chapter, and references have been updated
65975	59	13	55	13	Suggest move the parenthesis: 'Aschwanden et al. (2019)…' [Kushla Munro, Australia]	Taken into account. Starting page should have been 55 (Greenland observations section 9.4.1.1)
71821	59	18	59	18	I would add Velicogna et al. 2020 as it is the most recent syntheses of mass loss from GRACE in Antarctica and Greenland. [Jeremie Mouginot, France]	Accepted. This paper is cited now.
49987	59	18	59	19	Please cite Hamlington et al. (2020) in this set of syntheses. [Daniel Gilford, United States of America]	Rejected. Although Hamlington et al. does summarize ice sheet mass loss studies, it is a sea-level literature review and, in the domain of interest here, does not add substantial value to the primary literature and to the SROCC we cite here.
72015	59	18		31	need to add recent publication by Smith et al. Science 2020 [John Church, Australia]	Accepted. This paper is cited now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67289	59	19	59	19	A statement with the same content (just framed in terms of sea level is made in SROCC with very high confidence (see SPM A3) [Regine Hock, United States of America]	Noted. The SROCC statement conflates Greenland and Antarctica. Here we focus on Antarctica. We repeat the SROCC assessment of Antarctic mass losses over different periods until 2016 as a starting point. But we also have this: "However, recent work suggests that the mass loss acceleration has paused since 2016 because of regional mass gains in Queen Maud Land (Velicogna et al., 2020)". We state with very high confidence that there is an acceleration of mass loss in the WAIS and the Peninsula since 2000.
103837	59	25	59	25	delete 'about'; the uncertainty is already defined by the provided error bar. [Philippe Tulkens, Belgium]	Not applicable. The text was almost entirely rewritten.
80871	59	25	59	25	delete 'about'; the uncertainty is already defined by the provided error bar. [Louise Sandberg Sørensen, Denmark]	Not applicable. The text was almost entirely rewritten.
67223	59	28	59	28	Remove "Because of …evidence'. That is implicit in the 'high confidence' (and the many cited studies) [Regine Hock, United States of America]	Noted. The text was almost entirely rewritten.
67291	59	28	59	28	Remove the first part 'Because of … evidence', … This is implicit in 'high confidence' [Regine Hock, United States of America]	Noted. The text was almost entirely rewritten (see comment #67223, identical)
38105	59	28	59	31	It described as "There is high confidence in mass loss of the Totten Glacier of the EAIS (Miles et al., 2013; Li et al., 2016d; Mohajerani et al., 2018; Rignot et al., 2019; Schröder et al., 2019; Shepherd et al., 2019) since about 2000, linked to changes in coastal ice dynamics (Li et al., 2016d)." However, the changes in EA in Figure 9.18 is quite different from that of Rignot et al. (2019), which showed the decreasing mass change curve since 1997. [Junhee Lee, Republic of Korea]	Noted. The comment is correct concerning the entire East Antarctic. However, our text referred to the Totten Glacier Basin only. In the revised version, we refer to the SROCC assessment (high confidence that there is grounding line retreat, indicating dynamic thinning, on this glacier) and express high confidence in mass loss of the Totten Glacier of the EAIS since about 2000 based on a multitude of consistent studies (Miles et al., 2013; Li et al., 2016; Mohajerani et al., 2018; Rignot et al., 2019; Schröder et al., 2019; Shepherd et al., 2019)
159	59	30	59	30	"linked to changes in coastal ice dynamics" - vague. What does this mean? [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We write "dominated by ice dynamics", which is clearer, and identical to the original literature (Li et al., 2016)
67293	59	33	59	33	robust evidence can lead to high and very high confidence, so the 'therefore' is not correct. Confidence is product of evidence and agreement. [Regine Hock, United States of America]	Noted. The rewritten text does not contain such a formulation.
101911	59	33	59	34	"There is robust evidence and therefore high confidence that mass loss of the AIS is primarily due to reductions in the thickness and extent of floating ice shelves (Pritchard et al., 2012; Paolo et al., 2015)" - here a recent paper by Wåhlin et al 2020 (Nature 578, pages 568–571) should be added which clearly demonstrates based on both observational and lab-simulated evidence that ice front blocks a large portion of oceanic heat (its barotropic component) coming to an Antarctic shelf so that only the smaller baroclinic component of the heat flow can enter the sub-ice cavity. [IAPSO ECS group review, United States of America]	Accepted. The revised text assesses this paper.
69725	59	33	59	34	An important reference for this statement would be: Gudmundsson, G.H., Paolo, F.S., Adusumilli, S., Fricker, H.A., 2019. Instantaneous Antarctic ice sheet mass loss driven by thinning ice shelves Geophysical Research Letters. Geoph 46, 13903–13909. doi:10.1029/2019GL085027 [Matthew Hoffman, United States of America]	Accepted. The revised text assesses this paper.
83317	59	33	59	34	This fails to address the additional key factor of ice shelf disintegration e.g., Larsen A and B on the Antrarctic Peninsula. Please add this here, with references from Scambos and others. [Robert Massom, Australia]	Taken into account. The revised text states that there is very high confidence that the observed AIS mass loss since the early 1990s is primarily linked to ice shelf changes, and we mention ice shelf disintegration as a cause of loss of buttressing that leads to flow acceleration.
95953	59	33	59	35	The link between ice shelf thinning and ice sheet mass loss is not clear outside the Amundsen Sea. So I would highlight that this link is observed only in the Amunden Sea Embayment. [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We highlight the Amundsen Sea sector (not only the embayment) in the revised text, based on Gudmundsson et al 2019 and some older literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
161	59	33	59	40	Can we say that amost of the mass loss is due only to changes in thickness and extent of floating ice shelves? What about grounding line recession and changes in mass flux? We need to clarify the link here. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text was rewritten and clarifies this point (very high confidence that the observed AIS mass loss since the early 1990s is primarily linked to ice shelf changes based on available evidence).
72129	59	33	60	29	There are descriptions of the physical processes about ice-shelf mass loss with buttressing effect, grounding-line retreat, basal melting, Circumpolar Deep Water (CDW) and Antarctic sea-ice change. I understand that these still have high uncertainties, but it might better to understand these processes with an illustration figure. [Jun-Young Park, Republic of Korea]	Noted. Space constraints unfortunately do not allow us to include such a figure.
101913	59	34	59	34	"… reductions in the thickness and extent of floating ice shelves (Pritchard et al., 2012; Paolo et al., 2015)..." - consider adding something like 'due to ocean-ice shelf interactions' or 'driven by increased basal melting' behind floating ice shelves. [IAPSO ECS group review, United States of America]	Not applicable. The text was almost entirely rewritten.
62303	59	34	59	35	The clause that begins with "while" is a bit difficult to interpret. It would make the sentence more clear if "while there is no trend" is changed to "with no trend". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The text was almost entirely rewritten.
65973	59	35	53	35	Suggest deleting the open parentheses after "described in". [Kushla Munro, Australia]	Editorial. Taken into account. Wrong page number (59 instead of 53)
14747	59	35	59	35	Medley and Thomas (2019) find significant increasing AIS SMB. https://doi.org/10.1038/s41558-018-0356-x Comment on this finding, relative to IMBIE et al (2018) and why IMBIE (2018) prevails over Medley and Thomas finding. [Jeremy Fyke, Canada]	Noted. Medley and Thomas show increasing AIS SMB over the 20th century, but their figure 2a actually shows a negative trend (insignificant) over the satellite period, in agreement with IMBIE. Clarified in the revised text.
10063	59	36	59	36	The thinning reduces the buttressing of the ice shelf, not the buttressing of glaciers that feed the ice shelves [Tong Zhang, United States of America]	Noted. The revised text contains no such wording.
73861	59	37	59	40	The text after `unless the ice-shelf mass loss concerns…’ is not easy to understand at a first glance.The original definition in Furst et al.(2016) may be better. [Fuyuki SAITO, Japan]	Taken into account. We reformulated this closer to the original definition by Fürst et al.
10065	59	39	59	39	The Reese paper is not a good citation here. In the Reese paper, they discussed the sensicitivity of grounding line flux to basal melting at different locations on the ice shelf. It is not relevant here. [Tong Zhang, United States of America]	Accepted. Reference deleted.
62305	59	40	59	40	I suggest adding the following clause to the end of the last sentence of the paragraph: "…, and these sectors are where the largest mass losses have been observed (Shepherd et al., 2012)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This is true but not necessary, as it is stated before.
83319	59	42	59	46	Again, please add that rapid ice shelf disintegration events have been prominent on the Antarctic Peninsula, and have had a major and immediate effect pn the discharge rates of outlet glaciers associated with them (Scambos et al., 2003). [Robert Massom, Australia]	Accepted. This is now clearly written in a dedicated new paragraph.
112967	59	42	59	55	A good cite here would be Adusumilli et al., 2020, Nature geoscience, "Interannual variations in meltwater input to the Southern Ocean from Antarctic ice shelves", submitted in October 2018, published in June 2020 [Susheel Adusumilli, United States of America]	Taken into account. The paper is now assessed in this section. Thank you.
71823	59	45	59	46	I do not understand this sentence. If they are short periods of surface melt on Antarctica ice shelves. The total SMB is actually positive. = Ice shelves are overall gaining from surface processes. The total amount of ice coming from the continent is about 1700 Gt/yr, the total amount of ice coming from SMB is about 430 Gt. The amount of ice is then lost through calving 45% and basal melting 55%. Overall melt water is almost entirely coming from basal melting in the oceans (ice shelves + icebergs) (Rignot et al. 2013. Therefore, I do not think that it is "likely" but "virtually certain" that basal melting dominates. [Jeremie Mouginot, France]	Taken into account. The formulation was not clear. Paragraph rewritten (likely that basal melting and iceberg calving dominate Antarctic ice shelf mass loss over surface melting").
95955	59	45	59	46	Iceberg calving and basal melt contributions are roughly the same. So I would not say that basal melting dominates. It presumably dominates in warm regions such as the Amundsen Sea. [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The formulation was not clear. Paragraph rewritten (likely that basal melting and iceberg calving dominate Antarctic ice shelf mass loss over surface melting"). See comments 71823 and 107303
107303	59	45			Needs to be clarified does basal mass loss also dominate mass loss from iceberg calving? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is not clear overall. What is clear (likely) is that basal melting and iceberg calving dominate Antarctic ice shelf mass loss over surface melting, which is what we write now.
71825	59	46	59	46	I would add Depoorter et al. 2013 and Rignot et al. 2013 along with Liu et al. 2015 as they also estimates the amount of melting and its partionning on ice shelves. [Jeremie Mouginot, France]	Noted. These are pre-SROCC references, which we try to note use to much if they are taken into account in the SROCC, which is the case. We refer to the SROCC in this paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67295	59	46	59	46	Beter: 'There is medium confidenct that .... (if there is confidence there is also evidence; it's implicit) [Regine Hock, United States of America]	Accepted. No such formulation in the rewritten section.
62309	59	47	59	47	Change "basal melting" to "basal melt rates" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - "basal melt rates" used now systematically where appropriate.
61735	59	47	59	48	The statement "with calving rates often as high as basal melting in thinning ice shelves", does this mean the mass loss associated with calving rates and basal melting in thinning ice shelves are often of the same magnitude? If so, I think "mass loss associated with calving rates is often...' could make it a little clearer [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The section was rewritten. There is no comparison between calving rates and basal melting in the revised version.
62307	59	48	59	48	Do stable (not thinning) ice shelves have calving rates that are lower than basal melt rates? If so, I suggest adding this to this clause in the sentence: "with calving rates often as high as basal melting in thinning ice shelves, whereas stable ice shelves have calving rates that are lower than basal melt rates." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The section was rewritten. There is no comparison between calving rates and basal melting in the revised version.
14743	59	49	61	47	It appears that Bamber et al. (2018), IMBIE Team (2018), Rignot et al. (2019) are very highly cited w.r.t. Antarctic mass chance section. Is this appropriately weighted to provide a full review of this field since AR5? [Jeremy Fyke, Canada]	Noted. We are supposed to take into account SROCC, then start from SROCC and concentrate on the most recent references since the SROCC , not to provide a "full review" (or assessment) since AR5. We now also cite Smith 2020 and Velicogna 2020, reducing the relative weight of the former references.
14745	59	49	61	47	Similarly, Mouginot et al., 2014; Sutterly et al., 2014, Wouters et al., 2015, repeatedly cited in context of changing ice dynamics. Is this an appropriate coverage of this highly active field since AR5 in the AR5-AR6 intervening years? [Jeremy Fyke, Canada]	Noted. We are supposed to take into account SROCC, then start from SROCC and concentrate on the most recent references since the SROCC , not to provide a "full review" (or assessment) since AR5. The revised text does not cite these pre-SROCC references in this section.
129485	59	50	59	50	The Rintoul et al. 2016 reference is an excellent paper about Totten Glacier, but it makes no mention of upwelling anywhere. The intended reference here should be Greene et al., 2017, which is explicitly about upwelling at Totten. Citation: Greene, C. A., Blankenship, D. D., Gwyther, D. E., Silvano, A., and van Wijk, E. (2017). Wind causes Totten Ice Shelf melt and acceleration. Science Advances, 3(11), e1701681. [Trigg Talley, United States of America]	Noted. Not applicable. This sentence, and the long reference list that went along with it, is now replaced by a single reference to the relevant SROCC assessment.
163	59	53	59	53	Here the link is made between upwelling CDW and ocean thermal forcing of glaciers and ice shelves. However, this isn't really explained earlier. Why is CDW upwelling increasing - needs links to the SWW and ACC discussed earlier in the report. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Not applicable. This sentence, and the long reference list that went along with it, is now replaced by a single reference to the relevant SROCC assessment.
165	59	53	59	53	"reaches the grounding line of glaciers" - in the previous paragraph grounding lines weren't mentioned and the emphasis was on ice shelves. However, grounding lines are mentioned here. I think the ocean forcing of grounding lines and ice shelves needs attention and clarification here. Is all the mass loss only due to ice shelf thinning or is there also melting at the grounding line? [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Not applicable. This sentence, and the long reference list that went along with it, is now replaced by a single reference to the relevant SROCC assessment. The restructured section should be clearer to the reader on this distinction.
107305	59	53			Slightly confusing - CDW does not ned to reach GL in order to be effective in melt ice shelves [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Not applicable. This sentence, and the long reference list that went along with it, is now replaced by a single reference to the relevant SROCC assessment.
101915	59	54	60	1	"However, in situ measurements within the ice-ocean boundary layer are extremely sparse, and modelling ice-shelf basal melt remains challenging because of insufficient process understanding and paucity of in-situ obseravtions"- See comment regarding p 59, lines 33-34. Again it is essential that the most recent study by Wåhlin et al 2020 (see below) is added here as it presents evidence based on unique direct observations of the Getz Ice Shelf system and laboratory experiments on a rotating platform. Full reference: Wåhlin, A. K., Steiger, N., Darelius, E., Assmann, K. M., Glessmer, M. S., Ha, H. K., ... & Mazur, A. K. (2020). Ice front blocking of ocean heat transport to an Antarctic ice shelf. Nature, 578(7796), 568-571. [IAPSO ECS group review, United States of America]	Accepted. The paper by Wåhlin et al. Is cited now.
46511	59	54	60	1	The paper of Dinniman et al. (2016) could be referenced here (Dinniman, M., Asay-Davis, X., Galton-Fenzi, B., Holland, P., Jenkins, A., & Timmermann, R. (2016). Modeling Ice Shelf/Ocean Interaction in Antarctica: A Review. Oceanography, 29(4), 144–153. https://doi.org/10.5670/oceanog.2016.106). [Stephen Price, United States of America]	Accepted. Although it is one more pre-SROCC reference.

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1759	59	54	60	1	Suggest adding recommendations at the end of line 1 on page 60 of how to increase resolution of the processes and factors responsible for Antarctic ice-shelf mass losses. [Michael Kennish, United States of America]	Rejected, because of space constraints, and because we are supposed to provide an assessment of current research. Such recommendations are not the main purpose of the report.
101917	60	1	60	1	"... and paucity of in-situ observations" - Also necessary high spatial and vertical resolution poses a big challenge for larger-scale, realistic models. However, the exponential increase in computing power allows for more and more high-resolution model simulations already. [IAPSO ECS group review, United States of America]	Accepted. Added "required spatial resolution".
69727	60	1	60	1	Additional reference: Asay-Davis, X.S., Jourdain, N.C., Nakayama, Y., 2017. Developments in Simulating and Parameterizing Interactions Between the Southern Ocean and the Antarctic Ice Sheet. Curr. Clim. Chang. Reports 3, 316–329. doi:10.1007/s40641-017-0071-0 [Matthew Hoffman, United States of America]	Accepted. Now citing Asay-Davis et al 2017 along with Turner et al. 2017 and Dinniman et al 2016.
69729	60	1	60	1	Additional reference: Dinniman, M.S., Asay-Davis, X.S., Galton-Fenzi, B.K., Holland, P.R., Jenkins, A., Timmermann, R., 2016. Modeling ice shelf/ocean interaction in Antarctica: A review. Oceanography 29, 144–153. doi:https://doi.org/10.5670/oceanog.2016.106 [Matthew Hoffman, United States of America]	Accepted. Now citing Dinniman et al 2016 along with Turner et al. 2017 and Asay-Davis et al., 2017
40171	60	6			Fig 9.20: it is confusing to have the observed elevation change only for the grounded ice but the projected value covering also the ice shelves [TSU WGI, France]	Accepted. A method to remove the ice shelves was implemented
41463	60	6			As it stands, the main message of the current figure is that there is no scenario dependence of the future Antarctic SLR response, which would be a big departure from the discussion in the ice sheet modelling community that ice mass loss non-linerarities would be dependent on future warming. In fact, all but the ISMIP6 results presented in table 9.2 point to a scenario dependence. From the underlying study by Seroussi et al which also informs the emulator by Edwards et al it becomes clear that one of the key drivers of the apparent scenario independence is the wildly divergent CMIP5 forcing input, not necessarily the ice sheet modelling response to warming. To me, it seems that there is a real danger here that CMIP5 model uncertainty (the regional southern ocean temperature issues in CMIP5, for instance, were quite apparent in AR5 already, see e.g. WGI AR5 Figure 9.15) could mask scenario dependent ice sheet model responses with the consequence of sending the wrong message, in particular to policy makers. Given the CMIP5 forcing dependence, it would be particularlly important to compare RCP2.6 and RCP8.5 ice sheet reponses that are forced by the same CMIP5 models. 2 vs 6 CMIP5 inputs fro RCP2.6 and RCP8.5, respectively, could cause a great distortion of results if i am not mistaken. This comment should also be considered when revising SOD assessment presented in section 9.4.2.2. [Alexander Nauels, Germany]	Taken into account. We have extended the discussion to explain more clearly that the scenario dependence of net mass loss is unknown (rather than non-existent) when using larger ensembles of both climate and ice sheet models as in ISMIP6, and some of the factors that contributed to strong scenario dependence in the SROCC and previous studies that remain uncertain. We have also shown more clearly the dependence of the AIS response on assumptions about basal melt sensitivity. The final assessment for AIS includes not only the ISMIP6 emulator but also LARMIP-2, which assumes higher basal melt sensitivity values than ISMIP6 and therefore has stronger scenario dependence.
88661	60	10	60	10	The references Goelzer et al., 2016a and Goelzer et al., 2016b are the same. [Rosemary Vieira, Brazil]	Noted. Thank you.
65977	60	15	55	15	Suggest move the parenthesis: 'Furst et al. (2015)....' [Kushla Munro, Australia]	Taken into account. The references on these lines were not Furst et al, but Edwards et al and Nowicki et al., and the indicated page range was ambiguous, but we think we got the point and excluded the author names from the parentheses.
32151	60	17			Check whether the elevation change map 1978-2017 refers to (Schroeder et al., 2019) or (Schröder et al., 2019). Both handle data from the 1970s, but neither has the coverage shown in the map. Furthermore, (Schroeder et al., 2019) presents the historic data but not a comparison to recent observations. [Anja Wendt, Germany]	Accepted. There was a mistaken interpretation of which datasets were used. Now corrected to Schröder et al., 2019.
61717	60	23	60	26	Recommend adding a few words/short phrase to explain why reduced sea ice formation results in reduced vertical mixing & increased ocean stratification. Looking at the cited paper (Miles et al., 2016), I think it is because reduced sea ice formation reduces vertical mixing due to reduced brine channel drainage. Regardless, consider adding the mechanism linking these factors to the sentence. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We added a half sentence stating that this is either due to changes of ocean stratification or through mechanical coupling (both are hypothesized in the cited refs)
9039	60	23	60	26	First, "reduced vertical mixing" is no clear as this could also be related to wind-induced mixing; "reduced brine rejection and subsequent vertical convection" or just "reduced convection" would be clearer. Then, this sentence suggests that this is true in general, while Miles et al. (2016) only describe Wilkes Land, which is a specific region with intense polynya activity and related high sea ice formation rate, and where wind perturbations are supressing katabatic winds; so I guess that "likely" is only valid for Wilkes Land and I suggest to either mention Wilkes Land or "regions of intense coastal polynya activity". [nicolas jourdain, France]	Taken into account. The paragraph has been rewritten and no confidence statement is attached to the processes leading to this coupling.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
95957	60	23	60	26	At present there is no evidence of reduced sea ice formation (e.g. Tamura et al. 2016 JGRO etc…). So while reduction of sea ice formation can potentitaly reduce mixing and allows warmer intrusions, further evidence is required to assess whether this occurs in present climate. Therefore "likely" seems to me not correct. [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paragraph has been rewritten and no confidence statement is attached to the processes leading to this coupling.
69731	60	23	60	29	Perhaps more important than sea ice is the effect of atmospheric winds on ocean conditions. There is evidence for changing wind direction being responsible for increasing access of CDW. See: Holland, P.R., Bracegirdle, T.J., Dutrieux, P., Jenkins, A., Steig, E.J., 2019. West Antarctic ice loss influenced by internal climate variability and anthropogenic forcing. Nat. Geosci. doi:10.1038/s41561-019-0420-9 [Matthew Hoffman, United States of America]	Taken into account. This paper is now referred to at the appropriate places in the restructured section.
24035	60	23	60	29	The modulation of sea ice to sub-ice-shelf melting is found from observation. Sun et al., 2019 (S., Sun, Hattermann, T., Pattyn, F., Nicholls, K. W., Drews, R., & Berger, S. (2019). Topographic shelf waves control seasonal melting near Antarctic ice shelf grounding lines. Geophysical Research Letters, 46, 9824– 9832. https://doi.org/10.1029/2019GL083881) presented timeseries of sub-ice-shelf melt rates near the grounding line of Roi Baudouin ice shelf in Dronning Maud Land in year 2017. In combination with oceanography observations in the region, it’s shown that topography wave triggered by Gunnerus Bank controls the amount of sub-ice-shelf melting at the grounding line of Roi Baudouin ice shelf, and the presence of sea ice modulate the strength of the topography waves, hence also significantly influence the amount of sub-ice-shelf melting. [Sainan Sun, Belgium]	Taken into account. This paper and the process it evidences are now mentioned in the relevant paragraph of the restructured section.
96979	60	23	67	55	Please clarify what the new findings compared to SROCC are. [Nicole Wilke, Germany]	Taken into account. The SROCC starting points are now clearly identified in the restructured section, usually at the beginning of paragraphs discussing new issues.
107307	60	23			ned to make a stateemnt of confidence before attach statement likley. Not certain that this would be warrented on one study. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paragraph has been rewritten and no confidence statement is attached to the processes leading to this coupling.
14751	60	26	60	27	Is it true that 'consistent observations' indicate that sea ice thickness/extent modulate ice shelf flow? This could be not causual (i.e. sea ice and ice shelf flow simply both responding a common forcing). Specifically cite study that establishes sea ice control on ice shelf flow [Jeremy Fyke, Canada]	Taken into account. We add a caveat in this sense and attribute medium confidence to sea-ice control on glacier flow and terminus position through a variety of processes mentioned.
9041	60	26	60	29	I suggest replacing "modulate" with "can modulate" as it is not necessarily true for all ice shelves (depending on the local configuration). [nicolas jourdain, France]	Accepted. Change implemented as suggested by the reviewer.
83321	60	26	60	29	This doesn't do full justice to recent findings pf important direct mechanical linkages and indirect linkages between sea ice (both pack and fast ice) and ice shelf dynamics, calving and disintegration. Please change lines 26-29 as follows to reflect and capture these advances, which also open up an important new line of cross-cryosphere research around sea ice-ice sheet interaction processes: "There is high confidence from consistent satellite observations that changes in sea ice coverage and thickness (both stationary coastal fast ice and moving pack ice) modulate both iceberg calving (Massom et al., 2010) and ice-shelf flow and glacier terminus position around Antarctica (Miles et al., 2013, 2016, 2017; Massom et al., 2015; Greene et al., 2018) around Antarctica. This may in turn affect the dynamics of upstream grounded ice (medium cinfidence). In addition, there is evidence from a combined observational and modelling study (Massom et al., 2018) that regional loss of a protective sea-ice buffer played a role in the rapid disintegration events of the Larsen A and B and Wilkins ice shelves on the Antarctic Peninsula since 1995, by exposing damaged (rifted) outer ice-shelf margins to enhanced flexure by storm-generated ocean swells (medium confidence)." THE NEW REFERENCES CITED: (1) Massom, R.A., A.B. Giles, H.A. Fricker, R.C. Warner, B. Legresy, G. Hyland, N. Young and A.D. Fraser. 2010. Examining the interaction between multi-year landfast sea ice and the Mertz Glacier Tongue, East Antarctica: Another factor in ice sheet stability? Journal of Geophysical Research. 115, C12027, doi:10.1029/2009JC006083. (2) Massom, R.A., A.B. Giles, R.C. Warner, H.A. Fricker, B. Legrésy, G. Hyland, L. Lescarmontier, & N. Young. 2015. External influences on the Mertz Glacier Tongue (East Antarctica) in the decade leading up to its calving in 2010. Journal of Geophysical Research - Earth Surface, 120, doi:10.1002/2014JF003223. (3) Massom, R.A., Scambos, T.A., Bennetts, L.G., Reid, P., Squire, V.A. and Stammerjohn, S.E. 2018. Antarctic ice shelf disintegration triggered by sea ice loss and ocean swell. Article in Nature 558, 383-389. [Robert Massom, Australia]	Taken into account. The paragraph has been rewritten and the suggested wording largely incorporated, including a reference to Massom et al. 2018

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46515	60	26	60	29	It's not clear here if you are talking about a direct link between sea ice extent / thickness (e.g., the impact of those things on ice sheet dynamics, as is often argued the case for sea ice and iceberg 'melange') or if you are only referring to the indirect impacts as they apply to modulating submarine melting (as per the prev. sentence here). It would be good to clarify here which is meant. [Stephen Price, United States of America]	Taken into account. The rewritten paragraph states that a variety of processes have been proposed. We hope that this clarifies the issue.
107309	60	26			previously have been discssing floating ice shlves, this sentence seems to be on ground ice. Need to clarify. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The restructured section, and in particular the rewritten paragraph, should be clear in this respect.
61723	60	31	60	38	Consider adding discussion of Scar Inlet Ice shelf, and the potential role that fast sea ice may place in temporarily stabilization. See for instance: Gang Qiao, Yanjun Li, Song Guo, Wenkai Ye (2020): Evolving Instability of the Scar Inlet Ice Shelf based on Sequential Landsat Images Spanning 2005–2018, Remote Sensing 12(36) doi: 10.3390/rs12010036 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This paper is now cited, but in its main context (evolving instability of SCAR inlet). For fast ice, it mainly cites other papers (Massom et al) and does not add much particularly relevant to this issue.
107311	60	31			paragraph out of place and perhaps belongs in general discussion on prev page about links between ocean warming and ice sheet dynamics [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The whole section was restructured to improve the flow.
67225	60	36	60	36	Improper use of confidence language: a single study should not be assigned a confidence level but only the collective findings from multiple studies. Same problem in next paragraph [Regine Hock, United States of America]	Taken into account. A) We deleted medium confidence and give numbers; B) it's not necessarily a wrong use of the calibrated language if a paper draws on multiple lines of evidence.
69733	60	40	60	40	The phrase " can also be due to" suggests this is a distinct process, but this is the effect of reduced buttressing from the ice shelves as discussed above. [Matthew Hoffman, United States of America]	Noted. Not applicable as the section was restructured and the sentence as such has disappeared.
67909	60	40	60	41	cite Smith et al. 2016 here for PIG grounding line retreat since the 1940s and connections to tropical Pacific, also Holland et al., 2019, Nat. Geosc. [Martin Truffer, United States of America]	Noted. Not clear what this refers to, but we do cite Holland et al. and Smith et al. 2017 in appropriate places in the restructured section.
110713	60	40	60	41	This sentence is meant to introduce into grounding line observations, but it suggests that mass losses occur as a result of grounding line migration, while physically the grounding line position is simply a result of the mass distribution (dynamics) according to the flotation condition. [Torsten Albrecht, Germany]	Noted. Not applicable as the section was restructured and the sentence as such has disappeared.
1761	60	40	61	5	Suggest adding more information on the factors responsible for glacier grounding retreat. [Michael Kennish, United States of America]	Taken into account. The restructured section discusses ice shelf thinning first, providing the basis for a good understanding of the later discussion of grounding line retreat.
107313	60	40			clarify by adding 'in addition to ice thinning' [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Not applicable as the section was restructured and the sentence as such has disappeared.
71827	60	44	60	44	I think Amundsen sector as one of the most dynamic region in Antarctica should have more details about studies showing grounding lines retreat (in addition to Rignot et al. 2014) Additional citations for Pine Island = Millilo et al. 2017, Parker et al. 2013. Additional citations for Thwaites = Millilo et al. 2019 Additional citations for Pope, Smith and Kohler : Scheuchl et al. 2016 REF: - Milillo, P., Rignot, E., Mouginot, J., Scheuchl, B., Morlighem, M., Li, X., & Salzer, J. T. (2017). On the short-term grounding zone dynamics of Pine Island Glacier, West Antarctica, observed with COSMO-SkyMed interferometric data. Geophysical Research Letters, 44, 10,436– 10,444. https://doi.org/10.1002/2017GL074320; - Park, J. W., Gourmelen, N., Shepherd, A., Kim, S. W., Vaughan, D. G., and Wingham, D. J. (2013), Sustained retreat of the Pine Island Glacier, Geophys. Res. Lett., 40, 2137– 2142, doi:10.1002/grl.50379. - P. MILILLO, E. RIGNOT, P. RIZZOLI, B. SCHEUCHL, J. MOUGINOT, J. BUESO-BELLO, P. PRATS-IRAOLA (2019) Heterogeneous retreat and ice melt of Thwaites Glacier, West Antarctica, SCIENCE ADVANCES30 JAN 2019 : EAAU3433 - Scheuchl, B., Mouginot, J., Rignot, E., Morlighem, M., and Khazendar, A. (2016), Grounding line retreat of Pope, Smith, and Kohler Glaciers, West Antarctica, measured with Sentinel-1a radar interferometry data, Geophys. Res. Lett., 43, 8572– 8579, doi:10.1002/2016GL069287. [Jeremie Mouginot, France]	Taken into account. We refer to SROCC for an assessment of grounding line retreat studies, in particular in the ASE, before the SROCC literature cut-off date. We do refer to Milillo 2019 in the revised text.

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9013	60	45	60	45	New study of grounding line retreat of Denman Glacier since 1996 in V Brancato, E Rignot, P Milillo, M Morlighem… - Grounding line retreat of Denman Glacier, East Antarctica, measured with COSMO-SkyMed radar interferometry dataGeophysical Research Letters, 2020 [Eric Rignot, United States of America]	Accepted. Study cited in the relevant place in the revised section.
71829	60	45	60	45	I believe that recent discovery of large grounding retreat along Getz coast (outside Amundsen or Peninsula) should be mentionned. Berry glacier has retreated by 15 km between 1996 and 2017 (Millan et al. 2020) REF: Millan, R., St-Laurent, P., Rignot, E., Morlighem, M., Mouginot, J., & Scheuchl, B.. (2020). Constraining an ocean model under Getz Ice Shelf, Antarctica, using a gravity-derived bathymetry. Geophysical Research Letters, 47, e2019GL086522. https://doi.org/10.1029/2019GL086522 [Jeremie Mouginot, France]	Accepted. Study cited in the relevant place in the revised section.
71833	60	46	60	46	In addition to Totten Glacier, I think that the report should mention the retreat of Denman Glacier another maor outlet glacier in East Antarctica (Brancato et al. 2020). REF: Brancato, V., Rignot, E., Milillo, P., Morlighem, M., Mouginot, J., An, L., et al. (2020). Grounding line retreat of Denman Glacier, East Antarctica, measured with COSMO-SkyMed radar interferometry data. Geophysical Research Letters, 47, e2019GL086291. https://doi.org/10.1029/2019GL086291 [Jeremie Mouginot, France]	Accepted. Study cited in the relevant place in the revised section.
9015	60	49	60	50	Add missing reference on grounding line retreat in English coast: Increased ice flow in Western Palmer Land linked to ocean melting AE Hogg, A Shepherd, SL Cornford, KH Briggs, N Gourmelen, JA Graham, ... Geophysical Research Letters 44 (9), 4159-4167 [Eric Rignot, United States of America]	Noted. The paper does not clearly show grounding line retreat rates, although the reported thinning and flow rate increase clearly point towards GL retreat.
67227	60	52	60	52	This sentence is confusing. [Regine Hock, United States of America]	Noted. This is a paper that was assessed in SROCC, so we only refer to the SROCC assessment of GL retreat rates. The sentence was cut.
107315	60	52			I am not certain that it is wise to discuss GL retreat separately from thinning. They are intimately connected - thinning leads to GL retreat, GL retrat reduced buttressing and leads to thinning. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Both are indeed strongly linked, but it proved very difficult, and confusing for readers, to mix reports on evidences for both processes.
27639	60	56	60	56	IGE: It is worth mentionning Gudmundsson et al. (2019, https://doi.org/10.1029/2019GL085027) who do not discount the possibility that a MISI is engaged, but their findings lessen the need to invoke a mechanism of self-sustained retreat to explain current rates of mass loss and support the notion that increased ice discharge is related to external climate drivers. [Eric Brun, France]	Accepted. Paper cited in this context.
76737	60	56	61	3	The paper by Feldmann & Levermann does not state that the collapse of the WAIS is presently underway but shows that if the Amundsen Region is destabilized, then the entire WAIS could subsequently collapse. [Ronja Reese, Germany]	Noted. Paper not cited any more (pre-SROCC and not needed for increased clarity of the discussion).
9043	60	56	61	5	Here I suggest to also mention Gudmundsson et al. (2019, https://doi.org/10.1029/2019GL085027) who wrote "explanations previously put forward to explain ongoing mass loss in WAIS include the possibility that part of the ice sheet is currently undergoing an unstable and irreversible retreat driven by an internal instability mechanism (Favier et al., 2014). Although we do not discount this possibility, our findings lessen the need to invoke a mechanism of self-sustained retreat to explain current rates of mass loss and support the notion that increased ice discharge is related to external climate drivers". [nicolas jourdain, France]	Accepted. Paper cited in this context.
74097	60	56			the „about as likely as not (medium confidence)“ cites three studies that find a collapse. The references should be balanced with studies that support stability, or the statement be revised. [Matthias Mengel, Germany]	Accepted. We changed our assessment: no substantial clarification of the situation since SROCC, therefore evidence is, with medium confidence, compatible with MISI, but evidence suggesting that MISI is not needed to explain observed behaviour is cited.
61725	61	1	61	5	Consider adding a sentence about Thwaites specifically (rather than the more general grounding line retreat in the Amundsen Sea Embayment) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Milillo et al. 2019 is cited in this respect in the revised draft.

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14753	61	1	61	5	Cite recent work by Robel (2019; https://doi.org/10.1073/pnas.1904822116) and Hoffman (2019; https://doi.org/10.1029/2019JF005155) and/or other relevantpapers describing role of climate and ice sheet variability In regulating grounding line retreat, and caution in text that observed grounding line rates may carry a notable signal of natural variability that could confound detection/attribution of underlying climate trend (use D/A language explicitly to tie to other D/A AR6 text) [Jeremy Fyke, Canada]	Noted. We do cite relevant papers now (Jenkins 2018, Hoffman 2019), but the Robel 2019 paper focuses on MISI projections, not on D&A. We therefore do not cite that latter paper here.
115475	61	2	61	2	consider adding Arthern and Williams (2017) also demonstrating ongoing long-term retreat of WAIS under modern climate forcing. I think Larour et al., 2019, also demonstrates this, despite uplift/sea level feedback. Nias et al., 2016 too. [Robert DeConto, United States of America]	Not applicable. The paragraph was substantially rewritten, and now concentrates more clearly on evidence of currently ongoing WAIS retreat instead of future projections. Therefore the suggested references are not perfectly relevant here.
115477	61	3	61	5	The intent of mentioning PIG's recent slow down isn't clear. Is this sentence meant to refute the previous sentence that ASE retreat might already be underway? This will confuse some readers. [Robert DeConto, United States of America]	Taken into account. We hope that the rewritten paragraph, discussion pro and cons concerning ongoing MISI, is clearer.
61727	61	3	61	5	consider adding more about the timing & triggers for Pine Island Glacier ground line retreat, see for instance: J.A. Smith, T.J. Anderson, M. Shortt, A.M. Gaffney et al., (2017): Sub-ice-shelf sediments record history of twentieth-century retreat of Pine Island Glacier, Nature 541, p. 77-80, doi: 10.1038/nature20136 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Paper cited.
62311	61	4	61	4	Change "interpreted" to "previously interpreted" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested by the reviewer.
9017	61	5	61	5	The stability of PIG GL since 2012 has to be counterbalanced with the 1 km/yr sustained retreat of Thwaites (Mililloet al., 2019) and the 2 km/yr retreat of Smith (see Grounding line retreat of Pope, Smith, and Kohler Glaciers, West Antarctica, measured with Sentinel-1a radar interferometry data B Scheuchl, J Mouginot, E Rignot, M Morlighem, A Khazendar Geophysical Research Letters 43 (16), 8572-8579) [Eric Rignot, United States of America]	Accepted. Paper cited.
69735	61	5	61	5	Somewhere in section 9.4.2 there should be a discussion of climate variabilty effects on ice shelves and their adjacent glaciers. Such a discussion could go here or perhaps on p 64. This is important for putting observed variability in context. Some references to follow, beginning with: Holland, P.R., Bracegirdle, T.J., Dutrieux, P., Jenkins, A., Steig, E.J., 2019. West Antarctic ice loss influenced by internal climate variability and anthropogenic forcing. Nat. Geosci. doi:10.1038/s41561-019-0420-9 [Matthew Hoffman, United States of America]	Accepted. This is discussed in the attribution paragraph at the end of the section.
69737	61	5	61	5	Snow, K., Goldberg, D.N., Holland, P.R., Jordan, J.R., Arthern, R.J., Jenkins, A., 2017. The response of ice sheets to climate variability. Geophys. Res. Lett. 44, 1–23. [Matthew Hoffman, United States of America]	Accepted. The strong sensitivity of WAIS in the ASE to decadal and longer ocean heat flux variability is mentioned in the attribution paragraph at the end of the section.
69739	61	5	61	5	Robel, A.A., Seroussi, H., Roe, G.H., 2019. Marine ice sheet instability amplifies and skews uncertainty in projections of future sea-level rise. Proc. Natl. Acad. Sci. 201904822. doi:10.1073/pnas.1904822116 [Matthew Hoffman, United States of America]	Taken into account. This paper is now cited in section 9.4.2.2 in relation to representation of observed feedback mechanisms in ice sheet models.
69741	61	5	61	5	Hoffman, M.J., Asay-Davis, X., Price, S.F., Fyke, J., Perego, M., 2019. Effect of Subshelf Melt Variability on Sea Level Rise Contribution From Thwaites Glacier, Antarctica. J. Geophys. Res. Earth Surf. 124, 1–24. doi:10.1029/2019JF005155 [Matthew Hoffman, United States of America]	Taken into account. This paper is now cited in section 9.4.2.2 in relation to representation of observed feedback mechanisms in ice sheet models.
71837	61	5	61	5	I would add Millilo et al. 2017 along with Konrad et al . 2018. REF: Milillo, P., Rignot, E., Mouginot, J., Scheuchl, B., Morlighem, M., Li, X., & Salzer, J. T. (2017). On the short-term grounding zone dynamics of Pine Island Glacier, West Antarctica, observed with COSMO-SkyMed interferometric data. Geophysical Research Letters, 44, 10,436– 10,444. https://doi.org/10.1002/2017GL074320; [Jeremie Mouginot, France]	Accepted. Cited now.
91087	61	5	61	5	See also Bamber and Dawson, 2020 that shows that, based on tends over last ~decade, PIG GL is unlikely to retreat far over next 5 decades [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cited now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129487	61	5			[CONFIDENCE] Some observational studies however suggest that Thwaites Glacier and surrounding glacier are undergoing sustained grounding line retreat (Scheuchl, 2017; Milillo, 2019) compensating for the Pine Island Glacier stabilization. Given these new developments, and the potential for grounding line retreat also discovered in one East Antarctica glacier (Denman glacier, Brancato, 2019), the estimate for medium confidence is overly conservative. [Trigg Talley, United States of America]	Noted. There are also arguments against ongoing MISI, discussed in the revised text, so we stand by the SROCC assessment that there is medium confidence that the observed behaviour is compatible with ongoing MISI.
104425	61	7	61	10	SMB. In statement: "Although progress has been made recently in quantifying current Antarctic precipitation rates using CloudSat (Palerme et al., 2014), there is an uncertainty of at least 10-15% (medium confidence) in current observation- and model-based values of the surface mass balance (SMB) of the grounded parts of the AIS of around 2100 Gt yr-1 (Favier et al., 2013; Van Wessem et al., 2018; Agosta et al., 2019)" - the period needs to be added. Also, the value "around 2100 Gt yr-1" seems to be based on Agosta et al 2019: 2120+/-99 Gt/year (MAR3.6.4/ERAI; 1979-2015); 2068+/-93 (RACMO2.3p2). In Wessem et al (2018) SMB for the grounded ice sheet is calculated excluding the Peninsula (giving the overall value of 1885+/-95 Gt/year (RACMO2.3p2; 1979-2014; ERAI). There is a recently submitted paper by Mottram et al (in review), which is referred to in the Atlas, using the latest RACMO and MAR versions, and other CORDEX models (COSMO-CLM, MetUM, HIRHAM5). [Irina Gorodetskaya, Portugal]	Taken into account. Period cited, and the recent paper by Mottram et al, just accepted in time, is now used.
91089	61	7	61	21	See Bodart, J. A., & Bingham, R. J. (2019). The impact of the extreme 2015–2016 El Niño on the mass balance of the Antarctic ice sheet. Geophysical Research Letters, 46, 13862– 13871. https://doi.org/10.1029/2019GL084466. Shows that ENSO has big effect on WAIS and AP in particular. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is discussed in the attribution paragraph at the end of the section.
24037	61	11	61	11	AIS mass balance—AIS surface mass balance? [Sainan Sun, Belgium]	Noted. The paper by Rignot shows that on shorts (interannual and up to decadal), precipitation dominates the variability of the total (not only surface) mass balance.
104429	61	13	61	17	SMB trends. 9.4.2.1, p61, L13-17: "Contrary to older studies (Monaghan et al., 2006), there is some recent evidence (low confidence) from modelling, observations and reanalyses that increased Antarctic precipitation might have mitigated dynamical ice mass losses by about 50 Gt yr-1 since about 1990 (Lenaerts et al., 2018; Medley and Thomas, 2019) and at lower rates over the entire 20th century (Medley and Thomas, 2019)."........=> This is a somewhat different message compared to SROCC, which gives 'high and medium confidence' of increased snowfall [SROCC, p3-45 to 3-46]: "Mass gains due to increased snowfall have somewhat offset dynamic-thinning losses (high confidence). On the AP, snowfall began to increase in the 1930s, accelerated in the 1990s (Thomas et al., 2015; Goodwin et al., 2016), and now offsets sea-level rise by 6.2 ± 1.7 mm per century (Medley and Thomas, 2018). EAIS and WAIS snowfall increases offset 20th century sea-level rise by 7.7 ± 4.0 mm and 2.8 ± 1.7 mm respectively (Medley and Thomas, 2018) (medium confidence). AIS snowfall increased by +4 ± 1 then +14 ± 1 Gt per decade over the 19th and 20th centuries, of which EAIS contributed 10% (Thomas et al., 2017b)." This change in confidence needs to be clearly explained (different interpretation of confidence? references are the same in SROCC and Ch9.. and/or additional evidence). There is a different message also in Atlas Antarctica, which stresses large interannual variability in surface mass balance in East Antarctica during recent decades, masking the trend [Atlas-96, L48-51]: "West Antarctica likely experienced an increase in surface mass balance mostly seen over the Antarctic Peninsula and the east part of West Antarctica, while the surface mass balance in East Antarctica showed strong interannual variability over recent decades, masking any possible existing trends (medium confidence due to limited observations)". This ES statement in Atlas Antarctica is supported by detailed review of publications [Atlas 5.9.1.2, Agosta et al., 2012; Medley et al., 2013; Altnau et al., 2015; Wang et al., 2015; Thomas et al., 2017; Medley et al., 2018; Medley and Thomas, 2019]. Note that Medley and Thomas, 2018 in SROCC and Medley and Thomas, 2019 in Ch9 and Atlas is the same publication. [Irina Gorodetskaya, Portugal]	Noted. The trends depend very much on the periods considered. Over the satellite period, even Medley & Thomas show an insignificant SMB decrease. The rewritten paragraph should make this issue clear.
81557	61	16	61	16	"lower rates"> quite vague, can't a number be specified? [Melchior van Wessem, Netherlands]	Not applicable. Sentence replaced by the SROCC assessment.
32435	61	23	61	25	() for Seroussi and Edwards citations - in fact, many wrong in this chapter, check all in the end, to many to list here. [Olaf Eisen, Germany]	Noted

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89399	61	23	61	27	An observation on the language: Despite the recent advances and breakthroughs in Antarctic ice-sheet modeling (as e.g. detailed in Pattyn 2018 (DOI: 10.1038/s41467-018-05003-z), this paragraph in particular, and the Chapter in general seem rather critical of the modeling approaches. [Ricarda Winkelmann, Germany]	Taken into account. The chapter acknowledges the huge effort by the ice sheet modelling community. The tone is hopefully more positive while assessing that uncertainties remain. Box 9.3 is now dedicated to explaining ISMIP, LARMIP-2 (and GlacierMIP)
89627	61	23	61	37	More references needed in this paragraph, particularly in the first half. In fact this paragraph seems oddly placed and repetitive with later sections -- e.g. there is more detailed (and referenced) text about representation of ice-ocean interactions in models in 9.4.2.2 (p64) [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A revised section on model evaluation has been placed in it's own section (now 9.4.2.2) and repetition with later sections reduced.
85307	61	23	61	37	I wonder if this useful summary would be useful nearer to the start of the ice-shelf and ice-sheet modelling section? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A revised section on model evaluation has been placed in it's own section (now 9.4.2.2) and repetition with later sections reduced.
15209	61	23	61	37	Reiterating an earlier comment because it is so central to AR6: the ability of WAIS ice sheet models to simulate dynamic processes is so central to GMSL projections that our knowledge of the limitations of the models deserve confidence assessments. While confidence language normally applied to scientific results, it is consistent with the IPCC uncertainty guidance to state the confidence in any finding, and the limitations of models is a finding of the ice sheet science community over the past 10-20 years. [Simon Donner, Canada]	Taken into account. A revised section on model evaluation has been placed in it's own section (now 9.4.2.2) and this includes confidence statements as the reviewer suggests.
62315	61	23	61	47	I suggest moving these two paragraphs to the start of the next section, 9.4.2.2. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. A new subsection 9.4.2.2 on model evaluation has been created where this content is now treated.
67229	61	23	61	47	The previous paragraphs of this sections are all about observed recent/past changes. The sudden jump into background on ice sheet modeling seems misplaced here. Separate subsection as suggested for Greenland? [Regine Hock, United States of America]	Accepted. Now separate sections for observations and model evaluation.
74099	61	23			I see the main reason for less mature modelling in the later start of model development and in the data scarcity. [Matthias Mengel, Germany]	Taken into account. A new subsection 9.4.2.2 on model evaluation has been created where this content is now treated.
62313	61	24	61	27	References needed throughout this sentence: "Since AR5, simulations are now more commonly evaluated or formally calibrated with modern observations (i.e. separate to initialisation) and/or paleodata (e.g., Ritz et al., 2015, Nias et al.,, 2019, Edwards et al., 2019), either through perturbed parameter ensembles or model intercomparison projects (e.g., Seroussi et al., 2020, Goelzer et al., 2020)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. References added to support statement (text now in 9.4.2.2).
35793	61	24	61	28	Either here or elsewhere, say more about the nature of model improvements since AR5. No estimates from any of the more advanced process-based models now available were used for projecting the Anarctica contribution to SLR in AR5 because they had not yet been published by the time of the WGI submission/acceptance cutoffs. As a result, AR5 used a simplfied statistical model to make projections to 2100 for Anarctica. See also my comment for p.65, lines 22-43 [Michael Oppenheimer, United States of America]	Taken into account. A new subsection 9.4.2.2 on model evaluation has been created where this content is now treated.
74101	61	24			„Since AR5, ..." this sentence should be supported by references. [Matthias Mengel, Germany]	Accepted. References added to support statement (text now in 9.4.2.2).
1763	61	28	61	31	Improved modeling of the ice-ocean interface would advance investigations of Antarctic ice-shelf mass losses. More discussion is needed on modeling limitations and how to improve model outcomes. [Michael Kennish, United States of America]	Taken into account. A detailed discussion of subshelf forcing is now in 9.4.2.2
46545	61	28	61	35	The work of Jeong et al. could be cited here as it summarizes initial efforts at including ice sheet / ocean coupling in an Earth system model, something that is (currently) a fairly unique capability. This is based on simulations using a fully coupled Earth system model (E3SM) that includes the impacts of ice-ocean coupling via the explict representation of ocean circulation (and heat and freshwater exchange) within Antarctic ice shelf cavities (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). [Stephen Price, United States of America]	Taken into account. This paper is assessed in a number of sections: 9.2.2.3, 9.2.3.2, 9.3.2.1 and Box 9.4.
46517	61	28	61	37	This is another place where one or more of the review papers in the Oceanography special issue (2016, volume 29, issue no. 4) should be referenced (for example, Dinniman, M., Asay-Davis, X., Galton-Fenzi, B., Holland, P., Jenkins, A., & Timmermann, R. (2016). Modeling Ice Shelf/Ocean Interaction in Antarctica: A Review. Oceanography, 29(4), 144–153. https://doi.org/10.5670/oceanog.2016.106). [Stephen Price, United States of America]	Taken into account. A detailed discussion of subshelf forcing is now in 9.4.2.2

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67231	61	29	61	31	The numbering is confusing (I, ii, iii) since there seem to be only two principal types of models standalone and coupled [Regine Hock, United States of America]	Taken into account. This has now been removed.
30647	61	33	61	34	Parameterized modelling of grounding line evolution is not necessarily in 'most' models. 'Some' models is more appropriate. The link with the cavity geometry changes is not clear. The sentence improves by writing it as: 'parameterized modelling of grounding line evolution that influences changes in subshelf cavity geometry in some ice sheet models, ' [Frank Pattyn, Belgium]	Taken into account. This assessment has been revised in 9.4.2.2
76739	61	33	61	34	I'm not sure what you mean with "parameterised modelling of grounding line evolution (i.e. cavity changes) in most ice sheet models"? [Ronja Reese, Germany]	Taken into account. This assessment has been revised in 9.4.2.2 and this sentence no longer appears.
115479	61	35	61	37	Use of "topography" might confuse readers. [Robert DeConto, United States of America]	Taken into account. This no longer appears in revised 9.4.2.2.
44561	61	50	61	50	This section on projections only discusses physical models projections. In the meantime SEJ projections (Bamber et al. 2019) are also available. Discussing the similarities/differences between these two approaches seems necessary here to broaden the scope of the section. [Dewi Le Bars, Netherlands]	Rejected. SEJ already discussed a few lines later.
112965	61	50	61	50	There are three major limitations in current ice sheet models that is worth discussing in more detail here: i) lack of parameterisation of relevant processes (i.e. missing physics such as grounding zone processes); ii) inadequate/insufficient knowledge of the bathymetry and bedrock and iii) inadequate/insufficient knowledge of ocean heat flux (we also call this ocean forcing), driving much of the excess (out of balance) melting, esp. in Antarctica. Have the observations have tracked higher than SSP5-8.5 in the last two decades? [Susheel Adusumilli, United States of America]	Accepted. These limitations and the challenges of models reproducing historical trends are discussed in more depth in Sections 9.4.2.2 (model evaluation) and 9.4.2.3 (drivers of future change) and Box 9.3.
6771	61	50	61	50	"Future projections" should be changed to "Projections". There are several other occurrences of "future projections" in this chapter, and most if not all should be changed to "projections". See also comment 9 on the entire report. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Future projections" has been changed to "Projections"
89401	61	50	62	36	How can the AR6 projections be reconciled with the estimates from AR5 and SROCC? Some observations: In AR5, the dynamic sea-level contribution from Antarctica was still purely based on expert elicitation, so there was no scenario-dependence due to that. In SROCC, the upper limit of the AIS contribution in 2100 was substantially higher than in AR6-Chapter 9, and scenario-dependent. Now it seems we have rather similar estimates to AR5 (and negligible scenario-dependence, but based on numerical ice-sheet modeling/MIPs) - how can these differences be explained? [Ricarda Winkelmann, Germany]	Accepted. We have revised the text to reconcile/explain better how our estimate agree or differ from AR5 and SROCC
99705	61	50	67	40	this section is also very disjointed - needs better organization. For example, you start with projections for 2100, then have extended discussion of models and processes, then discuss projections for 2200, then for longer term, then back to 2100 for Amundsen Sea sector. [Peter Clark, United States of America]	see response to comment #83327
83327	61	50	67	40	Section 9.4.2.2 Future ptojections is rather long and at times hard to follow e.g., by comparison with the equivalent Section 9.5.1.3 Projections for Terrestrial Glaciers (concidely covered on pages 73-74) and other sections. [Robert Massom, Australia]	Accepted. We have revised the section to make it more concise and reorganized it so that it is easier to follow
76763	61	50	67	40	Many thanks for the great effort to create this document! The text is overall very clear and comprehensive. I think that it could highlight the advances that have been made in the ice-sheet modelling community since AR5 more. An external reader might at the moment miss out that besides remaining uncertainties and knowledge gaps, also huge advances have been made, for example, the model intercomparison efforts, individual projections, better representation and understanding of uncertainties related to ice-ocean interaction, … [Ronja Reese, Germany]	Noted with thanks. We have revised the text so that it also highlights the advances made by the ice sheet modelling community
115171	61	50	67	40	This section could be broken up into subsections, perhaps one on uncertainty, to help with readability. [Natalya Gomez, Canada]	see response to comment #83327

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15211	61	50			I appreciate that you are awaiting findings from the MIP to finalize projections for each SSP (to 2100). Just wanted to flag that, as this section is currently written, it is not clear how the model results and structure judgements are combined to come up with the values that appear in the Executive Summary. Two additional things are necessary: 1) The method used to derive the "likely" values stated in the ES need to be really clear. 2) The low probability, low confidence long tail projections (e.g., which MICI and MISI) NEED to be in the ES. I appreciate that the CLAs and LAs may worry about making a "value" judgement by listing low confidence information in the ES. However, the value judgement would be to omit that information. Our job as IPCC authors is to present all policy-relevant information, warts and all. The possibility, even if low confidence, for accelerated melt or 2m GMSL by 2100 is policy-relevant, so it is our job to make sure it is communicated. I apologize for repeating this message again and again, but it is really important, you're welcome to send me nasty emails. Thanks for reading. [Simon Donner, Canada]	Accepted. This is now clarified in Box 9.3 (emulator section) and 9.6.3 (introduction).
14845	61	50			The box says that there is low confidence on the mode to simulate LIG. Why would be the confidence higher for simulating the future? [Marie-France Loutre, Switzerland]	Rejected. Statement is about both models and reconstructions; near-term forcings are more certain than paleo-forcings; uncertainty in multi-millennial future projections is also low confidence due to feedbacks and instabilities operating on those timescales (9.4.2.6).
62335	61	52	61	52	Because this section is all about model projections, I suggest replacing "Based on recent observations and numerical simulations" with "Numerical simulations of" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text substantially revised.
77833	61	52	61	53	This sentence is an assessment. I think it would be better to postpone it until you have cited and discussed the evidence. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Chapter revised throughout to discuss evidence before assessment statements.
14755	61	52	61	53	I suggest revisiting 'medium confidence' rating on AIS mass loss by end of century upwards..? I think increasing this confidence could be made by basing paleoclimate analogs (e.g. as shown on Figure 9.20), in addition to observations and numerical simulations [Jeremy Fyke, Canada]	see response to comment #67297
67297	61	52	61	53	Same statement (even stronger 'increasing rate') is made in SROCC (SPM B1) with high confidence [Regine Hock, United States of America]	Taken into account. The SPM statement does not appear in the underlying chapter sections for Antarctica and is not supported by the SROCC projections, which include constant or decreasing rate of mass loss by 2100. We have further clarified why further exploration of uncertainties in surface mass balance and ice sheet models lead to only medium confidence in net mass loss under all emissions scenarios (9.4.2.5).
77835	61	52	62	36	This paragraph is too long. Please introduce some paragraph breaks. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #22607
22607	61	52	62	36	This is a long and complicated paragraph. Could it be split into two or more more digestable paragraphs? [Peter Thorne, Ireland]	Accepted. We have revised the text so that this paragraph is no longer too long by splitting the text in different paragraphs
83323	61	52	62	36	This is a very long paragraph that is very hard to follow. [Robert Massom, Australia]	see response to comment #22607
67233	61	52	62	36	can this paragraph be broken up. It's too long and therefore hard to read. [Regine Hock, United States of America]	see response to comment #22607
107319	61	52			this is a long, dense paragraph that is hard going to read. Suggest that it is broken up into easier chunks. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #22607
74103	61	52			replace „will likely“ by „is projected to likely“ [Matthias Mengel, Germany]	Rejected. Projection implied.
16403	61	54	61	54	MISI as far as I can see is not used until page 65, so define acronym there instead (big jump otherwise). [Julian Mak, China]	Noted. This sentence has been removed due to reorganisation of text, however we have ensured that acronyms are defined in the location to where they are used a lot.
62327	61	55	61	55	It is unclear what "the latter" is referring to. I suggest replacing with "MISI," if this is what this is meant to refer to. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	see response to comment #110715
110715	61	55	61	55	Not clear what "the latter" refers to. [Torsten Albrecht, Germany]	Noted. We have rewritten the sentence to be more specific and without using the word "the latter"
14757	61	55	61	55	Unclear what 'the latter' refers to [Jeremy Fyke, Canada]	see response to comment #110715
74105	61	55			what is exactly referred to by „The latter"? [Matthias Mengel, Germany]	see response to comment #110715

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116875	61		61		Please expand on the evaluation of CMIP6 compared to CMIP5 for Antarctic climate, and check consistency with chapter 2. [Valerie Masson-Delmotte, France]	Noted. Unfortunately, there is very little literature on CMIP6 in Antarctica. Content checked with Atlas and relevant chapters.
110717	62	1	62	1	Better use “decimeter” as unit. [Torsten Albrecht, Germany]	Noted. However, we keep the use of meters as a unit so that it is more consistent with units used in other sections of the chapter.
62329	62	2	62	2	Similar to previous comment: it is unclear what "this contribution" is referring to. I suggest replacing with "the contribution due to MISI" unless this is simply referring to the contribution due to all ice dynamics (not just MISI), in which case I suggest replacing "this contribution" with "the contribution." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	see response to comment #44563
99707	62	2	62	2	should list AR5 numbers here [Peter Clark, United States of America]	Accepted. We now list the AR5 numbers
44563	62	2	62	5	This sentence is confusing because it refers to the previous sentences that are about MISI while here it is about the whole ice dynamics, not only MICI. [Dewi Le Bars, Netherlands]	Accepted, we have rewritten the section to remove this confusion
106647	62	2	62	5	It is not entirely clear what is the difference between the projections 0.16 (0.02 to 0.37) m in RCP 8.5 and 0.12 (0.03 to 0.28) m in RCP8.5. Do the first projections respresent only the dynamical contribution to sea level rise. I am sure whether all the studies involved the SROCC reassement provide values for the dynamical contribution of the AIS (in general, they also include projections that account for the surface mass balance). [Kevin Bulthuis, United States of America]	Accepted, we have rewritten the section to remove this confusion
91091	62	3	62	23	The inference that there is little scenario dependence for AIS MB is only the case for the emulation results, which do not appear to be especially consistent with other numbers cited in this para. Such as those from the SROC where the median increases by a factor 3 bettwen 2.5 and 8.5 and also in the studies in Table 9.2. This will be pretty confusing and potentially baffling for most readers. It also doesn't make too much sense given that you later => that SMB sensitivity is ~50 Gt/deg C and Seroussi gets a factor 2 difference in the 83% between RCP2.5 and 8.5 [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Role of mass gain vs loss in different studies now explained further.
10067	62	5	62	5	Should RCP8.5 be RCP4.5? [Tong Zhang, United States of America]	Rejected. Refers to RCP8.5.
106649	62	5	62	8	It might be interested to explain why the projections by Bamber et al. (2019) are higher than the SROCC assessment. This may include the fact the results by Bamber et al. (2019) reflect recent studies that have explored the AIS sensitivity to CO2 forcing during previous warm periods and new positive feedback processes such as the Marine Ice Cliff Instability. [Kevin Bulthuis, United States of America]	see response to comment #107317
107317	62	5			This is important. In order to assess the literature, you will need to explain the difference between SROCC and supporting process-based studies and the expert elicitation. It is not sufficient just to state that there is a difference. However, it is not possible to assess the expert elictation in the way that it is normal for process-based studies, because we cannot know why the experts choose to give the answers that they did. Similarly, it would be useful to understand why the two elictations are different (line 8) but this is not possible. Did they ask different experts, did the literature move on? We don't know. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have made clearer the role of SEJ as part of an alternative 'low confidence assessment' (9.6.3), noting the ambiguity in which processes are incorporated and the low agreement of individual expert assessments. We also discuss the effect of correlated ice sheet contributions in increasing the SEJ projections (Box 9.4).
2991	62	8	62	8	Grammar: ‘… Bamber et al. (2019) provide ...’ [Petteri Uotila, Finland]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
99709	62	8	62	9	What's the purpose of this sentence? If an assessment, explain why this is "notable" [Peter Clark, United States of America]	Accepted, this sentence has been removed
69645	62	10	62	10	surely it's not just ISMIP6 and LARMIP2, there must be other publications since SROCC? [Nicholas Golledge, New Zealand]	Rejected. New projections are indeed "primarily" from ISMIP6 and LARMIP. However, there is now more focus on DeConto et al. (submitted).
62333	62	10	62	10	I suggest starting a new paragraph beginning with "New projections since SROCC …" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text substantially revised.
33463	62	11	62	13	Change: “(Nowicki et al., submitted, a, submitted, b; Seroussi et al., submitted)) and the Linear Antarctic Response Model Intercomparison Project, Phase 2 (LARMIP-2, (Levermann et al., submitted, a)).» by « (Nowicki et al., submitted, a, b; Seroussi et al., submitted)) and the Linear Antarctic Response Model Intercomparison Project, Phase 2 (LARMIP-2, (Levermann et al., submitted a)).». [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication

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74107	62	13	62	17	the last two last panels of Fig. 9.20 (Observations vs ISMIP6 Model mean) shows that the models are not capable of modelling ice sheet loss where it is expected from observations and process understanding (continued and increasing losses in the Amundsen sea area). This is a major caveat that should be named oncerning projections. [Matthias Mengel, Germany]	Taken into account. A plotting error meant that only a small number of models had been included; the corrected figure now shows substantial thinning of Thwaites Glacier and Totten Glacier (Figure 9.18).
99711	62	13	62	17	sentence needs to be broken up [Peter Clark, United States of America]	Accepted. Text substantially revised.
14759	62	13	62	17	It's not made clear to readers in this discussion of AIS loss resulting from ice sheet models forced by climate models, that these models all almost certainly do not sufficiently represent near-coastal Antarctic oceanic conditions that are relevant to sub-shelf melting, due to very local-scale oceanographic circulation patterns, bathymetry, and sub-shelf geometries and circulation dynamics. Since the majority of the papers that describe the 'translation' from CMIP6 models to sub-shelf melting are still 'submitted' - it is hard at this point to judge how to phrase this. But it should be clear in final text, that this is a significant impediment to confidence, in AIS SLR contribtuions. This also extends to the emulation of Edwards et al. (presumably) since this would be based on the CMIP-forced results? [Jeremy Fyke, Canada]	Accepted. We have made the discussion of model limitations of ocean forcing more in-depth and clear: see in particular Section 9.4.2.2 (model evaluation) and 9.4.2.3 (drivers of change: subshelf melting). We show sensitivity of simulations to basal melt parameter values and assess there is low confidence in this aspect of the projections.
89409	62	14	62	14	Change to: Seroussi et al. 2020 (+ update in references: H. Seroussi, S. Nowicki, A.J. Payne, H. Goelzer, W.H. Lipscomb, A. Abe Ouchi, C. Agosta, T. Albrecht, X. Asay-Davis, A. Barthel, R. Calov, R. Cullather, C. Dumas, R. Gladstone, N. Golledge, J.M. Gregory, R. Greve, T. Hatterman, M.J. Hoffman, A. Humbert, P. Huybrechts, N.C. Jourdain, T. Kleiner, E. Larour, G.R. Leguy, D.P. Lowry, C.M. Little, M. Morlighem, F. Pattyn, T. Pelle, S.F. Price, A. Quiquet, R. Reese, N.-J. Schlegel, A. Shepherd, E. Simon, R.S. Smith, F. Straneo, S. Sun, L.D. Trusel, J. Van Breedam, R.S.W. van de Wal, R. Winkelmann, C. Zhao, T. Zhang, and T. Zwinger: ISMIP6 Antarctica: a multi-model ensemble of the Antarctic ice sheet evolution over the 21st century, The Cryosphere Discussions (2020), DOI: 10.5194/tc-2019-324) [Ricarda Winkelmann, Germany]	Editorial - copyedit to be completed prior to publication
74109	62	14			how do you compare the scenarios if each is forced by a different number of models? This is also a caveat and the potential influence on the numbers should be made explicit. [Matthias Mengel, Germany]	see response to comment #76751
33465	62	15	62	17	Change the reference format: “(Nowicki et al., submitted, a)” by “(Nowicki et al., submitted a)”. [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
76747	62	15	62	17	ISMIP6 is a great and comprehensive effort that brought together the ice-sheet modeling community to perform and compare projections for the Antarctic and Greenland Ice Sheets! I think that discussing more of the outcomes and findings of this project, especially the melt rate parameterisation for the Antarctic Ice Sheet and the uncertainty ranges tested in the parameterisation, could help to better interpret the numbers mentioned. For ISMIP6 two different tunings of the melt rate parameterisations were done which both are in line with observational or model estimates but lead to substantially different projections (Jourdain et al., under review, TCD). I think that due to time and computational limitations, in ISMIP6 the parameterisation with low sensitivity (MeanAnt) was mainly used and the tuning with high sensitivity (PIGL) was tested only for one CMIP5 model under RCP8.5, with the upper range of 30cm reported here coming from this simulation. In addition, the uncertainty in the “low-sensitivity” tuning has been tested in the ISMIP6 experiments. A similar test for the high sensitivity experiments has not been done yet (as far as I know), but it was found that the spread of melt rate projections (without ice dynamics) due to uncertainty in the low sensitivity parameterisation is much smaller than the spread in the high sensitivity parameterisation (Figure 9 in Jourdain et al. TCD under review). This would mean that the lower range of the projections has been tested widely and is well understood from the ISMIP6 experiments, but the upper range of projections is less well constrained at the moment. A further question that arises then is if those results (testing for the tuning uncertainty and CMIP5 model simulations) can be fully derived with the emulator? [Ronja Reese, Germany]	Accepted. Added discussion of differences in basal melt sensitivity between ISMIP6 and LARMIP including results from Reese et al..
65979	62	16	55	17	Suggest revising the whole document for this type of error, found at page 62 line 11-13. "2100. (Golledge et al., 2019), and the coupled ESM-ice sheet simulations of (Muntjewerf et al., submitted, a)". Suggest correcting to: "2100 (Golledge et al., 2019), and the coupled ESM-ice sheet simulations of Muntjewerf et al. (submitted, a)". [Kushla Munro, Australia]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
62331	62	18	62	18	I would replace "a range" with "a representative range" or something similar [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Now clarified ("sampling potential regional future climate changes.") in Box 9.3.
69743	62	19	62	19	Long paragraph - perhaps could be broken here. [Matthew Hoffman, United States of America]	see response to comment #22607

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89403	62	19	62	23	"[...] suggesting a limited scenario dependence of projected mass losses of Antarctica." -- This statement is questionable, given that other studies (Ritz et al. 2015, Golledge et al. 2015, De Conto and Pollard 2016, Levermann et al. 2020, Reese et al. 2020) show that there is indeed a noticable scenario dependence, especially when considering higher sensitivities of sub-shelf melting to ocean warming. From Figure 9.20 (last two panels, Observations and ISMIP6 Model Mean) it seems that sub-shelf melt patterns might not be well-captured in the ISMIP6 ensemble (see also Figure 9.21), and higher melt rates and mass loss might be expected particularly in the Amundsen basin under RCP8.5 if including higher melt sensitivities. Do we really expect there to be no scenario-dependence (and can we explain this physically? Or, is it rather that we cannot quantify it (yet)? [Ricarda Winkelmann, Germany]	Taken into account. Text revised to explain opposing accumulation and dynamic losses in more detail. We also note that Ritz et al. only present one scenario, and Levermann et al. + Reese et al. estimate only the dynamic changes; after adding SMB contributions to Levermann et al. the scenario dependence is small. See also response to comment #76747.
110719	62	19	62	23	The reasons for the very similar range should be better discussed. One important issue, mentioned earlier in the paragraph is the unequal number of used climate projections for the two different considered scenarios. But there are also other factors with respect to the used parameterizations. [Torsten Albrecht, Germany]	see response to comment #76751
69647	62	19	62	34	this section reads more like a journal paper in which the use of a particular dataset is being justified, rather than as an objective assessment. I wonder if we could move this material to a Box in which these two different ensembles are described, so that in the main text we just *assess* the results? [Nicholas Golledge, New Zealand]	Accepted. Text substantially revised.
42561	62	22	62	23	"limited scenario dependence" is beneath the point. Values given reflect no scenario dependence at all! [Sabine Hüttl-Kabus, Germany]	Taken into account. "Little to no scenario dependence" now used for ISMIP and LARMIP together.
76749	62	22	62	23	“suggesting a limited scenario dependence of projected mass losses of Antarctica” is a strong statement and should be analyzed more, in particular with respect to the melt sensitivity used (see also comment on p62,line15). For the weak melt sensitivity, as mentioned here and in Seroussi et al, the increases in surface mass balance are similar to the ocean-induced dynamic losses for RCP2.6 and RCP8.5. However, when the sensitive tuning of the melt parameterization is used (PIGL), ocean-induced mass loss increases substantially (by a factor of 12 in RCP8.5 for the CMIP5 model tested in Seroussi et al., under review). The ISMIP6 Antarctic experiments from Seroussi et al. do not include a test of the high-sensitivity tuning of the melt parameterization for RCP2.6. However, let me speculate a bit: Reese et al (TCD, under review) report that ocean-driven mass loss alone (without SMB changes) is scenario-dependent for the one CMIP5 model with both, RCP2.6 and RCP8.5 projections of ocean changes from ISMIP6, tested. So if melting was scaled up using the high-sensitivity tuning of the melt parameterisation, the ocean-driven mass loss would exceed mass gains through the SMB increases, and this could potentially make these projections scenario-dependent in this case. [Ronja Reese, Germany]	See responses to comments #76747 and #89403.
76751	62	22	62	23	The ISMIP6 experiments tested the RCP2.6 scenario for only two different CMIP5 models, while the RCP8.5 scenario was tested for 6 CMIP5 models (no other RCP scenarios were used). As Figure 7 in Seroussi et al (TCD, under review) shows, the spread in-between CMIP5 models is very large. You could discuss that further RCP scenarios from different models would be needed to test the scenario-independency. [Ronja Reese, Germany]	Taken into account. Added note about differing number of simulations, and more detail about scenario dependence analysis in Edwards et al. (submitted). We note this study includes additional high basal melt sensitivity simulations, and estimates the contribution of uncertainty of basal melt sensitivity, to projections under RCP2.6.
76753	62	22	62	23	In the ISMIP6 experiments, the “high-sensitivity” tuning of the melt parameterization was not applied for RCP2.6 scenarios, and also the uncertainty range of both tunings was missing. You could discuss that such experiments would be needed to test the robustness of this statement. [Ronja Reese, Germany]	See response to comment #76751.
74111	62	23	62	26	The two sentences seem overconfident to me. They suggest a mechanism is known and our ability to quantify the uncertainty, while looking at the last two last panels of Fig. 9.20 (Observations, ISMIP6 Model mean) suggest that the ice sheet models are not capable of coninuing the observed patterns of ice loss (Amundsen Sea glacier loss observed, strong ice shelf thinning modelled). [Matthias Mengel, Germany]	Taken into account. A plotting error meant that only a small number of models had been included; the corrected figure now shows substantial thinning of Thwaites Glacier and Totten Glacier (Figure 9.18). We have also expanded the discussion of the reasons for the varying projections and scenario dependence across climate and ice sheet models (9.4.2.5) and discuss model limitations at reproducing observations in Box 9.3 and 9.4.2.2.
33467	62	23			Change: “As shown in (Seroussi et al., submitted),” by “As shown in Seroussi et al. (submitted),”. [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30645	62	25	62	26	Wrong parentesis before Edwards [Frank Pattyn, Belgium]	Editorial - copyedit to be completed prior to publication
41465	62	25	62	26	This is an incredibly important part of the assessment. In fact, what is really needed here is a higher resolution of the 2/3 uncertainty chunk with a quantification of how much uncertainty is actually due to CMIP5 forcing input. Once this figure is obtained, potential revisions on how to average and present scenario specific responses would have to be implemented. [Alexander Nauels, Germany]	Taken into account. Agree this would be useful but now removed from original cited paper. However, further explanation of scenario-dependence is now added based.
33469	62	25			Change: "(Edwards et al., submitted)" by "Edwards et al. (submitted)". [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
67235	62	26	62	26	ISM: another example of an acronym that is better avoided. There are to many acronyms ! [Regine Hock, United States of America]	Accepted. Acronym removed.
35795	62	26	62	36	The chapter could do a better job of making clear which estimates from the various sources (like SROCC, AR5) and the various models or model intercomparisons do or do not inlcude the ice mass change due to climate change prior to 2015, both in table captions and text. You should also give some idea of the magnitude of this effect so the reader can put these differences into some context. [Michael Oppenheimer, United States of America]	Accepted. Table 9.3 now states these in detail.
76741	62	29	62	32	LARMIP-2 is based on a similar set of models as ISMIP6, but models and initial configurations are not identical (e.g. Úa did participate in LARMIP-2 but not in ISMIP6). [Ronja Reese, Germany]	Taken into account. We have added an estimate using a common subset of models to help comparison.
76743	62	29	62	32	Your comment "without removing historical trends in the ice sheet simulation" could be misunderstood to mean that the trend inherent in the initial configuration is not subtracted (especially after reading the paragraph about ISMIP6 earlier). My understanding is that LARMIP-2 does remove the trend in the initial configuration, similar to ISMIP6, by subtracting a control simulation. What you probably mean is that the LARMIP-2 projections start in 1900? [Ronja Reese, Germany]	Accepted. We have revised the text and removed this sentence.
69747	62	30	62	30	I'm pretty sure that LARMIP2 also did remove historical trends, so I think the parenthetical statement here is incorrect. [Matthew Hoffman, United States of America]	see response to comment #76743
110707	62	31	62	32	There is a recent study that focuses on the role of the melt parameterizations used in LARMIP-2 vs. ISMIP6 with quite some different projection estimates: https://doi.org/10.5194/tc-2019-330 [Torsten Albrecht, Germany]	see response to comment #76745
33471	62	31	63	3	Change: "…(Levermann et al., submitted, a)." by "(Levermann et al., submitted a).". [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
76745	62	32	62	34	A comparison of the ISMIP6 and the LARMIP-2 projections is done in (Reese et al., TCD, The role of history and strength of the oceanic forcing in sea-level projections from Antarctica with the Parallel Ice Sheet Model) which can be summarized as in the following. For the same model (here PISM) and the same initial configuration results are close to the median in both intercomparison studies, however, ocean-driven mass loss differs by an order of magnitude between ISMIP6 and LARMIP2 in the experiments presented in the study. This is linked to the lower melt sensitivity in the ISMIP6 experiments used in this study (open experiments in ISMIP6) in contrast to the LARMIP-2 sensitivity. The latter is found to be more in line with a coupled ice-ocean experiment from Thwaites Glacier (a more detailed assessment of melt sensitivities in different Antarctic ice shelves would be required for future tuning of melt parameterisations). Important to note is that the higher melt sensitivity that was used in a few simulations in ISMIP6 (based on the PIGL-tuning) leads to mass loss in ISMIP6 that is comparable with LARMIP-2. [Ronja Reese, Germany]	See response to comment #76747.
90693	62	34	62	38	Regarding the contribution of melting of GIS to SLR, I personally think that the confidence level of long term projections of Table 9.2 is likely low, or at best medium. For example, Scanlon et al. (2018) reported that global models underestimate large decadal declining and rising water storage trends relative to GRACE satellite data. Scanlon et al., 2018, Global models underestimate large decadal declining and rising water storage trends relative to GRACE satellite data, PNAS, www.pnas.org/cgi/doi/10.1073/pnas.1704665115 [Thian Yew Gan, Canada]	Rejected. Table 9.2 is about AIS not GIS, and this reference is not about GIS.
42563	62	39	63	2	Table 9.2: Mix of „likely range" and percentiles in the "Notes" column. Unification desirable. Also state if the percentiles are "true" or "bogus", i.e. associated with bogusly scaled distributions. [Sabine Hüttl-Kabus, Germany]	Accepted. Corrected to likely range (for assessments) or percentiles (for original studies). We are not sure what 'bogus' refers to but none of the presented likely ranges or percentiles are derived from scaling factors.
33473	62	39	63	3	Add a space in: "IPCC AR5 (Church et al., 2013a)Table 13.5", meaning "IPCC AR5 (Church et al., 2013a) Table 13.5". [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
96981	62	41	62	41	Is it intended to include the Bamber et al 2019 study in Tab. 9.2 since it is mentioned in the text? [Nicole Wilke, Germany]	Accepted. Role of Bamber (i.e. for SROCC and Section 9.5) now clarified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72017	62	41		44	You need toinclude the note about the additional several tenths of a metre potential contribution - this was attached to the AR5 projections everywhere they were printed in the AR5. And it is inaccurate to say these were ecluded from the AR5 projections on line 1 of page 62. [John Church, Australia]	Accepted. We now attach the additional several tenths of a meter of potential contribution everywhere that AR5 projections are printed and have corrected the mistake on line 1 of page 62
61737	62	44	63	1	The range of values in the final line of the table, for Edwards et al. (submitted), are the same for each RCP (2.6, 4.5, 8.5) column, whilst all other entries in the table differ with each column [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Emulator results very similar across scenarios; explanation given in text.
30649	62	45	62	47	It is not so much about whether this will lead to a collapse of the WAIS, but when it will lead to a collapse. The largest uncertainty pertains to whether it will collapse by 2100. This should be rephrased or specified. [Frank Pattyn, Belgium]	Rejected. We disagree that collapse is inevitable under all possible scenarios. We also note the reviewer did not alter the text as contributing author.
69643	62	55	62	55	not clear here what 'The latter' refers to - is it the MISI-driven contribution? I think it's important to note that 'dynamic discharge' of the kind added into the AR5 numbers don't necessarily have to come from MISI - flow acceleration and thinning doesn't always occur due to an 'instability', it just reflects an along-flow flux imbalance. [Nicholas Golledge, New Zealand]	see response to comment #110715
82153	63	1	63	19	Incorrect row on Table 9.2 - Edwards et al. 2019 repeated first case should be Bamber et al. 2019 [Jeff Ridley, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The two rows were Edwards et al. with and without MICI. Now removed to focus on post-SROCC literature.
115481	63	5	63	7	I understand what is intended here, but this could be interpreted as your saying it is 'very likely' Antarctica's contribution to future sea level rise will be negative!! [Robert DeConto, United States of America]	see response to comment #110721
110721	63	5	63	7	It should be made clearer that in this paragraph only the SMB/precipitation is considered, and that warmer climate means more ice accumulation, which is hence, considered as isolated effect, associated with a negative contribution to sea-level rise (as mentioned later in the paragraph). [Torsten Albrecht, Germany]	Accepted. Text restructured with SMB subsection.
14761	63	5	63	7	Include Frieler et al. (2015) as primary AIS-snowfall-negative sea level contribution. [Jeremy Fyke, Canada]	Not applicable, this part of the sentence has been removed.
32443	63	7	63	9	There is by now observational evidence, that temperature and accumulation are increasing on the East Antarctic plateau, see Medley, B., McConnell, J. R., Neumann, T. A., Reijmer, C. H., Chellman, N., Sigl, M., & Kipfstuhl, S. (2018). Temperature and snowfall in western Queen Maud Land increasing faster than climate model projections. Geophysical Research Letters, 45, 1472–1480. https://doi.org/10.1002/2017GL075992 [Olaf Eisen, Germany]	Taken into account. Regional observations are discussed in 9.4.2.1 and Atlas Section 11.1.
88663	63	8	63	8	The references Genthon et al., 2009a and Genthon et al., 2009b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
67299	63	10	63	10	add 'air' to temperature, or 'near-surface air temperature? [Regine Hock, United States of America]	Accepted. (Surface air temperature: now in Section 9.4.2.3)
67301	63	10	63	11	is spartially-averaged tmperature increase over Antarctica and continental warrming the same thing? Perhaps use the same wording in that case. [Regine Hock, United States of America]	Taken into account. Text substantially revised.
110723	63	11	63	13	Does this statement refer to a selection of CMIP5 models that fits best to present day precipitation observations or is CMIP5 in fact better than CMIP6 in Antarctica at present? [Torsten Albrecht, Germany]	Not applicable. This part of the paragraph has been removed
74113	63	12			Does it mean CMIP5 models are better than CMIP6? Why? [Matthias Mengel, Germany]	Not applicable. This part of the paragraph has been removed
105651	63	16	63	18	there are much recent estimates of changes in precipitation, e.g. Beaumet et al. (2019, https://doi.org/10.5194/tc-13-3023-2019). [Cécile Agosta, France]	Not applicable. This part of the paragraph has been removed
14763	63	18	63	19	Surface melting will increase, however, it's not entirely correct to directly relate this to sea level, since (as demonstrated currently in GrIS), refreezing within snow/firn has potential to majorly buffer (and/or entirely negate?) AIS surface melt contributions to sea level rise. [Jeremy Fyke, Canada]	Not applicable. This part of the paragraph has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104433	63	18	64	3	SfcMelt projections. Comment about the following statement: 'It is also very likely that surface melting will increase with rising air temperatures, i.e. contributing to sea level, but there is low confidence in the exact magnitude and location (Trusel et al., 2015). Surface melting is predicted to remain confined to low-lying areas at the margins during the 21st century even under scenarios of high greenhouse gas concentrations, except over the Antarctic Peninsula (e.g., Trusel et al. 2015).' ....=> Trusel et al. only looked at the increased surface melting over ice shelves, and did *not* consider changes in surface melt that may affect SMB. As these are ice shelves, any surface melting and/or fracture will not result in changes to sea levels. The only possible way they can affect sea level rise is by thinning/fracturing so that dynamic ice loss from the ice sheet accelerates. So the statement is simply not compatible with the Trusel et al paper. A suggestion to rephrase to something like: 'It is also very likely that surface melting over Antarctic ice shelves will increase with rising air temperatures over the 21st century, with melt on several ice shelves approaching levels that have historically been associated with ice shelf collapse, but as this is highly scenario dependent there is low confidence in the exact magnitude of the melt and the location (Trusel et al., 2015).' [Irina Gorodetskaya, Portugal]	Not applicable. This part of the paragraph has been removed
105653	63	18	64	5	I do not agree with the confidence level of the statement "Overall, ice loss due directly to surface melting is projected to be lower than the gain from additional snowfall, so the future net SMB contribution up to 2100 is negative (high confidence)." All statements on Antarctic surface melt (P63 L18 to P64 L3) are based on one article only, Trusel et al. 2015, where snowfall increase vs. melt increase over the grounded ice sheet is not discussed. I suspect Antarctic precipitation and surface melt to be badly resolved in most of the CMIP6 GCMs, as it was in CMIP5 models. So at the end I don't think we have sufficient elements in our hands to give high confidence to this statement. I would give medium confidence at best. [Cécile Agosta, France]	ACCEPTED. 9.4.2.3 now reads "there is only medium confidence that the future contribution of Antarctic SMB to sea level this century will be negative under all greenhouse gas emissions scenarios."
32437	63	28			change -- hyphens in ocean-ice-sheet --ice shelf [Olaf Eisen, Germany]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
106653	64	1	64	3	In might be interested to mention that the study by Trusel et al. (2015) predicts surface melt intensities by 2100 under high atmospheric scenarios that may lead to a disintegration of ice shelves. [Kevin Bulthuis, United States of America]	Not applicable. This part of the paragraph has been removed
9045	64	3	64	5	First, "SMB contribution" may not be clear, specify "contribution to sea level rise". Then, speaking about surface melting probably requires to clarify whether you speak about the grounded ice sheet or the entire ice sheet. As surface melting is expected to increase mostly over the ice shelves, it could be specified that surface melting is not just "lower" but "much lower" than gain from additional snowfall. [nicolas jourdain, France]	Accepted. We have clarified these terms and definitions throughout 9.4 (this text now in Section 9.4.2.3: SMB)
88665	64	3	64	5	"Overall, ice loss due directly to surface melting is projected to be lower than the gain from additional snowfall, so the future net SMB contribution up to 2100 is negative (high confidence)". The sentence is correct? [Rosemary Vieira, Brazil]	see response to comment #9045
27641	64	3	64	5	IGE: Surface melting is not just "lower" but "much lower" than gain from additional snowfall over the grounded ice sheet. So it is worth making clear that the grounded part is most important for sea level rise. [Eric Brun, France]	see response to comment #9045
107321	64	3			important to note here that surfac elt in Antarctic may have important impacts on the ice dynamics via ice shelf collapse. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part of the paragraph has been removed
69649	64	4	64	4	future net SMB contribution...is negative' - presumably what is meant is 'contribution to GMSL'? This needs to be explicit; at the moment it is ambiguous. [Nicholas Golledge, New Zealand]	see response to comment #9045
69745	64	4	64	4	Suggest changing the phrase "so the future net SMB contribution" to "so the future net SMB contribution TO SLR" to avoid ambiguity on the sign convention [Matthew Hoffman, United States of America]	see response to comment #9045
62337	64	4	64	4	I suggest replacing "so the future net SMB contribution up to 2100 is negative" with "so the future net SMB contribution to global mean sea level up to 2100 is negative" or "so the future net SMB contribution to the mass of the ice sheet up to 2100 is positive." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	see response to comment #9045
14765	64	4	64	4	"so the future net SMB contribution up to 2100 is negative" -> "so the future net SLC contribution up to 2100 is negative" [Jeremy Fyke, Canada]	see response to comment #9045
95959	64	4	64	4	positive surface mass balance? [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #9045

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110725	64	5	64	5	This sounds like a quite radical statement. I recommend to rephrase in terms of "no substantial improvements in the extreme climate region of Antarctica" to emphasize that progress may have been made from CMIP5 to 6, but not in this specific region. [Torsten Albrecht, Germany]	Accepted.
77837	64	6	64	7	In the AR5 that is -0.07 m to -0.01 m SLE. Please clarify the sign. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
82949	64	7	64	7	I wonder if the statement related to CMIP6 results can be updated (depending on availability of respective CMIP6 results). [Sebastian Gerland, Norway]	Accepted. Projections from CMIP6 models given in Section 9.4.2.3, along with estimates from parametric fit using CMIP6 temperatures.
2993	64	7	64	11	This sentence is difficult to understand. How does aggressive mitigation result in stronger Antarctic surface warming than less aggressive one? This seems counterintuative. The brief explanation following with respect to relative warming and ozone should be elaborated. [Petteri Uotila, Finland]	Accepted. We have added "due to delayed response of the Southern Ocean to stabilised emissions" (Section 9.4.2.3)
62345	64	8	64	11	This statement needs more contextualization: "projected Antarctic surface warming by 2100 is somewhat stronger in stabilization and aggressive mitigation scenarios than in higher emission scenarios, due to stronger global-relative Southern Ocean warming and relatively stronger effects of ozone recovery." What impact does this stronger warming in mitigation scenarios have on surface melt versus increased precipitation? I see a placeholder statement here and I would suggest another sentence or two discussing this result in the next draft of the report. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	see response to comment #2993
105655	64	14	64	15	in West Antarctica (in the Antarctic Peninsula dynamical losses are mainly attributed to ice shelves collapses, e.g. Rignot et al. 2019 http://www.pnas.org/lookup/doi/10.1073/pnas.1812883116). [Cécile Agosta, France]	Accepted. Text substantially revised to clarify regional drivers (e.g. 9.4.2.1)
1765	64	14	64	18	Shelf basal melting is a major factor of uncertainty in AIS projections. Instability of marine parts of the ice sheet adds to the uncertainty of the ice shelf projections discussed on pages 64-67. It would be helpful if there is greater focus and discussion on model upgrades and other measures necessary to better constrain AIS and ice shelf melt projections, and how this will lead to improved estimates of sea level contributions. [Michael Kennish, United States of America]	Noted. We have made the uncertainties related to basal melting more explicit and have noted a few areas where improvements may arise in the future. However, we have tried to ensure that the text remains as an assessment of the current state of knowledge, rather than speculation as to what advances may be made in the future.
77839	64	14	65	1	This paragraph is too long. Please introduce some paragraph breaks. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text substantially revised.
22609	64	14	65	1	This is a long and complicated paragraph. Could it be split into two or more more digestable paragraphs? [Peter Thorne, Ireland]	see response to comment #77839
83325	64	14	65	1	Again this is a very long sentence that is difficult to follow. [Robert Massom, Australia]	see response to comment #77839
14767	64	14	65	1	Suggest moving this detailed (good) description of difficulty in modelling AIS sub-shelf melt rates, to before the section (~pg. 9-62) in which SLC contribution projections from AIS are presented. As it stands, these are presented without context (see comment above to this regard). This context is available in this section. [Jeremy Fyke, Canada]	Accepted. Now before projections - see subsection in Section 9.4.2.3.
107323	64	14			this is a long, dense paragraph that is hard going to read. Suggest that it is broken up into easier chunks. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #77839
105657	64	15	64	17	I don't understand why you cite these 3 papers here whereas you describe deeply the ISMIP6 effort above (P62 L10-L29). [Cécile Agosta, France]	Not applicable. This sentence has been removed.
61729	64	16	64	16	It's not clear to me what the "no additional changes in climate" is relative to. Provide a date, CO2 level, or some other context. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This sentence has been removed.
69651	64	19	64	19	suggest changing 'onto the Antarctic shelves' to 'onto the Antarctic continental shelf' to avoid confusion (since ice shelves are also mentioned earlier in the same sentence). [Nicholas Golledge, New Zealand]	Accepted. Revised (Section 9.4.2.3, subsection 'Sub-shelf melting').
9047	64	26	64	28	I suggest moving the second brack of citations after "other models exist", and only quote Dinniman et al. 2016 (which gives an overview of existing models), and maybe new model descriptions since that paper, i.e. Mathiot et al. (2017), Zhou and Hattermann (2020, https://doi.org/10.1016/j.ocemod.2019.101536), and maybe Gwyther et al. (2020, https://doi.org/10.1016/j.ocemod.2020.101569) that describes three models including two with no reference in Dinniman et al. (2016). [nicolas jourdain, France]	Taken into account. Text removed to focus on post-SROCC literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46547	64	28	64	31	The work of Jeong et al. could also be cited here as it summarizes simulations that already include ice sheet / ocean coupling in an Earth system model, something that is (currently) a fairly unique capability. This is based on simulations using a fully coupled Earth system model (E3SM) that includes the impacts of ice-ocean coupling via the explict representation of ocean circulation (and heat and freshwater exchange) within Antarctic ice shelf cavities (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). [Stephen Price, United States of America]	Not applicable. This sentence has been removed.
33475	64	28			Is this correctly written: “ocean--ice-sheet--ice-shelf”? [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
16405	64	29	64	29	Chronological order in referencing. [Julian Mak, China]	Editorial - copyedit to be completed prior to publication
88667	64	29	64	29	Goldberg et al., 2012. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
9049	64	30	64	30	Jourdain et al. 2018 is not relevant here, it uses a simple ocean model. However, Jordan et al. (2018, https://doi.org/10.1002/2017JC013251) is very relevant as it uses a novel coupling method. [nicolas jourdain, France]	Not applicable. Text removed to focus on post-SROCC literature.
88669	64	30	64	30	Jourdain et al., 2018. reference not found. [Rosemary Vieira, Brazil]	see response to comment #9049
67303	64	30	64	40	There appears a lot of repetation of earlier sections that details how the models work. Can this be streamlined? [Regine Hock, United States of America]	Accepted. Text deleted.
69749	64	31	64	31	Long paragraph - perhaps could be broken here. [Matthew Hoffman, United States of America]	see response to comment #77839
14769	64	31	64	47	These two extremely long run-on sentences should be broken up and clarified. Important information within. [Jeremy Fyke, Canada]	see response to comment #77839
69653	64	31	64	53	This block seems out of place - it doesn't read like an objective assessment of the literature, more like a review paper. I think it could be refocused to show how the respective shortcomings of each method manifest in terms of the confidence (or lack of) that we assign to the projections they produce. [Nicholas Golledge, New Zealand]	Taken into account. Text substantially revised.
88671	64	37	64	37	"Deconto" and Pollard, 2016. The correct is one "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
69751	64	37	64	37	A slightly more complicated version of iv is described in Hoffman, M.J., Asay-Davis, X., Price, S.F., Fyke, J., Perego, M., 2019. Effect of Subshelf Melt Variability on Sea Level Rise Contribution From Thwaites Glacier, Antarctica. J. Geophys. Res. Earth Surf. 124, 1–24. doi:10.1029/2019JF005155 [Matthew Hoffman, United States of America]	Accepted. Citation added.
106655	64	38	64	38	In the list of methods used to prescribe sub-shelf melt rates, the authors might consider to add the use of box models of the circulation in ice-shelf cavities (e.g. Lazeroms et al (2018) and Resse et al. (2018); see also Bulthuis et al. (2019) in which such a box model is used for sea-level rise projections). [Kevin Bulthuis, United States of America]	Accepted. We have now added a fuller consideration of results from box models used in Bulthuis and Reese.
32439	64	38	64	47	The limitations fail to list our still incomplete coverage of sub-ice shelf cavities and also ice thicknesses around some of the GLs. Should be listed here, especially as sub-km resolution is mentioned later as a benefit for lower uncertainties (e.g. 9-65 line 30). [Olaf Eisen, Germany]	Accepted. We have now added more details on the limitations that exist.
33477	64	38			Change: “Nowicki et al., submitted, a;” by “Nowicki et al., submitted a;”. [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
26389	64	40	64	40	models ((De Rydt ->Double (([María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
9051	64	40	64	41	Another reference for this is Jordan et al. (2018, https://doi.org/10.1002/2017JC013251). [nicolas jourdain, France]	Not applicable. Sentence has been removed.
90449	64	40			get rid of one open-parenthesis "(" - there are two at this moment [Holly Kyeore Han, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
33479	64	40			Erase first parenthesis: “…((De Rydt…”. [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
26391	64	43	64	43	(e.g (Donat-Magnin et al., 2017;->Extra parenthesis before Donat-Magnin [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33481	64	43			Add a parenthesis before: (e.g (Donat-Magnin et al., 2017; Timmermann and Goeller, 2017)". [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
26393	64	45	64	45	Check parenthesis : (e.g. (Lazeroms 45 et al., 2018; Reese et al., 2018; Pelle et al., 2019)) [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
89411	64	45	64	45	Change to: Reese et al., 2018b (+ include in references as Reese, R., Albrecht, T., Mengel, M., Asay-Davis, X., and Winkelmann, R.: Antarctic sub-shelf melt rates via PICO, The Cryosphere, 12, 1969–1985, https://doi.org/10.5194/tc-12-1969-2018, 2018b) [Ricarda Winkelmann, Germany]	Editorial - copyedit to be completed prior to publication
76757	64	45	64	45	The citation Reese et al. 2018 here is wrong and should be "Reese, R., Albrecht, T., Mengel, M., Asay-Davis, X., and Winkelmann, R.: Antarctic sub-shelf melt rates via PICO, The Cryosphere, 12, 1969–1985, https://doi.org/10.5194/tc-12-1969-2018, 2018. ". [Ronja Reese, Germany]	Editorial - copyedit to be completed prior to publication
26395	64	47	64	47	Check parenthesis: (Jourdain et al., submitted)) [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
26397	64	49	64	49	Check parenthesis: (e.g. (Seroussi et al., 2017, submitted; Goldberg et al., 2018)). [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
107325	64	53			Thisis a bold and important statement but needs to be made more precise, which ice shelevs ad increase relative to what? There is a lot of interannual and spatial variability in melt rates so need to be very careful. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have now changed this statement to fully acknowledge the uncertainties and the reasons for them.
27643	64	54	64	54	IGE: The sentence is unclear, there is low confidence because of limited observational constraints, uncertainty in the physics of parameterized processes, missing processes, and biases in climate models. [Eric Brun, France]	Accepted. We have now changed this statement to fully acknowledge the uncertainties and the reasons for them.
9053	64	54	65	1	I would say "low confidence because of limited observational constraints to calibrate parameterizations, uncertainty in the physics of parameterized processes, missing processes (e.g. tides, swell, sea ice, subglacial water), and bias in the ocean components of CMIP models, including those related to the absence of feedbacks related to ice-shelf melting (Golledge et al. 2019; Bronselaer et al. 2018; Donat-Magnin et al. 2017). [nicolas jourdain, France]	Accepted. This text has been modified as requested.
106709	65	22	65	23	I would argue that uncertainties are in ice-sheet models are indeed better quantified as compared with AR5. However, this uncertainty in the projections is still large (especially beyond 2100) especially due to uncertainty in physical processes (especially the inclusion or not of MICI) and the variety of computational ice-sheet models used. This should be better acknowledged in the report. [Kevin Bulthuis, United States of America]	Taken into account. In particular, see 9.6.3 for alternative 'low confidence' projections incorporating MICI and SEJ.
106707	65	22	65	26	Suggestion of a reference review paper for uncertainties in Antarctic projections (this reference may be of interest elsewhere in AR6): "Frank Pattyn and Mathieu Morlighem. The uncertain future of the Antarctic Ice Sheet. Science, 2020" [Kevin Bulthuis, United States of America]	Taken into account. Cited under instability section.
129489	65	22	65	43	While it is noted that ice sheet model uncertainty is dominated by ice shelf melt rates and grounding line response, it is not explicitly stated how dependent grounding line response is to estimates of bedrock topography. Bedrock topography (and its representation in ice sheet models) remains a source of uncertainty in future simulation results, especially in WAIS (e.g., Nias et al., 2016, Schlegel et al., 2018), and it has not yet been well quantified. Representation of bedrock topography is restricted by general errors in the quantification of bedrock elevation below the ice sheet (Morlighem et al., 2020) and the ability for ice sheet models to resolve them. The importance of bedrock topography is well-described for the GrIS section, but is not as detailed for Antarctica (though it may be much more important for the modeling of the AIS and quantification of uncertainty i.e. MISI and other thresholds). Citation: Morlighem, M., E. Rignot, T. Binder, D. D. Blankenship, R. Drews, G. Eagles, O. Eisen, F. Ferraccioli, R. Forsberg, P. Fretwell, V. Goel, J. S. Greenbaum, H. Gudmundsson, J. Guo, V. Helm, C. Hofstede, I. Howat, A. Humbert, W. Jokat, N. B. Karlsson, W. Lee, K. Matsuoka, R. Millan, J. Mouginot, J. Paden, F. Pattyn, J. L. Roberts, S. Rosier, A. Ruppel, H. Seroussi, E. C. Smith, D. Steinhage, B. Sun, M. R. van den Broeke, T. van Ommen, M. van Wessem, and D. A. Young. 2020. Deep glacial troughs and stabilizing ridges unveiled beneath the margins of the Antarctic ice sheet, Nature Geoscience. 13. 132-137. https://doi.org/10.1038/s41561-019-0510-8 [Trigg Talley, United States of America]	Taken into account. Added that modelling has improved due to improved knowledge of bedrock topography and cited this paper.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35797	65	22	65	43	Here again is a discussion of ice sheet model improvements since AR5 which should be consolidated with the discussion of p.61, lines 23-37. The apparent rationale for separating them is that one is under a section on Antartica generally and the other on projection but this really isn't optimal. I suggest combining the sections to better reveal how particular models improve on the AR5 state-of-the-art so as to allow a process-based projection in AR6. A table of model characteristics would be helpful. Then in the projection section, the discussion could indicate how each advance improves aspects of projection while leaving us short in other respects, e.g., to what degree do we believe that particular models capture MISI and what model characteristics lead us to believe that each model does or does not capture this process adequately? There are statements in the text here and there about uncertainty re MISI (and MICI) but separating those from the discussion of how processes are represented in models leads to incoherence. [Michael Oppenheimer, United States of America]	Taken into account. Text revised and restructured to highlight post-AR5 and post-SROCC changes, and to clarify advances and uncertainties by process.
67305	65	22	65	44	This is an example of a number of paragraphs in this chapter that are difficult to read since they are dominated by references, sometimes more >50% of the length of a sentence is references, often interrupted by a few words, so hard to read. Are all really necessary, esp in light of the recent SROCC? [Regine Hock, United States of America]	Taken into account. Text revised to focus on post-SROCC literature
69655	65	22	66	39	Again, this reads more like a review paper than IPCC assessment - can it be trimmed down to *just* policy-relevant statements, with supporting references? [Nicholas Golledge, New Zealand]	Taken into account. Text substantially revised.
107327	65	22			Key issue which is whether the parameterization of GL dynamics used in coarse resolution models are robust. Needs to refer to large MISMIP literature and make an assessment on whether coarse grid models that rely on GL parameterization really are good enough for projections to 2100. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We refer to MISMIP in Box 9.3 and 9.4.2.2, and described the post-AR5 progress, e.g. "All models participating in ISMIP6 and LARMIP-2, for example, simulate ice shelf and grounding line evolution, and include subshelf melt parameterisation, which was not the case in the SeaRISE intercomparison."
74115	65	22			You state here that models and estimates improved. As an overall comment on section 9.4.2: While there was a large effort of the community to advance process understanding, many parts of the section read rather destructve, often giving the impression that we know less than we knew in AR5. Care should be taken to not leave the reader with such impression. [Matthias Mengel, Germany]	Taken into account. Text substantially revised.
106703	65	23	65	23	intercomparison projects and perturbed parameter ensembles. I would also add uncertainty quantification methods as new methods to assess uncertainty in ice-sheet models. Uncertainty quantification methods are kind of distinct from more traditional perturbed parameter ensembles and have been used for instance in Bulthuis et al. (2019). [Kevin Bulthuis, United States of America]	Accepted. Emulators now described explicitly.
106705	65	23	65	26	I would add Schelgel et al. (2018) in the list of refences. [Kevin Bulthuis, United States of America]	Rejected. Focusing on post-SROCC advances due to space constraints.
16407	65	24	65	24	Chronological order in referencing. [Julian Mak, China]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
88673	65	24	65	24	DeConto et al., 2019. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88675	65	24	65	24	"Deconto" and Pollard, 2016. The correct is one "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
88677	65	24	65	25	The references Edwards et al., 2019a and Edwards et al., 2019b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
49989	65	24	65	26	Please add Gilford et al. (2019) to these citations as it explores this using an emulation technique of perturbed parameter ensembles (similar to that of the cited Edwards et al. 2019). [Daniel Gilford, United States of America]	Accepted. Added reference (text now in 9.4.2.2 Model evaluation).
9055	65	26	65	26	"particularly for ice-ocean interactions" may not be so clear (given the previous paragraph mentionning that melt rate is a big unknown). "grounding line dynamics" or "grounded/floating transition" may be more apropriate. [nicolas jourdain, France]	Not applicable. This part of the paragraph has been removed
46519	65	26	65	27	With respect to studies of ice sheet / ocean interactions using improved / next generation ice sheet models, Hoffman et al. (2019) should be included as a reference (Hoffman, M. J., Asay-Davis, X., Price, S. F., Fyke, J., & Perego, M. (2019). Effect of Subshelf Melt Variability on Sea Level Rise Contribution From Thwaites Glacier, Antarctica. Journal of Geophysical Research: Earth Surface, 124(12), 2798–2822. https://doi.org/10.1029/2019JF005155). [Stephen Price, United States of America]	Not applicable. This part of the paragraph has been removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27645	65	27	65	29	IGE: A way to avoid confusion between coupling in the previous paragraph and coupling here would be to say that even without resolving the ocean-ice interactions in ice shelf cavities, ice-sheet models increasingly account for heat and freshwater exchange between ice-sheets and the climate system. [Eric Brun, France]	Not applicable. This part of the paragraph has been removed
9057	65	27	65	30	First, the ice-shelf meltwater provides a global (maybe regional) feedback to the climate system, but I would not call it "local" in view of the provided references. Also, "regional" is a bit misleading here as the three references have a global scope. Then, the sentence is unclear because "coupling" is ill-defined. In the previous paragraph, coupling means that the ocean circulation and ice melting are resolved under an ice shelf of evolving geometry. Here, it means that ice-shelf melting in the ice-sheet model depends on temperature from an ocean model that does not resolve the ocean under the ice shelves, but with ice-shelf (and possibly iceberg) meltwater re-injected into the ocean. A way to make the sentence clearer would be to say that even without resolving the ocean-ice interactions in ice shelf cavities, ice-sheet models increasingly account for heat and freshwater exchange between ice-sheets and the climate system. [nicolas jourdain, France]	Not applicable. This part of the paragraph has been removed
88679	65	30	65	30	"Deconto" and Pollard, 2016. The correct is one "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
78825	65	32	65	35	Maybe confidence in ice-sheet projections could also be improved through "hindcasting" experiments in which the ice-sheet models, similar to the historic simulations of climate models, aim to reproduce observed mass loss? [Ronja Reese, Germany]	Not applicable. This part of the paragraph has been removed
9059	65	35	65	37	calving could be added to the list of heavily parameterized processes. [nicolas jourdain, France]	Not applicable. This part of the paragraph has been removed
24039	65	35	65	37	Key processes of dynamic ice loss that are not well known/largely simplified by parameterizations include also calving, fracturing. [Sainan Sun, Belgium]	Not applicable. This part of the paragraph has been removed
69657	65	36	65	36	suggest removing 'see earlier discussion' since this is an assessment, not a discussion. Maybe refer to the subsection by number instead? [Nicholas Golledge, New Zealand]	Not applicable. This sentence has been removed.
88681	65	39	65	39	Nowicki and Seroussi, 2018. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
46521	65	39	65	39	Perego et al. (2014) should also be referenced here as the goals / methods of that paper focus directly on these issues. I.e., initialization of ice sheet models in order to both be consistent with present day observations and minimize spurious trends due to the use of data assimilation methods (Perego, M., Price, S., & Stadler, G. (2014). Journal of Geophysical Research : Earth Surface Optimal initial conditions for coupling ice sheet models to Earth system models. Journal of Geophysical Research: Earth Surface, 119, 1894–1917. https://doi.org/10.1002/2014JF003181.Received). [Stephen Price, United States of America]	Rejected. Cited in Greenland section
129491	65	39			Add Schlegel (2018), which is directly relevant to the interplay between sensitivity to melting and sensitivity to basal friction. Citation: N.-J. Schlegel, H. Seroussi, M. P. Schodlok, E. Y. Larour, C. Boening, D. Limonadi, M. M. Watkins, M. Morlighem, and M. R. van den Broeke, Exploration of Antarctic Ice Sheet 100-year contribution to sea level rise and associated model uncertainties using the ISSM framework,The Cryosphere, 12, 3511-3534, https://doi.org/10.5194/tc-12-3511-2018. [Trigg Talley, United States of America]	Rejected. Focusing on post-SROCC advances due to space constraints.
129493	65	39			Add Larour et al. (2014), which was the first effort to calibrate a model against an existing dataset (altimetry). Citation: E. Larour, J. Utke, B. Csatho, A. Schenk, H. Seroussi, M. Morlighem, E. Rignot, N. Schlegel, and A. Khazendar, Inferred basal friction and surface mass balance of the Northeast Greenland Ice Stream using data assimilation of ICESat (Ice Cloud and land Elevation Satellite) surface altimetry and ISSM (Ice Sheet System Model), The Cryosphere, 8, 2335-2351, doi:10.5194/tc-8-2335-2014. [Trigg Talley, United States of America]	Rejected. Paper refers to Greenland (and is cited in that section).
26401	65	40	65	40	(e.g Nowicki and Seroussi, 2018; Seroussi et al., 2019)-> (e.g.,..) [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
49991	65	40	65	41	Please add to this list of open questions "how to interpret and incoorporate paleo climate observation to calibrate ice-sheet models (DeConto and Pollard 2016, Edwards et al. 2019, Gilford et al. 2019)" [Daniel Gilford, United States of America]	Taken into account. Increasing use of paleodata to evaluate models, and LIG constraint on MICI, discussed in 9.4.2.2 (Model evaluation).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16409	65	41	65	41	“remains” -> “remain”? [Julian Mak, China]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
26399	65	41	65	41	Check parenthesis: (e.g, (Goldberg et al., 2015; Reese et al., submitted)) [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
85309	65	45	65	45	Is it worth stating something along the lines that 'Modelling the Antartic shelf near-coastal T-S characteristics to drive ice-shelf models is extremely challenging, with serious implicatons for confidence in ice-shelf model projections? For example, there are considerable biases in the near coastal and offshore wind stresses, eddies, coastal and slope currents, larger scale Southern Ocean T and S characteristics, sea-ice characteristics, etc, all of which significantly adversely impact on Antarctic shelf T-S characteristics.' For example, large temperature biases near the west antarctic ice shelves in the Hadley Centre N216-1/4 HadGEM3 GC3.1 coupled climate model currently make it impossible to even use our new active ice shelf model to make projections. Presumably the same applies to Greenland ice-shelves, where models certaintly do not adequately capture T-S characteristics in narrow fjords and I suspect would not adequately represent them even with downscaling modelling efforts, given the issues with biases in the larger scale boundary conditions from coupled models? Given these many issues and the huge complexities of ice-sheet dynamics are we confident that we can really robustly provide upper limits via expert judgement on sea-level rise due to ice-shelf melt (or might it be better to say we cannot provide such estimates with sufficient confidence or to provide some qualificaiton to these estimates to explain the confidence in them)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We added a statement on internal climate variability and its effect of rapid mass loss and MISI of the Antarctic ice sheet. Expert judgement has not been cited in this section anymore.
2995	65	45	65	45	Should be: ‘… mass losses are ...’ [Petteri Uotila, Finland]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
7637	65	45	65	45	Change "The largest uncertainty … are …" to "The largest uncertainties … are … " [Daniel Lowry, New Zealand]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
14771	65	45	65	47	MISI/MICI are indeed large uncertainties. However, a citeable basis is needed for the claim 'the largest uncertainty' is these processes (versus, e.g., carbon emission scenario uncertainties) - otherwise this 'largest uncertainty' should probably be removed. [Jeremy Fyke, Canada]	Accepted. This has been rephrased as 'a major uncertainty'.
82951	65	45	66	1	I suggest to see consider findings from the publication by Morlighem et al. (2020, Nature Geosc., https://doi.org/10.1038/s41561-019-0510-), to be included in this discussion. [Sebastian Gerland, Norway]	accepted. The paper by Morlighem et al (2019) has been included
46523	65	45	66	7	Somewhere in the section on the future of the Antarcitic ice sheet (possibly here, where MISI is being discussed?), there should be some discussion regarding the potential impacts of internal climate variability on ice marine ice sheet evolution. At the moment, I don't see anything on that topic in here but it seems like an important topic to at least briefly summarize. Two recent papers (there may be more) that look at this in some details include Robel et al. (2019) and Hoffman et al. (2018). (Robel, A. A., Seroussi, H., & Roe, G. H. (2019). Marine ice sheet instability amplifies and skews uncertainty in projections of future sea-level rise. Proceedings of the National Academy of Sciences, 116(30), 14887–14892. https://doi.org/10.1073/pnas.1904822116 AND Hoffman, M. J., Asay-Davis, X., Price, S. F., Fyke, J., & Perego, M. (2019). Effect of Subshelf Melt Variability on Sea Level Rise Contribution From Thwaites Glacier, Antarctica. Journal of Geophysical Research: Earth Surface, 124(12), 2798–2822. https://doi.org/10.1029/2019JF005155) [Stephen Price, United States of America]	Accepted. Internal climate variability and its impact on the ice sheet and MISI has been mentioned in the text with references that are proposed.
15841	65	45	66	7	A discussion of MISI is (1) misleading and (2) outdated. Starting with (2), there is a growing body of recent studies that show that the grounding line can be stable on retrograde slopes e.g. Gudmundsson et al. (2012) doi: 10.5194/tc-6-1497-2012, Gudmundsson (2013) doi: 10.5194/tc-7-647-2013, Schoof et al(2017) doi.org/10.5194/tc-11-2283-2017, Haseloff and Sergienko (2018) doi: 10.1017/ jog.2018.30, Sergienko and Wingham (2019) doi: 10.1017/jog.2019.53 It is misleading becauce it applies to an ice sheet in a steady state only. The MISI and climate change are mutually exclusive, it's either instability with constant environmental conditions or changes in atmopsheric and oceanic forcing that drive the grounding line migration. [Olga Sergienko, United States of America]	Accepted. MISI may occur with lack of buttressing from ice shelves (for instance ice shelf collapse; Sun et al. (2020)) and it is true that with sufficient buttressing stable GL positions can be found (Gudmundsson et al., 2013). This has now been clearly stated, as well as the fact that rapid grounding line retreat is not necessarily MISI.
72019	65	45		56	Need to add here thestabilizing nfluence of bedrock rebound, particularly in the low viscosity region below WAIS. [John Church, Australia]	Accepted. Thank you for the suggestion. We have included this with appropriate references (Larour, Gomez).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61485	65	49	65	50	recommend one paper: Vaughan, David G. "West Antarctic Ice Sheet collapse–the fall and rise of a paradigm." Climatic Change 91.1-2 (2008): 65-79. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Old reference. Since then, significant progress has been made, especially since this is a review paper.
11165	65	49	65	50	recommend one paper: Vaughan, David G. "West Antarctic Ice Sheet collapse–the fall and rise of a paradigm." Climatic Change 91.1-2 (2008): 65-79. [Teng Li, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #61485
9019	65	50	65	50	It is a bit surprising that with glaciers retreating at more than 1 km/yr, twice faster than in Greenland, ten times faster that the World glaciers and ice caps, scientists are still on the fence to figure out is this is MISI or not in West Antarctica. Some of these glaciers are retreating 2 km/yr. What is needed for someone to call it MISI? 10 km/yr retreat? And there is medium confidence that the trend of the past 40 years will continue based on Ritz et al. 2015 (which is a very coarse model) and Golledge et al. 2017 (which does not have ocean forcing) and Leveermannn submitted? For an issue of such importance, this is a very poor assessment. [Eric Rignot, United States of America]	Taken into account. It is important not to confuse fast grounding-line retreat with MISI. MISI provokes fast grounding line retreat, but any fast GL retreat is not necessarily a MISI. MISI is a self-reinforcing mechanism in which the grounding line moves into deeper waters, irrespective of forcing. This has now been clearly explained. Moreover, the assessment is not only based on those cited papers. See also Nias et al (2019), Seroussi et al (2019), Seroussi et al. (2020), Sun et al (2020), ...
89629	65	54	65	56	Need to reference Morlighem et al. (2019) Deep glacial troughs and stabilizing ridges unveiled beneath the margins of the Antarctic ice sheet. Nature Geoscience, https://doi.org/10.1038/s41561-019-0510-8 [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #82951
61739	65	54	65	56	In this paragraph I think it might be good to mention specifically where topographic conditions that would allow for MISI have been identified, and not just the projections in the previous sentence. For example, the study by Ross et al. (2012) for the Weddell Sea sector, and the more recent Morlighem et al. (2020). One example from the latter, again for the Weddell Sea sector, is that the retrograde slope upon which the Recovery Glacier is grounded is 800 m deeper than previously thought. References: Ross et al. (2012) Steep reverse bed slope at the grounding line of the Weddell Sea sector in West Antarctica. Nature Geoscience, vol 5, p383-396, DOI: 10.1038/NGEO1468. & Morlinghem et al. (2019) Deep glacial troughs and stabilizing ridges unveiled beneath the margins of the Antarctic ice sheet. Nature Geoscience, vol 13, p132-137 https://doi.org/10.1038/s41561-019-0510-8 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. See also comment #82951. It is not only topography bed topography that is responsible for MISI, buttressing (and its absence) is important. Narrow deep troughs may well exhibit sufficient buttressing to keep stable grounding lines (see also #15841). The reference to Ross et al. (2012) is older and implicitly incorporated in Morlighem et al. (2020).
88683	65	55	65	56	Fretwell et al., 2021. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
32441	65	55	65	56	Sutter et al., (2020, GRL, in press) show by combination of paleo-ice core records with modelling that the Wilkes Basis did most likely not collapse during the LIG. [Olaf Eisen, Germany]	Accepted. We added the reference in a statement on the reduction of uncertainties in LIG and Pliocene sea levels.
2485	65	55			I think you schould also include Morlighem et al. 2020 (Fig. S60) with respect to EAIS MISI [Thomas Ronge, Germany]	see response to comment #82951
14773	66	1	66	1	Maximum rates of retreat are also dependent on climate variability (Robel et al. 2019, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2018JF004709; Hoffman et al. 2019, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JF005155), internal ice conditions (presumably) and other factors. Suggest expanding this sentence and references accordingly. [Jeremy Fyke, Canada]	Taken into account. We made reference to Robel et al. (2019).
49993	66	1	66	7	It should be noted here that MISI would lead to projections with long upper tails, with important implications for relative sea-level rise and planning/adaptation (Robel et al. 2019, Rasmussen et al. 2020)-------------Rasmussen, D. J., Buchanan, M. K., Kopp, R. E., & Oppenheimer, M. (2020). A flood damage allowance framework for coastal protection with deep uncertainty in sea level rise. Earth's Future, 8, e2019EF001340. https://doi.org/10.1029/2019EF001340-------------Alexander A. Robel, Hélène Seroussi, Gerard H. Roe. Marine ice sheet instability amplifies and skews uncertainty in projections of future sea-level rise. Proceedings of the National Academy of Sciences Jul 2019, 116 (30) 14887-14892; DOI: 10.1073/pnas.1904822116 [Daniel Gilford, United States of America]	Taken into account. We made reference to Robel et al. (2019).
77841	66	2	66	2	I think "even" should be deleted. Self-sustaining means it carries on if the forcing is removed. Retreat may indeed carry if the forcing is maintained, but that's not a self-sustaining instability. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part of the paragraph has been removed
69753	66	3	66	3	Additional reference: Waibel, M.S., Hulbe, C.L., Jackson, C.S., Martin, D.F., 2018. Rate of Mass Loss Across the Instability Threshold for Thwaites Glacier Determines Rate of Mass Loss for Entire Basin. Geophys. Res. Lett. 45, 809–816. doi:10.1002/2017GL076470 [Matthew Hoffman, United States of America]	Accepted. This reference has now been included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14775	66	4	66	6	If these rates are not consistent with remotely realistic forcing, these studies probably shouldn't be included here. For context, unrealistically-forced model results are unlikely to occur elsewhere in AR6, unless in the context of sensitivity study results. [Jeremy Fyke, Canada]	Not applicable. This part of the paragraph has been removed
62157	66	5	66	5	This wording does not convey objectivity. Consider re-wording to be more specific about why the rates are unrealistic - are the melt rates orders of magnitude larger than other studies? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This part of the paragraph has been removed
106711	66	6	66	6	Check the year of the reference for Cornford et al. (2016). Can it be 2015 instead of 2016? [Kevin Bulthuis, United States of America]	Not applicable. This part of the paragraph has been removed
9061	66	9	66	24	I'm not an expert on this, but it would be worth summarizing the results of Clerc et al. (2019, https://doi.org/10.1029/2019GL084183), who show that the MICI 90-m critical height implies ice-shelf removal in under an hour. [nicolas jourdain, France]	Accepted. We added that reference in the discussion
69755	66	9	66	24	The paragraph on MICI should include an italicized indication of confidence level in this mechanism, which presumably is low or very low, given the lack of corroborating evidence and modeling. [Matthew Hoffman, United States of America]	See response to comment #109087
106715	66	9	66	24	In this paragraph, I would explain the study by Edwards et al. 2019 has shown that the MICI was not necessary to reproduce the high sea levels in the Pliocene. This is a key point of this study to should be highlighted tobetter understand the limitations of the MICI mechanism. [Kevin Bulthuis, United States of America]	see response to comment #49995
15843	66	9	66	24	A discussion of MICI is premature. No stability analysis has been done to demonstrate that the process indeed leads to instability. There is no geophysical obsrevation that supports this mechanism. In contrast, the exisiting observations (e.g. Olsen and Nettles, 2019 doi:10.1029/2019JF005054) indicate that calving is buoyancy-driven and not a failure of the high subaerial ice cliff as required by MICI. [Olga Sergienko, United States of America]	Accepted. We now give a more in-depth discussion on MICI with its caveats.
109087	66	9	66	48	There are 3 "low confidence" statements. While the assessment of the diversity of MICI research is informative, can this section more succinctly summarize what is considered more substantial knowledge gained by the community? [Chaincy Kuo, United States of America]	Taken into account. We have rewritten this section and inform on the MICI potential as well as the criticism on the mechanism as reported in the literature.
72021	66	9		24	As well as commenting on the uncertainty of the paleo climate data used for tuning, you should also comment about the larger assumed surface melt rates in DeConto nd Pollard compared with Trusel et al. (2015) [John Church, Australia]	Accepted. Added to Section 9.4.2.3, subsection 'Ice shelf disintegration'.
88685	66	13	66	13	"Deconto" and Pollard, 2016. The correct one is "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
26403	66	15	66	15	Check parenthesis: DeConto and Pollard (2016) by (Edwards et al., 2019b) [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
88687	66	15	66	15	The references Edwards et al., 2019a and Edwards et al., 2019b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
106721	66	15	66	15	(Edwards et al., 2019b) -> Edwards et al. (2019b) [Kevin Bulthuis, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
49995	66	15	66	22	It is critically important to note that the interpretation of paleoclimate constraints (e.g. DeConto and Pollard 2016 vs. Edwards et al. 2019) has major implications for the conclusion reached with regards to MICI; this is one of the biggest drivers of deep uncertainty in the role of the AIS. Improvements in LIG sea levels have the potential to inform MICI in more detail (Giford et al. 2019) especially with regards to the role of MICI. Following an emulation method similar to Edwards et al (2019), Gilford et al. (2019) show that while there is a limit to how effective the LIG is for constraining instability processes (and the associated future AIS contributions to GMSL), but reductions in LIG uncertainties have potential to constrain and inform instability processes. How much the LIG constrains future projections depends on improvements in paleo sea level observations and the LIG sea-level storylines considered... but it is a path forward to reducing deep uncertainties associated with MICI. [Daniel Gilford, United States of America]	Accepted. We have added this in the discussion, with the reference proposed.
77843	66	19	66	20	Maybe it's relevant to mention that Edwards et al. find MICI is not needed for the mid-Pliocene. Are you suggesting that's because they use a different value for sea-level then? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #49995
115483	66	20	66	24	Excellent treatment of the MICI issue here. [Robert DeConto, United States of America]	Noted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106727	66	21	67	22	The expression "The volume susceptible to the MISI" seems a bit odd to me. I would suggest something like: The (complete) collapse of the WAIS due to the MISI correspond to 3.3 m SLE. [Kevin Bulthuis, United States of America]	Taken into account. This has been amended with the publication of Sun et al (2020)
69659	66	22	66	22	clarify that this is high ice shelf *surface* melting, not basal melting (their RCM was too warm and inconsistent with models like RACMO that agree better with observations) [Nicholas Golledge, New Zealand]	see response to comment #14777
9063	66	22	66	22	"due predominantly to the high ice shelf melting" remains mysterious. I suggest either removing or explaining further. [nicolas jourdain, France]	see response to comment #14777
62339	66	22	66	22	I would add to "due predominantly to the high ice shelf melting": "due predominantly to the high ice shelf melting required to initiate MICI in the model." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	see response to comment #14777
14777	66	22	66	22	"due predominantly to the high ice shelf melting" - unclear what this refers to [Jeremy Fyke, Canada]	Not applicable. Sentence has been removed.
9065	66	30	66	33	Another key reference for projections of firn air depletion, even if based on CMIP3 scenarios, is Kuipers Muneke et al. (2014, doi: 10.3189/2014JoG13J183). [nicolas jourdain, France]	Accepted. We added the reference.
88689	66	32	66	32	Deconto and Pollard, 2016. The correct one is "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
115485	66	35	66	35	Great to see the mention of a possible negative feedback on hydrofracturing here and the 'very low confidence' statement on the timing of future loss (line 38) is on the money. [Robert DeConto, United States of America]	Noted with thanks.
82953	66	37	66	37	I suggest rewording the part of the sentence related to the mass loss and the number 8 mm, since mm is not a mass unit, such as writing "mass loss equivalent to …". [Sebastian Gerland, Norway]	See response to comment #22611
22611	66	37	66	38	Over what timescale? Per year or by some specified date? [Peter Thorne, Ireland]	Not applicable. Sentence has been removed.
9067	66	37	66	38	An explanation may be needed here: hydrofracturing has a very small impact until 2100 not because the process itself is not important, but because in most regions, projected ice-shelf collapse occur at he very end of the 21st century or much later (which is also found in the projections of firn air depletion to 2300 from Kuipers Muneke et al. 2014). [nicolas jourdain, France]	We have added this reference and statement.
27647	66	37	66	38	IGE: Hydrofracturing has a very small impact until 2100 not because the process itself is not important, but because in most regions, projected ice-shelf collapse occur at he very end of the 21st century or much later (which is also found in the projections of firn air depletion to 2300 from Kuipers Muneke et al. 2014). [Eric Brun, France]	See response to comment #9067
107329	66	38			Might add that ere is only one study to explicitly include this process and so based on lack of eviedence along this must have low confidence. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have given an in-depth discussion on MICI, from which the confidence level is clear. It is stated that this only stems from one model.
87999	66	38			hydrofacture [Kathleen McInnes, Australia]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
106717	66	41	66	41	5-95%-> 5-95% range [Kevin Bulthuis, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
106719	66	41	66	43	range(s) -> uncertainty range(s) [Kevin Bulthuis, United States of America]	Accepted. Text modified as suggested.
35799	66	41	66	48	One brief paragraph devoted to the 2100-2300 timeframe is inadequate. SROCC highlighted this timeframe and there is plenty to be said about the results from the SROCC models as well as the non- and partial-process-based projections in this timeframe (elicitation, inrtegrated/probabilistic, etc.). Does AR6 agree with SROCC assessment of this timeframe or not? What is similar and what differs and why? There is great interest in this timeframe, as well as very-long-term commitment. A separate subsection on each would be the best approach. In the current draft, these timeframes are buried. Section 9.6.3.5 has a paragraph on projection beyond 2100 and there may be a way to increase coherence of the discussion of these timeframes by moving the current section, which is about projection, into 9.6.3.5, splitting into 2100-2300 and very long term, and assigning separate subsections for each. [Michael Oppenheimer, United States of America]	Accepted. We have now greatly expanded the text in this section to clearly present separate paragraphs for projections at 2200, 2300, 2300-2500, and for multi-millennial timescales. We have also prefaced each section with a summary of AR5 and SROCC findings, and conclude each section with a summary assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67307	66	41	66	48	This is hard to read and grasp with all those long lists of numbers. Can this be put in a table? [Regine Hock, United States of America]	Rejected. Whilst we accept that tabulating values is a useful approach, in this case many of the studies present figures in different ways or for different periods, meaning that it is not possible to tabulate them into uniform fields.
42565	66	42	66	42	Which probability/percentile range is meant by "ranges"? Other numbers in this paragraph are given with 5-95% ranges. [Sabine Hüttl-Kabus, Germany]	Accepted. The values referred to are the range of values from two different experiments, not a probability distribution, so this has now been stated explicitly.
88691	66	44	66	45	The references Edwards et al., 2019a and Edwards et al., 2019b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
44565	66	47	66	48	There is low confidence for Antarctic projections at 2200, and what is the confidence for 2100? This chapter should discuss this confidence as well, it is important for the sea level projection section. [Dewi Le Bars, Netherlands]	Accepted. We have attempted to clarify the confidence statements for different time periods to ensure consistency throughout the chapter.
62341	66	50	66	50	I suggest changing "millenial" to "multi-millenial" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text now substantially revised with specifics from SROCC and individual studies.
110727	66	50	66	51	There is also a more recent study, which may be cited here: https://doi.org/10.5194/esd-2019-78 [Torsten Albrecht, Germany]	Noted with thanks.
106725	66	50	67	17	Maybe it might be interested to also mention the results by Bulthuis et al. (2019) by 3000 (with uncertainty range). Especially for RCP 8.5, the collaspe of the Antarctic ice sheet is shown to occur before 3000. [Kevin Bulthuis, United States of America]	Noted with thanks. We now cite this paper extensively.
107331	66	50			long-term is sfficiently important to warrant its own subsection. Lost here. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have now greatly expanded the text in this section to clearly present separate paragraphs for projections at 2200, 2300, 2300-2500, and for multi-millennial timescales. We have also prefaced each section with a summary of AR5 and SROCC findings, and conclude each section with a summary assessment.
106723	66	51	66	51	(e.g. (Viczaino et al., 2008;Goelzer et al., 2012): Check for brackets [Kevin Bulthuis, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
33483	66	51			Add a parenthesis at the end of the sentence "(e.g. (Vizcaíno et al., 2008; Goelzer et al., 2012).". [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
88693	67	1	67	1	Deconto and Pollard, 2016. The correct one is "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109703	67	1	67	2	This is a good start but contains (even for this section) an excessive oversimplification reflecting a purely glaciological perspective. Any mention (as in this paragraph) of the river runoff consequences of mountain glacier recession under climate change - which has obvious implications to readers around water resources, hydropower, ecological instream flows, and so forth - needs to be accompanied by a water resource science and engineering perspective. Failure to do so can lead to wrong conclusions; we would also want to ensure that this section doesn't have any potential to be viewed as a little unprofessional by applied environmental and water scientists, engineers, and managers, of course! Refering the reader to Chapter 8 is not quite sufficient, as not all readers will follow that instruction, and because the corresponding sections of Chapter 8 as currently written are (though a great start) also told primarily from a glaciologist's perspective, though hopefully that will be improved in the next draft. So, this passage in Chapter 9 needs to have something like the following added at the end: "Detailed relationships between climate, glaciers, and watershed hydrology, however, can be very complex, requiring integrated river basin-wide water resources science and engineering studies using data-driven (Fleming and Clarke 2003; Dahlke et al 2012; Moore et al 2020) and process-simulation (Nolin et al 2010; Schnorbus et al 2014; Jost et al 2012) approaches. Glaciers damp downstream water resource variability at some timescales and change or augment it at others (Fleming and Clarke 2005). Moreover, the way that glaciers modify river runoff responses to intermediate-term climate variability, such as ENSO or PDO, will change as glaciers retreat under longer-term global anthropogenic climate change (Fleming et al 2016)." These references are admittedly drawn mostly from Western Canada, which is the region I'm most familiar with when it comes to glaciers, climate, and water, but they are sound choices (and, frankly, contributions from scientists in this region seem to be under-represented in this IPCC SOD, at least around glaciers and climate change). The full citations are: Schnorbus et al, 2014, Impacts of climate change in three hydrologic regimes in British Columbia, Canada, Hydrological Processes, 28, 1170-1189; Dahlke et al, 2012, Contrasting trends in floods for two sub-arctic catchments in northern Sweden – does glacier presence matter? Hydrology and Earth System Sciences, 16, 2123–2141; Fleming and Clarke, 2005, Attenuation of high-frequency interannual streamflow variability by watershed glacial cover, ASCE Journal of Hydraulic Engineering, 131, 615-618; Jost et al., 2012, Quantifying the contribution of glacier runoff to streamflow in the upper Columbia River Basin, Canada, Hydrology and Earth System Sciences, 16, 849-860; Fleming and Clarke,	Accepted. We have now greatly changed the text in this section.
88695	67	3	67	3	Deconto and Pollard, 2016. The correct one is "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
95961	67	3	67	5	Are you saying that 15 and 20 m occur 1000 years after the beginning of the simulation? Please clarify [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There was a mistake in these figures that we are glad was pointed out. We have now corrected the values.
22613	67	4	67	4	Are you sure that RCP4.5 really peaked at 15m after 1000 years? 5m seems more likely? [Peter Thorne, Ireland]	see response to comment #95961
69661	67	10	67	10	Suggest changing 'Numerical modeling' to 'One model study' [Nicholas Golledge, New Zealand]	Noted. Text has now been revised and more clearly attributes this finding to a single study.
69663	67	11	67	11	clarify that this study presents results from the end of a 10,000 year run - 'multi-millenial' therefore isn't incorrect, but it might be better to be precise. [Nicholas Golledge, New Zealand]	Accepted. Text revised as suggested.
14779	67	12	67	13	Perhaps more imporantly (and in context of previous discussions, page 64-65), these simulations likely to not include accurate representation of relevant ocean processes, nor, coupled interactions and feedbacks that will be dominant players over this timescale (Fyke at al., 2018, Rev. Geo). [Jeremy Fyke, Canada]	Noted. We have clarified the text in this section to better reflect the uncertainties remaining with respect to ice / climate processes.
88697	67	13	67	13	Deconto and Pollard, 2016. The correct one is "DeConto". [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
14781	67	16	67	16	"due predominantly to the high ice shelf melting" - remove 'we' [Jeremy Fyke, Canada]	Accepted.
67309	67	16	67	16	Personal language should be avoided: Better: There is very low confidence … [Regine Hock, United States of America]	Accepted. Text now substantially revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77845	67	16	67	17	Perhaps it would be better to phrase this in the way you did on the previous page, that you have very low confidence in *projecting* the magnitude. It would be good to have a positive statement, again like on the previous page, about the likelihood of GMSLR on multi-millennial timescales under a warmer climate being larger than by 2200, say, if you think that's justified. As it stands, it appears to suggest that GMSLR might not rise much more on long timescales, which isn't consistent with all the literature you've just cited. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	see response to comment #69665
69665	67	16	67	17	I think 'very low confidence' is too negative. We have a pretty good idea from geological evidence and models that it will be multiple metres under 'much warmer than present conditions'. I think this could be 'low confidence' in terms of magnitude, but 'high confidence' in terms of the likelihood of it happening. [Nicholas Golledge, New Zealand]	Accepted. Text has been revised to clarify the message here and the areas where we have high and low confidence.
77847	67	19	67	20	This sentence seems out of place, because it reverts to the 21st century, in the middle of a discussion of the further future. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	See response to comment #67311
67311	67	19	67	20	This statement is odd. What does consistency of satellite obs that measure the present and past have to do with confidence in future projections. The sat obs don't project the future. [Regine Hock, United States of America]	Accepted. This was intended to be a statement about confidence in models through evaluation with observations, which is now substantially written (Section 9.4.2.2).
62343	67	19	67	40	I suggest adding a reference to Robel et al. (2019): https://doi.org/10.1073/pnas.1904822116. That study showed that ice sheet instabilities are amplifiers of uncertainty so that there will always be larger uncertainties in projections during periods of time that the ice sheet is going through any kind of instability (MISI or MICI). In other words, the uncertainties associated with MISI will always be larger than at other times, no matter how much improvement is made in representing processes. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This reference has now been included.
46525	67	19	67	40	This is another potential place where the impacts of internal climate variability on marine ice sheet evolution could be mentioned (at least in passing in the sense that it's another source of uncertainty that's only now starting to be explored), including the references noted above in line 31. [Stephen Price, United States of America]	Accepted. Internal climate variability has been added.
14783	67	19	67	40	This paragraph on 21st century WAIS response should go before the previous discussion on multimillenial response. It should go with other 21st century projection information. [Jeremy Fyke, Canada]	Taken into account. The section has been rewritten and reorganized.
15845	67	19	67	40	Similar comment about MISI as above. For instance, Gudmundsson et al (2019) doi: 10.1029/2019GL085027 shows that there is no need to invoke MISI to explain the observed mass loss from Amundsen Sea sector. [Olga Sergienko, United States of America]	Accepted. This reference has now been included.
30651	67	21	67	22	The reference to that number (3.3m) from Bamber et al (2009) was already given in AR4. It uses a simple method of identifying grid cells below sea level on negative bed slopes and using this as a limit to infer the effect of MISI. However, in order to fully capture the effect of MISI, all dynamical effects need to be taken into account, which is possible using ice sheet models. At the time of the Bamber et al publication, ice sheet models were not capable of simulating MISI appropriately. More than 10 years later, improvements in ice sheet models allows now to do so. MISI occurs retrograde bed slopes in absence of ice shelf buttressing. Sun et al. (in review) provide such a multi-model experiment in which all ice shelves are removed and kept removed and the modelled ice sheet evolves through MISI. Most participating models effectively simulate a collapse of the WAIS. Based on the data in Table 3 of that paper and considering basins (1, 12-17) that roughly correspond to the WAIS domain in Bamber et al., we obtain a SLR contribution of 3.795 m (STD=2.13 m). The range of this value is clearly larger than what was estimated by Bamber et al (2009). At this point, the paper is under review, but given the importance of MISI and that the number of more than a decade ago continues to be referenced as the sole estimate of what MISI does, we could potentially add a more explicit section in the revision on the WAIS loss due to MISI. To anticipate this, we recalculated this for the WAIS area as in Bamber et al (2009). Excluding models that do not lead to a full collapse of WAIS over the time span considered, this gives a mean value of 3.67 m and a range of 2.86-5.08 m. Including all models, this gives a mean of 3.16 m (range: 1.91-5.08 m). [Frank Pattyn, Belgium]	Accepted. The new version cites the new numbers of that paper.
106729	67	22	67	22	marine sector -> marine sectors (if the intended meaning is preserved) [Kevin Bulthuis, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
106731	67	23	67	23	is susceptible to MISI: This expression sounds a bit odd to me. [Kevin Bulthuis, United States of America]	see response to comment #106727

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106733	67	23	67	24	I would add Pattyn et al. (2018) ("The Greenland and Antarctic ice sheets under 1.5°C global warming", Nature Climate Change) and Bulthuis et al. (2019) ("Uncertainty quantification of the multi-centennial response of the Antarctic ice sheet to climate change, The Cryosphere") in the references. [Kevin Bulthuis, United States of America]	Taken into account. AIS thresholds substantially rewritten and Bulthuis et al. (2019) is cited. Review by Pattyn et al. is not included.
69667	67	26	67	26	in estimates of this threshold' [Nicholas Golledge, New Zealand]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
69669	67	26	67	26	delete 'As discussed above' [Nicholas Golledge, New Zealand]	Accepted. Text now substantially revised.
14589	67	26	67	32	This is a very long sentence. Break into a couple of shorter sentences? [Roshanka Ranasinghe, Netherlands]	Accepted. Text substantially revised.
30653	67	28	67	28	whether MISI would continue if basal melting decreased'. This is very awkward, beacause the definition of MISI is that (unstoppable) mass loss continues independent of the forcing, and therefore ice loss does not scale linearly with the applied melt. If basal melting decreases and grounding line retreat slows down, then it is not a MISI in the first place. Therefore, it is more appropriate to write: 'whether grounding line retreat (or loss of grounded ice) would continue if basal melting decreased'. [Frank Pattyn, Belgium]	Taken into account. This has been rephrased.
115173	67	29	67	29	suggest changing to "on multi-century to millennial timescales" to be consistent with the two papers referenced. Note that Larour et al. simulate only the next 500 years, and the retreat is ongoing at the end of their simulations. Depending on the meaning of stabilized (full stop and re-advance? beginning to slow relative to a simulation that does not include these effects?) that reference may not be appropriate for this sentence, or the sentence could be reworded. [Natalya Gomez, Canada]	Accepted. Now cites SROCC ("multi-century and longer time scales" and also refers to WAIS decadal timescales (Section 9.4.2.4).
106735	67	29	67	29	millinial timescales -> maybe centennial-to-millenial timescales (feedbacks on centennial timescales are also important) [Kevin Bulthuis, United States of America]	see response to comment #115173
129495	67	29			Larour et al. (2019) strongly hints at large negative feeback over 300 years, not millennial time scales, with some negative feedback starting at 2150. Citation: E. Larour, J. Schiermeier, E. Rignot, H. Seroussi, and M. Morlighem, Sensitivity Analysis of Pine Island Glacier ice flow using ISSM and DAKOTA, J. Geophys. Res., 117, F02009, doi:10.1029/2011JF002146. [Trigg Talley, United States of America]	see response to comment #115173
115175	67	32	67	40	May need to include DeConto et al., Nature, in review. [Natalya Gomez, Canada]	Accepted. Cited in sections 9.4 and 9.6.
107333	67	32			Might be worth linking this discussion to SR1.5 [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thresholds discussion substantially rewritten, building more clearly on SR1.5 (now Section 9.4.2.6).
33485	67	32			Put dot at the end of the sentence in black. “al., submitted).”. [Guiomar Rotllant, Spain]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
16411	67	34	67	34	Weird referencing going on here [Julian Mak, China]	Editorial - copyedit to be completed prior to publication
49997	67	34	67	34	DeConto and Pollard 2016 is mistakenly cited 3 times in a row here. [Daniel Gilford, United States of America]	Editorial - copyedit to be completed prior to publication
61755	67	34	67	35	(Deconto and Pollard, 2016) is listed 3 times although always the same reference [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
33487	67	34	67	35	Change: “…MICI (Deconto and Pollard, 2016)(Deconto and Pollard, 2016)(Deconto and Pollard, 2016)(Deconto and Pollard, 2016; Gasson et al., 2016b) reach…” by “…MICI (Deconto and Pollard, 2016; Deconto and Pollard, 2016; Deconto and Pollard, 2016; Deconto and Pollard, 2016; Gasson et al., 2016b) reach…”. [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
106737	67	34	67	35	Multiple citations of DeConto and Pollard (2016). [Kevin Bulthuis, United States of America]	Editorial - copyedit to be completed prior to publication
106739	67	34	67	35	0.30-1.88 m: Are these projections projections from DeConto and Pollard (2016) or Edwards et al. (2019) [Kevin Bulthuis, United States of America]	Accepted. Text substantially revised and sources clarified.
106741	67	34	67	35	Projections from Bulthuis et al. (2019) ("Uncertainty quantification of the multi-centennial response of the Antarctic ice sheet to climate change, The Cryosphere")might also be considered. [Kevin Bulthuis, United States of America]	Accepted. Study now cited extensively in 9.4.2
32153	67	34			Delete repeated citations [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
88699	67	35	67	35	The references Edwards et al., 2019a and Edwards et al., 2019b are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
106743	67	35	67	35	Is the reference Gasson et al. (2016) appropriate here? [Kevin Bulthuis, United States of America]	Accepted. Citation removed (now Section 9.4.2.6).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110729	67	37	67	40	In order to avoid a “too late” reaction here, it should be stated, that also the speed of self-sustained collapse likely depends on the emission scenario. [Torsten Albrecht, Germany]	Taken into account. Thresholds discussion now substantially rewritten (Section 9.4.2.6) with assessment of timescales (multi-century vs multi-millennial).
74117	67	37			Is it „alternatively“? Even under collapse, melt rates potentially can have an influence on the speed of retreat. It is important to say that emission reductions can still have an influence even after a collapse started. [Matthias Mengel, Germany]	Accepted. This part of the sentence has been rephrased.
115487	67	39	67	39	Strong message regarding the long-term commitment to loss of WAIS. Great! [Robert DeConto, United States of America]	Noted with thanks.
69671	67	39	67	39	statement regarding regrowth needs a supporting citation [Nicholas Golledge, New Zealand]	Accepted. Garbe et al 2020 has been added.
107335	67	40			needs a summary state saying what the projection is and what confidence exists (eg increased confidence if number of independent studies agree). This should try to bring together all of the cited studies, in paricular ISMIP6 and LARMIP and explain why any study outside of the projected range has not been included. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have added, extended and clarified assessment statements throughout Sections 9.4.2.5 and 9.4.2.6. The assessed projections at 2100 are a combination of ISMIP6 and LARMIP, with other low confidence projections (MICI and SEJ) included in sensitivity tests (9.6).
74119	67	40			replace „those timescales“ by „shorter timescales“ or similar. [Matthias Mengel, Germany]	Taken into account. This has been rephrased.
289	67	43	72	35	What about our new observations of global glacier velocity? No trends to report? No contributions to models of mass balance? I'm thinking Gardner and Scambos work. [THOMAS Wagner, United States of America]	noted, observed velocity trends not discussed in Ch9 due to space limitation, only the mass loss is policy relevant
81461	67	43			Seems that the entire FOD section 9.5.2.6 on abrupt changes etc. had to og out. This would be a pitty. Or are these things now collected in a different chapter? In FOD it was too long an extensive, but some hints to processes that can lead to abrupt and possible irreversible changes might be good. See my comments on glacier dynamics. [Andreas Kääb, Norway]	Noted, FOD section 9.5.2.6 deleted due to space limitation, some discussion of abrupt glacier changes in Ch12
40761	67	45			section 9.5.1: improvements since latest report not necessarily clear [TSU WGI, France]	accepted, text revised and relation to SROCC clarified
88023	67	45			There are several occasions when only one or few papers are cited while many more would be avilable telling the same content. It would be useful to use more often the "e.g.". [Georg Kaser, Austria]	accepted, text revised
88027	67	45			In AR5 we had been asked many times by reviewers "what is 'w.e.', who are 'we'?" which led us to only use the SI conform "kg m^-2". I would suggest to change this also in AR6 accordingly. Community specific units should be replaced by SI units as far as possible for the wider audience. [Georg Kaser, Austria]	accepted, units coordinated to SI units
3373	67	47	67	47	For the thermal regime, could also refer to the study by Bohleber et al. (2018, Climate of the past, doi: 10.5194/cp-14-21-2018), which nicely illustrates the change in englacial temperature for Colle Gnifetti, 'the only non-temperate glacier to offer climate records dating back at least 1000 years' in the European Alps [Harry Zekollari, Belgium]	not applicable, section no longer included
99713	67	47	67	47	what's the significance of "thermal regime" [Peter Clark, United States of America]	accepted, text revised and no longer contains "thermal regime"
67313	67	47	67	47	A first sentence that contains more references than text is hard to read and not the greatest start of a new section. Perhaps references can be omitted altogether here since this is a very basic fact and the choice of references, esp for the glacier mass seems very random. There is probably thousands of references making this point. Same holds for next sentence. [Regine Hock, United States of America]	accepted, text revised, section no longer included
3095	67	47	67	48	For "thermal regime", at least a second, newer reference should be provided as well [Daniel Farinotti, Switzerland]	not applicable, section no longer included
11385	67	47	67	48	Unclear why these three references were chosen. Why not use a standard textbook to support this trivial statement? [Jacob Clement Yde, Norway]	not applicable, section no longer included
26495	67	47	67	48	Is it relevant to mention "thermal regime" here? It is not really a topic that is discussed in the remainder of section 9.5.1. In case it is removed, the references could be moved to the end of the sentence, which would also make the sentence easier to read. Alternatively, "Glacier mass and thermal regime" could also be replaced with "Glacier mass and length". [Ward van Pelt, Sweden]	accepted, text revised and no longer contains "thermal regime"
16043	67	47	67	48	the results of (Gilbert and Vincent, 2013) have been updated in a recent paper : Vincent C., A. Gilbert, B. Jourdain, L. Piard, P. Ginot, V. Mikhalenko, P. Possenti, E. Le Meur, O. Laarman and D. Six. Strong changes in englacial temperatures despite insignificant changes in ice thickness at Dôme du Goûter glacier (Mont-Blanc area). The Cryosphere, 2020,14,925-934, doi.org/10.5194/tc-14-925-2020 [Christian Vincent, France]	not applicable, section no longer included

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106193	67	47	67	48	In my understanding the sensitivity of glaciers to climate change is expressed by their changes in length as this is the long-term integrator smoothing out fluctuations on time periods shorter than climate. Glacier mass change is the direct reaction to the annual atmospheric conditions and has little to do with climatic trends. In other words, one does not see climate when looking at mass changes. Even continuous observations of mass changes over several years do not provide climate information. The role of glaciers as icons of global climate change comes from the well observable and highly sensitive length chnages. I thus suggest writing the intro differently. [Frank Paul, Switzerland]	not applicable, section no longer included
88021	67	47			You may consider mentioning the regional sections on glaciers in Ch12 [Georg Kaser, Austria]	accepted, reference to 12.4 made in relevant location
11387	67	48	67	49	The sentence "Glaciers are …" is superfluous and repeats the previous sentence. Delete this sentence. [Jacob Clement Yde, Norway]	accepted, text revised, section no longer included
3375	67	49	67	49	Glaciers as symbols of climate change: agree. One of the main reasons why is because they react so quickly (i.e. one can see the changes over the years). This time aspect may be worth specifying. [Harry Zekollari, Belgium]	accepted, text revised, section no longer included
61757	67	49	67	49	it says "section(Table 9.3)" I would delete the table 9.1 and replace by 9.5.1 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised, section no longer included
67315	67	49	67	49	avoid 'We' [Regine Hock, United States of America]	accepted, text revised
2873	67	49	67	50	I would rephrase as: "In this section, we consider ice masses listed in Table 9.3 that are located …" [Antoine RABATEL, France]	accepted, text revised, section no longer included
103843	67	49	67	51	Sentence is unclear. Exactly what glaciers are considered here? All ice except the large ice sheets? Maybe rephrase to (if this is what is meant): In this section (Table 9.3), we consider the ice masses outside the large ice sheets on Greenland and Antarctica. This includes those glaciers that are peripheral to the ice sheets. [Philippe Tulkens, Belgium]	accepted, text revised
80873	67	49	67	51	Sentence is unclear. Exactly what glaciers are considered here? All ice except the large ice sheets? Maybe rephrase to (if this is what is meant): In this section (Table 9.3), we consider the ice masses outside the large ice sheets on Greenland and Antarctica. This includes those glaciers that are peripheral to the ice sheets. [Louise Sandberg Sørensen, Denmark]	accepted, text revised
18431	67	49	67	51	I am sorry I did not understand the meaning of this sentence [Olga Solomina, Russian Federation]	noted, text revised
67317	67	51	67	55	seems like a waste of space; best to delete [Regine Hock, United States of America]	accepted, text deleted
62021	67	52	67	55	Recommend that the sections discussed in this text be written in sequential order [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
61759	67	53	67	53	later in this chapter the sea level rise has the abbreviation SLR so should be introduced here and used from now on [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	editorial, draft will undergo professional copy-editing prior to publication
61761	67	55	67	55	add Section e.g. Chapter8(Section 8.3.1.7.1) and may also Section 8.4.1.7.1? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, references to the two suggested section added to ch9.5.1 in relevant locations
61983	68	1	68	2	The line suggests that glaciers affect all river runoff. The sentence should be rewritten to ensure the meaning (glaciers affect river runoff on areas where they occur) is clear. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, section no longer included
61985	68	1	68	2	The paragraph does not flow very well. This is mostly because almost every sentence starts with 'Glaciers'. The paragraph can be shortened, and its flow improved, by using joining works and rewriting the sentences. For example, the last two sentences can be written as 'Glaciers are large freshwater reservoirs that can alleviate the effects of drought, and can directly and ... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, section no longer included
26497	68	1	68	2	I suggest to remove "seasonal to long-term" since time-scales are also mentioned at the end of the sentence. [Ward van Pelt, Sweden]	not applicable, section no longer included
167	68	1	68	5	See the Immerzeel et al. 2020 (Nature) paper that quantifies how mountains and glaciers provide water for downstream catchments. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	not applicable, section no longer included
67331	68	2	68	2	why 2006-2016 and not 2006-2015 as in SROCC and decided for all AR6 reports? [Regine Hock, United States of America]	taken into account, periods harmonized throughout report
106195	68	2	68	2	Citation of Bliss et al.: Is there a need to always write 'e.g.' in the beginning of a citation when there are several other references saying the same? [Frank Paul, Switzerland]	not applicable, section no longer included

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88263	68	2	68	4	The statement refers to an SROCC conclusion but a different reference is cited. If the conclusion is result of SROCC assessment then it isn't necessary to cite references on which the assessment is based. [Sharon Smith, Canada]	not applicable, section no longer included
67319	68	4	68	4	this is sentence says what SROCC concludes, so no other references needed. The conclusion is not based on this one reference. [Regine Hock, United States of America]	not applicable, section no longer included
99949	68	4	68	5	It will probably be better to clarify in 2 or 3 additional sentences the spatiotemporal domain in which the glaciers can alleviate effects of drought. The relief from drought might not be instant and at large spatial scale, and some additional information on this can be helpful for a non-specialist reader. [Bhardwaj Anshuman, Sweden]	not applicable, section no longer included
100039	68	4	68	6	1) The last sentence of this paragraph concerning glacier-related hazards does not seem to belong with the rest of this paragraph. I recommend moving this sentence to the end of the paragraph on pg 67, line 55. 2) the penultimate sentence of this paragraph (line 4-5) would be better positioned as second sentence (line 2). [Forrest Schoessow, United States of America]	not applicable, section no longer included
3377	68	5	68	5	Glaciers can alleviate the effect of droughts: indeed. Three recent high-profile papers that support this and could be considered referring to: Biermans et al. (2019, Nature Sustainability, doi: 10.1038/s41893-019-0305-3), Farinotti et al., (2019, Nature, doi: 10.1038/s41586-019-1740-z) and Immerzeel et al. (2020, Nature, doi: 10.1038/s41586-019-1822-y) [Harry Zekollari, Belgium]	not applicable, section no longer included
62007	68	5	68	6	A couple of examples would be welcome here. How can glaciers cause natural hazards, directly or indirectly? Yes, these things are discussed in Chap. 12, but just a few concrete examples (e.g. flooods, droughts) would be relevant here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, section no longer included
100027	68	5	68	6	Glaciers can cause natural hazards during their retreat as well as surge. I think that "surge" with some appropriate references should also be included in this line. [Lydia Sam, Sweden]	not applicable, section no longer included
61763	68	6	68	6	change reference so Veh et al., 2019a, 2019b [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	editorial, not applicable, references removed
81445	68	6			Veh et al. 2019ab are very specific in terms of hazard type and region. I would rather refer to SROCC Ch2 (Hock et al. 2019) which gives a wide overview and assessment. [Andreas Kääb, Norway]	not applicable, section no longer included
67321	68	9	68	10	not the full response' ??? This is unclear, also odd to say what it is not, instead of what it is. [Regine Hock, United States of America]	taken into account, text revised
93569	68	9	68	11	The wording could be confusing for a non-specialist. I suggest turning the last part of the sentence around to read "and even without further change in temperature they are committed to losing considerable mass in the future", or other formulation. [Miriam Jackson, Norway]	taken into account, text revised
28163	68	11	68	11	Miles et al (submitted) is a new piece of evidence here examining glacier-specific and regional committed mass loss of glaciers in High Mountain Asia [Evan Miles, Switzerland]	noted, the reference not found, perhaps not published before acceptance cut-off
2053	68	11	68	11	I suggest to cite Jouvet&Huss (2019, Jog) instead of Jouvet et al. (2011). This is an update of study cited now and makes the same point based on most recent data sets. (Jouvet, G., and Huss, M. (2019). Future retreat of Great Aletsch Glacier. Journal of Glaciology, 65 (253), 869-872. doi:10.1017/jog.2019.52.) [Matthias Huss, Switzerland]	accepted, reference updated
3379	68	11	68	11	Reference to the modelling work by Jouvet et al. (2011), who model the temporal evolution of the Aletsch glacier. Probably better to update to more recent work by Jouvet and Huss (2019, Journal of Glaciology, doi: 10.1017/ jog.2019.52), who updated the simulations for the Aletsch glacier with the newest climate scenarios for Switzerland (CH2018). [Harry Zekollari, Belgium]	accepted, reference updated
67323	68	11	68	11	air temperature [Regine Hock, United States of America]	accepted, text revised
67911	68	12	68	12	Truessel et al.,2015, J. Glaciol. provides an extreme example of committed change (disappearance of Yakutat Icefield) [Martin Truffer, United States of America]	accepted, reference is Truessel et al 2013 we think, added to assessment
67325	68	12	68	12	Is Zemp et al actually doing such model experiements? Best to stick to those references that actually model this. [Regine Hock, United States of America]	accepted, references revised
67327	68	12	68	12	what is climatic disequlibrium of glaciers? Do you mean mass disquilibrium? [Regine Hock, United States of America]	accepted, text revised
26499	68	12	68	14	This applies to the length and volume of glaciers, rather than the surface mass balance. Maybe that should be mentioned. [Ward van Pelt, Sweden]	taken into account, text revised

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109705	68	12	68	14	Change to "glacier geometries" to reduce potential for misunderstanding by readers, and improve the literature citations: "As glacier geometries respond to change climate, they acts as integrators of the climate forcing (e.g., Clarke et al, 2015; Roe et al., 2017) filtering out the inter-annual variability (Beedle et al., 2009)." The Clarke et al (2015) Nature Geoscience article is already cited elsewhere in the chapter. [Sean Fleming, United States of America]	taken into account, combined with other comments and text revised
69575	68	13	68	14	"filtering out the inter-annual variability (Beedle et al., 2009)." This is a slightly eccentric reference to make the filtering point, and the paper contains an apparent error in assuming each annual glacier length measurement is an independent degree of freedom. For an obervationsl reference, Luthi (JGlac, 2009) and Luthi et al (JGR, 2010) are a nice pair of papers that make this point from both theoretical and obsevational perspectives. The essentional point is obvious though and can be found in anything by Nye or Wertman from the 1960s and 1970s. [gerard Roe, United States of America]	accepted, references revised
61765	68	14	68	14	surface mass balance is later on used as SMB [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	editorial, acronyms are reduced as much as possible
3097	68	14	68	16	A reference is required for this claim. Zekollari et al., 2020 (https://doi.org/10.1029/2019GL085578) could be one. In any case, the publication should be cited somewhere in the next 5 lines or so. [Daniel Farinotti, Switzerland]	accepted, reference added to assessment
16045	68	14	68	16	A reference is missing here at the end of the sentence: "Although the point surface mass balance is a direct and immediate function of accumulation and ablation, the glacier geometry adjusts over a longer time to changed atmospheric conditions". The question relative to the difference between point surface mass balance changes and glacier-wide mass balance changes has been thoroughly studied in recent papers: Vincent, C., Soruco, A., Azam, M. F.,Basantes-Serrano, R., Jackson, M.,Kjøllmoen, B., E. Thibert, P. Wagnon, D. Six, A. Rabatel, A. Ramanathan, E. Berthier, D. Cusicanqui, P. Vincent and A. Mandal (2018). A nonlinear statistical model for extracting a climatic signal from glacier mass balance measurements. Journal of Geophysical Research: Earth Surface, 123. https://doi. org/10.1029/2018JF004702., and in : Vincent C., A. Fischer, C. Mayer, A. Bauder, S. P. Galos, M. Funk, E. Thibert, D. Six, L. Braun, M. Huss. 2017. Common climatic signal from glaciers in the European Alps over the last 50 years. Geophys. Res. Lett., 44, doi:10.1002/ 2016GL072094 [Christian Vincent, France]	taken into account, text revised and references to post-SROCC publication added (Zekollari et al 2020)
67913	68	16	68	16	Work by Will Harrison (e.g. Harrison et al., J. Glaciol., 2003) also shows that this time scale can be negative, which indicates an unstable response. This is the case when the mass balance - elevation feedback dominates. [Martin Truffer, United States of America]	noted, text revised but does not include negative time scale and unstable response
99951	68	16	68	17	There are recent papers highlighting the role of hypsometry too in this regard. That can be considered for inclusion here with relevant references. [Bhardwaj Anshuman, Sweden]	noted, text revised in response to several comments
69577	68	16	68	17	"This adjustment time is variable from one glacier to another. It depends strongly on the glacier geometry (thickness/vertical extent and slope/inclination) as well as the mass turnover and flow speed (Jóhannesson et al., 1989; Harrison et al., 2001; Lüthi, 2009)." There are a few points here. It is not clear what "vertical extent" means (is it the altitude difference from the head wall to the toe?). Nor is it clear what the intended difference is between slope and inclination. The response time depends only on the characterirtic thickness in the ablation zone and the net mass balance near the terminus (tau= ~H/bt). It does not depend directly or simply on the vertical extent of the glacier, or on the mass turnover or flow speed. Mass turnover scales as H/P, and yields timescales that are much too long. Nye, and Oerlemans created timescales involving flow speed. but to match obsevations Oerlemans has to incorporaate an empirical factor from numerical modeling. See Roe and Baker (JGlac, 2014) for a demonstration that if time is normalized by the oorrect tau (H/bt) the reponse function of differnt sizeed glaciers all align. All of the three studies cited in this quoted passage use a geometric scaling for the response time (i.e., nothing about flow speed). [gerard Roe, United States of America]	accepted, text revised in response to several comments
28165	68	16	68	23	Zekollari et al (2020, https://doi.org/10.1029/2019GL085578) clearly finds that the e-folding response time is independent of glacier size [Evan Miles, Switzerland]	noted, reference added to assessment, text revised
61767	68	18	68	18	Resource Johannesson et al 1989 is old and should probably be replaced. How about Vincent et al 2004 and Oerlemanns 2005? These are more recent studies and would also present field data, rather than just modelled results (Vincent, C., G. Kappenberger, F. Valla, A. Bauder, M. Funk, and E. Le Meur (2004), Ice ablation as evidence of climate change in the Alps over the 20th century, J. Geophys. Res., 109, D10104, doi:10.1029/2003JD003857 and Oerlemans, J.H.. (2005). Extracting a Climate Signal from 169 Glacier Records. Science (New York, N.Y.). 308. 675-7. 10.1126/science.1107046.) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, the reference used is the one that defines response time, suggested reference are pre AR5, not added to assessment

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96983	68	18	68	18	We suggest to also cite Ekollari et al. (2020, doi: 10.1029/2019GL085578) here. [Nicole Wilke, Germany]	accepted, reference added to assessment
3381	68	18	68	20	Description of the response time (range of adjustment time) of glaciers and how this depends on glacier size and steepness. Modelling evidence suggests/confirms that the mass balance gradient also plays an important role. Could consider refering to recent study in which the response time of all glaciers in the European Alps is modelled with a physically-based model (Zekollari et al., 2020, Geophysical Research Letters, doi: 10.1029/2019GL085578) [Harry Zekollari, Belgium]	accepted, text revised in response to several comments, reference to suggested publication added and "surface mass balance and it's gradient" added to description
24125	68	18	68	20	It is good to point to the original papers. They show that our knowledge and understanding has been robust for many years already. This referencing, however, should be improved. Historically, the papers by Johannesson et al. (1989; basc principles), Haeberli and Hoelzle (1995; on behalf of UNEP; application to large samples based on inventory information) and Oerlemans (2007; based on numerical flow models) were scientifically sound pioneering publications. The recent paper by Zekollari et al. (2020) correctly summarizes this and provides a modern analysis based on transient numerical model calculations for a larger glacier sample. The Lüthi papers have never been well perceived in the scientific literature (on average < 5 citations per year in Google scholar) as the corresponding calculations heavily rely on misleading self-correlation statistics. I recommend to reformulate the statement as follows: "It depends strongly on the glacier geometry (thickness/vertical extent and slope/inclination) as well as the mass turnover and flow speed (Jóhannesson et al., 1989; Haeberli and Hoelzle, 1995, Harrison et al., 2001; Oerlemans, 2007). The range of adjustment time is from a few years for smaller and steeper glaciers, to decades and up to hundreds of years for larger and low-pitched glaciers (Haeberli and Hoelzle 1995, Beedle et al., 2009; Rabatel et al., 2013; Zekollari et al., 2020). Additional References: (1) Haeberli, W., & Hoelzle, M. (1995): Application of inventory data for estimating characteristics of and regional climate-change effects on mountain glaciers: A pilot study with the European Alps. Annals of Glaciology, 21, 206–212. https://doi.org/10.3189/ S0260305500015834. (2) Oerlemans, J. (2007): Estimating response times of Vadret da Morteratsch, Vadret da Palü, Briksdalsbreen and Nigardsbreen from their length records. Journal of Glaciology, 53(182), 357–362. https://doi.org/10.3189/002214307783258387. (3) Zekollari, H., Huss, M., & Farinotti, D. (2020): On the imbalance and response time of glaciers in the European Alps. Geophysical Research Letters, 47, e2019GL085578. https://doi.org/ 10.1029/2019GL085578 [Wilfried Haeberli, Switzerland]	taken into account, suggested references added to assessment but only post-SROCC reference (Zekollari et al 2020) added to text
2055	68	19	68	19	I suggest to cite most recent research on this topic: Zekollari et al. (2020, GRL) have provided the first regional-scale inventory of glacier response times, confirming the statements made here (Zekollari, H., Huss, M., and Farinotti, D. (2020). On the imbalance and response time of glaciers in the European Alps. Geophysical Research Letters, doi:10.1029/2019GL085578) [Matthias Huss, Switzerland]	accepted, reference added to assessment
62023	68	19	68	20	…to decades and up to hundreds of years… recommend using the same language to discuss decades and hundreds of year, so that text reads either "…to tens of years and up to hundreds of years…" or "…to decades and up to centuries…" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
62009	68	20	68	20	For the non specialist in glacier science: is this common to use the phrase «low-pitched» for a glacier? I am guessing that we are talking about topography/slope here, so it would represent 'gentle-slope' (valley bottom), larger glaciers, right? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
61743	68	20	68	22	Some real-world exampels contradict the universal statement that all glaciers become more sensitive to climate change as they retreat. The study cited uses a self-proclaimed 'simplest model' that is not meant to represent complex real-world situations. See Sakai & Fujita (2017) and Huss & Fishcer (2016) for examples of mountain glaciers with decreased sensitivity given climate change in the Himalaya and Swiss Alps (respectively). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, references added to assessment, text revised
26501	68	20	68	22	This could be shortened to: "Glacier retreat accelerates in a changing climate due to feedback mechanisms." [Ward van Pelt, Sweden]	taken into account, text revised in response to several comments and this statement deleted
69579	68	20	68	22	As the glaciers retreat and become smaller, their sensitivity to climate change intensifies due to feedback mechanisms (Harrison, 2013)." This is not correct. As glaciers retreat, most of them retreat onto steeper slopes. This actually makes them less sensitive to climate change.A larger temperature change is required to change the ablation area.The Harrison feedbacks are important only for glaciers on shallow, constant slopes. [gerard Roe, United States of America]	taken into account, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106197	68	22	68	22	The increased sensitivity likely only applies to some of the shrinking glaciers (e.g. for ice caps or disintegrating glaciers). Glaciers loosing the lower parts of their flat tongues but keeping a large elevation range or those retreating into cirques with good radiation protection and maybe additional debris cover will likely *decrease* their sensitivity to climate change considerably. I suggest adding that this possibility also exists. [Frank Paul, Switzerland]	noted, text revised in response to several comments
96985	68	22	68	22	We are not sure if the confidence is really "very high" here, if the statement is made unconditional of the rate of warming in the coming decades. I.e., if the rate of warming is reduced substantially, glaciers may be able to "catch up", thus effectively reducing the disequilibrium. [Nicole Wilke, Germany]	accepted, text revised and references added to Marzeion et al that the disequilibrium reduces and then disappears around year 2070, for clarification
3383	68	22	68	23	Mention that climatic disequibilrium will persist in a warming climate. Could potentially add that the disequilibrium will be larger for scenarios with an important warming: e.g. under RCP2.6, for many regions on Earth the glaciers will be relatively close to equilibrium by the end of the 21st century (of course the glaciers will be considerably smaller than today). This is clear from the recent simulations from GlacierMIP2 (Marzeion et al., under review). [Harry Zekollari, Belgium]	taken into account, GlacierMIP2 discussed in projection section
69581	68	22	68	23	There is therefore very high confidence that climatic disequilibrium of glaciers will persist as warming continues." I tihnk this is an unclear statement. First, disequilibrium has been mentioned right at the start of the paragraph, and then the topic has drifted a bit. So it is not clear what reasons the "therefore" connects to. Second, it is certain that there will always be disequiibrium in a warming climate. Third, for most glaciers, the degree of disequilibrium will decrease in the future. As glaciers retreat to steeper slopes, they thin, and their response time decreases. The degree of disequillbrium scales directly with the repsonse time (i.e., how far behind the climate is the glacier lagging). The timescale and the disequilibrium will decrease for mountain valley glaciers. Not all glaciers retreat to steep slopes, but most do. [gerard Roe, United States of America]	accepted, text revised
40757	68	26			section 9.5.1.1: mass loss sometime expressed as a negative number sometimes positive e.g. p69, L3 vs L21 [TSU WGI, France]	accepted, consistency improved and mass loss now expressed as positive number
40759	68	26			section 9.5.1.1: units are not always consistent: sometimes Gt/yr only and sometimes m w.e. yr, sometimes both. [TSU WGI, France]	Accepted, mass loss expressed with units Gt/yr in text in figure 9.20 as rate for comparison between regions
18433	68	28	68	28	"are" instead of " is" [Olga Solomina, Russian Federation]	Accepted, text revised
103847	68	29	68	29	It could be relevant to mention here that the number of regularly observed glaciers (in the field) is very low (0.25 % of glaciers) (Zemp et al., 2015) [Philippe Tulkens, Belgium]	Noted, text is revised, this is not directly stated as the new geodetic results from Hugonnet et al 2021 estimate mass loss of all glaciers in RGI v6.0
80875	68	29	68	29	It could be relevant to mention here that the number of regularly observed glaciers (in the field) is very low (0.25 % of glaciers) (Zemp et al., 2015) [Louise Sandberg Sørensen, Denmark]	Noted, text is revised, this is not directly stated as the new geodetic results from Hugonnet et al 2021 estimate mass loss of all glaciers in RGI v6.0
67329	68	30	68	30	This does not seem correct. The data does not only come from the GTN-G. Eg. Wouters et al., 2019 cited later is based on GRACE and not GTN-G, also other studies may have used additional data that are not in GTN-G. Perhaps more informative is to say what measurement principles / type of data the observed changes are based on. [Regine Hock, United States of America]	Accepted, text revised
61769	68	31	68	31	figure 9.1--> Figure 9.1b [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial, the final draft will be professionally copy-edited prior to publication
62071	68	31	68	31	Capitlise figure [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial, the final draft will be professionally copy-edited prior to publication
106199	68	31	68	31	The orange numbers on Fig. 9.1 are very difficult to see and boundaries between regions are not really shown. I suggest testing other colours (yellow?) for the numbers of the RGI/GTN-G first order regions. [Frank Paul, Switzerland]	Noted. This figure has many purposes and so a balancing of glaciers, ice sheets, sea ice, oceans requires some compromises. The colour scheme has been improved since SOD.
88701	68	32	68	32	The references RGI Consortium, 2017a and RGI Consortium, 2017b are the same. [Rosemary Vieira, Brazil]	Editorial, the final draft will be professionally copy-edited prior to publication
100029	68	32	68	46	I feel that there should be some mention of inventroy related inherent uncertainities here and how they can affect the model projections. The glacier area and numbers vary from one global or regional inventory to another based on the methods of compilation and used source data. Providing some uncertainty statistics here might be useful. [Lydia Sam, Sweden]	Noted, text revised and stated that inventory is improving
81447	68	32			RGI might need some explanation? That it is a global, continously updated and improved inventory of glacier areas (outlines) or so? Mainly based on satellite data. [Andreas Kääb, Norway]	Accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62025	68	34	68	35	Is any of the 3% decrease due to glacier ice loss since the AR5, or is this all attributed to just change in what is categorized as glacier? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Area changes are due to improvements of glacier inventories in the different regions. These improvements are not intended to be used to track glacier changes. Text revised to state this.
3385	68	35	68	35	Decrease of the total glacier area between RGI2.0 and RGI6.0 is described. Without context, one may think that this is related to a change in glacier area over time. But in reality both datasets (largely) refer to the same time period. The reduction in glacier area mainly results from the fact that the spatial resolution has increased (i.e. more detail in RGI6.0 vs. RGI2.0). Would probably be good to consider clarifying this. [Harry Zekollari, Belgium]	Taken into account. Area changes are due to improvements of glacier inventories in the different regions. These improvements are not intended to be used to track glacier changes. Text revised to state this.
61771	68	35	68	35	why has inventoried glaciers decreased? due to uncertain areas or have they disappeared? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Area changes are due to improvements of glacier inventories in the different regions. These improvements are not intended to be used to track glacier changes. Text revised to state this.
96987	68	36	68	40	We would put less emphasis on the number of glaciers, or at least point out that the number says very little on the state of glaciers (since, e.g., a melting large glacier may decay into several small ones, increasing the number of glaciers). Also, the number is very uncertain, if you base your uncertainty on the lower size limit of glaciers to be included in the RGI. Because of this, we would actually put the confidence on the number of glaciers as low - or you could say something about the number of glacier above a given size (and probably, larger than the lower limit of the RGI), and then give a higher confidence for that number. [Nicole Wilke, Germany]	Accepted, text revised and number of glaciers not discussed only the glacier covered area
26405	68	37	68	37	(706±30) 103 km2 -> (706±30) x 103 km2 [María Santolaria-Otín, France]	Editorial, the final draft will be professionally copy-edited prior to publication
62027	68	37	68	37	Formatting of numbers as "(706±30) odd. Flagging this in the event the authors had intended to include an alternate unit as elsewhere in text where paratheses are used [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial, the final draft will be professionally copy-edited prior to publication
81449	68	37			remove brakets around (706+-30) [Andreas Kääb, Norway]	Editorial, the final draft will be professionally copy-edited prior to publication
103849	68	39	68	39	and wrongly mapped seasonal snow [Philippe Tulkens, Belgium]	not applicable, text removed
80877	68	39	68	39	and wrongly mapped seasonal snow [Louise Sandberg Sørensen, Denmark]	not applicable, text removed
3387	68	40	68	40	Source of uncertainty in RGI products. A nice description is given here. Another important source of uncertainty / source of debate is the distinction between glaciers and ice sheets for Greenland and Antarctica: e.g. should an ice cap that is losely connected to an ice sheet be considered as a 'glacier' or as a 'part of the ice sheet'? This is a (quite important?) source of uncertainty when counting the worldwide number of glaciers and the glacier area. [Harry Zekollari, Belgium]	noted, text revised and reduced, not applicable in current text
2057	68	40	68	42	In my opinion, it is not correct to state that the global ice volume estimate is "based" on GlaThiDa. GlaThiDa still provides information only for a few glaciers. It has been used to calibrate the global ice thickness models, where possible. The estimate is based on ice thickness modelling that makes use of global terrain models and corresponding considerations on ice flux. [Matthias Huss, Switzerland]	Accepted, text revised
61773	68	41	68	41	What does GlaThiDa 3.0 mean? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	noted, text revised to clarify
3389	68	43	68	43	The consensus estimate of the total glacier volume based on RGI v6.0 is given here. Perfect, but likely important also to stress that the RGI refers to the early 21st century is most cases (e.g. period around the year 2000), and that the total glacier volume thus represents glaciers in the early 21st century (especially as for many the DEM from SRTM is used, representing the glacier topography around 2000 if I'm not mistaken). Given the rapid mass global loss occuring to date (e.g. Zemp et al., 2019, Nature, doi: 10.1038/s41586-019-1071-0; Wouters et al., 2019, Frontiers in Earth Science, doi: 10.3389/feart.2019.00096), the real present-day volume (for the year 2020) is probably already lower: I would guess in the order of a few percent - could be calculated based on the studies by Zemp et al. (2019) and/or Wouters et al. (2019) that I mentioned earlier in this comment. [Harry Zekollari, Belgium]	Noted, table 9.A.2. states that the area and volume are for year 2000
106201	68	43	68	43	I suggest providing the total mass also in Gt. [Frank Paul, Switzerland]	Rejected, the observed and projected mass loss are presented mm SLE so for consistency the total mass is also presented in mm SLE
88025	68	43	68	46	the total volume is 15% higher but the SLR-efficient volume is 324 ....? Needs rewording. Maybe "total SLR potential is 324 ..." [Georg Kaser, Austria]	accepted, text revised

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2059	68	44	68	44	The term "potential sea-level equivalent" does not make sense to me. It should be "potential sea-level rise", as obviously the stated number accounts for the glacier ice volume presently below sea level. If the number is referred to as "volume" however, the number for sea-level equivalent should also include the volume below sea level, and be termed "sea-level equivalent" (without potential). [Matthias Huss, Switzerland]	Taken into account, values in table 9.A.2 are the SLE values from Farinotti et al 2019, this is now clarified in table caption
61775	68	44	68	44	confidence level missing but should be included after sea level equivalent (SLE) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
62073	68	44	68	44	This sentence doesn't currently make sense - do you mean: 'This estimate of global glacier SLE is about 21% lower than that given in AR5" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text revised
62075	68	44	68	44	The use of 'about' is not very scientific, maybe include the values from AR5 as a comparision here. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text revised
62029	68	44	68	46	Is any of the 21% lower SLE estimate in AR6 compared to the AR5 because some of the global glacier mass declined with warming since 2013, or is this estimate difference entirely because the 15% of glacier volume below SL? It would be helpful to specify here if any of these differences between estimates from AR5 and AR6 are because of physical differences in glacier size since the AR5, or just due to updated classification of glaciers and relationship to sea level. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text clarified, RGI is the state at year 2000 so the difference is because of improvement in the inventory
22615	68	44	68	46	Presumably largely because and not entirely because? Where does the balance of the difference arise from? Presumably partly melting that has accrued in the past several years (this should be quantified and added to this statement I think) but that can't be all of it? [Peter Thorne, Ireland]	Noted. Text revised to clarify, the difference is because of improvement in inventory for year 2000
3391	68	45	68	46	Total glacier volume has decreased since AR5. A large part of explanation is indeed linked to the fact that now (consensus estimate by Farinotti et al., 2019, Nature Geoscience) an explicit distinction has been made between the ice that is located above and under sea level (the latter will not contribute to SLR when it disappears). But this is not the the explanation for the lower volume estimate. Compared to the estimate available at the time of AR5 (when e.g. Huss and Farinotti (2012, Journal of Geophysical Research: Earth Surface, doi: 10.1029/2012JF002523) was the only study to model the distributed ice thickness for every individual glacier on Earth), the new estimate is based on results from several groups, of which several suggest a lower ice thickness compared to previous studies (i.e. independent of the fraction above/below sea level): see e.g. figure 3 in Farinotti et al. (2019). [Harry Zekollari, Belgium]	Taken into account. Text revised and clarified. Table 9.A.2 includes glacier mass estimated at 2000 by Farinotti, mass change rate estimated by Hugonnet et al 2021 in period 2000-2019 and projected mass loss for period 2015-2100
62077	68	45	68	46	If 15% of the difference between AR5 and new estimate can be explained by the total glacier volume being situated below sea level, what about the other 6% (as the new estimate is 21% lower than AR5). It would be useful to add a line to say where the rest of the difference may be from? Is it because 6% of the glacier mass has been lost from AR5 to present? If so, this should be included. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised and clarified
81451	68	45			what about: "… AR5, mainly because …"? [Andreas Kääb, Norway]	Noted, text revised due to several review comments
3393	68	48	68	48	Regional glacier volume changes. Placeholder indicates that additional regional estimates will be added. Will indeed be necessary, as some recent important studies are still missing, such as Brun et al. (2017, Nature Geoscience, doi: 10.1038/NGEO2999), Braun et al. (2019, Nature Climate Change, doi: 10.1038/s41558-018-0375-7) and Shean et al. (2020, Frontiers in Earth Science, doi: 10.3389/feart.2019.00363). On the other hand I was surprised to see a reference to the work by Groh et al. (2019, Geosciences, doi: 10.3390/geosciences9100415), as this study focuses on the ice sheets and not on glaciers (maybe I am missing the point here?), and I do not see this study appear it in the caption of Figure 9.22. [Harry Zekollari, Belgium]	Accepted. Text revised and further new regional studies included, including the suggested studies
88703	68	49	68	49	The references Marzeion et al., 2015a and Marzeion et al., 2015b are the same. [Rosemary Vieira, Brazil]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
9021	68	50	68	50	Fantastic update glaciers and ice caps, showing an acceleration of the loss for all the glaciers combined, should be mentioned here: Continuity of the Mass Loss of the World's Glaciers and Ice Caps From the GRACE and GRACE Follow-On Missions E Ciracì, I Velicogna, S Swenson Geophysical Research Letters 47 (9), e2019GL086926 [Eric Rignot, United States of America]	accepted, suggested reference added to assessment

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82955	68	50	68	51	New publications (Deschamps-Berger et al. 2019, Journal of Glaciology, doi: 10.1017/jog.2018.98; van Pelt et al. 2019, The Cryosphere, https://doi.org/10.5194/tc-13-2259-2019) address mass balance for Svalbard glaciers. This could be considered to be included in discussions, if not already done through glacier overview datasets. [Sebastian Gerland, Norway]	Accepted, both suggested references added to assessment
18113	68	50	68	51	Please consider for Region 16 and 17 Braun et al. 2019 (NCC), for Region 16: Seehaus et al. 2019 (TC) and 2020 (JoG) covering about 90% of the region. For Region 11 a study covering the entire alps were recently accepted/published by Sommer et al. 2020 (Nature Communications) covering more than 95% of the region. For HMA please include Brun et al 2017 and Shean et al. 2020 (Frontiers) [Thorsten Seehaus, Germany]	accepted, suggested reference added to assessment
106203	68	51	68	51	I would not use 'retreat' and 'position' when refering to glacier volume or mass changes. Both terms are usually used for glacier length changes (retreat/advance) and terminus positions. Maybe write 'but a globally consisent trend of glacier decline (or glacier volume loss)' over the past decades' [Frank Paul, Switzerland]	Accepted. Text revised
27649	68	51	68	54	IGE There are large differences of decadal ice losses between regions and therefore, giving a global trend since the LIA is may be true for some regions (e.g., European Alps) but it is not true for some others (e.g., Asia, Canada). If a generalization is needed, it may be better not to provide a general trend since the LIA but to group the regions according to similar trends of ice losses, and to provide a general view, based on these grouped regions. [Eric Brun, France]	Taken into account, text revised and figure 9.21 shows trends for 20th century for all 19 RGI regions
10701	68	51	68	55	The plots in Figure 5.22 start in 1900. Nothing can be deduced about the "Little Ice Age" from these plots. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised
61781	68	52	68	52	Little Ice Age should be LIA as later on used [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
96989	68	52	68	52	The LIA maximum is not seen on Fig. 9.22 with a time axis that starts in 1900. [Nicole Wilke, Germany]	not applicable, text removed
106205	68	52	68	52	Please do not use 'Little Ice Age' (LIA) here as this period is not covered in Fig. 9.22. LIA maximum (terminus) positions have also been reached in the 17th and 18th Century and the last maximum extent is often referred to as 1850 rather than 1900. [Frank Paul, Switzerland]	not applicable, text removed
20565	68	52	68	53	Concerning the units, replacing "a" by "yr" like everywhere else in the document would be easier to read. Also, is it adequate to qualify a loss of 40 cm water equivalent as "moderate"? For representative (???) glacier thickness around 100m, this induces disappearance in well below 25 years. [philippe waldteufel, France]	accepted, text revised and section that latter part of comment refers to deleted
62031	68	52	68	55	Is it possible to be more specific with estimates of ice loss in this sentence? Language is inconsistent and hard to distinguish how "a few tenths of a m" compares to "around 0.4 m" to "moderate to little ice loss" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
3395	68	53	68	53	Reference is made to glacier changes in the second part of the 19th century and values are even mentioned ('few tenths of m w.e. a^-1'). But this is not appearing in Figure 9.22, nor is it clear from which study these numbers originate. Could this be specified? [Harry Zekollari, Belgium]	not applicable, text removed
106207	68	53	68	53	Second half of the 19th century is not possible as the graph is only starting in 1900. [Frank Paul, Switzerland]	accepted, text revised
106209	69	2	69	2	I suggest writing 'the global average *specific* mass loss' [Frank Paul, Switzerland]	not applicable, text removed
61809	69	3	69	3	table 9.3 gives mm SLR not w.e. per year, so where does this number come from? Needs a reference [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
61811	69	4	69	4	change to SLR [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, text revised
61813	69	4	69	4	needs a reference behind 18 m to sea level rise. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
61815	69	5	69	5	change to SLR [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
61989	69	6	69	8	This sentence is ambiguous. It can be rewritten as '…contribution these glaciers to global sea-level rise of 17 to 48 mm between 1901 and 2015…' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
61817	69	7	69	7	change to SLR [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We change to SLR
88029	69	7	69	7	does "these" refer to too small and already disappeared glaciers? Maybe wording can be made clearer. [Georg Kaser, Austria]	Accepted we change "these" by "uncharted"
3397	69	8	69	11	Unprecedented global retreat since second half of the 19th century. Not clear which period the retreat is referring to: the 'ongoing'? May be worth being even more specific to avoid any confusion: e.g. 'global glacier retreat since the beginning of 1990s? / beginning of the 21st century/...?' [Harry Zekollari, Belgium]	Accepted. What is unprecedented in the observational record is the global glacier mass loss rate since the beginning of the 21st century, text revised

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22617	69	8	69	11	This is a dangerous statement and far less nuanced than that made by chapter 2. Unprecedented since when and how? This statement feels like a real potential hostage to fortune without being made far more explicit. It should be ensured it is consistent with the chapter 2 findings. [Peter Thorne, Ireland]	Accepted. Text revised and coordinated with Ch2
32155	69	9	69	10	What exactly is unprecedented? Glacier retreat now, glacier retreat since the second half of the 19th century, that glacier retreat is global? [Anja Wendt, Germany]	Accepted. Text revised
61987	69	10	69	10	The use of the word 'committed' is not suitable here. It assigns a human attribute to an inanimate object. Perhaps replace with a word such as projected or estimated. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This comments is for page 68 not 69. Text is revised
62033	69	13	69	13	Regional anomalies do exist… Recommend specifying what baseline (global trends in ice volume?) these anomalies are relative to so that it reads "Regional anomalies relative to [baseline] do exist" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, text revised
96991	69	13	69	13	It remains unclear what the 'Karakoram anomaly' is, and whether it can be seen in figures or table 9.3. [Nicole Wilke, Germany]	Accepted. We include more comprehensive explanation of what the Karakoram anomaly is based on discussion of Farinotti et al. (2020; https://doi.org/10.1038/s41561-019-0513-5). Text revised
2875	69	13	69	15	You should refer to the recentmost study by Shean et al. (Shean, D. E., Bhushan, S., Montesano, P., Rounce, D. R., Arendt, A., & Osmanoglu, B. (2020). A Systematic, Regional Assessment of High Mountain Asia Glacier Mass Balance. Front. Earth Sci, 7, 363.) [Antoine RABATEL, France]	Accepted. We include the most recent studies regarding the "Karakoram anomaly"
26503	69	13	69	31	It would have made more sense to me to discuss anomalies between regions here rather than anomalies within regions. The latter is less relevant from a global glacier change perspective. [Ward van Pelt, Sweden]	accepted, text revised
28167	69	16	69	16	The rigorous assessment of Shean et al (2020) also seems relevant here. https://doi.org/10.3389/feart.2019.00363 [Evan Miles, Switzerland]	Accepted. We include the findings of Shean et al (2020), text revised
3403	69	16	69	17	When referring to the time period for the Karakoram anomaly (dating back to 1970s), it would probably also be good to mention the study by Wang et al. (2018, Journal of Glaciology, doi: 10.1017/jog.2018.53). [Harry Zekollari, Belgium]	Noted, suggested citation considered, it is pre-SROCC and newer reference cited in revised text
27651	69	16	69	18	IGE: This Karakoram anomaly, very well explained in the SROCC, has combined explanations: atmospheric circulation variability leading to cooler regional conditions in summer (Forsythe et al., 2017) as already stated in this present report, but also enhanced atmospheric moisture leading to increased snowfalls, linked to increasing irrigation in the vicinity (de Kok et al, 2018, 2019). A recent paper (Farinotti et al, 2020) gives a comprehensive state of the art of what we know about this anomaly - ref: de Kok, R. J., Tuinenburg, O. A., Bonekamp, P. N. J., & Immerzeel, W. W. (2018). Irrigation as a potential driver for anomalous glacier behaviour in High Mountain Asia. Geophysical Research Letters, (45), 1–8. https://doi.org/10.1002/2017GL076158; de Kok, R. J., & Immerzeel, W. W. (2019). The Western Tibetan Vortex as an emergent feature of near-surface temperature variations. Geophysical Research Letters. https://doi.org/10.1029/2019GL085757; Farinotti, D., Immerzeel, W. W., Kok, R. J. de, Quincey, D. J., & Dehecq, A. (2020). Manifestations and mechanisms of the Karakoram glacier Anomaly. Nature Geoscience, 13(1), 8–16. https://doi.org/10.1038/s41561-019-0513-5 [Eric Brun, France]	Noted. We include the suggested literature in our discussion. Text revised and updated
28169	69	18	69	18	The Karakoram Anomaly has also been linked to topographic controls (Scherler et al 2011, JGR) and a variety of other processes that can't be summarized in one line, so to me it is odd to highlight this one only. I suggest to simply point to the piece by Farinotti et al (2020) which discusses each. [Evan Miles, Switzerland]	Accepted. We include the findings of Farinotti et al. (2020). Text revised
3399	69	18	69	18	cooler regional conditions in summer': compared to what? Compared to other time periods (e.g. before the 1970's, which are mentioned on p.69, l.16)? Compared to other regions (that are located in the direct vicinity and same latitude)? [Harry Zekollari, Belgium]	Noted. Farinotti et al (2020) point out the several studies highlighting a concomitant cooling of summer temperatures in the regions was observed despite a general warming trend in regional air temperatures. The cooling was particularly pronounced in the period 1960–1980 and has been attributed to a weakening of the monsoon. Text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3401	69	18	69	18	Another potential mechanism is linked to the role that intensive irrigation may have, which may have caused cooling and influenced atmospheric patterns: see e.g. de Kok et al. (2018, Geophysical Research Letters, doi: 10.1002/2017GL076158) and Thiery et al. (Nature Communications, 2020, doi: 10.1038/s41467-019-14075-4). [Harry Zekollari, Belgium]	Noted. We include the work of de Kok et al. (2020) in our discussion of the "Karakoram anomaly". Text revised
62035	69	18	69	20	...spatially very limited Somewhat vague language. Can this be written with more specificity? "...this anomaly is confined to the region of [spatial description]..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The extent of the region were the "Karakoram anomaly" is describe, text revised
61819	69	19	69	19	can you say how much (%) to get a feeling for how spatially limited (add behind spatially very limited) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	noted, text revised and clarified
3099	69	20	69	20	The example of "Iran" comes rather out of the blue. It looked like the section was dealing with the Karakoram Anomaly and High Mountain Asia? I don't think "Iran" can be counted to any of the two. [Daniel Farinotti, Switzerland]	Accepted. Text revised and better representation of regional variability presented
3101	69	20	69	20	A new paragraph should be started here; the break to the previous few sentences is enormous. [Daniel Farinotti, Switzerland]	Accepted, text revised
2877	69	20	69	20	The reference by Brun et al. (2017) could be replaced by the one by Shean et al. (2020) [Antoine RABATEL, France]	Accepted. We include also the reference to Shean et al 2020.
3405	69	20	69	20	Negative mass balances being predominant in High Mountain Asia. Logic to refer to seminal study by Brun et al. (2017). But may also be worth mentioning the recent study by Shean et al. (2020, Frontiers in Earth Science, doi: 10.3389/feart.2019.00363) here. [Harry Zekollari, Belgium]	Accepted. We include also the reference to Shean et al 2020.
106211	69	20	69	20	The Karimi et al. (2015) study is not really looking at mass balance but at (clean ice) area changes. This is something different. Moreover, I found the results of this study highly doubtful as the separation from seasonal snow is very difficult in this region and the derived area reduction is very high. [Frank Paul, Switzerland]	Accepted. Text revised
4227	69	21	69	21	"Instead of North America outside of Alaska" this should read "Instead of North America outside of Alaska and western Yukon" as Menenounos et al. (2019) do not include the large glaciers of western Yukon. [William Kochtitzky, Canada]	Accepted. We include western Yukon in the sentence. Text revised
4223	69	21	69	31	Should add that Canadian Arctic has expereince 1.1 deg C warming in the last two decades causing a doubling of mass lass when compared with pre-1996 acerages (Noël et al., 2018). Noël, B., van de Berg, W.J., Lhermitte, S., Wouters, B., Schaffer, N. and van den Broeke, M.R., 2018. Six decades of glacial mass loss in the Canadian Arctic Archipelago. Journal of Geophysical Research: Earth Surface, 123(6), pp.1430-1449. [William Kochtitzky, Canada]	Accepted. We include a sentence acknowledge the increase of mass loss in the Canadian Arctic. Text revised
3103	69	24	69	24	The use of the word "conversely" is creating confusion: how are "upper level zonal winds in Western North America outside of Alaska" linked to "volcanoes in South America and Iceland"? Here and elsewhere, the sudden change in geographical focus is somewhat hard to follow. [Daniel Farinotti, Switzerland]	Accepted. Text revised
3407	69	24	69	24	Impact of volcanoes on glacier change. At this point the formulation is not very specific. Two things that could be clarified: do you expect a positive impact on the mass balance (e.g. less melt due to aerosols in stratopshere) or a negative impact on the mass balance (e.g. more melt due to deposition of volcanic material, which will lower the surface albedo). Do you expect this to be a local or a more regional (global) effect? (e.g. when thinking of the reduction of incoming radiation due to increased aerosols) [Harry Zekollari, Belgium]	accepted, text revised
2879	69	24	69	25	I don't understand why this sentence is starting by "Conversly". Indeed, this sentence has nothing to see with what is said in the former ones. [Antoine RABATEL, France]	Accepted. Text revised
18435	69	24	69	29	I am sorry I cannot understand the logic linking these sentences. Why "Conversely"? How the volcanoes can impact the glacier retreat? Please explain. The advance of Pio XI glacier is a different story. I guess this part will be improved in the next version. [Olga Solomina, Russian Federation]	not applicable, text removed
11389	69	26	69	28	Inconsistency in unit notation. Here, year is noted as "a" in contrast to page 69, line 22. [Jacob Clement Yde, Norway]	editorial, prior to publication professional copy-editing of draft will me made
88705	69	26	69	28	It is important to note that Pio XI, the largest glacier on the SPI and in South America, is the only glacier of the Patagonian Ice Fields to have experienced a net large advance and the only known surge-type glacier on the SPI, result of complex dynamics associated with both surge mechanisms and terminus calving processes. (Foresta, L. et al. Heterogeneous and rapid ice loss over the Patagonian Ice Fields revealed by CryoSat-2 swath radar altimetry. Remote Sens. Environ. 211, 441–455 (2018)). [Rosemary Vieira, Brazil]	Noted. Due to length extent of the text we deleted this sentence and focus the text to assess the recent glacier mass changes at the scale of the regions shown in Table 9.3. text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3409	69	26	69	29	Explanation about glacier Pio X, which has a positive mass balance and advances. A bit strange to have this (very specific) example without a real context / explanation. Why is this due? Any hypothesis? I do not know the glacier/region, but this would be the first thing that I would want to know. Maybe due to increase in precipitation? Without explanation, can be misinterpreted and potentially be taken out of context. [Harry Zekollari, Belgium]	accepted, text revised
3411	69	29	69	31	Placeholder for regional anomalies for certain regions for the years 2017 and 2018. But can such a short time period really be used to say something significant about an anomaly? Are we not just looking at interannual variability here, e.g. years with slightly higher precipitation to on average. As mentioned, 2019 is again a more negative year for these regions. Would be careful for overinterpretation here. [Harry Zekollari, Belgium]	not applicable, placeholder removed and text revised
18119	69	30	69	30	For Peru and Bolivia a strong increase in the mass loss was reported for the period 2013-2016 by Seehaus et al. 2019 and 2020 which correlates with the strong El Nino event during this period [Thorsten Seehaus, Germany]	accepted, suggested reference added to assessment
106213	69	31	69	31	I suggest to not focus here too much on individual mass balance years but the more long-term trends. [Frank Paul, Switzerland]	accepted, text revised and new studies included in assessment
27653	69	34	71	2	For a better reading, we suggest that the table be on a one-page spread. [Eric Brun, France]	Noted. The final formatting will be done during the production phase.
20195	69	34	71	2	The legend of figure 9.1 should refer to this table 9.3, inasmuch as it supplies the numbers of glacierized regions shown on the figure [philippe waldteufel, France]	Accepted. Legend revised
89381	69	36	69	36	Change ‘glacier mass balance change’ in table header to ‘glacier mass balance’ or ‘glacier mass change’ - this is meant to be ‘recent mass change’ per the caption. [Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text revised
89383	69	36	69	36	Given the limitations in determining the number of glaciers as detailed in the text, I don’t know that including ‘number of glaciers’ adds anything to the table. [Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We delete from the table the number of glaciers
99953	69	36	69	36	As mentioned on P68 L37, the areal uncertainty statistics should probably be provided for various regions in Table 9.3 too if they are available. [Bhardwaj Anshuman, Sweden]	Accepted. Previous Table 9.3 moved to Appendix Table 9.A.2 and uncertainties in the extent of glaciers areas as assessed by Pfeffer et al (2014:doi: 10.3189/2014JoG13J176) included.
67333	69	36	69	43	Much of the table is a copy of SROCC, chapter 2, Appendix Table 2A1 which also has the volume and mass changes for the recent period. Is this necessary? The only difference seems that for HMA the wouters estimate was not considered and a new study for the southern Andes was included. [Regine Hock, United States of America]	Noted. table (now 9.A.2) includes the glacier mass change for the period 2000-2019 , Table 9.3 also includes the project glacier mass loss between 2015 and 2100 for different RCPs.
62011	69	36	69	43	"Table 9.3. The last 4 columns represent «future contribution of glaciers to sea level rise», so it makes sense to put numbers as positive values. However, the current title (header) of this section is «Total projected glacier change between 2016-2100 (mm sle)». It is thus confusing, because the reader would expect to see NEGATIVE values here, as it is the case for the recent mass balance changes (the 2 columns just on the left)." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We use mm of SLE. We change the header of this column to highlight that we are talking about future glacier contribution to SLR.
3415	69	36	69	43	Table 9.3. Why is only one regional study used in this table (Dusaillant et al., 2019), while others exist for the same region (e.g. Braun et al., 2019) and also for other regions (e.g. Brun et al., 2017, Shean et al., 2020). Will maybe be added later? (as is the case for Figure 9.22 according to the placeholder). Is probably important to be consistent here: e.g. only use results from global estimates or if you do consider regional estimates: do consider all studies that comply with some (non-arbitrary) criteria. [Harry Zekollari, Belgium]	accepted, suggested reference added to assessment
61821	69	36	69	44	table caption change sea level rise to SLR [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
93571	69	36	71	1	Table 9.3 - Number of glaciers is not a useful concept, and this column can be deleted. [Miriam Jackson, Norway]	Accepted. We delete from the table the number of glaciers
106215	69	36	71	1	Table 9.3: Heading: Please write 'Glacier mass change' rather than 'Glacier mass blance change' when reporting absolut values. Please also add for the 2006 to 2016 period the specific mass balance per year (in m w.e./a) as only these values are comparable across regions. [Frank Paul, Switzerland]	accepted, table revised, now 9.A.2 with mass change rate for period 2000-2019
40887	69	36			Table 9.3: why are current observations negative and future projection positive? This is inconsistent. [TSU WGI, France]	Noted. We shown glacier volume, mass change a future contribution to SLR as mm of SLE. We decide to use all values as positive to avoid confusions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107471	69	36			Why doesn't Table 9.3 include projections for RCP4.5? RCP4.5 projections are included in the ice sheet tables, so seems they should be included here for glaciers as well [Jennifer Walker, United States of America]	Accepted. We include the projection for the RCP 4.5 to be consistence with the rest of the chapter.
2063	69	40	69	40	Please indicate why for these regions the SROCC results have not been used. In my opinion it would also be important to state here on which studies the SROCC results were mostly based (Zemp et al., 2019; Wouters et al., 2019) [Matthias Huss, Switzerland]	noted, table, now 9.A.2 includes results from new study, Hugonnet et al, 2021
3413	69	40	69	40	is based on SROCC Chapter 2 Table 2A.1': for the reader not clear on what this was based (which type of observation?). Would be good to mention. [Harry Zekollari, Belgium]	noted, table, now 9.A.2 includes results from new study, Hugonnet et al, 2021
16413	69	44	69	44	Table 9.3 heading entry is split in a very weird way, presumably will be tidied up. [Julian Mak, China]	Noted. Table revised
3417	69	44	69	44	A few remarks on Table 9.3: (i) numbers given for the area are very specific (Worldwide glacier volume: 705738.8 km^2...): may be worth adding uncertainty (like is done for the glacier volume) here and/or adjust the precision, (ii) uncertainty for glacier volume: to what is this referring? 1 sigma? 2 sigma? Maybe not needed to specify if this is according to standard definitions followed throughout the whole report (not sure), (iii) total projected sea level change for the period 2016-2100: will you give the results from GlacierMIP phase 2 here (Marzeion et al., under review), or the ones emulated to the SSPs? (Edwards et al., under review)? Probably important to be consistent here to avoid confusion with the numbers (I am mainly thinking about the executive summary here, were the emulated numbers are mentioned: see. p.6, l. 40-43) [Harry Zekollari, Belgium]	Accepted, all points taken into account and table revised with new results
11391	69	44	69	44	Table 9.3: Under "Glacier volume", change to "(mm SLE)" [capitalize SLE] [Jacob Clement Yde, Norway]	Accepted, table revised and moved to appendix Table 9.A.2
61823	69	44	69	44	in the table: what does RCP mean, it's not in the glossary [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. RCP means Representative Concentration Pathway (RCP), which is a greenhouse gas concentration trajectory adopted by the IPCC.
93573	69	44	69	44	Glacierized is an ambiguous term (as a poll during SROCC showed). Glacier-covered area is better understood. [Miriam Jackson, Norway]	Accepted. We change Glacierized by glacier-covered area in the caption. Text revised
103853	69	44	71	1	About table 9.3 in column 6 the sle contribution from all worlds glaciersis provided as a negative number ?? [Philippe Tulkens, Belgium]	accepted, table revised
80879	69	44	71	1	About table 9.3 in column 6 the sle contribution from all worlds glaciersis provided as a negative number ?? [Louise Sandberg Sørensen, Denmark]	accepted, table revised
32157	69	44			Table 9.3 last column header "Total projected glacier change between 2016-2100 (mm sle)"; table caption says 2015-2100. Is it 2015 or 2016? [Anja Wendt, Germany]	accepted, table revised and 2015 stated in both places
32159	69	44			Table 9.3 Recent mass change is given negative (mass loss), while projected sea level rise contribution is positive (increase). Although this is consistent, the change in sign is confusing. Maybe recent change can also be given as sea level contribution as well? It's unit is in sle anyway. [Anja Wendt, Germany]	accepted, table caption revised
88031	69	44			Use SLE instead of sle in Table 9.3 [Georg Kaser, Austria]	accepted, SLE now in table
116877	69		69		Please provide an explanation for regional anomalies (is this related to precipitation multi decadal trends, any explanation for those trends). [Valerie Masson-Delmotte, France]	accepted, text revised
2061	70	1	70	1	Is the table stating "glacier volume" or "potential sea-level rise contribution" (accounting for ice volume below sea level)? This is not clear at present. I think it is the latter, but it is labelled as "glacier volume" [Matthias Huss, Switzerland]	taken into account, table revised and table caption clarified
100041	70	16	70	16	Dusaillant et al. 2019 only measures South American low latitude glaciers, so these figures are incomplete. Additionally, the numbers by Seehaus et al. 2019 for South American tropical glacier mass changes are probably more precise w/ better uncertainty estimation. [Forrest Schoessow, United States of America]	noted, table revised and updated with new estimates
3939	70	18	70	18	Update of NZ data for the year 2016 from Baumann et al. submitted to JoG (in review): number of glaciers = 2918; glacier area = 795 km² [Sabine Baumann, Germany]	Noted. For consistency, the glacier-covered area for the different regions in year 2000 from RGI 6.0 is presented in Table 9.A.2.
61825	70		70		in table: why are some numbers in bolt? Explain in the caption [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, table revised
18439	71	1	71	4	I find this sentence confusing. I would rather re-phrase somehow the citation from Shakun et al., 2015 - it is much more clear: "A reassessment of the cosmogenic-nuclide based chronology of glacier fluctuations spanning over 100 of latitude shows that glacier retreat was broadly synchronous with the increase in atmospheric CO2 and global temperature from 18–11 ka." [Olga Solomina, Russian Federation]	Accepted. Text revised. Comment misplaced, is page 72.
62037	71	5	71	6	Sentence is very hard to understand: it is unclear what "...processes surface climate-induced glacier melt and retreat" is describing [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81453	71	5	71	27	Much of this paragraph is more a review or text-book style. More assesment? Where is it observed? How much? Impact? How certain? The following paragraph, lines 29-49 same page are a good example. [Andreas Kääb, Norway]	Noted. Text completely rewritten
26505	71	5	71	27	I think it would be great if this paragraph could be expanded, since in section 9.5.1 there is currently very limited discussion about physical processes and mechanisms that are driving the glacier changes. In this paragraph, right now only a selection of potential factors modulating glacier melt and retreat are discussed, while some others appear to be missing. For example, the role of glacier dynamics is hardly or not discussed. It could be mentioned that increased melt has been found to affect the seasonal velocity cycle of Arctic glaciers, with increased summer velocities and reduced winter velocities on glaciers in Alaska (Burgess et al. 2013) and Svalbard (Van Pelt et al. 2018). At longer time-scales, multi-decadal trends generally suggest a decelleration of glacier flow (Waechter et al. 2015; Thomson and Copland, 2017). References: [1] Burgess, E. W., Larsen, C. F., & Forster, R. R. (2013). Summer melt regulates winter glacier flow speeds throughout Alaska. Geophysical Research Letters, 40, 6160–6164. https://doi.org/10.1002/2013GL058228. [2] Van Pelt, W.J.J., V.A. Pohjola, R. Pettersson, L.E. Ehwald, C.H. Reijmer, W. Boot, and C.L. Jakobs (2018). Dynamic response of a High Arctic glacier to melt and runoff variations. Geophysical Research Letters, 45, 4917–4926. doi:10.1029/2018GL077252. [3] Waechter, A., Copland, L., & Herdes, E. (2015). Modern glacier velocities across the Icefield Ranges, St Elias Mountains, and variability at selected glaciers from 1959 to 2012. Journal of Glaciology, 61(228), 624–634. https://doi.org/10.3189/2015JoG14J147. [4] Thomson, L., & Copland, L. (2017). Multi-decadal reduction in glacier velocities and mechanisms driving deceleration at polythermal White Glacier, Arctic Canada. Journal of Glaciology, 63(239), 450–463. https://doi.org/10.1017/jog.2017.3. [Ward van Pelt, Sweden]	Noted, text is completely rewritten based on new post-SROCC references
26507	71	5	71	27	Another topic that could be added to this paragraph is glacier surging in a changing climate. Studies in Svalbard suggest that surface melting may have triggered the surge of Basin-3 (Dunse et al. 2015), and that increased melting can cause temporal clustering of surges (Nuth et al. 2019). Oerlemans and Van Pelt (2015) showed that the occurrence of surges accelerates glacier retreat in a warmer climate. Generally, however, there is still limited evidence for changes in surging frequency in a warmer climate. References: [1] Dunse, T., Schellenberger, T., Hagen, J. O., Kääb, A., Schuler, T. V., and Reijmer, C. H.: Glacier-surge mechanisms promoted by a hydro-thermodynamic feedback to summer melt, The Cryosphere, 9, 197–215, https://doi.org/10.5194/tc-9-197-2015, 2015. [2] Nuth, C., Gilbert, A., Köhler, A. et al. Dynamic vulnerability revealed in the collapse of an Arctic tidewater glacier. Sci Rep 9, 5541 (2019). https://doi.org/10.1038/s41598-019-41117-0. [3] Oerlemans, J. and van Pelt, W. J. J.: A model study of Abrahamsenbreen, a surging glacier in northern Spitsbergen, The Cryosphere, 9, 767–779, https://doi.org/10.5194/tc-9-767-2015, 2015. [Ward van Pelt, Sweden]	Noted. Text revised. Unfortunately, the space is limited, suggested references are assessed in SROCC
106217	71	5	71	27	This sections reads a bit convoluted, jumping from theme to theme without a storyline. From collapsing to debris-covered glaciers, to black carbon deposition on snow and refreezing of firn, back to the role of ponds on debris-covered glaciers and ending with a note on calving glaciers. In the end, all these observations should give 'high confidence' that 'a changing climate' (I think you mean temperature increase?) is responsible for the observed glacier retreat. I think this conclusion is misleading. All of the mentioned processes and observations have a very limited relation to global warming, so it makes little sense citing them here in the first place. I am fine presenting these observations (better sorted) somehwere, but I think they do not reveal an 'improved understanding' of the glacier-climate relation. [Frank Paul, Switzerland]	Accepted, text revised
32161	71	5			Anthropogenic factors: There are only natural factors mentioned in this paragraph. [Anja Wendt, Germany]	accepted, text revised
3105	71	6	71	8	The formulation is somewhat too strong and thus misleading: the evidence for a direct link between climate change and the mentioned collapses is still weak. A more cautious wording seems appropriate. [Daniel Farinotti, Switzerland]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4225	71	6	71	8	I think it is a bit of an overstatement to say that the study by Kääb et al. (2018) is directly tied to climate. It was likely a surge instability exacerbated by warming and mass loss, but I think it it more complicated than climate alone. [William Kochtitzky, Canada]	accepted, text revised
106219	71	6	71	8	Collapsing glaciers are certainly a most interesting endmember of glacier behaviour. However, I doubt that the link to climate change as a reason can already be made. Moreover, I might not fully understand the sentence: What does '... is an extreme example of processes enhancing glacier response to climatic change' mean? [Frank Paul, Switzerland]	accepted, text revised
61857	71	8	71	9	I find it confusing that the start of this paragraph includes an example of decreasing sensitivity. Therefore I recommend to make clear that this is more of an exception or a contrast. I suggest: "In contrast to that an increasingly thick debris cover...." Just to make clear this is not usual and one of the little negative feedbacks we know [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
32165	71	8	71	9	Apart from detailed studies of single glaciers, theoretical models should also be cited, e.g. (Evatt et al., 2015, doi: 10.3189/2015JoG14J235) and (Nicholson and Benn, 2006, doi:10.3189/172756506781828584) [Anja Wendt, Germany]	noted, suggested references considered they are pre-SROCC and pre-AR5, text is revised and improved
28171	71	8	71	13	on retreating glaciers' is not always true, as many debris-covered glaciers stagnate and downwaste, rather than retreating. See e.g. Scherler et al 2011 (Nature Geoscience). Rather, a better start would be that the emergence and accumulation of englacial debris, if sufficiently strong, can alter glacier melt rates and modify their dynamics, potentially reducing their sensitivity to climate (Anderson et al 2016). However, mass losses for debris-covered glacier ice at the regional scale are occurring at the same rate as for debris-free glaciers (Kaab 2012; Pellicciotti 2015; Brun 2019). Refs: Scherler, D., Bookhagen, B., & Strecker, M. R. (2011). Spatially variable response of Himalayan glaciers to climate change affected by debris cover. Nature Geoscience, 4(3), 156–159. https://doi.org/10.1038/ngeo1068 Anderson, L. S., & Anderson, R. S. (2016). Modeling debris-covered glaciers: extension due to steady debris input. The Cryosphere, 10, 1105–1124. https://doi.org/10.5194/tcd-9-6423-2015 Kääb, A., Berthier, E., Nuth, C., Gardelle, J., & Arnaud, Y. (2012). Contrasting patterns of early twenty-first-century glacier mass change in the Himalayas. Nature, 488(7412), 495–498. https://doi.org/10.1038/nature11324 Pellicciotti, F., Stephan, C., Miles, E., Herreid, S., Immerzeel, W. W., & Bolch, T. (2015). Mass-balance changes of the debris-covered glaciers in the Langtang Himal, Nepal, 1974–99. Journal of Glaciology, 61(226), 1–14. https://doi.org/10.3189/2015JoG13J237 Brun, F., Wagnon, P., Berthier, E., Jomelli, V., Maharjan, S. B., Shrestha, F., & Kraaijenbrink, P. D. A. (2019). Heterogeneous Influence of Glacier Morphology on the Mass Balance Variability in High Mountain Asia. Journal of Geophysical Research: Earth Surface, 1331–1345. https://doi.org/10.1029/2018JF004838 [Evan Miles, Switzerland]	accepted, text revised, post-SROCC references added, thank you for many good references that supported assessment
27655	71	9	71	9	IGE: Brun et al (2016) deals with cliff backwasting on debris cover glaciers, not on increasing debris cover or sensitivity of debris covered glaciers to climate. For increasing debris cover cite Kirkbride and Deline (2013) and for debris covered glaciers sensitivity to climate cite Scherler et al, 2011 and Vincent et al (2016). - ref : Kirkbride, M.P., Deline, P., 2013. The formation of supraglacial debris covers by primary dispersal from transverse englacial debris bands. Earth Surf. Process. Landforms 38 (15), 1779–1792. https://doi.org/10.1002/esp.3416.; Scherler, D, Bookhagen B, Strecker M Spatially variable response of Himalayan glaciers to climate change affected by debris cover NATURE GEOSCIENCE, 4, 2011, DOI: 10.1038/NGEO1068; Vincent, C., P. Wagnon, J. M. Shea, W. W. Immerzel, P. D. A. Kraaijenbrink, D. Shrestha, A. Soruco, Y. Arnaud, F. Brun, E. Berthier, and S. F. Sherpa, Reduced melt on debris-covered glaciers: investigations from Changri Nup Glacier, Nepal, The Cryosphere, 10, 1845-1858, doi:10.5194/tc-10-1845-2016, 2016 [Eric Brun, France]	noted, suggested references considered they are pre-SROCC and pre-AR5, text is revised and improved
61827	71	9	71	9	is that caused by the change in albedo/ insulation effect of debris ? Include short reason [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, the text is revised
61745	71	10	71	10	Run-on sentence. Put period after (Scherler et al., 2018) citation and begin new sentence with 'Although only...' [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
106221	71	10	71	10	I suggest removing 'Recently' (it makes little sense in a few years from now ;) [Frank Paul, Switzerland]	accepted, text revised

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18121	71	10	71	13	There is a potential huge bias in the estimated debris cover area due to temporal inconsistency of the used glacier outlines and remote sensing data. In the Andes (especially in the Tropical Andes, the glacier retreated strongly and the retreat areas are classified as debris covered areas. This leads to unrealistically high values. [Thorsten Seehaus, Germany]	noted, the text about debris covered glaciers revised and new reference added
96993	71	10	71	13	We suggest to split sentence in two. [Nicole Wilke, Germany]	accepted, text revised
28173	71	11	71	12	Alaska is not 'low latitude'. I suggest a better description here.The 'high mountain' appears relevant, but active orogeny and more broadly geological setting are definitely plausible controls. I suggest just naming the regions here though. [Evan Miles, Switzerland]	accepted, text revised
62013	71	11	71	12	With a territory extending roughly from 50° to 70° N, I would not include Alaska as an example of «low-latitude» area. OK for «high mountain» area, though. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
2881	71	11	71	12	None of the regions within the brackets belong to the low-latitude regions! Consider replacing "low-latitude" by "mid-latitude". [Antoine RABATEL, France]	accepted, text revised
3419	71	11	71	12	low-latitude mountain areas': would be tempted to omit 'low-latitude': as the sentence still holds then and is more correct given that you also mention regions like Alaska. [Harry Zekollari, Belgium]	accepted, text revised
61747	71	12	71	12	Examples of low-latitude, high mountain areas include Alaska and Western North America. Unsure where cutoff for 'low-latitude' is, so western North America may be valid, but Alaska is not low-latitude [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
67915	71	12	71	12	'low-latitude' is not appropriate here. Alaska has lots of debris cover in the AK Range (~63N) and there is a large concentration of debris-covered glaciers in NE Greenland. [Martin Truffer, United States of America]	accepted, text revised
32163	71	12			Alaska is a low-latitude high mountain area? [Anja Wendt, Germany]	accepted, text revised
61991	71	13	71	13	The second sentence should start with 'Conversely'. The previous sentence indicates that debris cover can reduce glacial melting. This sentence now gives a reason for increase melt, due to light-absorbing particles deposited on glacier surfaces. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
3421	71	13	71	15	Changes in surface albedo that can cause enhanced surface melt: could consider adding a reference to recent work(s) by Di Mauro and colleagues that clearly illustrate this (2019, The Cryosphere, doi: 10.5194/tc-13-1147-2019; 2020, Scientific Reports, doi: 10.1038/s41598-020-61762-0) [Harry Zekollari, Belgium]	taken into account, suggested references added to assessment
109075	71	13	71	16	Suggest adding a reference section 6.3.1.4 here. [Chaincy Kuo, United States of America]	Accepted, reference to 6.3.1.4. added
3107	71	15	71	16	For a reference referring to Central Asia, Schmale et al. (2017) could be added: https://doi.org/10.1038/srep40501 [Daniel Farinotti, Switzerland]	taken into account, suggested references added to assessment
61855	71	16	71	16	change "With warming, the refreezing..." to " With increased warming...." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
26509	71	16	71	18	A similar decreasing trend of refreezing was found in a recent study (Van Pelt et al. 2019) for all glaciers in Svalbard during 1957-2018, and also in an earlier study on glaciers near Kongsfjorden, Svalbard (Van Pelt and Kohler, 2015) during 1961-2012. Both or one of these references could be added here, for example by adding "and on Svalbard (Van Pelt and Kohler, 2015; Van Pelt et al. 2019)". References: [1] Van Pelt, W.J.J. and J. Kohler (2015). Modelling the long-term mass balance and firn evolution of glaciers around Kongsfjorden, Svalbard. Journal of Glaciology, 61, 228, 731-744, doi:10.3189/2015JoG14J223. [2]: Van Pelt, W.J.J., V.A. Pohjola, R. Pettersson, S. Marchenko, J. Kohler, B. Luks, J.O. Hagen, T.V. Schuler, T. Dunse, B. Noël, and C.H. Reijmer (2019). A long-term dataset of climatic mass balance, snow conditions and runoff in Svalbard (1957–2018). The Cryosphere, 13, 2259-2280, doi:10.5194/tc-2019-53. [Ward van Pelt, Sweden]	accepted, thank you for pointing to reference, text revised
2883	71	17	71	17	Not sure that "than in melwater production" is needed. Likely to be removed. [Antoine RABATEL, France]	accepted, text revised

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28175	71	18	71	20	The Brun et al (2016) reference only addresses one of these aspects (ice cliffs). More suitable investigations that address each of these processes are Buri et al (2016, JGR); Miles et al (2018); Brun et al (2018). Buri, P., Miles, E. S., Steiner, J. F., Immerzeel, W. W., Wagnon, P., Pellicciotti, F., & Brun, F. (2016). A physically-based 3D model of ice cliff evolution on a debris-covered glacier. Journal of Geophysical Research: Earth Surface, 121, 2471–2493. https://doi.org/10.1002/2016JF004039 Miles, E. S., Willis, I., Pellicciotti, F., Buri, P., Steiner, J. F., Arnold, N. S., … Arnold, N. S. (2018). Surface Pond Energy Absorption Across Four Himalayan Glaciers Accounts for 1/8 of Total Catchment Ice Loss. Geophysical Research Letters, 45(19), 10464–10473. https://doi.org/10.1029/2018GL079678 Brun, F., Wagnon, P., Berthier, E., Shea, J. M., Immerzeel, W. W., Kraaijenbrink, P. D. A. A., … Arnaud, Y. (2018). Ice cliff contribution to the tongue-wide ablation of Changri Nup Glacier, Nepal, central Himalaya. The Cryosphere, 12(11), 3439–3457. https://doi.org/10.5194/tc-12-3439-2018 [Evan Miles, Switzerland]	noted, text revised, due to space limitation not possible to add all suggested references
3423	71	19	71	19	Importance superficial ponds for enhanced melting. Recent study that clearly highlights this: Miles et al. (2018, Geophysical Research Letters, doi: 10.1029/2018GL079678) [Harry Zekollari, Belgium]	noted, text revised, due to space limitation not possible to add all suggested references
99955	71	19	71	19	"superficial" or "supraglacial"? [Bhardwaj Anshuman, Sweden]	not applicable, text removed
26407	71	22	71	22	Check parenthesis: (e.g. (Luckman et al., 2015; Minowa et al., 2017)) [María Santolaria-Otín, France]	not applicable, text removed
2885	71	22	71	22	Some of the brackets are not needed [Antoine RABATEL, France]	accepted, text revised
3941	71	22	71	22	(e.g. Luckman et al., 2015; Minowa et al., 2017) (without double brackets) [Sabine Baumann, Germany]	accepted, text revised
67917	71	23	71	23	Citing Motyka et al., 2013, GRL would be appropriate here; they discuss observations of increasing runoff leading to increased submarine melt [Martin Truffer, United States of America]	noted, thank you for reference, due to space limitation not possible to include all suggested references
62079	71	24	71	25	Stating the main driver of recent retreat as surface air temperature should be 'low confidence' as the statement is derived from only 1 study and 1 region (Candian Arctic). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	noted, text revised, sentence removed
62015	71	29	71	29	What is meant here by «post-glacial period»? The period after the Last Glacial Maximum (~ 20 kyr BP)? The Holocene? The period after the Little Ice Age? The paragraph mentions different time spans, it is not clear. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	not applicable, text removed
96995	71	29	71	29	We suggest to change "appears" to "is". [Nicole Wilke, Germany]	Accepted. Text revised due to several comments
106223	71	29	71	29	I think when speaking about glacier retreat, we talk about length changes of glaciers? There are only few studies that have analysed length changes at a global scale (e.g. Zemp et al. 2015). I suggest adding a related citiation here. [Frank Paul, Switzerland]	accepted, text revised
22619	71	29	71	49	This paragraph requires revision. It should note, explicitly, the relevant findings from chapters 2 and 3 and then build upon them. Presently it undertakes a quasi-redundant assessment without any cross-referencing. For the report as a whole to stand together this requires remedying. [Peter Thorne, Ireland]	accepted, text revised and coordination between ch2 and ch3 undertaken
169	71	29	71	49	See also the new PATICE paper that contextualises present-day glacier recession with the longer term record of ice dynamics (35,000 years ago to present day) in Patagonia. We evaluate ice extent at multiple periods during the Holocene and argue that glacier recession (% a-1) is now faster than at any time recorded, and is on a parallel (km2 per annum) with that observed at circa 19-15 ka, for an ice sheet that was an order of magnitude larger (Davies et al., accepted manuscript, Earth-Science Reviews). This supports the idea that global glacier rectreat is exceptional within the current interglacial with new evidence and data. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	accepted, the suggested reference added to assessment
129497	71	29	71	49	[CONFIDENCE] The passage cites only one reference without any details, then concludes that the glacier retreat is due to the anthropogenic climate change. The passage citing Nesje et al. (2002) states that the retreat continued for thousands of years. Is this climate variability or anthropogenic climate change? More surprisingly, the passage assigns the anthropogenic origin as a conclusion of high confidence. [Trigg Talley, United States of America]	accepted, text revised
61829	71	30	71	30	at a very high rate: is it possible to quantify this high rate? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
69583	71	30	71	30	Repeat of a comment above. What are the exceptions? Over the 20th centiry all glaciers in the WGMS database have retreated, I think [gerard Roe, United States of America]	accepted, text revised

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62039	71	31	71	31	…and despite of various regional trends in precipitation regime. Recommend this language be more specific in terms of what is changing about precipitation regime. Is this implying that accumulation is increasing and should contribute to more positive mass balance? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
69585	71	31	71	31	"rather than degradation, and despite of" I think ths can be rephrased [gerard Roe, United States of America]	accepted, text revised
61993	71	33	71	33	Please rewrite 'show that now the glaciers' to 'show that glaciers are now'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
69587	71	33	71	34	"are smaller than ever since" Rephrase [gerard Roe, United States of America]	Accepted. We rephrase this sentence.
61831	71	34	71	34	archaeological artefacts: I find the word artefact misleading, would buildings or constructions work? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
3425	71	34	71	35	archeological artefacts were continuously covered with ice...': not entirely sure how this should be interpreted. Are these regions now deglaciating for the first time? If so, could potentially add this to clarify the statement: e.g. '...(Nesje et al., 2012), and are now emerging from newly deglaciated regions, implying that...' [Harry Zekollari, Belgium]	accepted, text revised
3109	71	35	71	35	The abbreviation "CE" is undefined. [Daniel Farinotti, Switzerland]	accepted, text revised
129499	71	35	71	36	How much larger? Quantitative statements help, particularly in places such as this where the comparison is made on the degree of changes. [Trigg Talley, United States of America]	accepted, text revised
35143	71	36	71	38	Exposure ages from Iceland also show that glaciers here are now retreating after having been covered for at least 2000 years. REFERENCES: Harning, D.J., Geirsdóttir, Á., Miller, G.H., Anderson, L., 2016. Episodic expansion of Drangajökull, Vestfirðir, Iceland, over the last 3 ka culminating in its maximum dimension during the Little Ice Age. Quat. Sci. Rev. 152, 118-131. Harning, D.J., Geirsdóttir, Á., Miller, G.H., 2018. Punctuated Holocene climate of Vestfirðir, Iceland, linked to internal/external variables and oceanographic conditions. Quat. Sci. Rev. 189, 31-42. [David Harning, United States of America]	accepted, text revised and suggested references added to assessment
61833	71	36	71	38	I found the sentence starting with Exposure of plants… hard to read and not clear. I would suggest to include the word dating and dead as follows: Dating of dead plants, emerging from beneath glaciers... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
61835	71	39	71	39	change sentence starting with Koch et al (2014) provided…. To "dramatic loss if snow and ice that occurred in the last decades in the mountains in Western Canada, as well as … " [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
3943	71	40	71	40	in Western Canada (without 'the') [Sabine Baumann, Germany]	Accepted, text revised
3111	71	41	71	41	Here and at L. 39, the section makes reference to "snow observations" that seem not pertinent to this "glacier" section. Thee should be probably moved to elsewhere. [Daniel Farinotti, Switzerland]	accepted, text revised
3113	71	41	71	41	The evidence for the evolution of "snow patches" at the scale of "several thousand years" seems scant at best. Possibly remove the claim. [Daniel Farinotti, Switzerland]	accepted, text revised
1767	71	45	71	49	Assessment of global glacier retreat should include a determination of how much of the retreat (percentage) is due to anthropogenic factors vs. other factors. This can be discussed on line 49. [Michael Kennish, United States of America]	accepted, this is topic of 3.4.3.1 and reference to that section made in text
96997	71	45	72	19	There is a bit of overlap here with Ch3 concerning D&A, but based on (partly) different references. The focus here is on the (multi-)millennial time scale, while Ch3 focuses on the ~centennial time scale. If D&A is discussed here in Ch9 as well, we suggest to include more references and lines of evidence also for the centennial time scale. Alternatively, and perhaps better, it could simply be pointed to the corresponding section in Ch3 for the centennial time scale (and formal D&A) studies, and explicitly said that the focus here is on the longer time scales. [Nicole Wilke, Germany]	accepted, coordination with Ch3 has improved this part and reference to 3.4.3.1 put in Ch9
106225	71	46	71	49	Isn't this known since the study by Reichert et al. (2002)*? Maybe add this study? And what about: doi.org/10.1126/science.1254702? *https://doi.org/10.1175/1520-0442(2002)015<3069:RGREIV>2.0.CO;3 [Frank Paul, Switzerland]	taken into account, the reference suggested are assessed in SROCC and in 3.4.3.1, new literature since SROCC is the focus of this assessment
82961	71	47	71	47	I wonder if not also related findings stated in SROCC should be mentioned here, along with AR4 and AR5. [Sebastian Gerland, Norway]	accepted, better reference to SROCC is made in whole chapter
62041	71	47	71	47	…with high confidence… The abundance of evidence listed in this paragraph is impressive: does this not lend to very high confidence in this conclusion? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account and assessment revised
88707	71	47	71	47	Roe et al., 2016. Reference not found. [Rosemary Vieira, Brazil]	noted, reference now corrected

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69589	71	47	71	49	"with high confidence that the character of the global glacier retreat in the 20th through early 21st centuries is exceptional within the current interglacial." Check for consistency/compatibility with Ch2 p61 L21: "There is insufficient evidence to conclude when the large-scale glacier retreat was last as rapid as recent changes" [gerard Roe, United States of America]	taken into account and consistency between Ch2 and ch9 improved
32167	71	47			replace (Roe et al., 2016) with (Roe et al., 2017) [Anja Wendt, Germany]	noted, reference now corrected
18437	71	49	71	49	I think the "interglacial" should be replaced here by "post-glacial" time because the retreat of the glaciers in the Late Glacial time was quite rapid and also global similar to the modern one (but occurred for different reasons). [Olga Solomina, Russian Federation]	accepted, text revised
106227	72	1	72	35	I am not quite clear what the relevance of glacier changes some 10 to 20k years ago is in the context of the former description and of what is presented in Fig. 9.22? What has the observed glacier change over the past 100 years (described before) to do with possible (modelled) changes over the lat 10000 years? I do not get the link. Maybe restructure the entire section 9.5.1.1 according to a consistent timeline? [Frank Paul, Switzerland]	accepted, text improved
61749	72	2	72	2	Change 'cosmogenic-nuclide production-rate calibrations' to 'cosmogenic-nuclide exposure ages'. Cosmogenic production rate calibration is a technique used to 'tune' the method in order to achieve more accurate exposure ages. The calibrations in and of themselves do not provide data constraining glacier retreats. The exposure ages which were re-calculated using new calibrations in this paper are the date informing glacial retreat. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
99715	72	2	72	2	should be "recalibrated cosmogenic nuclide ages" [Peter Clark, United States of America]	accepted, text revised
3427	72	4	72	4	List of forcings is given, which includes 'ice sheets': how should ice sheets be seen as a kind of forcing for glaciers? Potentially elucidate by being more specific here? [Harry Zekollari, Belgium]	accepted, text revised
171	72	4	72	4	Also topography influenced glacier responses. E.g. by damming large glacial lakes, that caused calving and rapid recession in Patagonia (Davies et al. 2020, ESR) [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, text revised due to several comments
3429	72	7	72	10	in the Northern Hemipshere the size of glaciers was generally increasing over the Holocene': indeed, but for many glaciers not throughout the entire Holocene. For many locations the size of the glaciers was first decreasing, until the Holocene Thermal Maximum. And only after this period an increase in the glacier size occured. This is supported by both observations (see e.g. review works by Solomina) and numerical modelling evidence (e.g. Gilbert et al., 2017, Geophysical Research Letters, doi: 10.1002/2016GL072394); Zekollari et al., 2017, Quaternary Science Reviews, doi: 10.1016/j.quascirev.2017.05.010). [Harry Zekollari, Belgium]	accepted, text revised and suggested references added to assessment
62043	72	8	72	8	...shows a certain correlation... Recommend re-writing as "...strong correlation" or "significant [negative/positive] correlation" (depending on the correlation coefficient and p-value) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
62081	72	8	72	8	Use of 'certain correlation' implies an IPCC certainity language without actually including one. It would be better and more transparent to remove 'certain' and include a liklihood assessment and confidence assessment? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
61531	72	8	72	10	I agree with this statement, although I think it could use some extra calrification: The Greenland ice sheet was retreating during the early Holocene, as documented in some of the stated references (e.g. Briner et al. 2016, Lecavalier et al. 2017) and advanced during the mid-to-late Holocene in line with northern hemisphere summer insolation. This is also stated in 9.6.2, page 9-89, lin 37-46. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text revised
99717	72	9	72	9	add Marcott et al. (2019, npj Climate and Atmospheric Science) [Peter Clark, United States of America]	taken into account, text revised and several additional reference added
18443	72	11	72	11	For Patagonia a good new refernce here is Reynhout, S. A., Sagredo, E. A., Kaplan, M. R., Aravena, J. C., Martini, M. A., Moreno, P. I., ... & Schaefer, J. M. (2019). Holocene glacier fluctuations in Patagonia are modulated by summer insolation intensity and paced by Southern Annular Mode-like variability. Quaternary Science Reviews, 220, 178-187. Please add. [Olga Solomina, Russian Federation]	accepted, suggested reference added to assessment
18445	72	11	72	11	Instead "in the tropics" it is better to write "in the tropical Andes" refereing to Jomelli et al., 2011, 2014. A recent paper by Jackson et al., 2020 in the Climate of the past (Holocene glaciation in the Rwenzori Mountains, Uganda) https://doi.org/10.5194/cp-2020-61 reports interesting results on the early to mid Holocene glacier retreat and subsequent advances in the late Holocene in the African low latitudes due to temperature changes. So, Africa should be mentioned separately as the pattern is different from the Andes. [Olga Solomina, Russian Federation]	taken into account, text revised and several additional reference added

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18447	72	11	72	11	For the New Zealand here the following referenes migh be appropriate (Schaefer et al., 2009; Putnam et al., 2012; Kaplan et al., 2013). A recent paper (Eaves, S. R., Winckler, G., Mackintosh, A. N., Schaefer, J. M., Townsend, D. B., Doughty, A. M., ... & Leonard, G. S. (2019). Late-glacial and Holocene glacier fluctuations in North Island, New Zealand. Quaternary Science Reviews, 223, 105914.) shows however that the patterns of glacier variations in the first part of the Holocene at the southern and northern islands in New Zealand are different. [Olga Solomina, Russian Federation]	taken into account, thank you for pointing to these publications, text revised with several of suggested references
173	72	11	72	11	See also PATICE (Davies et al. 2020, ESR), where we quantify glacier advances in the Holocene and argue that the LIA extent was smaller than advances at 1-2 ka and 5-6 ka. These advances are more strongly linked to dynamics in the Southern Ocean. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, the reference added to assessment
18441	72	12	72	14	I would recommend to include here the reference of interesting paper by Hall, B. L., Lowell, T. V., Bromley, G. R. M., Denton, G. H., & Putnam, A. E. (2019). Holocene glacier fluctuations on the northern flank of Cordillera Darwin, southernmost South America. Quaternary Science Reviews, 222, 105904. They demonstrated a specific pattern of Holocene glacier fluctuations in Cordillera Darwin different from the New Zealand glaciers (decreased sizes of glaciers in the Early to mid Holocene unlike the New Zealand). [Olga Solomina, Russian Federation]	accepted, reference added to assessment
62083	72	13	72	14	and in some other regions is very vague, which other regions? Are there more regional studies that haven't been cited, hence the vagueness of this statement. Either remove 'some other regions' or specify the regions and cite the study. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised and clarified
69591	72	14	72	15	"Solar and volcanic activity supported by positive feedbacks in the climate system were forcing glacier variability at the decadal and multidecadal frequencies" It is unclear what the positive feedback being referred to are. Could do with a citation. If you are thinking about sea-ice or ocean heat content providing persistence (along the lines of Giford Miller and colleagues have proposed), these are not climate feedbacks, but mechanisms of persistence. Unless you have very specific feedbacks in mind, probably best to avoid the phrase in a report where climate feedbacks has a very specific context! I also think stopping at multidecadal timescales undersells the role of volcanoes. Changes in the frequency of volcanic eruptions from the MWP to the LIA is the leading idea for the changes in glacier length seen between those periods. [gerard Roe, United States of America]	accepted, text revised
22621	72	17	72	19	The prior text has insufficient support for the greenhouse gas cause statement and requires rebalancing so that the trace for this is considerably clearer. [Peter Thorne, Ireland]	accepted, text revised and improved support for summary statement
69593	72	17	72	19	"In summary, there is high confidence that greenhouse gas concentration and orbital forcing are the main drivers of glacier extent on millennial time scale." I do not agree with this, and I think there is a risk of creating confusion about timescales. For instance, the leading idea for the difference between glacier lengths during the medieval warm period and the little ice age (which many would say was multicentennial to nealy millennial timescales) was a change in the fequency of volcanic eruptions. Glaciers are, by themselves, centennial-scale integrators of the climate signal - you do not need explicit external forcing on milennial timscales in order to see glacier response on those timescales. [gerard Roe, United States of America]	accepted, text revised
16415	72	18	72	19	“...one millenial time scaleS” or “...on THE millenial time scale” [Julian Mak, China]	accepted, text revised
3115	72	21	72	23	Would this modelling study based on the Open Global Glaicer Model not be better placed in the next section ("Model evaluation") than the one it is now ("Observed glacier extent and volume changes")? [Daniel Farinotti, Switzerland]	accepted, text revised
61995	72	21	72	23	Is the sentence attributed to Parkes and Goosse, or the authors of the chapter? If it is the former, the opening bracket could move in front of 201. If it is the latter the sentence should be amended to state 'we conclude', or 'it can be concluded'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
69595	72	21	72	35	I'd be careful with this whole paragraph. Many of the papers cited are inferences and interpretations rather than causal demonstrations, but that is not how it reads. In the context of future change, I think it is important to be clear that trends in precipiation are small compated to trends in temperature in terms of their impact on glacier mass balance. There are strong theoretical reasons for the relatively muted few-percent-per-kelvin sensitivity of average precipitation that models and instrumental observations show. [gerard Roe, United States of America]	accepted, text revised
2887	72	22	72	22	Change "(Parkes and Goosse, 2019)" by "Parkes and Goosse (2019)" [Antoine RABATEL, France]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62085	72	24	72	24	Using 'significant interregional variability" suggests a statistical confidence to the statement. Maybe more transparent to remove 'significant' and replace with a liklihood statement. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
3117	72	26	72	26	A short explanation for what "the Heinrich 1 event" is should be added (the chapter did not make any mention of Heinrich events so far). [Daniel Farinotti, Switzerland]	not applicable, text removed
3119	72	29	72	31	Here and elsewhere: The geographical switch from one local study to another (here from the Gichginii Range in Mongolia to Kangerlussuaq in Greenland) makes the text somewhat hard to follow. Not sure how to fix this easily but some actions seems required. [Daniel Farinotti, Switzerland]	accepted, text revised
106229	72	30	72	32	Why is this in the glaciers section? [Frank Paul, Switzerland]	accepted, text removed
2889	72	32	72	32	Consider rephrasing the beginning of the sentence as: "Finally, increased precipitation…" [Antoine RABATEL, France]	not applicable, text removed
61837	72	33	72	33	change Little Ice Age to LIA [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text revised
81455	72	34			Perhaps point to that it is not only the combination or precip and temp, but importantly also the combination of these to with the climate setting/sensitivity of the respective glaciers (continetal, maritime, cold/dry, warm/wet, etc.). In the HMA, for instance, this explains a lot of the regional variablity (Sakai, A. and Fujita, K.: Contrasting glacier responses to recent climate change in high-mountain Asia, Sci. Rep., 7, 13717, https://doi.org/10.1038/s41598-017-14256-5, 2017) . You hint already at that in the paragraph. [Andreas Kääb, Norway]	accepted, text revised
16417	72	40	72	40	Caption for Fig 9.22, "...and projectED volume..." [Julian Mak, China]	accepted, text revised
96999	72	40	72	52	The Figure is described in terms of volume. It could just as well be described in terms of mass (without changing the figure), since it is in percent anyway. This means, it's purely semantics - but I think glacier mass is more relevant that glacier volume, and we would suggest to write mass instead of volume. [Nicole Wilke, Germany]	accepted, figure revised
88709	72	44	72	44	The references Marzeion et al., 2015a and Marzeion et al., 2015b are the same. [Rosemary Vieira, Brazil]	accepted, references combined
116879	72		72		I am surprised by the direct link between forcing and glacier response. The assessment needs to focus on the link between forcing, surface mass balance (also through the effect of forcing on temperature and ablation, and precipitation and accumulation). The paragraph reads like correlation is used to establish causality, which is not common practice in the WGI context (attribution). Also, the text is unclear on drivers of glacier changes in the last centuries (what drove LIA extension, as global temperature changes are attributed to the recurrence of major volcanic eruptions in ch 1 and 2). These parts need to be sharpened (and I do not think that the literature cited provides support for the conclusion about forcing and response). For instance, GHG concentration is not the only factor involved durng the last deglaciation (role of ice sheet albedo). The statements related to precipitation could build on the assessment in chapter 8 for improved coherency. [Valerie Masson-Delmotte, France]	taken into account, text revised and improved coherency between chapters in the report improved
97001	73	1	73	1	The heading does not fit the content of the section well, which doesn't really discuss evaluation of models, but rather the state-of-the-art of global models. Please modify. [Nicole Wilke, Germany]	accepted, text revised and now fits the heading better
3431	73	3	73	3	Strong statement to link the challenge of global glacier modelling to accurate ice thickness data. This is indeed one of the important bottlenecks, but as it is formulated here, it sounds almost like it's the only problem. Could potentially consider also referring to the uncertainty in mass balance data at the individual glacier level (to which the prognostic glacier models are typically tuned) and limitations linked to computational resources. Especially the recent improvements in estimating the glacier mass balance at the individual level may be worth mentioning somewhere in this paragraph (l.3-l.22 and in l. 29-30, where model uncertainties are also addressed), as this has been a major improvement for future glacier projections. For this, you could refer to some of the seminal studies that are (mostly) also mentioned earlier in this chapter: e.g. Brun et al. (2017, Nature Geoscience, doi: 10.1038/NGEO2999), Braun et al. (2019, Nature Climate Change, doi: 10.1038/s41558-018-0375-7), Dussaillant et al. (2019, Nature Geoscience, doi: 10.1038/s41561-019-0432-5) and Shean et al. (2020, Frontiers in Earth Science, doi: 10.3389/feart.2019.00363). [Harry Zekollari, Belgium]	taken into account, text revised, suggested references included in assessment
26513	73	3	73	4	Thickness data are indeed essential for global scale model calibration and initialization. However, equally important for accurate global scale modelling are other observational datasets including glacier outlines, elevations, ice velocities, surface mass balance, and glacier mass change (GRACE). [Ward van Pelt, Sweden]	taken into account, text revised
97003	73	3	73	4	The sparsity of long-term mass change observations is a far more significant limitation for model calibration than that of ice thickness measurements. Please modify. [Nicole Wilke, Germany]	taken into account, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3121	73	3	73	33	This whole section is virtually based on two publications only (Hock et al, 2019, and Marzeion et al., submitted). This should probably be clarified in one sentence saying that, after all, there are not very many global-scale glacier projection studies in the literature. [Daniel Farinotti, Switzerland]	accepted, text revised to clarify this
81457	73	3	73	33	Another important effect that is not in the models are dynamic instabilities. Surges etc., in particular of tide-water glaciers, can have massive effect on regional mass balance (Dunse T., Schellenberger T., Hagen J.O., Kääb A., Schuler T.V. and Reijmer C.H. (2015): Glacier-surge mechanisms promoted by a hydro-thermodynamic feedback to summer melt.The Cryosphere, 9, 197-215) or (Willis et al. Massive destabilization of an Arctiv ice cap. https://doi.org/10.1016/j.epsl.2018.08.049). There is also a paper from Geir Moholdt on that soon out. And under current climate some of these may actually irreversible (Nuth, C., Gilbert, A., Köhler, A., McNabb, R., Schellenberger, T., Sevestre, H., Weidle, C., Girod, L., Luckman, A. and Kääb, A. (2019): Dynamic vulnerability revealed in the collapse of an Arctic tidewater glacier. Scientific Reports, 9, 5541) [Andreas Kääb, Norway]	taken into account, the suggested publications were assessed in SROCC, text is revised, not space for much discussion of dynamic instabilities
3433	73	4	73	4	improved inventories': indeed. But for someone who's not familiar with the field, this will likely sound like inventories linked to the glacier thickness, while the first reference (RGI Consortium, 2017a) refers to glacier outlines (which are of course also important to better delineate the glacier geometry and infer their volume). Could maybe consider specifying this? e.g. 'With improved inventories on glacier outlines and ice thickness (RGI Consortium, 2017a; GlaThiDa Consortium, 2019)...' [Harry Zekollari, Belgium]	taken into account, text revised
129501	73	8	73	8	The contribution from the Arctic and Antarctic regions is quoted as 67% of the total. This exact fraction did not appear anywhere. A reference for this value should be given. [Trigg Talley, United States of America]	taken into account, text revised (page 74 not 73)
61843	73	8	73	9	what does phase 1 and phase 2 refer to? What is the difference between the phases other than the input models [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text revised
3441	73	14	73	15	This statement is only valid for RCP2.6. Was not clear to me at first, and because RCP2.6 is only mentioned at the end of the sentence (between brackets), it is not entirely clear whether this refers to the entire sentence or only to the last thing that is mentioned ('stabilizing mass balance'). Could potentially avoid confusion by rewriting as: 'The peak in mass loss rate followed by a reduction under RCP2.6 is due to...' [Harry Zekollari, Belgium]	taken into account, text revised (page 74 not 73)
97005	73	17	73	18	Please note that Sakai & Fujita is also based on energy balance calculations. [Nicole Wilke, Germany]	accepted, reference added and text revised
3445	73	19	73	23	Nice summary of features of projections in 4 main points. Potentially, a fifth point for which there is much evidence and that could be added: '(5) a future glacier loss will persist in the coming decades, irrespective of the projected warming, due to the present-day imbalance between glacier geometry and present climatic conditions (i.e. a committed loss)': this is for instance very clear from the seminal study by Marzeion et al. (2018, Nature Climate Change, doi: 10.1038/s41558-018-0093-1). [Harry Zekollari, Belgium]	taken into account, text revised
26409	73	20	73	20	Check parenthesis:(JULES (Shannon et al., 2019)) [María Santolaria-Otín, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
97009	73	20	73	21	Please note that Slangen/van de Wal does not really account for retreat and advance (in the sense that in their model, terminus elevation is independent of glacier size; the only "response" is when a glaciers melts completely). [Nicole Wilke, Germany]	noted, text revised
61861	73	20	73	22	I find it confusing that the models and different types of models are mentioned at different parts in the paragraph. Is the term "all models account for glacial retreat…" related to all models of GLacierMIP in general or Phase 1 or Phase 2?could this be moved to line 10 after "279 simulations contributed to the intercomparison. " [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, text clarified
97007	73	20	73	23	Are there any specific results from the models to be reported or a reference to a latter section to be made? [Nicole Wilke, Germany]	noted, reference made, text revised
2065	73	22	73	22	When stating the inclusion of ice dynamics, the consideration of the process of frontal ablation (calving of marine-terminating glacier fronts) has only been accounted for in one of the global models (Huss&Hock, 2015). [Matthias Huss, Switzerland]	noted, text revised
3443	73	22	73	22	will depend on the emission scenario': yes, this is true, but it now sounds like this is the only driver. May be worth reformulating as: 'depend strongly on the...' [Harry Zekollari, Belgium]	accepted, text revised (page 74 not 73)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3447	73	23	73	26	Studies that have been run for a longer time period are mentioned here. Would be good to be more specific, as it is not clear to which time periods this refers (which is especially important because numbers are given!). Could consider referring to 'Beyond the end of the 21st century' (with which you refer to the period for the 'classic studies' and then give the years for the numbers (e.g. 'by 2300'). [Harry Zekollari, Belgium]	accepted, text revised (page 74 not 73)
26511	73	24	73	25	Another limitation of global glacier models is that they typically do not account for transient basal conditions that may for example arise from surface melt induced changes in subglacial drainage morphology. Also not mentioned here are 1) albedo changes due to shrinking accumulation zones, which I suppose not all models account for, and 2) potential trends of refreezing and liquid water storage in firn. [Ward van Pelt, Sweden]	noted, text revised, not possible to add details of calving due to space limitations
2067	73	24	73	33	This paragraph suffers from a lack of references (in comparison to other paragraphs). Probably the inclusion of the process of frontal ablation at marine-terminating glacier fronts should also be mentioned here [Matthias Huss, Switzerland]	accepted, text revised, reference added
3437	73	24	73	33	Another major source of uncertainty is related to glacier calving. This is extremely challenging to incorporate at the individual glacier level due to a lack of observations (to which the models could be calibrated or evaluated) (see e.g. for a rare detailed dataset by McNabb et al. (2015, Journal of Geophysical Research, doi: 10.1002/2014JF003276)) and due to the fact that the frontal glacier zones have a large uncertainty when it comes to the ice thickness reconstruction (which strongly affects calving rates): see e.g. Recinos et al. (2019, The Cryosphere, doi: 10.5194/tc-13-2657-2019). [Harry Zekollari, Belgium]	noted, text revised, not possible to add details of calving due to space limitations
100043	73	24	73	33	Should mention: low-latitude ablation processes not captured in melt models; microclimatic/orographic warming effects that uniquely affect mountain glaciers are not accounted for [Forrest Schoessow, United States of America]	noted, text revised not possible to add all suggested details due to space limitations
129503	73	24	73	33	[CONFIDENCE] There is no comprehensive and systematic assessment of the uncertainties resulting from these parameterizations. This makes it difficult to fairly assess the modeling fidelity. Yet, in many places, the statement over the modeling assessment is assigned at high confidence. This appears a logical inconsistency. [Trigg Talley, United States of America]	accepted, text revised, new reference about partitioning of uncertainties on global glacier projections models added and additional estimated (parametric fit and emulator results) compared in table 9.4
3435	73	28	73	28	Downscaling to the spacial scale of the glacier: could potentially add that this is extremely challenging, as has been shown in studies such as Jarosch et al. (2012, Climate Dynamics, doi: 10.1007/s00382-010-0949-1) and Vionnet et al. (2019, Frontiers in Earth Science, doi: 10.3389/feart.2019.00182) [Harry Zekollari, Belgium]	noted, text revised
97011	73	32	73	33	Please note that formal attribution is possible for glacier mass change (discussed in Ch3), just not for glacier geometry change. [Nicole Wilke, Germany]	accepted, text revised and reference to Ch3 added
81459	73	33	74	34	See my above comment. Also for projections the uncertainty of dynamic instabilities and their potentially accelerating influence should be mentioned. Also for the observations section of the glacier section a short statement on dynamics might be useful. [Andreas Kääb, Norway]	accepted, text revised, not possible to add much text about dynamic instabilities due to space limitations
41467	73	36			While the challenges and uncertainties involved in deriving a global glacier SLR contribution for the 21st century are appreciated, this section would still need a figure comparable to the other display items of major SLR contribution shown in this chapter (figures 9.19 and 9.20), basically an improved SROCC SPM figure 2 panel g. Please add if possible! [Alexander Nauels, Germany]	accepted, Figures 9.22 and 2.21 show this updated glacier contribution
97013	73	39	73	41	Some of the regional studies cited here are also included in GlacierMIP 2, and some regional studies that took part in GlacierMIP 2 are not mentioned. It would be good to homogenize the treatment (separate citation or not) for studies that took part in GlacierMIP. [Nicole Wilke, Germany]	accepted, text revised and references added
2891	73	42	73	42	Consider adding Réveillet et al., 2015 to the references in brackets. [Réveillet, M., A. Rabatel, F. Gillet-Chaulet, A. Soruco. 2015. Simulations of changes in Glaciar Zongo (Bolivia, 16°S) over the 21st century using a 3D full-Stokes model and CMIP5 climate projections. Annals of Glaciology, 56 (70), 89-97. doi: 10.3189/2015AoG70A113.] [Antoine RABATEL, France]	accepted, reference added to assessment
93567	73	44	73	46	Three of these regions (Western Canada and US, North Asia and Scandinavia) are unlikely to lost all their glacier mass even under RCP8.5. The lower limit of the RCP8.5 curves for these three either don't or barely touch the 0% value, and with the glacier thickness for the biggest glacier masses in these regions, it's difficult to to envisage an average of 10 m or more of ice melt from now to the end of the century, that is required to reach zero glacier mass. [Miriam Jackson, Norway]	noted, text revised
3125	73	46	73	46	When referring to RCPs: possibly worth mentioning that SSPs have not yet made it into published glacier change studies? [Daniel Farinotti, Switzerland]	accepted, the results of emulator that translates RCP forced results to SSPS is applied (Edwards et al., in review) and presented in table 9.4

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61847	73	47	73	47	sea level rise change to SLR [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	editorial, revised
16419	73	47	73	49	I think this needs a comma to separate out the two clauses ("...between glacier models, and the emmision pathway...")? [Julian Mak, China]	accepted, comma added, text revised
22623	73	47	73	49	This sentence was very unclear and took several reads to I think comprehend. It would be better to have two sentences - one about model uncertainty dominating before mid-century. The second about emission pathway uncertainty dominating at the end of the century. [Peter Thorne, Ireland]	noted, text revised
32169	73	47	73	49	Sentence is difficult to parse. Better split it or use a conjunction, e.g. while. [Anja Wendt, Germany]	noted, text revised
62045	73	47	73	50	Sentence does not make sense as written (seems to provide two competing explanations, disagreement between models and uncertainty in emission pathway, as the primary source of uncertainty in glacier contribution to SLR) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	noted, text revised
2893	73	48	73	48	Consider rephrasing: "[...] is the disagreement between glacier models, and the [...]" [Antoine RABATEL, France]	accepted, text revised
129505	73	48	73	48	Grammar. Should be "... in disagreement between glacier ..." [Trigg Talley, United States of America]	noted, text revised
15551	73	51	73	51	The projections of 75 mm SLE (RCP2.6) and 165 mm SLE (RCP8.5) do not tally with the corresponding projections shown in Table 9.3 (P.69). Please check and revise as appropriate. [SAI MING LEE, China]	accepted, numbers revised and made consistent
32171	73	51			What is the basis for the percentages? The year 2016? 100% would give 440 mm, larger than the total glacier volume in Table 9.3 [Anja Wendt, Germany]	accepted, text revised
3439	73	53	73	54	two new models with lower overall climate sensitivity': may be misinterpreted. Typically when referring to climate sensitivty, one refers to the change in temperature as a results of a doubling of CO2 in climate models. Therefore, it is in general recommended to not refer to 'climate sensitivity', but more specifically referring to the 'sensitivity of a glacier model to changing climatic conditions'. A potential reformulation of the sentence could be as follows (also specifying that you are referring to the glacier models and not the climate models used to force them): '...due to the inclusion of two new glacier models with a lower sensitivity to changing climatic conditions than the previous model ensemble'. [Harry Zekollari, Belgium]	accepted, text revised
61845	73	54	73	54	Figure 9.22 was already introduced in more detail in this paragraph so delete here [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	accepted, text deleted
61849	74	8	74	8	sea level rise change to SLR [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
175	74	8	74	8	Is Alaska included in the 'Arctic and Antarctic regions'? This is a substantial driver of SLR (Zemp et al. 2019) [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	noted, yes Alaska is included, text revised to clarify
88033	74	8	74	8	add "from glaciers in" (necessary to say since the Arctic and Antarctic regions contribute also through their ice sheets). [Georg Kaser, Austria]	accepted, text revised
9023	74	8	74	9	These glaciers do not belong here, they belong to Antarctica and Greenland, that's double counting. [Eric Rignot, United States of America]	rejected, the peripheral glaciers in Greenland and Antarctica are included in this section, ice sheets in 9.4
97015	74	8	74	15	It is worthwhile to point out that the regions that are projected to contribute most to sea-level rise tend to be those with particularly sparse data coverage from observations, and particularly great disagreement (i.e., uncertainty) between glacier models (particularly Antarctica & Subantarctic). [Nicole Wilke, Germany]	noted, text revised
61851	74	9	74	9	smallest relative loss: relative loss in which sense [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	noted, text clarified
61853	74	12	74	13	so far a-1 was used for "per year" now yr-1. Stay consistent [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
22625	74	12	74	14	Does this really mean that RCP2.6 has higher melt than RCP4.5. How can this be so? Or is the analysis not considering all 4 RCPs? If so this needs to be stated explicitly up front. This is very confusing otherwise. [Peter Thorne, Ireland]	accepted, text revised
100045	74	14	74	15	Perhaps this would be a good place to mention that retreat can promote negative (stabilizing) mass balance feedbacks that can drive positive mass balance signals while volume is still being lost. [Forrest Schoessow, United States of America]	noted, not clear what is meant here as stabilizing feedback resulting in positive mass balance is not detected in the projections, text is considerably revised
3127	74	17	74	17	Simplify to "There is medium confidence in the projected rates of mass loss (Section 9.5.1.2)". [Daniel Farinotti, Switzerland]	accepted, text revised
97017	74	17	74	23	Perhaps it is possible to distinguish between the quantitative results (medium confidence) and qualitative results (high confidence) in order to improve readability? [Nicole Wilke, Germany]	accepted, text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62047	74	17	74	26	There is a strong focus here on glacier contribution to SLR but it might be worthwhile to also add context for social implications (e.g. declining access to fresh water sources by the end of the 21st century?) as briefly touched on in the intro to section 9.5. unless this is covered elsewhere in the IPCC [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	noted, this is discussed in Ch12, reference made to sections, not included in Ch9
22627	74	17	74	26	The whole paragraph feels repetitious with earlier text. I would suggest merging and reconciling to avoid this. [Peter Thorne, Ireland]	noted, text revised
3129	74	20	74	20	Is "a largest" meant to say "the largest"? [Daniel Farinotti, Switzerland]	accepted, text revised
3131	74	22	74	23	it seems useful to turn this sentnece around, i.e. to say that glacier mass loss until 2050 is largely independent of the climate scenario. [Daniel Farinotti, Switzerland]	accepted, text revised
3133	74	22	74	23	Possibly worth re-stressing or mentioning the concept of "committed mass loss"? Those estimates are clearly model-based as well. [Daniel Farinotti, Switzerland]	accepted, text revised
2895	74	23	74	26	Is this sentence useful? You are mention longer simulations, millenial time scale in Levermann et al. (2013), but finally mention results that are for the 21st century and coming from Marzeion et al. (2012) when recentmost updates do exist. Well, I would remove this or completely rephrase. [Antoine RABATEL, France]	accepted, text completely rephrased
32173	74	23	74	26	Give a duration or a year for the longer simulations. [Anja Wendt, Germany]	accepted, text revised
97019	74	23	74	26	Marzeion et al. (2018, DOI: 10.1038/s41558-018-0093-1) might be a better (or additional) reference here. [Nicole Wilke, Germany]	accepted, text revised, suggested reference added to assessment
88035	74	24	74	24	ExSum should not take a single paper result, particularly not if numbers are so wideley spreading. (see respective comment on ExSum) [Georg Kaser, Austria]	accepted, text revised, now both parametric fit to results and emulator added to table 9.4
16421	74	28	74	28	“It WAS assessed...” [Julian Mak, China]	accepted, text revised
3135	74	28	74	28	Simplify to "The projected changes in glacial meltwater runoff vary greatly (Chapter 8)". [Daniel Farinotti, Switzerland]	not applicable, text removed
177	74	28	74	28	See also Immerzeel et al. (2020) who discuss how climate change will affect run off from global mountain glaciers. [Bethan Davies, United Kingdom (of Great Britain and Northern Ireland)]	noted, reference added to assessment, the runoff is not topic of Ch9
3137	74	28	74	34	This paragraph basically defers the reader to Chap. 8. The problem is that Chap. 8 has only ca. 27 lines of text (Subsection 8.4.1.7.1) dedicated to glaciers and that a good third of those lines are based on one single study (Huss and Hock, 2018). Besides the fact that the corresponding subsection should be mentioned directly, the literature has probably more to offer. [Daniel Farinotti, Switzerland]	noted, text in Ch8 and Ch9 better coordinated and further studies added to assessment
106231	74	31	74	31	Maybe add a citation here? [Frank Paul, Switzerland]	noted, text revised
106233	74	34	74	34	Maybe add a citation here? [Frank Paul, Switzerland]	noted, text revised
66417	74	37	78	4	I think that section 9.5.2 is an excellent assessment of the physical permafrost response to climate change. [Charles Koven, United States of America]	Noted. Thank you!
104431	74	37	78	4	Permafrost. Section [9.5.2] is referred to in Traceback matrix TS.A.1 (TS-160) for justifying ‘High confidence of decrease’ for Permafrost in West Antarctica (Ch12, Table 12.11/Table TS.20/SPM Box3), however there is no discussion or references about future projections in Antarctica. For recent trends, it says (9-75, L55-p.76, L1) “Records of Antarctic ALT changes are too limited for an assessment of long-term trends (Hrbáček et al., 2018). For future projections it only states (9-77, L53-55:) “p.77, L53-55: In summary, based on high agreement across CMIP6 and older model projections (see AR5), fundamental process understanding, and paleoclimate evidence, it appears virtually certain that near-surface permafrost extent will shrink as global climate warms.” Either there should be more references/discussion added to [9.5.2] with a particular focus on the Antarctic Peninsula or changing the confidence level for Permafrost in West Antarctica in Table 12.11/Table TS.20/SPM Box3. [Irina Gorodetskaya, Portugal]	Noted. Tight word count constraints, the shortness of the West Antarctic permafrost temperature records, and consistency with the Chapter 2 assessment (little to say about Antarctic permafrost trends) led us to cut reference to Antarctic permafrost trends in this section.
15213	74	37			Organization note: This Chapter team should consider discussing with other chapters where assessments of lake and river ice belong. It does not appear to be covered in WG1 at all, even though lakes, wetlands and rivers with seasonal ice cover a signficant portion of the northern hemisphere, and have shown signals of physical change (reduced ice season, warming, stratification changes, etc.). If the author teams prefer to leave this to WGII, it should be clearly communicated, else it may fall through the cracks. [Simon Donner, Canada]	Noted. Lake and river ice is taken up in Ch 12.
61863	74	42	74	42	Chapter 5 (Section 5.4.3.3)? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Has been implemented.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40763	74	45			section 9.5.2.1: improvements since latest report not necessarily clear [TSU WGI, France]	Accepted. There are no substantial improvements. We therefore write in the introductory paragraph of this section: "There were no new findings leading us to reconsider SROCC assessments concerning Arctic and mountain permafrost." SROCC is more clearly summarized in each paragraph treating a new variable.
1769	74	47	74	54	Section 9.5.2 indicates substantive loss of permafrost with ongoing global warming. It would be valuable to discuss the projected percentage loss of permafrost in the Northern Hemisphere and the global extent of permafrost with warming climatic conditions in the 21st century. [Michael Kennish, United States of America]	Noted. Expected permafrost decrease as a function of future warming levels is discussed in 9.5.2 (in terms of frozen soil volume). In response to earlier comments and other reviewer SOD comments, we think it is more relevant to show the volume of frozen soil to 3 meters depth than the simulated "extent of permafrost" which is systematically criticized as misleading, because fossil permafrost can remain at greater depth for long periods. Moreover, "near-surface permafrost extent" behaves very similarly to the frozen soil volume in the top 3 meters, so a specific discussion of this would be redundant.
82963	74	48	74	48	I suggest to move the three-word explanation of permafrost "(perennially frozen ground)" to line 39 on this page (start of section 9.5.2). [Sebastian Gerland, Norway]	Accepted. Implemented as suggested by the reviewer.
11393	74	49	74	49	The term "unglaciated" is not clearly defined. It could probably mean an area that has never been glacier-covered. A better term would be "deglaciated". [Jacob Clement Yde, Norway]	Noted. We now use "unglacierized". "Glacierized" is the correct term following the WGMS terminology (https://wgms.ch/downloads/Cogley_etal_2011.pdf): "Glacierized Of a region or terrain, containing glaciers or covered by glacier ice today. See glaciated, which refers to past coverage."
88265	74	49	74	49	remove "covers" --- permafrost underlies the land it does not cover it (unless you are referring to the permafrost regions) [Sharon Smith, Canada]	Accepted. Implemented as suggested.
39689	74	49		50	" 50% areal fraction of unglaciated land north of 60°N." what do you mean? This phrasing is maybe a bit convoluted. [TSU WGI, France]	Accepted. We simplified the wording. Now simply: "... and more than 50% of the unglacierized land north of 60°N."
81165	74	49			"22%" is wrong! This number refers to the permafrost region, not permafrost area (see Obu et al. 2019). Either use around 15% (or 14%), or exchange "underlies" by "affects", but the first correction would be better. [Andreas Kääb, Norway]	Accepted. Has been changed to 15%.
11395	74	51	74	51	The term "unglaciated" is not clearly defined. It could probably mean an area that has never been glacier-covered. A better term would be "deglaciated". [Jacob Clement Yde, Norway]	Noted. See response to comment #11393 (same comment by same reviewer on the same term, but two lines above)
99675	74	51	74	52	Please change: "... and unglaciated areas in Antarctica" to " and in most unglaciated areas in the Antarctic (Vieira et al. 2010; Obut et al. (2000)". The references are the following: Vieira, G., Bockheim, J., Guglielmin, M., Balks, M., Abramov, A.A., Boelhouwers, J., Cannone, N., Ganzert, L., Gilichinsky, D.A., Goryachkin, S., López-Martínez, J., Meiklejohn, I., Raffi, R., Ramos, M., Schaefer, C., Serrano, E., Simas, F., Sletten, R., Wagner, D. 2010 - Thermal State of permafrost and active-layer monitoring in the Antarctic: advances during the International Polar Year 2007-09. Permafrost and Periglacial Processes, 21(2): 182-197. and Obu, J., Westermann, S., Vieira, G., Abramov, A., Balks, M., Bartsch, A., Hrbacek, F., Kaab, A., & Ramos, M. (2020). Pan-Antarctic map of near-surface permafrost temperatures at 1 km2 scale, The Cryosphere, 14 (2): 497-519. doi.org/10.5194/tc-14-497-2020. [Goncalo Vieira, Portugal]	Accepted. Implemented as suggested.
61867	74	53	74	53	ground ice volume: how does ground ice volume link to permafrost? This needs to be linked more obviously [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We added: "Ground ice volume in permafrost is variable, reaching up to 90% in syngenetic permafrost deposits (Kanevskiy et al., 2013; Gilbert et al., 2016)."
100443	74	53	74	54	Ground ice content is an important characteristic of permafrost, the estimation here maybe a little out of time as permafrost degradation over the past two decades, are there any model results about ground ice content？ [Huiru Jiang, Sweden]	Noted. Unfortunately we do not know of any model results indicating global permafrost ice content.

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88267	74	53	74	54	This estimate is based on one reference that is pre AR5 so there is nothing really new here. This estimate is based on the ice contents given on the Brown et al. map which was based on limited observations. I'm not sure how useful this statelemnt is to the assessment. [Sharon Smith, Canada]	Noted. Although it is indeed not new at all, it is the only existing global estimate and relevant because it provides a sea-level equivalent. It was also assessed in SROCC.
61869	74	54	74	54	change sea level rise to SLR [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We would then have to explain what SLR means because people might read only this section, not the whole chapter. BTW we prefer to simply write "sea level" (dropping "rise").
116881	74		75		I have the impression that the chapter is re-assessing literature already assessed in SROCC, what is new, and does it support SROCC findings? I think that the confidence level should be revisited (also building on SROCC). Check chapter 3 confidence for links between Arctic warming and human influence. It is virtually certain that thaw is driven by warming. [Valerie Masson-Delmotte, France]	accepted, text revised, relation to SROCC and Ch3 made clearer
116883	74		75		Could the chapter assess literature related to abrupt permafrost thaw? [Valerie Masson-Delmotte, France]	accepted, text revised
61871	75	1	75	1	"There are consistent..." consistent in time? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We mean "consistent" in terms of "all showing the same". The text is revised accordingly.
4233	75	1	75	5	I object to the term 'medium confidence', this qualification is very poorly underpinned here. 'High confidence' is better justified. First, because of the abundant regional evidence of thawing permafrost and loss of permafrost cited in Chapter 12. Second, 'medium confidence' is at variance with the references cited here, which all mention substantial permafrost loss. Furthermore, what is meant with 'geographically uneven'? Is it lack of evidence in some regions because of underrepresentation in scientific literature, or regional evidence that permafrost is stable or aggrading? In the latter case, the term 'medium confidence' would be justified. However, it is clearly a matter underreporting - all references but one, refer to North America, nothing from the vast area of Siberian permafrost! There should be more on Eurasian permafrost. One useful reference on Western Siberia: Kirpotin S, Polishchuk Y, Bryksina N, Sugaipova A, Kouraev A, Zakharova E, Pokrovsky OS, Shirokova L, Kolmakova M, Manassypov R, Dupre B (2011) West Siberian palsa peatlands: distribution, typology, cyclic development, present day climate-driven changes, seasonal hydrology and impact on CO2 cycle. International Journal of Environmental Studies 68 (5):603-623. doi:10.1080/00207233.2011.593901. I suggest: 1) to add the reference of Kirpotin et al; 2) to change the text 'Since observations are geographically uneven, there is only medium confidence that permafrost loss is a pan-Arctic phenomenon, although there has been a widespread Arctic warming in recent decades. In spite of increasing evidence of landscape changes from site studies and remote sensing, quantifying permafrost extent change in the Arctic remains challenging (e.g., Jorgenson and Grosse, 2016).' into: Despite geographically uneven observations and challenges in determining permafrost extent (e.g., Jorgenson and Grosse, 2016), there is high confidence that permafrost loss is a pan-Arctic phenomenon given the references cited here and in Chapter 12. ' [Jacobus (Ko) van Huissteden, Netherlands]	Noted. The problem is that most references concerning permafrost thaw in Chapter 12 refer to active layer thickening, not to complete permafrost loss. Our text was probably not sufficiently clear on the fact that here we only talk about permafrost horizontal extent, not active layer thickening or degradation limited to near-surface permafrost. It's really about reports of observations of complete permafrost loss, which are considerably more elusive than observations of surface permafrost degradation, justifying the lower confidence level. The revised text states this more clearly, but also mentions coastal permafrost retreat, which is a form of complete loss.
61873	75	3	75	3	e.g. before Borge et al 2017 needs to be removed [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. "e.g." has been deleted.
23759	75	7	75	7	this is also thematized in detail in global monitoring context in Trofaier et al. 2017 http://dx.doi.org/10.1016/j.rse.2017.05.021 [Annett Bartsch, Austria]	Accepted. "e.g." has been deleted in front of Jorgenson and Grosse 2016, and a reference to Trofaier et al. has been added.
83563	75	7	75	9	A new speleothem based permaforst study for North America during the penultimate and last glacial cycle study was recently published by Batchelor, C.J., Orland, I.J., Marcott, S.A., Slaughter, R., Edwards, R.L., Zhang, P., Li, X., Cheng, H., 2019. Distinct Permafrost Conditions Across the Last Two Glacial Periods in Midlatitude North America. Geophysical Research Letters 46, 13318-13326, doi: 10.1029/2019GL083951. Given the dearth of paleo-evidence it might be worth to include here, at least as reference. [Antje H. L. Voelker, Portugal]	Accepted. We add a sentence indicating that the apparent high sensitivity of permafrost could be a consequence of high regional-scale variability, as the study of Batchelor et al suggests. Bachelor point out "globally integrated climate reconstructions are important but do not illuminate regional and hemispheric heterogeneities across glacial-interglacial cycles". So the inference by Vaks et al. on high sensitivity is to be used with caution.

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61751	75	10	75	13	Not yet published, but more speleothem-based permafrost reconstructions have been produced by Jeremy Shakun from Alaska, the Yukon, the Northwest Territories, and British Columbia. Data presented at the AGU Fall Meeting, 2017 (abstract #PP23E-07). Additionally, Anton Vaks extended his reconstruction back to 1.1 Ma using U-Pb dating and presented the data at the EGU 2014 General Assembly (Geophysical Research Abstracts, V. 16, EGU2014-7027) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Unfortunately these publications cannot be taken into account as they are either not published or not peer reviewed.
88269	75	10	75	13	Stetement unclear - do you mean the entire area north of 60N would be free of permafrost or just part of it, more southerly region? In the mid Holocene there were areas that were free of permafrost that currently have permafrost --- would this be a similar conclusion to the statement presented here? [Sharon Smith, Canada]	Noted. We cut this part of the sentence, which is confusing.
39839	75	10		11	"suggests that climatic conditions only about 1.5°C warmer than the preindustrial" do you mean local/regional or global warming? [TSU WGI, France]	Noted. This is global temperature, as Vaks et al. note in their conclusion. We now write "global climatic conditions".
62087	75	11	75	11	vague language used - 'only about 1.5 warm". If the evidence is limited then maybe a range rather than the use of 'about' is more useful here. The vaguness of this statement and the low conifidence applied to it mean it is not very useful at present. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. 1.5°C warming, on the global scale, is an important marker. We agree that the evidence is not extremely robust and we do complement this statement by a reference to the paper by Batchelor et al, which indicates that regional-scale variability, even on these long time scales, is important to take into account and can reduce the global-scale relevance of the finding on local permafrost changes.
61875	75	11	75	11	warmer than the preindustrial: add the temperature right now as a reference [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This information is given at other, more relevant place in this report and we think that this could unnecessarily increase the length of the statement, and make it more difficult to follow.
62089	75	15	75	16	...with few exceptions'. What are the exceptions, include an example with a reference so the reader can look at the sites where there are exceptions and think about why that may be. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The revised Figure 9.23, based on the openly available data by Biskaborn et al. 2019, shows that slight cooling trends have been seen at a few locations in various regions in warm permafrost. The revised text refers to Biskaborn et al. data so readers can exactly identify these points. In the revised version we do not explicitly state that there were a few exceptions because this is now obvious from the figure.
62051	75	15	75	34	limited discussion of Antarctic data, but referenced in the final summary. Are there sites in the southern hemisphere that can be added to the above discussion? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Tight word count constraints, the shortness of the West Antarctic permafrost temperature records, and consistency with the Chapter 2 assessment (little to say about Antarctic permafrost trends) led us to cut reference to Antarctic permafrost trends in this section. The revised final sentence therefore does not mention Antarctic permafrost trends.
61877	75	16	75	16	add temperature behind "reaching record high temperatures ()" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Why should we add the word "temperature" behind the word "temperature"?
88271	75	18	75	18	This could be updated to Romanovsky et al. 2019, most recent BAMS State of Climate report published (could potentially update to report to be published in late summer 2020) [Sharon Smith, Canada]	Accepted. Updated to the 2018 State of the Climate Report, published in 2019.
62049	75	18	75	20	...near the depth of zero annual amplitude is unclear. Is it that the annual amplitude has increased at a depth of zero? Suggest restructuring/rephrasing this sentence [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The depth of zero annual amplitude is a standard variable in permafrost studies. Readers who do not know why it is relevant can easily go to the paper by Biskaborn, cited in this sentence, where this is explained.
82965	75	23	75	23	I suggest to reword "or so" in connection with temperature (range). If possible, I suggest to include a specific range. [Sebastian Gerland, Norway]	Accepted. Replaced by "about -4°C". This can be easily seen in the figure 9.23
3945	75	23	75	23	(below about -4°C) [Sabine Baumann, Germany]	Accepted. Implemented as suggested.
88273	75	23	75	23	Be clear that -4°C refers to permafrost temperature -- e.g. "....paticularly where permafrost is cold (<-4°C)..." [Sharon Smith, Canada]	Accepted. Implemented as suggested.

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82967	75	25	75	25	I suggest to detail what is meant with "closer to", giving for example quantitative information/a temperature range on what is meant here (how close to 0 deg C). [Sebastian Gerland, Norway]	Noted. As there is an obvious link to the previous sentence, and the revised Figure 9.23 is pretty clear, we now just write "Warmer sites tend to show weaker…"
106235	75	26	75	26	melting the ice': shoudn't this be thawing here? [Frank Paul, Switzerland]	Noted. We think that "melting ice" is OK here. A frozen pizza thaws. Ice melts.
88275	75	27	75	30	These 2 sentences could be combined to make the text shorter: "Mountain permafrost temperature trends are heterogeneious, reflecting variations in local conditions such as topography, surface type and snow cover, but generally weaker warming rates are observed in warmer permafrost at temperatures close to 0°C (Noetzlie et al., 2019; Mollaret et al., 2019, PERMOS, 2019, Biskaborn et al. 2019)". The latent heat effects associated with warm permafrost has already been mentioned earlier in this section so do not need to be repeated. [Sharon Smith, Canada]	Accepted. Implemented as suggested.
100445	75	28	75	34	As for mountain region (like Tibetan Plateau), soil properties usually show high variations throughout the soil column, while thick snow cover may not persist for a long period, so maybe the influence of soil texture should be mentioned here. [Huiru Jiang, Sweden]	Accepted. Added "soil texture".
26411	75	32	75	32	between 2007 and 2016 (medium confidence). In summary,…-> medium confidence in italics [María Santolaria-Otín, France]	Noted. Deleted "medium confidence" here because a range is given.
88277	75	33	75	33	Antarctic has not been mentioned in the discussion earlier in this paragraph and seems a bit out of place here. There are some observations for the Antarctic but the records are limited in length so we can't make strong conclusions about trends - see chapter 2. [Sharon Smith, Canada]	Noted. That's exactly what we say. The rather detailed description of local and regional-scale permafrost changes in the global chapter 2 forces us to reduce the description of regional changes to a bare minimum to reduce redundancies. Therefore Antarctic temperature changes were not mentioned in detail here, except to say that there are not many. However, we do need to refer to mountain permafrost and Antarctic permafrost to justify only medium confidence in our assessment. We now explicitly refer to Ch 2 for Antarctic permafrost trends.
88279	75	39	75	41	Caption for Fig 9.23 - These regions are the same ones used in the SWIPA 2017 report and are based on the pilot regions for the AMAP Adaptation Actions for a Changing Arctic (AACA) assessments. See map at https://aaca.amap.no/#:~:text=Geographical%20coverage,just%20indicates%20the%20region%20boundaries.The regions do include more inland areas so not limited to coastal region. Note also the region is Bering/Chukchi/Beaufort Region so a little broader than just the Beaufort Sea. [Sharon Smith, Canada]	Noted. Figure 9.23 has been modified. It is now based on the Biskaborn et al. (2019) data.
88281	75	49	75	49	Trends are updated to 2018 in Romanovsky et al. (2019) - latest published state of climate report, and are also updated to 2019 in the report to be released later this summer. [Sharon Smith, Canada]	Accepted. Referring to SOTC 2018 now.
101919	75	49	75	50	"... report widespread, but non-uniform, ALT increases across the Northern Hemisphere permafrost zone. " - this ALT increase entails potential risks of massive Hg release into the environment from thawing permafrost, which stores more mercury than other soils, the ocean and the atmosphere combined (e.g. Rydberg et al., 2010 Science of the total environment, 408(20), 4778-4783; Sun et al., 2017 Environmental Science and Pollution Research, 24(17), 15078-15088.; Schuster et al, 2018 Geophys Res Letters 45, 1463-1471). This is not mentioned in the chapter so far and needs to be added. Full references: Rydberg, J., Klaminder, J., Rosén, P., & Bindler, R. (2010). Climate driven release of carbon and mercury from permafrost mires increases mercury loading to sub-arctic lakes. Science of the total environment, 408(20), 4778-4783; Sun, S., Kang, S., Huang, J., Chen, S., Zhang, Q., Guo, J., ... & Qin, D. (2017). Distribution and variation of mercury in frozen soils of a high-altitude permafrost region on the northeastern margin of the Tibetan Plateau. Environmental Science and Pollution Research, 24(17), 15078-15088; Schuster, P. F., Schaefer, K. M., Aiken, G. R., Antweiler, R. C., Dewild, J. F., Gryziec, J. D., ... & Liu, L. (2018). Permafrost stores a globally significant amount of mercury. Geophysical Research Letters, 45(3), 1463-1471. [IAPSO ECS group review, United States of America]	Noted. This issue of Hg release from thawing permafrost is not a WG1 issue. It could be treated in the WG2 report.

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104825	75	51	75	55	Particularly agree with soil moisture variations, which also affect vegetation and the modelling of methane emissions. To take account of the non constant variaons in the moisture levels of waste/soil the first order rate constant k (IPCC 2006 Waste Model) is modified with a term related to the molecular fraction of moisture f[H2O] necessary to saturate the sugars (minmum about 40% equivalent to 4 molecules of water) from the breakdown of cellulose as k x f[H2O]. At 10% moisture (f[H2O]=0.25 mainly CO2 is produced, at 40% moisture f[H2O]=1 equimolar quanties of CO2 and CH4 are produced (Hartz and Ham 1983). For methane to be produced 1 molecule of water must be transferred to the sugar ring (Rate determining step). Using field measurements of moisture (often from agricultural land measurements) methane emissions can be better estimated. Hartz, K.E., and R. K. Ham (1983) Moisture level and movement effects on methane production rates in landfill samples, Waste Management & Research, 1. 139-145, https://journals.sagepub.com/doi/pdf/10.1177/0734242X8300100116 I am writing a paper on the adpated rate constant and f[H2O] but if may be another month or so before it is sent for peer reveiw and publication. [Paul Dumble, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However this comment does not seem to refer to CH9 (on ocean, cryosphere and sea level)
55103	75	54	75	54	Consider citing O'Neill et al. 2019 in addition to Streletskiy et al. The Streletskiy paper shows settlement from a number of plots at one site. The O'Neill et al. 2019 shows long-term (25 year) subsidence from a network of thaw tubes in the Mackenzie Delta area and offers a regional perspective: Long-Term Permafrost Degradation and Thermokarst Subsidence in the Mackenzie Delta Area Indicated by Thaw Tube Measurements https://ascelibrary.org/doi/10.1061/9780784482599.074 [Nancy Hamzawi, Canada]	Accepted. This paper provides a larger scale perspective. The paper is cited now.
88283	75	54	75	54	O'Neill et al. (2019 - reference below) also consider thaw settlement and utilize records >20 years long to show the loss of permafrost in northwestern Canada - consider thaw penetration relative to a fixed reference point. I can provide copy of paper. citation: O'Neill HB, Smith SL, Duchesne C 2019. Long-term permafrost degradation and thermokarst subsidence in the Mackenzie Delta area indicated by thaw tube measurements. In: Bilodeau J-P, Nadeau DF, Fortier D, Conciatori D (eds) Cold Regions Engineering 2019, Proceedings of the 18th International Conference on Cold Regions Engineering and the 8th Canadian Permafrost Conference, Quebec, Quebec, Canada, August 18-22 2019. American Society of Civil Engineers, pp 643-651. doi:10.1061/9780784482599 [Sharon Smith, Canada]	Accepted (see reply to comment #3998)
88711	75	55	76	1	"Records of Antarctic ALT changes are too limited for an assessment of long-term trends (Hrbáček et al., 2018)." Why was used only one reference for the hole permafrost areas of Antarctica? The same authors and many others have several studies about this theme. [Rosemary Vieira, Brazil]	Noted. Hrbáček et al. is an overview article about active layer monitoring in Antarctica and as such the appropriate article to cite here.
88285	76	2	76	3	Christiansen et al (2010) is pre AR5 article and doesn't really provide information on trends over the past decade. It is suggested that this be removed. Noetzlie et al. (2019) could probably be used for recent information on ALT trends. [Sharon Smith, Canada]	Accepted. References changed.
71179	76	6	76	9	With regards to destabilization of rock glaciers, there is some debate / confusion because this term is used differently by different professions and research fields. As such it must be used carefully. The authors are referred to the following IPA document that illustrates the issue: IPA-RG (2020). Towards standard guidelines for inventorying rock glaciers: Baseline concepts (Version 4.0). , eds. R. Delaloye and T. Echelard Longyearbyen, Svalbard: International Permafrost Association (IPA) Action Group Rock glacier inventories and kinematics Available at: https://bigweb.unifr.ch/Science/Geosciences/Geomorphology/Pub/Website/IPA/Guidelines/V4/200117_Baseline_Concepts_Inventorying_Rock_Glaciers_V4.pdf. [Lukas Arenson, Canada]	Noted. Thank you. Unfortunately we cannot cite the suggested IPA document which is to be considered as grey literature in spite of its high quality and the indubitable expertise of its authors, but we consulted it. Taking into account, and citing, new peer-reviewed literature published since SROCC, we are confident that the careful formulation of the revised statement, necessarily highly synthetic because of word count constraints, is supported by the cited literature.
24127	76	7	76	7	Better focus on the involved material/process than on landforms. I recommend to write: " ... including collapse, of creeping perennially frozen materials on slopes (rock glacier flow) are indicators ..." [Wilfried Haeberli, Switzerland]	Taken into account. The paragraph was substantially shortened, so the suggested wording cannot be implemented as suggested, but the revised paragraph does better focus on the physical processes and materials than on the landforms, as suggested by the reviewer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24129	76	9	76	9	A brief statement should be added pointing to indications that the frequency and volume of large rock ice avalanches from relatively warm permafrost in rock walls are increasing. References: (1) Coe, J.E., Bessette-Kirton, E.K., Geertsema, M. 2018. Increasing rock-avalanche size and mobility in Glacier Bay National Park and Preserve, Alaska detected from 1984 to 2016 Landsat imagery. Landslides 15, 393-407. doi:10.1007/s10346-017-0879-7 (2) Haeberli, W., Schaub, Y. Huggel, C., 2017. Increasing risks related to landslides from degrading permafrost into new lakes in de-glaciating mountain ranges. Geomorphology 293, 405-417. http://dx.doi.org/10.1016/j.geomorph.2016.02.009 [Wilfried Haeberli, Switzerland]	Rejected. These are pre-SROCC references which we are supposed to limit to a minimum. We therefore focus on later references that show the same facts.
61881	76	12	76	12	AMIP has never been introduced as an abbreviation, not sure if necessary [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. LS3MIP and AMIP are not mentioned in the revised version.
16083	76	12	76	12	Reminder for myself - refer to FAQ 5.2 [Gerhard Krinner, France]	Noted. Not that relevant here after all (mostly carbon)
88287	76	12	76	13	Revision suggested - "… suggests that subsea permafrost that formed before submersion…" You could also mention that subsea permafrost formed during of period of lower sea level associated with last glaciation. [Sharon Smith, Canada]	Accepted. Added "during the last deglaciation"
23761	76	15	76	16	maybe add 'Overduin et al. (2018) estimate that 97% of permafrost under Arctic shelves is currently thinning.' 10.1029/2018JC014675 [Annett Bartsch, Austria]	Accepted. Text modified accordingly.
88289	76	18	76	19	I assume that you are referring to the impact that human activity has on GHG emission leading to climate warming when you refer to the attribution of permafrost change to human activity. You need to be clear about this because there are lots of examples of attribution of changes in permafrost (e.g. ground thermal regime) due to human activity such as clearing of vegetation for construction of infrastructure or installation of heated structures or artificial cooling of the ground. [Sharon Smith, Canada]	Accepted. Text clarified (see response to comment #4235)
4235	76	18	76	23	A confusion might arise here between human influence on permafrost thaw via anthropgenic climate change, and permafrost thaw caused by human disturbance of permafrost ecosystem, which is also widespread in the Arctic as a consequence of industrialization and mining. See e.g. Raynolds MK, Walker DA, Ambrosius KJ, Brown J, Everett KR, Kanevskiy M, Kofinas GP, Romanovsky VE, Shur Y, Webber PJ (2014) Cumulative geoecological effects of 62 years of infrastructure and climate change in ice-rich permafrost landscapes, Prudhoe Bay Oilfield, Alaska. Glob Chang Biol 20 (4):1211-1224. doi:10.1111/gcb.12500. I suggest to make a distinction between these two cause-effect lines here. I suggest 1) to change line 22-23 into 'that human influence via climate change is overall the dominant cause of the observed pan-Arctic permafrost changes.' and add the sentecte: 'Added to this, local permafrost change by soil and ecosystem disturbance is induced by increasing human industrial activities in the Arctic (Raynolds et al., 2014). [Jacobus (Ko) van Huissteden, Netherlands]	Accepted. Text modified accordingly.
109083	76	19	76	20	coordination with chapter 6 on section referencing section 6.3? [Chaincy Kuo, United States of America]	Rejected. This comment seems to be out of place, and it is not obvious where it should belong to.
81181	76	31	76	37	We found that the thermal effects of soil freezing and unfrozen soil water led to more realistic simulated permafrost extents by using the land surface models of MIROC, which is a part of the CMIP6 ensembles. We compared the models with and without considering the thermal effects of soil freezing, unfrozen soil water, and porous organic soils in tundra regions, and found that the former has more realistic simulation of permafrost extent. The paper is under review as follows. Yokohata T, Saito K, Takata K, Nitta T, Sato Y, Hajima T, Sueyoshi T, Iwahana G (2020) Model improvement and future projection of permafrost processes in a global climate model, Submitted to the same issue of Progress in Earth and Planetary Science [Tokuta Yokohata, Japan]	Accepted. Paper is cited now along with others.

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88291	76	34	76	34	If only considering upper 3 m of the ground then not really a real representation of permafrost extent - need to be clear about this. Also isn't one of the limitations of existing models the representation of deeper heat flow etc. [Sharon Smith, Canada]	Accepted. Simulated permafrost extent is analysed until 15 m depth here (where possible), and when the 3 m limit is applied, this is clearly indicated as near-surface permafrost (which is a very frequently used term in the literature) and the 3 m are clearly mentioned. This model limitation, and the lack of progress since CMIP5, is now mentioned: "Although it is well known that a total soil column depth of at least about 10 m is required to adequately simulate the seasonal-scale heat transfer in the ground and thus active layer thickness (Lawrence et al., 2008; Ekici et al., 2015), many CMIP6 models still have shallower total soil columns (Burke et al., 2020) and the proportion of models with deeper total soil columns has not increased since CMIP5 (Koven et al., 2013)."
16423	76	35	76	35	Get rid of semi-colon and start a new sentence, or use a colon instead. [Julian Mak, China]	Accepted. Implemented as suggested.
90697	76	36	76	42	In the GlacierMIP project, six global glacier models were driven with RCP2.6, RCP4.5, RCP6.0, and RCP8.5 simulated global glacier mass loss of 94 ± 25 millimeters for RCP2.6 to 200 ± 44 millimeters for RCP8.5 (Hock et al. 2019). However, glacier mass loss could be over-estimated in some regional and global time series, given glaciers with measured mass balances in some studies are concentrated in sub-regions with higher mass losses (Gardner et al., 2013). Gardner et al., 2013, A reconciled estimate of glacier contributions to sea level rise: 2003 to 2009, Science, 340, 852–857, https://doi.org/10.1126/science.1234532 [Thian Yew Gan, Canada]	noted, results from Marzeion et al 2020 included in the assessment
62053	76	37	76	37	…models that correctly represented the effect of snow insulation… Is correctly a subjective term here? Suggest rephrasing to "…models that incorporated the…" or something similar [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The paper by McGuire does state that a correct representation of this effect is required ("conclude that a well-simulated snow insulation is a condition for accurate simulation of permafrost area and soil thermal dynamics") and refers to other paper that also state this. Therefore, we do think that "correctly" would be the right term here. But it does indeed sound a bit like a value judgment. Therefore we replace "correctly" by "adequately".
88713	76	39	76	39	Lawrence and Swenson, 2011. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
61879	76	39	76	39	"in several models": in CMIP5 or 6? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Not all models are CMIP5 or CMIP6 models. Some of the publication referred to, however, do describe changes to models that participate to CMIP6. Tu clarify, we restrict our list of references to post-AR5 papers. However, not all of them describe changes that were effectively implemented in CMIP6 models, so we do not specify CMIP6 here.
88715	76	39	76	40	Chadburn et al., 2015b. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88717	76	40	76	40	Porada et al. 2016. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88719	76	40	76	40	Duel et al. 2017. Reference not found. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
55105	76	42	76	42	Suggest removing the term "abrupt". There is a trend in recent literature of using this term, but it is problematic in its ambiguity, including its use here. It is used in this instance to describe thermokarst formation. Thermokarst may include relatively slow ground surface subside in undisturbed terrain in response to warming temperatures, expansion of thermokarst lakes, more rapid subsidence following e.g., fire, or slope failures in the form of slumps and active layer detachment slides. These all occur at different rates. So, to use the term "abrupt" for all of these is arbitrary and not particularly accurate or meaningful. The sentence works better without it. [Nancy Hamzawi, Canada]	Accepted. Implemented as suggested.
88293	76	42	76	43	Thermokarst is more of a process (thawing) rather than a formation (although we do refer to formation of thermokarst lakes). Also thermokarst isn't necessarily abrupt and can be a slow process. [Sharon Smith, Canada]	Accepted. See reply to comment #88293.

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71181	76	43			how is "ice-rich" defined? Not in the glossary. The authors may consult CAN/BNQ 2501-500/2017: Geotechnical Site Investigations for Building Foundations in Permafrost Zones, which is a Canadian Standard and this document gives definitions about ice-rich etc. [Lukas Arenson, Canada]	Noted. The notions of ice-rich and ice-poor will certainly depend on the domain of application. In that sense, an engineering standard will not necessarily be appropriate. In the scientific literature, e.g. Kanevskiy et al. Cold Regions Science and Technology, 2013 (but there are many other examples), "ice-rich" is not explicitly defined. Zhang et al (2000, 2008) define high ice content as >20%. We now specify > approx. 20% for the definition of ice-rich permafrost.
22629	76	44	76	46	This makes no logical sense. The only way this could possibly be assessed is through observations and observations are always historical by definition - we cannot observe the future. This therefore requires a substantive revision for clarity. I think what you mean is that recent changes have been faster than changes in past decades or similar but all the changes are observed! [Peter Thorne, Ireland]	Accepted. Reworded: "there is high confidence that permafrost degradation through fire (Jones et al., 2015; Gibson et al., 2018) is currently occurring faster in some well-studied regions than during the first half of the 20th century"
62055	76	46	76	46	…occurring faster than historical observations suggest… historical observations is vague, and this would be improved with adding more specificity. What is the period of historical observation that is referred to here? Suggest rephrasing to "…occurring faster than the [timeframe] average of permafrost degradation due to fire…" or something similar that specifies the baseline that is being compared against [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This was reworded more precisely (see reply to comment #22629)
88295	76	48	76	50	Sentence unclear. The presence of the insulating organic matter can reduce the rate of ground warming and permafrost thaw, it is only when it eventually thaws that mineralizizaton occurs and reduces the insulating effect and promotes further warming. The way the sentence is written it makes it sound like the presence of insulating organic layer is causing the thawing. [Sharon Smith, Canada]	Accepted. The sentence was cut in two and reworded: "Another process that can accelerate permafrost thaw is warming-induced mineralization of the organic material that provides strong thermal insulation of underlying ground. This creates a positive feedback loop, usually not represented in the current (CMIP6) generation of climate models (Zhu et al., 2019)."
88297	76	53	76	53	It is better to refer to physical processes rather than only abrupt processes as any physical change, no matter the speed, that occurs in response to permafrost warming and thaw can potentially have feedbacks to the ground thermal regime. [Sharon Smith, Canada]	Accepted. We deleted the word "abrupt" in this paragraph.
88299	76	53	76	53	permafrost thaw or degradation are the terms usually used rather than permafrost decay [Sharon Smith, Canada]	Accepted. We use "degradation" now systematically.
71183	76	53			What do the authors mean by "decay"? The report uses permafrost thawing, thawing permafrost, decay, permafrost degradation, permafrost warming and even melting permafrost (which must not be used as stated in a comment below). It would be good if the report would be consistent in the use of the terminology and specifically decay doesn't mean much. [Lukas Arenson, Canada]	Accepted. We replaced "decay" by "degradation".
23763	77	2	77	3	phrasing is somewhat misleading. Obu et al. 2019 is satellite and reanalyses based, not just reanalyses based as the current formulation suggests [Annett Bartsch, Austria]	Accepted. The sentence referred to observations and reanalyses, and was thus formally correct because satellite data can be considered as observations (usually). We now separated the older Zhang et al paper (obs) and the more recent satellite and reanalysis-based studies. That should prevent misunderstandings.
88301	77	2	77	3	There isn't much in the way of observations in the permafrost extent based on the references cited here. Zhang et al. is based on the Brown et al. map which considers limited observations but essentially integrates climate, geology and Quaternary history. Gruber is based on modelling. Obut et al. is based on TTOP modelling which would represent equilirium condition. [Sharon Smith, Canada]	Accepted. We clarified this in the revised sentence: "Compared to estimates of present permafrost extent based essentially on site observations (Zhang et al., 1999), satellite data and reanalyses (Gruber, 2012; Obu et al., 2019), the extent…"
88303	77	4	77	6	Observational extent of what during 1979-1998 - permafrost extent (see comment on line 2-3) [Sharon Smith, Canada]	Noted. We clarified this: "...corresponding approximately to the average period of the three estimates mentioned above…"
40001	77	5			can you double check the period 1979-1998, it looks a bit suspicious for a period of observational estimates [TSU WGI, France]	Noted. The satellite and reanalysis products are, to some extent, also "observational" (admittedly very indirectly in reanalyses), but the rewritten sentence does not refer to these estimates as "observational" because in the mind of many readers observations are necessarily site observations.

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100447	77	7	77	9	Soil moisture migration and lateral flow are also important especially for mountain permafrost modeling, but this process is not well represented in most models [Huiru Jiang, Sweden]	Noted. However this is less relevant here because CMIP-type models will not represent mountain permafrost because of the small spatial scale.
4237	77	7	77	10	Models that do not include phase change of water or have a soil colum that is smaller than the depth of annual temperature variation are useless to simulate permafrost extent, and should not be used here. This has been demonstrated adequately by the studies of Burke and Chadburn cited here.I suggest to state this clearly bij changing the last sentence of this text int: These models are inadequate for simulating permafrost extent, since it leads to an overestimate of seasonal thaw depth, exceeding the total thickness of the models' soil column (Burke et al., submitted). [Jacobus (Ko) van Huissteden, Netherlands]	Accepted. We now write that these models are inadequate for our purposes. However, because they are clear outlier, they are easy to identify and need not be further excluded.
81183	77	7	77	11	We also found that the model without treating the soil water phase changes (i.e., the model which does not consider the heat conductivity of frozen soil) has large decrease in permafrost area in the future, because the soil freezing in winter is surpressed due to the underestimation of heat conduction by frozen soil. This is described in the paper below, which is uncer review. Yokohata T, Saito K, Takata K, Nitta T, Sato Y, Hajima T, Sueyoshi T, Iwahana G (2020) Model improvement and future projection of permafrost processes in a global climate model, Submitted to the same issue of Progress in Earth and Planetary Science [Tokuta Yokohata, Japan]	Taken into account. The paper is now cited as an example soil freezing and its thermal and hydrological effects are now included in a large number of land-surface modules that are part of the CMIP6 ensemble.
88305	77	10	77	11	Isn't part of the issue that the deeper heat flow is also not considered which artificially elevates warming of shallow layers? [Sharon Smith, Canada]	Noted. We clarified this in the text by saying that the missing deep soil "lead[s] to an underestimate of thermal inertia at depth".
39735	77	12			"corresponding AMIP land-atmosphere simulations, and the LS3MIP" what is that? I don't think it's clear given the context. [TSU WGI, France]	Noted. AMIP and LS3MIP are not mentioned in the revised text (see reply to comment #61881).
61883	77	21	77	21	change NH to Northern Hemisphere [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
40173	77	21			Fig 9.24: the upper part of panel bit is a bit hard to understand. [TSU WGI, France]	This part of the right panel is deleted in the final version.
16425	77	23	77	23	Caption of Fig 9.24, AMIP instead of Amip [Julian Mak, China]	Accepted. Implemented as suggested.
61885	77	23	77	23	the description of AMIP in the text only includes land-atmosphere simulations, but not the ocean, keep consistent [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. AMIP is not mentioned in the text anymore, and we mentioned the prescribed ocean forcing in the main text.
22631	77	36	77	38	Over what range of temperature change was this assessed? Clearly as there is a finite amount of permafrost it cannot be linear forever so I think it important to be explicit over what range this plausibly holds. Like the carbon budget the approximation cannot hold indefinitely. [Peter Thorne, Ireland]	Noted. Their figure 3 shows that this holds approximately until 2 to 3°C GSAT increase w.r.t. the 1961-1990 baseline.
90699	77	36	77	39	I think that the statement, "There is only medium confidence that permafrost loss is a pan-Arctic phenomenon" should be revised given unevenly distributed reports of observed loss of permafrost in areas discontinuous and sporadic permafrost since about 1980? Using a global permafrost temperature dataset from the Global Terrestrial Network for Permafrost, Biskaborn et al. (2019) evaluated temperature change across permafrost regions of Northern Hemisphere.. Between 2007 and 2016, they found permafrost temperature in discontinuous permafrost warmed by 0.20 ± 0.10 °C, mountain permafrost by 0.19 ± 0.05 °C and permafrost in Antarctica by 0.37 ± 0.10 °C. We will expect permafrost in these zones to thaw to some extent under warming? Further, alpine permafrost of the Tibetan Plateau has been subjected to serious permafrost degradation due to drastic climate warming in past decades (Ran and Chen, 2018). Biskaborn, B.K., Smith, S.L., Noetzli, J., Matthes, H., Vieira, G., Streletskiy, D.A., Schoeneich, P., Romanovsky, V.E., Lewkowicz, A.G., Abramov, A., et al., 2019. Permafrost Is Warming at a Global Scale, Nature Communications, 10, 264. Ran, Y., Li, X., Cheng, G., 2018. Climate Warming Over the Past Half Century Has Led to Thermal Degradation of Permafrost on the Qinghai–Tibet Plateau. The Cryosphere, 12, 595–608. [Thian Yew Gan, Canada]	Noted. The paper by Ran et al. is interesting, although it is only a modelling study. We cite this paper now.
88307	77	36	77	41	Mixing apples and oranges here so comparison doesn't make sense. Chadburn is an equilibrium model so it doesn't include the time period over which the change will occur. I believe it also considers entire extent not just upper 3m - considers whether permafrost exists under given climate conditions. Slater and Lawrence use transient model to consider what happens over the next century with respect to upper 3 m - they could run the model for a longer period which would then be more comparable with an equilibrium model (i.e. determine steady state condition). [Sharon Smith, Canada]	Noted. The point here is exactly to draw attention to the fact that the equilibrium permafrost changes and transient changes are not the same, quite as there is a difference between ECS and TCR. We think that pointing out this difference is important and policy-relevant.

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20567	77	43	77	45	What is the reason for selecting a limit 3m below the surface (rather than 2 or 4m)? According to Romanovsky et al (2018), the present active layer thickness seems to vary between 0,50 and 1,20m, so this cannot be the criterion [philippe waldteufel, France]	Noted. The criterion is that the is often a good compromise between the model soil column depth (some models do not go deeper than that, and some do not even go this deep) and typical active layer thicknesses, which are usually (but not always) less than 3m. We refrain from providing this detail here in the text because of space constraints.
61887	77	43	77	45	I don't see the link between the 15m permafrost extent and the changes in the top3m in this graph. Where do we get the information that changes happen in the top3 m from? This needs clarification [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We remove the first half of the sentence to prevent confusion.
88309	77	43	77	49	It is important to indicate that the loss of permafrost does continue below 3 m. In the analysis thaw will reach 3 m in some places but that doesn't mean that permafrost volume will no longer be lost as warming continues as it will continue below 3 m. A qualifying statement is required. [Sharon Smith, Canada]	Accepted. We add: "It is important to note that permafrost loss will not be limited to the top 3 m, leading to a higher transient sensitivity of total permafrost volume."
22633	77	45	77	48	But you just gave a different range above based upon a different study. It is your job to assess and compare these. Suggest to discuss these estimates together and compare / contrast. It does not help that the units are different. You need to synthesise and assess here I think. [Peter Thorne, Ireland]	Accepted. The revised text assesses these and an additional paper that was not available at the SOD stage. Considering the underlying hypotheses and approaches, we reach a judgment on the most plausible range of future changes.
71185	77	45			Permafrost volume in the top 3 m (see Comment Nr. 1) [Lukas Arenson, Canada]	Noted. It is unclear which comment the reviewer refers to (he does, in any case, not seem to refer to a comment on section 9.5.2), and therefore the purpose of the entire comment is unfortunately unclear to us.
61889	77	47	77	47	remove brackets and change unit to % C-1 to be consistent [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Implemented as suggested.
62091	77	49	77	51	This statement should have a confidence associated with it to be consistent with the statement above - medium confidence due to the model deficiencies? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We specify now that these are the simulated sensitivities, and therefore do not think that a confidence statement is absolutely required here.
62057	77	54	77	54	…it appears virtually certain… one of two incidences of this level of certainty in section 9.5 , but preceded by much discussion of the uncertainty in permafrost trends and records due to limited data. A quick call of attention to this so authors can decide if this level of certainty is appropriate [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We can be almost 100% sure that permafrost will be lost if climate continues to warm - the uncertainties only concern how much will be lost.
107473	77	54	77	54	How does something "appear" to be virtually certain? It should be or shouldn't [Jennifer Walker, United States of America]	Noted. We replace "appears" by "is".
88311	77	54	77	55	You could just refer to permafrost extent. Whether you consider vertical or lateral extent it will be smaller as climate warms based on the results presented. [Sharon Smith, Canada]	Accepted. We implement this suggested change, and add "volume".
62017	78	1	78	2	« New permafrost will generally form after disturbance or transient warming... » How? Could the authors back this statement by some sort of processes? The reference (Briggs et al.) refers to one lake in interior Alaska and is mostly based on modeling. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. As stated in the response to comment #4239, the intention here is to clarify the distinction between permafrost loss (not irreversible) and permafrost carbon loss (irreversibility discussed in Chapter 5), which we refer to. The revised sentence should be clearer. The reference to Briggs et al. (2014), referring to local disturbances, is not necessary and was deleted to reduce potential confusion.
88313	78	1	78	4	Briggs et al. (2014a) considers permafrost aggradation following lake drainage so is refering to a very specific case and does not really consider disturburance of impact of climate warming in general. Permafrost aggradation associated with shifting river shorelines or lake drainage (and cooler surface conditions) is well known (e.g. see Mackay and Burn 2002 Can J Earth Sci). Briggs et al does not support the statement that irreversibility is not to be expected - do you mean reversibility is not to be expected? Permafrost that is present today is not in equilibrium with current climate conditions and in some places like disc. zone, much colder conditions were required and permafrost is preserved today due to thick organic layer (ecosystem protected permafrost) - conditions on centennial to millenial scales therefore are important in determining permafrost conditions including its occurrence. These sentences therefore need to be substantially revised or perhaps removed. [Sharon Smith, Canada]	Taken into account. The sentence on irreversibility of permafrost thaw was cut because it is not the most relevant aspect of irreversibility of permafrost changes. The most relevant aspect is decay of organic carbon (chapter 5).

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4239	78	2	78	2	This formulation is vague and should stated more precisely. ' Irreversibility of permafrost loss on large spatial scales on the centennial time scale is therefore not to be expected'. In a strict sense this is true, but it is not very relevant; the focus should be more on the effects of permafrost loss. The effects of deeper permafrost thaw on carbon stores is largely irreversible; several studies cited in this report cite very long time scales in the order of centuries to milennia. See e.g.O'Donnell 2012 (ref. below) for the milennial scale recovery of permafrost carbon stores. Furthermore, effects on ecosystems, human activities and surface hydrology can be substantial over large areas and equally irreversible. Although loss of permafrost is not irreversible, it is the effect on ecosystems and human society. Furthermore it depends on what is understood as 'large spatial scale', and should take into account the effects on the various permafrost zones - continuous, discontinuous and sporadic. For the discontinuous and sporadic permafrost the loss of permafrost volume/area is likely to be substantial. O'Donnell JA, Jorgenson MT, Harden JW, McGuire AD, Kanevskiy MZ, Wickland KP (2012) The effects of permafrost thaw on soil hydrologic, thermal, and carbon dynamics in an Alaskan peatland. Ecosystems 15 (2):213-229 [Jacobus (Ko) van Huissteden, Netherlands]	Taken into account. The sentence on irreversibility of permafrost thaw was cut because it is not the most relevant aspect of irreversibility of permafrost changes. The most relevant aspect is decay of organic carbon (chapter 5).
99677	78	2	78	3	Please check if the following sentence is correct: "Irreversibility of permafrost loss on large spatial scales on the centennial time scale is therefore not to be expected (high confidence)"...I think that this is not a very clear phrasing. It is true that permafrost may reform after thawing in very cold areas, but the reality is that permafrost is still there, however, under the surface. The main problem in these areas relates to increased permafrost-carbon feedbacks, which can happen with only erosion of the surface, causing a downward progression of the position of the permafrost table associated to continued active layer thaw. Also, changes in hydrology and landcover will be very significant. Please, check if the sentence is relevant as it is. [Goncalo Vieira, Portugal]	Taken into account. The sentence on irreversibility of permafrost thaw was cut because it is not the most relevant aspect of irreversibility of permafrost changes. The most relevant aspect is decay of organic carbon (chapter 5), as the reviewer rightly states.
88315	78	3	78	4	This last sentence seems out of place as the preceding discussion in this section does not consider carbon loss. The introduction to 9.5.2 mentions that Ch 5 deals with carbon feedback so no need to repeat here and the sentence can be removed. [Sharon Smith, Canada]	Taken into account. The sentence on irreversibility of permafrost thaw was cut because it is not the most relevant aspect of irreversibility of permafrost changes. The most relevant aspect is decay of organic carbon (chapter 5).
61891	78	4	78	4	Chapter 5 (Section 5.4.3.3)? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence on irreversibility of permafrost thaw was cut because it is not the most relevant aspect of irreversibility of permafrost changes. The most relevant aspect is decay of organic carbon (chapter 5).
62093	78	7	78	7	This whole snow section does not mention Antarctica at all. Maybe I have missed something, but is there just no assessment of the snow changes for Antarctica, with everything focused upon the NH? Should there not be at least a line or two explaining why the Antarctic snow is not discussed/assessed in this section and that the section is focused upon the NH? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. To clarify, the section is now clearly called "seasonal snow cover". Antarctic snow is most relevant for ice sheet mass balance and assessed in 9.4.
15595	78	7	78	7	I suggest replacing "Snow" by "Snow cover", otherwise it could be misunderstood with "Snow fall" [Samuel Morin, France]	Taken into account. Further increase in clarity of the scope of this subsubsection, we now call it "Seasonal snow cover".
112477	78	7	80	6	Snow cover is also decreasing in the Southern Hemisphere, check this recent papers (Cordero et al doi: 10.1038/s41598-019-53486-7; Cortés et al. 2017 doi: https://doi.org/10.1002/2017GL073826; Cortes et al. 2014 doi: 10.1016/j.rse.2013.10.023 .) [Pedro LLanillo del Rio, Germany]	Taken into account. The snow trends in the Andes, which are the largest snow cover area in the Southern Hemisphere and still orders of magnitude smaller than the Northern Hemisphere snow extent, are spatially highly variable. Available satellite-based records of snow cover variations in the Andes since the mid-1980s show regionally varying trends in dry-season snow cover extent and annual snow cover duration, which tended to be dominated by ENSO variability in equatorward of about 34°S, the influence of SAM variability increasing towards the South (Saavedra et al., 2018; Cordero et al., 2019). In the revised version, we cite Cordero et al 2019 for ENSO-driven snow cover decrease during dry seasons in the low- to mid-latitude Andes, and Aguirre et al (2018) for warming-related winter snow cover reduction in the southern Andes.

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107237	78	7	82	14	[pt 1 of 3] As I noted in my comments on the FOD, the report needs to discuss the effect of a warming climate on snowfall accumulation in ice sheets. In a warming climate, snowfall accumulation on ice sheets can be expected to increase, for two reasons. The first is that warmer air carries more moisture. Each degree Celsius of warming increases saturated water vapor pressure by about 6-7%. The second is through the mechanism of Lake/Ocean-Effect Snowfall (LOES). Both Greenland and Antarctica are surrounded by oceans, which are often covered with sea ice. Evaporation from open seawater is much more rapid than sublimation from sea ice, so when the ice opens it results in dramatically increased snowfall downwind. If a warming climate reduces sea ice coverage in the Arctic, North Atlantic and Southern Oceans, it can be expected to increase snowfall accumulation on the ice sheets, reducing sea-level, and offsetting ice sheet mass losses through melting, iceberg calving, and sublimation. All of that needs to be discussed in the report, but none of it is. [cont'd] [David Burton, United States of America]	Noted. Thank you. To clarify, the section is now clearly called "seasonal snow cover". Antarctic snow is most relevant for ice sheet mass balance and assessed in 9.4, as already stated in replies to similar comments on the FOD by the same reviewer (see also reply to comments #107239 and #107241).
107239	78	7	82	14	[pt 2 of 3] Snow accumulation has a very large effect on grounded ice mass, which in turn affects sea-level. In both Greenland and Antarctica, snowfall is the most important factor affecting ice sheet mass balance, greater in magnitude than melting, sublimation, or iceberg calving. In fact, in Antarctica, snowfall accumulation is approximately equal to the sum of those other three factors. The magnitude of ice accretion from snowfall on ice sheets was illustrated by the amazing story of "Glacier Girl." She's a WWII Lockheed P-38 Lightning airplane, which was extracted in pieces from beneath 268 feet(!) of accumulated ice and snow (mostly ice), fifty years after she made an emergency landing on the Greenland Ice Sheet. Here's a diagram: http://p38assn.org/glacier-girl-recovery.htm That's more than 5 feet of ice per year, which is equivalent to more than 70 feet of annual snowfall, which had piled up on top of the airplane! That snow represents evaporated water, mostly removed from the Arctic and North Atlantic Oceans, which then fell as ocean-effect snow on the Greenland Ice Sheet. [cont'd] [David Burton, United States of America]	Noted. Thank you. To clarify, the section is now clearly called "seasonal snow cover". Antarctic snow is most relevant for ice sheet mass balance and assessed in 9.4, as already stated in replies to similar comments on the FOD by the same reviewer (see also reply to comments #107237 and #107241).
107241	78	7	82	14	[pt 3 of 3] Multiple studies have found that snowfall accumulation in Antarctica has been increasing: https://web.archive.org/web/20180104195908/https://www.washingtonpost.com/news/energy-environment/wp/2018/01/03/large-antarctic-snowfall-increases-could-counter-sea-level-rise-scientists-say/ https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL075992 https://www.clim-past.net/13/1491/2017/cp-13-1491-2017.html https://www.nature.com/articles/354058a0 https://www.nature.com/articles/s41558-018-0356-x ### [David Burton, United States of America]	Noted. Thank you. To clarify, the section is now clearly called "seasonal snow cover". Antarctic snow is most relevant for ice sheet mass balance and assessed in 9.4, as already stated in replies to similar comments on the FOD by the same reviewer (see also reply to comments #107237 and #107239).
15597	78	9	78	9	The structure of the section 9.5.3.1 Observed changes is a bit difficult to follow. I understand this covers SCE, then SCD then SWE changes. If this is indeed the case, I'd suggest that the structure is made more apparent (e.g. using some subsectioning). The mountain content of this section is a bit disappointing, as it does not seem to complement SROCC Chapter 2. There have been additional studies published since SROCC literature cut-off, which could be assessed here (e.g. on snow cover trends in the Pyrenees, see Lopez-Moreno et al., 2020, https://doi.org/10.1002/joc.6571). Overall, the current material makes it difficult to uptake in WG2 Cross-chapter paper on Mountains. [Samuel Morin, France]	Taken into account. Subsections were implemented as suggested. Note that the most important contribution of this assessment here is to complement SROCC spatially - SROCC did a great job assessing mountain and Arctic snow cover, which was its task, but it did not assess snow cover outside of these areas (as per scoping). Therefore, we concentrate on a hemispheric and global-scale changes, relying very much on the SROCC for mountain snow cover because we do need to save space. We do now more clearly re-state the SROCC starting point and confirm it, making the assessment easier to pick up for WGII. Furthermore, we do refer to the suggested publication and another recent one on the Alps.
29613	78	9	79	18	I see most attention is given to northern hemisphere snow cover. I think the information related to snow cover in the Southern Hemisphere should be covered as well. For example, Cordero et al 2019 (Scientific Reports) , Saavedra et al (2018, The Cryosphere) and references therein may help to provide information on snow cover trends in the Andes. [Villasenor Tania, Chile]	Accepted. These two papers are now cited in a short paragraph on snow cover variations in the Andes. Note that Saavedra et al had been cited in the FOD, but removed to save space.
15607	78	9	80	6	This section on "Observations" does not address "Detection and Attribution" explicitly. I think this is missing from ths section. In SROCC Chapter 2, we assessed evidence about detection and attribution of snow cover changes in mountainous regions. Detection and Assessment certainly warrants to be mentioned in such a section, even in the case where there are few (none ?) additional studies about this since SROCC literature cut-off dates. [Samuel Morin, France]	Noted. For attribution of snow cover changes, section 9.5.3.2 of the SOD (Page 80, lines 51-55) refers the reader to Chapter 3, to save space. We think that to remove redundancies within the report, this is appropriate.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40765	78	9			section 9.5.3.1: the structure of the section is not really obvious and sometimes the flow is a bit hard to follow (e.g. p79 L9-18 do not really appear connected to the rest of the text) and the phrasing can be convoluted (e.g. p78, L31-32 "(in excess of 45 million km2 over the 1967-2014 period") [TSU WGI, France]	Accepted. Subheadings have been introduced to clearly separate snow extent from snow mass, and the text has been reordered in several places and rewritten for clarity.
40767	78	9			section 9.5.3: improvements since latest report not necessarily clear [TSU WGI, France]	Taken into account. Particularly in section 9.5.3.1, the AR5 and SROCC starting points have been more clearly identified in the revised version.
85311	78	11	78	11	Perhaps it is not relevant to discuss in this chapter but am I right in thinking that there are lagged links between high latitude northerm hemisphere snow cover and subsequent regional climate variations such as the NAO/AO which could potentially be impacted in future by snow cover losses? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. As the reviewer rightly suspects, we do not think that these questions are relevant for this chapter.
62059	78	11	78	15	…remains below 1 million km… Can this number be reported as a % of land surface also, similar to the northern hemisphere in this paragraph? This would allow for easier comparison against 45% (line 12) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The Southern Hemisphere land surface is about 48 million km2, so 1 million km2 corresponds to 2% of this. We write this in the revised version.
51513	78	11	78	21	The term 'snow cover extent' is used before being defined later in the paragraph. It would be useful if the explanation precedes the use in the text and so I recommend lines 15 to 21 be moved to the start of the paragraph, before current line 11. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Change implemented as suggested.
82657	78	11	80	6	There is no assessment of changes in southern hemisphere snow. Whilst, as noted in the introduction, snow cover extent in the (non-Antarctic) SH is minimal compared with the NH, the limited findings available could be mentioned (along with an acknowledgement of the general lack of data), e.g. the lack of a significant 1979-2006 trend in the Foster et al SCE data set for South America, and alpine snow data sets for Australia (e.g. Pepler et al 2015 and references therein - http://www.bom.gov.au/jshess/docs/2015/pepler.pdf). [Blair Trewin, Australia]	Noted. We now do mention SH snow cover changes. The Foster dataset is pre-AR5, so we prefer citing more recent studies for the trends (see reply to comment #112477).
82653	78	15	78	15	Even 1 million square km is conservative - the Foster et al paper cited here gives a July mean of 324,000 and a highest recorded value of 701,000. [Blair Trewin, Australia]	Taken into account. We now write "well below".
88317	78	15	78	15	Foster et al. (2009) is pre AR5. If there isn't any new information how sure are we about current extent? [Sharon Smith, Canada]	Noted. We did not find a more recent study of total South American snow cover, unfortunately. Many recent studies focus on the Andes, which obviously make up for the by far dominant part of South American land subject to seasonal snow cover, by there is also snow in Patagonian plains that is not covered by these studies.
39849	78	15		21	"Terrestrial snow …. Of this report" wouldn't it make more sense to open the section with that information? [TSU WGI, France]	Accepted. Change implemented as suggested.
61997	78	16	78	18	For consistency with other sections, e.g. 9.5.1.3 page 74 line 19, or 9.5.2.1 page 75 line 51, these variables should also be indicated using number, i.e. 'variables (1) how much area…..'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
16427	78	17	78	17	Bracket with SCD in to mirror one aboe in line 16, with dashes etc) [Julian Mak, China]	Editorial. Change implemented in the sense that we now present the variables consistently, but in parentheses, not in dashes.
40603	78	18	78	18	Note thtat the SOD glossary definition for snow water equivalent (coming from AR5) is "The depth of liquid water that would result if a mass of snow melted completely." [TSU WGI, France]	Noted. The definition of SWE here has been implemented following a FOD review comment and is not inconsistent with the definition in the glossary if "a mass of snow" is seen as the entire snowpack.
61897	78	21	78	21	Chapters 2 (Section 2.3.2.2.), 3 (section 3.4.2) and Chapter 8 of this report [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
82655	78	23	78	23	Should specify that this is northern hemisphere SCE (this only needs to be done once in the paragraph). [Blair Trewin, Australia]	Accepted. Change implemented as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22635	78	23	78	39	This is largely repeating the chapter 2 assessment. Several issues here: i) not referencing that segment; ii) it invites the reader to play spot the difference; iii) unnecessary bloat for the report as a whole. It would be better to start from the chapter 2 assessment and add further regional and process detail as per the respective chapter charges. Regardless, chapter 9 should not be producing a new assessment finding on the largest-scale changes when this has been done by chapter 2. [Peter Thorne, Ireland]	Taken into account. There is indeed some danger of reader playing spot the difference. However, Chapter 9 needs to be reasonably self-contained. So there is, in our sense, a need for a repetition of the global (hemispheric) statements. The restructured text of our section on SCE now clearly refers to the Chapter 2 assessment and has been checked for consistency by Chapter 2 authors.
61895	78	26	78	26	level of confidence [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This paragraph has been cut into pieces. The requested level of confidence had been provided already, but it is now closer to the sentence that the reviewers refer to, so we trust that the reader easily links this specific sentence to the level of confidence (shared with Ch 2) provided " lines further.
61893	78	28	78	28	according to image description the period is 1981-2017 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The revised figure is updated to 1981-2018. The text is modified accordingly.
69675	78	28			Since a submitted paper is listed at 9.5.3 Snow section for instance, it is necessary to confirm a fact of acceptance and the validity of quotation contents at the time of publication. [Konosuke Sugiura, Japan]	Accepted. This paper is accepted now. The reference is updated accordingly.
39895	78	31			:"of -3 to -5% per decade" is it the likely range? The full range? This is not clear. [TSU WGI, France]	Noted. This sentence was cut because the information was based on older studies and not of critical importance.
23749	78	33	78	37	The report says: "all trends of spring snow-off dates from all datasets are negative". This is such a strong statement that it requires referencing more than 2 data sets. Please consider adding Anttila et al 2018 as reference to earlier snow melt date (line 35). The paper is based on different satellite data as the given references and different method for deriving snow melt date, giving more confidence on the observed trends. Full reference: Anttila, K., Manninen, T., Jääskeläinen, E., Riihelä, A., & Lahtinen, P. (2018). The role of climate and land use in the changes in surface albedo prior to snow melt and the timing of melt season of seasonal snow in northern land areas of 40 N–80 N during 1982–2015. Remote Sensing, 10(10), 1619. [Kati Anttila, Finland]	Accepted. We cite this paper along with the two satellite datasets (and reformulate the sentence to be clearer that we refer to large scales).
39135	78	34	78	36	Kouki et al. (2019) derived consistent satellite-based estimates for spring snow melt onset change from both optical and microwave satellite observations, reinforcing the robustness of the observation-based trend towards earlier melt onset. They also note some differences in said trend magnitude between Eurasia and North America. Please consider if these results provide information suitable for inclusion here. [Kouki, K., Anttila, K., Manninen, T., Luojus, K., Wang, L., & Riihelä, A. (2019). Intercomparison of Snow Melt Onset Date Estimates From Optical and Microwave Satellite Instruments Over the Northern Hemisphere for the Period 1982–2015. Journal of Geophysical Research: Atmospheres, 124(21), 11205-11219.] [Aku Riihelä, Finland]	Accepted. We cite this paper, it's important and helpful.
61999	78	37	78	37	The spelling of northern hemisphere is inconsistent with that of the remainder of this section (Section 9.5.1, line 67- 81). It should be written as Northern Hemisphere. Alternatively, all Northern Hemisphere should be changed to northern hemisphere. Similarly, all Southern Hemisphere should be changed to southern hemisphere. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now use Capitals throughout (or the abbreviation NH).
39731	78	41			"consistent evidence" is not part of the IPCC uncertainty language. Do you mean robust evidence? [TSU WGI, France]	Taken into account. The revised wording does not use a confidence statement here because this is not a new finding (but necessary for the consistency of the paragraph)
65981	78	42	57	49	Suggest citing: Massom et al., 2018: Antarctic Ice shelf disintegration triggered by sea ice loss and ocean swell. Nature. 558, 383-389, doi:10.1038/s41586-018-0212-1. This suggests a relationship between the absence of sea ice and sudden ice shelf collapse. Suggest also citing: Reid. P. and R. Massom, 2019; Antarctic Coastal Exposure. Submitted to Nature (December 2019). It is worth including since it provides a climatology of absence of sea ice, particularly along the West Antarctic coastline. [Kushla Munro, Australia]	Taken into account. Massom et al. (2018) already cited (Section 9.4.2.1). Reid and Massom not cited.
61899	78	45	78	45	change /C to C-1 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The exponent 2 and the degree sign could be read as exponent 20, which would be very confusing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51515	78	47	78	50	This section seems to be based entirely on one study. Though we acknowledge that you have emphasized that the results are based on 'limited evidence', it would be useful to make it clear to policymakers that it is a single study, perhaps by including 'In one study, ...' at the start of the section. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rewritten this passage, referring to "this study" to clearly indicate that it's only one study.
55107	78	47			I believe the cited reconstructions also make use of in situ data. I suggest "Reconstructions using a combination of observations and simple snow models...." [Nancy Hamzawi, Canada]	Accepted. This is correct. We have implemented this change as suggested.
61901	78	49	78	49	change snow cover extent so SCO [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now use SCE here.
61903	79	1	79	1	MODIS has never been introduces, should be in the glossary [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We do not mention MODIS in the revised text.
88319	79	5	15	6	Shouldn't the SROCC conclusions be given first in this section so it is clear what is new in the AR6 assessment? [Sharon Smith, Canada]	Taken into account. The SROCC starting point is much more clearly stated in the revised version.
26413	79	9	79	9	northern hemisphere -> in capitals [María Santolaria-Otín, France]	Accepted. Done.
51517	79	9	79	18	Another recent reference could be included here: Peng et al. 2013. This study discusses the timing and duration of the snow cover season in the Northern Hemisphere including snow onset. https://iopscience.iop.org/article/10.1088/1748-9326/8/1/014008 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This strengthens our point further. The paper is cited now, along with the JASMES paper by Hori et al. Completing the references given before, they all show that the NOAA CDR is an outlier in the fall season.
62001	79	10	79	10	fall' should be replaced with 'autumn'. This would also be in agreement with the use of 'autumn' in line 14. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Done.
55109	79	11			Meaning of "recent" is unclear. I suggest it be omitted [Nancy Hamzawi, Canada]	Accepted. Implemented as suggested. The assessment at the end of the paragraph indicates an exact period.
55115	79	13	79	15	I think the connection with the previous statements would be clearer by reversing the sentence order: "The positive trends from the NOAA-CRD are also inconsistent with reports demonstrating later autumn snow-on dates since 1980 (-0.6 to -1.4 days per decade) based on historical surface observations, model-derived analyses and independent satellite datasets (updated from Derksen et al., 2017)." [Nancy Hamzawi, Canada]	Accepted. Implemented as suggested.
55111	79	13			It's important to mention Hori et al., 2017 (10.1016/j.rse.2017.01.023) which is an analysis using data from optical satellite-borne instruments and reports negative trends for all seasons. [Nancy Hamzawi, Canada]	Accepted. This paper is cited now here, along with surface observations by Peng et al.
55113	79	13			I suggest referencing Mudryk et al. (2017) rather than the 2014 paper. While both examine October trends from NOAA, the 2017 paper examines other the other observation-based analyses mentioned [Nancy Hamzawi, Canada]	Accepted. Implemented as suggested.
62003	79	17	79	17	An inclusion of 'northern' before 'hemispheric' will explicitly attribute this SCE trend to the northern hemisphere, avoiding any potential ambiguity. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We use "NH" here.
73985	79	20	79	31	This paragraph actually states that the uncertainty in detected global trend is too large due to considerable regional and inter-dataset variability. The question arise: what was the main purpose of this analysis? It seems that the only purpose was to find one more signature of climate change. But what is the use of this information for decision makers? What conclusions do they make? [Elena Kozlovskaya, Finland]	Noted. The recent paper by Pulliainen et al. strongly reduces these uncertainties and allows us to provide more useful information.
64833	79	20	79	31	A new dataset of SWE report a decrease of SWE in North America over 1980-2018 whereas no significant trend is found in Eurasia. Tis is described in" Pulliainen, J., Luojus, K., Derksen, C. et al. Patterns and trends of Northern Hemisphere snow mass from 1980 to 2018. Nature 581, 294–298 (2020). https://doi.org/10.1038/s41586-020-2258-0" [Martin Ménégoz, France]	Taken into account. Thank you. Paragraph revised to assess these important findings.
55119	79	20	79	31	Consider citing Pulliainen et al. (2020, accepted, doi: 10.1038/s41586-020-2258-0). This paper shows that while total Northern Hemisphere snow mass is decreasing (due to strong North American trends) it is not significantly decreasing over Eurasia and furthermore that trends calculated from reanalysis products in this region may be biased low (too strongly negative). Together this suggests high confidence for changes in total NH snow mass, but less confidence for changes over Eurasia. [Nancy Hamzawi, Canada]	Taken into account. Thank you. Paragraph revised to assess these important findings.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84885	79	20	79	31	A recent study by Pulliainen et al, 2020 investigates patterns and trends of Northern Hemisphere snow mass from 1980 to 2018 and should be included in this paragraph. On the basis of bias-corrected GlobSnow 3.0 estimates, they find different continental trends over a 39-year satellite record. For example, snow mass decreased by 46 gigatonnes per decade across North America but had a negligible trend across Eurasia; both continents exhibit high regional variability. Reference: Pulliainen, J., Luojus, K., Derksen, C. et al. Patterns and trends of Northern Hemisphere snow mass from 1980 to 2018. Nature 581, 294–298 (2020). https://doi.org/10.1038/s41586-020-2258-0 [Jan Wuite, Austria]	Taken into account. Thank you. Paragraph revised to assess these important findings.
69677	79	22	79	38	Since 2 submitted papers are listed at 9.5.3 Snow section for instance, it is necessary to confirm a fact of acceptance and the validity of quotation contents at the time of publication. [Konosuke Sugiura, Japan]	Noted. Yes, this is fundamental to the IPCC process and routinely implemented. Only accepted papers are cited in the final version.
55117	79	22			Neither Mudryk et al. (2017) nor Mortimer et al. examine SWE trends. I suggest referencing Mudryk et al. 2020 (doi: 10.5194/tc-2019-320) and Pulliainen et al, 2020 (accepted, Nature, published May 21, doi:10.1038/s41586-020-2258-0) [Nancy Hamzawi, Canada]	Taken into account. Figure 7b of Mortimer et al does present SWE trends. As suggested, we drop Mudryk et al 2017 which should have been 2020. The Pulliainen paper is discussed afterwards in the context of bias corrections, which are the main point of that paper.
55121	79	26			The suggestion here that existing SAR measurements can address SWE trends is problematic. The Lievens et al paper which is cited provides no physical basis for why C-band SAR data should be sensitive to SWE. Furthermore, it uses cross-polarization C-band measurements which are only available since the launch of Sentinel-1A in 2014 so climate-relevant time series do not exist. [Nancy Hamzawi, Canada]	Taken into account. We had written "possible future progress". But indeed this part of the sentence is not necessary, so we cut it.
51519	79	29	79	29	Please add 'northern' before 'Eurasia' as both the Bulygina and the Brown references present results only for the northern part of the continent. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Implemented as suggested.
51521	79	29	79	29	Please consider adding another reference which considered snow depth changes across Eurasia (this time including southern data). Zhong et al. 2018 - DOI:10.5194/tc-12-227-2018 The authors found an increase in annual mean and maximum snow depth across Eurasia between 1966 and 2012. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence was clarified. We now provide 3 references for long-term (approx. 1960s - present) station data for 1) North America (Kunkel), 2) Tibet (Xu), and 3) Eurasia (Zhong). These confirm spatial and temporal variability, bearing some similarity with, but also clear discrepancies to, the shorter record of Pulliainen. We added Tibet also in response to comment #93577. See reply to comment 55123.
88321	79	29	79	31	Unclear - Do you mean there was a reversal of the trend after 2010? If this is the case you could say that although positve trends was observed 1966-2010, the trend was negative over the last decade (and you could say - resulting in an overall negative trend but maybe not a strong trend?). If you meant that a completely new data set was used in Brown rather than just adding data for the last decade which led to different results then you need to be clear that is the case. [Sharon Smith, Canada]	Taken into account. This has been replaced by a clearer assessment of long-term station data trends over the whole hemisphere (see replies to comments 55123 and 51521)
62095	79	29	79	31	The switch from earlier positive trends to the updated timeseries showing negative trends needs another line to clarify this. At present, it just highlights the huge uncertainity in these data if trends can switch from positive to negative through a updated time-series. Brown et al 2017 states "SWEmax has decreased over pan-Arctic land areas over the past ~20 years, but with large uncertainties in trend magnitude and regional pattern" so maybe a low or medium confidence statement could be associated to the statement here to reflect the uncertainities? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This admittedly imprecise sentence has been replaced by an assessment of long-term station data trends over the whole hemisphere (see replies to comments 55123 and 51521).
55123	79	30			Examining figure 3.10 from Brown et al. (2017) it is clear that that the update did not result in "predominantly" negative trends as there are still widespread regions of increasing SDmax; in fact by area there appears to be more increasing SDmax than decreasing SDmax. The Brown et al (2017) report states that the updated trends "show less evidence of significant increases and more evidence of significant decreases." Along with evidence from Pulliainen et al. (2020, doi: 10.1038/s41586-020-2258-0) I think this section requires a more carefully considered statement about observed SWE and SD trends over Eurasia. [Nancy Hamzawi, Canada]	Taken into account. This sentence was clarified. We now provide 3 references for long-term (approx. 1960s - present) station data for 1) North America (Kunkel), 2) Tibet (Xu), and 3) Eurasia (Zhong). These confirm spatial and temporal variability, bearing some similarity with, but also clear discrepancies to, the shorter record of Pulliainen. We added Tibet also in response to comment #93577. See reply to comment 51521.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62005	79	36	78	36	In Figure 9.26 the spelling of northern hemisphere is inconsistent with that of the remainder of this section (Section 9.5.1, line 67- 81). It should be written as Northern Hemisphere. Alternatively, all Northern Hemisphere should be changed to northern hemisphere. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
61907	79	36	79	38	SNE is normally measured in m, assumptions when transferring snow mass to SWE should be mentioned in this graph [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. It is true that many studies do report SWE in water height. But snow mass is a mass, and thus given in dimensions of masses (kg, or Gt=10^9 kg), here and in numerous publications.
3123	79	39	79	40	Consider adding a placeholder for Iceland and Scandinavia: Compagno et al. (submitted). Limited impact of climate forcing products on future glacier evolution in Scandinavia and Iceland. Submitted to the Journal of Glaciology. [Daniel Farinotti, Switzerland]	noted, publication not accepted before cut-off and therefore not included in text
61905	79	43	79	43	change snow water equivalent to SWE [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. After restructuring, it seems to us that it is useful to re-state the meaning of the acronyms SWE and SD at the beginning of this section. And we do delete "snow water equivalent" here, as suggested.
88323	79	48	79	49	Suggested revision: " .....SWE trends due to both temperature and precipitation changes…." [Sharon Smith, Canada]	Not applicable. The sentence was cut to comply with word count constraints.
88325	79	50	79	51	Suggested revision: "….SWE at lower elevations associated with shifts from solid…." (you could also put all the references at the end of the sentence and then Marty would not have to be repeated). [Sharon Smith, Canada]	Taken into account. The sentence was rephrased to make the SROCC starting point clearer, so there were no references left in the first part, but the rephrasing was implemented as suggested.
116885	79	52	79	55	Please change the logical flow : SROCC has an assessment, you consider new and additional knowledge, and you confirm (rather than vice versa). [Valerie Masson-Delmotte, France]	Accepted. The SROCC starting point is now given first.
88327	80	1	80	6	Reduction in albedo, reflectance - is this what you are getting at here - maybe you could say that? [Sharon Smith, Canada]	Not applicable. The paragraph was deleted, we refer entirely to Chapters 6 and 7 for these issues.
61909	80	4	80	4	move "Section 7.42 to the end of the sentence, as this is discussion in this section as well [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The paragraph was deleted, we refer entirely to Chapters 6 and 7 for these issues.
61911	80	4	80	4	snow albedo feedback (SAF) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The paragraph was deleted, we refer entirely to Chapters 6 and 7 for these issues.
109071	80	6	80	6	Can reference SOD section 6.3.1.4 for light absorbing particle effects on the cryosphere. [Chaincy Kuo, United States of America]	Accepted. We refer to this section right at the beginning of 9.5.3 now.
15609	80	9	80	24	I find it surprizing that results from the ESM-SnowMIP project are not quoted in this paragraph, such as Krinner et al. (2018, https://doi.org/10.5194/gmd-11-5027-2018) and potentially follow-up studies. [Samuel Morin, France]	Noted. Thank you. The paper mentioned by the reviewer only presents preliminary results. We therefore refrain from citing it. In addition, this helps us keeping the self-citation statistics at a reasonable level.
88329	80	11	80	12	Gallet 2009 and Domine 2012 are pre AR5 so don't really represent advancements since AR5 - probably don't need to include them. [Sharon Smith, Canada]	Accepted. Change implemented as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30061	80	11	80	14	Niwano et al. (2012) is the first study that succeded in simulating temporal evolution of optically equivalent snow grain size using a detailed physical snowpack model, which is fully recognized by Carmagnola et al. (2014). Therefore, I would like to suggest that the sentence "Progress in the observation, description and understanding of snow microstructure (Gallet et al., 2009; Calonne et al., 2017) and its links to physical (thermal and radiative) properties (Domine et al., 2012; Calonne et al., 2014) has prompted efforts to represent physical properties as a function of the evolving snow microstructure in models (Carmagnola et al., 2014; Calonne et al., 2015)." should be updated as follows: "Progress in the observation, description and understanding of snow microstructure (Gallet et al., 2009; Calonne et al., 2017) and its links to physical (thermal and radiative) properties (Domine et al., 2012; Calonne et al., 2014) has prompted efforts to represent physical properties as a function of the evolving snow microstructure in models (Niwano et al., 2012; Carmagnola et al., 2014; Calonne et al., 2015)." Niwano, M., Aoki, T., Kuchiki, K., Hosaka, M., and Kodama, Y. (2012). Snow Metamorphism and Albedo Process (SMAP) model for climate studies: Model validation using meteorological and snow impurity data measured at Sapporo, Japan. J. Geophys. Res., 117, F03008, doi:10.1029/2011JF002239. [Masashi Niwano, Japan]	Accepted. Cited in the revised version, as suggested. Although this is a pre-AR5 paper, it is not cited in AR5 (and not in SROCC either).
15599	80	11	80	17	These statements do not appear fully connected to climate change issues, and I also find it very much biased in terms of literature quoted. I think it could be more fruitful to refer to review papers, from which key messages in terms of scientific understanding and relevance ot climate change issues could be extracted. See e.g. Kinar and Pomeroy (2015) on snow observations (https://doi.org/10.1002/2015RG000481). While the content on snow modelling is quite slim, a starting point could also be Hock et al. (2017) (https://doi.org/10.3389/feart.2017.00064) [Samuel Morin, France]	Taken into account. The paper by Kinar and Pomeroy is a useful reference in addition to Calonne et al 2017 summarizing progress in observational techniques concerning snow structure, and now cited. However, we are not aware of recent review papers on snow modelling, and as the reviewer notes, there is not much content on snow modelling in the Hock et al. paper. Following Comment #15599, we add a reference to a work by the WSL group, diversifying the cited literature.
61921	80	12	80	12	people at WSL et al have been doing much work on that, include those as a reference? E.g.Löwe, H., Riche, F., and Schneebeli, M.: A general treatment of snow microstructure exemplified by an improved relation for thermal conductivity, The Cryosphere, 7, 1473–1480, https://doi.org/10.5194/tc-7-1473-2013, 2013 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
88333	80	12	80	13	Do you mean strengthening the confidence in these estimates (rather than strengthening the high confidence) - or confirming the high confidence of the estimate [Sharon Smith, Canada]	[accepted] text changed
62061	80	17	80	19	Recent progress includes the inclusion of… this phrasing is confusing. Suggest rewording to "Recent progress includes the incorporation of…" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30063	80	17	80	21	Please note Niwano et al. (2018) construct a state-of-the-art polar non-hydrostatic regional climate model that incorporates a detailed physical snowpack model. Therefore, I would like to suggest that the sentence "Regardless of these shortcomings, snow modules of climate models continue to be improved. Recent progress includes the inclusion of multiple energy balances within the canopy and between subgrid-tiles with different snow heights (Aas et al., 2017; Boone et al., 2017) and inclusion of advanced specific snow models in coupled climate models (Vionnet et al., 2012), opening the prospect of future progress in quantifying snow-related feedbacks in a changing climate." should be updated as follows: "Regardless of these shortcomings, snow modules of climate models continue to be improved. Recent progress includes the inclusion of multiple energy balances within the canopy and between subgrid-tiles with different snow heights (Aas et al., 2017; Boone et al., 2017) and inclusion of advanced specific snow models in coupled climate models (Vionnet et al., 2012; Niwano et al., 2018), opening the prospect of future progress in quantifying snow-related feedbacks in a changing climate." Niwano, M., Aoki, T., Hashimoto, A., Matoba, S., Yamaguchi, S., Tanikawa, T., Fujita, K., Tsushima, A., Iizuka, Y., Shimada, R., and Hori, M. (2018). NHM–SMAP: spatially and temporally high-resolution nonhydrostatic atmospheric model coupled with detailed snow process model for Greenland Ice Sheet. Cryosph., 12, 635-655, doi:10.5194/tc-12-635-2018. [Masashi Niwano, Japan]	Accepted. More papers could have been cited here. We cite the suggested reference as an example.
15601	80	20	80	20	As far as I know, Vionnet et al. (2012) does not describe the inclusion of a sophisticated snow cover model in coupled climate models. Voldoire et al. (2019, https://doi.org/10.1029/2019MS001683), for example, may be a better reference about how snow cover is handled in climate models, especially in CMIP6. In this case, a 12-layer snow model was used in CNRM-CM for CMIP6. [Samuel Morin, France]	Accepted. Change implemented as suggested.
61919	80	26	80	30	Does this section belong to permafrost? I don't sere how it fit in here! [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This belongs here, but it has links to permafrost indeed. As it is about model diagnostics and not a model development per se, we thought it should be treated separately from the preceding paragraph discussing model developments. We now include it in the preceding paragraph and rewrote it to better link it to the preceding content.
15603	80	26	80	30	This paragraph contains important information, but it seems a bit disconnected from the rest of the section. I think it could be better included within the text. [Samuel Morin, France]	Taken into account. It was rewritten to better link it to the preceding content and included in that paragraph.
88331	80	32	80	32	Revision "There is high…." [Sharon Smith, Canada]	Accepted. Change implemented as suggested.
85313	80	32	80	32	There' rather than 'The'? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Change implemented as suggested.
61913	80	33	80	33	change snow albedo feedback SAF to SAF [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This is the first mention of the snow albedo feedback in this section, so the acronym should be explained.
55125	80	35			The Thackeray et al. (2016) reference is wrong. The correct reference is Thackeray and Fletcher 2016 (doi: 10.1177/0309133315620999; a different paper from the same year, only 2 authors). [Nancy Hamzawi, Canada]	Accepted. Change implemented as suggested.
15605	80	44	80	47	This paragraph seems to be based on the work of one single team of authors. Isn't there additional analysis from other groups of authors ? [Samuel Morin, France]	Taken into account. We are not aware of any other multi-model analyses of snow in CMIP6 accepted before the literature cut-off date.
1771	80	44	80	55	Data shown in Figure 9.26 on snow cover extent are exceedingly important, but not adequately discussed in lines 44-55. Section 9.5.3 would be strengthened by providing more discussion in the text with analysis of the data from Figure 9.26. [Michael Kennish, United States of America]	Noted. Unfortunately, the section heavily relies on only one available paper. Therefore increasing the length of the discussion here would be rather unbalanced.
61915	80	45	80	45	does snow extent refer to depth or spatial extend ? Then use SCE [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested (using SCE).
69679	80	45	80	49	Since 4 submitted papers are listed at 9.5.3 Snow section for instance, it is necessary to confirm a fact of acceptance and the validity of quotation contents at the time of publication. [Konosuke Sugiura, Japan]	Accepted. This is standard IPCC practice. Only papers accepted by the relevant cut-off date are cited in the FGD.
55127	80	45			Thackeray et al. examined CMIP5 models, not CMIP6 models. Omit reference or rephrase statement [Nancy Hamzawi, Canada]	Accepted. Reference cut. Unclear how this error happened.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55129	80	51	80	55	Very difficult to understand this sentence. I think it would help if the sentence order was flipped e.g."...very likely anthropogenic... because 1) 2) 3) ...." [Nancy Hamzawi, Canada]	Accepted. Order flipped and sentence cut into two pieces.
15611	80	51	80	55	I don't understand this sentence, both in terms of the grammar and the content. This probably needs some editing to become more clear. [Samuel Morin, France]	Taken into account. The sentence was reformulated (see also comment 55129). We hope that it is clearer now.
61917	80	52	80	55	To make this sentence more strong I would suggest to start with : "In summary, because (1)..." as this is more like a conclusion [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This is actually not a summary of the above, it's a reference to an assessment made in Chapter 3. Therefore we prefer not to label this as a summary here.
16429	80	55	80	55	"...and thus IS (?) also caused..."? [Julian Mak, China]	Noted. It is assessed "as also caused by..." - so we think that this part of the sentence should remain as is.
116887	80		80		Please check coherency with chapter 7 on snow feedback. [Valerie Masson-Delmotte, France]	Taken into account. To prevent any inconsistencies, we signposted the relevant sections in Chapter 6 and 7 on surface albedo feedback and light absorbing particles right at the start of this section, and do not treat these issues here.
61923	81	3	81	3	change snow cover extent to SCO [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We suppose the reviewer intended to write "SCE".
69681	81	3	81	50	Since 3 submitted papers are listed at 9.5.3 Snow section for instance, it is necessary to confirm a fact of acceptance and the validity of quotation contents at the time of publication. [Konosuke Sugiura, Japan]	Accepted. This is standard IPCC practice. Only papers accepted by the relevant cut-off date are cited in the FGD (see also 69679).
61925	81	7	81	7	change NH to northern hemisphere [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We write "Northern Hemisphere".
39195	81	17	81	20	Based on only one study, calibrated language for the finding that the projected decrease of annual maximum snow water equivalent will exceed natural variability? [Lourdes Tibig, Philippines]	Taken into account. We delete the calibrated language here.
61927	81	19	81	19	change snow water equivalent to SWE without (SWEmax) as not used anymore [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
61929	81	19	81	19	changes snow cover duration to SCD [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
61931	81	19	81	19	I find the term "later" very confusing in this sentence. ...possibly use "with a time lag" or "delayed compared to..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We reformulated the sentence to make it clearer (hopefully).
55131	81	23			The Thackeray et al. (2016) reference is wrong. The correct reference is Thackeray and Fletcher 2016 (doi: 10.1177/0309133315620999; a different paper from the same year, only 2 authors). [Nancy Hamzawi, Canada]	Accepted. Change implemented as suggested.
22637	81	25	81	25	What is SAF? I suspect this needs to be long handed for clarity. [Peter Thorne, Ireland]	Accepted. Change implemented as suggested (although SAF has been used a few paragraphs before).
82969	81	28	81	29	I suggest adding information on time scales regarding reductions of snow cover projections. [Sebastian Gerland, Norway]	Noted. These were all studies limited to the 21st century, but the whole paragraph was cut to save space. Reference is made to SROCC at the beginning of the subsubsection; no new results are known to us that would challenge the SROCC assessment concerning future mountain snow cover.
73987	81	28	81	45	The larrge overall consistency may be because the same limited data is used in all projections mentioned. That is why this consistency is not a very good critery to demonstrate reliability of the results obtained. [Elena Kozlovskaya, Finland]	Taken into account. This is also very consistent with fundamental process understanding. We reformulated the sentence to make this clear.
51523	81	28	82	4	For all projections stated in this section, pleaes could you ensure that a timescale is included, e.g. 'by the end of the 21st century'. Or make sure to include at the start of the Projections section that 'all projections given in this section are by xxxx year'. Currently it is unclear and makes it difficult to know how imminent or avoidable these changes are. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Timescales (and magnitudes of expected decreases) are now explicitly indicated where appropriate.
61933	81	33	81	35	hard to read this sentence "with total compensation REMAINING A POSSIBILITY..." maybe .. This enables the option of a total compensation in the highest... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The whole paragraph was cut to save space (reference is made to the unchallenged SROCC assessment concerning mountain snow cover).
61937	81	49	81	49	change global surface air temperature to GSAT [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.

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61939	81	50	81	50	transitional season months: add (spring and autumn) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Change implemented as suggested.
61941	81	50	81	50	change /C to C-1 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. °C-1 would induce confusion with the ^2 just before and could then be read as ^20.
55133	81	52			The claim made in this statement is not shown in figure 9.26b (though it is shown by additional figures in the cited paper). Rephrase. [Nancy Hamzawi, Canada]	Accepted. We now clarify that 9.26b shows the situation for spring (MAM)
15613	82	6	82	6	By consistency with the formation in "autumn and winter", I suggest adding "spring and" before "summer". [Samuel Morin, France]	Accepted. Change implemented as suggested.
61943	82	10	82	10	change snow albedo feedback to SAF [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Another reviewer asked us (on a preceding page) to change SAF to snow albedo feedback. Therefore we think we should spell it out here, allowing more readers to follow to text.
55135	82	11	82	11	Is it appropriate to talk about irreversible glacier inception in the current climate context? I think this sentence makes a valid point but I suggest it end it after the two references. [Nancy Hamzawi, Canada]	Noted. As this part is about irreversibility, we would like to keep this part of the sentence.
64391	82	16	82	18	Literature shows is very vague can you provide references. If I recall correctly there is some informtation in the ocean figure in the SPM of SROCC on this point where you may wish to compare to. [roderik van de wal, Netherlands]	[accepted] changed to 'we conclude', as the literature is already cited throughout the paragraph.
1773	82	17	110	31	Section 9.6 Sea Level Change and Commitment is the most extensive, detailed, heavily cited, and well-crafted section in Chapter 9. As such, the chapter is somewhat unbalanced with greater weight given to sea level change than other chapter components. Section 9.6 is excellent. [Michael Kennish, United States of America]	[Noted] Thank you very much for this compliment.
103857	82	21	82	21	Since the title of the sect. 9.6.1 is 'Global and regional sea-level change in the instrumental era' but 9.6.1.1 is on the global signal I suggest to add 'Global' to the latter title. [Philippe Tulkens, Belgium]	[accepted] changed
80881	82	21	82	21	Since the titel of the sect. 9.6.1 is 'Global and regional sea-level change in the instrumental era' but 9.6.1.1 is on the global signal I suggest to add 'Global' to the latter title. [Louise Sandberg Sørensen, Denmark]	[accepted] changed
107477	82	21			Section 9.6.1.1 is on sea-level change since 1990, but the text only discusses the 20th century (1900-1990). Suggest changing the title [Jennifer Walker, United States of America]	[not applicable] the section title already was 'since 1900'
62189	82	23	82	24	Sentence is a little convoluted maybe edit. e.g. "...developed and applied, including one GMSL curve (compiled by Dangendorf et al., submitted) in addition to the reconstructions previously presented in SROCC [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[editorial] sentence rewritten
78347	82	23	82	25	This section depends on a submitted paper. Had the paper concerned new data that would probably have been unproblematic although clearly one can not review or consent or agree to a section based on an unpublished paper. So the leading authors should be very careful as procedurally this is completely wrong. As this is a statistcial analysis and recosntructions at best (for the period up to 1990) broadens the scope of of estimates. So, to be done properly other estimates must be compiled and this put in context. It also appears strange that the discussion is not by reference to the previous IPCC reprot but to the SROCC. Clearly the authors wish to update these results but this report has, in my understadning a different objective. [Michael Tsimplis, China]	[taken into account] the previous IPCC report that discussed sea-level change is the SROCC, so that is taken as our starting point. The submitted paper was a placeholder, the FGD assessment of reconstructions is done in Chapter 2 following the AR5/SROCC approach.
22641	82	23	82	35	This paragraph should make appropriate cross-reference to the corresponding section in chapter 2 to avoid redundancy of assessment and also to highlight cross-linkages. [Peter Thorne, Ireland]	[accepted] cross-references added in text and in table 9.4.
39111	82	23	82	35	Regarding the sea level budget, according to Vishwakarma et al, 21st centruy budgets have to consider ocean bottom deformation due to incresed mass load. Vishwakarma, B. D., et al. "Sea level budgets should account for ocean bottom deformation." Geophysical Research Letters 47.3 (2020): e2019GL086492. [Ola Kalen, Sweden]	[taken into account] Yes, but according to the same publication (and earlier publications), this is only applicable to satellite-era changes and therefore relevant in section 9.6.1.2, not in 9.6.1.1. The SROCC corrected their altimetry estimates of GMSL for bottom deformation, and we have followed the SROCC in this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129507	82	23	82	35	Given the estimates in this section, it is very lightly referenced. There is also little justification for why Dangendorf et al. (2019) is used as the reference for the 20th century GMSL numbers here and in Table 9.4. [Trigg Talley, United States of America]	[noted] The Dangendorf reconstruction was a placeholder value, as the assessment for GMSL still needed to be completed. It is now no longer the only reconstruction used for GMSL. In FGD the assessment of GMSL is presented in 2.3.3.3, a cross-reference has been added. Similarly, the numbers in table 9.4 are based on assessments elsewhere in chapters 2 and 9 - cross references have been added to the table.
50001	82	23	82	52	At some point in this section, the regional or local sea-level budgets should be at least mentioned, whether or not they are closed. If addressed below in 9.6.1.4, it should be referenced here. [Daniel Gilford, United States of America]	[noted] the section is now titled 'Global mean sea-level change budget since 1901'. Regional changes & the regional budget are discussed in 9.6.1.4.
37735	82	23	82	52	The models must reflect temperature cycles (eg pause 1945-1970) otehrwise 21st C models will run too hot [Howard Brady, Australia]	[noted] Climate models include internal variability, but its phasing it not tuned to the historical variability.
72025	82	23		28	As I understand, the Dangendorf et al (and earlier papers on which it is based) use the CMIP5 model spacial distributions in their reconstructions. What does this mean about their independence and ability to be used to evaluate the models? And what about the range of earlier reconstructions - are these ignored, and if so why? [John Church, Australia]	[accepted] for the FGD we have followed the AR5/SROCC approach, but have only used hybrid estimates to determine the uncertainty, not the ensemble mean. Details of the assessment of GMSL are in Section 2.3.3.3 of this report.
77849	82	24	82	24	What do you mean by a "GMSL curve"? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] changed to 'GMSL reconstruction'
76715	82	24	82	24	Dangendorf et al. Submitted (has this been published already. Is ti this one : https://www.nature.com/articles/s41558-019-0531-8? [Roelof Rietbroek, Germany]	[noted] yes this is the publication we meant, it is now accepted for publication.
20197	82	24	82	24	What does " including one GMSL curve" mean? [philippe waldteufel, France]	[noted] changed to 'GMSL reconstruction'
129509	82	24			The Dangendorf et al. (2019) is used very heavily as a reference throughout this chapter. There are processing and methodological choices within that paper that do not make it above criticism. Given the vast number of other studies on these topics, strongly recommend adding to the references. [Trigg Talley, United States of America]	[noted] The Dangendorf reconstruction was a placeholder value, as the assessment for GMSL still needed to be completed. It is now no longer the only reconstruction used for GMSL. In FGD the assessment of GMSL is presented in 2.3.3.3, a cross-reference has been added. Similarly, the numbers in table 9.4 are based on assessments elsewhere in chapters 2 and 9 - cross references have been added to the table.
62159	82	25	82	27	Section 9.6.1.2 stated that GMSLR has accelerated since 1993, as resolved by satellite altimetry measurements. Including the rate of change between 1993-2018 (or referencing the section above) highlights that the rate between from 1993 onwards represents an amplification of the 1900-1990 (longer timescale) GMSLR trend. This addition ensures consistency - e.g. arriving at the 0.15-0.22 m total sea level change requires an increase in the rate of sea level change [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] The rates for 1993 onwards are provided in Section 9.6.1.2 and acceleration is discussed in 9.6.1.1 and 9.6.1.2. We have added the rate for the period 1901-2018 in section 9.6.1.1.
78349	82	25	82	28	The quoted value disagrees with earlier estimates in the IPCC reports and the literature. Some detail why the previous estimates were wrong and why this one is right is naturally needed. [Michael Tsimplis, China]	[noted] The Dangendorf reconstruction was a placeholder value, as the assessment for GMSL still needed to be completed. It is now no longer the only reconstruction used for GMSL. In FGD the assessment of GMSL is presented in 2.3.3.3, a cross-reference has been added, and numbers are compared to previous reports.
78351	82	25	82	28	0-15-0.22 m between 1900-2018 gives linear trends of 1.26 mm/yr to 1.85 mm/yr. The 2007 report gives a value of 1.8 mm/re with a range of 1.3-2.3 mm/yr for 1960-2006. In any case, why should the section start with the sea level rate up to 1990 and not with the sea level rate for the whole period? Isn't that what is of interest for anyone interested. [Michael Tsimplis, China]	[noted] These numbers were based on the placeholder reconstruction of Dangendorf. The numbers have been revised following the updated assessment of GMSL in chapter 2 which now follows the AR5/SROCC ensemble approach. The numbers up to 1990 were presented in AR5 and SROCC and we therefore provide them here for traceability.
62161	82	26	82	26	incorrect usage of uncertainty language in IPCC. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[rejected] this phrasing is used in SROCC as well

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1795	82	26	82	27	The rate of GMSL rise adopted here for 1900-1990 (1.25 mm/yr, [0.98-1.52]) is meaningfully lower than the 1.5 mm/yr [1.3-1.7] what was reported in AR5. This is an important revision that should be emphasized much more strongly, not in the least due to its implications for closing the sea-level budget. I note that elsewhere in the text comparisons are made with numbers reported in AR5 for what I would consider details by comparison (e.g., the glacier contribution to future sea-level rise). [Torbjorn Tornqvist, United States of America]	[taken into account] These numbers were based on the placeholder reconstruction of Dangendorf et al. The number has been revised following the updated assessment of GMSL in chapter 2 which now follows the AR5/SROCC ensemble approach. It is noted that the current estimate is consistent with the SROCC assessment.
77855	82	26	82	27	How is the assessment of 1.25 [0.98-1.52] mm yr-1 arrived at on the basis of the various papers available? It is surprisingly precise. In Table 9.4, it is attributed to Dangendorf et al. Are you depending on that paper for your assessment? If so, please could you say so (as well as describing how it's done). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] the number has been revised following the updated assessment of GMSL in chapter 2 which now follows the AR5/SROCC ensemble approach. A cross-reference has been added to chapter 2.
69223	82	27	82	27	In SPM B.3.2, the discription 0.19m (likely 0.15-0.22m) should appear, 0.15-0.22m. "0.19 [0.15-0.22]m". [Kaoru Magosaki, Japan]	[taken into account] the SPM is based on the numbers presented in the FGD.
51973	82	28	82	31	It is not clear where in Zanna et al. (2019) the value of 0.38 [0.32-0.44] mm/yr stated here is derived from. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	[noted] The number has been derived from the time series of Zanna et al. A cross reference is added to section 2.3.3.1 where the OHC/thermosteric estimate is assessed.
97021	82	28	82	31	Thermosteric contribution from 1870 to ? Please complement the missing year. [Nicole Wilke, Germany]	[accepted] text revised
107475	82	30	82	31	The time period of this thermosteric contribution is unclear. There is a reconstruction back to 1870, but then next sentence is about 1900-1990 time period [Jennifer Walker, United States of America]	[accepted] text revised
76717	82	31	82	31	0.38 mm/yr Where does this number come from? I don't find it in Zanna et al 2019, who talks about 0.8-0.9 mm/yr over 1960-2017 and 1.1-1.2 mm/yr over 1990-2017 [Roelof Rietbroek, Germany]	[noted] This is derived from the time series of Zanna et al. A cross reference is added in the Table, pointing to section 2.3.3.1 where this estimate is assessed.
62163	82	31	82	35	This statement, and first half of Table 9.4, suggest that the sea level budget can be closed withoout Antarctica's contribution. Consider revising to clarify whether or not this is the case. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] text revised and Antarctic estimates added
39759	82	31		35	"in combination … Table 9.4)" what about the Antarctic ice sheet? Nothing is said here [TSU WGI, France]	[accepted] text revised
77851	82	34	82	35	Does this mean you conclude that the Antarctic contribution is indistinguishable from zero? If so, this should be stated, because it's a significant point. On the other hand, if there is evidence of a non-zero contribution, does that present a problem for the budget? In particular, how is this to be reconciled with a possible small long-term GMSLR rise in the past couple of millennia, or is there new evidence which constrains that to be consistent with zero? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] text revised
50003	82	37	82	38	Have any studies tried to project this with D/A or some strategy to cut through this internal variability? If so, can they be cited and discussed briefly here? [Daniel Gilford, United States of America]	[noted] section moved and rewritten
27657	82	41	82	41	About '[…] (Slangen et al., 2014b, 2015 [...]': For the effect on sea level change of the absence of volcanic forcing in the pre-indrustrial control run, more relevant publications addressing that specific issue would be references already cited in AR5, e.g. Gregory et al. 2010 (10.1029/ 2010GL045507), 2013 (10.1002/grl.50339). [Eric Brun, France]	[not applicable] this piece of text has been removed as it did not add new information w.r.t. SROCC
62191	82	42	82	44	this statement seems a little lost to me as it simply states that adding more models did not improve the analysis raising a question mark why that is. Model inter-dependencies? I think Reto Knutti probably has a paper to back this statement with a citation. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] this piece of text has been removed as it did not add new information w.r.t. SROCC. However, we would like to note that adding models does not necessarily mean that the analysis will improve. Apart from possible inter-model dependencies, an alternative explanation might be that the ensemble was already large enough to represent the spread in the models.
27659	82	44	82	44	The different modelled contributions to the sea level budget are not computed from the same ensemble of CMIP runs. As a result the uncertainty in each contribution are not consistent with each other. This should be probably stated somewhere like in the table 9.4 caption [Eric Brun, France]	[not applicable] this piece of text has been removed as it did not add new information with respect to SROCC
33489	82	44	82	47	Add dot at the en of the sentence: "The glacier estimate is based on 16 model runs forced by CMIP5 data (compared to 12 in SROCC), which leads to minor changes of the order of 0.01 mm yr-1 in the mean and spread (Marzeion et al., 2015b)". [Guiomar Rotllant, Spain]	[not applicable] this piece of text has been removed as it did not add new information with respect to SROCC

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77853	82	46	82	48	It would be helpful to state the range here from the sum of terms, for comparison with obs range in the previous paragraph, and then quantify the residual. I'm not clear what the conclusion is. Are the two ranges statistically distinguishable or not? If not, then the residual is insignificant and you don't need any hypotheses to explain it. On the other hand, if you think the residual does indicates an inconsistency, then can't you conclude which term is the problem, since in the previous paragraph you gave estimated contributions for each of the terms (except Antarctica) which can be compared individually with the modelled ones? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] this piece of text has been rewritten and sum of terms and total observed are compared in 9.6.1.1
27661	82	49	82	52	I suspect the residual remains between the mean of the modelled GMSL and the mean of the observed GMSL rather than between the medians. If yes please state it clearly [Eric Brun, France]	[noted] values in table 9.4 have been updated.
49999	82	51	82	51	It's unclear what "might have disappeared" means in this context and how it specifically would result in the residual. This should be rewritten for specificity. [Daniel Gilford, United States of America]	[not applicable] this sentence has been removed as it did not add new information with respect to SROCC
27663	82	52	82	52	What about a contribution from Antarctica peripheral glaciers that is not accounted for in observations? Given table 9.4 it could be an explanation no? [Eric Brun, France]	[rejected] This part of the text was about model simulations, not about observations. It no longer is present in the FGD
64389	82	52	82	52	is the confidence as in SROCC, you argue values are alike but you don't clarify whether you final confidence is also in agreement with SROCC [roderik van de wal, Netherlands]	[noted] this piece of text has been rewritten and now includes the confidence statements of SROCC
62193	82	52	82	52	Both hypotheses are assigned low confidence as a result of limited evidence. Maybe this sentence could be deleted and instead “(low confidence)” can be added inside the sentences i) and ii). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] this sentence has been removed as it did not add new information with respect to SROCC
53541	82				Although I did not review extensively CH9, it seems that it does not include a detailed assessment of the land contribution to global and regional sea level rise (beyond the glacier contribution). It is not covered by CH8 and you may want to check that the assessment about continental runoff in CH3 and CH4 is sufficient so that there is no major gap on this topic. Please, find hereafer a few useful recent references that could be quoted in CH9: https://link.springer.com/article/10.1007/s10712-016-9399-6 https://www.nature.com/articles/s41598-019-54239-2 https://link.springer.com/article/10.1007/s10712-019-09536-w [Hervé Douville, France]	Taken into account. The revised sea-level budgets do take into account land contribution to observed sea-level changes.
103861	83	1	83	1	Same comment as above [Philippe Tulkens, Belgium]	[accepted] 'global' added to title
80883	83	1	83	1	Same comment as above [Louise Sandberg Sørensen, Denmark]	[accepted] 'global' added to title
39815	83	1			"satellite era" should be defined in more clearer terms for non-remote sensing specialists. [TSU WGI, France]	[accepted] The year 1992 is mentioned in the revised text.
22643	83	3	83	17	This paragraph should make appropriate cross-reference to the corresponding section in chapter 2 to avoid redundancy of assessment and also to highlight cross-linkages. [Peter Thorne, Ireland]	[accepted] cross-references added in text and in table 9.4
37737	83	3	83	55	AR6 must address difference between the average world wide tide guage data (Liverpool) and the satellite database as these are two different methods and the difference is not acceleration [Howard Brady, Australia]	[noted] the tide gauge reconstructions and global mean satellite estimates are assessed in chapter 2 of this report.
37741	83	3	83	55	AR6 must address no accleration in satellite database 2008-2018 [Howard Brady, Australia]	[noted] satellite-based estimates of acceleration in global mean sea level are assessed in chapter 2 of this report.
83393	83	4	83	4	I suggest writing "to" instead of "-" between start and end years of given periods, so that suchs time spans can be easily distinguished from a range related to uncertainty (such as given here in […]). [Sebastian Gerland, Norway]	[editorial] This has been corrected.
50005	83	4	83	18	In this section it needs to be more clearly stated as to why it is so difficult to close the regional sea level budget. A listing of challenges would be helpful, with identification of the primary ones. i.e. is it only internal variability, measurement uncertainty, something else? [Daniel Gilford, United States of America]	[accepted] text revised
62165	83	6	83	6	The use of “uncertainty” here has a different meaning to that used in IPCC reports, consider changing to "measurement error" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] text revised
33491	83	8	83	9	Change: “…the glacier contribution is now based on (Zemp et al., 2019), AIS based on (The IMBIE Team, 2018) and GrIS is based on (The IMBIE Team, 2019).” By “…the glacier contribution is now based on Zemp et al. (2019), AIS based on The IMBIE Team (2018) and GrIS is based on The IMBIE Team (2019).”. [Guiomar Rotllant, Spain]	[not applicable] sentence has been removed, cross-references to assessment sections have been added to the table 9.4.
129511	83	10	83	11	Clarify the reference to the data in the parenthetical statement "(going from 23 to 26 years, and from 9 to 13 years, respectively)". [Trigg Talley, United States of America]	[not applicable] sentence has been removed in revisions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51975	83	11	82	11	How does this decrease the impact of internal variability on the trend estimates? I see how a longer timeseries should give lower error on a linear trend, but the implications for internal variability component are not apparent. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] sentence has been removed in revisions
62195	83	11	83	14	Some more discussion desirable. It is somewhat well documented that internal variability can mask or enhance observed trends on those timescales. So it is not surprising to see reverse impacts when including more data points. e.g. Swart et al 2015 Influence of internal variability [...] and references therein [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] sentence has been removed in revisions
39743	83	13		17	"for 2006-2018,,,, Table 9.4)" shouldn't the value be explicitly stated in the text? That seems like an important result to me. [TSU WGI, France]	[accepted] values added
20993	83	14	83	14	After Thermal expansion Add of the ocean in the tropical region [Ladislaus Chang'a, United Republic of Tanzania]	[rejected] thermal expansion occurs not only in the tropical region.
20995	83	14	83	14	After glacier Delete Changes Replace with melting [Ladislaus Chang'a, United Republic of Tanzania]	[taken into account] changed to 'mass loss'
76719	83	14	83	14	glacier changes → cryospheric changes or glacier and ice sheet changes [Roelof Rietbroek, Germany]	[rejected] We really meant glacier changes: these were already large in the period prior to the satellite era (up to 1990) and they continue to be large. Ice sheets, in contrast, were a much smaller contribution throughout the 20th century.
76723	83	16	83	17	I would consider adding a "disclaimer" for the uncertainty of some of the variable components in particular the hydrological component. I think the study by Bridget Scanlon et al 2018 (Global models underestimate..., pnas) justifies a low or medium confidence to the hydrological contribution to sea level. Also both positive and negative hydrological trend estimates exists from GRACE (Riva et al. 2010, Jensen et al 2012, Reager et al 2016, ..), with some estimates barely distinguishable from zero due to the variability. Hydrological variations are also party compensated by seteric sea level changes. [Roelof Rietbroek, Germany]	[taken into account] thank you for these suggestions, discussion on the land water component has been extended.
27667	83	19	83	19	About 'Using RCP8.5 model output, […]': Please specify if CMIP5 or CMIP6 model outputs are used here. [Eric Brun, France]	[not applicable] text has been removed in revisions
27665	83	19	83	20	We suggest to recall rapidly that the RCP8.5 scenario is a good proxy for the recent years forcing because the volcanic activity has been low and the emission rate close to the one from the RCP8.5. [Eric Brun, France]	[not applicable] text has been removed in revisions
90705	83	19	83	21	Under climate warming, snow water equivalent (SWE) could increase in high latitudes (Räisänen, 2008), broadly where the NDJFM mean temperature is ≤ –20° C in the late twentieth century, where an increase in total precipitation generally dominates over reduced snowfall. Below this threshold, the winter precipitation and snowmelt are more sensitive to warming and so SWE will decrease. Räisänen, J., 2008: Warmer climate: less or more snow? Climate Dynamics, 30, 307-19. [Thian Yew Gan, Canada]	Noted. This is an important paper, but it's a pre-AR5 reference.
78355	83	19	83	28	How can it be claimed that the budget is closed when the numbers in Table 9.4 are not provided - so they must not be known or agreed. Is the report based on beliefs rather than evidence? Extremely disappointing on the way this is produced. [Michael Tsimplis, China]	[noted] The budget closure was clearly indicated by placeholders. The non-placeholder numbers in this paragraph in SOD were based on available numbers and on SROCC assessments - this could have probably been indicated more clearly. The text and figures have been revised for the FGD.
42567	83	19	83	28	It is somehow surprising why RCP8.5 output is used - readers may 1) wonder why in a section concerning sea level state since 1900 projections are used and 2) expect to read results from the AR6-scenarios and not from AR5… I think a short preamble at the beginning of the sealevel chapter explaining the backgrounds would prevent from this confusion. [Sabine Hüttl-Kabus, Germany]	[not applicable] The text has been removed in revisions
3303	83	24	83	25	Next to Dangendorf et al. 2019, there's also a new sea-level curve currently in review (Frederikse et al. submitted), which gives a slightly higher trend over 1900-1990 (1.45 +/- 0.3 mm/yr), and shows closure of the sea-level budget. [Thomas Frederikse, United States of America]	[noted] thanks for this suggestion. The study has now been included.
129513	83	24	83	25	Next to Dangendorf et al. (2019, there's also a new sea-level curve currently in review (Frederikse et al., submitted), which gives a slightly higher trend over 1900-1990 (1.45±0.3 mm/yr), and shows closure of the sea-level budget. [Trigg Talley, United States of America]	[noted] thanks for this suggestion. The study has now been included.
62167	83	25	83	27	Here, "high confidence" is made less convincing by the word "good". If there is high confidence in the simulations and their results, why would they only be a "good" representation of what is happening in the real world? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] sentence has been removed in revisions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3307	83	31	83	31	Complementary to comments 14,15: Frederikse et al. (submitted) provides a set of trends in observed sea level and the components since 1900, which helps determining the closure of the budget in the table. This study, which does include some studies that are still considered 'low confidence', such as Parkes&Marzeion 2018, shows good closure of the sea-level budget over all periods considered in the table. [Thomas Frederikse, United States of America]	[noted] thanks for this suggestion. The study has now been included in the discussion.
129517	83	31	83	31	Frederikse et al. (submitted) provides a set of trends in observed sea level and the components since 1900, which helps in determining the closure of the budget in the table. This study, which does include some studies that are still considered "low confidence" (such as Parkes and Marzeion, 2018), shows good closure of the sea-level budget over all periods considered in the table. [Trigg Talley, United States of America]	[noted] thanks for this suggestion. The study has now been included in the discussion.
3305	83	31	83	35	Frederikse et al. (submitted), same study as referred to in comment 14, shows closure of the sea-level budget over 1900-2018, 1900-1990, and 1971-2018. It may be worthwhile to add the results of that study here, as it gives confidence in our understanding of global sea-level changes over the 20th-century [Thomas Frederikse, United States of America]	[noted] thanks for this suggestion. The study has now been included in the discussion.
129515	83	31	83	35	Frederikse et al. (submitted) shows closure of the sea-level budget over 1900-2018, 1900-1990, and 1971-2018. It may be worthwhile to add the results of that study here, as it gives confidence in understanding of global sea-level changes over the 20th century. [Trigg Talley, United States of America]	[noted] thanks for this suggestion. The study has now been included in the discussion.
78353	83	31	84	12	This Table is incomplete and again it cannot express consensus nor can it be reviewed properly. Had this been a research paper it would certainly be rejected and had it been a professional report it would also be considered substandard. Very disappointing that the IPCC report is going to be published without proper revision and thus the assessment would be a refelction of the views of a small of group of scientists. [Michael Tsimplis, China]	[noted] The numbers and references in this table are based on assessments earlier in the report. The table has been revised for FGD.
78357	83	31	84	12	Some parts of the Table reflect a collection of studies (first row) others are relied upon one study only. This suggests selective reviewing of the status of knowledge and is not appropriate for the report. Clearly proper revision of Table 9.4 is required in every respect. [Michael Tsimplis, China]	[noted] The numbers and references in this table are based on assessments earlier in the report. The table has been revised for FGD.
29251	83	31	84	14	This table is currently very busy and crowded, largely due to the inclusion of all the references within the table. I suggest either bolding the numerical values, or including the references with smaller text in order to allow readers to more easily see the numerical values for different components and time periods. [Andra Garner, United States of America]	[noted] the inclusion of references was meant to help reviewers track down sources easily. Table has been revised for the FGD.
42569	83	31	84	14	Table 9.4: I am missing a discussion of the differences between observations and modelled values. For the timespan 1900-1990 I would agree on the statement given in text p. 83, lines 23-24 - however, for the three later timespans including satellite era differences are obviously much stronger. [Sabine Hüttl-Kabus, Germany]	[noted] table 9.4 no longer includes model values, as no significant changes were published since SROCC.
42571	83	31	84	14	Table 9.4: Is the 5-95% range (cf. line 35) considered the „likely" range or the "very like" range? [Sabine Hüttl-Kabus, Germany]	[noted] this is the very likely range.
42573	83	31	84	14	#9: Table 9.4: Row „Sum of observed contributions": How are the uncertainty ranges for the sums computed from those of the contributing GMSL-components? Cf. also my comment #21. [Sabine Hüttl-Kabus, Germany]	[accepted] explanation has been added.
9025	83	33	83	34	These glaciers do not belong there, no data, unclear inventory. This has been a miscounting since Meier 1980, where all the numbers were made up from scratch. In fact there is only one study quoted here. I cannot believe thatIPCC continues this tradition of double counting the Antarcti glaciers. Back in the days of Meier, this was an honest guest. Since then, this should really be viewed as fraud. [Eric Rignot, United States of America]	[noted] glacier and ice sheet contributions have been revisited, and are based on assessment earlier in the chapter.
62197	83	33	83	35	Maybe merge last two sentences in figure caption. ...linear trend and 5%–95% confidence intervals [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] caption rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12499	83	33	83	36	I oppose to use the combination of Domingues+Levitus for 1971-2018 estimate, the reasons are: (1) the two separate time series Domingues and Levitus are from two groups, based on a set of completely different techniques including bias-correction, gap-filling, data processing methods. (2). The time scale represented by the two time series are different due to different reprentation of climate variability in their gap-filling methods (Levitus/Ishii time series has much stronger year-to-year variation than Domingues time series, which is much smoother), so the representation of the underlying physical processes are different. (3). It is impossible to give uncertainty estimate for the combination of two different time series, because each time series has their own methods for error bar and they are different. Therefore, they should not be simply added up, adding them up does not provide the best estimate for OHC change. Note that AR5 did so because Domingues time series suffers less for systematical bias associated with gap-filling method compared with other estimates, and there is only one 700-2000m time series available at that time, so Domingues+Levitus combination in AR5 is a trade-off. Since AR5, there are more estimates available for 0-2000m OHC since late-1950s including Ishii et al. 2017, Cheng et al. 2017, Levitus et al. 2012-updated, so my recommendation is to use these estimates rather than sum up two different estimates. These new time series also provide error bars in a self-consistent manner and should be used. [Lijing Cheng, China]	[noted] The values in table 9.4 are based on the cross-AR6 assessment of OHC/ThSL, which is detailed in chapter 2.
12501	83	33	83	36	One method to provide final AR6 OHC time series is: take an ensemble appoach: (1) pre-1960, use Zanna et al. and its uncertainty range. (1) 1960-2005, use ensemble of Cheng et al. 2017, Ishii et al. 2017, Levitus et al. 2012, take the ensemble mean for final estimate and their standard deviation for uncertainty range, this can be done for upper 2000m. If LAs want to add Domingues+Levitus as another ensemble member, it is fine because it was used in AR5 and Cheng et al. 2019. (3). Use all available data products after 2005 except Zanna et al. to make another ensemble, calculate ensemble mean and standard deviation for central estimate and error bar, respectively. This approach provides a OHC time series spanning from late-1800s to present and its uncertainty range, is a balanced IPCC-stype assessment, and to a large extend takes account of the progress made since AR5. [Lijing Cheng, China]	[noted] The values in table 9.4 are based on the cross-AR6 assessment of OHC/ThSL, which is detailed in chapter 2.
51977	83	33	84	1	First entry in Table 9.4: it is not clear where in Zanna et al. (2019) the value of 0.38 [0.32-0.44] mm/yr stated here is derived from. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	[noted] This is derived from the time series of Zanna et al. A cross reference is added to section 2.3.3.1 where this estimate is assessed.
20569	83	33	84	14	Every line on the observed budget contributes to the acceleration, excepting the continental glaciers. An interesting case, although not a major contributor, is the terrestrial storage, the rate of change of which changes sign over a century. The water cycle chapter does not comment this evolution [philippe waldteufel, France]	[noted] this is now included in section 9.6.1.1.
72023	83	33		36	This table is only half complete in the second order draft! What are the range of potential Antarctic contributions for the historical periods. [John Church, Australia]	[noted] the table was as complete as the literature allowed at the time of SOD. The table has now been completed with available ranges stated.
22645	83	36	84	1	The observed GMSL change row should use the assessed ranges arising from chapter 2 and not individual papers in the FGD. [Peter Thorne, Ireland]	[noted] It does. We used the numbers and references as provided by chapter 2 at the time of SOD. For FGD we have coordinated again with Ch2 and include the assessed range.
97023	83	36	84	1	Perhaps Table 9.4 would be more clearly arranged with only values in the table and references in the legend below. [Nicole Wilke, Germany]	[taken into account] Table has been revised for the FGD, with references to sections (and associated literature) provided.
32175	83	36			In Table 9.4, last column: reference Table 9.3 instead of Table 9.4 [Anja Wendt, Germany]	[editorial] changed
76725	84	1	84	1	I don't have the wada paper at hand. Does the terrestrial water storage trends now alos include climatic induced trends? (in contrast to the AR5 estimates) [Roelof Rietbroek, Germany]	[noted] thanks for the suggestion. We have used more recent estimates of the LWS estimate which do include the natural component.
77857	84	3	84	4	Is Cazenave The traditional global-mean energy balance framework employed for global energy budget estimates (Gregory et al., 2002)et al. the only available paper? Do you depend on it for your assessment? In Table 9.4, it's attributed to Oppenheimer et al. (2019). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] The sentence states what SROCC/Oppenheimer et al based their assessment on, which is Cazenave, which in turn is an assessment of the literature available in 2018. The numbers in SOD Table 9.4 were the assessed SROCC numbers. In FGD, these numbers have been extended for the period up to 2018, and are based on an ensemble of satellite products.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27347	84	9	84	9	Gregory et al. 2010, 2013 would be more relevant references than Slangen et al., 2014b, 2015 [Eric Brun, France]	[not applicable] text has been removed in revisions
129519	84	14	85	38	Definitive estimates of global or regional sea-level accelerations using long-term tide gauge records and 27-year satellite altimeter data studies cited here did not mention the existence of the long-period sub-harmonic ocean signals (at 55 or 60 year period) (Chambers et al., 2012; Iz et al., 2014), which impedes sea-level acceleration estimates. Published estimates of globally averaged sea-level acceleration using 27-year satellite altimeter records (1992-2019) (Chen et al., 2017; Yi et al., 2017; Dieng et al., 2017) -- e.g., at 0.084±0.025 mm/yr^2 (Nerem et al., 2018) -- are significantly higher than the estimates using tide gauge records (Iz, 2017; Iz et al., 2018), presumably because of the short record of satellite altimetry, and also the impact of long-period signals in the ocean. There are confirmations of statistically significant detections of regional sea-level acceleration using long-term tide gauge records up to 2017. Citations: Chambers, D. P., M. A. Merrifield, and R. S. Nerem, Is there a 60-year oscillation in global mean sea level?, Geophys. Res. Lett., 39, L18607, doi:10.1029/2012GL052885, 2012 Iz, H. Biki, Sub and superharmonics of the lunar nodal tides and the solar radiative forcing in global sea level changes, Jl. Geodetic Sci., 4:150-165, 2014. Iz, H.B., Acceleration of the global coastal sea level rise during the 20th century re-evaluated, Jl. Geodetic Sci., 7:51-58, 2017. Iz, H.B., C.K. Shum, Chungyen Kuo, Sea level accelerations at globally distributed tide gauge stations during the satellite altimetry era, Jl. Geodetic Science, 10.13140/RG.2.2.10496.48644, 2018. [Trigg Talley, United States of America]	[noted] acceleration text has been rewritten and is now mostly in chapter 2, which is referred to in section 9.6.1.1 and 9.6.1.2
103865	84	17	84	17	Same comment as above [Philippe Tulkens, Belgium]	[noted] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
80885	84	17	84	17	Same comment as above [Louise Sandberg Sørensen, Denmark]	[noted] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
20571	84	17	85	38	Neither does chapter 9 comment the contribution of terrestrial water storage to the MSL rise acceleration! [philippe waldteufel, France]	[noted] this is done in section 9.6.1.1
40769	84	17			section 9.6.1.3 : I feel this section is a bit misplaced: it's mentioning elements from the geological past and it's opening with information on regional sea level, which are both sections below . I would either include the data in the respective parts or move the section [TSU WGI, France]	[taken into account] Section 9.6.1 has been restructured, so that 9.6.1.3 now focus on regional sea-level during the instrumental era.
78359	84	19	84	23	The "visualy identified inflections" appear in a wide time period of several decades. If the report claims that this is evidence for global acceleration then it must explain how these appeared at so different periods and why most recent accelerations (in the 1990s) are uniform. If this is a data problem then surely the answer must be that while we have the earlier paper by Woodworth indicating acceleration in the Atlantic there is uncertainty about the rest of the world due to lack of data. This is a more accurate description of what we know. The techniques used by Ghehrels and Woodworth do not improve the accuracy of the tide gauge dataset they degrade it. As a result the regional acceleration for which we are almost certain becomes doubtful as it is mixed with procedures which fail in many locations to work. [Michael Tsimplis, China]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
88335	85	1	85	1	"greater" rather than "faster" [Sharon Smith, Canada]	[accepted] all 'faster' rates have been changed into 'greater'
77859	85	1	85	2	What do you reckon the rate of rise was during 0-1700 CE? (This is also relevant to my comment on p82 34-35.) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
78361	85	1	85	12	"on average over 0-1700". Surely this is not a good standard to assess acceleration of sea level. Were there periods equal or larger to what has been experienced in the past 100 years? If so natural variability covers the current range and other techniques need to be used to link with climate change. I also hold the view that meta-analyses are completely inappropriate for this type of task and they do not increase the confidence to the result as all analyses examine essentially the same data sets. So I personally disagree with the way this is used to claim high confidence in the acceleration over the past two centuries which, on the basis of direct measurements is evident in the Atlantic. [Michael Tsimplis, China]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
37739	85	1	85	38	AR6 should support NASA GRASP satellite to resolve altimetry problmes in JASON series [Howard Brady, Australia]	[rejected] the task of AR6 is not to resolve altimetry problems, but to assess the literature.
129521	85	4	85	6	Consider adding a figure to illustrate the results of this analysis. [Trigg Talley, United States of America]	[noted] There is such a figure in chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88337	85	6	85	6	If you say there is a rise than you don't need to indicate positve change. It is unlcear if you mean the rate is increasing (acceleration) [Sharon Smith, Canada]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
62169	85	6	85	6	"rise" implies that the direction/rate of sea level change is positive. To clarify that it is referring to accelerating sea level rise, replace "positive" with "accelerated". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
77861	85	7	85	7	“The same” means which one? You cited two at line 4. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
22647	85	10	85	12	This finding was already made, in a slightly separate way in chapter 2 and chapter 2 is concluding around unusualness of mean state and rate globally for many ECVs including sea-level. It would be better to refer to the chapter 2 finding and avoid undertaking a redundant assessment here of the global change. The focus here should be on the regional detail instead? [Peter Thorne, Ireland]	[noted] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
88339	85	11	85	11	Are you referring to an acceleration in GMSL rise? [Sharon Smith, Canada]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
88341	85	14	85	14	Are you referring to acceleration of sea level rise? [Sharon Smith, Canada]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
27349	85	14	85	14	There is a doubt about the meaning of “continuous” acceleration. If what is meant is that there is an acceleration over any small sub-period (of 1 to 30 year) within the 20th century then the data do not support this statement. All the data mentioned here are reconstructions that cannot reproduce precisely the interannual to decadal variability because of structural issues (see Calafat et al. 2014) or because of the tide gauge sampling (see for example Christiansen et al. 2010) . As such they can not be reliable for acceleration over small periods [Eric Brun, France]	[noted] sentence has been removed
78363	85	14	85	17	Isn't it really problematic for the high confidence that, despite the availability of much better and recent data, there is one reconstruction which does not produce, what the aythors claim a "robust" (does it mean statistically significant?) acceleration? Again this reads as beliefs rather than assessment of published results or even expert judgment. [Michael Tsimplis, China]	[noted] apologies that it was not clear that the confidence statement was the confidence statement of SROCC. This has been rewritten, and moved to section 9.6.1.1/2.
61155	85	14	85	24	Tide gauge records can have substantial errors in recording elevation. It might be in interesting to look at this publication Keogh, Molly E., and Torbjörn E. Törnqvist. "Measuring rates of present-day relative sea-level rise in low-elevation coastal zones: a critical evaluation." Ocean Science 15.1 (2019). [Udita Mukherjee, United States of America]	[noted] this is taken into account for the tide gauge reconstructions, as assessed in chapter 2.
107243	85	14		17	It says, "Tide gauge records support a continued long-term GMSL acceleration over the 20th century. As assessed by SROCC (Oppenheimer et al., 2019) and Chapter 2, estimates of the acceleration of sea-level rise over the twentieth century are generally consistent with those of AR5, with four of five available reconstructions showing robust accelerations from 1902-2010 (high confidence)." The highest quality measurement records show little or no sustained acceleration since at least the 1920s, and most show none for even longer than that. For instance, for Honolulu (the best Pacific measurement record, with 100% continuous measurements from 1/1905 through 4/2020, and a near ideal location) linear regression finds a trend = 1.507 ±0.206 mm/yr, and quadratic regression finds an acceleration = -0.00275 ±0.01381 mm/yr². https://sealevel.info/MSL_graph.php?id=1612340 Note that it's mid-Pacific site is nearly ideal, because Oahu is an old, stable island, with very little vertical land motion, because Hawaii gets only small tides, and because its location, near the pivot point of the east-west Pacific "teeter-totter," means it's little affected by ENSO "slosh." [David Burton, United States of America]	[noted] the sentence was about GMSL, not about individual tide gauge sites. Regional acceleration is discussed separately in 9.6.1.3.
80661	85	17	85	17	do you mean from 1902 to 2010 or starting from the period 1902-2010? This type of notation is used elsewhere (e.g. page 85, line 21) and this is confusing [Helene Jacot Des Combes, Marshall Islands]	[editorial] changed
77863	85	17	85	17	Please give references and numbers. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] added, text moved to 9.6.1.1.
99719	85	19	85	20	two episodes of acceleration previously identified? [Peter Clark, United States of America]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
33493	85	19	85	20	Change: “A new tide-gauge analysis (Dangendorf et al., submitted) decomposes the 20th century acceleration into two episodes -- one in the 1920s and 1930s, and one beginning in the 1960s and continuing to today –“ by “A new tide-gauge analysis (Dangendorf et al., submitted) decomposes the 20th century acceleration into two episodes -- one in the 1920s and 1930s, and the other beginning in the 1960s and continuing to today –“. [Guiomar Rotllant, Spain]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
109885	85	19	85	21	This implies that the 1940s-1950s pause in SLR is a new discovery. Should be clear that it has been well known for some time. [Donald Forbes, Canada]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129523	85	19	85	21	These time periods do not appear to be consistent with what is in the study referenced. [Trigg Talley, United States of America]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
78365	85	19	85	24	This paragraph is problematic on many aspects. Statistical analyses can synthesise different curves to the observed (or estimated one) in many ways. Of course these analyses do not correspond necssarily to physical forcings and certainly they do not "cause" the longer term changes. It would perhaps be instructive to state then how many more cm of sea level rise were due to this acceleration. (in any case is this the difference in the rates OR the a in u=uot+1/2 a t^2 ?). If this paragraph can link with any other parameter in the report to explain the acceleration/decalaration change that would be more persuasive otherwise it does not serve much of a purpose. [Michael Tsimplis, China]	[noted] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
77865	85	19	85	24	How does the assessment of acceleration in this paragraph relate to the previous one? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
72027	85	19		24	Earlier reconstructions have similar (but quantitivelt different) periods and accelerations [John Church, Australia]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
27351	85	20	85	20	it would be useful to provide physical insights on the causes of sea level acceleration for the period 1920-1930. [Eric Brun, France]	[noted] A note on the physical connection between sea level accelerations and variations in PDO and ENSO, citing Hamlington et al (2020b) is provided in section 9.6.1.3.
27353	85	22	85	24	Trends over the last 25 years are not comparable to century trends because of the level of internal variability that is substantially different in the two periods. I would be careful to compare periods of the same length. Our poor knowledge of the level of internal variability in the GMSL curve does not allow us to compare periods of such different lengths. [Eric Brun, France]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
62171	85	22	85	24	The word "change" is missing from the beginning of the sentence and could be clarified to: The accelerated GMSL change that began in the 1960s has led to rates of GMSL rise since … [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] this section has been removed, as it was redundant with discussions in ch2 and 9.6.2.
78367	85	24	85	27	The acceleration in the 1990s is visually evident in the sea level record in tide gauges and altimetry so the term (very likely) is probably an underestimation. If there are doubts expressed then these papers and their estimates must be cited. If the issue is that the data are "drift corrected" then this interdependence must be acknowledged expressly and the data not treated as completely independent.Notably, the stated acceleration (with the errorbars) is very close to the earlier cited acceleration for 1968-2015. Again a confusing point [Michael Tsimplis, China]	[noted] text moved and rewritten, GMSL acceleration assessed in chapter 2.
27355	85	26	85	26	It might be worth mentioning that it is an instrumental drift [Eric Brun, France]	[noted] text moved and rewritten, GMSL acceleration assessed in chapter 2.
103867	85	26	85	29	also relevant article: https://doi.org/10.1016/j.asr.2020.01.016 [Philippe Tulkens, Belgium]	[noted] thanks for the suggestion
80887	85	26	85	29	also relevant article: https://doi.org/10.1016/j.asr.2020.01.016 [Louise Sandberg Sørensen, Denmark]	[noted] thanks for the suggestion
77867	85	26	86	27	This sounds illogical, since the satellite data don't cover the whole 20th century. The satellite data and TG data are compared to obtain the assessment of the rate since the 1990s. That's already been dealt with in 9.6.1.2. If there is more to say on that subject, it should probably go in the earlier subsection, because it's not about acceleration. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] text moved and rewritten, GMSL acceleration assessed in chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115041	85	26		29	It says, "Drift-corrected satellite data (Chen et al., 2017; Dieng et al., 2017; Nerem et al., 2017) confirm the acceleration, with the satellite-estimated rate of GMSL rise since the early 1990s more than doubling relative to the 1900-1990 average (Table 9.4), leading to a very likely acceleration of 0.084 ± 0.025 mm yr-2 over the satellite era (Nerem et al., 2017; Cazenave et al., 2018)." You hide a LOT of dubious processing in that phrase, "drift-corrected satellite data." The report should note that successive adjustments to old measurement data from retired satellites have changed an apparent deceleration in sea-level trend into an apparent acceleration! Here's a pair of graphs from Cazenave et al 2014, showing how they managed to almost completely eliminate the decline: https://www.sealevel.info/nclimate2159-f1_large_trimmed1.png Source: https://www.nature.com/articles/nclimate2159 Then in 2017 & 2018 Cazenave et al and Nerem et al turned the formerly-decelerating-but-by-then-linear trend into an accelerating trend, by reducing the sea-level rise "measured" in 20 year-old Topex-Poseidon data; here's Cazenave's graph: https://www.sealevel.info/2017GL073308_fig4_scaled2match2.png Such extraordinary malleability does not prove sea-level acceleration. Rather, it proves that satellite altimetry measurements of sea-level are not fit for purpose. The much higher quality coastal tide station measurements, which do NOT require such radical corrections, show little if any sustained sea-level rise acceleration. [David Burton, United States of America]	[rejected] unsubstantiated claims and personal opinions
107245	85	26		29	It says, "Drift-corrected satellite data (Chen et al., 2017; Dieng et al., 2017; Nerem et al., 2017) confirm the acceleration, with the satellite-estimated rate of GMSL rise since the early 1990s more than doubling relative to the 1900-1990 average (Table 9.4), leading to a very likely acceleration of 0.084 ± 0.025 mm yr-2 over the satellite era (Nerem et al., 2017; Cazenave et al., 2018)." You hide a LOT of dubious processing in that phrase, "drift-corrected satellite data." The report should note that successive adjustments to old measurement data from retired satellties have changed an apparent deceleration in sea-level trend into an apparent acceleration! Here's a pair of graphs from Cazenave et al 2014, showing how they managed to almost completely eliminate the decline: https://www.sealevel.info/nclimate2159-f1_large_trimmed1.png Source: https://www.nature.com/articles/nclimate2159 Then in 2017 & 2018 Cazenave et al and Nerem et al turned the formerly-decelerating-but-by-then-linear trend into an accelerating trend, by reducing the sea-level rise "measured" in 20 year-old Topex-Poseidon data; here's Cazenave's graph: https://www.sealevel.info/2017GL073308_fig4_scaled2match2.png Such extraordinary malleability does not prove sea-level acceleration. Rather, it proves that satellite altimetry measurements of sea-level are not fit for purpose. The much higher quality coastal tide station measurements, which do NOT require such radical corrections, show little if any sustained sea-level rise acceleration. [David Burton, United States of America]	[rejected] unsubstantiated claims and personal opinions
129525	85	26			The use of the word "confirm" here is very strong. There are differences between these estimates that make these two numbers difficult to directly compare as being done here. [Trigg Talley, United States of America]	[noted] wording changed
3309	85	28	85	28	Here, it is worthwhile to note that the acceleration in sea level over the altimetry era is part of a persistent acceleration in global sea level since the 1960s, although the relative importance of the individual contributors differs for 1960s-now (mostly decrease in dam construction and steric) versus 1993-now (mostly related to the ice sheets). [Thomas Frederikse, United States of America]	[noted] text moved and rewritten
129527	85	28	85	28	The acceleration in sea level over the altimetry era is part of a persistent acceleration in global sea level since the 1960s, although the relative importance of the individual contributors differs for 1960s to now (mostly decrease in dam construction and steric) versus 1993 to now (mostly related to the ice sheets). [Trigg Talley, United States of America]	[noted] text moved and rewritten
72029	85	31			Should add Chen et al here. [John Church, Australia]	[noted] thanks for the suggestion
62199	85	32	85	34	“forced natural variability”...I think this is confusing as often internal variability is used to describe the unforced climate responses, whereas volcano etc is summarized under “natural climate forcings” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] text moved and rewritten
109887	85	33	85	34	Eruption caused acceleration in SLR? This is counter-intuitive. [Donald Forbes, Canada]	[noted] text moved and rewritten
100811	85	34	85	34	ENSO/PDO mentioned here. Also PDV is mentioned in box 9.1 but never before. Maybe PDO can be replaced with PDV everywhere in the chapter? There should be references to Annex VI.4 and VI.7. [Corti Susanna, Italy]	[taken into account] PDV is not mentioned in box 9.1. PDO is the more common name which is used elsewhere in this chapter.
77869	85	35	85	35	Tripled plus or minus what? The uncertainty in the short period is large. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] text removed
27357	85	35	85	35	It seems that it is “1993-2004” and not "1993-2014" [Eric Brun, France]	[noted] text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78369	85	35	85	38	Delete. It does not add anything as it is premature and due to decala variability. [Michael Tsimplis, China]	[noted] text removed
129529	85	35			The assertion that land water storage played the central role in GMSL acceleration between 1993-2014 and 2005-2015 cannot be supported. Rather the rebound arose from volcanic influences, per Fasullo et al. (2016, Scientific Reports). [Trigg Talley, United States of America]	[noted] text removed
76727	85	37	85	38	“It would be premature to project”. I agree but this statement can be easily taken out of proportions and context. Suggestion: “ The current uncertainties in observational acceleration estimates currently hinder a reliable extrapolation based on observations alone.” [Roelof Rietbroek, Germany]	[noted] text removed
86437	85	41	85	41	Some discussion on regional sea level rise in the Indian Ocean can be included as Indian Ocean is warming at a higher rate and Indian Ocean sea level rise is comaprable with the global estimates (Unnikrishnan et al.,2006; Unnikrishnan and Shankar 2007). IO sea level rise is dominated by thermosteric contribution (Thomopson et al., 2016; Sreenivasu et al.,2016; Swapna et al., 2017). [Swapna Panickal, India]	[noted] thanks for the suggestion
103869	85	41	86	19	A relevant paper on the Arctic sea level record: DOI: https://doi.org/10.3390/rs11141672 [Philippe Tulkens, Belgium]	[noted] thanks for the suggestion
80889	85	41	86	19	A relevant paper on the Arctic sea level record: DOI: https://doi.org/10.3390/rs11141672 [Louise Sandberg Sørensen, Denmark]	[noted] thanks for the suggestion
115489	85	41	87	13	This section on regional sea level looks great overall, but I was surprised not to see anything from Hamlington et al., 2020; Reviews of Geophysics. [Robert DeConto, United States of America]	[noted] thanks for the suggestion
129531	85	43	86	2	This whole section is questionably referenced and confusing. There is a mixture of timescales here that prohibits understanding what this section is trying to communicate. Dangendorf et al. (2019) deals with a 20th century sea level reconstruction, yet is being referenced to support regional trends during the altimeter era. Why not use a reference that uses actual altimeter rates? It's also possible to show the latest altimeter trend map, which would help in many sections of the chapter. Additionally, these regions appear arbitrary and rates during the altimeter time period can differ dramatically within these regions. [Trigg Talley, United States of America]	[noted] thanks for the suggestion, we have included more altimetry information
78371	85	43	86	18	My understanding is that there is a lot of regional work published albeit following different approaches than that of reconstructions which perhaps are appropriate only for the global scale. [Michael Tsimplis, China]	[noted] we have included approaches other than reconstructions
22651	85	46	85	47	Suggest to reference the technical annex of modes of variability here so that the interested reader can cross-reference if they want to know more about these modes. [Peter Thorne, Ireland]	[noted] thanks for the suggestion, added
77871	85	47	85	47	It would be better to cite original papers on these subjects as well as Oppenheimer (SROCC). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] that is not the policy followed throughout the chapter. SROCC is taken as the starting point of our assessment.
132595	85	48	85	54	Another place you could link the pattern of sea level rise to the pattern of ocean heat content trends (thermosteric sea level rise). The patterns look very similar over recent decades. [Kyle Armour, United States of America]	[noted] thanks for the suggestion, added
3311	85	49	85	52	These numbers seem to be inconsistent with a global sea-level rise of 3.2 mm/yr, which is larger than the sea-level trend in all but one basin. A consistent set of numbers can be computed relatively straightforward from a GIA- and GRD-corrected altimetry product. [Thomas Frederikse, United States of America]	[noted] thanks for the suggestion, numbers have been updated where appropriate
129533	85	49	85	52	These numbers seem to be inconsistent with a global sea-level rise of 3.2 mm/yr, which is larger than the sea-level trend in all but one basin. A consistent set of numbers can be computed relatively straightforward from a GIA- and GRD-corrected altimetry product. [Trigg Talley, United States of America]	[noted] thanks for the suggestion, numbers have been updated where appropriate
129535	85	53			This section misses some basic mechanisms. The regional patterns are not solely wind-driven noise. For one the spatial structure of sea water expansivity is at play and is part of the reason for the limited rate of rise in the eastern Pacific Ocean. In addition there is strong structure to OHC uptake and this drives patterns of sea level change. See 9.2.2.1 for example. Aspects of this are examined in the CESM large ensemble in Fasullo et al. (2020, JGRO) and Fasullo et al. (2020 JCLIM), both in revision. A key finding is that the forced response is not a single pattern but rather is a time-evolving pattern. This doesn't seem to be adequately acknowledged in this section. [Trigg Talley, United States of America]	[noted] section rewritten
97025	85	54	85	54	Please explain GRD again, it has been introduced on page 11. [Nicole Wilke, Germany]	[noted] a cross-reference to box 9.1 has been added. The whole point of the box is to avoid explaining the mechanisms every time they occur in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109889	85	54	85	55	Re. GRD effects in Arctic Ocean, it is not appropriate to treat the entire Arctic Ocean as one, because GRD effects diminish away from Greenland and Eastern Arctic Canada. [Donald Forbes, Canada]	[noted] text revised
77873	85	55	85	55	Reference for the GRD effect? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] text revised
116889	85		85		Please compare the rate of sea level rise in the 1920s 1930s with today, and explain the reason for a larger rate of increase at that time, if possible (cause?). [Valerie Masson-Delmotte, France]	[noted] this is done in chapter 2.
129537	86	1			The acceleration and associated uncertainty attributed to Dangendorf et al. (2019) is not consistent across the chapter. [Trigg Talley, United States of America]	[noted] text revised
62201	86	4	86	4	There is limited literature on regional sea-level budget closure. Some A limited number of... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable. Text revised] The regional budget is removed from the section.
39197	86	4	86	18	What is meant by sea level closure? [Lourdes Tibig, Philippines]	[noted] it is sea level budget closure, the term occurs earlier in 9.6.1.1, where is clear that this is the comparison of the total observed sea level change to the sum of the terms contributing to sea level change
129539	86	6	86	6	The current reference points at the wrong paper. The correct citation for the regional budget study of Frederikse et al. (2016) is: Frederikse, T., Riva, R., Kleinherenbrink, M., Wada, Y., van den Broeke, M., & Marzeion, B. (2016). Closing the sea level budget on a regional scale: Trends and variability on the Northwestern European continental shelf. Geophysical Research Letters, 43(20), 10,864-10,872. https://doi.org/10.1002/2016GL070750 [Trigg Talley, United States of America]	[noted] changed
3313	86	6	86	6	The correct citation of the regional budget study of Frederikse et al. 2016 is: Frederikse, T., Riva, R., Kleinherenbrink, M., Wada, Y., van den Broeke, M., & Marzeion, B. (2016). Closing the sea level budget on a regional scale: Trends and variability on the Northwestern European continental shelf. Geophysical Research Letters, 43(20), 10,864-10,872. https://doi.org/10.1002/2016GL070750 The current reference points at a different paper. [Thomas Frederikse, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
129541	86	6	86	7	Two interesting studies explain the observed trends and variability of sea-level changes along the U.S. east coast (closure of the regional sea-level budget): Frederikse et al., 2017: The sea-level budget along the Northwest Atlantic coast: GIA, mass changes, and large-scale ocean dynamics Piecuch et al. Origin of spatial variation in US East Coast sea-level trends during 1900-2017 [Trigg Talley, United States of America]	[not applicable. Text revised] The regional budget is removed from the section.
3315	86	6	86	7	Two other interesting studies about the closure of the regional sea-level budget are Frederikse et al. 2017: The sea-level budget along the Northwest Atlantic coast: GIA, mass changes, and large-scale ocean dynamics and Piecuch et al. Origin of spatial variation in US East Coast sea-level trends during 1900–2017 Both studies explain the observed trends and variability of sea-evel changes along the US east coast. [Thomas Frederikse, United States of America]	[accepted] Text revised
62173	86	7	86	9	Consider clarifying the difference in basin vs regional scale - e.g. "...but that the observational sea-level budget can be close for some regions on the smaller, basin-scale analysis". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] text revised
2527	86	7	86	9	Frederikse et al. (2016) GRL also closed the budget for the mean North Sea, that should be added [Tim Hermans, Netherlands]	[noted] thanks for the suggestion
129543	86	11	86	12	The statement that the non-closure of the regional budget was attributed to interannual variability cannot be supported. Rather, there appear to be systematic biases in the observing systems that are not yet understood. This included corrections like that for GIA, as well as contemporary GRD. [Trigg Talley, United States of America]	[noted] text revised
77875	86	11	86	12	By "mass" term here I think you mean "manometric" in the terminology of Gregory et al. (2019) - not GRD, for instance. By "regional sea-level" do you mean relative or geocentric? I'm not sure what the statement means. Both steric and manometric terms have interannual variability, and there is nothing else which could contribute to RSLC. Geocentric SLC is also affected by VLM. There could also be IB - is that included? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] text revised
129545	86	12	86	12	This explanation of the non-closure seems inconsistent. In Rietbroek et al. (2016), the interannual variability should be observed in all systems, so it's not a prime candidate for the non-closure, which would require that not all processes observe the variability. Limits in the spatial resolution of GRACE plays a large role in the non-closure, as well as uncertainties in GIA, GRACE and altimetry processing, and the inversion scheme used in R2016. [Trigg Talley, United States of America]	[noted] text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3317	86	12	86	12	This explanationof the non-closure seems inconsistent: In Rietbroek et al. 2016, the interannual variability should be observed in all systems, so it's not a prime candidate for the non-closure, which would require that not all processes observe the variability. I guess that limits in the spatial resolution of GRACE plays a large role in the non-closure, as well as uncertainties in GIA, GRACE and altimetry processing, and the inversion scheme used in R2016. [Thomas Frederikse, United States of America]	[noted] text revised
29949	86	12	86	12	This is exactly the point by Chafik et al. (2019). Suggest adding a reference after 'interannual variability'. The suggested new text is "...was attributed to interannual variability (Chafik et al., 2019)". Reference: https://doi.org/10.1038/s41598-018-37603-6 [Léon Chafik, Sweden]	[noted] text revised
77877	86	13	86	13	Spatial or temporal variability? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] text revised
62175	86	13	86	15	Does "local variability" refer to basin-scale or regional changes? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] local refers to small-scale changes, such as point measurements
129547	86	14	86	14	To stay consistent with the Gregory et al. (2019) notations, suggest to use steric only for global-mean steric changes, and 'dynamic' or 'sterodynamic' for regional and local changes. With that definition, bottom pressure signals due to ocean dynamics, which dominate variations in shallow water, are also included. [Trigg Talley, United States of America]	[noted] changed
3319	86	14	86	14	To stay consistent with the Gregory et al. (2019) notations, I'd suggest to use steric only for global-mean steric changes, and 'dynamic' or 'sterodynamic' for regional and local changes. With that definition, bottom pressure signals due to ocean dynamics, which dominate variations in shallow water, are also included. [Thomas Frederikse, United States of America]	[noted] changed
109891	86	14	86	15	VLM influencing regional closure – This is particularly the case for oceanic islands without GNSS stations, for many of which VLM is poorly known and not reliably exrapolated from nearby islands. Forbes, D.L., James, T.S., Sutherland, M. and Nichols, S.E. 2013. Physical basis of coastal adaptation on tropical small islands. Sustainability Science, 8, 327-344, doi:10.1007/s11625-013-0218-4. [Donald Forbes, Canada]	[noted] reference added
74025	86	15	86	15	For the lacking sea level data in the South Eastern Mediterranean, I can provide the following: A 22 years sea level monitoring at the GLOSS Station 80 Hadera off the Israeli coast in SE Mediterranean is offered, indicated higher sea level rise estimated due to Eastern Mediterranean Transient and to the increased flow after 1990's via the Suez Canal due to its deepening and widening reaching now an estimated yearly discharge in the Mediterranean of between 100 to 120 cubic kilometers and warm and saltier water density due to the passage trough the Bitter Lakes region of the Canal. See: Rosen, S.D., Raskin L., Lerner A., Ozer T., Gertman I., 2014. Investigation and monitoring of the Israeli shelf as a basis for decision making during development of national infrastructures - artificial islands as an example. Maintenance and operation of measuring systems of meteo-marine and environmental data at the tips of the Hadera and Ashkelon coal terminals and processing of the gathered data in the period April 2012-March 2014, Final Report, June 2014, IOLR report H28/2014, Haifa. (via IOLR for Ministry of National Infrastructures, Energy and Water Resources, in Hebrew with English titles) http://seashorerosen.com/wp-content/uploads/2016/08/IOLR-Report-H28-2014-meteomarine-and-biochemical-measurements-at-Hadera-and-AshkelonRosen-et-al.pdf [Sergiu Dov ROSEN, Israel]	[noted] thanks for this suggestion, however we are not allowed to base our assessment on non peer-reviewed literature.
77879	86	15	86	15	Spatial or temporal variability? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] spatial
88721	86	15	86	16	Suggestion of literature: (1) Kulp, S.A., Strauss, B.H. New elevation data triple estimates of global vulnerability to sea-level rise and coastal flooding. Nat Commun 10, 4844 (2019). https://doi.org/10.1038/s41467-019-12808-z (2) Leuven, J.R.F.W., Pierik, H.J., Vegt, M.v.d. et al. Sea-level-rise-induced threats depend on the size of tide-influenced estuaries worldwide. Nat. Clim. Chang. 9, 986–992 (2019). https://doi.org/10.1038/s41558-019-0608-4 (3) Vousdoukas, M.I., Ranasinghe, R., Mentaschi, L. et al. Sandy coastlines under threat of erosion. Nat. Clim. Chang. 10, 260–263 (2020). https://doi.org/10.1038/s41558-020-0697-0 [Rosemary Vieira, Brazil]	[noted] thanks for these suggestions. However, these all deal with impacts and consequences, for which we refer to chapter 12 and the WG2 report
77881	86	16	86	16	Which other literature shows? Or do you mean, "We conclude"? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] we meant 'we conclude' - changed
76729	86	17	86	17	Add statement on the particular difficulties in coastal shelf, which are strongly affected by strongly wind driven variability (e.g. Chafik et al 2019) [Roelof Rietbroek, Germany]	[noted] the contribution of winds to regional departures is made up front. This reference is given at that point.
109893	86	21	86	21	Meaning of 'emergence' in this section heading is ambiguous. [Donald Forbes, Canada]	[noted] changed to 'time of emergence'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
117289	86	21	87	13	The discussion of emergence in section 9.6.1.5 is a bit confusing and not consistent with Ch1. In chapter 1 emergence is used for "observed emergence" and here only for future projetions. Whereas for the observed regional SL changes the text talks about detection . Defenition and usage of these concepts need to be hamonised throughout the report. [Maisa Rojas, Chile]	[noted] In sea-level change literature, emergence can be used either for observed or projected changes.
129549	86	23	86	29	[CONFIDENCE] If the individual regional basins have large uncertainty in MSL change determination due to the signal to noise problem, then how can the change in GMSL be designated as "very likely"? Is this apparent conflict a result of different technical approaches used? Or methodological flaws in the confidence designation? [Trigg Talley, United States of America]	[noted] In GMSL, the signal-to noise is larger, due to averaging , so the GMSL is less impacted by variability and can therefore be assessed with higher confidence levels.
78373	86	23	86	29	If attribution is only confirmed after 1970 then this is THE result for the report. The data do not permit a conclusion for anthopogenic causation before that year. So sea level has been going up but it cannot be confirmed with the statistical methods used (new and old) that this is athropogenic. This is really a very bad conclusion and the euphimism that follows that regionaly it is more challenging to do the same is simply making it worse. I would rather accept that the methodologies used are inappropriate or search for a specific statistical relationship than accept that we can only attribute after 1970. But if the team believes the attribution methodology and is happy to abandon all the earlier arguments it made for acceleration, changes in acceleration then so be it, But then the conclusions in the beginning must change to reflect this. I would actually suggest that this and the following section are reconsidered. Wht exactly do they say for the anthropgenic contributions, [Michael Tsimplis, China]	[rejected] the reviewer is confusing a range of issues: 1. Detecting acceleration does not equal detecting anthropogenic influence. 2. GMSL Attribution after 1970 was already assessed in the SROCC, so this is not new in the AR6.
77883	86	25	86	25	“Dominant” is not precise. Do you mean "more than half"? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] the same phrasing was used in SROCC. Nonetheless, percentages have been added.
129551	86	26			Regional changes, their emergence, and drivers are examined in the CESM large ensemble in Fasullo et al. (2020, JGRO) and Fasullo et al. (2020, JCLIM), both in revision. The attribution of regional change as it relates to individual forcing agents is explored in Fasullo et al. (2020, JCLIM). Aerosol forcing (and its decrease over time) results in a pattern that is quite distinct from GHG forcing. Estimates of these patterns from CESM provide a framework for understanding future changes under a range of scenarios -- for example, where aerosols may or may not decrease or where GHG are the overwhelmingly dominant forcing. [Trigg Talley, United States of America]	[noted] thanks for the suggestions, text has been revised
62203	86	27	86	27	external forcing is more challenging than less robust for GMSL [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[editorial]
20997	86	31	86	31	Delete for all ocean basins and including all sea-level [Ladislaus Chang'a, United Republic of Tanzania]	[noted] text revised
77885	86	31	86	32	The first sentence seems unnecessary, as it repeats the last one of the previous paragraph, or could be appended to that. In the second sentence, I would say that "detection-only" is jargon. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] text revised
79083	86	31	86	52	A relevant new publication to check out in this context might be Richter et al (Accepted, ERL): "Detecting a forced signal in satellite-era sea-level change" [Aimee Slangen, Netherlands]	[noted] thanks for the suggestion
72031	86	31		52	Bilboa et al. should be cited here - this is the ost comprehensive regional sea level D&A work to date. [John Church, Australia]	[noted] thanks for the suggestion, this work was already cited.
20999	86	32	86	32	Delete not available Replace with limited [Ladislaus Chang'a, United Republic of Tanzania]	[noted] sentence has been removed
21001	86	33	86	33	Not clear what is meant by Individual sea level processes. This need clarification. [Ladislaus Chang'a, United Republic of Tanzania]	[noted] sentence rewritten
22653	86	33	86	35	How much does this relate to the issues around anomalous atmospheric behaviour in the tropics that is discussed in some detail in chapters 4 and 7? Is some effort to cross-link and integrate useful here? Suggest discuss with chapters 4 and 7. [Peter Thorne, Ireland]	[noted] we are not aware of literature discussing this in the context of sea level change
77887	86	34	86	34	The observed E-W dipole was large and long-lived - not difficult to detect. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] sentence rewritten
5557	86	35			add the bibliography references : Becker M., Meyssignac B., Llovel W., Cazenave A. and Delcroix T., (2012), Sea level variations at Tropical Pacific Islands since 1950, Global and Planetary Change, Volume 80-81, p 85-98. DOI: 10.1016/j.gloplacha.2011.09.004 Becker M., Karpytchev M., Marcos M., and Lennartz-Sassinek S. (2016), Do climate models reproduce complexity of observed sea level changes ?, Geophysical Research Letters, 43(10), 5176–5184. [Benoit Laignel, France]	[noted] thanks for the suggestions
77889	86	36	86	37	Over what period? What if the PDO was anthropogenic? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] over 1993-2010, added to text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62205	86	36	86	37	sentence is a little weak. Maybe restructure and say that the author linked or proposed this idea rather than using “could be linked” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] changed
21003	86	37	86	37	After Tropical Indian Ocean.Add via The Indonesian Throughflow associated positive ENSO events that origin lies in the Tropical Pacific Ocean (Diaz & Markgraf 2000) [Ladislaus Chang'a, United Republic of Tanzania]	[rejected] to much detail, this can be found in the original publication.
77891	86	38	86	38	I guess this sentence is worldwide, leaving the tropical Pacific. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] added 'worldwide'
97027	86	38	86	39	The sentence remains unclear, please modify. [Nicole Wilke, Germany]	[noted] changed
20573	86	38	86	39	The problem apparently solved by Becker et al is a very general one, and a major one in climate change research, where scientists spend a sizable fraction of their time trying to extract anthropogenic signals from natural variability. Yet, neither this paper nor its references are mentioned anywhere else in this SOD. [philippe waldteufel, France]	[noted] the paper is referenced here, as it deals with Detection & Attribution in the context of sea-level change.
77893	86	39	86	39	How was internal variability evaluated? Over what period? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] we refer to the cited study for details, sentence has been rephrased to clarify that this is the range of internal variability as determined by the authors of the study.
77895	86	42	86	44	Over what period? It doesn't sound right that SL change in recent decades in the tropical Pacific is entirely anthropogenic, since historical simulations with AOGCMs don't simulate anything like what was observed. That would be inconsistent with 36-37, I think. In the Southern Ocean this also seems surprising because it's not certain that westerly wind changes are entirely anthropogenic. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] 1970-2005. clarified that this is about thermosteric change only, not total SL change
33495	86	42			Change: “(Marcos and Amores, 2014) find…” by “Marcos and Amores (2014) find…”. [Guiomar Rotllant, Spain]	[editorial] change made
62207	86	44	86	47	sentenced could be condensed a little by introducing common detection and attribution terminology maybe using “fingerprints” “response pattern” “detected”. The latter already implies that a signal can be isolated (in some statistical sense) from natural variability so it does not have to be repeated explicitly. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] these terms were added in response to earlier comments for clarification - we therefore keep them here.
129553	86	44	86	52	This section appears to be inconsistent with page 40, lines 11-12. [Trigg Talley, United States of America]	[noted] checked
77897	86	45	86	45	Bilbao et al. (2015) disagree with this. I think the contradiction should be addressed. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] this paper is now discussed in section 9.6.1.4
77899	86	49	86	52	That doesn't seem to be a summary of this paragraph; it's a restatement of the first sentence. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] the first sentence has been changed
22655	86	54	86	55	How does this square with the statement in the preceding paragraph around the Pacific East-West dipole? On the face of it these are contradictory statements? [Peter Thorne, Ireland]	[noted] sentence removed
77901	86	54	87	3	I don't understand how it can be done at all without models. How do you obtain an estimate of unforced variability? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] sentence removed
129555	86	54	87	13	The way this section is ordered is confusing. Very specific assessments for certain regions based on one study are first given (Llovel et al., 2018), before a more general discussion of four other studies looking at model projections. It is said the results are similar, but this is not clear from what follows. [Trigg Talley, United States of America]	[noted] the section has been rewritten
109895	86	54	87	13	Because of its meaning with respect to areas uplft and faling RSL, use of the term 'emergence' in this context is unfortunate. I recommend finding an alternative term for the appearance and detectability of a regional signal. [Donald Forbes, Canada]	[rejected] 'emergence' is the term commonly used to express that a signal is emerging from background/internal climate variability.
52521	86	55	86	55	Does it make sense to discuss time to observe a signal without reference to the size of the signal? Shouldn't it eg be time to observe difference of x mm/yr ? [Joanne Williams, United Kingdom (of Great Britain and Northern Ireland)]	[noted] No, the time of emergence at each location depends on the internal variability at each location. The size of the signal can therefore be quite different: in some places a relatively small forced signal may emerge if the internal variability is small too, but if the internal variability is large, the same size signal would not emerge.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27359	87	1	87	2	This statement should be revised : in Llovel et al. 2018, the time of emergence corresponds to the time needed for the atmospherically forced trend to emerge from the intrinsic, chaotic ocean variability (that is not atmospherically forced and generated spontaneously by the ocean). The atmospheric forced signal in their study includes both the anthropogenic forcing and natural climate variability. Their time of emergence is thus not related to the emergence of an anthropogenically-only forced SL signal, contrary to e.g. Lyu et al. 2014, Bilbao et al. 2015 ect. [Eric Brun, France]	[noted] thanks for the correction, sentence removed to focus on anthropogenic forcing
51979	87	2	87	3	"Longer records are needed to isolate the forced part of sea level trends in regions where the internal variability is large." - There is some evidence for this in Hughes and Williams (2010; doi:10.1029/2010JC006102), their Fig. 7b, which suggests the need to observe for ~ 60-100 yr in such regions for the trend error to reduce to 1 mm/yr. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	[noted] thanks for the suggestion, text rewritten
129557	87	13			Where does the 50% come from? [Trigg Talley, United States of America]	[noted] this is from the publication of Lyu et al (2014). Text rewritten
22659	87	18	87	18	The title of the box is a bit esoteric and misses the important aspects around joint clusure of these budgets. [Peter Thorne, Ireland]	NOTED. The title of the box is in line with the joint box in ch7 and terminology used throughout AR6.
12503	87	18	87	55	error bar to be added. [Lijing Cheng, China]	ACCEPTED. We have added error bars to the total GMSL and total heat uptake in panels a) and b).
72033	87	18	88	18	Should relso refer to earlier papers who did similar budget studies, including similar figures. And on the figure should show the range of observational estimates (in situ up to 1993 and then satellite). [John Church, Australia]	TAKEN INTO ACCOUNT. We have included references to SROCC and AR5. Post-SROCC sea level budget papers are referred to in sections 9.6.1.1/2. Ranges of observational estimates are in chapter 2 - cross references have been added.
22657	87	18			The findings of this box are important and relevant to policymakers. I have a slight concern that this is not being sufficiently elevated via the ES to the TS and SPM. [Peter Thorne, Ireland]	TAKEN INTO ACCOUNT. We have revised and strengthened the text in the ES and promoted the inclusion of CCBox9.2 findings in the TS/SPM through Ch9 involvement in the drafting process.
80663	87	21	87	21	there is a comma missing between 'Germany)' and 'Aimée' [Helene Jacot Des Combes, Marshall Islands]	ACCEPTED. Changed.
88391	87	22	87	25	Misleading statement - Note Slater ad Lawrence only consider upper 3 m and permafrost may be several 100 m thick so permafrost may still be present in areas at depth where they conclude it has disappeared in the upper 3 m - not completely lost in these areas. [Sharon Smith, Canada]	Noted. The SOD text this comment refers to is probably on Page 77, line 38-41. The original and revised text specifies clearly this is about near-surface permafrost down to 3 m depth, and it is relevant here because the paragraph compares equilibrium permafrost extents, which are more relevant from a climate perspective than fossil permafrost at great depth that does not interact with the atmosphere.
12507	87	30	87	31	"best estimate" is not justified by any assessments in this chapter or in chapter-2. There should be a clear justification/reasoning for "best". [Lijing Cheng, China]	TAKEN INTO ACCOUNT. We have reworded the sentence, which now states that this is the assessed estimate from section 7.2.
12509	87	30	87	31	What about the GCOS assessment on earth energy budget? von Schuckmann, K., Cheng, L., Palmer, M. D., Tassone, C., Aich, V., Adusumilli, S., Beltrami, H., Boyer, T., Cuesta-Valero, F. J., Desbruyères, D., Domingues, C., García-García, A., Gentine, P., Gilson, J., Gorfer, M., Haimberger, L., Ishii, M., Johnson, G. C., Killik, R., King, B. A., Kirchengast, G., Kolodziejczyk, N., Lyman, J., Marzeion, B., Mayer, M., Monier, M., Monselesan, D. P., Purkey, S., Roemmich, D., Schweiger, A., Seneviratne, S. I., Shepherd, A., Slater, D. A., Steiner, A. K., Straneo, F., Timmermans, M.-L., and Wijffels, S. E.: Heat stored in the Earth system: Where does the energy go? The GCOS Earth heat inventory team, Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2019-255, in review, 2020 [Lijing Cheng, China]	TAKEN INTO ACCOUNT. The von Schuckmann et al (2020) paper is cited in Ch7 and linked back to from CCBox9.2.
2477	87	33	87	33	I see the estimate of heat accumulation in the oceans is now 92% of the total. That is down only 1% from the AR5 estimate. Might I suggest that at Page 9-20 line 28 where we see "There is high confidence that this ocean heat content increase represents over 90% of the total excess heat.." you be more specific, such as "There is high confidence that the ocean heat content increase represents 92% of the total excess heat..." At either place 9-20 or 9-87 it might be worth mentioning this estimate is very close to or down 1% from the AR5 estimate. Many find this number to be surprisingly high, I know it is about right, we need to be specific throughout, and I elieve mention the consistency. [Howard J. Freeland, Canada]	TAKEN INTO ACCOUNT. The reviewer's comment refers to Line 28 of Page 20. We have updated the quoted % according to revised assessment numbers in the FGD. We agree that consistency across chapters is key.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85315	87	35	87	35	Does the 5% of global heating that goes into land mostly go into melting snow cover and permafrost? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	NOTED. There isn't really space to comment on this in CCBox9.2, but the answer is "no" - simply because only a relatively small fraction of the Earth's land surface is covered by snow/permafrost. A cross-reference to chapter 7 has been added.
6773	87	35	87	35	"and" is needed before "heating at the beginning of this line, and the comma at the end of the previous line could be deleted. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	ACCEPTED. Text revised.
6775	87	36	87	39	There are indeed arguments for preferring ocean heat content to surface air temperature for tracking climate change, but is it known as accurately from the present back to the 19th century as the surface temperature? Surface temperature is the variable for which the targets of the Paris Agreement apply, and one of the basic variables for which climate change information is communicated to the public. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. We agree with the reviewer. However, this box is about our ability to track changes in total Earth system heating and show the linkages to the sea level budget. Since this point is also made in Ch7 and not central to CCBox9.2, this sentence has been removed.
12505	87	37	87	38	Cheng et al. 2018 make this point out by comparing the signal to noise in OHC, sea level and GMST changes. . Cheng L., K. E. Trenberth, J. T. Fasullo, J. Abraham, T. P. Boyer, K. von Schuckmann, and J. Zhu 2018: Taking the pulse of the planet, Earth and Space Science News, Eos, 99, 14-16. https://doi.org/10.1029/2017EO081839 [Lijing Cheng, China]	NOTED.
97029	87	45	87	45	Probably table 9.4? [Nicole Wilke, Germany]	ACCEPTED. We have corrected the reference to the corresponding table, which is now Table 9.5 in the FGD.
62177	87	45	87	45	Table 9.5 provides projections, Table 9.4 provides the summary of observed contributions [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	ACCEPTED. We have corrected the reference to the corresponding table, which is now Table 9.5 in the FGD.
129559	87	46	87	48	Is it possible to add confidence intervals to these percentages? The way it's written now suggests that we are really sure about this partition. [Trigg Talley, United States of America]	TAKEN INTO ACCOUNT. For clarity of presentation and consistency with previous IPCC assesment reports (e.g. AR5 and SROCC) we retain single % values. The corresponding deltas and uncertainties are available Table 7.1 and Table 9.5 for those readers who are interested in the addtional detail.
77903	87	46	87	48	These numbers should have uncertainties. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. For clarity of presentation and consistency with previous IPCC assesment reports (e.g. AR5 and SROCC) we retain single % values. The corresponding deltas and uncertainties are available Table 7.1 and Table 9.5 for those readers who are interested in the addtional detail.
3321	87	46	87	48	Is it possible to add confidence intervals to these percentages? The way it's written now suggests that we are really sure about this partition. [Thomas Frederikse, United States of America]	TAKEN INTO ACCOUNT. For clarity of presentation and consistency with previous IPCC assesment reports (e.g. AR5 and SROCC) we retain single % values. The corresponding deltas and uncertainties are available Table 7.1 and Table 9.5 for those readers who are interested in the addtional detail.
41159	87	47			what do you call peripheral glaciers? [TSU WGI, France]	NOTED. Peripheral glaciers no longer mentioned
33497	87	51			Add one parenthesis at the en on this sentence fragment: "…change ((Dangendorf et al., submitted), Cross-Chapter Box 9.2, Figure 1c)…". [Guiomar Rotllant, Spain]	NOTED. Sentence has been reworded and Dangendorf is no longer cited.
116893	87		87		Please link CCB 9.2 with chapter 1 and develop a clear summary statement, this aspect is important for the TS and SPM. [Valerie Masson-Delmotte, France]	ACCEPTED. Related summary statements appear in Ch9 and Ch7 ES, Box TS.4 in the TS and HS4 in the SPM.
40055	88	8			Cross chapter box 9.2, Fig 1:is land water in (b) the same as TWS? [TSU WGI, France]	TAKEN INTO ACCOUNT. Yes. Land water storage is now used across Ch9.
117287	88	21	88	21	It would be more in line with the rest of the chapters to call this section as "paleo contezt..." rather than "Geological context …" [Maisa Rojas, Chile]	Accepted. Section is now titled "Paleo context…"
77905	88	21	88	21	Sect 9.6.2. I suggest that this section should also deal with the last couple of millennia, for which there are quite a few papers, not directly addressed elsewhere. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We've moved discussion of regional RSL acceleration here from 9.6.1.3; there is not much literature to assess on the drivers of last millennia GMSL change, and GMSL as a metric is assessed in 2.3.3.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115491	88	21	89	53	Palaeo feels like it appears out of the blue, so many pages after the box on page 56. For Pliocene, what about the new Grant et al., 2019 (Nature) and Dumitru et al., 2019 (Nature), offering indepenent estimates that suggest, at times, most marine based Antarctic ice was lost? [Robert DeConto, United States of America]	Taken into account. References added.
40771	88	21			section 9.6.2: would be good to have an overall statement/summary for the section [TSU WGI, France]	Accepted. We have added an overview paragraph for the section.
40773	88	21			section 9.6.2: progress since AR5/SROCC not really apparent [TSU WGI, France]	Taken into account. Added statement tying to AR5 or SROCC in each subsubsection.
22661	88	21			This section concentrates upon the MPWP, LIG , deglaciation and the HTM. Should it also consider other periods such as stage 11 when sea level may have been considerably higher than today? Particular coordination is required with chapter 1 who are pushing (perhaps unwisely) a continuous reconstruction series going back over almost 2Ma. [Peter Thorne, Ireland]	Accepted. Added discussion of MIS 11.
110543	88	23	88	24	I think this sentence is unclear and should be reworded. Under future forcing, the climate is expected to react differently than under past periods of forcing. That includes, among many features, different temperature change patterns (and how fast changes can be expected to occur). [Keven Roy, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. These caveats in the overview have been restructured.
78375	88	23	89	54	This secion either needs to be linked with the previous section and explain how it helps or just be deleted/shortened. What is its message? It does not seem to be used in setting the errorbars for the future in the following section. [Michael Tsimplis, China]	Taken into account. The introductory paragraph provides an overview of the section and its motivation.
7883	88	23	99	36	In several places AMOC forcing is referred to. This is very vague. Do you mean ocean temperature changes associated with AMOC changes or something else? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. LIG AMOC forcing in 9.6.2 is now identified as ocean temperature forcing.
89275	88	30	88	44	Please update this section to reflect new MPWP sea level data from Dumitru et al., (2019) and Grant et al., (2019). [Edward Gasson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References added.
35303	88	31	88	31	I made a note on the MPWP sea level in cross-chapter box 2.4, please cross-check. [Alessio Rovere, Germany]	Noted. This comment is more appropriate for chapter 2, where GMSL is assessed.
83565	88	37	88	38	If you accept ice-rafted debris evidence, you could cite here in addition Bachem, P.E., Risebrobakken, B., De Schepper, S., McClymont, E.L., 2017. Highly variable Pliocene sea surface conditions in the Norwegian Sea. Clim. Past 13, 1153-1168, doi: 10.5194/cp-13-1153-2017. [Antje H. L. Voelker, Portugal]	Rejected. Paper does not offer a clear interpretation of IRD in terms of changes in ice extent, making it difficulty to identify relevance to section 9.6.2.
83567	88	37	88	38	There is also the study by Knutz, P.C., Newton, A.M.W., Hopper, J.R., Huuse, M., Gregersen, U., Sheldon, E., Dybkjær, K., 2019. Eleven phases of Greenland Ice Sheet shelf-edge advance over the past 2.7 million years. Nature Geoscience, doi: 10.1038/s41561-019-0340-8, although their timing is Pleistocene and not Pliocene. Their data could, however, be used to say that ice sheet advances to the shelf edge are evident after the MPWP. [Antje H. L. Voelker, Portugal]	Taken into account. Added reference to Knutz et al 2019 in the discussion of Pliocene Greenland.
110545	88	43	88	43	Minor comment, but I think the sentence should say that the 'main' contributions are expected to be from (but not exclusively) AIS and GrIS. [Keven Roy, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence no longer present.
39733	88	43			"contribution from GrIS" it is not clear from the evidence presented previously in the paragraph that GrIS has contributed so there is an apparent contradiction here [TSU WGI, France]	Taken into account. Clarified statement.
64393	88	44	88	44	the argumentation that quantification is not possible seems to be in contrast with line 31 where there is a quantification [roderik van de wal, Netherlands]	Taken into account. This has been rephrased.
35305	88	44	88	44	As I expressed while commenting Cross-chapter box 2.4, I think that one line of evidence -geological sea level proxies- has been overlooked. I hope that my comments on the box will be implemented or referenced also here to fill this gap. [Alessio Rovere, Germany]	Noted. This comment is more appropriate for chapter 2, where GMSL is assessed. The role of this section is to assess the processes underlying the GMSL assessment.
42919	88	46			"GMST was very likely 1-2°C above pre-Industrial" in LIG. I have challenged this assessment in chapter 2 and suggested 1+/-1 degree. I believe the assessment in chapter 2 is not sufficientlty rigorous , please adjust according to what is concluded there. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Aligned with revised Chapter 2.
35307	88	47	88	47	Chapter 2 (2.3.3.3) states that sea level was between 3 and 11 m with medium confidence. Please cross-check numbers! [Alessio Rovere, Germany]	Taken into account. Aligned with revised Chapter 2.
42921	88	47			I agree that 5–9 m is a good range, but Ch 2 (page 69, line 18) says 3-11 m, and refers to this very section of Ch 9. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Aligned with revised Chapter 2.
35309	88	49	88	50	cross-check with section 2.3.3.3, citing the same reference but a different uncertainty. [Alessio Rovere, Germany]	Taken into account. Aligned with revised Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42925	88	49			A newer estimate of the rise from global mean thermal expansion was deribed from the OHC in Shackleton et al 2020 for the LIG. 0.7+/-0.3 for the transient early LIG, and close to zero cf modern for the rest of the LIG. This replaces the McKay estimate. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now cite Shackleton et al. (2020).
33499	88	50	88	53	Change “As noted by SROCC (Oppenheimer et al., 2019), GrIS modelling studies yield peak GrIS contributions to GMSL between 0.3 m (5% estimates of (Stone et al., 2013)) and 6.2 m (97.5% estimate of (Yau et al., 2016)) (Helsen et al., 2013; Quiquet et al., 2013; Stone et al., 2013; Dutton et al., 2015a; Goelzer et al., 2016a; Yau et al., 2016).” By “As noted by SROCC (Oppenheimer et al., 2019), GrIS modelling studies yield peak GrIS contributions to GMSL between 0.3 m (5% estimates of Stone et al. (2013)) and 6.2 m (97.5% estimate of Yau et al. (2016), Helsen et al. (2013), Quiquet et al. (2013), Stone et al. (2013), Dutton et al. (2015a) and Goelzer et al. (2016a)).” [Guiomar Rotllant, Spain]	Not applicable. This section has been restructured.
99721	88	53	88	53	add Clark et al. (2020, Nature) [Peter Clark, United States of America]	Accepted. Added Clark et al., (2020).
64395	88	54	89	2	I think it is a bit a short cut to argue that the difference in the greenland estimates arise from the targets used for the different model studies. The differences in the models used are aslo very large. In addition it would be good to mention that there is dispute in the literature on the temperature reconstruction based on the ice cores. [roderik van de wal, Netherlands]	Noted. Due to space restrictions we have limited our assessment to just highlighting that the models agree well in terms of timing, but not in terms of magnitude of mass loss. A detailed analysis of paleo-thermometry is beyond the scope of this assessment.
116895	88		89		This section is important, and needs to be better integrated with other parts of the report dealing with MPWP (ccb in ch 2, dealing with the LIG - maybe a box in this chapter, and the last deglaciation (also covered in chapter 5) (and the ccb on polar amplification in chapter 7). Maybe having a summary table would be useful. We lack in the whole AR6 draft an assessment of proxy reconstructions of sea level change (including from oxygen isotopes shown in Chapter 1). Information here would benefit from being combined with the insights from past periods for ice sheets. Altogether, the assessment needs to be sh [Valerie Masson-Delmotte, France]	Taken into account. 9.6.2 has been restructured to integrated paleo-sea level and paleo-ice sheer assessment (though GMSL as a global metric is assessed in chapter 2).
116897	88		89		The chapter could provide contributions to FAQs related to insights from past climate placed in other chapters. [Valerie Masson-Delmotte, France]	accepted. This chapter has FAQ 9.1 and FAQ 1.3 shows past sea level
99723	89	1	89	1	see disucssion on this in Clark et al. (2020, Nature) [Peter Clark, United States of America]	Noted. Due to space restrictions we have limited our assessment to just highlighting that the models agree well in terms of timing, but not in terms of magnitude of mass loss. A detailed analysis of paleo-thermometry is beyond the scope of this assessment.
42927	89	1	89	12	Shouldn't you make clear that while the circumstatial evidence suggests sinificant loss of AIS, there is not yet any clear evidence whether this occurred or not. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now stated explicitly.
89809	89	5	89	5	See below, this citation is now published [Peter Croot, Ireland]	Accepted. Updated Clark et al. (2020) citation.
88723	89	6	89	6	Capron et al., 2017. Reference not found. [Rosemary Vieira, Brazil]	Accepted. Capron, E., Govin, A., Feng, R., Otto-Bliesner, B. L., & Wolff, E. W. (2017). Critical evaluation of climate syntheses to benchmark CMIP6/PMIP4 127 ka Last Interglacial simulations in the high-latitude regions. Quaternary Science Reviews, 168, 137-150.
35311	89	6	89	7	Multiple sea level peaks: a reference is needed here. I think Rohling et al 2019 states this? [Alessio Rovere, Germany]	Not applicable. Multiple peaks are now discussed in chapter 2, not here.
35313	89	7	89	8	I have not found the low confidence statement in Chapter 2. Please cross-check. [Alessio Rovere, Germany]	Taken into account. Aligned with revised Chapter 2.
139	89	8	89	12	Clarify from which ice sheet this potential 4m of sea-level rise contribution should have come. [Pepijn Bakker, Netherlands]	Taken into account. This has been restructured.
89811	89	9	89	9	See below, this citation is now published [Peter Croot, Ireland]	Accepted. Updated Clark et al. (2020) citation.
2491	89	9			With respect to the reduced deglacial AMOC it might be useful to include the Böhm et al. (2015) Pa/Th study. (Nature 517, 73pp) [Thomas Ronge, Germany]	Rejected. This is adequately addressed in Clark et al. 2020.
62209	89	14	89	14	This paragraph may benefit from including a statement of level of scientific agreement or level of confidence [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Given the nature of the statement, regarding the mismatch between budget estimates compiled from a variety of studies and GMSL constraints, it is not clear that there is an assessment that merits a confidence statement.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115177	89	14	89	14	This estimate of timing of LGM needs a reference, or to specify "(21-19ka in these models)" [Natalya Gomez, Canada]	Rejected. Assessed in chapter 2.
110547	89	14	89	15	I think the list of references is non-exhaustive. As Peltier et al. (2015) is listed as one of the only global GIA models providing full glacial-interglacial sea level evolution (Chapter 2, page 69), it should therefore be included here in a list of references for estimates of total GMSL change since LGM. [Keven Roy, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Assessment of LGM GMSL is in chapter 2.
27361	89	20	89	22	One or two sentence about the end of the LGM / onset of the deglaciation would help here. Indeed, we move directly from ca. 19 ka (lines 14-20 above) to 14.65 ka (from line 22 below). [Eric Brun, France]	Taken into account. Have clarified that the prior paragraph was the only update made to AR5 regarding size or timing of LGM and last glacial termination.
110969	89	22	89	32	Although I am not an expert in sea level changes, I wonder whether the classification of the Antarctic contribution to MWP-1A as "low confidence" is correct, given the studies of Weber et al. (2014), Liu et al. (2016) and Deschamps et al. (2012, Nature), the last of which is not cited in this section. [Ning Zhao, Germany]	Taken into account. Deschamps et al (2012) is among data sets analysed in Liu et al. (2016). Low confidence comes from the failure of prior studies to consider a significant Eurasian contribution.
115179	89	23	89	25	recent work suggests that the Eurasian ice sheets may have been significant contributors as well: Brendryen, J., Haflidason, H., Yokoyama, Y., Haaga, K. A., & Hannisdal, B. (2020). Eurasian Ice Sheet collapse was a major source of Meltwater Pulse 1A 14,600 years ago. Nature Geoscience, 13(5), 363-368. (as I see is referenced below) [Natalya Gomez, Canada]	Noted. Brendreyen et al. (2020) is cited, and the assessment here mentions the possibility of a Eurasian contribution.
7639	89	24	89	25	Include "Eurasian" [Daniel Lowry, New Zealand]	Rejected. No analysis has attempted to fingerprint a Eurasian source, as would be implied by stating this here.
39717	89	26			"3-6 m contribution" over how much time? (it's ambiguous given the rest of the sentence) [TSU WGI, France]	Accepted. Clarified this refers to duration of MWP-1A
62179	89	27	89	29	Include the estimated range for the Antarctic contribution from this study [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This study does not constrain magnitude.
33501	89	28	89	29	Change: "…the AIS modelling study of Golledge et al. (Golledge et al., 2014), while…" by "…the AIS modelling study of Golledge et al. (2014), while…". [Guiomar Rotllant, Spain]	Taken into account. Has been rephrased.
115181	89	29	89	29	could add this reference as a coming modeling study: Gomez, N., Weber, M., Clark, P.U., Mitrovica, J.X. and Han, H.K. Interhemispheric sea-level forcing of the Antarctic Ice Sheet during the last ice age. Nature (in review). And also marine records from the Ross Sea showing step-wise grounding line retreat (Bart et al., 2018) as further evidence of Antarctic retreat. Bart, P. J., DeCesare, M., Rosenheim, B. E., Majewski, W. & McGlannan, A. A centuries-long delay between a paleo-ice-shelf collapse and grounding-line retreat in the Whales Deep Basin, eastern Ross Sea, Antarctica. Scientific 344 Reports 8, 12392, doi:10.1038/s41598-018-29911-8 (2018). [Natalya Gomez, Canada]	Taken into account. We have added the Bart reference.
111397	89	29	89	32	Brendryen et al. (2020) is not a modeling study. It is a probabilistic reconstruction of the ice sheet retreat. [Jo Brendryen, Norway]	Accepted. Brendreyen et al. (2020) is now described as a statistical analysis.
99725	89	34	89	53	There is is quite a literature of Holocene changes in AIS - why just focus on GrIS? [Peter Clark, United States of America]	Accepted. New text on AIS Holocene changes now added.
1785	89	34	89	53	I believe this paragraph needs a major overhaul. It is said to focus on the early Holocene, the most recent warm period with rapid ice-mass loss and sea-level rise under relatively warm conditions. As such, it is important. However, the content focuses almost exclusively on the GrIS which is a relatively small player during this time interval. Also, it barely mentions studies that are primairly focused on early Holocene sea-level change (notably the review papers by Smith et al., 2011, QSR; Törnqvist & Hijma, 2012, NG; Carlson & Clark, 2012, RG). These sources highlight the fact that the partitioning of meltwater contributions between the Antarctic and North American ice sheets (and to a lesser extent the Eurasian Ice Sheet, prior to 10 ka) is extremely poorly constrained during this time period. This is problematic, given that nearly half of the total deglacial sea-level rise occurred during the early Holocene. Put differently, this provides another compelling example of important constraints from the paleo-record that to date have remained untapped. [Torbjorn Tornqvist, United States of America]	Accepted. We have expanded the text substantially to include Antarctica.
64407	89	44	89	44	Medium confidence seems questionable to me given the limited number of studies and little support from independent models [roderik van de wal, Netherlands]	Noted. We feel that it is reasonable to assign 'medium confidence' here, because otherwise we would be saying that "there is low confidence that the minima were reached at different times" which would imply they were synchronous, and that would be a far bolder claim, given the sparse evidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83571	89	45	89	52	I think I read BP (Before Present) here for the first time in the AR6. The meaning of BP might have to be explained (Annex II, Glossary?). My suggestion would be, however, to delete it here since you are not really discussing dates in decadal precision (text would be easier to read and understand for non paleoclimate experts). [Antje H. L. Voelker, Portugal]	Noted. Dates are now in Table 9.6
83569	89	52	89	52	"years" missing after 6700 [Antje H. L. Voelker, Portugal]	Taken into account. Has been rephrased.
100061	89	53	89	53	The change from RSL fall to rise in a number of records has been interpreted as evidence for GrIS regrowth in the mid-to-late Holocene. There is some ambiguity in this interpretation, however, due to the influence of Laurentide forebulge collapse which may dominate the RSL signal during this period as local uplift rates diminish. I recommned the authors look at Lecavlier et al. (2014) and references therein - notably Flemming and Lambeck (2004) and Simpson et al. (2009). [Glenn Milne, Canada]	Accepted. New references added.
62213	90	2	90	2	year missing for publication Palmer et al. (see next comments) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Added year.
20983	90	3	90	3	The Work of IPCC is to review Literature, therefore there is no value by writing " Global Mean sea level projections in Lieterature and their evaluation" We suggest to delete everything after projections. [Ladislaus Chang'a, United Republic of Tanzania]	Taken into account. This section has been restructured,
66165	90	3	91	36	Please consider updating this discussion so that new SLR studies such as Horton et al., 2020, "Estimating global mean sea-level rise and its uncertainties by 2100 and 2300 from an expert survey", npj Climate and Atmospheric Science are also included [Andra Garner, United States of America]	Accepted. Added reference.
85317	90	5	90	5	I am not a topic expert but given the huge issues with ice shelf modelling and potential for strong feedbacks are we confident that we can really robustly provide upper limits for sea-level changes, even from expert ellucidation. For example, might it be better to say we cannot provide such estimates with sufficient confidence or to qualify the estimates more to emphasize their deep uncertainties? For example, modelling the Antartic shelf near-coastal T-S characteristics to drive ice-shelf models is extremely challenging as there are considerable biases in the near coastal and offshore wind stresses, eddies, coastal and slope currents, larger scale Southern Ocean T and S characteristics, sea-ice characteristics, etc, all of which significantly adversely impact on Antarctic shelf T-S characteristics. In the Hadley Centre N216-1/4 HadGEM3 GC3.1 coupled climate model, large temperature biases (>2 deg C) near the west antarctic ice shelves currently make it impossible to even use our new active ice shelf model to make projections. I am not an expert but I suspect perhaps the same applies to Greenland ice-shelves, where models certaintly do not adequately capture T-S characteristics in narrow fjords and I suspect would not do adequtely even with downscaling modelling efforts, given the issues with biases in the larger scale boundary conditions from coupled models? Obviously as experts your judgement is far better informed than my thoughts. I was therefore not sure if to even raise this point but I thought perhaps it might be worth raising just in case it is felt to be worth qualifying these the upper limit with additional statements on uncertainties and confidences? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. High-end projections are discussed in Box 9.4.
5559	90	5	90	9	add the bibliography reference : Sweet, W.V., Kopp, R.E., Weaver, C.P., Obeysekera, T., Horton, R.M., Thieler, E.R., and Zervas, C. (2017). Global and Regional Sea Level Rise Scenarios for the United States. NOAA Tech. Rep. NOS CO-OPS 083. National Oceanic and Atmospheric Administration, National Ocean Service, Silver Spring, MD. 75pp. [Benoit Laignel, France]	Taken into account. Clarified that the studies here are based upon RCPs, which Sweet et al. 2017 is not.
83841	90	6	90	6	Palmer et al. citation has no year of publication given. [Mark Pickering, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication
88725	90	6	90	6	Palmer et al. (submitted ?) [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
52207	90	6			Citation "Palmer et al." is missing a year. [Gregory Garner, United States of America]	Editorial - copyedit to be completed prior to publication
64411	90	10	90	16	Here two methods are distinguished 1. physical models 2. probabilistic. It looks that without further notion method 1 and 2 are mixed in Table 9.6 to arrive at likely and very likely range. I consider this as misleading. It suggest to the readers that we now finally have a very likely range from models. This is however not the case. Our understanding from the physics is only marginally better than at the time of ar5 and SROCC which didn't provide a very likely range. SROCC clearly made this separation and offered people (users) interested in the tail an alternative by arguing that if you you really want you can use the expert elicitation. I fear that this separation gets completely lost in AR6 and for that reason my suggestion would be to remove very likey range from the Table 9.6 and discuss it separately in a probabilistic section. a comprise would be to have the standard likely range in a separate table then the very likely range and assign a different confidence level (low) to the very likely range. [roderik van de wal, Netherlands]	Accepted. We no longer present very likely ranges.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72035	90	10		15	I think this charactrerisation is inaccurate. The ar5 was the first probasbilistic projections, attempting to robustly estimate the mean and the standard deviation, even if they chose not to (blindly) extrapolate to lower porbability values, which in my opinion we know little about, as illustrated in Figure 9.27. [John Church, Australia]	Taken into account. This classification was based on Horton et al. 2018, but this section has been reworked and this discussion restructured.
107339	90	11			This is an odd classification based that mixes the technqiues used (SEMs) with the type of output (central range vs probabilistic). [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This classification was based on Horton et al. 2018, but this section has been reworked and this discussion restructured.
64409	90	13	90	13	here it is argued that projectiona are comparable to AR5 later it is argued comparable to SROCC [roderik van de wal, Netherlands]	Accepted. We now consistently refer to SROCC as the baseline, and to the AR5 only when referring to process-level choices that were unchanged in the SROCC.
77907	90	13	90	15	OK - that's a useful distinction. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This classification was based on Horton et al. 2018, but this section has been reworked and this discussion restructured.
97031	90	14	90	14	Which three probability levels? [Nicole Wilke, Germany]	Taken into account. Which three depends on the study, typically the 17th, 50th and 83rd or 5th, 50th, 95th percentiles. However, this text has been removed.
77909	90	16	90	16	Instead of "physically plausible high-impact" I would suggest "high-end" (Gregory et al., 2019). This is because (a) you're not discussing impact, just the size of the SLR - the impact depends on the resilience of impacted systems as well, (b) "physically plausible" is subjective - what some experts might consider plausible others argue to be impossible under any reasonable assumption. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This text has been restructured. High-end scenarios are discussed in Box 9.4.
31609	90	16	90	16	In fact, most probabilistic projections are exploring high-impacts scenarios, but some have also explored low-end scenarios as discussed page 101 in Box 9.3 and shown on figure 9,27. [Gonéri Le Cozannet, France]	Taken into account. This classification was based on Horton et al. 2018, but this section has been reworked and this discussion restructured.
64413	90	16	90	24	The use of semi-empirical models can be minimized to a note. I believe the point made here was already made in SROCC [roderik van de wal, Netherlands]	Taken into account. As extant semi-empirical models were assessed in the SROCC, this text has been removed.
3343	90	18	90	20	Just some thought on this discrepancy: some of the semi-empirical models might have to be re-evaluated after the downward revision of 20th-century sea-level changes after AR5 (Hay et al. 2015, Dangendorf et al. 2017). That downward revision probably decreases the discrepancy between process-based and semi-empirical models. [Thomas Frederikse, United States of America]	Taken into account. As extant semi-empirical models were assessed in the SROCC, this text has been removed. This point was already made in the SROCC.
129561	90	18	90	20	Just some thought on this discrepancy: some of the semi-empirical models might have to be re-evaluated after the downward revision of 20th-century sea-level changes after AR5 (Hay et al., 2015; Dangendorf et al., 2017). That downward revision probably decreases the discrepancy between process-based and semi-empirical models. [Trigg Talley, United States of America]	Taken into account. This may be true -- Kopp et al 2016 has 20th century GMSL consistent with Hay et al 2015, and little discrepancy with AR5 projections. This point was already addressed in the SROCC, and the text has been restructured.
62239	90	18	90	20	Instead of stating that the “Semi-Empirical models (SEMs) were in general systematically higher..” it should state that the “projections of the Semi-empirical models (SEMs) were in general systematically higher…” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. As extant semi-empirical models were assessed in the SROCC, this text has been removed. This point was already made in the SROCC.
77911	90	20	90	20	Not only for the discrepancy, but also because of doubts about the physical basis of the model. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. As extant semi-empirical models were assessed in the SROCC, this text has been removed. This point was already made in the SROCC.
117285	90	22	90	22	What is SEM ? [Maisa Rojas, Chile]	No longer applicable. This text has been restructured.
107341	90	22			In truth havent these models just be tuned to obtain a better fit to AR5? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Kopp et al 2016 calibrated to the Common Era sea level record, not AR5 projections. In addition, as extant semi-empirical models were assessed in the SROCC, this text has been removed.
107343	90	22			Wasn't Grinstead et al tuned using a two millennium record? Hence cannot simply be use of longer reconstruction that improved consistency. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Grinsted et al was tuned to the tide gauge record, and then used to hindcast the last millennium record. In addition, as extant semi-empirical models were assessed in the SROCC, this text has been removed.
65985	90	23	60	23	Suggest citing: Massom et al., 2018: Antarctic Ice shelf disintegration triggered by sea ice loss and ocean swell. Nature. 558, 383-389, doi:10.1038/s41586-018-0212-1. This suggests a relationship between the absence of sea ice and sudden ice shelf collapse. [Kushla Munro, Australia]	Accepted. This paper now is cited as providing evidence for such a link.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62211	90	28	90	30	this description is a little out of place. Maybe can be moved inside figure caption/description? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This text and figure have been restructured.
22663	90	28	90	30	This is figure caption material not main text material? [Peter Thorne, Ireland]	Taken into account. This text and figure have been restructured.
62181	90	30	90	33	Consider rephrasing to avoid incorrect usage of IPCC uncertainty language [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This text and figure have been restructured. The literature projections are now assessed based on level of agreement and confidence in projections of included processes, but no likelihood statements are made.
64415	90	31	90	31	it is unclear whether deep uncertainty is used here in the way it was defined in SROCC. My understanding in SROCC was that deep unceratinty referred to whether specific physical processes are important or not. Here the use seems different to me. I think the authors should clarify whether they use the new ly adopted SROCC language or whether they make up their own definition for deep uncertainty. Anyhow a rigourous check on ipcc uncertainty language is needed. To my understanding likely rnage is adopted in AR6. In this chapter likely range and likely seem to be used in random fashion and mostly mean likely range and not likely (66-100%) [roderik van de wal, Netherlands]	Noted. SROCC says "A situation of deep uncertainty exists when experts or stakeholders do not know or cannot agree on: (1) appropriate conceptual models that describe relationships among key driving forces in a system; (2) the probability distributions used to represent uncertainty about key variables and parameters; and/or, (3) how to weigh and value desirable alternative outcomes (adapted from Lempert et al., 2003; Marchau et al., 2019b)." The application here is a direct outcome of #1 and #2.
64417	90	31	90	36	I believe the authors may need to refer to Stammer et al. 2019 earth future on possibilities on how to handle the tails of the distribution. An assesment needs to rely on more that only expert judgment and need to discuss other approaches as well [roderik van de wal, Netherlands]	Not applicable. This discussion of p-boxes has been removed.
77913	90	35	90	35	I suggest deleting "plausible". It's enough that the expert is reasonable. A colleague might regard the scenario as implausible nonetheless. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This discussion of p-boxes has been removed.
62183	90	35	90	35	"Expert" is sufficient and maintains objectiviity [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This discussion of p-boxes has been removed.
107345	90	37			Wouldn't this be true if the available literature projections accounted for ALL uncertainty? They do not so that 'extremely likely' cannot be equated to the 5th/95th percentile. Perhaps I am missing something. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This discussion of p-boxes has been removed.
77915	90	38	90	38	I don't like "plausible" again! [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This discussion of p-boxes has been removed.
77917	90	38	90	39	I'm not convinced about "extremely likely by construction". This is not a likelihood that refers to the real world and objectively assessed uncertainties. It's a likelihood describing the judgements of experts. I would leave this out. It's enough that every value in this range is considered possibly by some reasonable expert. Consequently, in Table 9.5, I don't think it's justifiable to head the columns with likelihoods. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This discussion of p-boxes has been removed.
44567	90	42	90	44	I agree with the lower confidence given to studies including MICI but why is the confidence level of studies based on SEJ lower than that of ice sheet models? This seems at odd with all of the model caveats mentioned earlier in this chapter. To mention only one, the issue to reproduce observed basal melt forcing even with specific model tuning shown in Figure 9.21 (Jourdain et al. submitted). [Dewi Le Bars, Netherlands]	Taken into account. Now clarify that we have low confidence in SEJ because (1) the experts participating in SEJ studies may have attempted to incorporate processes (e.g. MICI) in whose quantification we have low confidence, and (2) the individual experts participating in the SEJ study of Bamber et al. (2019) exhibited limited agreement in their assessments.
29253	90	42	90	48	This section refers to portions of Figure 9.27 that are "shaded in dark grey" or "shaded in light grey"; however, there is no grey shading on the figure. The only shading in the figure is yellow or brown, with different styles of grey lines. The text needs to be amended to more accurately reflect the ways that portions of the plot are illustrated. [Andra Garner, United States of America]	Taken into account. This figure has been restructured.
107347	90	42			Comparing the p-box for 5th/9th percentile with ones included MICI and/or SEJ just begs the question which is right? Some form of assessment is required here. What is the basis of taking one p-box forward and not another. The issue then becomes how one assesses an SEJ which is essentially a black box (we know what the participants thought but we do not know why). Tempting to link SEJ to MICI (DeC & P very much in peoples' minds) but we cannot do this because we cannot know the minds of the participants. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This figure has been restructured.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65983	90	46	59	48	Suggest citing: Massom et al., 2018: Antarctic Ice shelf disintegration triggered by sea ice loss and ocean swell. Nature. 558, 383-389, doi:10.1038/s41586-018-0212-1. This suggests a relationship between the absence of sea ice and sudden ice shelf collapse. [Kushla Munro, Australia]	Accepted. This paper now is cited as providing evidence for such a link.
15215	90	50	92	4	These probalistic estimates require as much attention in the Executive Summary as the model results. It is a mistake to continually choose to elevate the model results (in the next section) above these other forms of assessment when we know there are limitations to the model results. A policy maker reading this report will come away focused on the "likely" range for the SSPs, and not appreciate that most of the community thinks they should be considering the long tail results from Structured Judgements, SEMs, and probabilistic assessments. We can't let that happen yet again. [Simon Donner, Canada]	Taken into account. Low-confidence SSP5-8.5 projections now appear alongside likely range SSP projections in text, figures, and tables, in an effort to avoid this outcome.
77919	90	55	91	4	I'm sorry to say that I find this bit practically unintelligible. Since it's motivated by agreement and quantity of evidence, I would say that a confidence statement is appropriate, but not a likelihood. Moreover, a likelihood in the tail isn't justified if confidence is not high. How is a user to interpret a statement that, "We think that X is virtually certain, but we have very low confidence in saying that?" Could you say, more simply, "We have high confidence that GMSLR will lie between the 5th and 95th percentiles of studies including those which consider MICI and SEJ, and medium confidence that it will lie between the 5th and 95th percentiles excluding those studies." This is for 2050, isn't it? Do you have a corresponding statement for 2100? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We no longer present likelihood statements based on the range of literature projections, instead focusing on agreement among projections and confidence in projections of processes.
22665	90	55	91	4	This is very confusing and I don't feel adds very much. It took me several reads to make out what was trying to be said and then I realised it had already been implied by prior text passages anyway. [Peter Thorne, Ireland]	Taken into account. This text and figure have been restructured.
83057	90				Section 9.6.3: the recent IPCC SROCC report should probably the default reference point for this section (rather than AR5). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have gone through the section and updated to use SROCC as a reference point.
83059	90				Section 9.6.3: this section seems more review than assessement. I would suggest that the authors start with SROCC and then discuss the main innovations since the publication of that report, eventually building up to a new assessment of the 21st century projections, based on the same "likely range" approach, before moving on to the trickier post-2100 period. Once the traceability to SROCC has been established, I think they are at liberty to include additional information - such as the greater range of percentiles made available by probabilistic methods. I would also recommend doing the comparison/traceability to SROCC for GMSL, before going on to look at regional/local sea-level change. Comparisons with SROCC should account for change in baseline period. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have gone through the section and updated to use SROCC as a reference point.
83061	90				Section 9.6.3: Rather that discussing "options for projections" I think it would be more useful to list the different strands of evidence and assess the level of agreement/confidence we can have in each before making an overall assessment. In AR5, most of the methodological details were included in Supplementary Materials. I think it might help the impact/accessibility of the report to do likewise here and this would help shif the emphasis to assessment. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section has been restructured.
107251	90				If you're reporting on sea-level projections in the literature, you need to add something like this: "However, Burton (2012) contends that, 'Since the rate of sea level rise has not increased significantly in response to the last 3/4 century of CO2 emissions, there is no reason to expect that it will do so in response to the next 3/4 century of CO2 emissions. The best prediction for sea level in the future is simply a linear projection of the history of sea level at the same location in the past, or about 7-8 inches by 2080, for Long Island.' doi:10.1007/s11069-012-0159-8" [David Burton, United States of America]	Rejected. Evidence for sea-level acceleration is assessed in Section 2.3.3.
108341	91	1	91	4	Is this sentence concluded by suign data given in table 9.5? The same paragraph cites table 9.5 but this particular sentence does not. I was wondering if citing table 9.5 or other source would be helpful for the reader or not. [Aakash Sane, United States of America]	Taken into account. This text and figure have been restructured.
64419	91	1	91	5	I don't see the added value of these statement. If the authors believe this is worth mentioning I would like to see a statement explaining why this is mentioned at all. [roderik van de wal, Netherlands]	Taken into account. This text and figure have been restructured.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107349	91	4			Need to expalin why this is the assessed projection. Clearly at this stage it is based on RCPs not SSPs but by the time of the next drafts a similar literature may well exist for SSPs. What is lacking in this literature that means it cannot be used as a basis for projection? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. SSP-based projections in the literature are very thin (unsurprising, since they are derivative in part of CMIP6 and CMIP6 was still in process for most of the writing of AR6). The primary assessed projections are based on the process-level assessment in this chapter is opposed to a survey of literature projections.
65987	91	7	80	6	Suggest including studies on Australian snow given the lack of information on Southern Hemisphere snow currently presented. See, e.g.: Fiddes et al. 2014 https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/asl2.549; McGowan et al. 2018 https://www.nature.com/articles/s41598-018-22766-z.pdf?origin=ppub [Kushla Munro, Australia]	Taken into account. (Section 9.5.3) However, the suggested papers are pre-SROCC references. The revised version contains a specific paragraph on observed SH snow cover changes focusing on South America. Australian snow cover changes are spatially rather restricted and in Chapter 12.
66867	91	7	91	20	Table 9.5 (9-91) and Table 9.6 (9-96) provide SLR projections for near-term and long-term projection changes. Chapter 7 metrics should include shorter-term metrics to help policymakers discuss these near-term impacts. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Noted. No changes suggested to ch. 9.
66869	91	7	91	20	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Noted. No changes suggested to ch. 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68441	91	7	91	20	Table 9.5 (9-91) and Table 9.6 (9-96) provide SLR projections for near-term and long-term projection changes. Chapter 7 metrics should include shorter-term metrics to help policymakers discuss these near-term impacts. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Noted. No changes suggested to ch. 9.
68443	91	7	91	20	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Noted. No changes suggested to ch. 9.
69225	91	8	91	18	The overview of SLR projections in AR6 is summarized in Table 9.5. As t he projections of SLR have been referred and used by many different adaptation sectors widely, it would be requested to add the projections in AR5/WG1 and SROCC as references. [Kaoru Magosaki, Japan]	Taken into account. The table has been merged with a replacement for Figure 9.27.
115493	91	9	91	18	I'm not sure about the utility of this table. Too easy to be misinterpreted. Why bother including the MICI results when they excluded from the projections? [Robert DeConto, United States of America]	Taken into account. The table has been merged with a replacement for Figure 9.27.
31611	91	9	91	18	I think this table is very informative, but there could be a comment that the "lowest 5th percentile" are very low, and well below 0,3m, which is already considered extremely optimistic although physically plausible in box 9,3. In fact, the sample size to estimate the 5th/95th percentiles at a confidence of 95% is 59 following Wilk's formula, and we do not have this number of models to constrain SLR PDFs. May be the authors could consider to replace the 5th percentile culumn by a "low-end" column using a physically plausible low end, as discussed in box 9.3. Another option could be to limit the low part of the distribution to the 10th percentile. Wilks, S.S. Determination of sample sizes for setting tolerance limits. Ann. Math. Stat. 1941, 12, 91–96. (this comment is based on a similar discussion in https://doi.org/10.3390/w11071507) [Gonéri Le Cozannet, France]	Taken into account. The table has been merged with a replacement for Figure 9.27.
64423	91	9	91	18	I thought the IPCC report should be more than a review and that value should be given to the quality of the underlying literature. Here bluntly the highest value it the literature is assigned a likelihood statement being extremey likely. First of all I thought with AR4 extremely likely was abandoned. Secondly a likilyhood statement requires a quantified statement of unceratinty. Taking the highest published value does not seem to be a qunatified statement of uncertainty to me. So I think the Table should be removed in its present format. [roderik van de wal, Netherlands]	Taken into account. The table has been merged with a replacement for Figure 9.27.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64421	91	11	91	11	it seems incorrect to see that the infromation in Table 9.6 is based on CMIP6 this is not true. [roderik van de wal, Netherlands]	Noted. The new projections are built upon CMIP6 (ScenarioMIP, ISMIP6) inputs. However, this table has been restructured.
35837	91	13	91	14	Referring also to p.110, lines 39-41, I am mystified about using extremely likely to describe high-end estimates for RCP8.5 circa 2100 in Table 9.5 when this domain is widely recognized, including in this report, as characterized by deep uncertainty. As long as less ice than the theoretical maximium of 3.3m SLE in WAIS that is potentially subject to instability, has been lost from AIS, how can we have confidence in these upper bounds sufficient to use likelihood language? [Michael Oppenheimer, United States of America]	Taken into account. We now report likely ranges for those contributions in which we have medium confidence, and additional low-confidence ranges for SSP5-8.5 with no likelihood assigned.
14595	91	13	91	18	Add a cautionary note on taking projections with SEJ into account (to avoid allegations of being alarmist)? [Roshanka Ranasinghe, Netherlands]	Taken into account. The table has been merged with a replacement for Figure 9.27.
62235	91	23	91	35	Figure 9.27 : The extreme projections for the 2050 scenario under the RCP 8.5 and RCP 4.5 for cases including MICI and SEJ are higher than 0.4 m which is not shown in the figure. The x axis cuts away at 0.4 m which tends to be a little misleading. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This figure has been simplified.
20199	91	23	91	35	In Figure 9.27 there seems to be a mix-up between dashed and dotted lines [philippe waldteufel, France]	Not applicable. This figure has been restructured and simplified.
27363	91	26	91	26	The lower corresponds to RCP2.6 and not RCP4.5 [Eric Brun, France]	Not applicable. This figure has been restructured and simplified.
22667	92	3	92	3	the choices made is not really what I think you intend to say here. The earth system will determine the changes - it is not an aspect over which humanity has any role of choice. [Peter Thorne, Ireland]	Accepted. Section has been rewritten.
129563	92	3	92	11	[CONFIDENCE] A choice has been made to use the probabilistic framework shown in Figure 9.28. Has this framework been fully vetted in the literature to the point where confidence can be given in its use? There are surely assumptions and limitations of this framework that impact the results. Have these been detailed anywhere? [Trigg Talley, United States of America]	Taken into account. This framework is based on the assessments earlier in this chapter.
64425	92	3	92	11	can the authors explain what co-variance matrix they use for combining the different components. Are they all independent or all dependent? Or? If this is not crystal clear the whole effect of a probabilistic projection is meaningless. As the covariance matrix is likely as ad-hoc as in AR5 and SROCC it seems again stretching the science too much to come up with a very likely range. [roderik van de wal, Netherlands]	Taken into account. A very likely range is no longer provided; instead, we present a high-end scenario alongside the likely ranges.
72049	92	3		11	Suggest you need to more clearly spell out the steps involved, and the implications for confidence ranges. This is a big step from the AR5. [John Church, Australia]	Accepted. We have expanded on the steps involved.
97033	92	5	92	5	Wrong reference, should probably be Box 9.2 [Nicole Wilke, Germany]	Rejected. Box 9.1 is the correct reference.
77921	92	8	92	9	The AR5 methodology also produces PDFs, but the AR5 reported only one probability level from them. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been rewritten.
64427	92	9	92	9	Can the authors justify that global mean values can be forced with GSAT? Why woud TWS be dependent on GSAT or the Greenland contribution? I might misunderstand this but I am lost. [roderik van de wal, Netherlands]	Taken into account. Clarified that we are specifically talking about temperature-dependent components.
42575	92	9	92	11	Please give an explanation why GSAT projections from an (additional) simple climate model are used instead using the original results from CMIP6 GSAT - as was done in AR5. A short explanation of the FAIR model and a link to chapter 7 would be very helpful! [Sabine Hüttl-Kabus, Germany]	Taken into account. We note that this is for consistency with the assessments prior in the report. The rationale for not using the CMIP6 range for primary projections is spelled out in chapter 4 and 7.
7475	92	9	93	42	I think the description of how these projections are done is lacking way too many details for the reader to understand how the different versions are constructed. It says basically that the approach is similar to that in AR5 but probabilistic. But does it e.g. take into account correlations between different components (like in Le Bars 2018), and I could not follow if/how the remapping from 95th to 83th percentiles was done for all components? Overall I am happy with the section, its great that there is now more than the likely range given, and I hope that the pdfs or cdfs will also be published eventually. However, it would be great with perhaps an appendix with more details on how these calculations are actually done. Note also that power laws relating sea level to temperature and such things that these projections are based upon, would also be of great interest to researchers and I hope those can also be published somewhere. [Magnus Hieronymus, Sweden]	Taken into account. This section has been rewritten.
62217	92	10	92	10	add 'and' before calibrated (and calibrated to be consistent ...) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The two-layer model has been calibrated to be consistent with ECS and TCR.
97035	92	11	92	11	Please introduce the abbreviations ECS and TCR, and consider not using these abbreviations but to write out full expressions. [Nicole Wilke, Germany]	Accepted. Written out in full.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29265	92	14	111	52	Throughout these sections, it is repeated that CMIP6 model run availability is extremely limited, which has caused some scenarios not to be discussed, or causes the authors to expect results to change as more models become available, etc. Are additional models expected to be available and able to be incorporated into these results before the final report is published? If so, and the results will be able to be more complete, then perhaps it is not an issue. But if model results remain extremely limited at the time of publication, I think there needs to be more discussion of why the results were not available, and consideration of whether or not other models (CMIP5, perhaps?) could be combined with what we have from CMIP6 to give more robust results. In its current state, the report seems very tentative, and somewhat incomplete because of the lack of CMIP6 input. [Andra Garner, United States of America]	Rejected. The limited availability of CMIP6 models was entirely expected at the time of the SOD, and is no longer a concern.
107351	92	14			I think it would be better to couch this in terms of the preceedings asessments for each comonment rather than linking to a particular study. The job of the individual subsection was to assess the literature to come up with a projection for their component. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have more clearly laid out how the component-wise projections are traceable to the assessments earlier in the chapter.
85319	92	15	92	15	I'm not sure if it is relevant here, earlier in the chapter when historical sea-level rise is discussed or perhaps both, but I am wondering about the potential implications for sea-level projections of the fact that most coupled models form the Antarctic Bottom water than occupies much of the ocean below 2000m by completely the wrong mechanism, i.e. in models it is formed by spurious open open convection whilst in reality it is formed by complex near coastal interactions with sea-ice in polynias and ice-shelves followed by dense water cascaids to depth, see Heuze et al 2013, 2015. Existing coupled models therefore cannot be expected to accurately simulate future changes in bottom water characteristics? For example, what is the (fractional) contribution to sea-level rises in future from changes in the T-S characteristics of the waters below 2000m, and how well are these changes simulated in the past by the coupled models? Perhaps the contribution to sea-level rise of waters below 2000m is a small contribution to the total sea-level rise so my concern is not important? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Changes in AABW, and changes in the formation and export of AABW as part of the lower cell overturning of the Southern Ocean are respectively assessed in 9.2.2.3 and 9.2.3.1. This assessment include discussion of limitation of climate model as suggested in the review comment. Beyond those limitations, as noted by the reviewer, the contribution of AABW to SLR is small, as shown in Cross Chapter Box 9.1 Figure 1b, so these limitations do not represent a major limitation to our understanding of future SLR
77923	92	16	92	16	You don't need dynamic sea level for GMSLR projections. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Moved to regional section.
33503	92	16			Change: “Following the approach of (Kopp et al., 2014),…” by “Following the approach of Kopp et al. (2014),…”. [Guiomar Rotllant, Spain]	Not applicable. This paragraph has been rewritten.
77925	92	18	92	18	In the AR5, the 5-95% range of models was interpreted as a likely range (i.e. a 2/3 probability range) for projections because of the similarity of (on the one hand) the 5-95% TCR range of CMIP5 models and (on the other hand) the AR5 assessment of the likely range of the TCR using multiple lines of evidence including observations. Without this argument, one would expect 5-95% to be judged a "very likely" range. Is the same argument made in the AR6 for temperature projections and hence GMSLR projections? If not, what is the basis for this judgement? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The thermal expansion projections are now based on the two-layer model calibrated in chapter 7. Broader questions about the interpretation of the CMIP6 range are addressed in Box 4.1.
42577	92	18	92	23	#11: The statement confirms my understanding that as to the steric contribution the „likely“ range, defined as the inner 66%, is equal to the 90% range of the original (unscaled) distribution. Now, Table 9.6 also specifies „very likely“ ranges for the steric component (and others). What are the actual! percentile limits of the very likely range? Those associated with +/-1.7*1.7*StdDev of the unscaled Gaussian? Apparently, distribution scaling is also applied to total SLR (cf. my comment #1 on Table 4.5, Ch4) as to determine bandwidths. Are the bandwidths of ALL contributions (to total GMSL as listed in Table 9.6) constructed in this fashion? Information on this issue should be provided at least in a Footnote to Table 9.6. [Sabine Hüttl-Kabus, Germany]	Taken into account. Very likely ranges are no longer provided.
39719	92	18			"5th-95th percentiles … likely range" isn't 90% probability the very likely range? (same for p93, L50-51) [TSU WGI, France]	Rejected. No, the 5th-95th percentile of the truth is the very likely range, the 5th-95th percentile of model results has been taken in AR5 and SROCC as likely ranges.
77927	92	19	92	19	This is also the method of the AR5, except that a normal distribution was used, with z=1.645. Is t=1.7 larger because of the number of degrees of freedom? How many degrees of freedom? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The thermal expansion projections are now based on the two-layer model calibrated in chapter 7. 1.7 was a rounded version of 1.645.
77929	92	20	92	20	It's a multiple of the SD, I would say; you haven't changed the SD. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The thermal expansion projections are now based on the two-layer model calibrated in chapter 7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64429	92	21	92	21	credible range that seems a new term being a look a like of the likely range? The sentence continues with this yielda likely global. Could it be that the authors mean likely range rather the likely. This is not the same and I fear that at many places they mean likely range rather than likely [roderik van de wal, Netherlands]	Accepted. We now use 'likely range' instead of 'credible range.'
42579	92	21	92	23	The likely ranges (here: 5th-95th percentile) of the thermosteric contributions are narrower than the ones given in SROCC for the 17th-83th percentile. This is explained (cf. p. 92, lines 23-27 & Fig. 9.28, p.214) with the availibility of only 2 models, currently. This important information will get lost on readers, who are skimming through the report until hitting Table 9.6. Table 9.6 should definitely contain a note on this issue (cf. also comment #20 on Table 9.6.) [Sabine Hüttl-Kabus, Germany]	Not applicable. The thermal expansion projections are now based on the two-layer model calibrated in chapter 7.
77931	92	21	92	26	Do you mean the SSP ranges stated are based on *two* AOGCMs, and on only *one* for SSP1-1.9? If so, how did you obtain a SD from one model? When you have more models, it will be important to use a consistent set across all scenarios, or otherwise somehow to adjust the mean and SD. For this reason, AR5 filled in missing scenarios using the abrupt4xCO2 runs as a basis (Good et al., 2013). The two-layer model could also be used for this e.g. Palmer et al. (2018). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. No range was reported for SSP1-1.9 in the SOD.
64431	92	27	92	27	You have full blown gCMs very capable of estimating the thermosteric component why would you like to go back to GSAT as a simplification?? [roderik van de wal, Netherlands]	Taken into account. The full blown GCMs represent a biased sample of ECS and TCR relative to the AR6 assessment. See discussion in Box 4.1 and Chapter 7.
77933	92	27	92	28	You could use the step model (Good et al., comment on line 21-26), as in the AR5. It requires less work to calibrate than the two-layer model. I'm concerned that this statement suggests that new research may have to be done in a hurry after this draft. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We are using the two-layer model for consistency with ECS and TCR assessments.
3147	92	29	92	42	“1996-2014”should be changed to "1995-2014" [Hui Wang, China]	Accepted. Now state 1995-2014.
15553	92	34	92	34	Re: The projected glacier contribution is based on the GlacierMIP Phase 1 results (Section 9.5.1). Section 9.5.1 however shows the GlacierMIP Phase 2 results. Please revise. [SAI MING LEE, China]	Accepted. We now use Marzeion et al. (2020).
62237	92	34	92	39	The projected glacier contribution to the global mean sea-level change projections is based only on CMIP5 data. Does the placeholder literature (Edwards. Submitted) look at CMIP6 data ? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The GlacierMIP projections are driven by CMIP5, but the emulator is used to produce SSP projections.
64433	92	34	92	44	Marzeion et al. 2020 is available thereby Hock et al. 2019 seems surpassed [roderik van de wal, Netherlands]	Accepted. We now use Marzeion et al. (2020).
77935	92	39	92	39	How is the range given decided to be "likely"? I guess that it must correspond to the "likely" range of assessed temperature projections, does it? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now clarify that the likely ranges are based on likely ranges of temperature, plus uncertainty in the relationship between temperature and mass loss.
15555	92	39	92	44	The differences betwwen AR5 and AR6 projections of glacier contribution are indeed not minimal. In particular, the mean contribution for SSP5-8.5 by 2100 is 0.14 m, 0.03 m smaller than that for RCP8.5, i.e. ~18% difference. Please consider revision. [SAI MING LEE, China]	Not applicable. This section has been rewritten.
44569	92	43	92	44	“As with global mean thermal expansion, differences between the AR5 and AR6 projections are minimal.” The uncertainty ranges of glaciers contributions in the AR6 projections are around half of what they were in AR5 for all scenarios. This is a huge difference and it seems important to discuss the source of this difference. Methodological difference or reduced uncertainty due to increased convergence between glacier models? [Dewi Le Bars, Netherlands]	Accepted. We now note the narrowing of the likely ranges.
64435	92	46	92	55	the TWS estimate seems rather ad-hoc and completely independent of climate change. Is there evidence to support this? [roderik van de wal, Netherlands]	Noted. To our knowledge, there is no literature attempting to link anthropogenic changes to terrestrial water storage at a global scale to climate change.
129565	92	46	93	1	Consider moving this paragraph to follow the discussion of ice sheet contributions to sea-level rise, since TWS has followed ice sheets throughout the SLR subchapter thus far (e.g., the opening paragraph of 9.6.1.1 and Table 9.4). [Trigg Talley, United States of America]	Accepted. We now employ oceans/ice sheets/glaciers/other factors structure throughout, consistent with the chapter structure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77937	92	48	92	49	Why does reservoir impoundment decline with population growth? This seems odd, since more people might mean more use of water, as for groundwater extraction. In recent decades reservoir impoundment has indeed been declining, but this is due to silting up, lack of new suitable sites, and environmental concerns about building new reservoirs, if I understand correctly. While these might correlate with population, because both have been changing, that's not a causal connection. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reservoir impoundment does not decline with population growth; the rate of reservoir impoundment growth declines with population growth. This is an empirical observation, and causality is hard to separate out here. From an econ 101 perspective, however, the marginal cost of dam construction increases as the number of dams increases (e.g., due to limited site availability), and so one would expect the elasticity of impoundment to be < 1, and the empirical observation is consistent with this. That said, as now noted, planned dam construction breaks this trend, and so we add an additional term to account for this.
26415	92	50	92	54	Check parenthesis: (e.g.,(Wada et al., 2012)) and (multiplied by 0.8 following (Wada 54 et al., 2016)) [María Santolaria-Otín, France]	Accepted. Parentheses fixed.
77939	92	51	92	51	By "natural" I guess you mean changes in TWS other than by reservoir impoundment and groundwater extraction, such as in lakes, soil moisture and snow. "Natural" is a confusing word to use here, since in IPCC terminology it usually refers to non-anthropogenic but forced change. Sometimes it means unforced variability, but I don't think that's what you mean. Unforced variability in the past has produced changes but they are not expected to result in long-term trends - there are papers which could be cited about that, to support your approach. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now refer to climatically driven changes in land-water storage.
76731	92	52	92	52	“as they are not well quantified or are considered negiible”. Jensen et al 2019 (Long-Term Wetting and Drying Trends… JGR-A, https://doi.org/10.1029/2018JD029989) showed that the detection of global trends in terrestrial water storage from CMIP5 output would require about 30 years of GRACE-like observations.) [Roelof Rietbroek, Germany]	Accepted. Added reference.
33505	92	52			Change: “…(Wada et al., 2016))…” by “…Wada et al. (2016))…”. [Guiomar Rotllant, Spain]	Accepted. Parentheses fixed.
97037	92	53	92	53	Why is groundwater extraction reduced by 20%, please explain. [Nicole Wilke, Germany]	Accepted. Clarified that this is because not all runoff from groundwater extraction reaches the ocean
80665	92	55	95	55	it would be useful here to use the full nake of the scenarios [Helene Jacot Des Combes, Marshall Islands]	Rejected. SSP terminology is used throughout the report.
83029	92		92		The Chapter 9 SOD text suggests that GMST projections from the FAIR model “calibrated to be consistent with the assessment of ECS and TCR in chapter 7” will be combined with CMIP6 simulations of global thermal expansion (GTE) / ocean heat content (OHC) in the AR6 sea-level projections. Note that emulator simulations from Ch7 will all be based on the Two-Layer model, rather than FAIR. I have concerns that a “mix and match” approach will break important correlations between GMST and GTE, and mix ensembles that are likely occupying different projection space. The suggestion we have discussed in Ch7 is to use the Two-Layer model simulations for sea-level projections presented in Ch9. This way, simulations of GMST and GTE are physically consistent and underlying correlations are preserved. GTE can be calculated by combining OHC from the Two-Layer model with the narrow range of expansion efficiencies (e.g. Lorbacher et al, 2015), i.e. the ratio of GTE/OHC, from CMIP5/CMIP6 models, as we did in Palmer et al (2018; 2020). The GMST simulations could potentially be used to determine various other sea-level components, as for the 21st century projections presented in AR5/SROCC. The Two-Layer model may also be a useful basis for extended sea-level projections to 2300 in a more physically-consistent approach (i.e. same model framework under well-defined scenarios) than used for the post-2100 projections presented in AR5/SROCC (e.g. Palmer et al, 2020; UKCP18 Marine Report). One possibility is for Ch9 include two sets of projections: (1) a set tied to the assessed ECS/TCR range of Ch7 based on Two-Layer model; (2) a more “conventional” set, based on CMIP6 models that would allow a more direct comparison with AR5 and SROCC assessments. The presentation could potentially even superimpose individual CMIP6 models on top of the projected range from set (1) to show the individual simulations in a more complete projection space? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The primary projections in the FGD now use the 2-layer model to generate a set of thermal expansion, glacier, and ice sheet projections consistent with the ECS/TCR assessment of chapter 7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83051	92		92		21st century sea-level projections. I would be really helpful to be able to see the evolution of the new sea-level projections relative to AR5/SROCC and understand where the differences come from, in terms of: (i) scenarios; (ii) CMIP6 vs CMIP5; (iii) methods. I think it is important to show this for GMSL sea-level change and its components, even if regional comparisons are out-of-scope. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added tables to enhance traceability to SROCC.
7477	93	3	92	20	There are rather many versions of the projections for this century, and a lack of detail on how they are created. Some of these versions I have a hard time seeing the point with. For example, why is LARMIP used without an Antarctic SMB component? Surely this estimate could be complemented with the SMB from some other estimate to make it more comparable to the main projection? The emulation approach seem to make a big difference, I think this must be elaborated on, explaining e.g. why emulated models are favored over the real data from the MIPs? I am also a bit sceptical of the value of the projection using SEJ based ice. To me this is a questionable methodology to use in a projection especially when only one part of the projection is based on SEJ. I don't see the purpose apart from creating a much higher projection (I would much rather see higher percentiles from the main projections if the purpose is to illustrate a very bad possible future). Also interpreting what the uncertainty range actually means seems very difficult when most of its components reflect modelling uncertainty and one part reflects uncertainty in what experts believe are possible futures. I do believe that the SEJ is a good addition to the table showing different estimates of the ice sheet contributions. I also have some troubles with the SROCC consistent Kopp et al 2014 version. Firstly, I could not follow how this projection was done (i.e. what is SROCC and what is Kopp et al 2014). I suppose that the higher percentiles must some how rely on Kopp et al since they were not given in SROCC, but its hard to follow. Moreover, since the SROCC projection was identical to that in AR5 for all components except that from the AIS I would think that having the SROCC AIS estimate in the table with different ice sheet estimates would be enough. [Magnus Hieronymus, Sweden]	Taken into account. This section has been restructured and traceability to AR5/SROCC improved.
115497	93	3	93	9	Adequate description of ISMIP6 model set up- consistent with the description in Seroussi et al., would be helpful, as would the emulation of the output. [Robert DeConto, United States of America]	Accepted. This is provided in the new Chapter Box 9.3.
42581	93	3	93	20	The paragraph lists several approaches, which have been considered to obtain ice sheet projections. In the end, emulated ISMIP6 results are used for further presentation. It remains unclear how this choice was motivated. Comparing the options in Tables 9.1, 9.2 and 9.A.1, it seems that the chosen results from ISMIP6-Emulator are on the lower side for RCP8.5 scenario. However - the higher values are important - especially for policy makers and risk assessments. [Sabine Hüttl-Kabus, Germany]	Taken into account. Primary results are now based on the combination of ISMIP6 and LARMIP+SMB, and low-confidence results are presented in parallel.
77941	93	3	93	20	Because this is the final draft, I would say that the approach should by now have been decided upon. This paragraph should therefore be rephrased as an explanation of why ISMIP6 is chosen as the basis for the assessment. All the other approaches have by now been set aside, and hence don't need to be mentioned, or mentioned only to explain why ISMIP6 is preferred. At the end of this paragraph and in the next two paragraphs, you refer to the ISMIP6-derived projections as "primary". This choice needs to be justified, and also defined - does "primary" mean that the others are disregarded in your quantitative assessment, and if so why? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Primary results are now based on the combination of ISMIP6 and LARMIP+SMB, and low-confidence results are presented in parallel. Rationale is spelled out in greater detail.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29267	93	3	96	13	I have major concerns about the primary sea-level projections in this report, particularly for high-emissions scenarios. The high-emissions projections here (0.60-0.90 likely range) represent a substantial reduction in the upper bound of the likely range of sea-level projected for high-emissions futures compared to both IPCC SROCC (0.61 - 1.10 m). When accounting for the difference in start year ranges, the upper bound is similar to that from AR5 (0.52 - 0.98 m). After accounting for differences in the start years, the upper bound of the "very likely" range provided here for the SSP5-8.5 scenario (1.07 m) is similar to the upper bound of just the "likely" range from IPCC SROCC for RCP8.5 (1.10 m). Based on the discussion in the report, the downward revision of SLR projections for high-emissions scenarios here is primarily driven by the choice of using emulated ISMIP6 results for ice sheet projections. Section 9.6.3.2.1 provides a good discussion of the emulated ISMIP6 results and how they compare to other potential candidates for ice sheets in this report (LARMIP2 results) as well as other recent ice sheet studies in the peer-reviewed literature (Bamber et al., 2019, SEJ). Notably, the emulated ISMIP6 results are substantially lower and show far less (i.e., none) scenario dependence by 2100 for even high-emissions scenarios, compared to the LARMIP2 results or Bamber et al., 2019 results that are also discussed. Although there is a note in the text about potential over-estimation of the LARMIP2 results for AIS contributions, at this time the decision to use the most conservative and narrow estimate of future ice sheet contributions in the primary sea-level rise projections has not been justified. Both this report (page 95 lines 30-42) and the peer-reviewed literature (e.g., Garner et al., 2018; Horton et al, 2018) suggest that individual studies have tended to project upper bounds of sea-level rise for high-emissions scenarios that are well above estimates from previous IPCC reports. There has been discussion in the literature about reports such as the IPCC being overly conservative, or "erring on the side of least drama," particularly for sea-level rise projections (Brysse et al., 2013; Garner et al., 2018). Given the visibility of this report, and the tendency of many local and regional planning committees to use the results from this report for their own coastal planning purposes, (e.g., "Hawaii Sea-level Rise Vulnerability and Adaptation Report 2017"; "Climate Change and Sea Level Rise Scenarios for Viet Nam"; Unified Sea Level Rise Projection: Southeast Florida"), it is essential that we as a scientific community get this right.	Taken into account. Primary results are now based on the combination of ISMIP6 and LARMIP+SMB, and low-confidence results are presented in parallel.
72037	93	3		42	This says a range of different approaches were considered. But what apprach was used and why? [John Church, Australia]	Taken into account. Primary results are now based on the combination of ISMIP6 and LARMIP+SMB, and low-confidence results are presented in parallel. Rationale is spelled out in greater detail.
77943	93	4	93	4	Since ISMIP6 isn't with SSPs, as you note, more detail on how the scenarios are translated is needed here, since it's something you have done as part of the assessment. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The parametric and GP emulators used for translation are explained in greater detail.
42623	93	6	93	6	I cannot find the a) in this sentence. [Sofie Schöld, Sweden]	Accepted. Corrected reference.
44571	93	6	93	11	This assumption indeed requires a lot more evaluation. I don’t think this is the job of IPCC authors to do. Already adding up available contributions to make a sea level projection is more than simply assessing research, but here complementing with additional controversial assumptions is going too far. Assuming the linear trend is forced but not the acceleration seems arbitrary. Also implicitly assuming that pre-2015 and post-2015 ice sheet response to forcing can be added linearly is not reasonable when the physics of basal and surface melt is known to be non-linearly related to temperature forcing. [Dewi Le Bars, Netherlands]	Taken into account. The baseline adjustment is now spelled out in Box 9.3.
33507	93	6			Change: “...(Levermann et al., submitted, b)...” by “...(Levermann et al., submitted b)...”. [Guiomar Rotllant, Spain]	Accepted. Corrected reference.
35801	93	7	93	9	Earlier text repeats many times over that ice sheet loss due to climate change before 2015 is not included in ISMIP projections. Yet here, the story seems to change but unfortunately, you do not highlight that this is a change from previous discussion nor do you say WHERE in the text the numbers begin to include this adjustment - I assume table 9.6 and associated text. If I am interperting this correctly, then earlier where you mention what ISMIP does NOT include, you should say that proections beginning with Table 9.6 and associated text DO include an adjustment. This is one example of the difficulty you create for the reader to understand which table and text values can be compared - differences in baselines are similarly problematic for the reader. Please find a way to eliminate this problem. [Michael Oppenheimer, United States of America]	Taken into account. The baseline adjustment is now spelled out in Box 9.3.

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107353	93	8			Worth making it clear that the uncertainty in these trends was incorporated into the projection as well as the mean value. Assuming that it was. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The baseline adjustment is now spelled out in Box 9.3.
77945	93	10	93	11	There isn't any opportunity for further consideration in this assessment. It seems like a reasonable assumption that trends at the start of the projection period are due to forced climate change that has already occurred, leading to a committed response. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The baseline adjustment is now spelled out in Box 9.3.
64439	93	11	93	11	It seems too much of a short-cut to ignore LARMIP2 because that study ignores increased SMB. It is probably a useful exercise to estimate the dynamical mass loss. This is an example of an arbitrary poorly jusitified choice of which there are more. [roderik van de wal, Netherlands]	Taken into account. LARMIP is now incorporated with a SMB adjustment similar to that employed in AR5.
42583	93	11	93	13	Please quantify the order of the surface mass balance contribution and its relation to the dynamic part. Isn't it small/negligible in comparison to the dynamic contribution. [Sabine Hüttl-Kabus, Germany]	Taken into account. This is discussed in 9.4.
107355	93	11			However separate results for AIS SMB are also widely available so that it is insufficient to exclude this study because it is dynamics only. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. LARMIP is now incorporated with a SMB adjustment similar to that employed in AR5.
99727	93	12	93	12	may or do? given the assessment that SMB will "very likely result in a negative contribution to future sea level rise" [Peter Clark, United States of America]	Taken into account. LARMIP is now incorporated with a SMB adjustment similar to that employed in AR5.
64441	93	17	93	17	If you use SEJ for context of SSP8.5 Can you elaborate on the fact that SEJ Greenland is nearly twice any other study available at the time of the SEJ survey? Apparently the motivation from Experts is not what other papers suggests. I think a more in-depth discussion of the value of SEJ would be very helpful for people on judging whether they should rely on it or not. For this reason I believe a clear explanation needs to be given on why the tails are less reliable. The current draft reads too much as if we understand the shape of the distribution of all components and the co-variance between all those components. [roderik van de wal, Netherlands]	Taken into account. High-end Greenland outcomes are discussed in Box 9.4.
97039	93	18	93	18	What is meant by 'primary sea level projections'? [Nicole Wilke, Germany]	Taken into account. All occurrences have been rephrased.
44573	93	18	93	20	It is not explicitly argued that the primary projections are better than other possible projections based for example on LARMIP2 or SEJ but it seems that these projections will become the "updated sea level projections" from IPCC. I think ISMIP6 is not a good basis for 3 reasons: (1) (see my previous comment related to p.9-93, l.6-11), (2) The plausibility that future Antarctic mass loss will be independent of the scenarios is low. If SMB compensates basal melt in the future why is the ice sheet projected to loose mass at all then? (3) The complexity of the method is enormous: ISMIP6 model projections, reassessment with an emulator and combined linearly with a linear extrapolation of past mass loss. The combination of complexity, arbitrary/controversial choices and physically strange results will end up reducing the credibility of the IPCC projections. In a situation of deep uncertainty, as acknowledged by the authors, methods should be kept simple to let users of the projections understand how they are made. [Dewi Le Bars, Netherlands]	Taken into account. The medium-confidence projections now use both ISMIP6 and LARMIP, and low-confidence projections incorporated the SEJ are also shown in parallel.
41469	93	18	93	20	As mentioned in a previous comment, the ISMIP6 results (which currently indicate no scenario dependence of corresponding projections when emulated based on the submitted emulator Edwards et al) are highly dependent on the divergent CMIP5 forcing input for the same scenarios, which may cause a distorted picture in the presented results. Please closely coordinate between relevant sections to find a way to isolate this potential ISMIP6 focing bias that may greatly affect the main projections presented here. All other lines of evidence suggest a strong scenario dependence of the AIS SLR contribution. If ISMIP6 in its current form remains the main source for AIS projections, the authors have to be much more clear about the caveats underying this assessment and further reduce their confidence levels. [Alexander Nauels, Germany]	Taken into account. The medium-confidence projections now use both ISMIP6 and LARMIP, and low-confidence projections incorporated the SEJ are also shown in parallel. Note that both ISMIP6 and LARMIP, once adjusted for SMB, show little scenario dependence by 2100 in the median. This is discussed in 9.4.
77947	93	19	93	20	How are ISMIP6 and FAIR combined to give your projections? (This is related to my comment on line 4.) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now clarify how the GSAT projections (now from the two-layer model emulator calibrated by chapter 7) are used to drive the ice-sheet emulator.
107357	93	20			Need to be very careful. This is only acceptable if ISMIP6 has been assessed against all other studies and either found to be superior for methodological reasons or has range that fairly represents those of the studies. I do not think that either has been demonstrated. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The medium-confidence projections now use both ISMIP6 and LARMIP, and low-confidence projections incorporating the SEJ and MICI are also shown in parallel.

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115499	93	21	93	28	This important paragraph is confusing- it’s difficult to decipher what is being compared in different sentences. AR6 vs AR5 or AR6 vs SROCC? AR6 ice sheets vs SROCC ice sheets or the effect of different baselines? The issue of different baselines between AR6 and SROCC could also be better explained to improve traceability between reports. [Robert DeConto, United States of America]	Accepted. We have added a table and restructured text to improve traceability.
77949	93	22	93	22	How is the range given decided to be "likely"? I guess that it must correspond to the "likely" range of assessed temperature projections, does it? (Same comment as p29 line 39.) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now clarify that likely ranges are based on a combination of GSAT uncertainty and statistical uncertainty in the temperature/outcome relationship.
91107	93	22	93	42	Elsewhere in the chapter the very likely and extremely likely ranges are mentioned and discussed but here the discn is limited to the likely range only. This may be as far as you can go for the models but other approaches provide wider confidence intervals that are important for context, for demonstrating the non Gaussian DPFs for projections (which doesn't seem to be discussed anywhere). Many scientists and policy makers want to know about low probability high impact. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now focus on the likely range, complemented by presentation of low-confidence projections
77951	93	27	93	29	What conclusion do you draw from this comparison? In which do you have greater confidence? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now distinguish between medium-confidence and low-confidence processes, and show both.
115495	93	31	91	42	Is this paragraph inteded to somehow justify not incorporating LARMIP2 into the "primary projection". It should be clearly articulated why LARMIP2 is not used. [Robert DeConto, United States of America]	Taken into account. The medium-confidence projections now use both ISMIP6 and LARMIP, and low-confidence projections incorporated the SEJ are also shown in parallel.
77953	93	31	93	31	How is the range given decided to be "likely"? I guess that it must correspond to the "likely" range of assessed temperature projections, does it? (Same comment as p29 line 39.) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now clarify that likely ranges are based on a combination of GSAT uncertainty and statistical uncertainty in the temperature/outcome relationship.
50007	93	31	93	42	It should be emphasized that MICI is not included in these projections, except (perhaps indirectly) through the SEJ Bamber study. [Daniel Gilford, United States of America]	Not applicable. We now explicitly include a MICI sensitivity case based on Deconto et al 2021.
42585	93	31	93	42	Obviously ISMIP6 emulator results for Antarctica are different to previous and other studies but they are chosen for primary sea-level projections - please motivate why you favor these. If the discussion can be found in literature please include citation. On account of the apparent importance of "emulator business" it would appear appropriate to explain the (presumably) Edwards' method in a box. I am aware of cross chapter box 7.1 on "Physical emulation ...", but that box is of little help in this here context, and hence not referred to in Ch9. [Sabine Hüttl-Kabus, Germany]	Taken into account. The medium-confidence projections now use both ISMIP6 and LARMIP, and low-confidence projections incorporating the SEJ are also shown in parallel.
42587	93	31	93	42	#16: Isn't it surpring to find no scenario dependence in the Antarctic? I would strongly expect to find one. What is the physical explanation for that? The ocean results (eg. Fig. 9.7) show a strong warming of 3-4°C in the ocean (SSP585) and increases in GSAT for SSP585 are of the same order. For SSP126 the numbers are 0.5-1°C - it's hard to believe that changes of this magnitude result in (almost) zero net change for Antarctica. See also my comment #22. [Sabine Hüttl-Kabus, Germany]	Taken into account. Note that both ISMIP6 and LARMIP, once adjusted for SMB, show little scenario dependence by 2100 in the median. This is discussed in 9.4.
77959	93	31	93	42	This paragraph compares various projections but does not explain their differences or the reason for your having greatest confidence in your primary choice. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now pool ISMIP6 and LARMIP+SMB results in the medium confidence projections, and explain why we have lower confidence in the SEJ and MICI projections, which are also shown.
107359	93	31			Why jump from AIS to GrIS and then back again? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now have separate treatments of Greenland and Antarctica.
27365	93	32	93	32	0.11 is not the same number as in the Executive Summary [Eric Brun, France]	Accepted. Numbers now align.
77955	93	33	93	34	This is notable, since the AR5 also has little scenario dependence. That was because the SMB term, which is scenario-dependent, is fairly small, while the dynamic term was the same for all scenarios owing to a lack of published evidence for evaluating any scenario-dependence. What is the reason for the lack of dependence in your assessment? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is discussed in 9.4. In the median, SMB grows more negative and dynamics more positive with warming.
107361	93	34			In ISMIP6 SMB and dyamics are included and can cancel each other (warmer atmos more snow, warm ocean more melt). This is not the case for LARMIP, which is dynamics only. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now adjust LARMIP to incorporate SMB.
77957	93	38	93	39	The AR5 has some sensitivity, but not much: at 2100 RCP2.6 is 0.06 [-0.04 to 0.16] m and RCP8.5 0.04 [-0.08 to 0.14]. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Discussion has been moved to 9.4.2.5.
27367	93	38	93	39	This is unclear as in AR5, there is little sensitivity of the Antarctic contribution to 21st century SLR to the different RCPs: SMB projections are quite similar, except for RCP8.5, and the dynamical contribution was scenario independent. [Eric Brun, France]	Accepted. We now note the similarity to AR5 in terms of limited scenario dependence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107363	93	40			I don't think a simple comparison helps anyone. This is an assessment report so need to make a statement about why the studies come up with different numbers. See previous comment for explanaton of why LARMIP is likely to be higher than ISMIP6. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is now addressed in 9.4 and Box 9.3.
35803	93	42	93	42	Don't use "primary" - say which projection you mean. [Michael Oppenheimer, United States of America]	Accepted. We no longer refer to a 'primary' projection.
2529	93	45	93	45	Unclear how ocean dynamic is dedrifted [Tim Hermans, Netherlands]	Accepted. This is now described in detail in the Appendix.
20979	93	45	93	45	The Subtitle "Contributors to regional relative sea level change projections" does not read and resonate well. Not clear what is meant by regional relative sea level change projections. This need to be improved to enhance clarity. Does Contributors mean Factors? [Ladislaus Chang'a, United Republic of Tanzania]	Taken into account. We now use the term 'drivers'.
129567	93	45			This section seems to view regional sea level changes in the coming decades as mainly unpredictable noise. In fact, recent work shows an important contribution from a predictable component of the forced response, which region differences depending on the time evolving forcings. See, for example, Fasullo et al. (2020, JGRO), Fasullo et al. (2020, JCLIM), and Fasullo and Nerem (2018, PNAS). [Trigg Talley, United States of America]	Taken into account. This was not the intention of what was previously written, which did not clearly indicate that the t-distribution fitted was varying with scenario and time. We now emphasize these projections are based on CMIP6 and remove the details to the Appendix.
22669	93	45			I feel peppered by acronyms reading this segment. Are all these acronyms necessary? Some really are not intuitive and also are not defined (maybe they were much earlier in the chapter). The section is very heavy going owing to the propensity for use of acronyms. [Peter Thorne, Ireland]	Taken into account. Most of these acronyms are defined in Box 9.1. Some of the details have been moved to the Appendix.
85321	93	46	93	46	Is it worth stating with references that the representation of many aspects of mean sea-level rise, particuarly in shallow water near the coast, are expected to depend on ocean model resolution and might therefore not be expected to be adequately captured in low resolution IPCC coupled climate models? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now stated.
44575	93	46	93	48	Is drift correction also applied to zos ? It is only mentioned for zostoga. Zos was also detrended in AR5 (13.SM.2.1) which I think is necessary to remove regional drift. [Dewi Le Bars, Netherlands]	Accepted. Now stated in the appendix.
77961	93	49	93	50	The AR5 also assumed no correlation between global mean thermosteric SLR and the patterns of dynamic SLC. In the absence of evidence from CMIP6, this assumption could be tested in CMIP5. I don't remember whether anyone has done that in a published paper. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. As discussed in the Appendix, we calculate and apply the correlation between GMTE and ocean dynamic sea level, following Kopp et al 2014. Bilbao et al. (2015) examined the correlation between global mean thermosteric SLR and DSL, among other variables.
77963	93	50	93	51	What is the basis for the judgement that 5-95% of models cover a likely range? (See comment on p92 line 18.) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Now discussed in the Appendix. See also Box 4.1.
77965	93	51	93	52	It's a multiple of the SD, I would say; you haven't changed the SD (same comment as on p92 line 20). Why is the factor 1.7? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Now discussed in the Appendix.
77967	93	52	93	54	Since you assume the pattern is independent of global mean thermosteric SLR, you could use CMIP5 patterns as well as CMIP6. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. With the increased availability of models, we no longer assume independence.
116899	93		93		Please be explicit on changes since AR5, SR15 and SROCC when developing key findings, and provide explanations (eg effect of CMIP6 vs CMIP5). [Valerie Masson-Delmotte, France]	Accepted. We have structured the section to improve traceability to prior reports.
77969	94	1	94	1	"Barystatic" would be clearer than "mass change" here. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This has been rewritten.
89631	94	6	94	8	Confusing sentence particularly from "and the opposite signs in parts…". Opposite to Australia? A negative vs positive GRD effect? Opposite between NE NA vs NW Europe? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded.
77971	94	7	94	8	The second part of this sentence is grammatically faulty. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded.
3323	94	14	94	14	Maybe I'm wrong, but as far as I'm aware, the GRD solver used for Slangen et al. 2014 is incompressible. But again, I could be wrong on this… [Thomas Frederikse, United States of America]	Rejected. The GRD solver is compressible

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115161	94	17	94	17	This definition is not quite consistent with the one on page 12, line 1. Could this be rephrased to say GIA (as defined on p. 12) associated with ice cover changes during last deglaciation and Holocene, and not including historical melting. If not already in the report somewhere, it could be useful to define the meaning of different time periods (e.g. modern, historical, Holocene, paleo) [Natalya Gomez, Canada]	Taken into account. Time periods are defined in Cross-Chapter Box 2.1. The definition of GIA in Box 2.1 is " ongoing GRD response to past changes in the distribution of ice and water on Earth's surface," which is consistent with the definition here ("ongoing GRD as a result of past changes in the distribution of ice and water, dominated by changes in ice sheets after the last glacial maximum")
100067	94	17	94	31	It is a shame that recent GIA modelling studies that have addressed model uncertainty and focused on projections are not mentioned here. I appreciate that the focus is at the global scale but global models are limited (in the sense that the Earth is not spherically symmetric) and so regional calibrations are more accurate when a 1D Earth model is being applied. I encourage authors to at least acknowledge recent efforts to better quantify uncertainty in regional analyses (Love et al., Earth's Future, 2016 is a good example). Also, the GIA community are making advances towards quanfitying uncertainty in Earth models that accomodate full 3D structure and so a 'nod' to this work is also appropriate in my opiion (a good example would be Li et al., GRL, 2020). [Glenn Milne, Canada]	Accepted. References added.
115159	94	18	94	20	Whitehouse (2018) is a review paper, and uncertainty in GIA due to Earth structure and ice history has been known for many decades. I suggest adding references to early papers on this, and perhaps one that discusses uncertainty associated with lateral variations, e.g. Li et al. (2020) [Natalya Gomez, Canada]	Taken into account. Added reference to Li et al. IPCC assessment reports are not reviews, and so it is not necessary to add references to early papers here; a recent review suffices.
129569	94	21	94	23	Next to the ICEnG and Lambeck estimates, a noteworthy GIA model to include is the recent model from Caron et al. (2018): GIA Model Statistics for GRACE Hydrology, Cryosphere, and Ocean Science. This model uses observational constraints to obtain a GIA model as well as robust estimates of the uncertainties, based on both the ice-sheet histories and the solid-earth properties. [Trigg Talley, United States of America]	Accepted. References added.
61157	94	21	94	23	Why are only these two references useful? [Udita Mukherjee, United States of America]	Accepted. References added.
3325	94	21	94	23	Next to the ICEnG and Lambeck estimates, a noteworthy GIA model to include is the recent model from Caron et al 2018: GIA Model Statistics for GRACE Hydrology, Cryosphere, and Ocean Science. This model uses observational constraints to obtain a GIA model as well as robust estimates of the uncertainties, based on both the ice-sheet histories and the solid-earth properties. [Thomas Frederikse, United States of America]	Accepted. References added.
115163	94	21	94	25	I think there needs to be material added about the effects of 3D variations in Earth structure and their uncertainties (e.g. Love et al., 2016, Li et al., 2020, Yousefi et al., 2019 away from ice sheets; Milne et al (2018) over Greenland). There may be overlapping relevance for this section and the section on contributions of GIA to modern sea level and deformation. The ice sheet referrences are for the modern rather than future projections. I suggest these references be included in one place or the other. Note that there are also studies over Antarctica (Gomez et al., 2018 and van de Wal et al. 2015 e.g.) but these are less directly connected to coastal adaptation. [Natalya Gomez, Canada]	Accepted. References added on lateral variations.
33509	94	22	94	23	Change: "…the ICExG models from (Peltier et al., 2015) and the models from (Lambeck et al., 2014)." By "…the ICExG models from Peltier et al. (2015) and the models from Lambeck et al. (2014).". [Guiomar Rotllant, Spain]	Taken into account. Revised wording.
27369	94	23	94	23	we suggest to insert "anthropogenic induced" before " subsidence" [Eric Brun, France]	Taken into account. We now refer to 'anthropogenic subsidence'
129571	94	23			ICExG and ANU are really not state-of-the-art models. They are just well known and well used. Before the sentence starting ""VLM other ..."" add: ""... are quite well-known examples. However, new global models that rigorously treat ice and stratification uncertainty (Caron et al., 2018) and global models that more rigorously adhere to material properties characterizing flow of the upper and lower mantle rock (Huang et al., 2019) can provide improved assessments."" Citations: Huang, P.P., P. Wu and H. Steffen (2019), In search of an ice history that is consistent with composite rheology in Glacial Isostatic Adjustment modelling, Earth and Planetary Science Letters, 517, 26 - 37, doi:10.1016/j.epsl.2019.04.011. Caron, L., Ivins, E. R., Larour, E., Adhikari, S., Nilsson, J. and Blewitt, G. (2018) GIA model statistics for GRACE hydrology, cryosphere, and ocean science, Geophysical Research Letters, 45, 2203-2212, doi:10.1002/2017GL076644. [Trigg Talley, United States of America]	Accepted. References added.
27371	94	24	94	24	We suggest to refer to Wöppelman and Marcos 2016 (10.1002/2015RG000502) and SROCC references on that point [Eric Brun, France]	Accepted. References added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100065	94	27	94	28	Is is correct to call these VLM trends? They are estimates of secular RSL change unrelated to contemporary climate change and so can be attributed to solid Earth deformation processes. For example, on-going GIA also causes geoidal changes, not just VLM. This is strue for any solid Earth deformatio process. [Glenn Milne, Canada]	Accepted. Clarified these are long-term background rates of change.
129573	94	28	94	30	An alternative approach to include local VLM could be the globally-interpolated GPS maps from the Nevada Geodetic Laboratory. That network is much denser than the tide-gauge network along many coastlines. Another idea is to only use large-scale (GIA+GRD) VLM over the ocean, and use local VLM just for coastal projections. From Figure 9.29, one gets the impression that large coastal VLM signals get propagated into the open ocean, which could cause some trouble when people start comparing the GMSL estimates with the sum of the gridded fields. [Trigg Talley, United States of America]	Noted. This would be a novel methodology not currently published, and go beyond the scope of our assessment.
3327	94	28	94	30	An alternative approach to include local VLM could be the globally-interpolated GPS maps from the Nevada Geodetic Laboratory. That network is much denser than the tide-gauge network along many coastlines. Another idea I got, is to only use large-scale (GIA+GRD) VLM over the ocean, and use local VLM just for coastal projections. From Figure 9.29, I get the impression that large coastal VLM signals get propagated into the open ocean, which could cause some trouble when people start comparing the GMSL estimates with the sum of the gridded fields. [Thomas Frederikse, United States of America]	Noted. This would be a novel methodology not currently published, and go beyond the scope of our assessment.
1787	94	28	94	31	I agree that relying on tide-gauge records is the best we can do for the time being, but it should be recognzied that in rapidly subsiding low-elevaion coastal zones that are most vulberable to coastal flooding, rates of RSL rise are likely to be underestimated due to subsidence in shallow strata that is not recorded by tide gauges (Keogh & Törnqvist, 2019, Ocean Sci). In other words, the numbers used are really minimum values, especially if anomalously high subsidence rates associated with fluid extraction (e.g., Minderhoud et al., 2017, ERL) that are often localized and often missed by tide gauges are also considered. [Torbjorn Tornqvist, United States of America]	Accepted. Added discussion of Keogh & Törnqvist 2019 and Minderhoud et al 2017.
72039	94	28			How is VLM determined from tide gauges (alone)? [John Church, Australia]	Accepted. Spelled out In greater detail here and in the Appendix.
100063	94	31	94	31	How sensitive are the resuts to using a different GIA model as the source of prior information? I encourage the authors to consider at least one other model (from a different group) to quantify the importance of this input. [Glenn Milne, Canada]	Noted. This verges on original research and goes into greater detail than appropriate for this assessment, but unpublished analysis by Kopp's research group suggests this effect is < ± 0.05 mm/yr along the US East Coast and can be safely neglected.
42589	94	34	94	42	Figure 9-28: This (great!) figure needs a detailed introduction and much more explanations - it is not obvious why the global mean sea level changes are calculated in this specific way. Why is there a mix of CMIP6-generated thermal expansion and CMIP5/RCP-driven ice-sheet and glacier components? Why do we need an addional "simple climate model" for calculation of the ice/glacier contributions? What is the motivation of using ice-sheet and glacier emulators instead of the "pure" model outputs? The answers can be found in some cases somewhere else in the chapter/report but I think they should be given here in a coherent way. It is very frustrating to search very often in the different subchapters. The figure is cited only once (p. 92, line 7) but I think it is a very essential one that helps to understand how key findings of the chapter are generated. Results from LARMIP or the SEJ are missing in this workflow but also contribute to GMSL change discussion. [Sabine Hüttl-Kabus, Germany]	No longer applicable. This figure has been replaced with a table.
27373	94	36	94	40	The color mentioned in the legend are not very similar to the colors used on the graph. [Eric Brun, France]	No longer applicable. This figure has been replaced with a table.
41471	94	36			This is a very valuable chart, thanks! [Alexander Nauels, Germany]	No longer applicable. This figure has been replaced with a table.
20201	94	45	94	54	In Figure 9.29 there seems to be a mix-up in the way subplots are identified in the legend [philippe waldteufel, France]	Taken into account. Figure has been restructured.
78039	94	47	94	52	Fig 9.29. Do the maps show the median projection? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified that these shown median results.
27375	94	49	94	49	These are not graphs (b)-(k) but graphs (c)-(h). [Eric Brun, France]	Taken into account. Figure has been restructured.
40003	94	50			caption Fig 9.29 : "ocean contribution" is called ocean dynamics in the figure. This is confusing. [TSU WGI, France]	Taken into account. Figure has been restructured.
27377	94	51	94	51	It's not graph (d) but graph (e). [Eric Brun, France]	Taken into account. Figure has been restructured.
77973	95	1	95	1	I suggest for the title you could say "based on SSPs". Warming levels are also scenarios, of a different kind. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Title now includes "based on SSP scenarios."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77975	95	1	95	3	I don't think you need to call them "updated" and it might be confusing. These are your assessments. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. No longer refer to these as updated.
39199	95	1	95	19	For non-technical readers, how do we reconcile projections of sea level increase/rise (i.e., between those which used RCPs (levels of warming) with those which used SSPs (socioeconomic scenario-based projections)? Is there an easy way of telling policymakers why we should use which set of projections to follow? [Lourdes Tibig, Philippines]	Taken into account. This question is addressed in Cross-Chapter Box 11.1; we add a reference here. We emphasize that the primary result of this analysis is the alignment between the warming-level and SSP projections.
22671	95	1			This feels largely repetitious because you have already outlined contributions and their scenario dependence in an earlier sub-section. This points to perhaps a need to reconcile and merge that section with this one. [Peter Thorne, Ireland]	Taken into account. This section now focuses more narrowly on GMSL.
35805	95	1			Section 9.6.3.3 lacks a coherent explanation of why total SRL projections from this chapter differ, e.g., for SSP5-8.5, 0.6-0.9m compared to 0.61-1.10 for SROCC under RCP8.5. For example, p,95, lines 21-23 can be read to assert that the difference in projected total SLR for all scenarios is almost entirely due to the difference in the Antarctic projections even when corrected for differing base years. It can also be read differently - that is not good. SROCC Table 4.4 gives 0.12 (0.03–0.28) based on 1986-2005 for the Anarctic contribution in 2100 under RCP8.5 while Chapter 9, table 9.6 gives 0.11 (0.01--0.26) based on 1995-2014, again for 2100 under SSP5-8.5. How can the 0.2m difference in total SLR arise from Antarctica? There are indeed notable differences for SSP1-2.6 and SSP2-4.5 in the Antarctic contribution which surely contribute to the differences with SROCC in total SLR but these are not clearly explained either - why do they occur? what's going on at the process or emulator level? There are also differences in some of the other components. I would like to see an additional column for SROCC and perhaps another for AR5 in this table so the various contributions, adjusted to the same baseline, can be compared component by component. The text should not merely explain the differences one component at a time but point to the dominant causes of the differences in the total. If the differences are due to many small contributions but none is dominant, say that. I needn't remind you that the overall difference will be a key focus of policy makers when this report is released. Stakeholders will want a crystal clear explanation of the lowering of the mean and upper end of the likely range for 8.5. [Michael Oppenheimer, United States of America]	Taken into account. We have worked to improve the traceability to SROCC through 9.6.3.
107365	95	3			The use of very likely ranges (see line 17) leaves little room for deep uncertainty. The definition of very likley is 90-10% probability so that the deep uncertainty can must be less than 10%. Perhaps I am missing something. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. While some experts on deep uncertainty recommended the use of very likely ranges to do a better job of communicating tails, we have chosen instead to focus on likely ranges complemented by low-confidence ranges.
77977	95	5	95	5	To be precise, they are based on SSPs. CMIP6 includes many other scenarios, mostly idealised. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Title now includes "based on SSP scenarios."
77979	95	5	95	7	I think this sentence should appear earlier, since you've already been talking about availability of data, for instance for thermal expansion. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Given greater model availability, this is no longer relevant.
65989	95	6	90	6	Suggest including the year of the citation "Palmer et al." [Kushla Munro, Australia]	Editorial - copyedit to be completed prior to publication
77981	95	9	95	10	This remark about SSP1-1.9 belongs with the sentence at lines 5-7. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is no longer germane.
20575	95	12	95	13	A minimum explanation ought to be given about the very high value of projected SL uncertainty in the northern Pacific (plots e, f, g on figure 9.30). Unless we have here dummy FOD panels? [philippe waldteufel, France]	Accepted. In areas (e.g., the western Gulf of Mexico) where rapid subsidence occurs in a cluster of tide-gauges, the associated rates are interpolated to the space in between the tide gauges; in areas (e.g., Alaska and the Bering Strait) where the available tide-gauges exhibit large, tectonically driven VLM that changes considerably in rate over short distances, a sizable uncertainty propagates into the RSL projections
22673	95	12	95	42	This is a lot of time trying to identify and explain apparent discrepancies with SROCC. It feels like you are tying yourselves up in knots over this and it isn't clear to the reader what the value proposition of this focus is. [Peter Thorne, Ireland]	Taken into account. We have made the comparison to SROCC more cumulative throughout 9.6.3.
3149	95	13	95	15	"1996-2014"should be changed to "1995-2014" [Hui Wang, China]	Accepted. Changed to "1995-2014".
69227	95	13	96	11	The reference period of GMSL rise is inconsistent between chapter 9.6.3.3 (1996-2014), Figure 9.30 (1995-2014) and Table 9.7 (1995-2014). Using 1995-2014, which is defined as "modern period" in the SPM as the reference period is suggested. [Kaoru Magosaki, Japan]	Accepted. Changed to "1995-2014".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27379	95	15	95	15	Is it not 1995 instead of 1996 ? (consistency with Table 9.6 and using 20-yr mean) [Eric Brun, France]	Accepted. Changed to "1995-2014".
42591	95	15	95	17	#18: "conditional upon the modelling choices": What exactly are the choices taken? Why were they taken? And why were which other options dismissed? The phrase "conditonal ..." is used several more times in 9.6.3.4 producing an atmosphere of arbitrariness as to what results are eventually popping up here. I would find it much more convincing, if a SEJ approach had been chosen over the ISMIP emulator, since SEJ is a way to account for the full spectrum of approaches, and, hence, yields a more integrative measure of the full bandwidth of estimates on the Antarctic SLR contribution. [Sabine Hüttl-Kabus, Germany]	Taken into account. This presentation of alternative cases has been restructured, including by the incorporation of low-confidence ranges.
72043	95	15		19	The text emphasises these projections are "conditional on the modelling choices". Does this mean that if all information was considered then the confidence ranges would be broader, and perhaps have a different mean? The users need the best information availble not a usbset of that information. [John Church, Australia]	Taken into account. This presentation of alternative cases has been restructured, including by the incorporation of low-confidence ranges.
77983	95	17	95	18	Here you give "very likely" ranges. The AR5 did not do that, and up to now you have mentioned only "likely" ranges, which comI don't understand this paragraph. Once the forcing is gone, consequent ocean heat uptake and thermal expansion will be reversed at the same rate, regardless of what caused the forcing. Absent forcing has no effect on future evolution.e from the 5-95% range of models. How do you derive a "very likely" range? This is a big step, which has not been explained. We must be convinced that it's justified on the basis of the evidence available. Considering the low confidence in numbers at the high end for the Antarctic dynamic contribution in particular, which you discussed earlier, it seems hard to justify evaluating a 95-percentile. Comparing with the AR5, you need to explain the basis of your increased confidence. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have shifted the focus to likely ranges, accompanied by a low-confidence range.
99729	95	17	95	19	How did you derive "very likely" ranges??? [Peter Clark, United States of America]	Taken into account. We have shifted the focus to likely ranges, accompanied by a low-confidence range.
42593	95	21	95	28	I would expect to find the results of all different methods calculating Antarctic projections in Table 9.6 and not only ISMIP6 emulations and the other results in the appendix (Table 9.A1 and Table 9.A.2). It seems to me you can get very different results and likely/very likely ranges by using different approaches simulating the Antarctic contribution. This should be visible more clearly in the final discusion about total GMSL changes. The deep uncertainty should be reflected not only in the supplementary Table 9.A.2, but also in Table 9.6. [Sabine Hüttl-Kabus, Germany]	Taken into account. This is discussed in detail in 9.4, and the medium-confidence projections now rely on both ISMIP6 and LARMIP+SMB, while SEJ results are included in a low-confidence range.
77985	95	21	95	28	I don't see an explanation in Sect 9.6.3.2 of the differences between your and the SROCC assessment of the Antarctic ice sheet contribution. I assume that this difference must be the main interest of this paragraph. It might be clearer to describe qualitatively what the difference is and the effect it has on the numbers, rather than detailing all the numbers, which could distract attention from your own assessment. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is discussed in detail in 9.4, and the medium-confidence projections now rely on both ISMIP6 and LARMIP+SMB, while SEJ results are included in a low-confidence range.
14591	95	21	95	28	Might help to re-iterate that all numbers given in this para related to 2100 projections [Roshanka Ranasinghe, Netherlands]	Taken into account. The second sentence of the paragraph now explicitly identifies the period to 2100.
27381	95	25	95	26	Please specify the time-horizon for the projected rise [Eric Brun, France]	Taken into account. The second sentence of the paragraph now explicitly identifies the period to 2100.
97041	95	25	95	28	Please introduce here for the comparison between AR6 and SROOC the changed baseline again and discuss the differences in median and ranges. [Nicole Wilke, Germany]	Taken into account. We have worked to improve the traceability to SROCC through 9.6.3.
27383	95	27	95	27	please specify that this is the likely rise [Eric Brun, France]	Taken into account. The chapter consistently uses parentheses to identify likely ranges and brackets to identify very likely ranges.
14593	95	30	95	42	Might help to re-iterate that all numbers given in this para related to 2100 projections [Roshanka Ranasinghe, Netherlands]	Taken into account. The second sentence of the paragraph now explicitly identifies the period to 2100.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77987	95	30	95	42	I don't follow the thread or perceive the purpose of this paragraph. What are you trying to say? In the end, it leads to a conclusion "of high-end GMSL outcomes above substantially above the very likely range of the primary projections." This is an important statement. Hence it is very important to be clear about the reasoning. That means firstly being clear about how "very likely" is decided (comment on p95 17-18), which is not described, as far as I can see. Secondly, what does "substantially" mean? This is crucial. As you know, the AR5 settled on several tenths of a metre. This was chosen in order to indicate the order of magnitude, which many people argued was important for planners to be aware of, without offering a precise number to which some readers would probably "anchor" themselves more firmly than science could justify. "Substantially" gives *less* guidance than the AR5. Thirdly, if you indicate that the result could be beyond the likely (or very likely) range, I think you have also to say *how* this might happen, in physical terms. Stated another way, it means you envisage the possible occurrence of a phenomenon which you judge to be "[very] unlikely". What is this phenomenon, and under what circumstances could it occur? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The presentation of medium- and low-confidence results has been revised throughout 9.6.3 to clarify.
27385	95	31	95	31	The wording is unclear. SSP5-8.5 projections are lower and narrower than SROCC/RCP8.5. [Eric Brun, France]	Not applicable. Revised results closely match those of SROCC.
97043	95	31	95	42	L 32-32 state "The greater disagreement for higher emissions scenarios highlights the presence of deep uncertainty in GMSL projections for SSP5-8.5", but L 36-38 "whereas the very likely range for SSP5-8.5 is substantially narrower than the comparable literature range for RCP8.5 (0.50-1.07 m vs. 0.34 m-1.57 m)." How can the "deep uncertainty" lead to a "substantially narrower" range? The current information seems inconsistent, maybe SEJ reduces the range in AR6? Please revise explaining this inconsistency and the role of SEJ. [Nicole Wilke, Germany]	Taken into account. The presentation of medium- and low-confidence results has been revised throughout 9.6.3 to clarify.
62219	95	34	95	34	very likely in Italic (confindence language) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Very likely ranges are no longer provided.
42625	95	34	95	41	Likely and very likely is not italicized in this paragraph. [Sofie Schöld, Sweden]	Accepted. Likelihood terms now consistently italicized.
99731	95	41	95	41	"substantially" is very imprecise! [Peter Clark, United States of America]	Taken into account. Storylines leading to high-end sea-level rise are discussed in Box 9.4.
62221	95	41	95	42	two times 'above', better: ...outcomes substantially above the very likely ... [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Removed redundant 'above'
80667	95	50	95	50	A recent report from NOAA (Sweet et al., 2017) proposes scenarios for sea level rise based on the accelerate rate of melting in Greenland and Antarctica with projected SLR higher that what is presented in this table. The methodology used for these scenarios is completely different from the one used for the results presented in this table and are probably not comparable. However, it could be useful to include them in this discussion and assessment to inform decision makers who have access to these scenarios and help them decide which scenarios are the most useful for them [Helene Jacot Des Combes, Marshall Islands]	Taken into account. Sweet et al. (2017) scenarios are not comparable to those here or in 9.6.3.1, as these are user tools intended to span an assessed literature range, not process-driven products (see discussion of the distinction in Horton et al., 2018). The goal of the IPCC is to assess the scientific literature, not assessments of the scientific literature. More generally, we have made a decision not to assess grey-literature national assessments in this chapter, as that would balloon into an infeasible task. Our hope is that the inclusion of the high-end scenario alongside the likely ranges of SSP-driven projections will facilitate user application.
78041	95	50	95	54	Fig 9.30. The map of uncertainties is not very informative due to the choice of contours. Is the uncertainty range wider than 0.5 m everywhere in the world? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have adjusted the contours.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78043	95	50	95	54	Fig 9.30. I can't find any comment in the text about the pattern and magnitude of regional sea level rise, its deviation from global mean sea level rise, its relation to the contributory processes and forcings, the effect of unforced variability on it, its consequence for particular locations, and its regional uncertainties. There are quite a number of papers about these subjects since the AR5. Just to give one example (which I'm particularly familiar with!), the FAFMIP project (Gregory et al., 2016) was motivated principally by the need to understand and reduce the regional uncertainty in the ocean dynamic contribution, as shown in the AR4 and earlier work, and progress has been made (Couldrey et al., submitted). These two papers are cited in Sect 9.2.2.1 in connection with the interpretation of the past, but they also have things to say about the future. Because RSLR is how sea level change is experienced, it's of more interest to many readers than GMSLR, and I feel it deserves a more thorough treatment, as it was given in the AR5, where it had quite a long section with many figures. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A paragraph has been added to 9.6.3.3. Additional details are provided in the regional chapters and the Atlas.
65991	95	55	91	1	Suggest clarifying the statement "It is extremely likely that 21st century GMSL rise under different emissions scenarios will be above the lowest 5th percentile of projections (high confidence)". Is it necessary to state due to the definition of the 5th percentile? Alternatively, if this is not the correct interpretation, suggest revising. [Kushla Munro, Australia]	Taken into account. This has been rewritten.
66871	96	1	96	13	Table 9.5 (9-91) and Table 9.6 (9-96) provide SLR projections for near-term and long-term projection changes. Chapter 7 metrics should include shorter-term metrics to help policymakers discuss these near-term impacts. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Not applicable. No changes recommended for chapter 9.
66873	96	1	96	13	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Not applicable. No changes recommended for chapter 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68445	96	1	96	13	Table 9.5 (9-91) and Table 9.6 (9-96) provide SLR projections for near-term and long-term projection changes. Chapter 7 metrics should include shorter-term metrics to help policymakers discuss these near-term impacts. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Not applicable. No changes recommended for chapter 9.
68447	96	1	96	13	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Not applicable. No changes recommended for chapter 9.
42595	96	1	96	13	#20: Table 9.6 could well be one of the most important pieces of information on GMSL rise. The current state of Table 9.6, however, makes it difficult to compare the new estimates with estimates published in previous IPCC publications such as SROCC (Table 4.4) and AR5 (Tables 13.5 and 13.SM.1). To improve the coherency among these publications, I suggest to 1) Expand the acronyms used in the 1st column and try to stick with previous names/terms as much as possible. 2) Explain how the ranges come about, and which percentiles they stand for. In particular: clarify if the ranges are true "likely" and "very likely" ranges, or if they are bogusly obtained by scaling original distributions. 3) Specifying sample size together with ranges might support building confidence in range estimates. By all means, include a note that estimates of the thermosteric contribution to sea level rise are currently based on two CMIP6-models, only. 4) AIS is subdivided into 3 geographical sectors. SROCC dealt extensively with the dynamic contribution of the AIS. A subdivision into Surface-Mass-Balance and Dynamics would appear more sensible to afford an intercomparison. The same partitioning is recommended for GrIS. 5) It is unclear to which table entries the footnote "* The ISMIP6 emulator …" applies. The statements on "temporal correlation" are too abridged and require further explanation. It is unclear if the percentile ranges are bogus or true. [Sabine Hüttl-Kabus, Germany]	Taken into account. We have worked to improve traceability to SROCC and AR5 throughout 9.6.3. Detailed assessment of ice-sheet projections is in 9.4.
42597	96	1	96	13	#21: Table 9.6: Transparency and replicability issue. Explain how the "total 2100", the "AIS" or any other composite ranges are computed from the ranges of the contributing components. This is crucial to know if one would like to take into account a different "modelling choice" (cf. my comment #18) e.g. from Table 9.2. Also cf. my comment #9. [Sabine Hüttl-Kabus, Germany]	Taken into account. The framework for combining process-level projections is described in the appendix.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42599	96	1	96	13	#22: Table 9.6 vs. SROCC Table 4.4 Row „AIS“ vs Row „Antarctica 2100“. Antarctic contribution to GMSL is (virtually) scenario-independent in AR6, while SROCC values increase with higher emission scenarios. Great pains were taken to arrive at the SROCC estimates published not even 1 year ago. The short half-life of the SROCC results is disturbing and anything but generating confidence in the AR6 update. See also my comment #16. [Sabine Hüttl-Kabus, Germany]	Taken into account. Traceability to SROCC is now addressed at length in 9.4 and 9.6.3.2.
115501	96	1	96	14	Considering the ongoing lack of understanding of key glaciological processes, and the use of ice flow models that lack representation of observed but possibly very impactful processes (like meltwater-enhanced calving/hydrofracturing), I hope you reconsider providing a “very likely” range. This will be easily misinterpreted as something more than simply the range of the 5th-95th percentiles. Use of “long-term” here (end of the 21st century), is inconsistent with SROCC (beyond 2100). I wonder if SROCC projections can play a role in your post 2100 projections? [Robert DeConto, United States of America]	Taken into account. While the use of a 'very likely' range was recommended by some of our WG2 colleagues with expertise in risk management, we have opted instead for a likely range together with accompanying low-confidence projections. "Long-term" is defined in cross chapter box 1.2.
15217	96	1	96	29	Presenting the results by level of warming is smart. The problem, yet again, is that these results only come from models using the SSPs and RCPs, models which we know have limitations. Policymakers will act on this information -- so we need to offer the long tail of higher GMSLn for each of these warming levels that comes from the non-ESM/GCM studies. I apologize for continuining to harp on this point, but it is so critical to avoiding the problems with intepretation of previous assessments. We need to embrace the reality that in some instances results NOT from GCMs/ESMs can really be as valuable in making numerical assessments. Otherwise we're back to AR4 again. [Simon Donner, Canada]	Taken into account. Low-confidence SSP5-8.5 projections now appear alongside likely range SSP projections in text, figures, and tables.
72041	96	1		6	I must have missed it in the text. How was the sum of the individual terms computed to evaluate the mean and the confidence limits? [John Church, Australia]	Taken into account. The framework for combining process-level projections is described in the appendix.
77989	96	3	96	3	Table 9.6. What is the method and basis of confidence for "very likely" ranges? (See comment on p95 line 17-18.) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A very likely range is no longer provided; instead, we present a high-end scenario alongside the likely ranges.
2479	96	3	96	5	I am surprised that there is an entry for "Thermosteric" but then no "Halosteric". I realise that the halosteric contribution is much smaller than the thermosteric contribution, but if you do separate out the thermosteric contribution you need to include the halosteric contribution. Alternatively, and perhaps better, list the "steric" contribution only. In fact since the equation of state is non-linear we know that thermosteric plus halosteric is not exactly equal to the steric contribution (though it will be close). So listing the complete steric contribution is probably best. [Howard J. Freeland, Canada]	Taken into account. We now use the term 'thermal expansion', consistent with the SROCC. As noted by Gregory et al. (2019), "When freshwater enters the ocean, such as from melting continental ice sheets, it adds to the ocean mass and in turn increases global-mean sea level (barystatic sea-level rise). Ocean salinity also changes due to the dilution of sea water, thus suggesting a role for a global halosteric sea-level change (Munk 2003; Levitus et al. 2005). However, the net effect on global-mean sea level is almost entirely barystatic since the global halosteric effect is negligible (Lowe and Gregory 2006). We can understand why this is so by recognizing that freshwater entering the ocean sees its salinity increase while the ambient sea water is itself freshened. These compensating salinity changes (which are often ignored, as by Munk 2003 and Levitus et al. 2005) have corresponding compensating sea-level changes, thus bringing the global halosteric effect to near zero. We demonstrate this effect in the following sub- sections, by considering a two-bucket thought experiment where one bucket holds fresh- water (bucket-1) and the other holds sea water (bucket-2). We ask how the total water volume changes upon homogenizing the water in the two buckets, while conserving the masses of freshwater and salt. As we will see, the total volume of homogenized water is very nearly equal to the sum of the volume initially in the two separate buckets (to within 0.1%)."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69229	96	3	96	11	The overview of SLR projections by SSP-RCP scenarios is summarized in Table 9.6. As the projections of SLR are referred and used for many different adaptation communities, it would be adequate to add the projections in AR5/WG1 and SROCC in the total short, medium and long term as references. [Kaoru Magosaki, Japan]	Taken into account. We have added a table showing changes from AR5 and SROCC.
15557	96	3	96	11	Considering the larger contribution from the Antarctic Ice Sheet, SROCC revised upward the global mean sea level rise projection for 2100 under the high greenhouse gas concentration scenario by 0.1 m, or around 14% higher than the projection given by AR5. In the latest projection given in Table 9.6 based on CMIP6 scenarios, the projection for SSP5-8.5 by 2100 is 0.73 m, or 0.08 m lower than the corresponding projection for RCP8.5 from SROCC after accounting for the shift in baseline period. On the contrary, the projection for SSP1-2.6 and SSP2-4.5 by 2100 from Table 9.6 are noticeably higher than the corresponding projection for RCP2.6 and RCP 4.5 from SROCC (by 0.07 m and 0.03 m respectively). As a result, the difference between high and low concentration scenarios (SSP1-2.6 vs. SSP5-8.5 and RCP2.6 vs. RCP8.5) by 2100 has significantly narrowed to 0.26 m in AR6 against 0.41 m in SROCC, i.e. the latest mean sea level rise projections have apparently become less scenario dependent. Some analysis and discussion about the reasons and credibility of the downward adjustment from SROCC of the projection at century end under high concentration scenario and narrowing of the projection range between high and low concentration scenarios would be required. [SAI MING LEE, China]	Taken into account. Traceability to SROCC is now addressed at length in 9.4 and 9.6.3.2.
64437	96	6	92	6	How can the Antarctic contribution be so high for the low scenarios, for ssp1-2.6 more than for SSp5-P8.5?? [roderik van de wal, Netherlands]	Taken into account, and discussed at length in 9.4.
1789	96	6	96	7	Here (Table 9.6) as well as in Table 9.7, I'm pleased to see the addition of predicted rates of sea-level rise. This wil be very useful for a variety of applications. [Torbjorn Tornqvist, United States of America]	Noted.
65993	96	21	95	23	Please clarify how to compare AR5, SROCC to AR6 if the baseline is not the same. As noted in Section 9.6.3.2, differences in ice sheet projections, particularly Antarctic ice sheet projections, are the primary driver of differences between the AR5 and SROCC projections for RCP2.6, RCP4.5 and RCP8.5, and the projections here for SSP1-2.6, SSP2-4.5 and SSP5-8.5. [Kushla Munro, Australia]	Taken into account. We have worked to improve the traceability to SROCC through 9.6.3.
77997	97	1	97	29	In the second paragraph you conclude that "The projections for 2°C and 3°C are closely aligned with the projections for respectively SSP1-2.6 and SSP2-4.5, while those for 4°C are intermediate between those for SSP3-7.0 and SSP5-8.5." That being so, I wonder whether this subsection is really useful. You could simply state that conclusion in the previous subsection. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Temperature-level projections are a running thread throughout the AR6.
39201	97	1	98	2	It has become increasingly confusing to provide technical advice to policymakers using this varied ranges of projected SLR, especilly in terms of assessing risks. [Lourdes Tibig, Philippines]	Noted.
129575	97	2	97	29	How do these conclusions compare to values in Table 9.4 on page 9-83? The SLR and projections are scattered in different places across chapters. There are several repeated discussions of the same subject, but with different sets of evidence and publications. This is very confusing and falls short of coherent synthesis. [Trigg Talley, United States of America]	Taken into account. We have added text to clarify that 9.6.3.4 is presenting projections based on global warming levels, consistent with the use of global warming levels to supplement SSPs throughout the report.
107367	97	3			Opportunity to link with SR1.5 [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now tie to SR1.5.
77991	97	4	97	5	The phrase "crossing a certain warming level at 2100" makes the concept upclear to me. Do you mean GMSLR expressed as a function of 2091-2010 time-mean GSAT 2091 relative to the 1850-1900 time-mean? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have clarified.
77993	97	5	95	9	Since the GSAT projections end with values which are unlikely exactly to equal any of the thresholds chosen, how do you use them to derive GMSLR projections for those thresholds? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified that we apply windows around the 2081-2100 temperature targets.
27387	97	20	97	20	Is is not 1995 for consistency with table 9.7 ? [Eric Brun, France]	Accepted. Baseline period now correctly identified as 1995-2014.
77995	97	23	97	23	I'm not convinced of the basis for "very likely" ranges (see comment on p95 line 17-18). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A very likely range is no longer provided; instead, we present a high-end scenario alongside the likely ranges.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65995	97	30	95	31	Please clarify how comparisons have been conducted due to different baseline periods. The likely projections under the standard assumptions for SSP1-2.6 and SSP2-4.5 are quite similar to SROCC projections for RCP2.6 and RCP4.5, but are lower and narrower than for SSP5-8.5/RCP8.5. [Kushla Munro, Australia]	Taken into account. We have tried to make the comparison to SROCC more cumulative throughout 9.6.3.
62241	97	32	97	39	Figure 9.31 : Similar to Figs. 9.29 and 9.30, the X-Axis is not labelled as “Year”. Also, the labelling of the scenarios is too cluttered and ineffective. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure follows IPCC style.
69231	97	34	97	37	Regarding the Figure 9.31, it would be adequate to add SLR projections in AR5/WG1 and SROCC, similarly to the Figure 4.9 of SROCC. It would enhance the understanding of the changes of SLR projections in AR6 for the policymakers. [Kaoru Magosaki, Japan]	Taken into account. We have broadly made efforts to improve traceability, including incorporating SROCC projections for individual time points into figures.
65997	98	1	96	13	Suggest clarification. Are there no projections for 2150 & 2300? [Kushla Munro, Australia]	Taken into account. Primary projections have been extended to 2150. Projections for 2300 are discussed in 9.6.3.5.
21011	98	7	98	7	The title and cotents are not harmonized. The title is not clear. "Multi-century and multi-millennial sea-level commitments" Commitments by Who? Better change to "Multi-century and multi-millennial sea-level projections" [Ladislaus Chang'a, United Republic of Tanzania]	Rejected. "Commitment" has a well-established meaning in the climate science literature (e.g., see section 4.7.2)
41473	98	7			This section provides crucial and policy relevant information and table 9.8 is very helpful. However, this section crucially needs an additional figure that provides the line of sight of figure SPM.7 panel f (which is hopefully modified to show 2300 SLR for FGD). This also calls for more detailed information on post-2100 SLR to be elevated to the ES. [Alexander Nauels, Germany]	Accepted. We have worked to ensure the SPM figures are based upon chapter figures.
35807	98	7			As noted in a previous comment, Section 9.6.3.5 could be merged with material from p. 66, placed here, and then split into two subsections for 2100-2300 and very long term. If so, please add comparisons to AR5 results for 2300. [Michael Oppenheimer, United States of America]	Taken into account. Primary projections have been extended to 2150. Due to deep uncertainty, we discuss projections beyond 2150 together with the very-long term.
129577	98	10	98	11	Sweet et al. (2017) includes projections out to 2200. [Trigg Talley, United States of America]	Rejected. Sweet et al. (2017), as discussed in Horton et al. (2018), is an example of an end-user tool; it generates scenarios of sea-level rise for planning based on an assessment of the literature. It cannot be tied to process-level studies separate from its own assessment, and the chapter is not evaluating national-level assessments.
77999	98	11	98	11	What meaning does "deep" convey? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rephrased sentence.
27389	98	16	98	16	We recommend to use "likely range" instead of the percentile range [Eric Brun, France]	Rejected. We reserve the use of assessment terminology for IPCC assessments; here we are reporting results of individual studies.
51525	98	21	98	27	It would be useful here to include the GMSL projections to 2300 for higher emission scenarios (RCP4.5 and RCP8.5) as detailed here, to the relevant section in Chapter 9 Executive Summary (pg 7, lines 28-30), whilst still acknowledging the deep uncertainty with these projections. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. 2300 projections now included in ES.
78001	98	21	98	27	Given the limited confidence in ice sheet projections in particular, I'm not convinced it is justified to make likelihood statements beyond 2100 at all. What is the basis for this judgement? If it is only a few models, without independent evidence of a different sort (recalling the dimensions of evidence and agreement in the confidence table), I would argue it is not sufficient. SEJ studies do not give me confidence, because we don't know the basis of the experts' subjective choices. In particular, it strikes me as unreliable to assess an event as "extremely likely" if you have only low or medium confidence in the evidence. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Ice-sheet projections beyond 2100 for RCP 2.6 and RCP 8.5 are now assessed in 9.4. Primary projections in 9.6.3.3 are extended from 2100 to 2150 based on the assumption of no further ice-sheet acceleration, which yields only small differences through 2150 compared to the assessed projections for RCP 2.6 and RCP 8.5. Low confidence is applied to all post-2150 projections, discussed in 9.6.3.5.
41475	98	24	98	26	Despite the low confidence level, this is still highly policy relevant information e.g. for SIDS, in particular given the fact that the "extremely likely below 15.5 m under RCP8.5" includes the uncertain MICI response which somewhat marks the most pessimistic case. There is no literature available that would suggest higher SLR contributions in 2300. If all the necessary caveats are provided, this would be very useful information for the SPM, also marking progress since SROCC (low confidence likely ranges for 2300). [Alexander Nauels, Germany]	Accepted. 2300 projections now included in ES.
42627	98	33	99	1	There is a minus sign in the table (Palmer et al., RCP2.6) that probably shouldn't be there. [Sofie Schöld, Sweden]	Taken into account. Table has been corrected and moved to Appendix.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66167	98	44	98	44	Suggest including 2050 since this could be useful from a planning/adaptation perspective. [Kushla Munro, Australia]	Rejected. 2050 projections are discussed earlier; this section is on multi-century and multi-millennial changes.
41477	99	8	99	10	Please also refer to the follow up study by Clark et al in 2018 that was published as a NatCC commentary (https://www.nature.com/articles/s41558-018-0226-6). As the Clark et al figure is very similar to figure 9.32 conceptually, it would be worth considering complementing the current figure 9.32 with additional information provided by Clark et al, e.g. uncertainty infornation. [Alexander Nauels, Germany]	Rejected. Clark et al 2018 uses the same simulation as Clark et al 2016, but translates to cumulative emissions based on specific assumptions about TCRE and ECS. As these parameters are separately assessed in AR6, it would introduce inconsistencies to use Clark et al 2018 here.
39113	99	8	99	20	Add into references and discussion the study Breedam et al. Regarding the Antarctic contribution, findings from Golledge et al could be used here. Van Breedam, J., Goelzer, H., and Huybrechts, P.: Semi-equilibrated global sea-level change projections for the next 10 000 years, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-20, in review, 2020. Golledge, Nicholas R., et al. "The multi-millennial Antarctic commitment to future sea-level rise." Nature 526.7573 (2015): 421-425. [Ola Kalen, Sweden]	Taken into account. Single-process studies are assessed in the appropriate part of the chapter. Added Van Breedam et al 2020.
1791	99	8	99	20	This paragraph is hard to follow. For example, "peak warming" is said to be determined with respect to the cessation of emissions, but that would imply that the overall warming (relative to pre-industrial values, for example) is much larger. Thus, I find these temperature values difficult to interpret. On the other hand, the associated Fig. 9.32 says "temperature anomaly" which leaves the impression that it is something different, adding to my confusion. I am quite familiar with the Clark et al. (2016) paper, so I don't think I should have that much trouble interpreting this information. [Torbjorn Tornqvist, United States of America]	Taken into account. Clarified that this is the peak GMST anomaly relative to pre-industrial.
7479	99	8	100	36	The equilibrium thermosteric contribution has also gotten revised since AR5 (see Hieronymus 2019 ERL https://doi.org/10.1088/1748-9326/ab1c31) I think this should also be considered. [Magnus Hieronymus, Sweden]	Accepted. Now discussed in 9.2.4.
27393	99	16	99	17	It mostly repeats line 5-6 of this page [Eric Brun, France]	Not applicable. Lines 5-6 of this page have no content.
115503	99	22	99	36	Again, reference to palao as a possible model constraint is great, but disjointed from other paleo text. This might be helpful: Dutton et al., 2015, show ~6 m of GMSL rise by ~129-128 ka, indicating a large contribution from the AIS if you believe most Greenland LIG modeling (indicating a minor contribution from Greenland early in the LIG). Getting this much GMSL rise out of Antarctica early during the LIG requires very fast AIS retreat. [Robert DeConto, United States of America]	Taken into account. Paleo text here has been narrowly focused to relate to multi-millennial commitments. Other text has been consolidated into 9.6.2.
76767	99	27	27	99	Here it is said GMSL during LGI was likely 5-9 m higher than todays, in chapter 2 line page 74, line 29 the range 3-11 m is given (same as in Figure 2.33). In Figure FAQ Figure 1.3. a range of 3-10 m is given. [Gema Martínez-Méndez, Germany]	Taken into account. We have ensured consistency across chapters.
42929	99	27	99	31	Again, I agree 5-9 m but that is not what Ch 2 says; and I don't agree 1-2 degrees is "likely" and have proposed 1+-1 in Ch 2. This affects the statements in lines 30-31 as well. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have ensured consistency across chapters.
83843	99	33	99	33	MORLIGHEM et al.; has no year and is in upper case lettering. [Mark Pickering, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication
88727	99	33	99	33	MORLIGHEM et al., (year) ? [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
78003	99	43	99	44	How do you obtain the numbers of 6 and 13 m degC-1? Is it really useful to state these numbers, given the low confidence, and the possibility that the response may be non-linear, which means it can't be properly be described by a number in m degC-1? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Now reference commitments for specific levels of peak warming, and discuss in terms of deg/K only for comparison to AR5.
83053	99		99		There is a typo on the first line of the table: "-2.2m" should read "2.2m" for the RCP2.6 estimate of Palmer et al (submitted). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Table has been corrected and moved to Appendix.
27391	99		99		Table 9.8 / Raw 1 in page 99 : -2.2 m is a typo [Eric Brun, France]	Taken into account. Table has been corrected and moved to Appendix.
116903	99		100		This section could be sharpene, discussing the method used in the few studies assessed. What is the rationale supporting a linear response of sea level committment to levels of warming? A summary of the various points related to sea level committment need to be summarized, and be explicitely reported in the ES. [Valerie Masson-Delmotte, France]	Taken into account. We have clarified the methods used and made stronger use of the figure to explain non-linearity in the sea-level commitment.
78005	100	4	100	11	You've already said that GMSLR might be non-linearly related to GMST change. That means a sensitivity in m Eg-1 is of limited use. Unless there is literature about this metric (you don't cite any), I would delete these lines. The numbers from Clark et al. in the following lines make the point. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We retain this metric for more limited use because it is used in an assessment statement in AR5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50009	100	4	100	21	Please also note the results of Solomon et al. (2009), which showed that thermosteric sea-level rise continues for millenia after forcing ceases.Susan Solomon, Gian-Kasper Plattner, Reto Knutti, Pierre Friedlingstein. Irreversible climate change due to carbon dioxide emissions. Proceedings of the National Academy of Sciences Feb 2009, 106 (6) 1704-1709; DOI: 10.1073/pnas.0812721106 [Daniel Gilford, United States of America]	Rejected. Solomon et al 2009 was assessed in AR5 and does not need to be revisited here.
39843	100	6			"TCRE " please spell out acronyms [TSU WGI, France]	Not applicable. This paragraph has been cut.
78007	100	17	100	21	I don't understand this paragraph. Once the forcing is gone, consequent ocean heat uptake and thermal expansion will be reversed at the same rate, regardless of what caused the forcing. Absent forcing has no effect on future evolution. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Per Ehlert and Zickfeld, "GMTSL rises faster than it declines in response to a symmetric rise and decline in atmospheric CO2 concentration partly because the deep ocean continues to warm for centuries after atmospheric CO2 returns to the pre-industrial concentration". We now state "The slow response of the deep ocean to forcing leads to global-mean thermosteric sea-level fall under declining CO2 being substantially slower than rise under increasing CO2, taking over a millennium to reverse"
33511	100	19			Change: “For example, (Zickfeld et al., 2017) find…” by “For example, Zickfeld et al. (2017) find…”. [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
50011	100	23	100	36	Albedo modification or shelf engineering should not be mentioned unles there is a citation and discussion of such studies, specifically noting which papers these are. As written, each of the examples mentioned appears to be along the lines of a proxy for carbon dioxide removal, and if that is the case it should be carefully noted to be so. In matters of noting geoengineering in the AR6 report, it is /critical/ to be clear about what the results from studies show and do not show. To state that there are studies looking at albedo modification and then not discuss them or point out specifically that is what a study is, is misleading at best. This entire paragraph should be rewritten to clearly state the assumptions/methods of each study, and highlight the concerns/lack of solutions in the sea-level and ice-sheet changes that could be associated with any such solutions. [Daniel Gilford, United States of America]	Taken into account. Clarified studies being referred to.
38495	100	29	100	29	The word of 'geoengineering' should be avoided to be consistent with Chapter 4, 4.6.3. [LONG CAO, China]	Accepted. The term 'geoengineering' is no longer used.
38355	100	29	100	34	Given that the term ‘geoengineering’ is not used in this report as stated in line 50 on page 20 in the Glossary, and based on 4.6.3.2 of Chapter 4, it is suggested that both the words ‘geoengineered’ in line 29 and and ‘geoengineering’ in line 34 be changed to ‘carbon dioxide removal’. [Yaming LIU, China]	Accepted. The term 'geoengineering' is no longer used.
78009	100	31	100	31	The possible irreversibility of Greenland deglaciation was also studied by Ridley et al. (2010) and Gregory et al. (submitted https://doi.org/10.5194/tc-2020-89), and something can be inferred about it from Robinson et al. (2012). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Robinson et al. (2012) is pre-SROCC so not referenced explicitly but has been taken into account in the assessment with Gregory et al. (2020) and Ridley et al. (2010)
38497	100	34	100	34	The word of 'geoengineering' should be avoided to be consistent with Chapter 4, 4.6.3. [LONG CAO, China]	Accepted. The term 'geoengineering' is no longer used.
78011	100	34	100	34	The reversibility of ocean heat uptake and thermal expansion was also studied by Bouttes et al. (2013, 10.1175/JCLI-D-12-00285.1) and Bouttes et al. (2015, 10.1002/2014GL062807). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Bouttes et al. (2013) is included in 9.2.2.1
41479	100	41			Please consider also using data shown in Clark et al 2018 (https://www.nature.com/articles/s41558-018-0226-6). Similar to the Clark et al 2018 figure, a panel split and the visual highlighting of specific warming targets by grey shading would improve clarity and messaging. [Alexander Nauels, Germany]	Not applicable. This paragraph has been cut.
80669	100	42	100	42	what is meant here by Common Era is unclear. It is not described in the text so it should be defined in a footnote or in the glossary. [Helene Jacot Des Combes, Marshall Islands]	Rejected. "Common Era" is common usage, and is used throughout the report.
35809	101	1	103	53	I very much like the pathway approach taken in Box 9.3 but the writing seems dashed off rather too quickly. See following comments for some specifics but there is more than mentioned in these comments and it requires a close read and edit. Unfortunately, the end of the box goes seriously off the rails with respect to substance in discussing MICI - see also following. [Michael Oppenheimer, United States of America]	Accepted. The text has been rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31613	101	1	103	54	This box proposes high-end and low-end pathways based on physical arguments. In the main text, many low and high numerical values are based on probabilistic projections (e.g., figure 9,27, section 9,6,3,1). A third approach that could be also considered is to start from "upper physical limits that can not be exceeded" (i.e., upper bounds) as discussed by Stammer et al (2009 - https://doi.org/10.1029/2019EF001163) (their Fig 2) and Pfeffer et al. (2008) for ice. Pfeffer, W. T., Harper, J. T., & O'Neel, S. (2008). Kinematic constraints on glacier contributions to 21st-century sealevel rise. Science, 321, 1340– 1343. [Gonéri Le Cozannet, France]	Noted. We thank the reviewer for this suggestion but we have decided not to explicitly describe this approach.
115505	101	1	103	55	To summarize the above, before I get to my final (main) comments, I think you have done a fantastic job with this chapter overall and you should be proud of all the hard work! As noted in previous comments, I noticed some inconsistencies in the use of confidence/assessment language in different parts of the text. I also think you could use the palaeo text more effectively with some minor reorganizing. Justification for some choices, like the exclusion of LARMIP2, and sole reliance on ISMIP6 for Antarctica could/should be better explained too, but I know this is all work in progress. [Robert DeConto, United States of America]	Taken into account. Paleo is now covered in more detail in 9.4.3 and LARMIP2 use is better clarified.
115507	101	1	103	55	I do have concerns with box 9.3, and the way the cited literature is interpreted. An uninformed reader might get the impression that ice shelves don't break up or only break up slowly, uplifting bedrock will stop WAIS retreat, and ice cliffs don't fail. That's not what the cited literature says. [Robert DeConto, United States of America]	Accepted. The text has been rewritten.
85323	101	4	101	4	I really like the Box 9.3 on High and Low end sea-level rise projections - it reminds me of the really nice paper by Steve Rintoul on two contrasting futures for Antarctica which it could perhaps even refer to - Rintoul, S. R. et al. Choosing the future of Antarctica. Nature 558, 233–241, 2018. I wonder, however, if a summary schematic diagram might help to clearly get the contrasting scenarios across reducing the need for less technical readers to follow the details? Again, as discussed in other comments on this section, the description of the myriad of issues contributing to the 'deep uncertainties' made me question how robust are our estimates from expert ellucidation on the sea-level rise upper limits and whether they need to be accompanied by more explanatory text to qualify them? I really like the phrase deep uncertainties and wonder if could be useful elsewhere in the chapter, such for the southern ocean and related aspects of global climate? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We thank the reviewer for these useful comments. However, we do not have sufficient space for an additional figure and after careful consideration, we prefer to use low confidence for the Southern Ocean
62347	101	4	101	4	I suggest renaming this box to: "Ice sheet feedbacks in high and low end sea-level rise projections" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We have changed the title to reflect high-end sea-level storylines
105983	101	4	103	55	It would be good to mention somewhere in this box that the high- and low-end pathways follow the definition of physical storylines given in the Glossary and discussed in Section 10.5.3 of Chapter 10. [William Gutowski, United States of America]	Accepted. We now explicitly refer to the Glossary as we introduce the term "storyline".
39983	101	4			Box 9.3: it is not really clear how the processes mentioned in the box are included in the sea level projections. [TSU WGI, France]	Accepted. The text has been rewritten.
107369	101	4			there seems to be a lot of additional assesment in this box, which is often at odds with the assesment in the main text. This is likely to cause confusion. I thought uncertainty language was not to be used in boxes? [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Assessment is consistent with that in 9.4. Uncertainty language can be used in boxes.
111741	101	4			This box is very welcome, and it does a good, thoruogh job of reviewing the processes behind high- and low-end scenarios. However for me a key element is missing, especially for the high end scenarios. While the box reviews many studies and processes and will be of great value to researchers, there is no attempt so synthesise what is known from the literature in terms of plausible scenarios of high-end sea level rise. THis is what adaptation researchers and policy users need. While accepting that there is deep uncertainty around many of the processes, it should nevertheless be possible to construct plausible scenarios or storylines, indicating plausible magnitudes and rates, and maybe the circumstances under which these scenarios might occur. It does not matter that the probabilities can't be robustly assessed. I appreciate that this kind of synthesis is difficult, but in a sense it is the job the IPCC ARs are there to do. The danger of leaving it undone is that it leaves a vacuum to be filled by others, perhaps with less expertise than the authors. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. There is now a storyline for the high end. The potential magnitudes are now quantified.
35811	101	6	101	17	This paragraph is wordy and has some redundancy [Michael Oppenheimer, United States of America]	Taken into account. Text has been rewritten.
78013	101	11	101	11	What meaning does "deep" convey? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Deep uncertainty has a standard meaning as described in SROCC and the glossary. The reasons for deep uncertainty are referred to in the opening paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78015	101	13	101	13	For "physically plausible", which is a subjective judgement, I would suggest "physically possible", which is less judgemental. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence no longer exists.
78017	101	14	101	15	Instead of "physically plausible high-impact" I would suggest "high-end" (for reasons given in my comment on p90 line 16). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Sentence no longer exists.
11377	101	15	101	15	There is an overriding difficulty in the sea-level treatment; I have flagged this line, but the difficulty is broad and deep in the chapter. The IPCC AR4 correctly assessed that certain physical processes were not properly represented in the projective ice-sheet models on which quantiication of sea-level rise is partially based, and thus that fully quantified projections of sea-level rise were not possible. This is still accurate; the main models almost all lack representation of certain well-known but poorly modeled physical processes that could accelerate or greatly accelerate sea-level rise. Because the models lack these representations, the models cannot quantify the likelihood that the processes will contribute any specifed amount of sea-level rise. Thus, the approach from the AR4 should still be used; many terms in the sea-level budget can be projected skillfully with high confidence and low uncertainties, but the possiblity of greatly accelerated loss of marine ice from Antarctica (and particularly West Antarctica) cannot be similarly quantified. Key physical points that bear on this result include: 1) All modern ice shelves buttress, restraining flow of non-floating ice into the ocean; 2) ice shelves are restricted to areas with cold air and cold water; there are no warm ice shelves; 3) ice-shelf loss has been observed, with atmospheric or oceanic warming beyond some threshold triggering complete ice-shelf loss (not monotonic reduction) and leaving a calving cliff that is persistent, with no regrowth of a persistent ice shelf yet observed (yes, multi-year sea-ice growth in the location of the former Larsen B ice shelf is interesting, and short-lived short-length shelves have formed in major Greenland outlets at certain times, but not persistent shelves); 4) ice-shelf loss in models and data triggers faster ice flow, thinning near the coast, and a tendency for retreat from a point of relative stability (a bottleneck formed by a fjord narrowing or sea-floor high) to the next point of stability; 5) in observed cases, the sea-level contribution from any retreat triggered by ice-shelf loss has been globally small, but the geometry of some Antarctic basins would cause the same ice-shelf-loss-triggered retreat to have globally major effect on sea level; 6) The DeConto-Pollard model has attempted to parameterize these processes, but other models do not include both ice-shelf loss and grounded-cliff retreat from calving; 7) Proper physical representation of these processes remains very difficult and uncertain; 8) The DeConto-Pollard model is NOT a worst-case scenario; indeed, that model restricted calving rates to values that have already been exceeded, but there is important information not cited in the chapter showing that i) ice shelves are preconditioned for breakoff (e.g., K. Alley et al., 2019, doi: 10.1126/sciadv.aax2215); ii) retreat in major Antarctic basins could generate longer, wider calving	Taken into account when revising text. In particular, the AR4 approach is discussed under the term of deep uncertainty, which is now highlighted as the underlying thread for this box
78019	101	15	101	15	How does "deeply uncertain" different from "very uncertain"? Since it seems to qualify a probability, is it actually an expression of confidence? If so, please use the standard low [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The term "deep uncertainty" is now implicitly explained as "we can't quantify the likelihood that something will happen"
51981	101	19	101	27	Why not "below-linear" for the low-end example? That would make more of an unbiased comparison with "above-linear" for the high-end example. As it stands, these examples could be criticised for biased language. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Low-end text no longer exists.
78021	101	23	101	23	Obviously alternative pathways exist, since you're choosing possibilities simply for the sake of illustration. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Noted
31607	101	29	101	48	This is a very relevant subsection for WG2. Here, the "low-end" sea-level rise scenarios is discussed for low greehouse gas emissions only. It would be relevant to discuss what a low-end sea-level rise scenario could be assuming RCP8,5, in order to help assess minimum adaptation needs for RCP8,5. For example, I understand that section 9.6.3.1 the figure 9,27 refers to our paper https://doi.org/10.3390/w11071507, and not Le Cozannet et al 2019 Sci Rep (may be correct referencing at line 40 page 147). This paper tentativly delivers low end scenarios and might be useful to determine physical pathways for a low end scenario under RCP8,5. [Gonéri Le Cozannet, France]	Not applicable. Low-end text no longer exists.
1775	101	29	101	55	Greater clarity is needed regarding the probability of occurrence of a pathway for low-end sea-level rise and how that compares to a pathway for high-end sea-level rise. General comments are made about the probability of the pathways in lines 11-17. More detailed discussion would be informative. [Michael Kennish, United States of America]	Not applicable. Low-end text no longer exists.
65999	101	30	99	5	Suggest including a median (50th percentile) estimate. [Kushla Munro, Australia]	Not applicable. Low-end text no longer exists.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42601	101	32	101	37	The pathway for low-end sea-level rise described in this paragraph gives a GSAT rise of 1.5°C-2°C. However, the following SLR contributions are given without a range (e.g. 0.07 m for mountain glaciers). Why is there no range, corresponding to the 1.5°C-2°C? It is stated, that GMSL rise could be around the low end of their likely ranges. Are the numbers for sea level rise contirubutions thus eqivalent to the 13. percentile? Please clarify. [Sabine Hüttl-Kabus, Germany]	Not applicable. Low-end text no longer exists.
62225	101	33	101	33	likely here not in Italic because it refers to likely ranges [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Low-end text no longer exists.
26417	101	39	101	43	Check parenthesis: (Golledge et al., 2019)) and (observed up to 41 mm yr-1; (Barletta et al., 2018)) [María Santolaria-Otín, France]	Accepted, but this sentence no longer exists in its original form
51983	101	41	101	41	Although it's a nice word, "mollifying" is an unhelpful personification. "ameliorating" or "moderating" are a couple of alternatives. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text no longer exists.
35813	101	41	101	41	"mollifying" should be modifying. [Michael Oppenheimer, United States of America]	Not applicable. Text no longer exists.
116907	101	41	102	41	check "mollifying". I suggest to sharpen and shorten the box by around 1/3 [Valerie Masson-Delmotte, France]	Accepted. The box has become 1/3 shorter and only focuses on the high-end scenario
46527	101	44	101	45	Kachuk et al. (2020) may also be appropriate here (Kachuck, S.B., D.F. Martin, J.N. Bassis, S.F. Price. Rapid viscoelastic deformation slows marine ice sheet instability at Pine Island Glacier. Geophys. Res. Lett. (submitted, 2019)). [Stephen Price, United States of America]	Not applicable. Text no longer exists.
52523	101	46	101	46	It would be useful if the high-end pathway were summarised in a similar way to the low-end pathway. [Joanne Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. There is now a single paragraph storyline for the high-end case.
78023	101	46	101	47	What is the physical mechanism which would cause the 21st century rate of GMSLR to be less than the rate in the last three decades, considering that GMST would still be rising? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text no longer exists.
51985	101	48	101	48	Is "luck" the appropriate concept here? It's quite unscientific language that contrasts sharply with what precedes it. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text no longer exists.
16431	101	48	101	48	I don't have any better suggestions but the use of "physical luck" seems oddly vague considering the tone employed generally in the report. Not even sure what this is really trying to say... [Julian Mak, China]	Not applicable. Text no longer exists.
42603	101	48	101	48	I do not think that "physical luck" is a valid concept here. [Sabine Hüttl-Kabus, Germany]	Not applicable. Text no longer exists.
78025	101	48	101	48	What is "physical luck"? [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text no longer exists.
2531	101	48	101	48	Is physical luck the right terminology? [Tim Hermans, Netherlands]	Not applicable. Text no longer exists.
100069	101	48	101	48	What do the authors mean by "physical luck" - an unlikey chain of physical responses? [Glenn Milne, Canada]	Not applicable. Text no longer exists.
35815	101	48	101	48	"physical luck" is a poor choice of words [Michael Oppenheimer, United States of America]	Not applicable. Text no longer exists.
114907	101	48			"physical luck" -- is this the best term? [Robert Nicholls, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text no longer exists.
35817	101	50	102	9	The meaning of "very likely only" stumped me (and I spent a lot of time in SROCC on uncertainty language). Is this a probabilistic statement? I suggest authors go over this section very carefully, consider what they were trying to say, and reword it in simpler language that is at least understandable in IPCC uncertainty gudiance terms. [Michael Oppenheimer, United States of America]	Not applicable. Text no longer exists.
14597	101	50	103	53	For the pathway for low end SLR above this part, numbers are provided. But not in such a clear way for this high end pathway. It would be very useful for policy makers that do want to make plans under deep uncertainity to have an IPCC endorsed "best estimate" of GMSL under deep uncertianity. [Roshanka Ranasinghe, Netherlands]	Accepted. There is now a single paragraph storyline for the high-end case.
62227	101	51	101	51	sentence not clear: either based on or incorporating [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text no longer exists.
80671	101	51	101	51	do you mean 'based on incorporating'? [Helene Jacot Des Combes, Marshall Islands]	Not applicable. Text no longer exists.
34495	101	51	101	51	There is a syntax problem in this sentence. I suggest to suppress "based". [Claire Waelbroeck, France]	Not applicable. Text no longer exists.
80673	101	53	101	53	does the 'very likely' also includes the Antarctic contribution? It may be clearer to clarify it [Helene Jacot Des Combes, Marshall Islands]	Not applicable. Text no longer exists.
62229	101	55	101	55	for the lower end scenario the reference was always a RCP scenario (2.6), now comapring with SSP, it might be better to have the same type of reference for both scenarios [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now generically refer to a "strong-warming scenario" and refer to the respective section 4.8
116905	101		102		Are these "pathways" or "storylines", and how to embed them into other storylines developed in other places of the report? [Valerie Masson-Delmotte, France]	Accepted. Now referred to as storylines with reference to glossary and chapter 10.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83063	101				Box 9.3: I think this box may be more useful if it focusses on high-end scenarios (rather than also including low-end scenarios). I would argue that the real issue in a risk framework is the tails of the distribution - the main chapter text can still make assessment statements about the *minimum* GMSL rise we will see under a range of different scenarios. I also think it would be very helpful to visually represent the high-end scenario(s) in some way - and identify the observable quantities and how we might expect to monitor the real-world trajectory. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Low end is no longer discussed. High end is presented visually in 9.6.3. Observable quantities are discussed.
114911	101				BOX 9.3: High and low end sea level rise projections -- I like this box which deals with an important issue of concern to stakeholders [Robert Nicholls, United Kingdom (of Great Britain and Northern Ireland)]	Noted
78027	102	1	102	3	In view of this qualification, unless you suggest a physical mechanism for it, you must regard 2.0 m as practically impossible, so this pathway should not be discussed, because it's not relevant for policy. The remainder of this paragraph reinforces that conclusion. If 1.2 m is the sum of 95-percentiles, it must already have a considerably smaller probability than 5%, and 2.0 m must have a vanishingly small probability. The SEJ cited at the end of the paragraph is out of line with the other, more objective, estimates and I would argue that it is unreliable. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted in principle. We now more explicitly outline which processes would need to work faster/stronger than currently modelled to combine to a higher sea-level rise than reflected by the likely range.
291	102	1	102	9	Pathway to high end sea level rise: Think this is the most important text in the entire chapter IT PLACES THE UPPER BOUND ON SEA LEVEL RISE and references two submitted papers. This is what planners will use for coastal zones around the world. I don't know if it should be a box. I feel it belongs in a separate section, and then a box to highlight the results. The last sentences of this paragraph leave the reader hanging; which one is correct? [THOMAS Wagner, United States of America]	Noted. The box has been carefully revised given it's importance. The choice of the high-end number is clearer with traceability to 9.6.3 where the high end appears in text and figures.
114913	102	3	102	9	This text stresses the uncertainty of high end sea level rise, depending on the method -- this could be said more clearly to the reader. [Robert Nicholls, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This text has been revised.
114915	102	3	102	9	How is this uncertainty handled by adaptation users? What should they do. The paper by Stammer et al (2019) on high-end scenarios and the lines of evidence supporting provides a way of analysising this type of problem with stakeholders and builds on the concept of H++ scenarios used in the UK since Lowe et al (2009) and carried forward in UKCP18 sceanrios -- see Nicholls et al (2014) (doi: 10.1002/wcc.253). The utilizationof these high end numbers would be useful in Box 9.3. [Robert Nicholls, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. There is now a paragraph on stakeholders with low risk tolerance.
42605	102	6	102	6	Why is the upper end of the "very likely range" used here (above, in the low-end sea-level rise scenario, the lower end of the "likely range" was used)? [Sabine Hüttl-Kabus, Germany]	Not applicable. Low end text no longer exists.
293	102	11	103	53	Why is this material in the box? It's a great assessment--and dismissal--of the mechanisms that can cause more rapid rise. I feel the section needs to explain istelf better up front. We're going to tell you why we think sea level rise cant' be more than a meter... [THOMAS Wagner, United States of America]	Accepted. The box has been carefully revised given it's importance. The choice of the high-end number is clearer with traceability to 9.6.3 where the high end appears in text and figures.
98573	102	16			"interior lowering' -> part of the interior is simulated to iniitally thicken from enhanced snowfall [Miren Vizcaino, Netherlands]	Not applicable. Text no longer exists.
80675	102	18	102	18	different spelling for 'Vizcaino' (accent on the i) [Helene Jacot Des Combes, Marshall Islands]	Not applicable. Text no longer exists.
33513	102	18			Change: “…(Vizcaíno et al., 2010; Vizcaino et al., 2015). » by « … (Vizcaíno et al., 2010, 2015). » . [Guiomar Rotllant, Spain]	Editorial - copyedit to be completed prior to publication
78029	102	24	102	24	Gregory et al. (submitted https://doi.org/10.5194/tc-2020-89) find the SMB-elevation feedback increases the negative SMB perturbation by 20% on average in the second and third centuries of prolonged warm climate (like late 21st century), but on the longer term it is much less important than the contraction of area and the effects on regional climate of the changing topography, which give strong negative feedbacks on mass loss. (Same comment as p55 line 31.) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The importance of the elevation-mass balance-feedback is no longer central to this box
42629	102	26	102	26	There is a "that" too many in the sentence. [Sofie Schöld, Sweden]	Not applicable. Text no longer exists.
78031	102	26	102	40	This material doesn't appear in the earlier discussion of Greenland SMB (Section 9.4.1.1). It reads like an list of selected items from the papers cited, without the context and balance of the earlier section. Please could you make them consistent, or delete this paragraph. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, this paragraph has been reduced to half a sentence, with references to 9.4 for a detailed treatment
35819	102	26	102	40	Switching units from % to Gt dilutes the message. [Michael Oppenheimer, United States of America]	Not applicable. Text no longer exists.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33515	102	26			Erase the second “that”: “Based on consistency between multiple studies there is medium confidence that that over decadal to multi…”. [Guiomar Rotllant, Spain]	Not applicable. Text no longer exists.
46489	102	34	102	36	In addition to "Reductions in cloud cover", I suggest to change of solid to liquid phase clouds could enhance the melt over the Greenland ice sheet according to "Hofer, S., Tedstone, A.J., Fettweis, X. and J. Bamber. Cloud microphysics and circulation anomalies control differences in future Greenland melt. Nat. Clim. Chang. 9, 523–528, doi:10.1038/s41558-019-0507-8, 2019." [Xavier Fettweis, Belgium]	Accepted. This reference is now cited.
46491	102	36	102	40	I also suggest to add that the future projection consider that no change in general circulation in summer (as the ones currently observed) will occur in future. If such changes will occur, the melt increase should be multiplied by a factor 2 according to Delhasse et al. 2018 [Xavier Fettweis, Belgium]	Accepted. This reference is now cited.
115509	102	43	102	46	First, (line 43-46) for the low-end projections. Larour et al. (2019), Gomez et al., (2015), Pollard et al., (2017), and DeConto et al., (in review), all consistently show that bedrock uplift/sea level feedback has little impact on retreat for several centuries into the future, even if extremely viscous mantle (as reported in Barletta et al., (2018)) is assumed to underly all of West Antarctica (DeConto et al., in review). The negative feedback effect is demonstrated to be considerable in the long term future, but not in 2100. PIG has been retreating for more than a half century (Smith et al., 2016), in the one place on Earth where very viscous mantle (Barletta et al., 2018) should be offering a negative feedback to stop/slow retreat. Use of the phrase “physical luck” here seems inappropriate. [Robert DeConto, United States of America]	Not applicable. Text no longer exists.
64453	102	45	102	45	SROCC did not only raise the upper bound of the likely range but also the mean, so as it stands it is wrong please adjust [roderik van de wal, Netherlands]	No longer applicable. In shortening the box, the discussion of earlier approaches has been removed.
72045	102	45		49	I do not understand this argument at all. The additional allowance in the AR5 is not increased by the SROCC projections. In fact if anything, the AR% allowance to 2100 is most likely overestimated, as indeed confirmed by miost of the post AR5 publications and as used in Chapter 9 (with the exception of DeConto and Pollard). [John Church, Australia]	No longer applicable. In shortening the box, the discussion of earlier approaches has been removed.
78033	102	48	102	49	It would be appropriate here to state your assessment, for comparison with the "several tenths" of AR5, and citing the section in which you describe your arguments. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	No longer applicable. In shortening the box, the discussion of earlier approaches has been removed.
35821	102	48	102	49	Understanding the significance of this assertion would be easier if the specific new literature were cited. Otherwise, it's a very general and not very meaningful claim. [Michael Oppenheimer, United States of America]	Accepted. Text has been revised and this sentence no longer exists.
9027	102	51	102	51	Right, this is correct, yet the models do not include that effect, they reveal Antarctica driven by SMB. Funny how the report self contradicts itself. Other sections state that the effect of precipitation will dominate … [Eric Rignot, United States of America]	Accepted. The text in the other sections is now consistent in the drivers for Antarctica.
115167	102	51	102	51	While this may be clear for the modern and next century, I do not think there is evidence that atmospheric forcing does not play a role in the long term future or paleo evolution of marine sectors of Antarctica. This sentence currently reads as universally true that oceanic forcing dominates, which text starting on line 33 suggests otherwise. Could this sentence be adapted to discuss timescale, with references added? [Natalya Gomez, Canada]	Accepted. The statement on oceanic forcing as dominant has been removed from the box.
15847	102	51	103	20	Similar comment about MISI as above. [Olga Sergienko, United States of America]	Accepted in line with the respective answer in 9.4.2.2
78035	102	51	103	53	This appears to be a separate discussion of MISI and MICI from those in Box 9.2 and Section 9.4.2.2. It's confusing and redundant to go over it again. Please could you make them consistent, or delete these paragraphs. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have reduced redundancies to a minimum to allow the box to be self-explanatory yet substantially shorter than before
30655	102	54	102	54	MISI is different from grounding line retreat by the fact that it is not controlled by the applied forcing. To make that difference, it is better to rephrase this as 'results in a sustained/uncontrolled landward migration of the grounding line'. [Frank Pattyn, Belgium]	Not applicable. In shortening the box, this statement has been removed.
62231	103	1	103	1	even instead of ever [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text no longer exists.
72047	103	1		20	Shuld add here the stabilisation tendency from vertical uplift f the bedrock in this low viscosity mantle region. [John Church, Australia]	Not applicable. Text no longer exists. This discussion is now in 9.4.3.
89633	103	3	103	20	Some recent studies that demonstrate the long upper tail of MISI-related SLR contribution include Robel et al. (2019) doi: 10.1029/2019GL085027; and Nias et al. (2019) doi: 10.1029/2019GL084941 [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The relevant section of this box has been substantially shortened and no longer discusses these details.
100071	103	9	103	12	"Despite some…" This sentence seems to have gotten garbled. [Glenn Milne, Canada]	Not applicable. Text no longer exists.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46529	103	23	103	26	Recent work of Jeong et al. (2020; submitted 2019), based on fully-coupled Earth system modeling that allows for circulation and heat and freshwater fluxes within sub-ice shelf cavities, should also be cited in support of these statements (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). [Stephen Price, United States of America]	Accepted. This reference is now cited.
100119	103	28			Phrase "changes in buoy" is meaningless on it's own. Should it be "changes in buoy types" or "changes in buoy data gathering"? [Ronadh Cox, United States of America]	Accepted. The text has been rewritten, and the wording has been changed. [refers to p. 105, not 103]
64447	103	33	103	53	This part reads as an attempt to bash MICI rather than as an explanatory box, Clerc uses idealized conditions in their abstract whereas later in the paper they allow for much smaller cliffs. Banwell is not a physical model [roderik van de wal, Netherlands]	Accepted. Text has been revised.
115165	103	33	103	53	Deconto et al. (in review) should discussed in this section. [Natalya Gomez, Canada]	Accepted. This reference is now cited.
15849	103	33	103	53	Similar comment about MICI as above. [Olga Sergienko, United States of America]	Accepted in line with the respective answer in 9.4.2.2
50013	103	40	103	53	A discussion of Gilford et al. (2019) should included with respect to MICI, and how it suggests that best available constraints from paleocimate observations are indicative that MICI may be required to reach those levels of AIS loss in the early-LIG. At very least, it should be noted from Gilford et al. (2019) that interpretations of paleoclimate observations have a strong bearing on teh conclusions one reaches with respect to MICI. [Daniel Gilford, United States of America]	Accepted. This is no longer covered by this box, but is now reflected and cited in 9.4.2.2
35823	103	42	103	53	This argument about MICI is overstuffed with references compared to text in trying to compress a complex situation into two overlong sentences. It does not do justice to the clearer discussions of MICI earlier. As a result, the final message of the box is dismissive of MICI, in contrast to the more balanced presentation earlier in the chapter. In addition, several of the references are not informative on the key point. Finally, the contorted final sentence unjustifiably dismisses rapid ice shelf collapse (in face of the multi-week collpase of a large part of Larsen B), and offers contradictory evidence on the same point. A much more nuanced discussion is justified. All that seems to be agreed upon at this point is that it is unlikley that MICI will result in any largescale losses in this century. If you are going to discuss deep uncertainties, MICI needs to be taken seriously, at least for the longer timescales, and perhaps for the latter part of this century. [Michael Oppenheimer, United States of America]	Accepted. Text has been revised.
115511	103	45	103	46	Line 45. Great to see that possible negative cooling feedback with meltwater input is mentioned again here. Like Trusel et al., (2015), the new DeConto et al., work indicates the onset of extensive surface meltwater is delayed until the very end of the 21st century and beyond, ~25-30 years later than simulated in DeConto et al., 2016. [Robert DeConto, United States of America]	Accepted. This implication of the DeConto et al 2021 paper is now discussed in section 9.4.2.4
129579	103	46	103	46	Combine Bronselaer, Golledge, and Bell into the same set of parentheses. [Trigg Talley, United States of America]	Not applicable. In shortening the box, this statement has been removed.
115513	103	49	103	50	Firstly, ice-cliff cliff calving in DeConto and Pollard, 2016, in review, is based on a diagnosis of the vertically integrated stresses at the ice front, as a function of cliff height, crevassing, and buttressing, not simply cliff height as implied here. [Robert DeConto, United States of America]	Not applicable. In shortening the box, this statement has been removed.
115515	103	49	103	53	Perhaps most important here is the use of Clerc et al., (2019) to suggest ice-cliff calving isn't a possibility. In their Maxwell-style (viscoelastic) analysis, very tall marine-terminating ice cliffs are shown to be stable if they become unbuttressed slowly. Importantly, Clerc et al show strong very sensitivity of their results to assumed ice properties (i.e., fracture toughness, grain size). Their reported findings are appropriate for "undamaged ice", not for natural, fast flowing glacial ice. Assuming damaged ice properties, they predict tensile failure at ice cliffs as low as 60 m tall, supporting the notion that MICI is something to be taken as a serious possibility. Simply assuming rates of buoyancy-driven calving like that seen in some Greenland outlets (not full blow MICI) are possible in a future, warmer Antarctica, the ice sheet would contribute much faster sea level rise (cm/yr) than indicated by models lacking these processes. As this box currently reads, I worry that sea level outcomes outside the "very likely" range in table 9.6, and even more importantly in post 2100 projections, will not even be considered a possiblity which would be a serious omission. [Robert DeConto, United States of America]	Accepted . The framing of the box has been revised to now outline a plausible, but unlikely scenario for high-end sea-level rise throughout
26419	103	51	103	53	Check parenthesis: (e.g. years, (Cook and Vaughan, 2010)) and (e.g. weeks, (Robel and Banwell, 2019)). [María Santolaria-Otín, France]	Not applicable. In shortening the box, this statement has been removed.
129581	103	53	103	53	It's also been pointed out that "MICI is not required to reproduce sea-level changes," in the mid-Pliocene" (Edwards et al., 2019, Nature). [Trigg Talley, United States of America]	Not applicable. Text no longer exists.
295	104	1	104	5	There needs to be some "connect the dots" between section 9.6.3 and 9.6.4. These two sections are too disconnected. [THOMAS Wagner, United States of America]	[taken into account] The section has been substantially rewritten with aim of improving the narrative.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
297	104	1	108	21	I know this is hard to write. But I found this section challenging to follow. I think it needs a narrative thread. The paragraphs stand alone fine, but seem disconnected overall. [THOMAS Wagner, United States of America]	[taken into account] The section has been substantially rewritten with aim of improving the narrative.
132409	104	1	108	33	It might be useful for the authors to check the assessment on extreme sea levels that was provided in the IPCC SREX report (2012; chapter 3: https://www.ipcc.ch/site/assets/uploads/2018/03/SREX-Chap3_FINAL-1.pdf) [Sonia Seneviratne, Switzerland]	[taken into account] The section has been rewritten in many places, aiming to bring a more coherent flow and story in line with prior assessments.
40775	104	1			section 9.6.4: improvements since AR5/SROCC not very clear [TSU WGI, France]	[taken into account] The section has been substantially rewritten with aim of improving the narrative and connection to AR5/SROCC.
66003	104	6	104	6	Suggest clarification of the statement: "primarily follow trends", since there have been numerous papers that have documented a very rapid non-linear increase in coastal flood frequencies (i.e. extreme sea levels), e.g.: - Sweet and Park (2014) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014EF000272 - Hague et al. 2019 http://www.bom.gov.au/jshess/docs/2019/Hague2_early.pdf - Hino et al. 2019 - https://advances.sciencemag.org/content/5/2/eaau2736. Refer to Figure 5 a & b for evidence to support the assertion that extreme sea level frequency is much more non-linear than sea level rise . [Kushla Munro, Australia]	[Taken into account] This issue is addressed in the following paragraph, where increasing trend in high tide flooding is addressed. Specific wording 'primarily follow trends' no longer appears in edited/clarified text.
62245	104	9	104	10	Could you please especify the "early periods". When? That will help to contextualize the public. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] The time period has been defined, dating back to the early 19th period (earlier for limited examples). The limited examples remains vague, but not worth reporting individual long records.
14599	104	10	104	10	Suggest to use ESWL to refer to extremes in SWL. See also my comment on ESL definition in the box on Pg 12. [Roshanka Ranasinghe, Netherlands]	[taken into account] Following redefinition in Box 9.1, the use of ESL, ESWL, ETWL and ECWL has been revisited throughout the section, and appropriately used in each instance.
52527	104	16	104	16	Detecting and forecasting trends in extreme sea level records requires additional considerations to trends in mean sea level. One is the 18.6 year nodal cycle in amplitude of the astronomical tide. For effects of this on flooding, see eg "Tidally-driven interannual variation in extreme sea level probabilities in the Gulf of Maine, Hannah Baranes et al https://doi.org/10.1002/essoar.10502881.1 " [Joanne Williams, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] Reference Baranes et al. 2020 added, with comment on 18.6 nodal cycle of astronomical tide as another contributor to interannual-interdecadal variability of ESL.
129583	104	16	104	18	"High tide flooding" is a NOAA term (formerly nuisance flooding). The original reference using NOAA flood height thresholds to assess changes in flood frequency was: Sweet, W.V. and Park, J., 2014. From the extreme to the mean: Acceleration and tipping points of coastal inundation from sea level rise. Earth's Future, 2(12), pp.579-600. The other references use probabilistic thresholds that do not necessarily equate to impacts or flooding. [Trigg Talley, United States of America]	[accepted] Reference Sweet and Park, 2014 added, relevant to "high tide flooding'.
82617	104	17	104	17	It needs to be clarified what period the 165% increase is over: Figure 9.33 implies that it is the difference between 1995-2014 and 1961-1990, but other parts of the text (e.g. P7 L45) suggest it is over the 20th century. [Blair Trewin, Australia]	[accepted] Variation of text clarifies the period 1961-1990 to 1995-2014 as that over which the defined increase occurs.
14601	104	20	104	20	What do you refer to as ESL here? Extreme SWL or extreme TWL? [Roshanka Ranasinghe, Netherlands]	[taken into account] Following redefinition in Box 9.1, the use of ESL, ESWL, ETWL and ECWL has been revisited throughout the section, and appropriately used in each instance.
14605	104	26	104	26	Please also consider the newer paper by Arns et al on this non-linear interactions effect https://www.nature.com/articles/s41467-020-15752-5 [Roshanka Ranasinghe, Netherlands]	[accepted] Reference added.
31615	104	26	104	28	May be the physical links between modes of climate variability and extreme sea-levels could be highlighted a little bit more here, as developped below lines 47 and following. For example, Seirstard et al. (2007) shows how teleconnection patterns change storminess modes, which is seems the most obvious reasons for teleconnected extreme water levels. A Seirstadt et al. 2007: https://doi.org/10.1111/j.1600-0870.2007.00226.x [Gonéri Le Cozannet, France]	[taken into account] Rephrasing of paragraph, and citing the AR5 comments on ESL variability associated with modes of climate variability.
66001	104	30	99	5	Suggest clarification. Is the -ve sign on "-2.2m" an error? If not, suggest explaining. [Kushla Munro, Australia]	[Not applicable] This text does not appear

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62247	104	39	104	41	Talking about resonance, could be relevant to mention seiches processes. As there is a recurrent process in some basins (areas), this ones will be more prone to be affected than other basins with different dimentions? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[rejected] While variations in resonant characteristics of a basin will alter seiche response, it was thought the current text was sufficient. No studies linking seiches to climate effects were identified.
115169	104	39	104	45	Relevant for here, or section 9.6.4.2., p. 107 lines 1-11. Wilmes et al (2017, already referenced), which shows predictions of global changes in ocean tides associated with geographically variable sea level changes associated with ice loss, highlighting that geographic vairability is important to consider, especially in Antarctica where sea level falls and marine ice sheets retreat. Hayden et al. (in review) on the impact of GIA and future ice loss on tides in the Hudson Bay also shows this, and in particular shows that sea level rise or fall can impact the resonant characteristics of the Hudson Strait. Hayden, A., Wilmes, S.B., Gomez, N., Green, J.A.M., Pan, L., Han, H.K., Golledge, N.R.. Multi-century impacts of ice sheet retreat on sea level and ocean tides in Hudson Bay. J. Geophys. Res. Oceans (in review since 2019). [Natalya Gomez, Canada]	[taken into account] This section has been modified, with SROCC as starting point.
34497	104	42	104	43	"measured M2 changes" should be replaced by "measured lunar semidiurnal tide changes". [Claire Waelbroeck, France]	[not applicable] This text no longer appears in the revised version.
52529	104	44	104	44	there have been several studies suggesting that changes in stratification have contributed to tidal changes. [Joanne Williams, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] Refence is made to stratification driving tidal changes, citing the Haigh et al. review.
14603	104	47	104	47	What do you refer to as ESL here? Extreme SWL or extreme TWL? [Roshanka Ranasinghe, Netherlands]	[taken into account] The definitions of ESL have been clarified in Box 9.1. In this instance, ESL (as a catch-all term for all sub definitions) is relevant. This paragraph relates to changes in storm surges, which is relevant to all definitions.
34945	104	47	104	55	The recent US East Coast flooding (and apparent sea level rise) is predominantly due to sinking land mass, not climate change, see: https://tidesandcurrents.noaa.gov/sltrends/sltrends.html, and general comment #6. [Jim O'Brien, Ireland]	[taken into account] In the opening paragraph of Section 9.6.4, comment is made on the basis of AR5 and SROCC to the importance of geomorphological processes, but that this section focuses on oceanographic drivers of change.
15559	104	47	105	5	Suggest including the reference below (Oey and Chou, 2016) which used 64 years (1950–2013) of observations and model simulations, and found evidence of a significant rise in the intensity as well as poleward-shifting of location of typhoon storm surges in the western North Pacific after 1980s: Oey, L.Y, and S. Chou, 2016: Evidence of rising and poleward shift of storm surge in western North Pacific in recent decades. J. Geophys. Res., 121, 5181-5192, doi: 10.1002/2016JC011777 [SAI MING LEE, China]	[accepted] The Oey and Chou 2016 reference has been added as representative of a broader pattern reported in the Western North Pacific.
14607	104	47	105	12	Several occurrences of the use of the aconym ESL to refer to extreme storm surge height. This is even more confusing. Suggest that Ch 9 makes very clear and distint definitions and associated acronyms for extreme TWL, extreme SWL and extreme SS. [Roshanka Ranasinghe, Netherlands]	[taken into account] The definitions of ESL have been clarified in Box 9.1. In this instance, ESL (as a catch-all term for all sub definitions) is relevant. This paragraph relates to changes in storm surges, which is relevant to all definitions.
116909	104				please build on SROCC, and be explicit on what additional evidence affects findings. Check coherency with other chapters eg chapter 11 on cyclones so that this section is integrated within the AR6 WGI assessment. Summary conclusions with levels of confidence. [Valerie Masson-Delmotte, France]	[taken into account] The section has been substantially rewritten with aim of consistency with other chapters.
33079	105	1	105	1	generally, persian gulf and oman sea also should be included in consideration. [Sahar Tajbakhsh Mosalman, Iran]	[rejected] No relevant literature. There is no relevant literature on historical storm surges in the Persian Gulf. Lin and Emanuel (2016) https://doi.org/10.1038/nclimate2777 provide 21st Century projections of TC driven surges in the Persian Gulf, but note there have been no historical occurrences. No historical change data, relevant to this section, is available.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32749	105	1	105	1	generally, persian gulf and oman sea also should be included in consideration. [sadegh zeyaeyan, Iran]	[rejected] No relevant literature. There is no relevant literature on historical storm surges in the Persian Gulf. Lin and Emanuel (2016) https://doi.org/10.1038/nclimate2777 provide 21st Century projections of TC driven surges in the Persian Gulf, but note there have been no historical occurrences. No historical change data, relevant to this section, is available.
19451	105	1	105	1	Generally, Persian Gulf and Oman Sea also should be included in consideration [Mostafa Jafari, Iran]	[rejected] No relevant literature. There is no relevant literature on historical storm surges in the Persian Gulf. Lin and Emanuel (2016) https://doi.org/10.1038/nclimate2777 provide 21st Century projections of TC driven surges in the Persian Gulf, but note there have been no historical occurrences. No historical change data, relevant to this section, is available.
22675	105	2	105	12	Chapter 2 has a longer and more involved assessment of paleo-tempestology primarily from storm surge proxies and this should be reconciled with that text. The CA was Lisa Orme who may be able to help here. [Peter Thorne, Ireland]	[taken into account] On consultation with Peter Thorne, he has directed me to section 2.3.1.4.3 Extratropical jets, storm tracks, and blocking, which discusses some paleo record of atmospheric drivers. Reviewing references contained therein, I have been unable to find mention of storm surge proxies. No changes.
62249	105	5	105	5	could you please delete the double (()) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] Editorial
62251	105	11	105	11	could you please delete the double (()) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] Editorial
78045	105	13	105	17	In view of this statement, which is consistent with the AR5 and oher papers, RSL is the most important determinant of local ESL change i.e. more important than tides, surges and wave. I think that suggests a need for discussion of RSL projections and their implications in this section. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] Projections of RSL are contained in Section 9.6.3, prior to discussion of projections of ESL in 9.6.4.2. Here (in S 9.6.4.1), this comment seems misplaced.
62253	105	15	105	15	Could you please include a mark of the location where you show the annual mean sea levels? Include names of the plots (a, b, c, …) to refer in the plot description. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] Figures updated.
52533	105	15	105	15	Really, in assessing trends in extremes, it would be better to remove the tide and handle it separately. Possibly also the local relative mean sea-level. There is no mention here of using skew surge as a measure for extreme water levels, which separates the astronomical tide from the storm component, much more effectively than non-tidal residual surge. It might also be helpful to mention the joint-probabilty method of assessing flood risk, although perhaps this belongs elsewhere. [Joanne Williams, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] Clarification has been added to the Reference to Mawdsley and Haigh (2016), where the assessment of global historical changes in storm surge is based on analysis of the skew surge (tide and RSL removed).
80677	105	17	105	17	if this is the definition for minor extreme sea levels, what is the definition for major extreme sea levels? [Helene Jacot Des Combes, Marshall Islands]	[rejected] The definition of minor extreme sea-levels is given here as relevant. Major extreme sea-levels not referred to.
62255	105	17	105	21	Include the right names of the plots (a, b, c, …) to refer in the plot description. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] Figures updated with labels.
27395	105	19	105	19	Please replace (e-e) with (b) for better understanding [Eric Brun, France]	[accepted] Figures updated with labels.
55137	105	26	105	26	“ESL at the coast is also affected by surface wind-waves via wave setup” (should be wave runup, including swash too, as specified in ESLs description of BOX 9.1) [Nancy Hamzawi, Canada]	[accepted] The Dodet et al reference is added. This also makes contribution of swash more explicit.
27397	105	26	105	27	The review paper by Dodet et al. 2019 could be cited here: "The contribution of wind-generated waves to coastal sea-level changes". doi: 10.1007/s10712-019-09557-5 Please note that swash is also important for coastal impacts, e.g. through overtopping of defences. It is mentioned in box 9.1 and fig 9.1 but not discussed otherwise. [Eric Brun, France]	[accepted] The Dodet et al reference is added. This also makes contribution of swash more explicit.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82829	105	26	105	27	"[…] ESL at the coast is also affected by surface wind-waves via wave setup" This topic is thoroughly investigated by Dodet et al. 2019. This study could be cited at the end of the statement for completeness. Dodet, G., Melet, A., Ardhuin, F., Bertin, X., Idier, D., Almar, R., 2019. The Contribution of Wind-Generated Waves to Coastal Sea-Level Changes. Surv Geophys. https://doi.org/10.1007/s10712-019-09557-5 [Guillaume Dodet, France]	[accepted] The Dodet et al reference is added. This also makes contribution of swash more explicit.
77553	105	28	105	28	Should modify to "buoy type/technology" [Emer Griffin, Ireland]	[taken into account] This comment is addressed in the rewording of this section.
82831	105	28	105	29	"A number of studies indicate that changes in buoy have resulted in inhomogeneous datasets generally unsuitable for long term trend analysis (e.g., Gemmrich et al., 2011)." The term "changes in buoy" could be further detailed : e.g. "changes in buoy's hull, sensor and processing unit". [Guillaume Dodet, France]	[taken into account] The reference Centurioni et al. 2019 has been added, detailing sources of bias in the buoy data.
82833	105	28	105	29	"A number of studies indicate that changes in buoy have resulted in inhomogeneous datasets generally unsuitable for long term trend analysis (e.g., Gemmrich et al., 2011)." To my knowledge, only very few studies provide evidence of such inhomogeneities. Therefore, "a number of studies" should be replaced by "few studies" or additional references should be provided. [Guillaume Dodet, France]	[taken into account] The section of text has been reworded, using Gemmrich et al reference as an example of inhomogeneity issues which have held up use of buoy records for long term trend analysis
14609	105	31	105	31	Melet et al 2018 is not the best reference here as that analysis includes the oscillatory swash related component of wave runup. Perhaps replace with Vousdoukas et al 2018 (Nature communications) and Kircezi et al (submitted) that consider only the wave setup (in addition tp RSLR, tide and surge). Please contact Ch 12 CLAs if needed. [Roshanka Ranasinghe, Netherlands]	[rejected] The Vousdoukas et al. 2018 and Kircezi et al 2020 papers relate to projected changes in ETWL, which are captured in 9.6.4.2. Here, we are discussing historical changes in ECWL, for which Melet is an appropriate reference.
82835	105	31	105	33	"[…] the drawback is the inhomogeneity among reanalysis and hindcast wave products associated with the temporal changes in assimilated data" This topic is thoroughly investigated by Stopa et al. 2019 and could be cited here. Stopa, J.E., Ardhuin, F., Stutzmann, E., Lecocq, T., 2019. Sea State Trends and Variability: Consistency Between Models, Altimeters, Buoys, and Seismic Data (1979–2016). J. Geophys. Res. Oceans 124, 3923–3940. https://doi.org/10.1029/2018JC014607 [Guillaume Dodet, France]	[accepted] The Stopa et al. 2019 reference is added,
88001	105	31			in addition to the the inhomogeneity among reanalysis and hindcast wave products associated with the temporal changes in assimilated data, should also note the limitations of the assumptions used on shoreline slope to estimate wave setup in these studies. see for example Aucan, J., Hoeke, R.K., Storlazzi, C.D., Stopa, J., Wandres, M. and Lowe, R., 2019. Waves do not contribute to global sea-level rise. Nature Climate Change, 9(1): 2-2. [Kathleen McInnes, Australia]	[rejected] The Aucan et al. 2019 reference does not add to the discussion already contained in the original Melet et al. 2019 paper cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82837	105	35	105	39	“The satellite altimeter-derived wave record now spans over 33 years (1985 to present), and shows small increases in mean wave height over 1985-2018, with stronger increases in extreme wave heights (90 th percentile), and strongest increases in the Southern Ocean (Young and Ribal, 2019). Recent results on wave height trends based on a new altimeter product are missing here. Here is a proposition to insert these restuls in this paragraph. “However, several limitations in altimeter products question the robustness of these observed trends. First, altimeter undersampling combined with the changing number of in-orbit satellites over the period 1985-2020 can lead to very large overestimations of long-term trends of these extreme values over this period (Jiang et al. 2020). Second, the different processing techniques used for data editing and mission inter-calibration in various altimeter products (e.g., Ribal and Young, 2019; Dodet et al., 2020) result in significant differences of the long-term trends of the monthly mean significant wave height, particularly in the tropical regions (Timmermans et al. 2020). Dodet, G., Piolle, J.-F., Quilfen, Y., Abdalla, S., Accensi, M., Ardhuin, F., Ash, E., Bidlot, J.-R., Gommenginger, C., Marechal, G., Passaro, M., Quartly, G., Stopa, J., Timmermans, B., Young, I., Cipollini, P., Donlon, C., 2020. The Sea State CCI dataset v1: towards a Sea State Climate Data Record based on satellite observations. Earth System Science Data Discussions 1–28. https://doi.org/10.5194/essd-2019-253 Jiang, H., 2020. Evaluation of altimeter undersampling in estimating global wind and wave climate using virtual observation. Remote Sensing of Environment 245, 111840. https://doi.org/10.1016/j.rse.2020.111840 Timmermans, B.W., Gommenginger, C.P., Dodet, G., Bidlot, J.-R., 2020. Global Wave Height Trends and Variability from New Multimission Satellite Altimeter Products, Reanalyses, and Wave Buoys. Geophysical Research Letters 47, e2019GL086880. https://doi.org/10.1029/2019GL086880 [Guillaume Dodet, France]	[taken into account] The Timmermans et al reference is given, as one presenting the uncertainty around observed remotely sensed trends in wave conditions.
82839	105	35	105	39	However, due to observational limitations, there is low confidence in trends in surface wind-wave conditions.” Given the previous comment, I suggest to replace this phrase by: “Due to these observational limitations, there is low confidence in trends in surface wind-wave conditions.” [Guillaume Dodet, France]	[taken into account] The associated text has been reworded.
55139	105	36	105	39	It should be mentioned that there are also uncertainties among satellite products (Timmermans et al 2020) Timmermans, B.W., Gommenginer, C.P., Dodet G., and Bidlot, J-R, “Global wave height trends and variability from new multimission satellite altimeter products, reanalysis, and wave buoys”, Geophysical Research Letters, https://doi.org/10.1029/2019GL086880. [Nancy Hamzawi, Canada]	[taken into account] The Timmermans et al reference is given, as one presenting the uncertainty around observed remotely sensed trends in wave conditions.
62257	105	38	105	39	Limitation in wind-wave conditions. There is a wind range of this confidence. In this case we can compare with average or local wind wave conditions to know the places more prone to have accurate values. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] The reviewer asks to consider changes in marine winds to comment on reliability of reported trends in wave conditions. Both marine winds and waves suffer from same data limitations. The cited papers explore trends in satellite derived winds and waves. Resolving consistency between the two is not straightforward, and not yet attempted. Requires representation of spectral distribution of the wave field, accompanied by winds. For reasons identified in section, this is not addressed.
35825	105	43	105	44	Sentence makes no sense to me. [Michael Oppenheimer, United States of America]	[accepted] The sentence as been reworded to make sense.
26421	105	48	105	48	Check: (e.g.(Mori et al., 2014)(Takayabu et al., 2015)). [María Santolaria-Otín, France]	[accepted] Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31617	105	48	105	48	I am not sure to understand this statement on the lack of litterature: for example, two references cited above support the statement that mean SLR is the main (although not the only) driver of extreme SLR (Wahl and Chamber 2017; Marcos et al 2015). Many other papers support this statement including Menéndez and Woodworth (2010), Woodworth et al (2011), Rohmer and Le Cozannet (2019), Wahl and Chambers (2017, Marcos and Woodworth (2017) Mawdley and Haigh (2016) and references therein. So may be the statement on the lack of litterature needs to be reconsidered, or the sentence needs to be clarified (?). Mawdsley R J and Haigh ID2016 Spatial and temporal variability and long-term trends in skew surges globally Frontiers Mar.Sci. 3 29 Menéndez and Woodworth 2010 Changes in extreme high water levels based on a quasi-global tide-gauge dataset J. Geophys. Res. 115 C10011 Woodworth P L, MenéndezMand GehrelsWR 2011 Evidence for century-timescale acceleration in mean sea levels and for recent changes in extreme sea levels Surv. Geophys. 32 603–18 Rohmer, J., & Le Cozannet, G. (2019). Dominance of the mean sea level in the high-percentile sea levels time evolution with respect to large-scale climate variability: A Bayesian statistical approach. Environmental Research Letters, 14(1), 014008 Wahl T and ChambersDP 2016 Climate controls multidecadal variability in US extreme sea level records J. Geophys. Res. Oceans 121 1274–90 Marcos and Woodworth 2017 Spatio-temporal changes inextreme sea levels along the coasts of the North Atlantic andthe Gulf of Mexico J. Geophys. Res. Oceans 122 7031–48 [Gonéri Le Cozannet, France]	[taken into account] This paragraph has been extensively reworded, citing the AR5 (Ch13) and SROCC (Ch4), which capture well this comment.
5561	105	48			There exist study about the relationships between the storm surges and the atmospheric circulation in the English Channel : Turki I., Massei N., Laignel B., 2019. Linking sea level dynamic and exceptional events to large scale atmospheric circulation variability : A case of the Seine Bay, France. Oceanologia, 203, 1-11, doi.org/10.1016/j.oceano.2019.01.003. [Benoit Laignel, France]	[accepted] Turki et al. 2019 reference is added, as another example of attribution of climate effects in storm surge changes.
33517	105	48			Change: "…(e.g.(Mori et al., 2014)(Takayabu et al., 2015)). » by « (e.g.(Mori et al., 2014) (Takayabu et al., 2015)). ». [Guiomar Rotllant, Spain]	[accepted] Editorial.
22677	105	49	105	49	robust attribution rather than attributability (which I'm not even sure is a word)? [Peter Thorne, Ireland]	[accepted] Editorial.
14611	106	1	108	21	Many occurrences of using the acronym ESL to refer to Extreme SWL and Extreme TWL interchangeably. This is in contrast with terminology used in Ch 12 where ESL is used only to refer to Extreme TWL. This needs to be made consistent between the two chapters. Please contact Ch 12 CLAs. [Roshanka Ranasinghe, Netherlands]	[taken into account] Following redefinition in Box 9.1, the use of ESL, ESWL, ETWL and ECWL has been revisited throughout the section, and appropriately used in each instance. This edit has been made in consultation with Ch 12 CLA.
24487	106	1	108	21	We need summary table of this subsection as similar to Table 9.7 in the previous section. It gives better understanding and is good preparing for future improvement of projections in AR7. [Nobuhito Mori, Japan]	[noted] While we agree with the sentiment of this comment, the localised nature of ESL contributing processes results in a very large range of potential contributions to ESL for each process. To present this range, along with indications of combined uncertainty could not be achieved without a large complex table. Space and the need for clarity ultimately precluded inclusion of such a table. As a consequence, this suggestion has been rejected.
72051	106	9		10	Should add the papers of Hunter (2010, 2012) and Hunter et al. (2013) here. [John Church, Australia]	[rejected] These papers are captured within the context of the statements of AR5 and SROCC.
78745	106	11	106	11	If the result depends on the chosen distribution, please specify what method was used to produce Fig 34 [Erwin Lambert, Netherlands]	[taken into account] Methodology and assumption of Gumbel distribution stated.
78743	106	12	106	12	The difference between still and total water levels was examined more extensively by Lambert et al (2020), doi:10.1088/1748-9326/ab8336 [Erwin Lambert, Netherlands]	[accepted] The lambert et al 2020 reference is added.
3329	106	13	106	13	Some more shameless self-promotion: in Frederikse et al. 2020: Antarctic Ice Sheet and emission scenario controls on 21st-century extreme sea-level changes the effects of scenario and AIS uncertainty on ESL's are quantified, which helps to find answers on questions like what's going to happen with ESLs regardless of the RCP and Antarctica? [Thomas Frederikse, United States of America]	[accepted] The section has been slightly reworded, and reference Frederikse et al. 2020 added. https://doi.org/10.1038/s41467-019-14049-6

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129587	106	13	106	13	In Frederikse et al. (2020: Antarctic Ice Sheet and emission scenario controls on 21st-century extreme sea-level changes), the effects of scenario and AIS uncertainty on ESLs are quantified, which helps to find answers on questions like what's going to happen with ESLs regardless of the RCP and Antarctica? [Trigg Talley, United States of America]	[accepted] The Frederikse et al 2020 reference has been added, in comment on the uncertainty surrounding RSL projections.
129585	106	13	106	17	This is an ill-defined argument or logic. Mathematically it is not correct. The prob 0.01 exceedance for multiple times is not equal to the 100-1000th increase in ESL frequency stated in the passage. [Trigg Talley, United States of America]	[rejected] This comment seems to be a misinterpretation of the sentence (1% annual probability being equivalent to the 0.01 expected annual event).
66005	106	16	104	18	Suggest clarification. None of the references cited here discuss flooding due to tides. They only discuss extremes in still water level (i.e. tide + surge + trend). See also line 45 page 7. Suggest reviewing two papers where the role of tides are explicitly considered in an impact-based framework (and hence results can be used to refer to "flooding"). Neither of these papers are global. See: - Sweet et al. (2018) - https://tidesandcurrents.noaa.gov/publications/techrpt86_PaP_of_HTFlooding.pdf - Ray and Foster (2016) - https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016EF000423 Linking statistical metrics of extreme sea levels to their impacts at the coast is an area where research is required and ongoing, especially outside the US (which is covered well by Sweet et al. 2018). We cannot find the figure of 175% increase in either of the three papers cited in the report. As such, if it is not a global figure and/or does not relate to defined flood thresholds, then this should be stated explicitly. Or, the figure should be removed from the report. Suggest reviewing a recent study on coastal flooding frequency changes (but not considering explicitly the role of tides) from Australia: - http://www.bom.gov.au/jshess/docs/2019/Hague2_early.pdf. A similar report from NZ is at: - https://www.pce.parliament.nz/media/1382/the-effect-of-sea-level-rise-on-the-frequency-of-extreme-sea-levels-in-new-zealand-niwa-2015.pdf [Kushla Munro, Australia]	[taken into account] I dispute that the cited references do not capture flooding due to tides. The amplification factor implicitly includes flooding by tides, along with surge and trend. The papers to which the comment refers are centred around increased tidal flooding associated with RSL (amplification) as presented. As a consequence, it's felt that this comment is well addressed already. It's unclear what the commenter is referring re 175%. Perhaps this comment is misplaced.
22679	106	17	106	20	This statement needs supporting literature citations otherwise there is an insufficient trace to justify the use of confidence language in this manner? [Peter Thorne, Ireland]	[accepted] The confidence statement has been removed. It was thought confidence was overstated, particularly given the projected changes are based on assumption of stationary distributions of other ESL contributors.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31619	106	18	106	20	In these types of tropical environment with few storms and cyclones, the "amplification factor" that has been used in previous IPCC reports and shown here in Fig 9,34 is a little bit difficult to interpret, and the approach consisting in evaluating the number of flood days (chronic or nuisance flood event) might be prefered (see Sweet and Park, 2014 and papers below). For example, frequency amplification factors based on tide gauge records only are overestimates because tide gauge measure sea-levels with no or less wave setup than adjacent beaches (Lambert et al 2020). Furthermore, it is difficult to speak about amplification of events that barely exist today. However, it is clear that chronic flooding will be a critical impact in such tropical islands with few cyclones and storms. These chronic flood events are overlooked in tropical islands whereas they is much more considered in the US (Ezer and Atkinson, 2014;Sweet and Park, 2014;Moftakhari et al., 2015;Moftakhari et al., 2017;Dahl et al., 2017). Dahl, K. A., Fitzpatrick, M. F., and Spanger-Siegfried, E.: Sea level rise drives increased tidal flooding frequency at tide gauges along the US East and Gulf Coasts: Projections for 2030 and 2045, Plos One, 12, 10.1371/journal.pone.0170949, 2017. Ezer, T., and Atkinson, L. P.: Accelerated flooding along the US East Coast: On the impact of sea-level rise, tides, storms, the Gulf Stream, and the North Atlantic Oscillations, Earths Future, 2, 362-382, 10.1002/2014ef000252, 2014. Lambert, E., Rohmer, J., Le Cozannet, G., & van de Wal, R. S. W. (2020). Adaptation time to magnified flood hazards underestimated when derived from tide gauge records. Environmental Research Letters Moftakhari, H. R., AghaKouchak, A., Sanders, B. F., Feldman, D. L., Sweet, W., Matthew, R. A., and Luke, A.: Increased nuisance flooding along the coasts of the United States due to sea level rise: Past and future, Geophysical Research Letters, 42, 9846-9852, 10.1002/2015gl066072, 2015. Moftakhari, H. R., AghaKouchak, A., Sanders, B. F., and Matthew, R. A.: Cumulative hazard: The case of nuisance flooding, Earths Future, 5, 214-223, 10.1002/2016ef000494, 2017. Sweet, W. V., and Park, J.: From the extreme to the mean: Acceleration and tipping points of coastal inundation from sea level rise, Earths Future, 2, 579-600, 10.1002/2014ef000272, 2014. [Gonéri Le Cozannet, France]	[taken into account] This comment expresses concern around presentation of projected changes in ESL on back of amplification factors, being unable to capture effects of now rare events. The rewording of this paragraph, and addition of the Lambert et al 2020 and Sweet and Park references aim to encompass the sentiment of this comment, but the section remains committed to presenting results via amplification factors.
14613	106	22	106	33	The projections provided here are based on what kind of historical ESL distribution (ref pg 106, lines 10-12)? [Roshanka Ranasinghe, Netherlands]	[taken into account] Assumed distribution shape is now specified.
85325	106	22	106	33	These findings on huge proprtional changes to extreme sea level risks are obviously really important to clearly get across to policy makers? Am I right to assume that they are sumamrised sufficiently in the chapter summary and perhaps also the Summary for Policymakers? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] Executive Summary has been edited, capturing details.
83845	106	26	106	26	will have an increased to an 11-14%' should this read 'will have increased to an 11-14%' [Mark Pickering, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] Editorial.
52525	106	28	106	28	by 2050 compared to what period? 1995-2014? Or does it depend on which gauge, as not all will have measurements 1995-2014? Is there an assumption of no change between the gauge measurements and 1995-2014? Also what is the reference for line 22-28, this is not in Oppenheimer 2019. [Joanne Williams, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] The historical period is clarified. The reference to Oppenheimer has been moved to the general statement around rare events becoming increasingly common.
129589	106	31	106	33	This quantitative statement for the high confidence inference is interesting and useful. Provide a reference(s) or details of the analysis. [Trigg Talley, United States of America]	[taken into account] Have removed the quantitative confidence statement, as feel it is overstated, given analysis, and broad assumptions of stationary contributions from other processes.
85327	106	35	108	21	Is it worth including a paragraph on some of the challenges of providing projections of changes to the time varying aspects of near-coastal sea-level and river-level contibutions to surges? For example, issues related to the representation in low resolution coupled climate models of historical and future changes to tropical and extra-tropical cyclone tracks, intensities and occurrences; the sensitivity to ocean model resolution and representation of very fine details of near coastal topography and wind forcing of surge events in regional ocean models (including some aspects thereof which are amplified in shallow water); etc? It is worth noting that for many regions of the world, we do not even have adequate operational weather-related forecast systems for either storm-related surge events or even moreso for near coastal surge contributions from river flow (river level forecats and projections are further compounded by important human decisions through engineered controls on flows on most rivers) [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] A new paragraph has been added, outlining the many issues surrounding dynamical projections of these other contributions to ESL, and the associated uncertainties.
26423	106	37	106	37	Check parenthesis: (e.g., (Woth et al., 2006; Marcos et al., 2011)) [María Santolaria-Otín, France]	[accepted] Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78747	106	38	106	38	I believe the recent study by Muis et al (2020) should be mentioned and compared here. doi:10.3389/fmars.2020.00263 [Erwin Lambert, Netherlands]	[accepted] Two new references added here. Muis et al. 2020 and Melet et al. 2020.
35827	106	39	106	39	Mention the difficulty that hydrodynamic models have in reproducing TC surge. [Michael Oppenheimer, United States of America]	[taken into account] This issue of HD models being able to resolve TC driven surges is addressed through the added and following paragraphs.
2533	106	41	106	44	Grammatically very complicated sentence, split up in several sentences; also can the AF be expressed in the same way as the results in the previous paragraph [Tim Hermans, Netherlands]	[taken into account] Have attempted to simplify sentence, split up, and present results in consistent format to how expressed elsewhere.
35829	106	48	106	49	Confidence level on this statement is too high given the discussion on the next page about TC climatology projections and the fact that some models have shown intensification of TCs having an effect on ESL that is as large as RSL for high emissions. [Michael Oppenheimer, United States of America]	[accepted] The confidence level has been revised from high to medium.
20577	106	48	197	8	Page 106 lines 48-49 and Page 107 Lines 6-8 suggest that the results when using the "dynamic approach" have much in common with those obtained with the "static" approach. However, when considering the whole of the subsection, it is difficult to conclude that the more expansive dynamic approach is not worth the effort. [philippe waldteufel, France]	[taken into account] In the revision of the section, the uncertainties associated with the dynamical approach have been made more apparent. Also, that there are few studies that have presented projections of ETWL (or ECWL). Together, these uncertainties, and that results are dependent on single model configurations, we have maintained that the most robust result to present is the projected changes in ESL associated with RSL only (amplification), and recommend further dynamical studies be carried in order to express confidence in available results.
82973	106	51	106	51	I suggest to add also information in kilometers/kilometer ranges in addition to degrees (resolution of models). [Sebastian Gerland, Norway]	[accepted] Resolution (O(10km) added.
109245	106		107		Projected changes of frequency amplification factors for Extreme Water Levels (static approach) is shown in Figure 9.34. It may be useful to ad and additional plot on projected changes of Extreme Sea Level (dynamic approach) similar to Figure 9.34. A reference to Chapter 12 text and Figure 12.7 can be useful in this section which shows projections of extreme sea level (1:100 yr return period total water level). [A.K.M Saiful Islam, Bangladesh]	[taken into account] In section 9.6.4.2 we have strengthened the argument that ESL projections are presented on basis of RSL projections (via amplification factors) only, owing to the high uncertainties associated with dynamical projections of other contributors at the global scale. As such, we've not altered figures to be included in Chapter 9. In Chapter 12, dynamical projections of ETWL are presented on basis of a single approach (Vousdoukas et al., 2018). In 9.6.4.2 we point to associated uncertainties.
82975	107	9	107	9	I suggest adding a reference for the example regarding the East China Sea. [Sebastian Gerland, Norway]	[accepted] reference Song et al. 2013 added.
129591	107	15	107	22	This section correctly lists tropical cyclones as a major factor enhancing flood hazard (high confidence). However, this factor was not discussed as such in preceding sections. Besides tropical cyclones, other extreme events such as winter storms (e.g., N'easter in Atlantic coast) and typhoon in Pacific basin are also responsible and should be discussed. [Trigg Talley, United States of America]	[taken into account] Rephrasing has cited Chapter 11 observed trends in TCs and ETCs, and the consistency with observed changes in ESL.
88003	107	15			Also seen in McInnes, K.L., Hoeke, R.K., Walsh, K.J.E., O'Grady, J.G. and Hubbert, G.D. 2016: Tropical Cyclone Storm Tide Assessment for Samoa. Natural Hazards. 80: 425–444. DOI 10.1007/s11069-015-1975-4 [Kathleen McInnes, Australia]	[accepted] McInnes et al. 2016 reference added.
99541	107	17	107	18	Result in sentence is given as statement of fact, please rephrase with a range and likelihood assessment. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	[accepted] The sentence has been rephrased to express as finding from published work.
62259	107	24	107	25	Reference to chapter 12 about coastal morphology. Could you please point atleast the section or main topic to use as a example to explain the idea?. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[taken into account] The reference to Chapter 12 has been removed.
129593	107	24	107	34	This review and synthesis on "mean wave height" and "extreme wave height" appear to be incomplete and lack understanding of current research in this field. The terminology "astronomical tide" and "storm surge" should be used. Observations and projections in future climate have been long documented in literature, for which "very high confidence" and "high confidence" investigations are available in the literature. [Trigg Talley, United States of America]	[taken into account] This comment appears to confuse wind-waves with long wave (tide or surge) contributions. The paragraph has been reworded to ensure that it is clear wind-waves are being referred to.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55141	107	24	107	45	Changes in the Arctic waves are not mentioned at all. Arctic waves are affected by additional drivers and their projections exhibit increased robustness compared to other areas. After line 45, for example, add comment about changes in Arctic region: In the Arctic Ocean, changes in ocean surface wave conditions are also affected by sea ice retreat (e.g. Casas-Prat and Wang, 2020b ; Thomsons and Rogers, 2014). Some ice-driven factors contributing to wave height increase: (1) increase of fetch (distance over which wind can blow over water), (2) water areas becoming exposed to fall (more energetic) storms as ice-free season lengthens, and (3) increase of probability of strong winds to occur in widening ice-free areas (e.g. Casas-Prat and Wang, 2020b; Liu et al 2016; Thomsons and Rogers, 2014;). Existing studies (Casas-Prat and Wang, 2020a; Khon et al 2014) agree with a notable and statistically significant projected increase in the extreme wave height along the Arctic coastlines (medium confidence due to limited number of models analyzed). However, although there is high confidence of the positive increase in extreme wave heights, the exact estimate of such increase remains uncertain due to the small set of models considered in the studies available (sea ice retreat is robust in the whole Arctic basin but there are uncertainties at regional scale), and the complex feedback mechanisms between sea ice and waves (e.g. wave-induced ice-break up) which is still not properly resolved and is an area of active research under continuous development (Squire, 2020) Casas-Prat, M and Wang, X.L. (2020a), “Projections of extreme ocean waves in the Arctic and potential implications for coastal inundation and erosion”, Journal of Geophysical Research: Oceans, accepted. Casas-Prat, M. and Wang, X.L. (2020b), “Sea-ice retreat contributes to projected increases in extreme Arctic ocean surface waves”, Geophysical Research Letters, https://doi.org/10.1029/2020GL088100. Liu G, Babanin A V, Zieger S, Young IR, and Guan C (2016) Wind and wave climate in the Arctic Ocean as observed by altimeters., Journal of climate, 29, 7957-7975, https://doi.org/10.1175/JCLI-D-16-019.1 Thomson J and Rogers, W. E (2014). Swell and sea in the emerging Arctic Ocean. Geophysical Research Letters, 41, 3136-3140, https://doi.org/10.1002/2014GLO59983	[taken into account] The section has been rephrased so that recognition of melting ice, and associated variations in fetch conditions , on wave climate are noted, with references added. A reference to the influence of sea-ice on historical wave variations from SROCC has been provided. No recent literature has updated SROCC.
129595	107	28	107	28	What is "wave climate projection"? This appears to be unusual terminology. [Trigg Talley, United States of America]	[taken into account] The phrasing has been reworded to indicate we are expressing projected changes in climatology of wind-waves.
14615	107	36	107	39	What is relevant for ESLs is changes in extreme waves during storms, not those in mean waves. Therefore lines 36-39 are irrelevent for the ESL discussion here. A new paper by Meucci et al that provides projections in extreme wave heights is now in press at Sciene Advances (Please contact Ch 12 CLAs). [Roshanka Ranasinghe, Netherlands]	[accepted] The reference to Meucci et al 2020 has been added, with acknowledgement of high uncertainties around single model configurations.
129597	107	40	107	41	The attribution to model inability as the result of uncertainties in local-scale geography (bathymetry and coastal orientation) is inaccurate. There is a somewhat confined uncertainty in projection of future bathymetry for a given SLR. Uncertainties in storm surge (extreme water height) largely comes from the pressure field and hurricane tracks. See Liang et al. (submitted) and others already in literature. [Trigg Talley, United States of America]	[taken into account] referring to storm surge uncertainties. This aligns with reviews that seem to have misinterpreted a distinction between wind-waves and storm surges.
105985	108	6	108	7	This and other compound events should refer to 10.5.2.4 and, especially, 11.8 for more detailed discussion. [William Gutowski, United States of America]	[accepted] Editorial - added reference to Ch 10 and Ch11 sections.
88005	108	6	108	13	Compound rainfall and storm surge have also been studied for the Australian coast, finding that there is strong dependence between storm surge and rainfall on the northern tropical coasts and indepence along the southern mid-latitude coast. Wu, W., K. L. McInnes, J O'Grady, R. K. Hoeke, M. Leonard and S. Westra, 2018 Mapping dependence between extreme rainfall and storm surge. JGR Oceans doi:10.1002/2017JC013472 [Kathleen McInnes, Australia]	[accepted] Reference Wu et al added.
62261	108	9	108	13	Include a short list of the main locations. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[accepted] Locations for each provided reference is given.
99543	108	14	108	15	Result in sentence is given as statement of fact, please add confidence assessment. [Richard Jones, United Kingdom (of Great Britain and Northern Ireland)]	[taken into account] The sentence has been softened from being a statement of fact, referring off to SROCC, and listed as likely effects.
5563	108	14	108	16	In the coastal zones, the flooding will increase by the combining of the sea level rise, storm surges and river flooding, but not only, because there are a combining also with tide (high tide) and wave. Moreover the sea level rise will block the flow of the river and lead to more flooding in estuaries, tidal rivers and all coastal rivers. [Benoit Laignel, France]	[taken into account] The section has been rephrased, taking into account the SROCC reference, which addresses these issues in greater depth.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129599	108	19	108	21	[CONFIDENCE] The designation of "low confidence" does not reflect the presence of many pertinent publications on this topic in the U.S. Atlantic basin. [Trigg Talley, United States of America]	[taken into account] While several publications on this topic are available in the US Atlantic basin, the expression of low confidence is on the basis of the complexity of compound flooding, and resolving to which extent rain and surge events will change. The assessment of low confidence remains.
41481	108	26			Please increase the size of individual figure panels. [Alexander Nauels, Germany]	[taken into account] Figure replotted with updated RSL info, with edits to panels.
3331	108	34	108	34	Is it an idea to explcictly warn in section 9.7 for the large and unaccounted effects of local subsidence in sea-level projections? As in: don't use the gridded projection for your local situation without assessing local factors that could cause a major deviation from the gridded projection. [Thomas Frederikse, United States of America]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections
129601	108	34	108	34	Is it an idea to explicitly warn in Section 9.7 for the large and unaccounted effects of local subsidence in sea-level projections? As in: Don't use the gridded projection for your local situation without assessing local factors that could cause a major deviation from the gridded projection. [Trigg Talley, United States of America]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections
130617	108	34	110	31	Please note that in this section, there is not much "Robustness" information on Ocean. [Panmao Zhai, China]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections
34947	108	34	110	31	It is welcome that the SOD evaluates the robustness or otherwise of the assessment of the ocean and cryosphere dynamics and the uncertainties therein, which should be reflected in an appropriately reduced level of confidence in the conclusions. See general comment #15 above. [Jim O'Brien, Ireland]	[noted] confidence and likelihood statements are designed to reflect the level of understanding and therefore already include this information
24489	108	34	110	31	Although the section 9.7 discusses robustness and limitation of the assessment, it does not follow all key process dealing with Chapter 9. For example, there is no description of marine heatwaves and extreme sea levels. It is better to follow the contents and order in this chapter from 9.2 to 9.6 in this section. [Nobuhito Mori, Japan]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections
88007	108	34	110	31	Could also discuss limitations on modelling to assess extreme sea level changes including waves such as limitations of the GCMs to represent severe weather systems such as TCs in global storm surge studies and also resolution issues in the representation of near coastal bathymetry which is key to simulating nearshore wave processes such as wave setup or wave runup [Kathleen McInnes, Australia]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections
78037	108	34	110	31	Sect 9.7. I am sorry to say that I don't find this section helpful. The robustness and limitations of the assessment should be made clear in the discussions which lead up to it, and in the confidence attributed to it. Describing them separately leads to repetition, and I don't see why it is useful. It isn't the role of the IPCC to set out a research agenda. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	[noted] the setup of this section has changed an now points out where there has been most progress and where there are still major uncertainties.
259	108	36	110	31	Why isn't there any discussion of uncertainties in precip over the ice sheets? Snow over Antarctica may be increasing etc. [THOMAS Wagner, United States of America]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections
85329	108	36	110	32	I really liked this section, which I thought clearly emphasized the unceratinties in a way that did not come over to me clearly in some of the sections in the chapter, particuarly the use of the very clear term 'deep uncertainty'? I am wondering if it might even be appropriate to use the 'deep uncertainties' term to qualify other aspects of this chapter, for example for aspects of the Southern Ocean projected changes and their important contributions to global scale climate change projections? I also wonder if this section would be better placed right at the start of the chapter as a initial qualification which sets the context for the details that follow? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections
85331	108	36	110	32	I really liked this section, which I thought clearly emphasized the unceratinties in a way that did not come over to me clearly in some of the sections in the chapter, particuarly the use of the very clear term 'deep uncertainty'? I am wondering if it might even be appropriate to use the 'deep uncertainties' term to qualify other aspects of this chapter, for example for aspects of the Southern Ocean projected changes and their important contributions to global scale climate change projections? I also wonder if this section would be better placed right at the start of the chapter as a initial qualification which sets the context for the details that follow? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	[noted] the setup of this section has changed, and robustness and limitations are now in detail discussed in the relevant sections. The use of 'deep uncertainty' is in line with the use throughout the chapter.
27399	108	42	108	42	It would be useful to recall the meaning of GrIS and AIS. [Eric Brun, France]	[accepted] AIS and GrIs no longer used here

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101921	108	42	108	42	We suggest to repeat here (for readers only reading the robustness-section) what GrIS and AIS stand for. [IAPSO ECS group review, United States of America]	[accepted] AIS and GrIs no longer used here
62263	108	48	108	48	I don't be sure if in a previous chapters was defined but, what is "strong warming"? There is a range? In that way the public will get this values in mind. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[not applicable] text removed
9029	108	48	108	50	No modeling of undercutting in Greenland despite this process controlling half of the mass ablation at glacier fronts … [Eric Rignot, United States of America]	[not applicable] text removed
30657	108	51	108	51	This should be marine ice sheet instability. Shelf instability is never used in the text. [Frank Pattyn, Belgium]	[not applicable] text removed
39785	108	51			"marine ice shelf instability" should be "marine ice SHEET instability" [TSU WGI, France]	[not applicable] text removed
83847	108	54	108	54	projected losses that those' should this read 'projected loses than those'? [Mark Pickering, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] text removed
257	108	55	109	2	This sentence is hard to follow. "The importance of these processes in the past could provide physically…" I think it's trying to say that these processes could have driven the massive ice loss that we see in the paleo record. It's an important point. [THOMAS Wagner, United States of America]	[not applicable] text removed
35831	109	2	109	3	This sentence raises an interesting issue. Can we really have much confidence in the narrowed uncertainty in total SLR in this chapter compared to SROCC while the underlying uncertainty in ice sheet behavior remains? What is the message we want to transmit on uncertainty? When placed in the context of the expert elication and the various integrated (probabilistic approaches), it would make sense to highlight that the likely range still reflects largely what the mechanistic model world produces (seasoned with some expert judgment) and that policy makers should perhaps be open to considering a wider range - a statement similar to one found in SROCC SPM. [Michael Oppenheimer, United States of America]	[noted] in contrast to previous assessments, this chapter provides sea-level projections based on processes in which we have medium confidence and based on processes in which we have low confidence.
62265	109	7	109	7	Is there another manuscript already published that you can use as a reference? Submitted is not enough for this AR6, there is no peer-review validation yet of this information. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] references are provided in section 9.5.1 - all assessments are based on accepted manuscripts
110549	109	9	109	9	The word 'small' in 'small regional scales' is superfluous. [Keven Roy, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] text removed
62267	109	12	109	16	Zalazar et al, and Zorzut et al., both manuscripts just submitted. As I already mentioned, we need some validation or at least a peer-review of that information to get the credibility. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] references are provided in section 9.5.1 - all assessments are based on accepted manuscripts
33519	109	18			Add dot at the end of the sentence : « … projections (RGI Consortium, 2017b; WGMS, 2017; GlaThiDa Consortium, 2019; Millan et al., 2019a)". [Guiomar Rotllant, Spain]	[not applicable] text removed
97045	109	20	109	20	What is meant by 'limited discovery'? [Nicole Wilke, Germany]	[not applicable] text removed
82971	109	20	109	20	I suggest rewording the second part of this sentence; I wonder if the intention is to express that the assessment of the Southern Ocean is difficult because there are only few datasets. [Sebastian Gerland, Norway]	[not applicable] text removed
16433	109	20	109	20	"...limited discovery" of what, be more specific. [Julian Mak, China]	[not applicable] text removed
22681	109	20	109	20	Discovery isn't the right term here in my view. Perhaps something like the ability to undertake sustained monitoring activities? [Peter Thorne, Ireland]	[not applicable] text removed
16435	109	23	109	23	Consider "...significant reservoir FOR heat and carbon UPTAKE" [Julian Mak, China]	[not applicable] text removed
80679	109	33	109	33	it should be 'due to any' [Helene Jacot Des Combes, Marshall Islands]	[not applicable] text removed
16437	109	36	109	36	"Modelling uncertainties" could mean something different than what is intended here (e.g. in the statistics community or uncertainty quantification field). Consider insteda "Uncertainties associated with models, particularly process..." [Julian Mak, China]	[not applicable] text removed
62269	109	39	109	41	There is any range or estimation of emissions that can increase the probability to quantify the effects? In the way is writed seems under estimated and should be more relevant. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] text removed. This is quantified in the relevant section of the chapter (9.6.3.5).
35833	109	39	109	41	Here again, as in Box 9.3, MICI is dismissed with an incoherent and self-contradictory statement. Current evidence is insufficient to dismiss MICI out of hand. "The probability of occurrence is so low as to be inestimable" is ridciulous, given all the uncertianties - it's hard to estimate due to a lack of knowledge about whether it is small or large or negligible under particular assumed future conditions. We don't know yet. Dismissing it brings to mind the two decades of side-tracking the Weertman paper, something this community came to regret. In addition, using extremely likely here and in table 9.5 is unwise and inconsistent with the deep uncertainty that surrounds ice sheet response under high emission scenarios. [Michael Oppenheimer, United States of America]	[not applicable] text removed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7885	109	45	109	45	Not 'less stable' (the paper is not about stability) - maybe 'more sensitive to increased CO2'? Also the first part of the sentence suggests that this study (Jackson et al submitted - of which I'm the lead author) is about abrupt changes which it is not. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	[not applicable] text removed
16439	109	46	109	46	“do” unnecessary [Julian Mak, China]	[not applicable] text removed
16441	109	47	109	47	“rule” is a overly strong given there are obvious counter-examples, consider “trend” or “behaviour” [Julian Mak, China]	[not applicable] text removed
62271	109	47	110	2	Chassignet et al., and Jackson et al., manuscripts just submitted. We need at least the peer-review to confirm the previous statement. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] references are provided in section 9.2 - all assessments are based on accepted manuscripts
46531	109	50	110	2	As noted above, the work of Jeong et al. (2020; submitted 2019) could be mentioned here w.r.t. a CMIP6 model (E3SM) that represents improvement over CMIP5 models through it's ability to simulate circulation (and heat and freshwater exchange) within a fully coupled Earth system model (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). This applies to lines 50-53 on p. 109 and line 2 on p. 110. [Stephen Price, United States of America]	[noted] text removed. Detailed discussions with literature references may be found in section 9.2.
1777	109	54	110	4	How can we improve model resolution to better represent mesoscale, coastal and shallow-water processes that would improve understanding of climate change effects on estuarine and coastal marine ecosystems? It would be good to add some discussion about this at the top page 110. [Michael Kennish, United States of America]	[noted] the setup of this section has changed, and this kind of detail is now discussed in the relevant sections. The link to estuarine and marine ecosystems is beyond the scope of this working group and my be found in the WG2 contribution to AR6.
2535	109	54	110	7	References to post-AR5 studies on the influence of resolution of ocean models on coastal sea-level projections specifically are missing [Tim Hermans, Netherlands]	[noted] text removed. Detailed discussions with literature references may be found in section 9.2.
88343	110	6	110	6	to improve clarity you could refer to the representation of landscape change associated with changing permafrost conditions [Sharon Smith, Canada]	[noted] the setup of this section has changed, and this kind of detail is now discussed in the relevant section.
35835	110	9	110	28	Please check the claims about this chapter closing the sea level budget for the first time. I think I saw the same claim, perhaps for a different timeframe, asserted in one or more previous assessments. [Michael Oppenheimer, United States of America]	[not applicable] text removed.
27401	110	12	110	14	What does "sea level budget" mean ? Would not the term "contribution to sea level" be more relevant? [Eric Brun, France]	[rejected] the sea level budget is the comparison of the sum of the contributions to the total observed change
22683	110	12	110	14	This seems inconsistent with cross-chapter Box 9.2. Wouldn't it be better to say that while the sea level budget can be closed in the data rich modern era (x-chapter box 9.2) a paucity of available observations precludes such a closure for the 20th Century observed rises? Equally, unless we believe there are observations that may be recoverable this is iredeemable and it is therefore unclear what the value proposition of its inclusion might be. There are many things we can't do because we didn't observe e.g. surface air temperatures in the 1500s. Knowledge gaps should at least theoretically be able to be addressed without the invention of a time machine? [Peter Thorne, Ireland]	[not applicable] text removed.
68101	110	24	110	25	What do you mean by “projections have recent been shown to be quantitavely accurate and precise?” Under what assumptions? The only way to show accuracy would be to compare the projections against the actual observations when they occur in the future. And why use the word “precise”, useage here does not conform to scientific useage of that term unless the meaning is trivial. This statement needs to be made more scientifically precise and accurate. [Lev Tarasov, Canada]	[not applicable] text removed.
22685	110	24	110	31	This is not a knowledge gap so what is it doing in this section? [Peter Thorne, Ireland]	[noted] text rewritten
62273	110	30	110	30	Edwards et al., submitted. Is good to have fresh research involved but we need a minimum parameters to transmit confidence to the reders, and its about to use published manuscripts. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	[noted] all assessments in FGD are based on accepted manuscripts
41139	111	0			very Nice FAQ9.1, clear, interesting and nicely written [TSU WGI, France]	Noted. Thank you.
39203	111	1	114	12	FAQ 9.1 and FAQ 9.2 is very informative, and could provide an easy read for non-technical readers. [Lourdes Tibig, Philippines]	Noted. Easy read for non-technical audience is the goal.
42631	111	1	116	25	It is a very nice idea to have a FAQ, thanks for this! [Sofie Schöld, Sweden]	Noted. Thank you.
83329	111	3	111	52	What about sea ice? [Robert Massom, Australia]	Rejected. The FA Question itself specifies ice sheets and space is very limited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38813	111	3	111	52	Very useful FAQ! Would it be possible to also give readers an idea how large impacts or risks from those changes that are not reversible on human timescales are in comparison to those that might be reversible? What I am also missing is at least a hint that even changes that are reversible can have wide-ranging (and probably irreversible) effects for example on certain species or ecosystems. [Maike Nicolai, Germany]	Rejected. This level of detail is found within the chapter and WGII.
249	111	5	111	5	This should answer the question up front with "No, not on human time scales. [THOMAS Wagner, United States of America]	Rejected. The headline answer responds to the second question first, then the first.
38795	111	5	111	6	I am not a native speaker, but is "release of carbon stores" really correct? I assume that carbon is released, not the stores? Because at least one example is given, this could probably be rephrased as: "...the release of carbon from thawing permafrost and other natural sources into the atmosphere..." [Maike Nicolai, Germany]	Noted. Phrase removed.
129603	111	5	111	10	The summary conclusion to FAQ9.1 is justified. But the reference of time span (hundreds to thousand years) is only correct if the "distant past" refers to the studies of pre-historical climate. But in fact, the chapter and sections are mostly focused on paleoclimatic evidence (e.g., Quaternary climate). Then the reference for the time span is scientifically inaccurate. [Trigg Talley, United States of America]	Accepted. Last 800,000 years now specified for FAQ. Other epochs in chapter not relevant.
251	111	5	111	52	This whole answer is too long and should be rewritten in plain language because it's an FAQ; it's not a research paper. As well, it's just too convoluted and misleading in that processes like permafrost thaw results in erosion (imagine how much a coastline changes!) so it is not reversible. This section also jumps around in time so much that is challenging to follow for an expert, let alone a non-expert. I appreciate that care was taken to be comprehensive. See FAQ 9.2 for a good example. [THOMAS Wagner, United States of America]	Noted. Text simplified.
38797	111	7	111	10	I wonder if "adjust" might give a wrong impression in this context because the verb can be interpreted in several ways. It might be more neutral to speak of a "reaction". "Adjust" could also be perceived as a contradiction to the "return to their present state" mentioned in line 42. [Maike Nicolai, Germany]	Rejected. Adjustment to temperature change is the standard phrasing.
38799	111	13	111	13	What does "realise" mean in this context? Many (probably non-native) readers might at first think you mean "understood" and wonder by whom. Would "come into existence" or "unfold" be clearer? [Maike Nicolai, Germany]	Noted. Text removed.
22687	111	22	111	22	Rapid may mean very different things to different people. A lay person view may see this as minutes to hours to days. It would be better to specify the assymetry as accumulation being multi-millenial and decay being multi-centennial because most people views of rapid is not multi-centennial! [Peter Thorne, Ireland]	Noted. Timescales are made explicit.
21207	111	24	111	28	Note that in chapter 2 (p. 73) the role of volcanic emissions during or before the PETM is (over)emphasized. Should be conformable here. [Robert Speijer, Belgium]	Accepted. Text removed.
21205	111	25	111	25	55 Ma should be ~55.9 Ma for the onset of the PETM [Robert Speijer, Belgium]	Noted. Text removed.
2997	111	27	111	27	'... the Arctic and mountain regions ...' [Petteri Uotila, Finland]	Rejected. Mountain regions are not central to the ice sheet story here.
66419	111	27	111	28	In principle, FAQ 5.2 also briefly covers the clathrate question as well as the terrestrial permafrost question. [Charles Koven, United States of America]	Accepted. Text removed.
38801	111	28	111	29	Are you able to say how likely any of these events are, on human timescales or in the more distant future, in case carbon emissions are cut substantially compared to if we follow the path we are currently on? Right now, this reads like a very abstract threat. How serious is it? [Maike Nicolai, Germany]	Noted. A discussion of probabilities is not appropriate for the FAQ audience, but more importantly this discussion is carried out in depth within the main chapter and no simple probability relationship is found. See, for example, the high end sea level box 9.3.
62185	111	29	111	29	Permafrost thaws and does not melt [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text removed.
71187	111	29			Melting permafrost does not exist. It can thaw or degrade, but permafrost is not a material (see glossary) that can melt like ice or steal. [Lukas Arenson, Canada]	Accepted. Text removed.
38803	111	31	111	31	What "kind of heightened response" do you mean? [Maike Nicolai, Germany]	Accepted. Text removed.
73841	111	33	111	34	the statement of the magnitude of MPWP warming here is not consistent with the values stated in Chapter 2 (which say ~3*C) [McClymont Erin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62187	111	34	111	36	In section 9.6.2 (page 88, line 31) the stated range is 5-25 meters. Additionally, section 9.6.2 indicates that "This large reconstructed GMSL range means that the MPWP provides only a relatively weak constraint for individual ice-sheet simulations" and "...simulations of MPWP GrIS volume change are effectively unconstrained, ranging from growth from the present volume by 0.6 m sea-level equivalent (SLE) to complete ice sheet loss". Consider rephrasing this FAQ sentence to be consistent with section 9.6.2, e.g.: "At that time, carbon dioxide concentrations in the atmosphere were about the same as they are today, but changes in ice sheet extent meant that global mean sea level was 5-25 meters higher than now, ..." [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text removed.
22689	111	36	111	36	Numbers are inconsistent with main body text and chapter 2 assessment? [Peter Thorne, Ireland]	Accepted. Text removed.
68927	111	36			The mid Pliocene global sea level estimate is assessed in CH2. Please double check for consistency (Fig. 2.33). [Darrell Kaufman, United States of America]	Accepted. Text removed.
38805	111	37	111	38	Which relationship do you mean excactly? [Maike Nicolai, Germany]	Accepted. Text removed.
38807	111	40	111	40	"Conclusions" appears to be more suitable here than "implications" in my (non-native speaker) opinion. [Maike Nicolai, Germany]	Accepted. Text removed.
38809	111	40	111	40	Suggestion to rephrase as "for example the thinning of..." [Maike Nicolai, Germany]	Noted. Text removed.
26429	111	44	111	44	CO2 -> subindex [María Santolaria-Otín, France]	Noted. Text removed.
33521	111	44			Change: "…CO2…" by "…CO2…". [Guiomar Rotllant, Spain]	Not applicable. Text removed.
38811	111	45	111	45	Will CO2 circulate back to the atmosphere under all conditions? [Maike Nicolai, Germany]	Not applicable. Text removed.
26425	111	49	111	49	northern hemisphere ->in capitals [María Santolaria-Otín, France]	Not applicable. Text removed.
62121	111	49	111	50	Can you clarify "probably return within a few years"? The other statements in this paragraph have some sort of timescale attached with them; is this 5-10 years? A few decades (less than 5)? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Text removed.
15615	111	50	111	50	Temperature does not need to drop back to "pre-industrial level" for snow cover and sea ice to increase. I suggest cutting "to pre-industrial levels". [Samuel Morin, France]	Not applicable. Text removed.
41019	113	0			the text is a bit too long, it should be shortened to respect the 650-750 word limits of FAQs [TSU WGI, France]	Accepted. Reduced to <700 words.
40275	113	0			for a clearer flow of the text, it would be worth considering bringing the last paragraph after L31 to have near term change then long-term changes [TSU WGI, France]	Accepted. Reordered paragraphs.
41055	113	0			There is currently a mismatch between the title and the text: the titles focuses on the coming decades but then parts of the text talk about 2100 and even 2300. [TSU WGI, France]	Accepted. Now only 1 paragraph discusses beyond 2050, and in a general sense.
41141	113	0			very nice FAQ9.2, which is a great summary of ch9 [TSU WGI, France]	Accepted. Easy read for non-technical audience is the goal. Thank you.
253	113	1	111	55	THIS IS GREAT! Should be the model for FAQ 9.1 [THOMAS Wagner, United States of America]	Accepted. Easy read for non-technical audience is the goal. Thank you.
2999	113	3	113	4	Recent observed global average sea-level increase of 5 cm from 2000-2015 1/3 cm/yr which is a much higher rate than 7 mm by 2050. We can not expect this lower rate be realistic as it is not realistic to expect the global sea-level rise rate to decrease in the next three decades at least. I suggest adjusting the lower range of 7-20 cm to 11-20 cm which agrees with the currently observed rate 1/3 cm/yr. [Petteri Uotila, Finland]	Noted. Projected rates are being estimated more accurately now.
38815	113	3	113	52	This is a very succinct intro of a very useful FAQ! You could add that even a sea level rise of a few centimetres is able to incrase risks for example related to storms and storm tides. So the sea level rise does not only differ regionally, it also has different implications in different seasons or under different conditions. [Maike Nicolai, Germany]	Accepted. Addressing increased risk briefly.
107253	113	3		14	[pt 1 of 4] It says, "Scientists estimate that global average sea level – in 2015 already about 20 cm higher than in 1900 and 5 cm higher than in 2000 – will rise by a further 7–43 cm by 2050. Thermal expansion of water from increasing temperatures is a major reason for this, but the melting of ice-caps, glaciers and ice sheets all contribute. Local sea level change will be higher or lower than the global average in many locations, with the lowest rates in formerly glaciated areas and the highest rates of rise in low-lying river delta regions. Across the globe, sea level is rising. The rate of global mean sea level change has increased from an average of 1.4 ± 0.1 millimetres per year over the 20th century to 3.1 ± 0.3 millimetres per year from 1993 to 2017." That is misleading. I suggest the following replacement: [cont'd] [David Burton, United States of America]	Rejected. Unsubstantiated claims and personal opinions and greatly exceeds the word limit for FAQ format.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107255	113	3		14	[pt 2 of 4] "The global average rate of coastal sea level rise has been approximately 1.5 mm/year, with some decadal variations, but no significant sustained acceleration or deceleration, since the late 1920s. In 2015, globally averaged coastal sea-level was about 17 cm higher than 1900, and 2 cm higher than 2000. The rate is so small that in many places it is exceeded by local factors, either positive or negative, like post-glacial rebound (positive vertical land motion), sedimentation, erosion, or subsidence (negative vertical land motion). That causes local sea-level trends to be negative in some places, so that local sea-level is falling, but it also causes local sea-level rise to be much more rapid than the global average in other places. Because of that, calculating 'global average' sea-level trends is tricky, and it depends critically on the choice of sites used in the calculation. If the mix of sites included in the index varies over time, the result can be a spurious apparent acceleration or deceleration in the 'average,' which is not present at the individual sites." [cont'd] [David Burton, United States of America]	Rejected. Unsubstantiated claims and personal opinions and greatly exceeds the word limit for FAQ format.
107257	113	3		14	[pt 3 of 4] "Satellite altimetry cannot measure sea-level at or near the coasts, so it is not useful for coastal planning. Most of the satellite altimetry measurements are finding rates of sea-level rise which are substantially larger than those measured by the best tide gauges. Combining the two types of measurements can create the illusion of acceleration, typically seen as a step-increase in rate at about 1993 (when the satellite measurements began). You can see the effect in this graph, from Hansen & Sato; fortunately, they use contrasting colors, so the cause for the 'acceleration' is obvious: https://sealevel.info/SL.1900-2018b.png " [cont'd] [David Burton, United States of America]	Rejected. Unsubstantiated claims and personal opinions and greatly exceeds the word limit for FAQ format.
107259	113	3		14	[ot 4 of 4] "Thus far there is no sign that manmade climate change is significantly affecting global sea-level trends. Since the rate of sea level rise has not increased significantly in response to the last nine decades of CO2 emissions, there is little reason to expect that it will do so in response to the next nine decades of CO2 emissions. Thus, the best prediction for sea level in the future is simply a linear projection of the history of sea level at the same location in the past." ### [David Burton, United States of America]	Rejected. Unsubstantiated claims and personal opinions and greatly exceeds the word limit for FAQ format.
5457	113	4	113	6	I would suggest to add the words “ the addition of water from” in front of “the melting of glaciers and ice sheets” to make the comparison to the first part of the sentence [Marie Cavitte, Belgium]	Rejected. Too wordy and meaning is clear.
5459	113	6	113	7	I would suggest to explicitly replace “lowest rates of rise” with “lowest rates of sea level rise”. [Marie Cavitte, Belgium]	Not applicable. Text removed.
83045	113	9	113	9	Suggest you state "about 3 millimeters per year" and consider giving a more precise number. Depending on the dataset used, I find a value of 3.3 or 3.4 mm per year. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Higher precision is easily available elsewhere in the report. If two digits are used, then a range is required which greatly increases the complexity of the mathematics, probably beyond the FAQ intended audience.
62243	113	10	113	13	In this sentence it is highlighted that global sea level rise was mainly caused by natural factors, such as the post-Little Ice Age (LIA) glacier retreat. I think it would be nice to go a little bit more in detail here. How is this delayed response explained? What does the delayed response of glaciers to the post-LIA warming tell us about the contribution to future sea level rise? I personally guess that the glaciers are even more out of equillibrium with climate as they were during the 20th century. Is this issue considered in current sea level rise projections? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. While phrasing was improved there is not room for more detail.
38817	113	10	113	14	I think it needs to be made clearer that a more "natural" warming has caused the former rise in sea levels wheras the latter is caused by human-induced warming (among other factors). In both cases, warming causes the ice melt, but the warming itself has different causes, I think? [Maike Nicolai, Germany]	Accepted. Addressing this point improved the paragraph.
62275	113	16	113	22	Precipitation - evaporation rate. We should talk about this rate, at least mentioned as another factor to change sea level. The variation (final budget) is small but relevant to keep in mind if the atmospheric balance change. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This concept is far too abstract and likely to incorrectly misdirect the reader to think that E-P anomalies are preserved in SSH rather than mostly compensated by the rapid barotropic (Goldsborough) dynamic adjustment in a matter of a week or so.
5461	113	16	113	22	It seems that the percentages don’t add up to 100%. [Marie Cavitte, Belgium]	Accepted. Numbers are improved to add up, although they are only approximate within a range in any case.
83047	113	16	113	22	I think the contributors for GMSL and local sea-level could be more clearly explained in the FAQ. It may help to begin with the discusion of GMSL and then move to local scales and regional processes? It may also be useful to state that for some regions sea-level is currently falling? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Local sea level is now collected into one paragraph where the distinction is made clear.

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38819	113	16	113	22	Non-specialists might wonder why melting of ice from mountain glaciers and ice caps is differentiated from ice melt from ice sheets in Greenland and Antarctica. Taken together, the ice melt would be the bigger source, wouldn't it? What about the contribution from other ice sheets in the Arctic, apart from Greenland? The current differentiation of the sources of sea level rise puts a too large focus on thermal expansion in my (lay) opinion. [Maike Nicolai, Germany]	Noted. Glaciers and ice sheets are now treated more uniformly, and a figure helps to combine the cryosphere together
51987	113	24	113	25	The first sentence could be rewritten so that it is less likely to be misunderstood or mis-interpreted if quoted out of context. The simple take-home message for some might be that it doesn't matter what we do in the next 30 years, so why worry? Perhaps a revision similar to this might work: "Reduction in greenhouse gas emissions has a delayed influence on global mean sea level. The amount by which global mean sea level will rise over the next three decades depends more on relative changes to greenhouse gas emissions in the past than whether or not they are reduced over the same period." [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten to emphasize delayed response.
38821	113	24	113	28	I think there is quite a bit of room for misundestanding here. It does matter if emissions are reduced or not, the thing, I think, is just that the response will be delayed. Also, "all realistic scenarios" and "near term" might not be understood without background knowledge or outside the context of the report. My suggestion to rephrase this would be: "According to model projections, the global sea level will rise for the next three decades. Its amount will not respond immediately to reductions in greenhouse gas emissions. This is partly because oceans, glaciers and ice sheets mostly respond on timescales of decades, centuries or even longer periods, and will still be responding to warming that has occurred since the pre-industrial era for XX years. In addition, future scenarios assume that emissions remain similar in the near term and only start to differ after YY years." [Maike Nicolai, Germany]	Noted. Paragraph has been improved along these lines.
83049	113	24	113	31	A key part of the "committed" sea-level rise (i.e. insensitivity to emissions scenario) is that the ocean and cryosphere are still responding to the radiative forcing associated with past GHG emissions - suggest that you state this explicitly. This, in turn, is basically because CO2 in particular, has a very long atmospheric residence time (100s of years). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Addressing this point improved the paragraph.
5463	113	28	113	30	The sentence syntax is not easy to understand, perhaps a typo?\ [Marie Cavitte, Belgium]	Accepted. Rewritten
42633	113	28	113	30	The second "between" in this sentence should probably be "above", i.e. "…above the average level…" Also, the statement at the end of the sentence is unclear to me, "and the level in 2100 under all scenarios…" as the level itself will not be between 0.2 to 1.1 m. [Sofie Schöld, Sweden]	Not applicable. Text removed.
22691	113	28	113	30	As written this is misleading because the underlying assessment shows a clear divergence between low and high emission scenarios. Text as written incorrectly implies that e.g. 1.1m could be reached under SSP1-2.6. [Peter Thorne, Ireland]	Accepted. Rewritten
5465	113	30	113	31	It would be good to reiterate that the four times faster than that will be for 2100, to be explicit. [Marie Cavitte, Belgium]	Accepted. Rewritten
129605	113	33	113	47	The distinction between geocentric sea level and relative sea level is not clear in this. The question is "how much will sea level rise", but a significant portion of the explanation is on subsidence. Also, confusing why extreme events are mentioned, given the question that is asked. [Trigg Talley, United States of America]	Rejected. This comment suggests a technical framing that is appropriate for standard chapter text, not an FAQ which has a less technical audience.
29271	113	35	113	40	"In regions where large ice sheets covered the land during the last ice age, such as Scandinavia and North America, the land is still slowly rising up . . . And can even lead to a local fall in sea level" While this is true for some locations in North America, there are also many densely populated coastal regions of North America (e.g., New York/New Jersey region) that fall instead into the "regions just beyond where the former ice sheets reached, where . . . The land is now falling, and as a result local sea level rise is faster than the global rate". This section simply needs to be amended to be clear that North America as a whole does not fall into the region where land is now rising, causing a fall in sea level. It needs to be clear that some portions of North America actually experience a faster than average rate of SLR due to GIA as well. [Andra Garner, United States of America]	Accepted. Rewritten
52209	113	38			Consider a qualifier that emphasizes some areas of Scandinavia and some areas of North America can experience a local fall in sea level since the effect is not homogenous across the entire land masses. The current language could be misinterpreted. [Gregory Garner, United States of America]	Accepted. Rewritten
38823	113	39	113	40	Can you give an example for this region as well? [Maike Nicolai, Germany]	Rejected. Text rewritten to emphasize named ice sheets, which can be looked up by reader.
38825	113	40	113	42	Can you give examples for such cities? [Maike Nicolai, Germany]	Rejected. Text rewritten to emphasize named ice sheets, which can be looked up by reader.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
62123	113	43	113	47	The discussion concerning isostatic rebound and global effects here would benefit from a quantitative value here which would lessen the counter-intuitiveness pointed out by the author. How much of this effect translates into sea level rise (either in terms of mm or %)? Is this considered a part of the contributions from Greenland and West Antartica %ages listed previously? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Added "tens of centimetres" by region by 2100.
83849	113	46	113	47	This FAQ relates to SLR in the next few decades, however it might be worth considering inserting as part of this sentence that the regions of increased sea-level in the opposite hemisphere from the melt (when considering a single source of melting) also acts to amplify any tidal changes experienced in these regions (as shown in Pickering et al. (2017)). When melt from both polar ice sheets is considered equatorial SLR is greater and hence tidal changes there are amplified, especially in Asia (e.g. Pickering et al 2017 Both ice sheet melt scenario- similar to sea-level fingerprint Figure 9.29e). [Mark Pickering, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Too technical for this audience. Extremes are already noted in previous paragraph at an appropriate level of detail.
3001	113	46	113	47	Would be important to highlight here that the biggest net impacts of the melt both polar ice caps are experienced in low latitudes (cf. Figure 9.29 e). [Petteri Uotila, Finland]	Rejected. Text rewritten to emphasize named ice sheets, which can be looked up by reader.
24491	113	49	113	52	There is description about changes in frequency of extremes. It is better to describe cause/forcing of changes in ESLs to support scientific evidence and improve understanding. [Nobuhito Mori, Japan]	Accepted. Clarified
40359	113	49			I am not sure that a lay audience is clear with what extreme sea-level is. Could you clarify this? [TSU WGI, France]	Accepted. Clarified
29273	114	2	114	2	The last sentence of the FAQ should be worded to be clear that we are not currently on a pathway that is consistent with SSP1-2.6. The way it is currently written, it could be interpreted as meaning that we're currently on the SSP1-2.6 pathway, and that if future emissions rise above this level, we could have more sea-level rise. For additional clarification and comparison, I suggest including the values for the 95th percentile of sea-level rise under a higher emissions scenario as well as the value already listed for SSP1-2.6. This will help to emphasize the weight that our decisions today carry for our future world. [Andra Garner, United States of America]	Not applicable. Text removed.
62277	114	7	114	12	The figure FAQ9.2 does not present values, just XX. Please feel and complete the figure. Bigger size for easy reading. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Figure revised
39979	115	0			avoiding acronyms is better for a lay audience, so could you consider removing the AMOC one in the text? [TSU WGI, France]	Rejected. AMOC is the key scientific concept and is most often referred to by acronym (as in the chapter text). It is defined here on the same page.
40363	115	0			I am uneasy about the current title, which seems to focus very much on one region of the world, though the text also mentions other regions, would it make sense to rephrase the title and use the gulf stream as one example amongst others? Or explain more clearly and earlier why the gulf stream? or maybe focus more on the overturning circulation/ AMOC and less on the gulf stream or any specific current? [TSU WGI, France]	Noted. Other regions of the world are specifically contrasted versus the Gulf Stream, but the unique character of the Gulf Stream in both gyre and overturning is now more clearly highlighted.
40161	115	0			FAQ9.3 is very well written and clear [TSU WGI, France]	Accepted. Revisions have intended to preserve the clarity.
40937	115	0			the FAQ is a bit too long it should be 650-750 words long [TSU WGI, France]	Accepted. Now less than 700 words
68551	115	1	115	9	Because the Gulf Stream is predominantly a wind driven current, with a modest contribution from AMOC (maybe 18 Sv out of some 130 Sv at the Gulf Stream's peak), we know that the Gulf Stream will not "shut down". However this passage suggests the possiblity that the Gulf Stream might shut down, specifically in the clause on p. 115, line 7 "If it [the Gulf Stream] slows further or shuts down...". The predominant role of the winds in driving the Gulf Stream are correctly noted later in lines 32-35, but the italicised leading paragraph of this FAQ incorrectly identifies the Gulf Stream as "a part of "AMOC and suggests that the answer to the question "Will the Gulf Stream shut down?" might be yes. A part of the Gulf Stream does contribute to AMOC, but we know very well that the answer to the question "Will the Gulf Stream shut down?" is emphatically NO! The introductory paragraph of FAQ 9.3 needs to be rewritten to properly describe the relationship between the Gulf Stream and AMOC. [Robert Hallberg, United States of America]	Accepted. Rewritten for greater clarity
79933	115	1	115	55	I might suggest to change the question since different concepts as AMOC, Gulf Stream (or orther western boundary currents) are mixed in the answer, and these are typically source of misunderstanding. Something like 'Will the AMOC or any of its components shut down, and what happens if it does?' [Somavilla Raquel, Spain]	Rejected. The complexity of the discussion has been kept under control, toward the FAQ intended audience. The Gulf Stream is the better known current, and as such it draws this audience into the more detailed description of its composition in the body of the FAQ.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
97047	115	1	116	14	FAQ 9.3 „Will the Gulf Stream shut down, and what happens if it does?“ is still relevant for European public and so authors should take care about there writings taking into account the non-scientific background of most of the IPCC audiences. [Nicole Wilke, Germany]	Accepted. The complexity of the discussion has been brought under control, toward the FAQ intended audience.
55145	115	1	116	14	The answer to the question "Will the Gulf Stream shut down" is clearly no, but the text of the FAQ is somewhat ambiguous as currently written. Revised text should emphasize that while the Gulf Stream will NOT shut down because we expect winds to continue to blow over the surface of the ocean and we expect planet Earth to continue to rotate, it is possible for the AMOC to shut down. And if the AMOC shuts down, then the volume of water transported by the Gulf Steam would be diminished by about 15% to 20%. In other words, the Gulf Stream will not shut down, but the AMOC fraction of the Gulf Stream may shut down. [Nancy Hamzawi, Canada]	Accepted. Rewritten for greater clarity
42635	115	1	116	14	This is a very common question and as such the answer could perhaps be clarified a bit. First, I understand it as the northward transport of thermal energy is driven mainly by wind patterns that are not expected to change much, basically: "These currents will continue to transport thermal energy poleward from the equator much as they do now..", and that is is the southward transport of cooler water that is expected to slow down. Then, however, there is a paragraph on what will happen as the overturning slows or shuts down where a weakened transport of thermal energy northward is implied, i.e. "But the reduced oceanic poleward heat transport means the North Atlantic will warm more slowly..." So, will it or won't it? [Sofie Schöld, Sweden]	Accepted. Rewritten for greater clarity
111745	115	1	116		This box is welcome. However I think the terminology needs to be reconsidered. There is the usual problem of dancing round whether it's OK to refer to the AMOC as the Gulf Stream. The approach taken here seems to have been to avoid talking about the AMOC at all costs. This results not only in imprecision but I think in making the box confusing to read for the lay reader. Considering the level of the language used in the rest of the box, I think it would be clearer to explain the distinction between Gulf Stream and AMOC at the start, and then refer to the AMOC throughout where that's what you mean. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten for greater clarity
38827	115	3	115	3	Line 3 says "The Gulf Stream is part of a large ocean current in the North Atlantic". Line 11 says "The Gulf Stream is the biggest current in the North Atlantic Ocean ". This might sound confusing. Please clarify. [Maike Nicolai, Germany]	Accepted. Rewritten for greater clarity
98707	115	3	115	9	The first paragraph of this FAQ incorrectly states that the Gulf Stream could shut down. Later in the text, it states that the part of the Gulf Stream that is wind-driven is not expected to change much. The Gulf stream will NOT therefore shut down. The AMOC is expected to weaken, and a few models show it shutting down, but the Gulf stream and the AMOC are not synonymous. [Sonya Legg, United States of America]	Accepted. Rewritten for greater clarity
255	115	3	115	9	Answer the question, as is done for FAQ 9.1. The question isn't what is the Gufstream, it's what happens. Lines 50-55 are the answer that should be upfront. The rest of the answer is OK. [THOMAS Wagner, United States of America]	Accepted. Rewritten for greater clarity
101923	115	4	115	5	The statement, "Scientists estimate that this current is slowing down" does not agree with page 28 - 29 statements: "The short length of the record and the surprisingly large variability compared to CMIP models (Roberts et al., 2014) (Figure 9.10) gives low confidence in a meaningful long term trend" and "Based on these results we have low confidence in the reconstructed AMOC weakening over the last 150 years, low confidence that the presently observed AMOC reduction is anthropogenically forced and low confidence that the AMOC has been declining since the mid-twentieth century". Suggest tempering language to something like "There is limited evidence that this current may be slowing down" [IAPSO ECS group review, United States of America]	Noted. This FAQ has been revised to be consistent with the Chapter 2, 4, 9 text and assessments.
111743	115	5	115	6	Suggests that there will be no slowing in the 21st Century. Suggest rrephrase [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This FAQ has been revised to be consistent with the Chapter 2, 4, 9 text and assessments.
5469	115	7	115	7	I would specify what period “further” refers to explicitly. Is it with respect to present? [Marie Cavitte, Belgium]	Accepted. Rewritten for greater clarity
55143	115	7	115	7	Delete "or shuts down". As currently written, this sentence could be interpreted as meaning that the Gulf Stream could actually shut down. But the Gulf Stream cannot shut down unless winds stop blowing and the Earth starts rotating. What the Gulf Stream can do however is to diminish in strength if the AMOC stops. If the AMOC shuts down, then the Gulf Stream would become more similar to the Kuroshio because no deep convection takes place in the North Pacific. [Nancy Hamzawi, Canada]	Accepted. Rewritten for greater clarity
38829	115	11	115	11	Line 11 says "The Gulf Stream is the biggest current in the North Atlantic Ocean ". Line 3 says "The Gulf Stream is part of a large ocean current in the North Atlantic". This might sound confusing. Please clarify. [Maike Nicolai, Germany]	Accepted. Rewritten for greater clarity

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68549	115	12	115	13	The Gulf Stream transport through the straits of Florida is about 30 Sv, but further north there are recirculation gyres and contributions from the North Atlantic subtropical gyre itself that increase the Gulf Stream transport to several times this value. The description of the Gulf Stream in these lines, which echo old descriptions of the Gulf Stream as part of a constant "conveyor belt", are factually incorrect and need to be revised. I appreciate that this is an attempt to use simplifying language in an FAQ to try to make this passage more accessible, but we have an obligation not to allow simplicity to lead to significant inaccuracy. [Robert Hallberg, United States of America]	Accepted. Rewritten for greater clarity
68547	115	13	115	14	The Gulf Stream is not 5 to 15 degrees warmer than "the surrounding water", e.g., in the middle of the subtropical gyre at comparable latitudes. This is a reasonable temperature difference compared with the deeper return flows, which is why AMOC transports a lot of heat with a modest transport compared with wind driven gyres, but it is hard to see how anyone would describe this deep return flow as "surrounding water". This sentence should be revised for correctness. [Robert Hallberg, United States of America]	Rejected. It is indeed this much warmer in observed SSTs, particularly when compared to the colder rim current leaving the Labrador Sea.
101925	115	20	115	32	It is unclear why the Kuroshio is brought up in this section. If the FAQ concerns the Gulf Stream, bringing up the Kuroshio is confusing. [IAPSO ECS group review, United States of America]	Rejected. The unique nature of the Gulf Stream in highlighted by contrasting it against the Kuroshio.
3003	115	22	115	23	The Greenland Sea should be added here as a region of deep water formation. [Petteri Uotila, Finland]	Not applicable. Text removed.
101927	115	37	115	37	The statement, "the Atlantic Meridional Overturning Circulation is slowing" does not agree with page 28 - 29 statements: "The short length of the record and the surprisingly large variability compared to CMIP models (Roberts et al., 2014) (Figure 9.10) gives low confidence in a meaningful long term trend" and "Based on these results we have low confidence in the reconstructed AMOC weakening over the last 150 years, low confidence that the presently observed AMOC reduction is anthropogenically forced and low confidence that the AMOC has been declining since the mid-twentieth century". Suggest tempering language to "the AMOC may be slowing" [IAPSO ECS group review, United States of America]	Accepted. Rewritten for greater agreement with present chapter summary statements (which are more technical, of course)
132597	115	37	115	37	I thought it was assessed that AMOC slowdown in recent years may be just variability and we have low confidence that it has slowed in recent decades. The phrase "AMOC is slowing" makes it seems like we have observed a forced change, while the chapter text above says otherwise. [Kyle Armour, United States of America]	Accepted. Rewritten for greater agreement with present chapter summary statements (which are more technical, of course)
51529	115	37	115	39	I find this statement contradicts the main text in section 9.2.3.1 (see 9-28 line 54 to 9-29 line 26). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten for greater agreement with present chapter summary statements (which are more technical, of course)
3005	115	38	115	39	Freshwater from the Arctic rivers has increased in the recent decades and should be mentioned here. Perhaps also the increase of relatively fresh Pacific water inflow, although it is mainly captured in the Beaufort Gyre. Reference: Vihma, T., Uotila, P., Sandven, S., Pozdnyakov, D., Makshtas, A., Pelyasov, A., et al. (2019). Towards an advanced observation system for the marine Arctic in the framework of the Pan-Eurasian Experiment (PEEX). Atmospheric Chemistry and Physics, 19(3), 1941–1970. https://doi.org/10.5194/acp-19-1941-2019. [Petteri Uotila, Finland]	Rejected. Too technical for this audience and limited length.
7887	115	38	115	39	I'm not aware of studies showing this. Gregory et al (2005) shows the AMOC weakening was mainly driven by heat fluxes https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2005GL023209 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text makes it clear that "one reason why" is freshening, which is not the same as a sole cause.
5471	115	40	115	40	I would suggest somehow specifying that “warmer” relates to the increased precipitation only. The melting of the ice sheet and sea ice only helps in stratifying the ocean water with the fact that it adds fresh water. [Marie Cavitte, Belgium]	Accepted. Rewritten for greater clarity
38831	115	40	115	40	In case "fresher and warmer" also means "less dense", I would add this here to connect this part to the explanations from line 22 to 24. [Maike Nicolai, Germany]	Accepted. Rewritten for greater clarity
7889	115	40	115	41	I'm pretty sure that there are no studies showing that Lab and Irm seas are warmer and fresher overall. There is large variability in this region and I think at the moment they are fresher and colder than average, however I don't think anyone has shown any ongoing trend [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten for greater clarity
38833	115	41	115	41	Has the array of moorings monitored the Gulf Stream or has it been used by scientists to monitor the Gulf stream? I also wonder if "moorings" will be understood. "moored instruments" or "moored observatories" might be clearer. [Maike Nicolai, Germany]	Accepted. Rewritten for greater clarity
32913	115	41	115	42	should say "Since 2004 and array of moorings across the Atlantic at 26.5˚N has been monitoring the AMOC." The Gulf Stream has been monitored for much longer using an undersea cable (Meinen et al.,2010, DSR I, 57:835-846) [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten for greater clarity

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101929	115	42	115	42	Use of the word "new" for an observing system that has been around since 2005 is potentially confusing to the public. [IAPSO ECS group review, United States of America]	Accepted. Rewritten for greater clarity
32915	115	42	115	42	a slowdown is apparent in the AMOC - see Smeed et al. (2014, 2018) - but pernhaps not in the Gulf Stream (Meinen et al., 2010) [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, the assessment in this report in chapters 2, 3, 9 is that there is not yet consensus detection of an attributable trend.
7891	115	42	115	42	A slowdown has been detected, but many people think this is variability (see AMOC section). I suggest either expanding on this or rewording (ie We don't yet have enough years of data to be able to detect an ongoing weakening...) [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, the assessment in this report in chapters 2, 3, 9 is that there is not yet consensus detection of an attributable trend.
68545	115	54	115	55	The main reason why a slowing AMOC leads to extra sea level rise along the east coast of North America is not an accumulation of heat, as described here, but rather a reduction in the geostrophically balanced slope across the weakening Gulf Stream. [Robert Hallberg, United States of America]	Accepted. Rewritten for greater clarity
38835	116	2	116	2	Please replace "we" by "scientists". [Maike Nicolai, Germany]	Accepted. Rewritten for greater clarity and brevity
129607	116	2	116	14	Who are "we" in the sentence? On the substance, the description of AMOC process is justified for its climate effects. However, the discussion and summary are rather shallow and unsubstantiated. No synthesis of literature, but a general description of the process. Some recent publications that revealed additional complexity and new findings on the AMOC evolution and interactions with other oceanic processes could be summarized here. [Trigg Talley, United States of America]	Accepted. Rewritten for greater clarity and brevity
68543	116	6	116	8	The densest water in the world does not from around Antarctica. The salty deep waters in the Mediterranean and Red seas that overflow into the Atlantic and Indian Oceans are much denser than the Antarctic Bottom Water source waters. As discussed in the review by Legg et al. (2009), the reason why AABW fills the abyssal ocean and not Med water or Red sea water is because AABW entrains much less ambient water than the other two overflows, and not because because the source water is denser. Although the overall point that is being made here is helpful, this line should be revised for correctness. [Robert Hallberg, United States of America]	Noted. However, marginal sea pre-entrainment outflow at the sill is not generally considered as a large-scale water mass, and surely a particularly intense Meddy would be even more dense, but would not constitute a water type, so most traditional oceanographic literature take AABW as the densest (although this is admittedly a pedantic stance).
5473	116	6	116	10	I would suggest to explain more explicitly why when the salinity of the water is reduced, the rate at which it sinks is slowed. [Marie Cavitte, Belgium]	Accepted. Rewritten for greater clarity and brevity
97049	116	6	116	14	The AABW got warmer with not much change in salinity. In the Weddell Sea, the major contributor to AABW, the warming is caused by entrainment of ambient Circumpolar deep Water into the precursor of AABW while sinking into the abyss (i.e. Huhn et al., 2013), The CDW got warmer (and older) in the last decades, warming also the AABW. Please include this information. [Nicole Wilke, Germany]	Rejected. The detailed changes to AABW go beyond the level of detail and space for this FAQ.
7893	116	6	116	14	This paragraph is not about the Gulf Stream so I suggest cutting [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. As this report has a global audience, the similar processes elsewhere serve to illustrate the global nature of MOC connections to climate change.
33075	116	43	116	45	evidence for heating the troposphere locally must be added for other parts of the world (with same natural drivers such as arid and semi-arid desert, if any). Without this correction it seems that these natural derivers are just working in this part of the world. [Sahar Tajbakhsh Mosalman, Iran]	Noted. Rewritten for greater clarity and brevity
32745	116	43	116	45	evidence for heating the troposphere locally must be added for other parts of the world (with same natural drivers such as arid and semi-arid desert, if any). Without this correction it seems that these natural derivers are just working in this part of the world. [sadegh zeyaeyan, Iran]	Noted. Rewritten for greater clarity and brevity
83331	117	1	180	24	REFERENCE LIST - there is some discrepancy in the style of referencing e.g. inconsistency in the use of capitals in the titles of papers. [Robert Massom, Australia]	Editorial - copyedit to be completed prior to publication
20203	117	1	180	24	It is suggested that the duplicated "submitted" be removed in many references [philippe waldteufel, France]	Editorial - copyedit to be completed prior to publication
52211	119	2	119	4	In addition to the overall higher sea levels due to Antarctic ice melt, consider adding text about the increased rate of sea-level rise due to the increased rate of melt and potential threshold effects that may have impacts at decision-relevant timescales. [Gregory Garner, United States of America]	Taken into account. Processes contributing to high-end SLR are addressed in Box 9.4.
30705	119	12	119	13	Please correct reference details for this paper. They should be: Batbaatar, J., A. R. Gillespie, D. Fink, A. Matmon and T. Fujioka, 2018: Asynchronous glaciations in arid continental climate. Quaternary Science Reviews, 182, 1-19, doi: 10.1016/j.quascirev.2017.12.001. [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication
88729	120	26	120	26	Berloff, P., Hogg, A. M. C., and Dewar, W. (repeated names in the same line) [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88731	121	2	121	2	Bishop, S. P., Small, R. J., et al. (repeated names in the same line) [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
30707	121	24	121	26	Article number of Bliss paper is: 045003 [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30709	122	10	122	11	This paper has now been published. Details are: Bracegirdle, T. J., G. Krinner, M. Tonelli, F. A. Haumann, K. A. Naughten, T. Rackow, L. A. Roach and I. Wainer, 2020: Twenty first century changes in Antarctic and Southern Ocean surface climate in CMIP6. Atmospheric Science Letters, 21, e984, doi: 10.1002/asl.984. [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication
111399	122	24	122	26	This paper is now published: https://www.nature.com/articles/s41561-020-0567-4 [Jo Brendryen, Norway]	Editorial - copyedit to be completed prior to publication
88733	125	36	125	37	Chassignet, E. P., Xu, X. (repeated names in the same line) [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88735	125	36	125	37	Cheng et al., year? [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88737	126	4	126	5	Church et al., 2013b. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
89891	126	40	126	43	The IPCC special report is now published, the reference needs to be updated. [Peter Croot, Ireland]	Editorial - copyedit to be completed prior to publication
88739	127	39	127	40	Danabasoglu et al., 2010. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
129609	127	41	127	42	Update the Dangendorf et al. (2019) reference. It has been published: https://www.nature.com/articles/s41558-019-0531-8 [Trigg Talley, United States of America]	Editorial - copyedit to be completed prior to publication
16443	127	57	127	57	Inconsistency with captilisation of “de Boer” [Julian Mak, China]	Editorial - copyedit to be completed prior to publication
32177	128	3	128	6	Delete link to Google search and give doi instead: doi:/10.1029/2004JC002378 [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
16445	128	15	128	15	No captial on “De” (de Lavergne) [Julian Mak, China]	Editorial - copyedit to be completed prior to publication
21005	129	14	129	15	Add reference between line 14 and 15 : Diaz Henry & Markgraf 2000: El Niño and the Southern Oscillation: Multi- scale Variability and global and regional Impacts pp1-45 [Ladislaus Chang'a, United Republic of Tanzania]	Editorial - copyedit to be completed prior to publication
88741	130	15	120	16	Dowsett et al., 2016. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
106713	131	11	13	15	The reference Edwards et al. 2019 seems to appear two times in the bibliography (correct for the link to this reference in the report) [Kevin Bulthuis, United States of America]	Editorial - copyedit to be completed prior to publication
61795	131	11	131	15	twice same reference for Edwards 2019 a and b [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
30711	131	11	131	15	Repetition here [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication
88743	131	16	131	17	Edwards et al., submitted. Repeated reference. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88745	131	21	131	22	Edwards et al., submitted. Repeated reference. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
21007	132	31	132	32	Add reference between line 31 and 32: Flather R.A & Khandker H 1993: The storm surge problem and possible effects Of Sea level changes on coastal flooding in the Bay of Bengal (Cambridge University Press 93 on Climate and Sea Level Change pg 229-245) [Ladislaus Chang'a, United Republic of Tanzania]	Editorial - copyedit to be completed prior to publication
88747	133	8	133	8	Frederikse, T., Jevrejeva, S., Riva, R. E. M., Dangendorf, S., Frederikse, T., Jevrejeva, S., et al. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
16447	134	14	134	14	Should really be “Naveira Garabato A. C.” [Julian Mak, China]	Editorial - copyedit to be completed prior to publication
41893	134	37	134	38	The reference here is an old submission. The paper is currently under review and the citation should be: Garry, F. K., Roberts, C. D., Blaker, A. T., McDonagh, E. L., Frajka-Williams, E., and King, B. A. Increasing importance of deep ocean heat storage in 21st century climate projections. Scientific Reports (submitted). [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication
88749	134	42	134	42	Gasson, E., DeConto, R. R. M., Geology, D. P.-, 2016, U., and Pollard, D. (2016b). [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88751	134	59	135	1	Genthon et al. 2009 (a-b) The references are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
61793	135	38	135	41	twice exactly same reference Goelzer 2016 a and b is the same [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
90493	135	38	135	41	Goelzer et al. 2016a and 2016b are exactly the same refs? [Holly Kyeore Han, Canada]	Editorial - copyedit to be completed prior to publication
80515	135	38	135	41	There are two different papers Goelzer et al., 2016 that may both be of relevance; Goelzer, H., Huybrechts, P., Loutre, M.-F., and Fichefet, T.: Last Interglacial climate and sea-level evolution from a coupled ice sheet–climate model, Clim. Past, 12, 2195–2213, https://doi.org/10.5194/cp-12-2195-2016, 2016. Goelzer, H., Huybrechts, P., Loutre, M.-F., and Fichefet, T.: Impact of ice sheet meltwater fluxes on the climate evolution at the onset of the Last Interglacial, Clim. Past, 12, 1721–1737, https://doi.org/10.5194/cp-12-1721-2016, 2016. [Heiko Goelzer, Belgium]	Noted. The existing citation was considered relevant in this context .
88753	135	38	135	41	Goelzer et al. 2016 (a-b). The references are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
30713	135	38	135	41	This reference is repeated, and there is only one 2016 paper of relevance. Noite that the correct author details are: Goelzer, H., P. Huybrechts, M.-F. Loutre and T. Fichefet, 2016: Last Interglacial climate and sea-level evolution from a coupled ice sheet-climate model. Climate of the Past, 12, 2195-2213, doi: 10.5194/cp-12-2195-2016. [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32179	135	40	135	41	Delete repeated reference Goelzer(2016b), change Goelzer (2016a) in Goelzer (2016) [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
32181	135	45	135	50	Delete repeated references Goelzer(2017) and Goelzer (2018a), change Goelzer (2018b) in Goelzer (2018) [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
90495	135	45	135	53	Same refs? [Holly Kyeore Han, Canada]	Editorial - copyedit to be completed prior to publication
80513	135	45	135	53	These three reference list entries are for the same paper that was published in 2018. Goelzer, H., Nowicki, S., Edwards, T., Beckley, M., Abe-Ouchi, A., Aschwanden, A., Calov, R., Gagliardini, O., Gillet-Chaulet, F., Golledge, N. R., Gregory, J., Greve, R., Humbert, A., Huybrechts, P., Kennedy, J. H., Larour, E., Lipscomb, W. H., Le clec'h, S., Lee, V., Morlighem, M., Pattyn, F., Payne, A. J., Rodehacke, C., Rückamp, M., Saito, F., Schlegel, N., Seroussi, H., Shepherd, A., Sun, S., van de Wal, R., and Ziemen, F. A.: Design and results of the ice sheet model initialisation experiments initMIP-Greenland: an ISMIP6 intercomparison, The Cryosphere, 12, 1433–1460, https://doi.org/10.5194/tc-12-1433-2018, 2018. References in the text (Goelzer et al., 2017, Goelzer et al., 2018a, Goelzer et al., 2018b) should be renamed to Goelzer et al., (2018). [Heiko Goelzer, Belgium]	Editorial - copyedit to be completed prior to publication
88755	135	45	135	53	Goelzer et al. 2017, 2018 (a-b). The references are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
61791	135	48	135	53	twice exactly same reference Goelzer 2018 a and b is the same [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
80517	135	54	135	55	This paper is in discussions: Goelzer, H., Nowicki, S., Payne, A., Larour, E., Seroussi, H., Lipscomb, W. H., Gregory, J., Abe-Ouchi, A., Shepherd, A., Simon, E., Agosta, C., Alexander, P., Aschwanden, A., Barthel, A., Calov, R., Chambers, C., Choi, Y., Cuzzone, J., Dumas, C., Edwards, T., Felikson, D., Fettweis, X., Golledge, N. R., Greve, R., Humbert, A., Huybrechts, P., Le clec'h, S., Lee, V., Leguy, G., Little, C., Lowry, D. P., Morlighem, M., Nias, I., Quiquet, A., Rückamp, M., Schlegel, N.-J., Slater, D., Smith, R., Straneo, F., Tarasov, L., van de Wal, R., and van den Broeke, M.: The future sea-level contribution of the Greenland ice sheet: a multi-model ensemble study of ISMIP6, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-319, in review, 2020. [Heiko Goelzer, Belgium]	Editorial - Will check publication status for final report.
61789	135	61	136	1	is now published doi.org/10.1002/wcc.634 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
64369	135		135		it seems that several a and b labeled references are identical [roderik van de wal, Netherlands]	Editorial - copyedit to be completed prior to publication
88757	136	22	136	23	Gomez et al., 2013. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
61787	137	11	137	11	before the doi the year of publication is mentioned, which needs to be deleted [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
30715	138	6	138	7	Part of reference scrambled [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication
61561	138	7	138	7	endash coding is not working [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
89813	141	44	141	47	This IPCC special report is now published, the reference needs to be updated. [Peter Croot, Ireland]	Editorial - copyedit to be completed prior to publication
89815	141	48	141	51	The IPCC special report is now published, the reference needs to be updated. [Peter Croot, Ireland]	Editorial - copyedit to be completed prior to publication
67377	142	29	142	30	Add this reference between these two lines: Jansen, E., Christensen, J.H., Dokken, T., Nisancioglu, K.H., Vinther, B.M., Capron, E., Guo, C., Jensen, M.F., Langen, P.L., Pedersen, R.A., Yang, S., Bentsen, M., Kjær, H.A., Sadatzki, H., Sessford, E. and Stendel, M. (2020): Past perspectives on the present era of abrupt Arctic climate change. Nature Climate Change, accepted. [Martin Stendel, Denmark]	Editorial - copyedit to be completed prior to publication
32183	143	42			Delete – [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
61785	144	31	144	36	twice exactly same reference Khazendar 2019 a and b are the same [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
30717	144	31	144	36	Reference is repeated (as a and b) [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication
32185	148	37			Delete repeated initials P. U. and G. A. [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
61563	148	42	148	42	"submitted, a" not "submitteda" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61783	148	42	148	47	twice exactly same reference, Levermann submitted a and b are the same [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61779	152	9	152	14	twice exactly same reference, Marzeion 2015 a,b are the same [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
88759	152	9	152	14	Marzeion et al., 2015 (a-b) The references are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
32187	152	9	152	14	delete repeated reference and change the labels in the text deleting a/b [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
61839	154	7	154	7	change to: at three different time scales (triple hatch marks; millions of years, thousands of years and years) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61841	154	9	154	10	change to Cross-Chapter Box [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
88761	155	35	155	36	MORLIGHEM et al., (year) ? [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21009	157	30	157	30	Add reference between line 30 and 31:Nicholls N, Chang P and Zebiak S 2003: El Niño and the Southern Oscillation (Encyclopedia of Atmospheric Sciences, Volume 4, pp 713-724) [Ladislaus Chang'a, United Republic of Tanzania]	Editorial - copyedit to be completed prior to publication
61565	158	14	158	14	“submitted, a” not “submitteda” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61797	158	37	158	40	twice same reference for Oerder 2015 a and b [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61799	159	28	159	31	twice same reference for Palmer submitted [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
88763	159	28	159	31	Palmer et al. (submitted). The references are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
32189	159	58			references undefined + two in one [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
89413	162	25	162	26	Change to: Reese, R., Albrecht, T., Mengel, M., Asay-Davis, X., and Winkelmann, R.: Antarctic sub-shelf melt rates via PICO, The Cryosphere, 12, 1969–1985, https://doi.org/10.5194/tc-12-1969-2018, 2018 [Ricarda Winkelmann, Germany]	Editorial - copyedit to be completed prior to publication
76755	162	25	162	26	Author list is only “Reese, R., Gudmundsson, G. H., Levermann, A., Winkelmann” [Ronja Reese, Germany]	Editorial - copyedit to be completed prior to publication
32191	162	40			Delete "DOI" [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
61801	162	55	162	58	same reference for RGI Consortium 2017 a and b [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
88765	162	55	162	58	RGI Consortium, 2017 (a-b). The references are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88767	163	39	163	41	Rind et al., 2018. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
30719	163	55	164	56	Paper now published: Roach, L. A. et al., 2020: Antarctic sea ice area in CMIP6. Geophys. Res. Lett., 47, e2019GL086729, doi: 10.1029/2019GL086729. [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication
24009	164	28	164	28	insert "Rodrigues, R. R., Taschetto, A. S., Sen Gupta, A., and Foltz, G. R. (2019).Common cause for severe droughts in South America and marine heatwaves in the South Atlantic. Nat. Geosci. 12, 620–626. Available at: https://doi.org/10.1038/s41561-019-0393-8. [Moacyr Araujo, Brazil]	Editorial - copyedit to be completed prior to publication
32193	164	28	164	32	correct format of doi, delete "available at" here and elsewhere in the references [Anja Wendt, Germany]	Editorial - copyedit to be completed prior to publication
88769	165	14	165	16	Ruprich-Robert et al., 2018. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88771	165	37	165	37	Sallée, 2018. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
61803	165	40	165	45	same reference for Sallee 2013 a and b but with different doi and page numbers [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
30721	166	17	166	20	Reference is repeated [Ian Simmonds, Australia]	Editorial - copyedit to be completed prior to publication
61805	166	17	166	20	same reference for Schloesser 2019 a and b [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61807	166	57	166	61	looks like same reference for SenGupta2009 a and b but the reference details seem not consistent [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
61567	167	16	167	16	This is a duplicate reference. The more complete reference is located a few references down from this one. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
88773	167	16	167	17	Seroussi et al., submitted. Repeated reference. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
106651	167	23	167	28	Update reference for Seroussi et al. 2019. Paper has been accepted. [Kevin Bulthuis, United States of America]	Editorial - copyedit to be completed prior to publication
88775	167	24	167	26	Seroussi et al., submitted. Repeated reference. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88777	169	55	169	57	Soudzilovskaia et al., 2013.Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88779	171	59	171	61	Swingedouw et al., 2006. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
61569	174	28	174	28	Need to be consistent with capitalization of “van den” [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication
88781	175	52	175	55	Wang et al., 2015 (a-b). The references are the same. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88783	177	55	177	57	Wullschleger et al., 2014. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
88785	180	5	180	6	Zu et al., 2015. Reference not found in the text. [Rosemary Vieira, Brazil]	Editorial - copyedit to be completed prior to publication
22693	181	3	181	37	These details should be in the figure captions and not in an appendix in my view. Figures should be self-contained between the figure and its caption. [Peter Thorne, Ireland]	Accepted. Most figure details were moved to caption, with only one exception where excessively long referencing and methodology needed more space.
16449	181	7	181	7	Move “(in Gt)” forward (“...amount of fresh water added (in Gt) is then this flux...”), as “number of years” or “flux” is not measured in Gt. [Julian Mak, China]	Rejected. This form does have the correct units and method.
88787	181	25	181	25	Goelzer et al., 2016 (a-b). The references are the same. [Rosemary Vieira, Brazil]	Accepted. Repeated references have been corrected.
88789	181	35	181	35	Goelzer et al., 2016 (a-b). The references are the same. [Rosemary Vieira, Brazil]	Accepted. Repeated references have been corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42607	181	40	182	2	Why are the values for ISMIP6 (emulated) 3-4 times larger than the ISMIP6 (raw)? Please include an explanation. [Sabine Hüttl-Kabus, Germany]	Taken into account. Table has been removed and contents discussed at greater length in 9.4.
3007	182	8	182	8	Remove 's' from 'from s'. [Petteri Uotila, Finland]	Not applicable. Table has been replaced with table in main text 9.6.3.2.
103871	182	8	182	8	from s -> from [Philippe Tulkens, Belgium]	Not applicable. Table has been replaced with table in main text 9.6.3.2.
80891	182	8	182	8	from s -> from [Louise Sandberg Sørensen, Denmark]	Not applicable. Table has been replaced with table in main text 9.6.3.2.
3009	182	9	182	9	Explain briefly what 'alternative treatments' are, now the reader is left puzzled. [Petteri Uotila, Finland]	Not applicable. Table has been removed, details now discussed in 9.6.3.2.
3011	183	2	183	3	Footnote mark (*) is missing from Table 9.A.2. [Petteri Uotila, Finland]	Not applicable. Table has been replaced with table in main text 9.6.3.2.
3013	184	2	184	5	Does 'temporal correlations' mean autocorrelations? If so, the statement that does not accounting autocorrelation overestimates uncertainties seems wrong. When assuming no autocorrelation and independent samples the uncertainties are actually underestimated if time series has autocorrelation. I suggest checking and rewriting. [Petteri Uotila, Finland]	Taken into account. This is now discussed in 9.6.3.2. Because the emulator does not account for temporal correlations, it does in fact generate overly large rate uncertainties. Consider, for example, a projection in time 1 of .1 ± 0.1 m and in time 2 of .3 ± 0.2 m. Without correlations, the change between these two time steps would be 0.20 ± 0.22 m. However, suppose that there was a perfect correlation in GMSL between these two time steps, such that the low end projects grows from 0.0 m to 0.1 m, and the high end grows from 0.2 to 0.5 m. The variance of the change would be given by 0.1^2 + 0.2^2 - 0.1 * 0.2, so the change between the two time steps would be 0.20 ± 0.17 m. Moreover, failing to account for the correlation would allow for a fall between time 1 and time 2, though there may be no physics to justify such a fall.
85335	185	0	185	0	Fig 9.1 - nice fig but at least in my print out not very clear, perhaps due to insufficient pixel resolution? I also wonder if the n umbers in the panels could be made clearer by using different colours or fonts? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Higher resolution in final version
35329	185	1	185	1	Figure 9.1 :The color of ice sheet, snow and ice is very similar, could the authors pick higher contrasts between the three? [Etienne Pauthenet, France]	Accepted. Colours improved
359	185	1	185	1	The volume of glacier of 0.5 m sea level equivalent is too high (see Farinotti et al., 2019) [Etienne Berthier, France]	Accepted. Revised number to 0.3m
97051	185	1	185	7	Small circular graphs in 9.1a are too small and text is unreadable. Please increase font. Numbers in 9.1b are not explained, please make reference to Table 9.3. [Nicole Wilke, Germany]	Noted. It is the speed of the surface ocean currents. Clarified in caption.
83333	185	1	185	7	Figure 9.1, top schematic - Please mark "Sea ice" in the top right in the ocean adjacent to the Antarctic Ice Sheet. [Robert Massom, Australia]	Noted. The sea ice distribution is apparent in the lower panel.
98709	185	1	185	7	The top panel of this figure is very cryosphere centric. While many different cryosphere processes are labeled, the ocean only has "ocean currents" "steric expansion" and some crude unlabeled arrows intended to show the overturning circulation. There are many more ocean processes in the interior mentioned in the text - e.g. mixed layers, thermocline, mesoscale eddies, overflows, which should be added. The ocean currents can be made more specific, e.g. Western boundary currents, ocean gyres. The MOC upper and lower cells (e.g. in the South) need to be labeled. [Sonya Legg, United States of America]	Noted. The lower panel is intended to provide the geographic perspective, where the ocean and its surface currents are given room.
98711	185	1	185	7	Bottom 3 panels of this figure: I find it hard to distinguish the different colors for ice-sheet, snow, and sea-ice. Sea-ice and snow in particular are so similar, I thought sea-ice had erroneously been placed on the continents. [Sonya Legg, United States of America]	Accepted. Colours improved
100215	185	1	223	1	General comment about all figures: For the sake of accessibility, I think there should be more detail in some of these figure captions. [Carlye Peterson, United States of America]	Accepted. Captions have all been rewritten with greater detail. Furthermore, supplementary information on all figures and their underlying data is being prepared for inclusion.
3947	185	3	185	3	include grounding line in small right circle [Sabine Baumann, Germany]	Accepted. Added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31599	185	3	185	3	On the subfigure showing total water levels on the beach: the figure should preferably be modified so that the wave setup takes place only once the waves have broken (in practice it is true that regional wave setup can take place further offshore, but nothing indicates this in the figure). Furthermore, for clarity, some arrows could be added to show what are values referenced with respect to a geodetic framework (mean sea level, runup if added to the figure), and the quantities that correspond to contributions that can be summed to others (tidal range, storm surge, wave setup, swash) [Gonéri Le Cozannet, France]	Rejected. This level of detail not possible in space allowed.
22695	185	3	185	4	The text in the two circular callouts in the top panel is too small to be legible [Peter Thorne, Ireland]	Accepted. Text enlarged and differentiated in colours
3015	185	3	185	4	Should terrestrial and subsea permafrost be marked separately in Figure 9.1? Now only terrestrial permafrost is marked, although their impacts on sea level can be expected to be quite different. [Petteri Uotila, Finland]	Rejected. This level of detail not possible in space allowed.
101931	185	3	185	5	Figure 9.1: Generally this figure is not intuitive, many of the labels need explanation. A figure that is that prominent in the report should be self-explanatory. [IAPSO ECS group review, United States of America]	Accepted. Caption improved
107479	185	3	185	7	a and b are not labeled on the figure [Jennifer Walker, United States of America]	Accepted.
107481	185	3	185	7	Are there units for ocean speed? Or is it just relative? [Jennifer Walker, United States of America]	Noted. It is actually based on speed from an observational dataset, but the colormap is not optimized for quantitative interpretation.
107483	185	3	185	7	"Patterned ground" is labeled on the figure, but this term does not appear anywhere in the text [Jennifer Walker, United States of America]	Accepted.
66453	185	3			Comment to Figure 9.1a: This figure very well presents processes and interactions between cryosphere and the Ocean. I'm aware it is a scheme and presents many proccesses so it has to be general, but for sake of being precise I would suggest correcting the presentation of accumulation process. Firstly, "accumulation" text is placed on what seems to be an ablation zone (I see surface melt there). So maybe it would be good to extend the white-ish color (which I see on the top of the representation of Antarctic Ice Sheet) further down to be covered by "accumulation" text. Secondly, accumulation is not presented by any graphical form - I would like to suggest placing the cloud and snow (snow-example of accumulation) above the AIS (i.e. accumulation), not mountains. [Barbara Barzycka, Poland]	Noted. This level of detail not possible in space allowed.
66455	185	3			Comment to Figure 9.1a: I see white ribbons representing thermohaline circulation. Maybe it would be an asset to add a description ("THC") and/or colour gradient representing changes in temperature?... [Barbara Barzycka, Poland]	Accepted. Caption improved
66457	185	3			Comment to Figure 9.1a: I would definitely add a description/appropiate color gradient to white ribbon between "meltwater runoff" and "land water storage" - to be honest, I have no clue what it represents... [Barbara Barzycka, Poland]	Accepted. Arrows revised.
66459	185	3			Comment to Figure 9.1b: Firstly, very good idea of using three projections, so not only the Arctic and Antarctic is highlited (in comparison to figure 4.1 in the AR5). However… is the colour gradient for the Ocean necessary (i.e. speed)?... I guess this is to show the dynamics of the Ocean and its interaction with the cryosphere. But this overshadows the overall message of this figure - the crysopheric components are hardly visible. There is no scale of the speed, I haven't found any direct reference to this component of the figure, therefore I guess the speed is mainly for its "pretty" effect... My suggestions to improve the figure: - change the Ocean's speed for a simple scheme of THC/ocean currents; - change the presentation of the speed to simple vectors (i.e. arrows) on a pale background instead the gradient; - change the saturation of the gardient, it will be less vivid thus maybe less "eye-catching"; or - present the Ocean as a plane blue color, the interactions and dynamism is shown on Figure 9.1a. The last suggestion is "fair" to the cryospheric components on this figure, e.g. ice sheets are only outlined, there is no gradient for their flow (i.e. ice sheet's speed). In case of the first suggestion (THC/ocean currents scheme), a transparency to the sea ice could be added so the processes in the Ocean, under the sea ice, are visible as well - this way, the Ocean-cryosphere interactions are better recognised. [Barbara Barzycka, Poland]	Noted. The lower panel is intended to provide the geographic perspective, where the ocean and its surface currents are given room. The upper panel highlights the cryosphere.
66461	185	3			Comment to Figure 9.1b: (detail) Maybe it's the resolution of the image but numbers of RGI in some places blurrs with the bacground. I would suggest adding "halo" or "shadow" effect to the numbers, there's also almost no distinction in colours of sea ice and snow. [Barbara Barzycka, Poland]	Accepted. Colours improved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88037	185	3			Fig. 9.1(b): glaciers in East Afica and Irian Jaya are missing. [Georg Kaser, Austria]	Noted. At this resolution they do not appear in the datasets used to construct the figure. Furthermore, they are not numbered in the glacierized region convention here.
103873	185	4	185	4	The colour of the numbers in Fg 9.1 b makes them difficult to read. [Philippe Tulkens, Belgium]	Accepted. Colours improved
80893	185	4	185	4	The colour of the numbers in Fg 9.1 b makes them difficult to read. [Louise Sandberg Sørensen, Denmark]	Accepted. Colours improved
45331	185	4	185	9	I would encourage a better color scheme. Colors tend to become blurry, e.g. between ice sheet, snow and sea ice, and the ocean colors too - what does the gradient green and blue refer to speed up and speed down, respectively? [Kristian Kjelden, Denmark]	Accepted. Colours improved
100033	185	6	185	6	Subfigures (a) and (b) of Figure 9.1 should be marked within the figure. Also it will probably be good to include periglacial mountain permafrost in the schematic diagram given in Figure 9.1a. [Lydia Sam, Sweden]	Rejected. This level of detail is not graphically possible
67089	185	6	185	7	Fig.9.1: upper part: the figure wrongly suggests that there is surface melt only in Greenland and not Antarctica, but calving and accumulation in Antarctica but not in Greenland. All components happen in both ice shsets. [Regine Hock, United States of America]	Rejected. As with sea ice, it is not necessary to have this level of detail in this schematic.
67091	185	6	185	7	Fig.9.1: the lower figure is not very helpful and largely confusing. What is the purpose? As it is it may better be deleted. Other comments: First, the color scale seems odd since this is not the same variable. 1) Each component should have its own color box rather than a continuous bar. 2) the numbers seem quite meaningless to a reader. What is the purpose. The color orange makes clear where there are glaciers and regions can be subdivided in many other ways. 3) It is unclear why 'glaciers' has a sliding color scheme? What does it mean. If the idea is presence of glaciers than one single color is enough? 4) What is ocean speed? Where? 5) Wha does 'Snow' mean? Thee is snow almost anywhere on Earth at some point? What is the variable? 5) Several components happens simulaltaneously e.g. there can be snow on permafrost, snow on glacier, glacier on permafrost ... 6) SLE of both ice sheets and glaciers should be given with same number of decimals. Mountain glacier number is 0.4, Farinotti et al., 2019 [Regine Hock, United States of America]	Rejected. The lower panel is intended to provide the geographic perspective, where the ocean and its surface currents are given room. The upper panel highlights the cryosphere..
101951	185	6	185	7	The caption is too brief. It is unclear where some of the information in the figure originates from and concepts presented in the figure are not self-explanatory. [IAPSO ECS group review, United States of America]	Accepted. Caption improved
12173	185	6	185	7	Fig 9.1. : Consider using more different colors, especially for snow, sea ice, and ice sheets. “non-frozen land” is missing from the legend. [Thomas Lavergne, Norway]	Noted. Colours improved
62097	185	6	185	7	Refer to Table 9.3 in the figure caption, to make it clear to reader where the informaion for the glacier regions can be found, otherwise the numbers on the figure are not helpful. E.g. (b) Geographic distribution of ocean and cryosphere components (numbers indicate glacierized regions, detailed in Table 9.3). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Caption improved
98713	185	6	185	7	The caption needs to indicate what the sea-ice extent corresponds to - e.g. maximum winter extent? Over which period? Similarly for snow - locations with N days of snow of depth M? [Sonya Legg, United States of America]	Accepted. Caption improved
54495	185	6	185	7	Please include the meaning of "GIA" (glacial isostatic adjustment) and "SLE" (potential sea level equivalent) in the figure caption (Figure 9.1). [Maria del Pilar Bueno Rubial, Argentina]	Accepted. Caption improved
32195	185	6			Figure 9.1: I recommend printing the numbers in the maps in black, at least the number for the Alps and the Canadian Arctic are difficult to read on some maps. [Anja Wendt, Germany]	Noted. Colours improved
88791	185	7	185	7	Figure 9.1 (...numbers indicate glacierized regions). Which regions? See Figure 9.22 for glacierized regions? [Rosemary Vieira, Brazil]	Accepted. Caption improved
27403	185		185		The representation of islands here is is not relevant. It should represent steep flanks down to the ocean floor as for most volcanic islands that form SIDS. Same for the continental margin representation where the influenece of the shelf and slope is not represented. Processes that take place in these boundaries are mentionned as important processes in some cases (e.g. for Antarctic fjords and ice covered lands). It is recognized that these processes are difficult to account due to their complexity as mentionned in section 9.2.3.5.2 Coastal Systems and Marginal Sea. There is no seafloor topography in the scheme, despite their role in the THC is starçing to be accounted . They still need to be identified in the scheme and their contribution to vertical mixing accounted (e.g. DOI: 10.1038/ncomms14197). Could this be developped ? [Eric Brun, France]	Noted. Unfortunately in such a high-level schematic these aspects cannot be managed due to the aspect ratio, etc. Thus, the text in the section mentioned has to serve this role.
27405	185		185		Some texts on the figure are illegible [Eric Brun, France]	Accepted. Text enlarged and differentiated in colours
27407	185		185		a) and b) and their respective legends are missing on the Figure [Eric Brun, France]	Accepted. Added lettering.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27409	185		185		In figure (b) it would also be necessary to list verbatim the glacierized regions. [Eric Brun, France]	Noted. The list of these regions is given in later figures in Section 9.5, now explicit in caption
101933	185		185		Figure 9.1: The caption mentions panels/subfigures a) and b), but the figure consists of 4 panels, and what a) and b) refers to is not clearly marked. [IAPSO ECS group review, United States of America]	Accepted. Added lettering.
101935	185		185		Fig. 9.1a (?): The abbreviations "mCDW" and "GIA" are not clear/defined. [IAPSO ECS group review, United States of America]	Accepted. Added to caption.
101937	185		185		Both bubbles associated with upper figure (9.1a?) are too small, in particular their fontsize. [IAPSO ECS group review, United States of America]	Accepted. Text enlarged and differentiated in colours
101939	185		185		Fig. 9.1a (?): The process labled "stable" in the lower right circle is unclear. The arrows are pointing to lithospheric bumps/bathymetric highs, but it is unclear whether this suggests isostatical, tectonical, or water-mass stability. [IAPSO ECS group review, United States of America]	Accepted. Noted in caption.
101941	185		185		A legend to explain some of the arrows in the top image (Fig. 9.1a ?) could be helpful, e.g. is that land-ocean arrow indicating water shift, energy shift (might be the solid gray arrows, but there are wide and narrow solid grey arrows). [IAPSO ECS group review, United States of America]	Accepted. Noted in caption.
101943	185		185		Figure 9.1b (?): It is not clear what glacierized regions the numbers are referring to. It would be helpful if they were cross-referenced to a table where the regions are named/explained. [IAPSO ECS group review, United States of America]	Noted. The list of these regions is given in later figures in Section 9.5, now explicit in caption
101945	185		185		Figure 9.1b (?): The contrast in color between land, snow and ice sheet is not big enough to be easily recognized. This applies to the figure itself as well as the colorbar. [IAPSO ECS group review, United States of America]	Accepted. Colours improved
101947	185		185		Orange numbers in lower part of the figure (9.1b ?) are hard to see. The authors may consider chosing a different color for glaciers or have white as a background color. [IAPSO ECS group review, United States of America]	Accepted. Colours improved
101949	185		185		Figure 9.1b (?): What does "ocean (speed)" mean, is it vertical or horizontal movement, which color indicates which direction of movement, is it even relevant for sea level? [IAPSO ECS group review, United States of America]	Noted. It is the speed of the surface ocean currents. Clarified in caption.
61507	185		185		Figure 9.1 caption: I would suggest that (b) of the figure caption states that the numbers indicate the GTN-G glaciarized regions specifically. "glacierized regions" only is a bit vague. Also is the choice of wording "glacierized regions" correct? I see online that the word "glacierized" refers to regions affected by the action of glaciers. However, the GTN-G refers to the Global Terrestial Network for Glaciers, i.e. active glaciers. Should the word be changed to "glaciated regions"? Also, for (a) of the figure caption, i would suggest to ad the following information "Schematic of processes that have an impact on SLC". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The list of these regions is given in later figures in Section 9.5, now explicit in caption
55147	185		185		For the 3 maps in the bottom half of Figure 9.1, the color contrasts between sea ice, snow, and ice is too subtle. Information content would be improved with better choice of colors for these three elements. [Nancy Hamzawi, Canada]	Accepted. Colours improved
109229	185		185		Figure 9.1: Color bar is not labelled properly. Glacier regions are labelled which should be annoted like Figure Atlas.2 (Page 181) [A.K.M Saiful Islam, Bangladesh]	Rejected. The list of these regions is given in later figures in Section 9.5, now explicit in caption
116911	185		185		Why "collapse" with permafrost? [Valerie Masson-Delmotte, France]	Accepted. Phrase removed.
61753	185				In Figure 9.1: a) and b) are missing in the images. Furthermore, I think it would be better to divide this figure into two figures, instead of a) and b) to have Figure 9.1 and Figure 9.2 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Addition of "a)" and "b)" accepted and suggestion to divide the figure rejected. The combination of these two figures solves a lot of space and referencing problems so will not be changed.
61159	185				The inset with relative sea level, astronomical tide , storm surge etc is a little difficult to comprehend visually. [Udita Mukherjee, United States of America]	Accepted. Text enlarged and differentiated in colours
33523	185				Figure 9.1: Why ocean have two colours? Identify green and blue differences; different ranges of current speed?. [Guiomar Rotllant, Spain]	Noted. It is the speed of the surface ocean currents. Clarified in caption.
22697	186	1	186	1	It is nice to call out other chapters in this way but this would need adopting for all chapters? Text is borderline too small to be legible in print version though? [Peter Thorne, Ireland]	Accepted.
67093	186	1	186	3	This figure is a waste of space and can be deleted. It adds nothing to the table of contents. [Regine Hock, United States of America]	Rejected. This figure has received many positive comments and is critical for later referencing throughout the chapter.
97053	186	1	186	25	The visual guide in fig. 9.2 is a good idea, but visually not very appealing. Please consider larger font, especially in the quick guide. [Nicole Wilke, Germany]	Accepted. This figure has been improved in revisions.
32197	186	3			It would be very helpful if this guide was interactive allowing to navigate to the respective chapters. [Anja Wendt, Germany]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
101953	186		186		Figure 9.2: Fontsize in the quick guide seems slightly too small. [IAPSO ECS group review, United States of America]	Accepted. Enlarged as possible given space constraints.
116913	186		186		please consider links to other chapters to help readers navigate in the report [Valerie Masson-Delmotte, France]	Accepted. These links are given now.
85337	187	0	187	0	Fig 9.2 - This figure is not very clear (and some of the panels were not yet completed which made it harder to fully assess the text). In particular could the colour scales be stratched (or made so levels were irregular) to better show up the anomalies and differences in regional warming signals (including polar amplification)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure redrawn and caption revised.
103877	187	0	187	1	Figure9.3. Is the unites on the small figures in the upper left corner the same as the upper right? I would suggest that all axis are provided with labels. [Philippe Tulkens, Belgium]	Accepted. Modified as suggested.
80895	187	0	187	1	Figure9.3. Is the unites on the small figures in the upper left corner the same as the upper right? I would suggest that all axis are provided with labels. [Louise Sandberg Sørensen, Denmark]	Accepted. Modified as suggested.
35331	187	1	187	1	Figure 9.3 : Could the authors add SSP370 and SSP245 in color on the SST anomaly timeserie, the same way it is done on figure 9.6? [Etienne Pauthenet, France]	Accepted. Modified as suggested.
22699	187	1	187	1	Addition of an overarching figure title would increase accessability [Peter Thorne, Ireland]	Accepted. Modified as suggested.
61355	187	1	187	1	In Figure 9.3 : Top Left Panel should have an y axe label. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61357	187	1	187	1	In Figure 9.3 : Top Right Panel should have an y axe label. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61359	187	1	187	1	In Figure 9.3 : Title for the top pannels should be placed above /outside the right pannel to allow the reader to understand that the title is for the two pannels easily. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61361	187	1	187	1	In Figure 9.3 - Top Right Pannel: Be careful that the x axe is croped, we cannot see the last zero of 2100 [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61363	187	1	187	1	In Figure 9.3 - Bottom pannels - Allow more space between bottom and top pannels will help the visibility of the figures. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61365	187	1	187	1	In Figure 9.3 - Bottom pannels - As it is menitonned '1950-2014 SST Change' I would recommend to place similar title above top left pennel such as '1995-2014 Climatology HadISST'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
45289	187	1	187	1	Is there a particular reason to use 1970-1980 as climatology? My understanding is that climatology is often defined over a longer period (often ~30 year period). Using a 10 year period as climatology can bias the y-axis of the SST anomaly timeseries and can create confusion about the 1.5C warming target. [Anson Cheung, United States of America]	Noted. Baseline adjusted to 1950-1980, which is the longest possible in all datasets shown and is consistent with the assessed change "since 1980"
45291	187	1	187	1	The paleo SST anomaly on the top left panel is confusing. Why is there a break between positive and negative? Also, what are the sources of these data? What do the bars mean? [Anson Cheung, United States of America]	Accepted. Upper left panels combined, labelled, and data sets described in caption/data tables
7349	187	1	187	2	Higher resolution needed in this figure, same goes for figure 9.9, 9.23, 9.25, 9.26 and 9.32 and FAQ 9.3, Figure 1 [Svenja Halfter, Australia]	Accepted. Modified as suggested.
100253	187	1	187	9	Instead of referring to the figure panels as “top”, “top left”, etc. please use letters. [Carlye Peterson, United States of America]	Rejected. Figure redrawn and caption revised.
100255	187	1	187	9	To save space, use abbreviations in the figures and be sure to define them in the caption as this is nicely done in the caption for SST. For example, if you need the label in the upper middle-right “timeseries” plot, simply use “SST Anomaly (°C) Timeseries” (but I'm not sure the sub-label that says “from paleo data...” helps clarify, I think it's actually more confusing), and also in the left-most lower panel color bar, label it as “SST (°C). Another example is in the upper left panel, Before Present (BP), simply use BP in the figure label. [Carlye Peterson, United States of America]	Accepted. SST is defined and used throughout. Overarching title introduces plotted variable and Y-axis labels are units consistent with IPCC recommendations.
100257	187	1	187	9	Is the upper left panel supposed to be a part of this figure? Is it model bias (°C) in the geologic past? I’m not sure what the point is in this figure. I'm not sure I understand why the upper left panel is split into two sections when the y-axis of the two sections both have zero….and it seems that the upper portion can end at 3 or so (are the units bias in °C?). [Carlye Peterson, United States of America]	Accepted. This upper left panels has been combined and labelled
100259	187	1	187	9	Which CMIP version does “CMIP models” refer to, CMIP5, CMIP6? [Carlye Peterson, United States of America]	Accepted. Clarified in overarching title
100261	187	1	187	9	HighResMIP doesn’t need to be defined in the figure if it is defined in the caption (lower-most middle panel). [Carlye Peterson, United States of America]	Accepted. Modified as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100263	187	1	187	9	Are the middle and rightmost map panels supposed to demonstrate the regional rates of change as mentioned in the text (p. 12, lines 46-47)? Are they the mean SST relative to the 1970-1980 climatology like in the upper timeseries panel? Or is it the magnitude of change across that time period? If it's a rate of change shouldn't the units be something like °C/decade or something more similar to what is written in the text? [Carlye Peterson, United States of America]	Accepted. These are rates of change and the units/labels have been clarified.
100265	187	1	187	9	I don't think the black arrows across the middle are helpful, if you want to demonstrate that the middle and right map columns correspond to those time ranges in the timeseries figure, I think some vertical grey dashed lines in the timeseries panel would be more effective and less confusing because the time period is labeld in the figure making the arrows redundant. [Carlye Peterson, United States of America]	Accepted. Figure redrawn
100267	187	1	187	9	In the caption re-write below, I'm assuming the upper left split panel is one sub-figure, the time series is another, and each box within the bottom figure is a separate sub-figure to be labeled alphabetically. Caption re-write: Sea Surface Temperature (SST) changes through time. (a) model bias °C in the geologic past (b) timeseries plot of SST anomaly relative to 1970-1980 climatology from historical CMIP (black), HadISST observations (orange), High-Resolution Models (HighResMIP) (green), and the SSP future projection experiments including envelopes of uncertainty (95% CI? 90%?) (SSP585 in dark red, two others in red and yellow, and SSP126 in dark blue). (c,e,h) Leftmost columns shows maps of mean SST of (c) HadISST observations and model bias for (e) CMIP and (h) HighResMIP, (d,f,i) Center columns show maps of SST change from 1950-2014 for (d) observations, (f) CMIP models, (i) HighResMIP, (g,j) Rightmost columns show maps of SST change projections for 2015-2079 under SSP5- 8.5 from (g) CMIP models and (j) HighResMIP. [Carlye Peterson, United States of America]	Rejected. Figure redrawn and caption revised.
97055	187	1	187	10	Fig. 9.3 appears a bit too busy. Can it be split into two? [Nicole Wilke, Germany]	Rejected. Figure redrawn and caption revised.
82647	187	1	187	10	Is there a reason for the use of a 1970-1980 climatology here? [Blair Trewin, Australia]	Noted. Baseline adjusted to 1950-1980, which is the longest possible in all datasets shown and is consistent with the assessed change "since 1980"
88793	187	3	187	3	Figure 9.2 Sea Surface Temperature (SST) and its changes with time. What does mean the stippled area (ltop eft column)? [Rosemary Vieira, Brazil]	Accepted. Observation stippling has been removed.
34499	187	3	187	9	Figure 9.3 caption: - information on upper left panel (paleo data) is missing. - what do the dotted areas on the observation map represent? [Claire Waelbroeck, France]	Accepted. Observation stippling has been removed.
54497	187	3			Figure 9.3. "Dummy Fig." ...delete and re-write the epigraph. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. Figure redrawn
84037	187	4	187	5	At the Top panel, all SSP experiments should be identified. Just so it matches the Image, change the order in line 4-5 to "Shown are observations (HadISST), and multi-model means from the CMIP historical, the HighResMIP experiment, and SSP experiments". [Marco Tulio Cabral, Brazil]	Noted. Figure redrawn and legend revised.
101955	187		187		Figure 9.3, top-left panel: It is unclear what this panel shows. The panel is not explained in the caption, and it is not stated where the estimates of the different time periods originate from. There is no label that explains what the y-axis shows. [IAPSO ECS group review, United States of America]	Accepted. Panel has been clarified.
101957	187		187		Figure 9.3, top-left panel: What is the reason for splitting the top-left panel into two parts? This is confusing to me. The zero line could just be highlighted. [IAPSO ECS group review, United States of America]	Accepted. Combined into one panel
101959	187		187		Figure 9.3, top-left panel: The upper and lower part of this panel do not seem to align, with the lower one being slightly wider. This is noticable in particular when looking at the black line. [IAPSO ECS group review, United States of America]	Accepted. Modified as suggested.
101961	187		187		Figure 9.3 seems to be cut off at the right side, the last zero of 2100 is not fully visible. [IAPSO ECS group review, United States of America]	Accepted. Modified as suggested.
101963	187		187		Figure 9.3: Lower left two panels (Model Bias): It seems as if the range for the colorbar could be reduced at both ends since the maximum values are not reached on the maps and it looks rather pale. [IAPSO ECS group review, United States of America]	Accepted. Modified as suggested.
116915	187		187		This chapter makes no comment on the upward revised SST increase which is important for the estimated current level of warming, it could be highlighted (in relationship with the upper panel) (this could also be done for the maps displaying past ice sheet extent (agreement of evidence assessed?). [Valerie Masson-Delmotte, France]	Noted. Text covers this topic.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
85339	188	0	188	0	Fig 9.3 - How robust are these trends given the known issues with flux products and impacts of changes in observational densities on trends in all re-analysis based products? Should a qualification statement be included? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The multi-product uncertainty was not assessed, but the same technique to determine trend significance is used as in Yu (2019). The stippling now is used to indicate regions where trend is insignificant. Caption provides details
80897	188	0	188	1	Figure 9.4: time span not provided for 'observed fluxes, wind stress magnitude' figure [Louise Sandberg Sørensen, Denmark]	Accepted. Modified as suggested.
103881	188	0	188	1	Figure 9.4: time span not provided for 'observed fluxes, wind stress magnitude' figure [Philippe Tulkens, Belgium]	Accepted. Modified as suggested.
80899	188	1	188	1	Is the observed Freshwater flux figure actually a figure of change/rate in freshwater flux? Maybe just a matter of terminology, but could be good to clarify. [Louise Sandberg Sørensen, Denmark]	Accepted. Rates of change, observations, and trends have been clarified.
22703	188	1	188	1	Clear mismatch between the figure middle column and the caption [Peter Thorne, Ireland]	Noted. Figure redrawn and caption revised.
61367	188	1	188	1	In Figure 9.4- For clarity, may I suggest to place date of each observed map as subtitle over each one, in order to have only information with unit on the left side, as it is an info for all the 3 columns. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Units vary by trend and by value
61369	188	1	188	1	In Figure 9.4- Please, can you use 'yr^-1' instead of /yr [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61371	188	1	188	1	In Figure 9.4- Please remove the empty ' () ' in the Wind Stress subtitle [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
103885	188	1	188	1	Is the observed Freshwater flux figure actually a figure of change/rate in freshwater flux? Maybe just a matter of terminology, but could be good to clarify. [Philippe Tulkens, Belgium]	Accepted. Rates of change, observations, and trends have been clarified.
98715	188	1	188	7	In the lower left panel of this figure, the time period over which the observed wind stress magnitude is compiled is missing. [Sonya Legg, United States of America]	Accepted. Modified as suggested.
88795	188	2	188	2	Figure 9.4: Global maps of (left) observed mean fluxes. What does mean the sippled area (bottom left column)? [Rosemary Vieira, Brazil]	Accepted. Stippling now defined on figure
101965	188		188		Figure 9.4: The time range for wind stress magnitude is missing on the bottom left. [IAPSO ECS group review, United States of America]	Accepted. Modified as suggested.
101967	188		188		Figure 9.4, right hand column: The label "CMIP6 trends 2015-2100" over the right column needs clarification: which scenario is used? Is it the multi-model ensemble mean? Do models agree well on the sign of the trends (if multi-model agreement is available, it would be a useful information to plot). [IAPSO ECS group review, United States of America]	Noted. Figure redrawn and legend revised.
101969	188		188		Figure 9.4, right hand column: This does not look like what we would expect for 21st century trends under increased global warming. The fact that the ssp scenario is not specified reinforces our suspicion that the trends shown seem to be more coherent with the observational period. [IAPSO ECS group review, United States of America]	Noted. Figure redrawn and legend revised.
101971	188		188		Figure 9.4, middle column: Only one period is indicated (1995-2014). This seems inconsistent with the left hand column, as this time range appears to only be valid for the upper panel, in agreement with the time where observations are available. For the net heat flux, a different time period is used in the left hand column (2001-2014), and the trends shown here are supposedly given for the same time period. [IAPSO ECS group review, United States of America]	Noted. Figure redrawn and legend revised.
101973	188		188		Figure 9.4, middle column: The units for the trends in the middle pannel need to be specified. [IAPSO ECS group review, United States of America]	Accepted. Modified as suggested.
101975	188		188		Figure 9.4: It seems as if there is stippling in some panels but in the upper pannel, middle column this is hard to see. Also, this should be mentioned in the caption. [IAPSO ECS group review, United States of America]	Accepted. Stippling now defined on figure
101977	188		188		Figure 9.4: The freshwater flux trend from CMIP6 cannot be evaluated due to the missing figure. [IAPSO ECS group review, United States of America]	Accepted. Modified as suggested.
109231	188		188		Figure 9.4: An extra curly bracket before the unit of Wind Stress Magnitude (N/m2) [A.K.M Saiful Islam, Bangladesh]	Accepted. Modified as suggested.
61179	188		188		In Figure 9.4, units should be given for the figures appearing in the middle and right hand side columns. [Patrick Cummins, Canada]	Accepted. Modified as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84039	188				The caption needs a review, specially because wind stress magnitude is not a flux. We propose the following: "Global maps of (left box) observed mean freshwater fluxes (1995-2014), net heat flux (2001-2014), and wind stress magnitude (1995-2014), and projected (2015-2100) trends (middle and right columns, respectively). The fluxes shown are (top row) freshwater flux, (middle row) net heat flux, and (bottom row) wind stress magnitude, with positive numbers indicating ocean freshening, warming, and accelerating, respectively. Objective Innterpolation from CERES EBAF v4 (Kato et al., 2018), OAFlux-HR (Yu, 2019), and GPCP (Adler et al., 2003) of fluxes and flux trends.". Aslo, the observation period for wind is not written in the Figure. [Marco Tulio Cabral, Brazil]	Noted. Caption revised. The wind stress is indeed a flux (momentum flux), but the comment does reflect confusing wording.
101979	188				Figure 9.4: For the map on wind stress magnitude (observed fluxes), the vector arrows are too small for it to be useful for presenting directionality. Either increase the arrow sizes or leave it out completely [IAPSO ECS group review, United States of America]	Accepted. Arrow size increased
33525	188				Figure 9.4: Change "cm/yr" by "cm yr-1". Homogenise units representation. Add years in wind stress magnitude (). [Guiomar Rotllant, Spain]	Accepted. Modified as suggested.
85341	189	0	189	0	Fig 9.4 - For me this figure was not very clear? Is there a way to adjust the colour scales (and make them different for summer and winter) to better show up model biases and changes in mixed layer depth? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure redrawn
22705	189	1	189	1	The three different colour scales used are really unhelpful for accessability of this image. Adding a self describing overall figure title would also help (more minor) [Peter Thorne, Ireland]	Noted. Overall figure title has been added, 3 colour scales are retained as each shows an incomparable quantity (MLD. Bias, and change)
61377	189	1	189	1	In figure 9.5 - May I suggest to defined MLD acronym in the text legend. It will allow to use it in the title of the first column such as 'Observed MLD'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61379	189	1	189	1	In Figure 9.5 - Please consider placing as generally used, y axe labels (vertically) ,the words 'Winter' and 'Summer'. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Figure redrawn
7351	189	1	189	2	Increase font size of the legend, same goes for Cross-Chapter Box 9.1, figure 9.12, 9.15 and 9.17 [Svenja Halfter, Australia]	Accepted. Modified as suggested.
101981	189	7	189	8	Figure 9.5: The definition for the mixed layer depth can be more specific. I believe what you are trying to define here is the base of the mixed layer following sigma-theta criterion of 0.03 kgm-3 for mixed layer thickness and that this is relative to the density of the near-surface depth of 10m. [IAPSO ECS group review, United States of America]	Accepted. Caption clarifies.
101983	189		189		Figure 9.5: Fontsize of colorbars is too small. [IAPSO ECS group review, United States of America]	Accepted. Modified as suggested.
38107	189		189		The color of Figure 9.5 could be improved (too light). [Junhee Lee, Republic of Korea]	Accepted. Figure redrawn
84041	189				The maps are very tiny, maybe if the Winter/Summer maps were displayed vertically they could be larger? [Marco Tulio Cabral, Brazil]	Accepted. Figure redrawn
33527	189				Figure 9.5: Make figure bigger, impossible to read it properly. For instance, you could put the prediction under the observation [Guiomar Rotllant, Spain]	Accepted. Figure redrawn
80901	190	0	190	11	The numbers on the colourscale are small and difficult to read. [Louise Sandberg Sørensen, Denmark]	Accepted. Figure revised.
103889	190	0	190	11	The numbers on the colourscale are small and difficult to read. [Philippe Tulkens, Belgium]	Accepted. Figure revised.
61383	190	1	190	1	In Figure Cross-Chapter Box 9.1, Figure 1, please consider increasing size of the y axes labels and be careful for wrong croping (top left b) and top right y axes c)) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Figure revised.
98717	190	1	190	11	The caption indicates hatching in panel c, but I cannot see any hatching in the figure. [Sonya Legg, United States of America]	Accepted. Figure revised.
101985	190	3	190	3	Figure 9.6: This figure misses a panel that shows how CMIP models have simulated heat waves over the same time period that is shown for observations. [IAPSO ECS group review, United States of America]	Accepted. Figure revised.
40053	190	3			Cross chapter box 9.1, Fig 1: it is confusing to have different boundaries for the colour scales which all use the same palette. (b) and c) go to 100 the max of a) is 3) [TSU WGI, France]	Accepted. Figure revised.
88797	190	9	190	9	Box 9.1, Figure 1: The hatched areas indicate permanent marine heatwaves. It is not possible to visualize the hatched areas. [Rosemary Vieira, Brazil]	Accepted. Figure revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66007	190	15	106	16	Suggest consideration of "critical levels" defined by the significance of impacts rather than the frequency at which the event occurs. There are many papers that have taken this approach (e.g. Hino et al., Sweet et al., Ray and Foster, Hague et al. as mentioned above). An alternative approach is to define extreme sea level by their "remarkability". See: Moore & Obradovich (https://doi.org/10.1038/s41467-019-13935-3). In the last five years the literature has starting moved beyond defining extreme sea levels in a statistical sense and towards defining in an impact-based sense. Suggest this development be reflected in the report. [Kushla Munro, Australia]	Rejected. This is outside of the assessment supported by the literature considered and extends into WGII territory.
27411	190		190		The shaded areas are barely visible on the map. [Eric Brun, France]	Accepted. Figure revised
101987	190		190		Figure 9.6: Fontsize on colorbars too small [IAPSO ECS group review, United States of America]	Accepted. Figure revised
101989	190		190		Figure 9.6: What are contour levels in b and c? [IAPSO ECS group review, United States of America]	Accepted. Figure revised
101991	190		190		Cross-Chapter Box 9.1, Figure 1: Hatching is hard to see, consider using a lighter color. [IAPSO ECS group review, United States of America]	Accepted. Figure revised.
80903	191	0	191	11	Figure 9.6: what are the uits on the y-axit of the upper figure? [Louise Sandberg Sørensen, Denmark]	Accepted. Units included on either y-axis or colorbars
103893	191	0	191	11	Figure 9.6: what are the uits on the y-axit of the upper figure? [Philippe Tulkens, Belgium]	Accepted. Units included on either y-axis or colorbars
35333	191	1	191	1	Figure 9.6 : The OHC Trend colorbar is stuck between two panels and hard to find. Cold the authors put the OHC Trend and OHC Bias colorbars together, one above the other, at the bottom of the plot? [Etienne Pauthenet, France]	Noted. The colorbars have been adjusted and clarified for the panels.
80681	191	1	191	1	on the observations part of the figure 9.7, the latitude scale is incorrect, the sign '-' is missing and there is one label too many under the top figure (global) [Helene Jacot Des Combes, Marshall Islands]	Accepted. Figure redrawn
61385	191	1	191	1	In Figure 9.6 - Top pannel, please consider placing the a) at the top of it and add an y axes label [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Units included on either y-axis or colorbars
7353	191	1	191	2	Fix legend under e and f [Svenja Halfter, Australia]	Accepted. Modified as suggested.
12511	191	1	191	11	Oppose to use Zanna et al reconstruction, it is not comparable to models [Lijing Cheng, China]	Noted. We have included additional observational and hybrid datasets, and clarified that Zanna et al is a hybrid dataset
3333	191	3	191	3	In Figure 9.6, there's only a comparison between Zanna et al. 2019 and CMIP. Zanna et al. use surface temperature observations and a model to transfer SST anomalies into subsurface heat anomalies. We also have direct subsurface temperature observations (Ishii et al. 2017, Cheng et al. 2016, EN4, World Ocean Atlas etc etc), which agree well with Zanna et al. Including these numbers gives a nice triple accord over the observational period as both SST+model, in-situ obs and CMIP6 models mutually agree about the global OHC changes since ~1960. [Thomas Frederikse, United States of America]	Noted. We have included additional observational and hybrid datasets, and clarified that Zanna et al is a hybrid dataset
67095	191	3	191	3	put (OHC) after it is spelled out the first time here [Regine Hock, United States of America]	Accepted. Modified as suggested.
129611	191	3	191	3	In Figure 9.6, there's only a comparison between Zanna et al. (2019) and CMIP. Zanna et al. use surface temperature observations and a model to transfer SST anomalies into subsurface heat anomalies. There are also direct subsurface temperature observations (Ishii et al., 2017; Cheng et al., 2016; EN4, World Ocean Atlas, etc.), which agree well with Zanna et al. Including these numbers gives a nice triple accord over the observational period as both SST+model, in-situ obs and CMIP6 models mutually agree about the global OHC changes since ~1960. [Trigg Talley, United States of America]	Noted. We have included additional observational and hybrid datasets, and clarified that Zanna et al is a hybrid dataset
55519	191	3			Figure 9.6: "Dummy Figs." ...delete and re-write the epigraph. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. Modified as suggested.
27415	191	7	191	7	It's not graph (e) but graph (c). [Eric Brun, France]	Accepted. Modified as suggested.
27417	191	10	191	10	It's not graph (d) but graph (g). [Eric Brun, France]	Accepted. Modified as suggested.
27419	191	10	191	10	It's not graph (g) but graph (d). [Eric Brun, France]	Accepted. Modified as suggested.
101993	191	10	191	11	Figure 9.6: To be consistent with other parts of the text and the 20-year periods usually chosen, maybe show the change in 2081-2100 rather than 2091-2100 [IAPSO ECS group review, United States of America]	Noted. Baseline period is chosen to match duration of the observation period (2005-2014)
27413	191		191		The unit for OHC should be specified in the legend. [Eric Brun, France]	Accepted. Units included on either y-axis or colorbars
27421	191		191		Why choosing the reference period 2091-2100, when the reference period is 2081-2100 in most of the previous figures? [Eric Brun, France]	Accepted. Figure redrawn

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83031	191		191		Figure 9.6: It is hard to see the comparison of the CMIP simulations and the "observations" for the period 1850-present. I wonder whether the figure couple be split, using different y-axis ranges to make this clearer? Perhaps subjective, but I think of the Zanna et al (2019) as more of a "reconstruction" than "observations". There are no subsurface ocean data included in that product and some of the trends since the 1970s seem at odds with more traditional estimates based on the available subsurface temperature profile data. Probably worth discussing the options and agreeing a way forward (in terms of observational datasets) with Chapters 2, 3 and 7? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure redrawn
101995	191		191		Figure 9.6: This whole figure is clearly about ocean heat content, hence it does not seem necessary for OHC to be labelled in all the panels. [IAPSO ECS group review, United States of America]	Rejected. As with temperature, it is important to clarify that all figures show the same variable
101997	191		191		Figure 9.6: The colour bar for the map on OHC trend should be placed below its associated figure. The current placement makes the reader think that it is for the upper figure instead of the map below. [IAPSO ECS group review, United States of America]	Accepted. Figure redrawn
101999	191		191		Figure 9.6: The observed and simulated changes in OHC cannot be reviewed properly due to missing figures in Figure 9.6. It is not understandable why observation based figures are missing at this stage of the review process. [IAPSO ECS group review, United States of America]	Noted. This was an issue in cross-chapter decision-making about which data are the correct ones to use for a basis for bias
109233	191		191		Figure 9.6: There is an inconsistency of the periods shown in the maps of the observed trends of OHC for (b) 0-700m for the period 1971-2014, and (e) 0-2000m for the period 2005-2017. Is it possible to plot OHC for the same period for different depth layers? [A.K.M Saiful Islam, Bangladesh]	Rejected. It is not possible to do so due to the lack of observational data below 2000m
83033	191		192		Could figure 9.6 and 9.7 be merged? I think it would be useful to show the depth profile of total ocean heat uptake in a additional panel, if possible (perhaps indicated the % represented in the 0-700 m and 0-2000m layers?) [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. But both figures redrawn
33529	191				Figure 9.6: I would suggest to separate units (m) from number in the figure. [Guiomar Rotllant, Spain]	Accepted. Figure redrawn
98005	192	1	192	6	The panels in Fig 9.7 are very small and are difficult to see (lots of whitespace). Enhancing this to be larger and more visible would be preferred [Paul Durack, United States of America]	Accepted. Figure redrawn
27423	192		192		Why choosing the reference period 2091-2100, when the reference period is 2081-2100 in most of the previous figures? [Eric Brun, France]	Noted. Baseline period is chosen to match duration of the observation period (2005-2014)
33531	192				Figure 9.7: I would suggest to put the temperature bars in the top of the figure and add information in the x axis in the bottom of the figure (eg. Latitude, N & S). [Guiomar Rotllant, Spain]	Accepted. Figure redrawn
85343	193	0	193	0	Fig 9.8 - Is it worth defining T change and V change in the captions as they are labelled on the figure panels but not described in the caption? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Figure and caption redrawn
35335	193	1	193	1	Figure 9.8 : What are the red dots on the pannel (d)?, Please add a legend. [Etienne Pauthenet, France]	Accepted. Legend revised and text added.
98843	193	1	193	1	The meaning of the two data clouds in Fig. 9.8, panel d, were not immediately obvious to me. I think that some additional labeling of the clouds, like "Gyre heat transport" and "Overturning heat transport" would help the reader to make sense of this figure. [Robert Hallberg, United States of America]	Accepted. Clouds labelled.
19271	193	1	193	1	Figure 9.8, panel d: the two clusters of points should be explained by labels "gyre" and "total" in the drawing area, the legend is not sufficient to grasp the meaning at first sight. [Anne-Marie Treguier, France]	Accepted. Clouds labelled.
22707	193	1	193	1	The right hand panel axis values and key are borderline too small to be legible in print [Peter Thorne, Ireland]	Noted. Figure altered and sizing improved.
61395	193	1	193	1	In Figure 9.8, right pannel, Please consider addition ' ° ' before 'N' to mention the posiiton [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61397	193	1	193	1	In Figure 9.8, pannels c), I am sorry I cannot understand the variable contained under the 'V' symbol on the contrary of 'T' refering to temperature in b). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Figure altered and caption improved.
7895	193	1	193	1	The figure key does not make sense - I can see red symbols which are not in the key but not cyan which are. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Legend revised and text added.
98719	193	1	193	9	In panel d of figure 9.8, the caption does not indicate what the colored triangles represent. The black circles are obs from Rapid for the total, the black triangles are the obs from Rapid for the gyre. Does the same convention apply for the models: split into total and gyre components, circles and triangles? If so, this needs to be indicated more clearly. [Sonya Legg, United States of America]	Accepted. Clouds labelled.
97057	193	1	193	28	The two different lines in d are neither referenced in the text nor properly explained in the caption. Based on the RAPID legend I assume it is the total versus the gyre component. Are both necessary? [Nicole Wilke, Germany]	Accepted. Clouds labelled.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34501	193	6	193	7	"V" in Fig. 9.8(c) should be defined. [Claire Waelbroeck, France]	Noted. Figure altered and caption improved.
88799	193	7	193	9	Figure 9.8: (d) Relationship between northward heat transport and Atlantic Meridional Overturning Circulation in HighResMIP models (1950-2050) and observations during the RAPID period (2004-2018). RAPID-MOCHA Program is not mentioned in the text. [Rosemary Vieira, Brazil]	Noted. Figure altered and caption improved. RAPID now discussed in text.
102001	193		193		Figure 9.8: Consider labeling the two datapoint clouds in (d), i.e. total for the top and gyre for the bottom. How are gyre and total defined? [IAPSO ECS group review, United States of America]	Accepted. Figure redrawn
102003	193		193		Figure 9.8d: The legend does not fully explain the symbols on the figure. There are red scatterpoints on the figure, but the legend does not have any red point. It does have blue points, which are not on the figure. Moreover, the differentiation between circles and triangles is only shown for the observations, not the model. It should be clarified that these symbols apply to both observations and models [IAPSO ECS group review, United States of America]	Accepted. Legend revised and text added.
109235	193		193		Figure 9.8: "T Change" and "V Change" should be elaborated to improve readability. [A.K.M Saiful Islam, Bangladesh]	Noted. Figure altered and caption improved.
33533	193				Figure 9.8: In figure b) T is temperature? In figure c) V is ? Please add this information. [Guiomar Rotllant, Spain]	Noted. Figure altered and caption improved.
79049	194	1	194	1	HadCRUT is not an SST data set. HadSST4 is the most recent SST data set which is used for the ocean component of HadCRUT5. [John Kennedy, France]	Noted. This data set has been removed, and clarification provided for other data sets
98721	194	1	194	8	I think it would be helpful to have an additional panel or inset, zooming into the timeperiod around the present day, to show in particular the timescale for the peak GMSST etc. [Sonya Legg, United States of America]	Accepted. Zoomed in present day time period added.
22709	194	1	194	8	What is the HadCRUT product labelled in the graph? There is no explanation in the caption. HadCRUT is a merged land and ocean surface temperature product. Which version are you using? And do you instead mean HadISST or HadSSTv4? You should reference the primary source which is not Durack et al or Johnson et al. [Peter Thorne, Ireland]	Noted. This data set has been removed, and clarification provided for other data sets
68553	194	1	194	9	I think that Figure 9.9 presents a very effective message, and I particularly like the perspective that comes from the very long and unscaled time axis. [Robert Hallberg, United States of America]	Noted. Thank you.
88801	194	3	194	5	Figure 9.9: Measured changes in past mean ocean heat content OHC (20 kyr BCE to present) estimated from ice-core rare gases. Do the different colors represent different sources or just distinct values? [Rosemary Vieira, Brazil]	Noted. Some of these datasets have been changed, and the legends have been modified to clarify data sources
80905	195	0	195	12	legend on lower right figure too small making it difficult to read. [Louise Sandberg Sørensen, Denmark]	Accepted. Enlarged to extent possible
103897	195	0	195	12	legend on lower right figure too small making it difficult to read. [Philippe Tulkens, Belgium]	Accepted. Enlarged to extent possible
61581	195	1	195	1	Figure 9.10, bottom left : the empty legend box (gray rectangle, approximate coordinates 110 ; -0.5) needs to be either deleted or filled with model families. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61583	195	1	195	1	Figure 9.10, bottom right : increase y axis span for the top point and bottom triangle to stay inside the chart. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Balancing priorities for large legend fit.
61585	195	1	195	1	Figure 9.10, top right : in the legend, rcp and ssp should be in capital for consistency with the text. Points (e.g. RCP4.5 instead of rcp45) and dashes (e.g. SSP1-2.6 instead of ssp126) should be added for the same reason. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61587	195	1	195	1	Figure 9.10, bottom right : I would suggest to put the legend on the right of the plot, at the same height than the upper right corner. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Would reduce size of figure
61589	195	1	195	1	Figure 9.10, bottom right : the y label needs to include % as e.g. "AMOC change (%)" [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Redundant with title
61591	195	1	195	1	Figure 9.10, top right title : add a degree to 35.0N (35.0°N). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61593	195	1	195	1	Figure 9.10, top right title : I would suggest to explain "1000m" in the caption. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Described in text and caption improved.
61595	195	1	195	1	Figure 9.10, bottom left : I would suggest to leave the title as bottom right and top left doesn't have one. Or add one to all of them (but limited space for top left). [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Would reduce size of panel.
61597	195	1	195	1	Figure 9.10, top right : a precision about the vertical dashed lines is needed in the caption. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. These are just provided to guide the eye to the dates on the x-axis

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61599	195	1	195	1	Figure 9.10, bottom left legend : "Percentage changes" instead of "% change" for consistency with bottom right. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Too little space for wording.
61601	195	1	195	1	Figure 9.10 : the different charts of the figure need to be aligned vertically and horizontally. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Alignment improved.
22711	195	1	195	1	The panel key for the bottom right hand panel is barely legible and would be better if made larger and placed below the pair of lowermost panels in part because as far as I can tell the colours also refer to the lower left panel. That said the lower left panel contains at least one point in a colour that does not match any of the panel keys so it may need its own panel key? Is the figure too complicated? Can it be simplified? [Peter Thorne, Ireland]	Accepted. Enlarged to extent possible
98723	195	1	195	12	Top right panel: Need to include an explanation (in figure legend or in figure caption) of the lightly shaded regions in the far right of this plot. [Sonya Legg, United States of America]	Noted. These are the uncertainty ranges.
98725	195	1	195	12	Bottom left panel: Include explanation of the colors in the legend or figure caption (more detail than "model families") [Sonya Legg, United States of America]	Rejected. Excess complexity not serving the intent of figure.
97059	195	1	195	29	Please add explanation of the coloured boxes on top right figure in caption. Text on page 9 seems to imply there are also observational time series included in top figure. But where? [Nicole Wilke, Germany]	Noted. Coloured boxes indicate uncertainty as shown in legend. Text revised.
102005	195	3	195	3	Figure 9.10, top-left panel: Include explanation of LIG and mHOL in figure caption. [IAPSO ECS group review, United States of America]	Defined in Table 9.6 and other locations describing paleo-periods (e.g., chp 2).
89363	195	3			Fig. 9.10. The stream function at a fixed depth (1000 m) is not a robust indicator of the AMOC since it measures flow only above 1000 m. If the AMOC volume transport changes due to changing thickness of the northward flow layer, this will not register in that metric. This is why in Caesar et al. 2018 we used the stream function maximum (at a given latitude) to describe the AMOC in the CMIP5 models, since that covers the full overturning volume transport. I strongly recommend to do the same here. [Stefan Rahmstorf, Germany]	Noted. Left by this definition due to inter-chapter comparisons requiring consistency in definitions.
40217	195	3			Figure 9.10: it is hard to understand the key messages from the two bottom panels. [TSU WGI, France]	Noted. These are addressed in the text.
102007	195		195		Figure 9.10, top panels: AMOC Anomaly from what? Include in caption (for both top panels). [IAPSO ECS group review, United States of America]	Noted. AMOC anomaly vs. 1850, spelled out in reference noted, PMIP vs. 1850 conditions simulation, spelled out in dataset notes
102009	195		195		Figure 9.10, bottom-right panel: Is it possible to plot y-axis of the lower right panel in units of % AMOC change per year so that it is directly comparable with the bottom left panel? [IAPSO ECS group review, United States of America]	Noted. Not possible due to varying rates.
102011	195		195		Figure 9.10, bottom-left panel: The caption states that the colors in the bottom-left panel indicate the model family, but a legend should be added to indicate what each color means. [IAPSO ECS group review, United States of America]	Rejected. Excess complexity not serving the intent of figure.
102013	195		195		Fontsize in upper left pannel should match the other panels [IAPSO ECS group review, United States of America]	Noted. Improved match upon redrawing figure.
85345	196	0	196	0	Fig 9.11 - Panel (e) has the wrong colour scale as current speed can't be negative? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Modified as suggested.
80907	196	0	196	10	Figure 9.11: The units on the plots are too small and difficult to read [Louise Sandberg Sørensen, Denmark]	Noted
80909	196	0	196	10	Figure 9: 11. The inert figure of wind speed is plotted on a colorscale that also contains negative values. I assume that a more relevant colorscale can be used as the wind speed is not negative. [Louise Sandberg Sørensen, Denmark]	Accepted. Modified as suggested.
103901	196	0	196	10	Figure 9.11: The units on the plots are too small and difficult to read [Philippe Tulkens, Belgium]	Accepted. Modified as suggested.
103905	196	0	196	10	Figure 9: 11. The inert figure of wind speed is plotted on a colorscale that also contains negative values. I assume that a more relevant colorscale can be used as the wind speed is not negative. [Philippe Tulkens, Belgium]	Accepted. Modified as suggested.
51969	196	1	196	1	The colourbar for panel e contains negative values, yet the surface speed must be non-negative. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Modified as suggested.
61447	196	1	196	1	The markers for CMIP5/6 historical/future runs in panels d and h are indistinguishable from each other. I recommend using different colors and maybe differet markers (circles and square for example), instead of different linestyles. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Figure redrawn
35337	196	1	196	1	Figure 9.11 : the pannel (e) should be presented in the caption. [Etienne Pauthenet, France]	Accepted. Modified as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35339	196	1	196	1	Figure 9.11 : for the pannel (e) there is a mistake in the colorbar range, the value only go from 0 to ~0.7 m/s and the range of the colorbar is going from -0.7 to 0.7m/s.. The range should be corrected to represent the data. [Etienne Pauthenet, France]	Accepted. Modified as suggested.
22713	196	1	196	1	Figure panels are too small and text is barely legible. Would this make more sense as a portrait with stream function and speed as columns so that the figure panels could be largere and thus easier to read? [Peter Thorne, Ireland]	Noted
7897	196	1	196	1	Dashed lines in d and h do not work when lines are short - it's difficult to see the information here. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Other icons were tested but these were best. Size and resolution have been improved, which also helps.
7355	196	1	196	2	Fix title of a, b and c, same goes for 9.12, 9.13 and 9.14 [Svenja Halfter, Australia]	Accepted. Modified as suggested.
100217	196	3	196	10	I suggest removing titles above the maps from the tops of the maps...it might give more space for larger maps as they are currently quite small (especially the scale bar text). [Carlye Peterson, United States of America]	Accepted. Removed or reduced where possible.
100219	196	3	196	10	I'm happy to see figure labels (e.g., (a), (b), etc.) and color-deficiency-friendly color maps, very nice. [Carlye Peterson, United States of America]	Noted
100221	196	3	196	10	The markers in figure d and h for CMIP5 vs CMIP6 are very difficult to tell apart, consider changing the marker type. [Carlye Peterson, United States of America]	Noted. The figure size and resolution have been increased which helps.
100223	196	3	196	10	Are there dots for the median? I can't tell the difference between the ranges and medians. Open circles and closed circles might work, but the figure is so small it might still be hard to tell the difference. Maybe open circles and open triangles or stars? [Carlye Peterson, United States of America]	Noted. The figure size and resolution have been increased which helps.
100225	196	3	196	10	I just noticed the y-axes are scaled differently. Why not plot deep and shallow in the same plot and make it the height of the two rows of subplots (e.g., spaces "4" and "8" in a 2 row X 4 column Matlab subplot)? [Carlye Peterson, United States of America]	Rejected. After experimenting with this approach, it was now workable.
100227	196	3	196	10	The caption for Figure 9.11 is confusing as written, so I have provided a way of rephrasing it. Re-written caption: Figure 9.11: Maps of surface current barotropic streamfunction and speed for the historic period, 1995-2014 (a, e), and future projections (2081-2100) under scenarios SSP126 (b, f) and SSP585 (c, g) relative to the historic period. Plots comparing CMIP5 and CMIP6 estimates for the historic period and future projections (2081-2100) of WBC transport at deep (d) and shallow (h) depths for seven currents that have the largest transport change or largest fractional change (Sen Gupta et al., submitted). (a) the mean barotropic streamfunction for 1995-2014 and (b) scenario SSP1-2.6 projected barotropic streamfunction change from 2081-2100 versus 1995-2014 and (c) same as (b) but for scenario SSP5-8.5. Units are Sv. (e) the mean surface (0-100 m) speed for 1995-2014 and (f) scenario SSP1-2.6 projected surface speed change from 2081-2100 versus 1995-2014 and (g) same as (f) but for scenario SSP5-8.5. The units are m s-1. WBC transport median and likely range for CMIP5 and CMIP6 historic and future simulations of (d) deep currents (0-1000 m): Agulhas Leakage (ACleak), Gulf Stream (GS), Gulf Stream Extension (GSx), Tasman Leakage (TASL), East Australia Current Extension (EACx), and Indonesian Throughflow (ITF); and (h) shallow currents (0-100 m): as for (d) but with New Guinea Current (NGC), and without GSx. Units are 10^9 kg/s. [Carlye Peterson, United States of America]	Noted. Caption rewritten.
51967	196	3	196	10	Panel e is not described in this caption as it stands. [Chris Wilson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Caption rewritten.
27425	196	4	196	4	It's not the graph (d) but of the graph (e). [Eric Brun, France]	Accepted. Caption rewritten.
97061	196	4	196	4	(d) should be replaced by (e). Why distinction between shallow and deep currents and should this not be transports instead of currents? [Nicole Wilke, Germany]	Accepted. Caption rewritten.
102015	196	4	196	4	Figure 9.11: Panel (e) is called panel (d) in the caption. [IAPSO ECS group review, United States of America]	Accepted. Caption rewritten.
102017	196	6	196	8	Figure 9.11, caption: (d,h) Median and likely range... What does likely mean here? Does this refer to the projected range calculated for volume transports? It is an unusual use of the confidence language. [IAPSO ECS group review, United States of America]	Accepted. Caption rewritten. Likely range used throughout the chapter to indicate assessed 17-83rd percentile range.
38109	196		196		The color of Figure 9.11 could be improved (too light). [Junhee Lee, Republic of Korea]	Accepted. Modified as suggested.
83035	196		198		I wonder if it would be useful to merge Figure 9.11 and 9.13? The barotropic streamfunction and its changes should leave an expression in sea-level change and it might help to understand/decompose the signals seen in figure 9.13? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. A synthesis of figures in this section has been made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33535	196				Figure 9.11: Regarding Units (a-c) Sv. (e-g) m s-1. Use in the legend the same as in the figure capitation and allover the chapter the same way of representing them (eg. Always m s-1, not m/s). Idem for Y axis in figure (d) and (h), and add a space between 100 and m. Increase font in all Y axis. [Guiomar Rotllant, Spain]	Accepted. Modified as suggested.
88803	197	2	197	2	Figure 9.12: The figures should be fixed: the font has different sizes; the legend for "robust changed" is covered in some parts. [Rosemary Vieira, Brazil]	Noted. This figure has been removed.
27427	197		197		Please add the label a to d on the corresponding graphs [Eric Brun, France]	Accepted. Modified as suggested.
102019	197		197		Figure 9.12: Although the presentation of the four main upwelling systems is interesting, it would also be good to label them on the map. E.g. a label for the California Current System and the other EBUS Systems. One shared colourbarfor all the figures would be better since they all share the same scale. [IAPSO ECS group review, United States of America]	Noted. This figure has been removed.
102021	197		197		Figure 9.12: A point of concern is that the figure has a legend titled "robust change" but it is not mentioned what the definition for 'robust change" is in the caption nor in the text. In the main body of text referenced to this figure, the wind changes within the EBUS areas are described as "(weaker and/or shorter) at low latitude, and enhancement (stronger and/or longer)". Is this what the "robust change" label is for? [IAPSO ECS group review, United States of America]	Noted. This figure has been removed.
85347	198	0	198	0	Fig 9.13 - Is it worth defining Dyamic, Thermosteric and Halosteric Sea level, and how they relate to each other, in the caption so that the figure is self-explanatory? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. These definitions were collected together into a box of definitions and not repeated elsewhere for space considerations.
80911	198	0	198	5	Figure 9.13: As the vaues in these plots do not reach the maximun or minimum values in the colorscale I suggest to choose a smaller range in the color scale to enhance the patterns in the figures. [Louise Sandberg Sørensen, Denmark]	Noted: this figure has been merged with 9.14, and the presentation improved.
103911	198	0	198	5	Figure 9.13: As the vaues in these plots do not reach the maximun or minimum values in the colorscale I suggest to choose a smaller range in the color scale to enhance the patterns in the figures. [Philippe Tulkens, Belgium]	Noted: this figure has been merged with 9.14, and the presentation improved.
78741	198	1	198	1	The sum of these fields does not appear to add up to the total sterodynamic fields in Fig 29c,d. Specifically, the expected peak in Arctic RSL is not visible in the halosteric component. If the inconsistency between these figures cannot be resolved, Fig 13 should be omitted or the discrepancy should be discussed. [Erwin Lambert, Netherlands]	Accepted. Figure redrawn.
68559	198	1	198	5	Why is the ocean's dynamic sea surface height change in the top panel of Figure 9.13 positive almost everywhere, with a global mean that seems certain to be strongly positive? It is my understanding that with the typical definition of dynamic sea level rise, the global mean should be very small compared with the spatial anomalies. [Robert Hallberg, United States of America]	Noted: this figure has been merged with 9.14, and the presentation improved.
98007	198	1	198	5	The text notes that halosteric affects are negligible, yet the lower panels show magnitudes that are ~half of the corresponding thermosteric changes. This requires a text update to more accurately reflect the regional (and even basin-scale) importance, which may offset ~half of the thermosteric change e.g. Atlantic, or amplify this e.g. Pacific. As an FYI, placeholder figures are included in Ch3 Fig 3.27 which are planned for update in the FGD to CMIP6 [Paul Durack, United States of America]	Noted. The revised text clarifies that the global mean halosteric effects are negligible, not the regional halosteric effects which are discussed in the related section.
98727	198	4	198	4	Delete "the" before "a method" [Sonya Legg, United States of America]	Accepted. Caption rewritten.
3335	198	4	198	5	Something goes wrong with the caption. Changes in steric sea level from period a to b do not require a reference level. [Thomas Frederikse, United States of America]	Noted. This is added for clarity as to why this particular calculation method was chosen.
129613	198	4	198	5	Changes in steric sea level from period a to b do not require a reference level. [Trigg Talley, United States of America]	Noted. This is added for clarity as to why this particular calculation method was chosen.
102023	198		198		Figure 9.13: Reduce fontsize of row labels [IAPSO ECS group review, United States of America]	Noted: this figure has been merged with 9.14, and the presentation improved.
102025	198		198		Figure 9.13: The colormap range seems to be dominated by very large values in the Mediterraenan Sea, which does not seem to be the focus. In combination with the chosen interval for the discrete colorbar it seems as if a lot of information is lost in the major oceans (unless spatial patters are indeed this uniform). [IAPSO ECS group review, United States of America]	Noted: this figure has been merged with 9.14, and the presentation improved.
102027	198		198		Figure 9.13: One shared colourbar for all panels is sufficient since they are all similarly scaled. [IAPSO ECS group review, United States of America]	Noted: this figure has been merged with 9.14, and the presentation improved.
38111	198		198		The color of Figure 9.13 could be improved (too light). [Junhee Lee, Republic of Korea]	Noted: this figure has been merged with 9.14, and the presentation improved.
33537	198				Figure 9.13: At the top of each column, you could include the years: 1984-2015 and 2081-2100. [Guiomar Rotllant, Spain]	Noted: this figure has been merged with 9.14, and the presentation improved.

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22715	199	1	199	1	Calling out solely SSP5-8.5 in this manner is not ideal. It provides no indication of the impact of our emissions choices. At least the 2.6 scenario should be added. [Peter Thorne, Ireland]	Noted: this figure has been merged with 9.13, and the presentation improved.
54499	199	3			Fig. 9.14. "Dummy Fig." ...delete and re write the epigraph. [Maria del Pilar Bueno Rubial, Argentina]	Noted: this figure has been merged with 9.13, and the presentation improved.
102029	199		199		Figure 9.14, left-hand panel: unit (m) should not be in italics [IAPSO ECS group review, United States of America]	Noted: this figure has been merged with 9.13, and the presentation improved.
109237	199		199		Figure 9.14: Spatial plot of SSH should include SSP1-2.6 projections [A.K.M Saiful Islam, Bangladesh]	Noted: this figure has been merged with 9.13, and the presentation improved.
38113	199		199		The color of Figure 9.14 could be improved (too light). [Junhee Lee, Republic of Korea]	Noted: this figure has been merged with 9.13, and the presentation improved.
85349	200	0	200	0	Fig 9.15 - the polar projection panels are not very clear - is there a way to re-arrange the panels so these figures can be much bigger (and perhaps also improvement the colour scales)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
80913	200	0	200	16	Figure 9.15: The maps on the right are very difficult to read; should be enlarged. [Louise Sandberg Sørensen, Denmark]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
103915	200	0	200	16	Figure 9.15: The maps on the right are very difficult to read; should be enlarged. [Philippe Tulkens, Belgium]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
22717	200	1	200	1	polar projection maps and the text around them including colour bars are way too small to be legible and useable. Either split into two figures or reorder so as to be portarait and enable the reader to ascertain properly the features being plotted and actually read the colour bar scales. [Peter Thorne, Ireland]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
61511	200	3	200	16	Figure 9.15: it is difficult to separate out the sea ice cover from the continents which are gray as well. If not too late, would it be possible to either choose a different color for the continents or outline them in some way so that they can be easily distinguished? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted: We have improved the colouring of the continents to improve clarity
93049	200	3			It would be informative to add a paleoclimate perspective to this figure, using the results from the CMIP6 lig127k experiments. 17 CMIP6 models have completed the lig127k experiment. A figure with the same format as the SSP245 (2045-2054) panel is included as Figure 7 in the Climate of the Past Discussion paper: Otto-Bliesner et al., [https://www.clim-past-discuss.net/cp-2019-174/] which is being updated during the paper revision stage. The distributions of minimum Arctic sea ice have notable similarities between the SSP245 and lig127k ensembles. Relevant recent papers reviewing LIG sea ice proxies include: Stein et al., Nature Comm., 2017; Kremer et al., QSR, 2018; Kageyama et al., CPD, 2020. [Bette Otto-Bliesner, United States of America]	Noted. Unfortunately, owing to space constraints in our chapter, we cannot add such a plot.
12175	200	6	200	7	Fig 9.15 : the maps inlet show the polar observation hole. A first issue is that the color black is very similar to that of open ocean. A different color should be used, and referenced in the color map. Alternatively, the sea-ice cover could be “filled / interpolated” with data (since I presume you do this interpolation anyway to compute SIA). As it is, the polar observation hole distracts and maybe confuses the analysis. [Thomas Lavergne, Norway]	Accepted: we now colour the observation hole white to improve clarity of the figure
88805	200	7	200	7	Figure 9.15: (Right): Sea-ice coverage in the Arctic - the font size of the sea ice concentration scale is too small. [Rosemary Vieira, Brazil]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
109033	200	11	200	11	Figure 9.15 Given the size of the figure, the stippling is difficult to perceive. Will the TSU call for consistency across chapters on whether stippling will be drawn over significant or insignificant regions? [Chaincy Kuo, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
109035	200	12	200	12	Figure 9.15 Can the caption mention the CMIP6 experiment used in the figure? It is labeled on the figure, but the caption could read better to have the experiment name included, as the results are dependent on this? [Chaincy Kuo, United States of America]	Accepted: The caption now includes the CMIP6 experiment name
109037	200	12	200	13	Figure 9.15 Would the fraction or percentage of CMIP6 SSP2-4.5 models run which led to over 15% mean sea-ice concentration relative to the observation period quoted be more informative than the number of models reporting this? Probably this is just a preliminary plot for the SOD deadline, and chapter 6 does intend to plot such information. [Chaincy Kuo, United States of America]	Noted. We think that the current way of plotting the model results is more informative than the plot suggested by the reviewer.
27429	200	13	200	13	The diagram indicates 2054 and not 2055 [Eric Brun, France]	Accepted: This has been corrected.
102031	200		200		Figure 9.15: Map panels on the right could be bigger and in particular the fontsize of the colorbars is too small. [IAPSO ECS group review, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
102033	200		200		Figure 9.15: Consider presenting the subpanels as top and bottom, instead of side by side. This would allow for map panels on the right to increase in size. [IAPSO ECS group review, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps

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116919	200		200		I think that data are available back to 1850 (historical estimate) and for longer periods (paleo), would they deserve to be displayed. [Valerie Masson-Delmotte, France]	Rejected: This data is too uncertain to be included in the context of this figure
38115	200		200		The color of Figure 9.15 right (sea-ice concentration) could be improved to be distinguished clearly. [Junhee Lee, Republic of Korea]	Accepted: We have changed the colour bar of the right hand figure
109031	200		200		Figure 9.15 Size of Arctic stereo view colorbar text is too small to read [Chaincy Kuo, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
33539	200				Figure 9.15: scale labels not visible. Increase font. [Guiomar Rotllant, Spain]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
3017	201	1	201	1	In Figure 9.16 (d) and (h) the black dots and surrounding grey shaded rectangles are not explained in the caption. [Petteri Uotila, Finland]	Accepted: This information is now added to the caption
83335	201	3	201	7	Figure 9.16 - Is this for Arctic or Antarctic sea ice, or both? Please clarify in the figure caption. [Robert Massom, Australia]	Accepted: This information is now added to the caption
102035	201		201		Figure 9.16: Panels d and h, what does shading around black dot mean? [IAPSO ECS group review, United States of America]	Accepted: This information is now added to the caption
33541	201				Figure 9.16: In the figure legends: change “CO2” by “CO2“ and “ºC/1000Gt” by “ºC 1000Gt-1”. [Guiomar Rotllant, Spain]	Accepted: This has been corrected.
80915	202	0	202	15	Figure 9.17: same comment as above [Louise Sandberg Sørensen, Denmark]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
103919	202	0	202	15	Figure 9.17: same comment as above [Philippe Tulkens, Belgium]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
22719	202	1	202	1	polar projection maps and the text around them including colour bars are way too small to be legible and useable. Either split into two figures or reorder so as to be portarait and enable the reader to ascertain properly the features being plotted and actually read the colour bar scales. [Peter Thorne, Ireland]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
9031	202	1	202	6	Rignot et al. PNAS 2019 includes Antarctic mass balance since 1979. Why is this not included in the Antarctic plot? In Greenland, you included Mouginot PNAS 2019, but you cannot do the same for Antarctica? [Eric Rignot, United States of America]	Accepted. Included in subpanels below.
88807	202	6	202	6	Figure 9.17: (Right): Sea-ice coverage in the Antarctica - the font size of the sea ice concentration scale is too small. [Rosemary Vieira, Brazil]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
12177	202	6	202	7	Fig 9.17 : the maps could be extended to lower latitudes so as not to crop some of the observed changes. [Thomas Lavergne, Norway]	Rejected: the current plots do not crop any of the observed changes
3019	202	8	202	8	'(Left): First column ...’ [Petteri Uotila, Finland]	Rejected. We do not agree that the 'first column'-disclaimer is needed for the left plot
109041	202	11	202	11	Figure 9.17 Given the size of the figure, the stippling is difficult to perceive. Will the TSU call for consistency across chapters on whether stippling will be drawn over significant or insignificant regions? [Chaincy Kuo, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
27431	202	12	202	12	The diagram indicates 2054 and not 2056 [Eric Brun, France]	Accepted: This has been corrected.
109043	202	12	202	12	Figure 9.17 Can the caption mention the CMIP6 experiment used in the figure? It is labeled on the figure, but the caption could read better to have the experiment name included, as the results are dependent on this? [Chaincy Kuo, United States of America]	Accepted: The caption now includes the CMIP6 experiment name
109045	202	12	202	13	Figure 9.17 Would the fraction or percentage of CMIP6 SSP2-4.5 models run which led to over 15% mean sea-ice concentration relative to the observation period quoted be more informative than the number of models reporting this? Probably this is just a preliminary plot for the SOD deadline, and chapter 6 does intend to plot such information. [Chaincy Kuo, United States of America]	Noted. We think that the current way of plotting the model results is more informative than the plot suggested by the reviewer. We suspect that the reviewer intended to type "9" instead of "6".
102037	202		202		Figure 9.17: Map panels on the right could be bigger and in particular the fontsize of the colorbars is too small. [IAPSO ECS group review, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
102039	202		202		Figure 9.17: Consider presenting the subpanels as top and bottom, instead of side by side. This would allow for map panels on the right to increase in size. [IAPSO ECS group review, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
38117	202		202		The color of Figure 9.17 right (sea-ice concentration) could be improved to be distinguished clearly. [Junhee Lee, Republic of Korea]	Accepted: We have changed the colour bar of the right hand figure
109039	202		202		Figure 9.17 Size of Arctic stereo view colorbar text is too small to read [Chaincy Kuo, United States of America]	Accepted: We have redrawn the figure to increase label size and subpanels with maps
817	203	0	203	0	Why not use centimeters or millimeters for the Sea Level Equivalent axis, since this is the order of magnitude? [Michael Wood, United States of America]	Rejected for consistency across multiple figures.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80917	203	0	203	6	What is the reason for have a reference point in 2015?It doesn't seem so pedagogical to report negative SLE. [Louise Sandberg Sørensen, Denmark]	Noted. This is to allow comparisons with other figures in the chapter and tables, as 2015 is a key year for multiple datasets/simulations
103921	203	0	203	6	What is the reason for have a reference point in 2015?It doesn't seem so pedagogical to report negative SLE. [Philippe Tulkens, Belgium]	Noted. This is to allow comparisons with other figures in the chapter and tables, as 2015 is a key year for multiple datasets/simulations
35341	203	1	203	1	The authors could write EAIS in white in the green part of the Antarctica map, so we can relate the curve, the text and the location easily. [Etienne Pauthenet, France]	Accepted. Label added.
14701	203	1	203	1	Figure 9.18 is difficult. Why was 2015 taken as the baseline year from which to calculate other yearly mass change anomalies from? What if 2015 was an anomalous year due to internannual variability in mass change? More generally, plotting mass change as such (without the word 'anomaly' or similar being used) would immediately suggest to readers a mass GAIN for all years <2015, and mass loss for all years >2015, for both ice sheets (obviously not reality, and contradicting referencing Section 9.4 text). Moreover, there is no way to actually assess actual absolute mass change with these figures, which is the primary metric for SLC discussions. Strongly recommend redoing these figures to more intuitively reflect actual absolute mass change for each year, per ice sheet. [Jeremy Fyke, Canada]	Noted. This is to allow comparisons with other figures in the chapter and tables, as 2015 is a key year for multiple datasets/simulations
61603	203	1	203	1	Figure 9.18b : extend the upper limit of the y axis as in 9.18a would allow to enlarge the Antarctica inset figure and then make the Antarctic Peninsula and its blue colored area more visible. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. These panels have been resized so as to maximize the inset map sizes.
97063	203	1	203	37	Greenland mass change is given relative to 2015, would it not be better to use a early reference maybe 1995? Please change the figure caption in a to clarify that the period 1972-2018 is from Mouginot and 1992-2018 is from Colgan. [Nicole Wilke, Germany]	Noted. The labels in the figure now clarify.
88041	203	1			Figure 9.18: The message would be much clearer and more powerful if you would use the same scale for mass loss in A and AA. [Georg Kaser, Austria]	Rejected. This figure has a lot of detail, and zooming out to match axes leaves too much unused space.
29695	203	3	203	6	Contemplate the convenience of adding in the figure caption of Figure 9.18 a short sentence pointing out that in the two panels the ranges and the scales of the three axes (left x-axis, right x-axis and y-axis) and also the maps scales are all different. [Hernan Edgardo Sala, Argentina]	Rejected. This figure has a lot of detail and reemphasizing this additional detail would shrink the figure on the page.
27433	203		203		Please add the label a and b on the corresponding graphs [Eric Brun, France]	Accepted. Labels added.
102041	203		203		Figure 9.18. right-hand panel: State in caption what shading means. [IAPSO ECS group review, United States of America]	Accepted. Caption revised.
109239	203		203		Figure 9.18: Antarctic "mass" change should be replaced with "Ice-sheet mass" change. Same for Figure 9.17: Greenland mass change [A.K.M Saiful Islam, Bangladesh]	Accepted. Titles revised.
109241	203		203		Figure 9.18: Figure numbers in the figure caption are not marked in the figure (e.g. (a), (b) etc.) [A.K.M Saiful Islam, Bangladesh]	Accepted. Labels added.
33543	203				Figure 9.18. I would suggest to put the units of Y axis of figures (a) and (b) at the same maximum; like that it will be cleat at glance the difference between both poles. Abbreviations in the figures related to artic and Antarctica should be described. [Guiomar Rotllant, Spain]	Rejected. This figure has a lot of detail, and zooming out to match axes leaves too much unused space.
90467	203				In Figure 9.18. panel (a): include a label for the year "2015" on the x-axis? [Holly Kyeore Han, Canada]	Rejected. Too busy to double the tick labels.
73863	203				It seems that the units of sea level changes are not identical through the text (I found both mm and m), thus I suggest to repesent, in particular for this figure, the right vertical scale in units of cm. [Fuyuki SAITO, Japan]	Rejected for consistency across multiple figures.
815	204	0	204	0	The figure 9.19 organization/presentation is a little messy (copy/pasted, random grey lines in bottom left, lat/lon references on some maps but not others) and should be cleaned up before publication [Michael Wood, United States of America]	Accepted. Modified as suggested.
85351	204	0	204	0	Fig 9.19 Is it possible to privde a summary definiton of sea-level equivalent volume in the caption? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is provided in Box 9.1
813	204	0	205	0	The title of figure 9.19 (top right panel) should say "Cumulative Mass Change relative to 2015". Same for figure 9.20 [Michael Wood, United States of America]	Accepted. Modified as suggested.
31601	204	1	204	1	On the bottom right figure (elevation changes), it looks like the elevation changes on the Greenland margin reach the maximum value of the color palette (-1m). Is there either a way to see the actual elevation change by changing the color scale (e.g. logarithmic scale?) or could this be clarified in the legend? [Gonéri Le Cozannet, France]	Rejected. The values are not saturated very often, and are chosen to match the preceding figure.
61605	204	1	204	1	Figure 9.19, bottom left : the frame is only partly visible (only top and left borders). It should be completed or removed. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.
61607	204	1	204	1	Figure 9.19, top right : it has to be precised that the changes are relative to 2015 (or present day if so) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
61609	204	1	204	1	Figure 9.19, top right : Bamber and IMBIE are not visible and hard to make so. I would suggest to have one of the two lines as a dashed line and/or work on the opacity. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Yes they do overlap. They are contrasted in greater detail in Fig. 9.17, but in this figure are meant to frame the scale of changes to come rather than show observational detail.
61669	204	1	204	15	Figure 9.19: Where are the results from SSP1-2.6 and SSP5-8.5 in the top right panel? They should appear here as mentioned in the caption. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. They are present and labelled with colour-matching labels.
61671	204	1	204	15	Figure 9.19: Is the bottom right map the average over 2093-2100 as indicated on top of the map or over 2061-2100 as indicated in the legend? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected.
61673	204	1	204	15	Figure 9.19: Is the bottom right map (ISMIP6 model mean) the mean for all scenarios or only a selection of scenarios? This information should appear in the caption. If this is the mean for all scenarios, doesn't it make sense to have one map for RCP2.6/SSP1-2.6 and another map for RCP8.5/SSP5-8.5? It is very crude to mix these extreme scenarios together. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Modified caption to clarify as suggested.
14705	204	1	204	30	LGM extent: is it worth noting where/if GrIS abuts/joins to the broader Laurentide Ice Sheet (e.g. by dashed lines on GrIS boundary..?) [Jeremy Fyke, Canada]	Rejected. This figure has a lot of detail, and this additional detail is not consistent with discussion.
14707	204	1	204	30	"Greenland Cumulative Mass Change" plot incorrectly suggests to general readers that GrIS was gaining mass (e.g. causing sea level fall) prior to 2015 and losing mass thereafter. This is incorrect/inconsistent with 9.4.1. text ("GrIS lost mass… over past 25 years"). Would it be more appropriate to at least make the mass/SLC anomalies relative to, e.g., year 1850? [Jeremy Fyke, Canada]	Rejected. This interpretation is not supported by the figure and 2015 is a convenient baseline for chapter-wide comparisons
107293	204	3			not certain that having paleo information in a key figure for projections is helpful. Having box plot and time series next to each other on top row despite the y axis being two order of magnitude different is misleading. Paleo not discssued in section that refers to this figure. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Shading is now used to emphasize changes in scale. This figure is a synthesis over all timescales and methods considered. Observations alone are in chapter 2, projections and recent regional detail in chapter 12, projections alone in chapter 4, etc. Chapter 9 has the mandate to synthesize toward process understanding.
93047	204	3			it should be noted here that the LIG and LGM GrIS shown in Figure 9.19 (also Fig. 2.22) are based on model simulations constrained by ice core and sea level data [Bette Otto-Bliesner, United States of America]	Rejected. There are no uncertainties, but they are explicitly noted as schematic interpretations in the caption. Their small size is meant to deemphasize their detail.
9035	204	4	204	15	The inflection point between observations and models is particularly evident here in this plot. A great way to illustrate that the models do not reproduce the past 40 years of observations. This plot could be used to demonstrate that we should not rely on the current models. They seem to be dragging observations by 50 years. [Eric Rignot, United States of America]	Accepted. The 1979 data is now also shown in an earlier figure (9.17)
80529	204	6	204	6	The order of refernces does not match the order of reconstrutions in the figure, making it unclear what the sources are.The order of references should be (Koenig, Goelzer, Lecavalier). Also, replace reference Goelzer et al 2017 --> Goelzer et al 2016a [Goelzer, H., Huybrechts, P., Loutre, M.-F., and Fichefet, T.: Last Interglacial climate and sea-level evolution from a coupled ice sheet–climate model, Clim. Past, 12, 2195–2213, https://doi.org/10.5194/cp-12-2195-2016, 2016.] [Heiko Goelzer, Belgium]	Noted. The labels follow the referencing standard throughout the report and dividing them up by subpanels leads to unnecessary lengthening.
27435	204	14	204	14	The diagram indicates 2093-2100 [Eric Brun, France]	Accepted. Corrected labels as appropriate.
80533	204	14	204	14	The specified range (2061-2100) does not match with the range given in the figure (2093-2100) [Heiko Goelzer, Belgium]	Accepted. Corrected labels as appropriate.
33545	204				Figure 9.19: change unit format in figure: m/yr to m yr-1. [Guiomar Rotllant, Spain]	Accepted. Modified as suggested.
61719	205	1	205	1	Figure 9.20: top right panel. I can't tell which part of the figure (which line) "IMBIE" is referring to. At this scale, do the IMBIE and Bamber lines overlap? If so, please state. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Yes they do overlap. They are contrasted in greater detail in Fig. 9.17, but in this figure are meant to frame the scale of changes to come rather than show observational detail.
61721	205	1	205	1	Figure 9.20: Consider adding the blue color to the legend as well as the red color. For instance: thin red line [thin blue line] Emulator median (RCP 8.5 [RCP 2.6]) so it is more clear that the thin lines and shaded region show both RCP scenarios and are differentiated by color [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This was decided to be too complicated, as the same colour scheme for scenarios is used throughout the chapter

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42931	205	1	205	15	I do not understand the basis for the LIG reconstruction of Antarctica at all. Based on chater 2, page 176, where it also appears, I assume it's from Goelzer et al 2016, but this is a modelling study based on an EMIC, and has no basis in evidence at all. This is highly misleading, and should not be used. If it is, it's essential to label it as a modelling study with no evidential support. However I would argue strongly against including it. This applies even more strongly to the top left panel where you seem to imply 4-8 m from Antarctica alone in the LIG, an estimate that is entirely guesswork, and seems excessive even in relation to the known sea level evidence of 5-9 m from all sources. [Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There are no uncertainties, but they are explicitly noted as schematic interpretations in the caption. Their small size is meant to deemphasize their detail.
14749	205	1	205	33	Figure 9.20 is difficult, like Fig. 9.18. Why was 2015 taken as the baseline year from which to calculate other yearly mass change anomalies from? What if 2015 was an anomalous year due to internannual variability in mass change? More generally, plotting mass change as such (without the word 'anomaly' or similar being used) would immediately suggest to readers a mass GAIN for all years <2015, and mass loss for all years >2015, for both ice sheets (obviously not reality, and contradicting referencing Section 9.4 text). Moreover, there is no way to actually assess actual absolute mass change with these figures, which is the primary metric for SLC discussions. Strongly recommend redoing these figures to more intuitively reflect actual absolute mass change for each year, per ice sheet. [Jeremy Fyke, Canada]	Rejected. This interpretation is not supported by the figure and 2015 is a convenient baseline for chapter-wide comparisons
9033	205	3	205	14	Rignot et al. PNAS 2019 includes Antarctic mass balance since 1979. Why is this not included in the Antarctic plot? In Greenland, you included Mouginot PNAS 2019, but you cannot do the same for Antarctica? [Eric Rignot, United States of America]	Accepted. The 1979 data is now also shown in an earlier figure (9.17)
9037	205	3	205	14	The inflection point between observations and models is less evident here because you chose not to include the observations from 1979 to present in Rignot et al. PNAS 2019. Is this an honest omission or a choice based on ? [Eric Rignot, United States of America]	Accepted. The 1979 data is now also shown in an earlier figure (9.17)
107337	205	3			not certain that having paleo information in a key figure for projections is helpful. Having box plot and time series next to each other on top row despite the y axis being two order of magnitude different is misleading. Paleo not discssued in section that refers to this figure. [Isabel Nias, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Shading is now used to emphasize changes in scale. This figure is a synthesis over all timescales and methods considered. Observations alone are in chapter 2, projections and recent regional detail in chapter 12, projections alone in chapter 4, etc. Chapter 9 has the mandate to synthesize toward process understanding.
80531	205	7	205	7	The order of refernces does not match the order of reconstrutions in the figure, making it unclear what the sources are. [Heiko Goelzer, Belgium]	Noted. The labels follow the referencing standard throughout the report and dividing them up by subpanels leads to unnecessary lengthening.
88809	205	7	205	7	Goelzer et al., 2016 (a-b). The references are the same. [Rosemary Vieira, Brazil]	Accepted. This was a failure of the automated referencing software. Corrected.
61161	205	7			Are there any errors associated with these paleo extents? 'Grey shading shows modeled extent of grounded ice.' might be suggested. [Udita Mukherjee, United States of America]	Rejected. There are no uncertainties, but they are explicitly noted as schematic interpretations in the caption. Their small size is meant to deemphasize their detail.
91095	205	13	205	13	Don't think Schroeder produced dh/dt for the whole ice sheet for 78-2017 from IPR tracks… [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This should have been Schröder
95951	205	14	205	14	"selected" [Alessandro Silvano, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten correcting this error.
102043	205		205		Figure 9.20: Put the acronyms for mid-Pliocene Warm Period, the Last Interglaciation, and the Last Glacial Maximum [IAPSO ECS group review, United States of America]	Rejected. The acronyms are used in panel a, but not below due to space constraints. They are all spelled out in the caption and defined in Table 9.6
72131	205		205		I don't clearly understand that why the thick lines of RCP2.6 (blue) and RCP8.5 (red) show similar trend in the time series of the Antarctic Cumulative Ice Mass change (top right) in figure 9.20. Could you explain about this in detail? [Jun-Young Park, Republic of Korea]	Noted. This is the result of the emulator assessment of the change including all available ice sheet simulations (Edwards et al. 2021). The degree of scenario dependence is discussed in detail in the text, because in total there is little but in regions and budgets and within certain model families there is strong sensitivity.
38119	205		205		The color of Figure 9.20 bottom right (elevation change in obe. and model) could be improved to be distinguished clearly. [Junhee Lee, Republic of Korea]	Rejected. It is chosen to be identical between the GrIS and AIS versions of the figure, so cannot be rescaled in one without the detriment of the other.
29249	206	1	206	1	It's very awkward to have the inset placed overtop of the main figure. The authors should consider some other way to highlight this region. [Andra Garner, United States of America]	Rejected. Space is at a high premium in this report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22721	206	1	206	1	Text is too tiny in titles and on the colour bars. Colour bars are identical and not needed twice. Suggest transpose to below the lower row and use whole width so colour bar can be given in a legible manner. [Peter Thorne, Ireland]	Accepted. Improved sizing.
102045	206		206		Figure 9.21: Increase fontsize, for colorbars definitely and maybe also for titles (could be 2 lines) [IAPSO ECS group review, United States of America]	Accepted. Improved sizing.
33547	206				Figure 9.21. In the figure font letter above maps and in the scales are not readable. Please increase font size. [Guiomar Rotllant, Spain]	Accepted. Improved sizing.
2069	207	1	207	1	The figure needs to be revised substantially in my opinion for several reasons: I find it highly problematic to show model results for the past (Marzeion et al., 2015) along with the observations (Zemp et al., 2019). For many regions there is a substantial systematic misfit. Like this, the figure focusses on what we DO NOT KNOW, instead of on what WE KNOW. I strongly suggest to separate models and data for the past, even if it is not possible then to go back to 1900 (which would not be a loss to the main statement of the report). [Matthias Huss, Switzerland]	Noted. We are fully aware of the misfit. The glacier contribution to SLR for the 20th century (1900-1990) for AR5, SROCC, and now for AR6 is based on Marzeion et al. (2015). Our assessment of the glacier contribution is based on this data, too. Showing it in the same plot with our best estimation for observed glacier mass change for the last 60 years is a clear message that between error bars both show the same picture, and we can trust it.
2071	207	1	207	1	In the text it is stated that results of SROCC were used for the past mass changes, here only Zemp et al 2019 is used. Why? This should be made consistent. [Matthias Huss, Switzerland]	Noted. For the FGD, captions, figures, and the main text is consistent. For the SOD, we use SROCC as the most updated data for the recent period. The FGD presented the global and regional glacier mass change rate between 1960 and 2019 in Figure 9.20. The SROCC values are presented together with the new evidence.
2073	207	1	207	1	The axis label is not precise: It should be "Volume relative to 2016", instead of "volume change" [Matthias Huss, Switzerland]	Accepted. For FGD, the y-label is glacier mass relative to year 2015.
361	207	1	207	1	Although in principle I applaud the effort to put observations and projections in the same figure I am not thrilled by the readability of this figure. Its cumulative character means that it is hard to see if there is acceleration or not of the mass loss. It also put a lot of weight in the Zemp et al., 2019 estimate when another studies (Wouters et al., 2019) found quite different values in some regions. I am not sure what I can propose to improve the readability of the figure so this comment is a bit useless (sorry!) but I am also hoping that others reviewers will have better ideas. Maybe showing pentadal or decadal average values could help instead of cumulative values? [Etienne Berthier, France]	Noted. The main message of the figure is that the glacier mass is shrinking since the 1900s in all the different regions and will continue in the future. We want to highlight what was and what will be the glacier mass for the different regions relative to the year 2015. It is graphical support to our statements. On the contrary, and particularly for future projection, our confidence for glacier mass change rates is low (Marzeion et al. 2020), and this why we preferred not to focus on the rates but the directions.
97065	207	1	207	16	Fig. 9.22: the Reg Obs are not to be seen on the figure. If the changes are relative to 2016, should the graphs all go through 0% change in 2016 instead of 100 %? [Nicole Wilke, Germany]	Accepted. The FGD presented the global and regional glacier mass change rate between 1960 and 2019 in Figure 9.20. The plots in Figure 9.21 show the relative mass to 2016 and not the relative mass changes as the y-label indicates. In the FGD we use 2015 as the reference, and the y axis label is "Glacier mass relative to the year 2015 [%]".
26515	207	1	207	16	The y-axis in Fig. 9.22 does not show "volume change relative to year 2016", but rather "percentage of volume relative to year 2016" [Ward van Pelt, Sweden]	Accepted. For FGD, the y-label is glacier mass relative to year 2015.
88043	207	1			Figure 9.22: I doubt that the ordering different from Tabel 9.3. and following a mass loss ranking adds value. If it is kept liek this, the caption must be corrected from "largest mass loss" to "largest relative mass loss". [Georg Kaser, Austria]	Accepted. For the FGD, we use the ordering from 1 to 19 (like in table 9.3). Also in the FGD, we include two new plots; 1) the mass change relative to year 2015 for all regions; 2) and for all regions except the peripheral glaciers of the Ice sheets to be consistence with section 9.6. Finally, similar to SROCC we merged regions 13 to 15 as the High Mountains of Asia.
88045	207	1			Adding the uncertainty shads in the legend would be useful [Georg Kaser, Austria]	Noted. For the FGD, the style and readability of the figure are improved.
67101	207	3	207	3	Fig.9.22: for consistency with SROCC, better to use RGI regions. The term is also far more widely used than GTN-G. Here it is also tightly connected to the actual RGI data sets for these regions since athe projections are based on the RGI outlines for those regions. [Regine Hock, United States of America]	Accepted. For the FGD, we use RGI regions instead of GTN-G.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102047	207	3	207	3	Figure 9.22, caption: Define GTN-G [IAPSO ECS group review, United States of America]	Noted. For the FGD, we use the regions from RGI (Randolph Glacier Inventory) instead of GTN-G (Global Terrestrial Network for Glaciers; https://www.gtn-g.ch/) regions.
100031	207	3	207	3	"Project" should be changed to "projected". [Lydia Sam, Sweden]	Accepted. In the FGD, we improved the caption and legend of the figure.
4219	207	3	207	15	Work by Möller and Kholer (2018) could be added to this figure for Svalbard. They derice mass loss from 1900 to 2010 using climate models. https://doi.org/10.3389/feart.2018.00128 Möller, M. and Kohler, J., 2018. Differing Climatic Mass Balance Evolution Across Svalbard Glacier Regions Over 1900–2010. Frontiers in Earth Science, 6, p.128. [William Kochtitzky, Canada]	Noted. For the FGD we considered include this time series. Unfortunately, the complexity of the figure was already too high, so adding it was not possible.
4221	207	3	207	15	Noël et al. (2018) provide data for both the North and South Canadian Arctic mass loss for 1958 to 2015. Noël, B., van de Berg, W.J., Lhermitte, S., Wouters, B., Schaffer, N. and van den Broeke, M.R., 2018. Six decades of glacial mass loss in the Canadian Arctic Archipelago. Journal of Geophysical Research: Earth Surface, 123(6), pp.1430-1449. [William Kochtitzky, Canada]	Accepted. The FGD presented the global and regional glacier mass change rate between 1960 and 2019 in Figure 9.20.
61777	207	3			Figure 9.22: it would be good to mention that the numbering of the 19 regions can be seen in bracket behind the name of the location, otherwise this numbering seems random and is confusing. Suggestion. In line 4 say something like: (area numbers are shown in bracket, see Fig 9.1 for locations) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. In the FGD, we improved the caption and legend of the figure.
67097	207	4	207	5	Fig.9.22: you mean 'the largest relative mass loss' ? [Regine Hock, United States of America]	Noted. For the FGD, the ordering from 1 to 19 (like in table 9.3) is used. Also in the FGD, we include two new plots; 1) the mass change relative to year 2015 for all regions; 2) and for all regions except the peripheral glaciers of the ice sheets to be consistence with section 9.6. Finally, similar to SROCC we merged regions 13 to 15 as the High Mountains of Asia.
88813	207	7	207	7	Marzeion et al., 2015 (a-b) The references are the same. [Rosemary Vieira, Brazil]	Noted. For the FGD, citation errors are fixed
88811	207	8	207	8	Figure 9.22: Green lines and shaded areas (Region 16 and 19). The green line is observed in the Regions 16 and 17. [Rosemary Vieira, Brazil]	Noted. For the SOD, we only include "other observations" for region 16 and 19. The FGD presented the global and regional glacier mass change rate between 1960 and 2019 in Figure 9.20
67099	207	11	207	13	Fig.9.22: sentence can be deleted and next one better since it is not the same as % changes: For some regions the percentages in 2016 exceed 200% for some regions (>1000% in XXX, >XXX% in XXX ....) [Regine Hock, United States of America]	Accepted. In the FGD, we improved the caption and legend of the figure.
18115	207		207		Please consider for Region 16 and 17 Braun et al. 2019 (NCC), for Region 16: Seehaus et al. 2019 (TC) and 2020 (JoG) covering about 90% of the region. For Region 11 a study covering the entire alps were recently accepted/published by Sommer et al. 2020 (Nature Communiactions) covering more than 95% of the region. For HMA please include Brun et al 2017 and Shean et al. 2020 (Frontiers) [Thorsten Seehaus, Germany]	Accepted. The FGD presented the global and regional glacier mass change rate between 1960 and 2019 in Figure 9.20. These and others regional estimates are showing together with global assessments for each region.
33549	207				Figure 9.24. a) Why each scenario have different permafrost extent?; b) Identify grey circles, observations? [Guiomar Rotllant, Spain]	Taken into account. For a, these are not scenarios but simulations by different models for the historical period. For b), taken into account in revised version: grey circles are the historical period, and this is specified.
62099	208	1	208	1	X-axis displays the 'average permafrost temperature', but what is the error (maybe 2 standard deviation) of this? Could error bars be displayed to show the range of uncertainity? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The graph has been revised, with more values included. Tests have shown that with this increased number of sites, an uncertainty estimate of 0.1°C would clutter the figure without adding much information. Moreover, the interannual variability would be of the order of the symbol size in many cases.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88345	209	1	209	10	Fig. 9.24 (a) - The results based on model depth of <15 m are not clear. How can you make comparisons when the maximum depth is not the same for all models. Is the large range in area an artefact of the difference in maximum depth? The title of the graph indicates that the area is for 1995-2014 but the caption indicates 1979-1998. It is unclear why results in upper 15m are given for (a) while (b) only considers upper 3 m (assume baseline used for b is for 3 m depth and not 15 m depth). By deepest model level do you mean the bottom boundary or just the deepest level used in calculation of extent - 15 m is rather shallow for a lower boundary for simulations of a century. [Sharon Smith, Canada]	Taken into account. The legend is clarified concerning the question how temperature is diagnosed for the models that do not go to 15m. The text acknowledges that shallow soil can be a reason for outliers. The data are 1979-1998, consistently indicated now (thank you for spotting this error).
67105	209	3	209	3	Fig.9.24: What is 'diagnosed'? Is 'modeled' better? [Regine Hock, United States of America]	Noted. "Diagnosed" in modelling means that it is not directly model output, but calculated from that, which is the case here, as indicated in the legend.
62101	209	3	209	3	Define NH in figure caption, so that figure can stand-alone without the reader having to refer back to the text. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Done.
88347	209	6	209	8	See earlier comment - results in these studies aren't really observational. [Sharon Smith, Canada]	Accepted. Legend modified ("observed and reanalysis-based").
67103	209	9	209	9	Fig.9.24: What is GSAT? Best to avoid acronyms. Also NH [Regine Hock, United States of America]	Noted. GSAT is used extensively across the report.
62103	210	1	210	1	Is it possible to include an uncertainity estimate in panels a and c to provide a more transparent assessment to the trends? Or does that take any meaning away from the figure as it will be too unclear? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Indeed we considered this and did not find a way to produce a readable figure with uncertainties.
22723	210	1	210	5	I cannot work out how the left hand panels match with the right hand ones in part because of the 3 orders of magnitude mismatch in the scales used between them. The figure caption does not help explain this. [Peter Thorne, Ireland]	Noted. Left hand panels are trends, right hand panels are individual years. The units are chosen to minimize the space taken up by the numbers, which would be very large or very small (and in both cases, long) if the same calibration was chosen on the left and right part of the figure. These have been reorganized in FGD presentation for better clarity.
67107	210	3	210	3	Fig.9.25: NH not explained [Regine Hock, United States of America]	Taken into account. Spelled out NH in the legend.
33551	210				Figure 9.25. Please correct unit format in fig a (103km2/year to 103km2 year-1) and c (Gt/yr to Gt yr-1). [Guiomar Rotllant, Spain]	Accepted, although this is longer and drove us to reduce the title size, making it harder to read. Not clear why Gt yr^{-1} should be easier to read for anybody than Gt/yr.
62105	211	1	211	1	Panel A is not colour-blind friendly. The red line and green area cannot be distinguished, please change the colour scheme to ensure it can be accessed by all. And please make sure to consider this for all figures. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Colour scheme adapted to be more colour-blind visible.
61935	211	1	211	10	Figure 9.26 a is hard to understand what the dots above the whiskers are, are these outliers? I don't understand how the median in October till December can be so low if the mean is so high, and the interannual distribution is around the mean value. Is it just one value that mainly contributes to that low median? [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. These are outliers indeed, explaining the strong difference between means and medians.
64835	211	3	211	10	The caption should specify whether one or several members have been used to build the Figure 9.26. Behind, there is the question to highllight in the text whether the mean and trends simulated in different members for one model are similar or not. [Martin Ménégoz, France]	Accepted. The caption now indicates that the first ensemble members are used.
85353	212	0	212	0	Cross chapter 9.2, Figure 1 - is this figure name correct? Also what does this partiition by ocean depth look like in models (one might expect it to be rather different due to large errors in the representation of many vertical processes, including the completely spurious mechanism by which Antacrtic bottom waters are formed in models, e.g. Heuze et al, 2013, 2015)? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	TAKEN INTO ACCOUNT. Yes, the figure name is correct - this figure belongs to Cross-Chapter Box 9.2 on Earth's energy inventory and the sea-level budget. The vertical partitioning of heat uptake in CMIP models is shown in Figure 3.25 of Chapter 3.
72151	212	1	212	1	In Cross-Chapter 9.2, Figure 1, panel a with the breakdown of components for Total Earth System Warming, the smaller components like antarctica, greenland, glaciers and sea ice are not well distinguishable. For comprehensiveness I suggest to add the inland water component. As the heat uptake is very small, this component will be not visible on the figure. If interested, we are happy to offer the timeseries of heat uptake by inland waters relative to 1971 (see Figure 1 of Vanderkelen et al., 2020 DOI: 10.1029/2020GL087867). You can contact be at inne.vanderkelen@vub.be. The same comment is made for Box 7.2, Figure 1, panel d. [Inne Vanderkelen, Belgium]	TAKEN INTO ACCOUNT. Figure revised addressing these points.
12513	212	1	212	5	In Sea level budget panel, it will be nice to add an observational total sea level time series (i.e. Dangendorf et al. time series in 19th century or others, Altimetry time series since 1993). [Lijing Cheng, China]	Accepted. Figure revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102049	212	2	212	2	Cross-Chapter 9.2, Figure 1, panel c: Awkward white space on the right-hand side. [IAPSO ECS group review, United States of America]	Accepted. Figure revised.
3337	212	3	212	3	In panel c, could you also show the trends and budget over 1900-1990, 1971-2018, and 1993-2018? That shows that global sea level is clearly accelerating, and that we understand the underlying causes of this acceleration. [Thomas Frederikse, United States of America]	Accepted. Figure and text revised to address acceleration as warranted.
129615	212	3	212	3	In panel c, could authors also show the trends and budget over 1900-1990, 1971-2018, and 1993-2018? That shows that global sea level is clearly accelerating, and that the underlying causes of this acceleration are understood. [Trigg Talley, United States of America]	Noted. These topics are addressed in the revised chapter text and a table, but are not able to be included in the Box.
38121	212		212		Figure 1 left (global energy inventory) could be improved to be distinguished clearly. Too blurry and difficult to recongnize small contributors. [Junhee Lee, Republic of Korea]	TAKEN INTO ACCOUNT. Figure revised addressing these points.
85355	213	0	213	0	Fig 9.27 - this is a really useful figure but it look me a while to understand it. Could it be clarified with a demonstration of how to interpret it for example by labelling a horizonal line at a particular CDF level in at least one panel and explaining in the caption its implications for likely sea-level ranges? [Patrick Hyder, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have replaced this figure with a simpler bar-and-whisker representation.
1793	213	1	213	1	In Fig. 9.27, why are the 2050 panels cut off at 0.4 m? If space is an issue, it would make more sense to cut off the 2100 panels at 2.5 m. [Torbjorn Tornqvist, United States of America]	Taken into account. We have replaced this figure with a simpler bar-and-whisker representation, and sized so that 95th percentile values are not cut off.
67109	213	1	213	1	Fig.9.27: predicted --> projected [Regine Hock, United States of America]	Not applicable. We have replaced this figure with a simpler bar-and-whisker representation.
69233	213	1	213	1	In upper left figure (for 2050 and for RCP8.5), all range of low confidence should be displayed like upper right figure (for 2100 and for RCP8.5) [Kaoru Magosaki, Japan]	Taken into account. We have replaced this figure with a simpler bar-and-whisker representation, and sized so that 95th percentile values are not cut off.
76721	213	1	213	1	Fig 9.27 From the caption itself it is not clear whether all contributions to GMSL are contained in this plot. Is it possible to specify the underlying data sources more explicilty? [Roelof Rietbroek, Germany]	Taken into account. We have replaced this figure with a simpler bar-and-whisker representation, and include a corresponding table in the supplemental material.
2541	213	1	213	1	Suggest to change the brown color of RCP585 in Fig9.27, now it has the same color as the medium confidence range [Tim Hermans, Netherlands]	Not applicable. We have replaced this figure with a simpler bar-and-whisker representation.
29255	213	1	213	18	It looks like the upper tail of the 2050 CDFs for RCP8.5 is cut off. I assume this is because the authors were trying to keep all 2050 CDFs on the same axes, but I think that an adjustment needs to be made to ensure that the tail is not hidden. [Andra Garner, United States of America]	Taken into account. We have replaced this figure with a simpler bar-and-whisker representation, and sized so that 95th percentile values are not cut off.
29257	213	1	213	18	It would be helpful to have the region of the x-axis that is actually shown in the 2050 plots noted in some way on the 2100 plots, to visually emphasize the differences in uncertainty at longer time scales. [Andra Garner, United States of America]	Taken into account. We have replaced this figure with a simpler bar-and-whisker representation, and sized so that 95th percentile values are not cut off.
29259	213	1	213	18	I suggest moving the legend showing the line styles elsewhere. Upon an initial read-through of this figure, I thought for a while that this legend was only referring to the RCP2.6 figure, since that is where it is shown. I think that this legend (as well as the color legend) could be eliminated by noting these categories and how they are shown in the figure caption. [Andra Garner, United States of America]	Not applicable. We have replaced this figure with a simpler bar-and-whisker representation.
67111	213	2	213	10	Fig.9.27: too many acronyms. Please spell out. [Regine Hock, United States of America]	Taken into account. We have replaced this figure with a simpler bar-and-whisker representation.
62215	213	3	213	4	wrong RCP in figure description (twice RCP 4.5 instead of RCP 2.6) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. We have replaced this figure with a simpler bar-and-whisker representation.
102051	213	3	213	4	Figure 9.27: "RCP 4.5 (lower)" shoud be corrected to RCP 2.6 (lower) [IAPSO ECS group review, United States of America]	Not applicable. We have replaced this figure with a simpler bar-and-whisker representation.
108343	214	1	214	1	Flowchart is really confusing, was wondering if it can be made cleaner or just replaced with a table. [Aakash Sane, United States of America]	Not Applicable. This figure has been removed.
68555	214	1	214	7	Figure 9.28 is full of technical details, many of which are not well explained, I think that would be better suited for the technical annex than the main report chapter. [Robert Hallberg, United States of America]	Not Applicable. This figure has been removed.
102053	214	3	214	3	Figure 9.28: With this figure, I have appreciated that it is not trivial to compile sea level change estimates; however, the details of the figure are obscure (at least to me). I suggest to include more information in the figure caption, explaining the terms of the boxes and how the flow chart works; I presume many people will focus on figures when skimming through the report, my perspective is that the figures should be self-explanatory. [IAPSO ECS group review, United States of America]	Not Applicable. This figure has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88815	214	3	214	7	Figure 9.28: Flowchart illustrating the procedure for computing the global mean sea level change (red), regional relative sea level change (purple) and extreme sea level change (purple) projections framework. White blocks with black outline represent external forcing, blue blocks represent model options, orange block represents tide gauge data, yellow blocks represent sea-level contributors, white blocks with purple outline the corrections for vertical land movement. The block colors are not correct [Rosemary Vieira, Brazil]	Not Applicable. This figure has been removed.
83037	214		214		Figure 9.28: While I support the authors attempt to show the process of sea-level projections, I find this diagram a little hard to follow and I have some subsequent comments that might be helpful. I wonder whether it might be better to put this figure (and more detailed discussion) in Supporting Materials, as was done for Ch13 AR5 WG1. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. This figure has been removed.
83039	214		214		Figure 9.28: My understanding is that GIA also affects Earth's geoid and is not limited to vertical land motion, and therefore also affects geocentric sea-level change. I think this should be clarified in the text somewhere (even if for the schematic it is stated that VLM is the dominant process for local relative sea-level change). For clarity, I think "thermal expansion" should read "global thermal expansion". I also wonder if it would be clearer to have the "global thermal expansion" box flow into a subsequent box called "sterodynamic sea-level" (for consistent nomenclature with Gregory et al, 2019), since the procedure for CMIP models is to combine the zostoga and zos fields? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. This figure has been removed.
102055	214		214		Figure 9.28:Purple outline in boxes hard to see; consider drawing the outline thicker [IAPSO ECS group review, United States of America]	Not Applicable. This figure has been removed.
33553	214				Figure 9.28. Please indicate what represent the red lines vs de black lines. [Guiomar Rotllant, Spain]	Not Applicable. This figure has been removed.
78737	215	1	215	1	The Vertical Land Movement field shows a number of spatially smoothed peaks which dominate regional sea level. It is not physically realistic that a local effect such as subsidence,measured at 1 tide gauge (e.g., Galveston) affects sea level over a wide region (e.g., the whole Gulf of Mexico). In addition, if these peaks result from groundwater extraction, this effect should be partially offset by a negative peak in the Land Water Storage field. I strongly suggest using GIA models for VLM rather than extrapolation of tide gauge records. Otherwise, this spatial smoothing must be resolved. [Erwin Lambert, Netherlands]	Rejected. The degree to which peaks spread depends upon the spatial coherency of signals in the tide gauges. For example, in the western Gulf of Mexico, there is a broad region of subsidence shown because this appears, at a varying amplitudes (in mm/yr, with 1-sigma errors: CORPUS CHRISTI : 2.68 ± 0.3; ROCKPORT 3.75 ± 0.25; FREEPORT 5.87 ± 0.25; GALVESTON II 4.59 ± 0.13; SABINE PASS NORTH 3.54 ± 0.47; EUGENE ISLAND 8.08 ± 0.45; GRAND ISLE 7.07 ± 0.25). Given this, it is entirely appropriate for a statistical model to extend the area of high substance into the GoM between the Texas and Louisiana coasts.
68563	215	1	215	1	The label "ocean dynamics" over panels (c) and (d) is confusing, because "ocean dynamics" should contribute a spatial pattern with a near-zero global mean. "Ocean Steric & Dynamics" would make it clearer why there is a strongly postiive contribution. [Robert Hallberg, United States of America]	Accepted. New version of figure (9.26) has been revised for clearer labelling.
29263	215	1	215	2	These figures are extremely small and difficult to read in their current form. Would it be possible to break this into two figures, one for 8.5, and one for 2.6? The figures could then be arranged similar to 9.30. This would greatly aid with the readability of plots and maps, and would likely also help to correct the apparent distortion of the maps. [Andra Garner, United States of America]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
69235	215	1	215	2	In Figure 9.29, the size of distribution maps should be larger, as are Figure 9.30 and Figure 9.31. [Kaoru Magosaki, Japan]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
22725	215	1	215	2	All panels are very small and I wonder whether a portrait orientation and commensurately larger panels would help here? [Peter Thorne, Ireland]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
2537	215	2	215	2	Color of AiS should be different from the very likely range in Fig. 9.29 [Tim Hermans, Netherlands]	Accepted. New version of figure (9.26) has been revised for clearer component identification.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88817	215	3	215	8	Figure 9.29: Global mean and regional relative sea-level projections (m) by contribution for the SSP5-8.5 and SSP1- 4 2.6 scenarios. (a) and (b) global mean contributions to sea-level change as a function of time, relative to 1995-2014, (b)-(k) regional projections of the sea-level contributions in 2100 relative to 1995-2014 for SSP5-8.5 (c, e, g) and SSP1-2.6 (d, f, h). (c)-(d) Ocean contribution (includes ocean dynamics, steric change, inverse barometer), (d)-(f) ice sheets and glaciers, (g)-(h) Terrestrial Water Storage, (i) Vertical Land Movement (common to both SSPs). The black letters do not seem to be correct. [Rosemary Vieira, Brazil]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
29261	215	3	215	10	There are a number of mistakes in the caption pertaining to the letters for the different sections of the figure that cause the caption to not completely make sense with the figure at the current time. [Andra Garner, United States of America]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
83041	215		215		Figure 9.29: I would prefer that the different cryosphere elements be separated out in this figure – I think it is useful for the reader to understand which features correspond to Greenland, Antarctica and glaciers. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
102057	215		215		Figure 9.29: For figures (a) and (b), change the ylabel to " △ Sea Level (m) " to be more specific [IAPSO ECS group review, United States of America]	Accepted. New version of figure (9.26) has been revised for clearer labelling.
38123	215		215		The color of Figure 9.29 left (2-d contribution maps) could be improved to be distinguished clearly. [Junhee Lee, Republic of Korea]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
33555	215				Figure 9.29. Figs a and b, add label in the Y axis (eg. Projected Sea Level Change (m)). In Fig. a, it is not possible to read de colours of contributors, please increase font. [Guiomar Rotllant, Spain]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
22727	216	1	216	1	The three duplicated labels in the left hand panel are not required in my view [Peter Thorne, Ireland]	Not applicable. Time series (now Fig. 9.27) and maps (Fig 9.28) have been separated.
2539	216	1	216	1	The high uncertainty west of North America should be explained [Tim Hermans, Netherlands]	Accepted. The high uncertainty in projections around the Alaska and the Aleutian islands arises from the tectonic contribution to vertical land motion, which varies greatly over short distances in this region.
78739	216	1	217	1	There are a number of odd discontinuities and spatial fields in the uncertainties in Fig 30efg and 31efg. These are very difficult to interpret, in particular around the Aleutians and appear erroneous. [Erwin Lambert, Netherlands]	Accepted. The high uncertainty in projections around the Alaska and the Aleutian islands arises from the tectonic contribution to vertical land motion, which varies greatly over short distances in this region. While the boundary of this region is arbitrary, it is not erroneous.
69237	216	1	217	5	Due to the difference of the scale of y-axis, panel (a) of Figure 9.30 and panel (a) of Figure 9.31 give different impression although the values of projected sea level change are almost the same level. It would be better to use the same scale of y-axis in both Figures. [Kaoru Magosaki, Japan]	Accepted. New version of figure (9.27) has been revised for clearer identification. The other figure has been removed from the main text.
3339	216	3	216	3	This figure looks really clear! Don't know whether it helps, but would it be an idea to also show the 20th-century sea-level curve in panel a? That could make clear that 21st-century sea level will rise much more than 20th-century sea level. [Thomas Frederikse, United States of America]	Accepted. New version of figure (9.27) has been revised for clearer component identification. The historical data has also been added.
129617	216	3	216	3	Consider also showing the 20th-century sea-level curve in panel a. That could make clear that 21st-century sea level will rise much more than 20th-century sea level. [Trigg Talley, United States of America]	Accepted. New version of figure (9.27) has been revised for clearer component identification. The historical data has also been added.
39115	216	3	216	6	Would prefer to have projected S L change under scenarios in a figure of its own and have panels b-g in a separate figure since panel a will be widely used. Also would like to see the very likely ranges better visualized so they all can be uniquely distingushable. [Ola Kalen, Sweden]	Accepted. New version of figure (9.27) has been revised to show only timeseries. The historical data has also been added.
38125	216		216		Legends for solid lines of Figure 9.30 left is not clear. The color of Figure 9.30 middle (median sea level) could be improved to be distinguished clearly. [Junhee Lee, Republic of Korea]	Accepted. New version of figure (9.26) has been revised for clearer component identification.
83043	216		217		I wonder if Figure 9.30 and 9.31 could be usefully combined? (i.e. showing the warming level and scenario perspectives side-by-side). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure 9.31 is now removed from the main text.
33557	216				Figure 9.30. Figs a, add label in the Y axis (eg. Projected Sea Level Change (m)). [Guiomar Rotllant, Spain]	Not applicable. Figure 9.27 now provides the time series projection. Figure follows IPCC style.
62223	217	1	217	1	figure 9.31: label of colormaps for b)-g) overlaps with the plots [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Figure removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80683	217	1	217	1	it would be useful on panel a to indicate on the x axis marks for the other decades (2030, 2050, 2070 and 2090). Figure 9.31 is extremely important for small low lying island states and it will probably be used for planning purposes so this additional information is needed. [Helene Jacot Des Combes, Marshall Islands]	Not applicable. Figure removed.
22729	217	1	217	1	The three duplicated labels in the left hand panel are not required in my view [Peter Thorne, Ireland]	Not applicable. Figure removed.
39117	217	2	217	5	Would like to see the very likely ranges better visualized so they all can be uniquely distingushable. [Ola Kalen, Sweden]	Not applicable. Figure removed.
67113	217	3	217	7	Fig.9.32: too many acronyms. Please spell out. [Regine Hock, United States of America]	Accepted. New version of figure has been revised and labels have been simplified.
102059	217		217		Figure 9.31: It seems this figure follows on from Figure 9.30 and the time period used there was 1995-2014. However here the time period is listed as 1996-2014. [IAPSO ECS group review, United States of America]	Not applicable. Figure removed.
33559	217				Figure 9.31. Figs a, add label in the Y axis (eg. Projected Sea Level Change (m)). [Guiomar Rotllant, Spain]	Not applicable. Figure removed.
16451	218	1	218	1	Fig 9.32, possibly move “long-term sea-level change (m)” or add “m” as the y-axis label [Julian Mak, China]	Accepted. New version of figure has been revised and labels have been simplified.
5679	218	1	218	7	Figure 9.32: Please revise this figure. The Y-axis lacks a legend (what is shown? Sea level rise in [cm]?). The abbreviations should be explained and there are nor grey dots that can be identified in the graph as provided for review. [Joachim Rock, Germany]	Accepted. New version of figure has been revised and labels have been simplified.
107485	218	1	218	7	There is no grey dot and range indicating constraint from the Common Era as stated in figure caption [Jennifer Walker, United States of America]	Accepted. No longer indicate Common Era range in the caption.
17157	218	1	218	9	Consider leveraging the Cenozoic sea-level record of Miller et al. (2020, 110.1126/sciadv.aaz1346) [Robert Kopp, United States of America]	Rejected. Other useful datasets were added.
39119	218	3	218	7	Add minor ticks on x-axis for clarity. [Ola Kalen, Sweden]	Accepted. New version of figure has been revised and labels have been simplified.
102061	218	4	218	5	Figure 9.32: There should be an acronym for early Eocene Climatic Optimum (eCCO) in the caption. LIG and MPWP needs to be defined as well. [IAPSO ECS group review, United States of America]	Not applicable. EECO has been removed. Other acronyms are now defined earlier in the chapter (Table 9.6) and elsewhere in the report.
80685	219	1	219	1	the indication of labels a-e is not visible on the figure [Helene Jacot Des Combes, Marshall Islands]	Accepted. Corrected.
52531	219	1	219	1	Fig 9.33 is rather dubious altogether, although at first glance it is compelling. Each subplot is effectively rescaled to maximise the similarity in trend between the left and right axes, which are on completely different scales (and not even in the same dimensions). There is no way of knowing whether zero days flooding is due to lower levels, or specific missing data (perhaps the instrument broke during storms, but worked on enough days to give mean sea-level?). There is no indication of which sites are subject to large nodal variations. 1995-2014 is a poor period for comparison for extremes, as it includes 2 peaks in the nodal tidal (each end), so is potentially biased high. [Joanne Williams, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The figure is supported by considerable text in Chapter 9 and 12, which allows a deeper appreciation. The two axes are clearly laid out and in different units.
107487	219	1	219	6	a, b, etc are not labeled on the figure [Jennifer Walker, United States of America]	Accepted. Labels added.
66009	219	1	219	7	Suggest changing title on (a) to: "Change in current average minor extreme sea level frequency relative to 1960-1980 average" or "Change in exceedance frequency of 99th percentile value relative to 1960-1980 average". Currently, the text does not show that flooding (a physical impact of extreme sea level) actually occurs at the level associated with the 99th percentile. [Kushla Munro, Australia]	Rejected. The definition of minor flood is not included in the caption.
66011	219	1	219	7	Suggest clarification. Is considering the exceedance of 99th percentile over two (seemingly arbitrary) 20 years periods representative of long term trends? For example, Sydney had much more frequent storms in 1970s than now. Therefore its trend is negative, even though coastal flooding is becoming more common overall. See: https://www.researchgate.net/publication/335854347_Rising_Tides_Tidal_Inundation_in_South_East_Australian_Estuaries. [Kushla Munro, Australia]	Noted. The figure shows these data alongside, without particular interpretation. The text goes into process detail about what causes changes in floods and changes in still water levels. Chapter 12 provides regional analysis of changes in means and extremes.
97067	219	1	219	25	Fig. 9.33: what is minor extreme levels in caption compared to the extreme still water levels mentioned in the text? [Nicole Wilke, Germany]	Accepted. Definition provided.
80687	219	4	219	4	it should be '(b-e)' not '(e-e)' [Helene Jacot Des Combes, Marshall Islands]	Accepted. Caption corrected.
66013	219	4	219	4	Suggest changing to: " b-f" rather than " e-e". [Kushla Munro, Australia]	Accepted. Caption corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88819	219	4	219	5	Figure 9.33: (e-e ?) Annual mean sea level (blue) and annual occurrences of extreme sea levels over the 1995- 5 2014 99th percentile daily maximum (yellow) at six selected tide gauge locations. [Rosemary Vieira, Brazil]	Accepted. Caption corrected.
29269	220	1	220	16	Figure (maps) need to be larger [Andra Garner, United States of America]	Accepted. Enlarged.
102063	220		220		Figure 9.34: We think this should be presented as a different colour map because the darker blue along the coastlines does not do justice to the clarity of the information [IAPSO ECS group review, United States of America]	Accepted. Colormap improved and logarithmic scale used.
109243	220		220		Figure 9.34 shown projected frequency amplification factors for the 1% average annual probability coastal extreme water level in 2050 (left) and 2100 (right). The frequency amplication factors should be "by 2050" and "by 2100" ? [A.K.M Saiful Islam, Bangladesh]	Accepted. Colormap improved and logarithmic scale used.
22731	221	1	221	2	Sharp is not exactly the right term I think here. It still takes several thousand years which isn't exactly analogous to fast in the publics minds. [Peter Thorne, Ireland]	Accepted. Caption rewritten
7907	221	1	221	2	FAQ 9.1, Figure 1. The y-axis of the diagram on the left lacks the labels to indicate the elevation in km [Emilia Guisado-Pintado, Spain]	Accepted. Label removed
32451	221	2			What are the lines in the ice sheet? Isochrones? Trajectories? Needs to be explained and validated that they are drawn correctly. [Olaf Eisen, Germany]	Accepted. Caption rewritten and figure redrawn
5449	221		221		FAQ 9.1 Figure 1: the figure caption seems incomplete. I would advise something like “Sharp sea level rises (during abrupt ice sheet retreat) are followed by long gradual sea level falls (during the slower ice sheet build-up).” [Marie Cavitte, Belgium]	Noted. Caption rewritten
5451	221		221		FAQ 9.1 Figure 1: I am not sure that having the MISI cartoon is relevant here. It is not discussed in the corresponding FAQ 9.1 text and might induce more questions than answers? [Marie Cavitte, Belgium]	Noted. Text rewritten
5453	221		221		FAQ 9.1 Figure 1: The warming with altitude cartoon doesn’t seem to illustrate very well the point it is making. I’m wondering (if it’s not too late), if it wouldn’t help in coloring the (now blue) air with a gradient of blues-reds (or something illustrating that lower elevations are warmer). Then it would help the readers to visually see that ice at lower elevations are not in direct contact with the warmer air at lower altitude? I would only keep this sub-cartoon panel to illustrate what is discussed in the text. However, I wonder if it might be possible to add the permafrost and sub-sea clathrates as a sub-panel to illustrate what is discussed in the FAQ 9.1 text. Or at least ref to Fig 9.1 from the main text which has these processes illustrated. [Marie Cavitte, Belgium]	Noted. This figure has been revised, but the specific suggestions here are too complex for the FAQ context. Instead, the figures have been further simplified.
5455	221		221		FAQ 9.1 Figure 1: The thin gray variable sea level curve is not labeled. What is its purpose? If it doesn’t add information, I would suggest removing it. However, it would be good to add some level of uncertainty quantification, as it is an important concept to insist on. [Marie Cavitte, Belgium]	Accepted. Caption rewritten
88821	221		221		FAQ 9.1, Figure 1: There is not any number relative to the Elevation scale (km). [Rosemary Vieira, Brazil]	Accepted. Label removed
106747	221		221		There should be a caption explaining the different mechanisms shown in the figure. I assume this is the marine ice sheet instability mechanism and melt-elevation feedback. [Kevin Bulthuis, United States of America]	Accepted. Caption rewritten
33561	221				FAQ 9.1, Figure 1. In smaller figures add scales and units in Y axis. [Guiomar Rotllant, Spain]	Rejected. These are indicative vertical axes only, not quantitative.
80919	222	0	222	5	To be updated [Louise Sandberg Sørensen, Denmark]	Accepted. Legend rewritten and figure redrawn
103923	222	0	222	5	To be updated [Philippe Tulkens, Belgium]	Not applicable. Figure revised.
93575	222	1	222	1	FAQ9.2 - Pie charts are not an intuitive way of showing the components of sea-level rise. Using a bar chart or some other method using rectangles will be much more easily understood. [Miriam Jackson, Norway]	Accepted. Legend rewritten and figure redrawn
91103	222	1	222	1	FAQ 9.2 Fig 1. This is a nice way to show the partitioning but as it stands it does not show any uncertainties. Tus by 2300 ~half the SLR will be due to thermal expansion according to this figure but that is an extremely uncertain inference....Even if the % cis changed for the final figure (I can see they're all the same) the uncertainty will still be unclear. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Legend rewritten and figure redrawn including some sense of uncertainty
7909	222	1	222	2	FAQ 9.2, Figure 1. The figure is a bit confussing. SLR is represented as oblique arrows intead of straight. Also the scenarios are represented as SSPx-x.x which is not clear what scenarios refers. Further, the right end of the figure ends with two arrows but it's not clear is represneting the future?. I suggest to re do the figure including elements that help the interpretation. [Emilia Guisado-Pintado, Spain]	Accepted. Legend rewritten and figure redrawn

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5467	222		222		FAQ 9.2 Figure 1: I would recommend making the legend larger, it's difficult to read it as is. [Marie Cavitte, Belgium]	Accepted. Legend rewritten and figure redrawn
88823	222		222		FAQ 9.2, Figure 1: The size of the font is too small. [Rosemary Vieira, Brazil]	Accepted. Legend rewritten and figure redrawn
106745	222		222		In the figure, the different contributions associated with the different components seem to be the same in terms of percentage for the high and low emissions scenarios and 2050-2100 and 2250-2300. It would be interesting to have different percentages that reflect more accuretaly the predicted contribution of the different sources. I mean this figure is rather qualitative and should be more quantitative. [Kevin Bulthuis, United States of America]	Accepted. Legend rewritten and figure redrawn
33563	222				FAQ 9.2, Figure 1. Please increase the font of the letter of the legends, as they are is not possible to read them. [Guiomar Rotllant, Spain]	Accepted. Legend rewritten and figure redrawn
83065	222				FAQ 9.2. Figure 1. I think it may be helpful to show some example projections to help convey the message. For example, Palmer et al (submitted) show local tide gauge records alongside RCP2.6 and RCP8.5 projections. This would help to illustrate the main points from the box; (1) projections for coming decades are largely scenario-independent; (2) large local year-to-year sea-level variability; (3) large geographic variations in the rate of projected sea-level change. Optional extras would include: (i) a Hawkins and Sutton (2009) analysis to illustrate these points; (ii) a component-breakdown for the projections at each location to show differences (GIA is paritcularly important in this regard). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Legend rewritten and figure redrawn
83337	223	1	223	3	Mark "Greenland" on the schematic for FAQ 9.3 Figure 1. [Robert Massom, Australia]	Noted. Figure improved
5475	223		223		FAQ 9.3 Figure 1: The name of the RAPID-WATCH/MOCHA is never explicitly stated in the text. So I would make sure it is not here, or added in the text of FAQ 9.3. I see that the figure is still a placeholder so will not comment further on this figure [Marie Cavitte, Belgium]	Accepted. Legend rewritten and figure redrawn
102065	223		223		FAQ 9.3, Figure 1: Unclear what the point of this figure is, particularly without a caption or a reference in the text. Suggest either deleting the figure or explaining it in the text. [IAPSO ECS group review, United States of America]	Accepted. Legend rewritten and figure redrawn
81705	All				As the lead author of Zanna et al 2019, I feel uncomfortable with the use of "observations" when referring to our product in a few places in the chapter. We are not using time-dependent direct measurements in the ocean interior such that the term observation could be misleading, "observation-based" or "reconstruction" are probably more appropriate in my opinion. [Laure Zanna, United States of America]	Accepted. We have revised that aspect and do not refer to Zanna et al., 2019 as an observation product.
46615	Full chapter				Regularly unclear whether referring to models or observations (e.g. p23, lines 38:54) [Céline Heuzé, Sweden]	Accepted. Text is now clearer as to when referring to models and observations.
46639	section 9.4				Consider creating subsections just about ice shelves to improve clarity [Céline Heuzé, Sweden]	taken into account, the last paragraphs of section 9.4.2.1 present assessment of ice shelves
46635	throughout s				Confusing choice of word: sea ice "coverage" for most observationalist means availability of data, when it is here used in the context of sea ice "cover" or "extent". [Céline Heuzé, Sweden]	Rejected. We think that the term "sea-ice coverage" is sufficiently distinct from "data coverage"
90475		52	52	52	double space between "termperature" and "evoution". [Holly Kyeore Han, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
33565					nothing [Guiomar Rotllant, Spain]	Noted
115517					A general comment about use of palaeo in Ch9: it seems a bit scattered and disjointed throughout. The messages from Palaeo are powerful and could be used more effectively in the storyline. Perhaps consider consolidating/reorganizing? [Robert DeConto, United States of America]	Taken into account. paleo information is organized where relevant in ocean, cryosphere and sea level sections. Now each section has been rewritten with considering storyline style with a more consistent order in each section, making paleo discussion easier to find in the context of particular processes being assessed.
115519					Check use of "deep uncertainty" throughout- perhaps with an effort to be consistent with SROCC? [Robert DeConto, United States of America]	Accepted. We have worked to ensure consistency with the definition of deep uncertainty in SROCC, specifically related to lack of agreement on "(1) appropriate conceptual models that describe relationships among key driving forces in a system; (2) the probability distributions used to represent uncertainty about key variables and parameters"
116809					Congratulations for the maturation of the chapter, and also for coordination and complementarity with other chapters. [Valerie Masson-Delmotte, France]	Noted

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116811					Please consider carefully statements related to "acceleration". There is clearly an acceleration signal for GMSL, but for other variables it could be a higher rate of increase rather than an acceleration (strictly speaking). [Valerie Masson-Delmotte, France]	Taken into account. Apart from GMSL, acceleration is only referred to in its correct usage associated with glaciers.
116813					I have two main concerns with the style for some of the chapter sections. The first one is the starting point (AR5, SROCC) often missing (with a reassessment of literature). The second one is the use of the calibrated language. Often, confidence levels are provided under parenthesis, for a long sentence, without a clear attribution of that confidence level to a specific finding. There is a need to improve clarity in the use of the confidence language. [Valerie Masson-Delmotte, France]	Accepted. AR5/SROCC starting points are clearer and confidence language is linked better to findings.
116815					While the chapter includes an assessmet of insights from paleo information, this is not reflected in the ES. [Valerie Masson-Delmotte, France]	Taken into account, key paleo advances are now in ES merged into warming levels
129619					[GAPS] It would be useful to highlight knowledge gaps in each chapter. One example is the inability to "close the budget" on local sea-level rise (page 86, lines 7-9). Highlighting those gaps could trigger new research, which in this particular case could be of great use to local decisionmakers. [Trigg Talley, United States of America]	[noted] the setup of this section has changed, the gaps and uncertainties are discussed in detail in the individual sections.
116821					Relevant aspects related to abrupt change and irreversibility could be reflected in the ES. It is hard from the ES to see what is particularly novel compared to AR5 and SROCC (what has been strengthened, or modified, or new aspects). [Valerie Masson-Delmotte, France]	Noted. Advances since SROCC are highlighted in the text.
129621					There are many acronyms used; several are never defined. It would be helpful to have all acronyms compiled and spelled out in full. [Trigg Talley, United States of America]	Taken into account. Acronyms are spelled out where possible.
129623					[ACCESSIBILITY] There are important topics, such as AMOC changes and dynamics, that are scattered in different places with similar contents. This organization issue makes the document difficult to read and unnecessarily long. The issue is more apparent between chapters. [Trigg Talley, United States of America]	Noted. Figure 9.1 is intended to address this issue and direct the reader to the correct sections in other chapters.
129625					[CONFIDENCE] The uncertainty lexicon is helpful and necessary for climate science, as it is evolving and often associated with confidence in data and conclusions. However, in practice, it appears the use of the designations in Chapter 9 is at will and lacks consistency. [Trigg Talley, United States of America]	Taken into account. The confidence statements have been rechecked to ensure that language has been used correctly.
129627					Many sections in this chapter are a descriptive collection of others' investigations, which is not equivalent to a fine synthesis or assessment. No tangible conclusions or scientific consensus is offerred for readers and technical managers. [Trigg Talley, United States of America]	Noted. The text has been revised to have clearer assessment statements
129629					[CONFIDENCE] Single references are relied on for very important statements in many parts of the chapter. This is concerning in many cases, and raises questions about the level of consensus that is being communicated on some of these topics. [Trigg Talley, United States of America]	Noted. Single papers generally give lower confidence in the chapter.
116831					There are sometimes long sections reviewing literature but without a clear summary paragraph. Why is the assessment done, and what are the new insights? [Valerie Masson-Delmotte, France]	Noted. The text has been revised to have clearer assessment statements
129631					Several [placeholders] are referenced throughout this chapter. These placeholders, which correspond to data, results, and interpretations, need to be resolved and inserted to complete this chapter. Otherwise the chapter is viewed as incomplete. [Trigg Talley, United States of America]	Noted
129633					The Paris Agreement does not discuss temperature targets. Re-word to Paris Agreement temperature goals to be consistent with other chapters in this report. [Trigg Talley, United States of America]	Noted. Paris Agreement is no longer referred to in chapter 9.
129635					[PRECISION] Citations are very poorly done, and single references and/or authors are very heavily relied upon. A quick analysis is that Golledge is cited 63 times. Edwards is cited 49 times. This bias should be addressed throughout the Chapter 9 draft. In addition, citations are often used in discordance with the publications they actually reference -- for example, Seroussi (2017) not cited in the WAIS modeling section (which is difficult to understand), and cited somewhere else for ice/ocean interactions (which is fine). The lack of clear coherence in the way citations are used is a detriment to the overall clarity of the document. [Trigg Talley, United States of America]	Taken into account. The citations for CLAs and LAs has been checked. They represent 6% of the total citations and the numbers are: [('Adalgeirsdottir', 7), ('Drijfhout', 0), ('Nurhati', 0), ('Sallee', 7), ('Hemer', 0), ('Krinner', 1), ('Ruiz', 1), ('Xiao', 2), ('Mix', 2), ('Hewitt', 3), ('Fox-Kemper', 4), ('Nowicki', 10), ('Yu', 11), ('Slangen', 15), ('Edwards', 17), ('Notz', 19), ('Golledge', 20), ('Kopp', 26)]. Given the small % of total citations, we think that this demonstrates that the chapter has worked to represent a huge body of literature.
129637					Make sure to add a note early in this chapter about how ocean acidification and deoxygenation are covered in Chapter 5. [Trigg Talley, United States of America]	Taken into account. The introduction links to Chp 1 and chapter map where connections to these topics are made.

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129639					[RISK] Chapter 9's discussion of mechanisms that could drive higher sea level rise outcomes by 2100 than showcased (e.g., very likely) is informative and important. However, there is little-to-no-mention of these less probable outcomes within the Summary for Policymakers. There is a need to know more about the 1% chance outcome, for example, for many types of risk-based decisionmaking that also include the highest emission scenario. Simply not providing it essentially takes it off the table. Also, it would be more helpful if the time target was 2100-2150 at this point to keep with century scale focus and perhaps more discussion of how measured variability stacked on top of scenario projections would compound annual flood risk as it does already. [Trigg Talley, United States of America]	Accepted. LLHI sea-level rise and century-scale sea-level rise is now highlighted in the ES.
116841					What is the cause of the pattern of AMOC variations in CMIP6 and is it a robust feature or an artefact of some models? [Valerie Masson-Delmotte, France]	Noted. Comment is unclear as to where the objection is located. AMOC section has been rewritten.
116853					Check the use of the subscript for CO2 systematically [Valerie Masson-Delmotte, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
33403					All over the chapter. Avoid starring sentences with abbreviations: AABWs, AR5, CMIP3, CMIP5, CMIP6, ESMs, GIA, GMSL, ISMIP6, LARMIP, LARMIP2, MHWs, MICI, MISI, MPWP, R2, RCP8.5, RSL, SMB, SROCC, [Guiomar Rotllant, Spain]	Taken into account. Acronyms have been avoided throughout the chapter where possible.
116873					Please be explicit on changes compared to SROCC when reporting findings, this is really something helpful for the audience of IPCC. [Valerie Masson-Delmotte, France]	Accepted. All sections now discuss advances since SROCC.
29325					very good work. [Zangari del Balzo Gianluigi, Italy]	Noted
116891					The assessment of the time of emergence of human induced signals provided in several sections of this chapter, on different aspects, could be reported in the ES and are relevant for the TS/SPM. [Valerie Masson-Delmotte, France]	Noted. ToE is discussed in some parts of the chapter and the emergence of regional sea level is elevated to the ES.
116901					Some aspects of this chapter could contribute to the Annex on models (eg ice sheet models) [Valerie Masson-Delmotte, France]	Accepted. Both ice sheet and glacier models are included in Annex II.
61865					include permafrost (perennial frozen ground) [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	taken into account, permafrost is included
132013					Sea level estimates may appear overly conservative for the mid term up to 2100. To reflect the preliminary nature of these estimates a box discussing current methodological advances and insufficiencies e.g. around ISMIP6 as well as results from alternative approaches could be presented to illustrate the background of deep uncertainty around these estimates and more generally, around the development of sea level rise over time. Last not least, the potential for much larger changes is illustrated by relevant comparisons with palaeo periods. [Hans Poertner and WGII TSU, Germany]	Taken into account. High-end sea-level rise is addressed in Box 9.4 and integrated into the presentation of sea-level projections.
116917					Please seek to report the confidence level (eg for observational datasets) in figures [Valerie Masson-Delmotte, France]	Rejected. The confidence level for datasets is not reported in figures. A number of datasets are shown in figures to represent the uncertainty.
83895					Generelly well written with good breferencing to SROCC chapters. [Ulf Molau, Sweden]	Noted
83897					Lots of placeholders remaing, making evaluation of text and tables difficult at this stage. [Ulf Molau, Sweden]	Noted
112069					The SOD version of the Interactive Atlas (IA) includes some oceanic variables (O2, pH and SST) for CMIP5 (the idea is to compute these indices also for CMIP6) and incorporates specific open ocean regions ("biomes"). We would need to coordinate with Ch9 the most suitable oceanic variables (including SRL) and the regions (one or several categories) to be included in the IA so they support and extend some of the assessment made in Ch9 (if possible at all using only CMIP data). The IA could also include specific gridded /points products used in Ch9. [jose manuel gutierrez, Spain]	Accepted. This transfer of data and evaluation has been accomplished.
112071					Would be good to coordinate with the Interactive Atlas the possiblity to include specific products (gridded/point timeseries if possible) for sea level, sea ice and/or snow cover. We could include information building on CMIP (scenarioMIP) in case that is suitable (there are no plans to include HighResMIP and OMIP2 information though). [jose manuel gutierrez, Spain]	Accepted. This transfer of data and evaluation has been accomplished for relevant variables.

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115915					FAQ9.1 Please check coherency with the FAQ on permafrost (especially on timescales of responses). I am not sure that the whole third paragraph (paleo) provides elements of response to the FAQ question (response time, reversibility). I am also concerned about the links between global temperature and sea level, without being explicit on polar temperature change. I do not understand what relationship is described to be consistent, and also on which time scale of response. I would expect more clarity on what is reversible and what is not (see also chapter 6 of SROCC, table), and on which timescale. [Valerie Masson-Delmotte, France]	Noted. Text rewritten, especially headline where the direct answer to questions is provided.
115917					FAQ9.2 "further 7-25 cm" by 2050 compared to what ? (today? 2000?). The reference to the "Little ice age" could include an explanation for this (delayed response to cooler conditions linked to frequent major volcanic eruptions)?. I am puzzled by the use of the term "predictions" instead of projections ("scientists predict"). It would be better to stress what are the main sources of uncertainty (incl the Antarctic ice sheet dynamical instability). For the proposed figure, please check the meaning of the size of the circle and how to capture extreme sea level event change. What about providing insights for 2050 (committed), 2100 (effect of scenario), and 2250-2300 (rather than an average between 2050-2100 which could mask differences by the end of the century) + source of deep uncertainty (as a "decision tree", what if...)? [Valerie Masson-Delmotte, France]	Accepted. These changes have all been implemented except the 2300 case, for which projections are not settled.
6863					Second Order Draft – Chapter 9 – IPCC AR6 WG1 Expert Review 2020 by Nils-Axel Mörner Last year I gave a very critical review of the First Order Draft. Now it will be interesting to see if anything has improved and changed. Of course, it is the same shameful dis-balance of the authors involved to the applauding side of the IPCC. Nothing is being written of mentioned about all the available facts giving quite another picture of what has been going on, is going on and probably will occur along the shores at the land/sea interface all around the globe. Chapter 9 has a fine set up and organization, and could have become a wonderful well of information, if it were not for the destruction of the authors presenting just the same old story following a predetermined agenda, instead of facts. I have gone through the new manuscript (Second Order Draft). Is there any improvement? – no. Is there any listening to the critique from reviewer? – no, hardly not at all. FAQ 9.1: You claim that the effects “will continue for a very long time”. But this is only if things are as you claim. If the climate changes primarily are the function of Solar variability, the answer would be completely wrong. FAQ 9.2: After all exaggerations in the main text, one gets quite surprised to read that “sea level will rise by a further 7-25 cm by 2050”. Such a rise would hardly pose any real problems. (personally I would even claim: +5 cm ±15 cm by year 2100). FAQ 9.3: The Gulf Stream exhibits a beat that follows the changes in Earth’s geomagnetic shielding (from the Solar Wind control) very closely. This has, of course, nothing to do with any “man-made” effects. This alternative explanation is not even mentioned. In conclusion, things have NOT improved in this Second Order Draft, which implies two things: (1) The authors are totally immune for critiques, new facts and alternative solutions – just pushing their IPCC agenda forward. (2) In this situation, there is only one constructive thing to propose and that is that a totally similar parallel project is set up where the authors are experts on sea level changes not working from a pre-set IPCC agenda but from available facts and theories.	Rejected. Unsubstantiated claims and personal opinions
115919					FAQ9.3 I find the title of the FAQ confusing, as it refers to the Gulf Stream, while the substance is about AMOC. Also, my understanding is that the Gulf Stream is also driven by the Earth's rotation (not said). Can the Gulf Streamreally shut down? Also, I do not understand the reerence to "especially during the end of ice ages" (this is not fully consistent with the substance in the chapter assessment). Can you please check the coherency between the FAQ and chapter 9? [Valerie Masson-Delmotte, France]	Noted. The principle here is that the Gulf Stream is more familiar than the AMOC. The title is a narrative hook to get the general audience reading (the popular media tends to talk about a Gulf Stream shutdown) and then explain the two circulations that the GS participates in and which is sensitive to climate change and which is not. Coherency has been improved.
115183					A general comment on the chapter is thatI found a few places where time periods (e.g. by 2100, within multiple centuries or millenia in the future, universally) are not clearly defined. I also feel that the point that much greater changes could happen beyond 2100 could be further highlighted in the executive summary. [Natalya Gomez, Canada]	Accepted. Time periods have been made clear as far as possible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115185					Thank you to all the authors of this chapter for your substantial and excellent contributions [Natalya Gomez, Canada]	Noted
35323					The authors could also cite the recent paper Moorman et al, 2020 for "shelf water will be too light to sink to the bottom ocean, thereby slowing the lower cell overturning circulation" (Moorman, R., Morrison, A. K., & Hogg, A. M. (2020). Thermal responses to Antarctic ice shelf melt in an eddy rich global ocean–sea-ice model. Journal of Climate, (2020).) [Etienne Pauthenet, France]	Accepted. Reference now considered in the Southern Ocean section