Comment ID	From Page	From Line	To Page	To Line	Comment	Response
130489	0	0	0	0	in Chapter 4 we should try our best to cover CMIP6 projections. [Panmao Zhai, China]	Noted.
7415	0	0	192	0	9999 in many cited references. Please replace by 2020, 'in press' or 'submitted' [Geremy PANTHOU, France]	Taken into account. This date is used for citations that were not accepted at the time of SOD. For the FGD, those papers have either been accepted before the literature cut-off deadline, in which case the references have been updated, or, if the papers didn't meet the acceptance deadline, then the citations have been removed.
17379	0		190		Accuracy and validity of climate models outcomes, under various scenarios, are strongly laid on the availability and sufficiency of data. West Asia and Central Asia is suffered from limited or lack of adequate data. Attention should be made, relative adequacy of data in East Asia and South East Asia not dominate on West Asia and Central Asia output interpretation. [Mostafa Jafari, Iran]	Noted.
67851	0				Incorporate analysis of marine pollution from urban and coastal community waste. Waste entering the sea generally contains a lot of plastic and metal debris, which decomposition process may take from 50 - 400 years. Marine waste may impact the economic and tourism sectors, disrupting the lives of marine life, coastal ecosystems and human health. Based on the data of the Ministry and Environment Indonesia (2017), the average generation of marine waste in Indonesia reached 106,385 grams/m2 with an estimated total waste of 1.2 million tons. Marine waste actually has the potential to be converted into energy through the use of thermal incinerator technology and refused derived fuel. [Ruandha Agung Sugardiman, Indonesia]	Rejected. Far away from the scope of this chapter.
114453	0				Sometimes it is a bit difficult to know if raw or assessed reults are given and used. This needs to be clear. [Jan Fuglestvedt, Norway]	Taken into account; care has been taken throughout to watch out for this distinction.
7449	0				General comment: Chapter 4 provides near-term climate information for the period 2021-2040 using an ensemble of experiments also including initialized decadal predictions as shown for GSAT in Box4.1 Similar, near-term climate change information is provided on a global scale in section 4.4, but mainly on the basis of the CMIP6 ensemble (Eyring et al., 2016). We think that section 4.4 can benefit directly from initialized decadal climate forecasts, since the initialized predictions undergo a standardized verification process and therefore underline the confidence of utilizing this data for the provision of near-term climate information on a global scale. We therefore suggest to include multi-model decadal forecasts and respective skill maps in section 4.4. DCPP is happy to help. [Wolfgang Müller, Germany]	Taken into account. In the FGD, we have increased the number of DCPP models used to eight. We have extensively assessed literature on initialized decadal climate forecasts and decadal predictability in Section 4.2.3, 4.4.1, and 4.4.3 (especially for Atlantic Multidecadal Variability).
7451	0				General comment: Box 4.1 uses 3 models for initialized decadal predictions (BSC, UK MetO and MPG). To date forecasts for 2019-2028 from at least three more models are available (MetNOR, CanESM, MIROC), DCPP is happy to help. [Wolfgang Müller, Germany]	Accepted. Eight DCPP models are used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7473	0				General Comment: Finally, I wonder whether in chapter 4 or chapter 10 the provision of near-term climate information on a regional scale can be considered. At least decadal predictions have been downscaled with a focus for the European region. (Reyers et al, 2019, Feldmann et al. 2019). Reyers M. et al., Development and prospects of the regional MiKlip decadal prediction system over Europe: Predictive skill, added value of regionalization and ensemble size dependency. Earth Syst. Dynam., 10, 171-187, 2019. Feldmann, H., Pinto, J. G., Laube, N., Uhlig, M., Moemken, J., Pasternack, A., et al. (2019). Skill and added value of the MiKlip regional decadal prediction system for temperature over Europe. Tellus Series A-Dynamic Meteorology and Oceanography, 71, 1-19. doi:10.1080/16000870.2019.1618678. [Wolfgang Müller, Germany]	Noted. Given Chapter 4's focus on global properties, this regional consideration would be Chapter 10's mandate.
114497	0				For good reasons, much focus is given to the low (2.6) and high (8.5) scenarios in this chapter. But some explanations for these choices could be given. Regarding the high scenario, this is not only about plausibility of emmisions, but also about adressing high outcomes due to high sensitivity in resposne. But readers may not percieve it like this, so some more efforts could be given to explaining the rationale for choice of scenarios. The focus on WLs are useful in this context, [Jan Fuglestvedt, Norway]	Accepted and implemented.
96341	0				We are strongly concerned that the RCP1.9 that is representative for 1,5C warming and therefore relevant for the Paris Agreement is omitted. [Nicole Wilke, Germany]	Noted. RCP1.9 was never a marker/priority scenario, meaning the comprehensive climate models have not been forced with it. Furthermore, SSP1-1.9 was already included in the SOD of Chapter 4. However, SSP1-1.9 is not represented in the CMIP6 database as frequently as other scenarios.
96343	0				In addition of reporting progress since AR5, please focus on more recent findings assessed in the SROCC whenever appropriate (for instance sea level, sea ice, sea level, ocean). Progress since AR5 is reported, but findings from SROCC often ignored. [Nicole Wilke, Germany]	Taken into account. Reference to SROCC is now more systematically included.
96345	0				What does "in initialized climate predictions" mean? Please use more obvious wording throughout Ch4 for "initialized predictions", "non-"initialized projections", "initial condition ensembles", including in section 4.2.3 and Box 4.1. [Nicole Wilke, Germany]	Rejected. These are standard terms in the published literature that is being assessed here, and it is not within the chapters' mandate to provide tutorials.
106843	0				Across the entire chapter, in analyses/metrics and plots, I would recommend to use the maximum members of simulations available in CMIP6 databases for all ensembles (historical, ssp etc.). This is crucial to correctly account for internal variability and associated uncertainties, in particular for the near-term time window. Large-ensembles are presented in AR6 as a clear added-value compared to AR5. It would be insconsistent with such a statement if all ensemble members were not used in Chap4. [Christophe CASSOU, France]	Rejected. This approach has been extensively discussed within the Ch04 author team and has been rejected. This approach would introduce bias toward models with more realizations OR, if averages across individual-model ensembles are performed first, introduce inhomogeneous statistics/sampling. It is precisely because of the large ensembles that we can afford not skewing the multi-model ensemble's statistics while obtaining good estimates of internal variability. The reviewer's suggestion would not lead to a traceable separation of IV and model differences.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106853	0				Changes in seasonality is never assessed or mentionned in Chap4. This is treated in Chap8 for water cycle but I believe that is also important for temperature for instance (e.g. Cassou and Cattiaux, 2016 and references herein https://doi.org/10.1038/nclimate2969). In any case, cross-chapter reference to Chap8 is needed. [Christophe CASSOU, France]	Taken into account and implemented. In particular, we added cross-chapter reference to Chap 8 for precipitation seasonality change. Changes in the amplitude of the seasonal temperature cycle are assessed in 4.5.1 and related figure and reference to Cassou and Cattiaux is added.
114561	0				The use of uncertainy language can still be improved (e.g. unclear what the confidence statements are based on) [Jan Fuglestvedt, Norway]	Taken into account. Attention has been paid to proper use of uncertainty language.
21635	0				Many references are X et al., 9999 which will need to be corrected in the final draft. I assume these are submitted manuscripts? [Peter Thorne, Ireland]	Taken into account. This date is used for citations that were not accepted at the time of SOD. For the FGD, those papers have either been accepted before the literature cut-off deadline, in which case the references have been updated, or, if the papers didn't meet the acceptance deadline, then the citations have been removed.
21637	0				Like in chapter 3 there is an erroneous ":" that has been placed after very many figure citations in the text. [Peter Thorne, Ireland]	Editorial. This kind of issues will be solved during the (professional) copy-editing phase of the report, if not before
28817	0				Congratulations on an excellent SOD! Chapter 8, Sections 8.2 and 8.4 will be updated to make this consistent with the global precipitation projections presented here. There is also an opportunity to link wet/dry region/regime response discussion with Ch8 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Connections with Ch08 have been strengthened even more. Ch9.2 and 8.4 in FGD are consistent with Ch4 assessment.
21651	0				In general in the opening rejoiners to variables which were covered by SR1.5, but particularly SROCC and SRCCL there is not sufficient attention paid to characterisiing what their principal of findings of relevance were to place the new findings herein in context. A little more effort to consistently bring in the principal findings of these AR6 cycle special reports would be very useful. [Peter Thorne, Ireland]	Accepted. Opening parts have been strengthened.
21653	0				There is a tendancy in several places to put figure caption like text in the main text. For example lines 24-27 of p.32 (just as a random example) is really material that arguably should be in the caption and not the main body text. There are numerous similar examples. [Peter Thorne, Ireland]	Taken into account. Text has been scrutinized for these instances.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34713	0				"There are some uncertainties associated with rainfall projection over the East Mediterranean (EM) region. (Alpert et al., 2008) predicted an increasing trend in precipitation over south and central Israel. The trend in A2 and B2 scenarios indicated extreme events as well as drier and wetter conditions in the upcoming years. Using a regional model, (Hochman et al., 2018) have predicted an increasing trend in winter and spring precipitations (~40% under Representative Concentration Pathway RCP4.5 scenario) in south Israel. (Ajjur and Riffi, 2020) examined the trends in 11 extreme precipitation indices in Gaza Strip (Palestine). Most indices increased during 1974-2016. Total precipitation, for example, has risen over two periods 1985-2004 and 2009-2016. References Ajjur, S., Riffi, M., 2020. Analysis of the observed trends in daily extreme Precipitation indices in Gaza Strip during 1974–2016. International Journal of Climatology. https://doi.org/10.1002/joc.6576. Alpert, P., Krichak, S.O., Shafir, H., Haim, D., Osetinsky, I., 2008. Climatic trends to extremes employing regional modeling and statistical interpretation over the E. Mediterranean. Global and Planetary Change 63, 163-170. https://doi.org/10.1016/j.gloplacha.2008.03.003. Hochman, A., Mercogliano, P., Alpert, P., Saaroni, H., Bucchignani, E., 2018. High-resolution projection of climate change and extremity over Israel using COSMO-CLM. International Journal of Climatology 38, 5095-5106. https://doi.org/10.1002/joc.5714." [Salah Ajjur, Qatar]	Noted. No action item identified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39585	0				Since (a) in Fig. 1(a) of Box 3.1, CMIP6 models are not validated by observations, even worse than CMIP5, (b) CMIP6 models are unable to capture the increase of 11,300 km2 per year of sea ice extent in Antarctica (www.pnas.org/cgi/doi/10.1073/pnas.1906556116), (c) based on infrared spectra of the atmosphere, http://dx.doi.org/10.1155/2013/503727 concludes to a radiative forcing of 2.6 W/m2 at doubled CO2 concentration whereas CMIP6 models use much higher unvalidated radiative forcings, (d) ECS and TCR used in CMIP6 models are even not in agreement between themselves by a factor up to 3, they are much too high and ignore a list of not less than 120 peer-reviewed papers which reports climate sensitivity equal or lower than 1°C, notrickszone.com/50-papers-low-sensitivity/ published by prominent climatologists like S. Schneider who published in 1971 in Science a climate sensitivity of 0.8°C, or R. Lindzen a similar value, (e) in Fig. TS.17(b), the observations show an increase of sea ice extent whereas models show a decrease, (f) Figure 1.7 shows that, independent on baseline choice, the projections of climate models are ALL above observations in 2014, for all these reasons although only one would be sufficient, the alarmism of the projections reported in this Chapter which are based on questionable CMIP6 models is unconvincing and should be toned down. [François Gervais, France]	Noted. The assessment performed here is deeply rooted in the available literature and assessment in previous IPCC reports and other AR6 WGI chapters. The reviewer cherry-picks evidence and ignores the much, much larger body of evidence supporting the assessment performed here.
34725	0				I think it is worthwhile to highlight the present inconsistency of scenario projections in the Mediterranean region. This may be attributed mostly to the scale of the study, data limitations and data sources. For example, (Zittis, 2017) concluded a wide variation, over the Mediterranean region, between monthly precipitation parameters when obtained from different observational datasets (rain gauge and satellite information). Zittis, G., 2017. Observed rainfall trends and precipitation uncertainty in the vicinity of the Mediterranean, Middle East and North Africa. Theoretical and Applied Climatology 134, 1207-1230. https://doi.org/10.1007/s00704-017-2333-0. [Salah Ajjur, Qatar]	Noted. However, this regional detail goes beyond the mandate of this chapter.
21675	0				Most sections have clear closing assessment findings statements, but then some do not. It would be easier on the reader if greater consistency in approach could be adopted. Easiest would be to add closing summary assessment statements where presently missing. I started noting these individually but it seems to be a feature not a bug so am resorting to a catch-all comment here to review all segments for adherence to what I assume are chapter guidelines more strictly. [Peter Thorne, Ireland]	Accepted. Closing assessments have been strengthened where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41389	0				Thanks a lot for the work that has gone into this global projections chapter. It provides very useful information, the summary assessments of projected changes in global key variables works very well. The chapter however suffers from a very weak post-2100 assessment. For some variables, like global mean sea level, this time horizon is of particular importance, for instance regarding committed climate impacts. Key post-2100 sections are currently not longer than a couple of sentences, which cannot be considered a rigid and comprehensive assessment given that there enough published peer-reviewed literature out there (and information provided in the process chapters, too). Please strengthen this part of the chapter. [Alexander Nauels, Germany]	Accepted. Section 4.7 has been strengthened considerably, by using the now-available post-2100 CMIP6 simulations and emulator-based information. Sea level and AMOC post-2100 are comprehensively covered in chapter 9 and not repeated here, but we make clearer links.
41391	0				One major concern at this stage is the lacking ES coverage of the scoping item "Committed climate response, climate targets, overshoot, irreversibility, abrupt change". Currently, only two bullets cramped under "Climate response to mitigation, carbon dioxide removal, and solar radiation modification" cover this highly policy-relevant field. Climate commitments, irreversibilty and abrupt change, in particular, call for a separate ES subsection. There is clear need to include more information related to these scoping items in the ES, as information already provided in the chapter text is highy relevant content that could and should be elevated further to the SPM. Please revise the ES to better account for the existing chapter assessment. [Alexander Nauels, Germany]	Taken into account. Please note limited space in ES, though.
21683	0				I expected to see in 4.5 some cryospheric indicator long-term projections but I see none. I understand ice sheets may not be possible but at least sea-ice and SCE would seem relevant? [Peter Thorne, Ireland]	Noted. Long-term cryo change is the domain of Ch09.
21707	0				Overall the FAQ feel a bit more technical than those I have read in other chapters thus far. Some further efforts towards lay-person accessibility would I think be beneficial. [Peter Thorne, Ireland]	Taken into account. Text has been simplified where possible.
32203	0				Relatively complete and well-illustrated Chapter. Further coordination with the process chapters could be useful. Section 4.8.1 evacuates the issue of "low-probability high-impact scenarios" (Sutton, Bams 2019) by focusing on the tail end of the CIRR distribution in CMIP6 models. This is a good start but a more systematic approach could have been considered. For example, what about the short-term regional climate response (2021-2040) if internal variability is pulling in the same direction as anthropogenic forcings? The discussion of extra-tropical modes of variability (NAM and SAM) lacks a common framework (Chapters 2,4 and 8) of analysis and interpretation to distinguish changes in variability from changes in mean state that are projected onto these modes of variability. The treatment of the SRM issue may require a little more caution. [Eric Brun, France]	Taken into account. These are all excellent comments that have been implemented across the chapter. (a) Near-term: Explicit effects of high sensitivity and internal variability are now included in 1.5 °C threshold-crossing times. (b): NAM and SAM: FGD has substantially improved cross-chapter coherence. (c): SRM has been scrutinized for further required modifications.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21713	0				Figures are overall very good and clear but often very minor tidies and adding of text / avoidding use of acronyms could greatly increase their accessability. A little more effort to try to ensure each figure can be used, to the extent possible, without reference to text or caption wherever possible would help to maximise potential use of these in education, outreach etc and reduce chances of mis-use [Peter Thorne, Ireland]	Taken into account. All figures have received another round of scrutiny.
71917	0				Overall this chapter is good. I would prefer to see the projections to 2300 be grouped with the changes to 2100 so that the policy maker can immediately see the longer term implications of decisions [John Church, Australia]	Taken into account, thank you. The data base is much poorer post-2100, which is why the treatment has to be separate. Articulation of the approach and the assessment itself are substantially expanded in FGD.
114413	0				The approach to arrive at assessed ranges for delta GSAT is defintely an improvement, but it comes with some communication challenges. E.g., which results are based on the assessed ranges - I guess only very few of the figures - and which are not. Many temperature results shown are, as far as I can see not based on the assessed ranges. And for other variables, e.g. precip, how is the relation here between assessed and not assesed ranges? It woudl be great if this could me made more clear. (Sorry, if I overlooked something here) [Jan Fuglestvedt, Norway]	Taken into account. See response to comment 127629 by the U.S. government.
114415	0				Related to my prevuous comment on assessed temp ranegs: Are implications for WGII considered? [Jan Fuglestvedt, Norway]	Noted. This goes beyond the Ch04 mandate but is dealt with through regional-chapter participation in Cross-Chapter Box 2.3.
114417	0				It woudl be very useful if the chapter could give more explanation of how much of the changes in future warming is due to updated science and how much is due to changes in the scenarios considered. [Jan Fuglestvedt, Norway]	Taken into account. Both the near-term/1.5°C crossing (4.3.4) and the longer-term/ERF (4.6.2) are now more explicit concerning where the contributions to the changed assessment come from.
114419	0				The chapter is in good shape and gives a useful update on both well established knowledge, but also some new perspectives, such as emergence. Good structure and clearly written. [Jan Fuglestvedt, Norway]	Noted, thank you very much!
114421	0				Setion 4.4.4 needs some consideration wrt to linkages to other parts of the report. It cointains topics that are touched in several chapters and more effort on division of what is covered and consistency check are in my view needed [Jan Fuglestvedt, Norway]	Taken into account. Connections have been strengthened.
21757	0				Overall a very nice chapter. There is a degree of feeling of déjà vu when similar things are covered but I'm not sure there is a better way to structure the chapter. Rather, try to reduce overt covering of similar things repeatedly instead making greater use of intra-chapter referencing. [Peter Thorne, Ireland]	Taken into account. More intra-chapter cross referencing has been introduced to avoid repetition.
11079	1	1	1	1	The title of the chapter is about "near-term information",but actually it has one Section 4.4 of only 11 pages about near-term information, but two Sections (4.5 and 4.7) of in total 28 pages about mid- to long-term information. Should the title be changed? [Wen Wang, China]	Rejected. Title is prescribed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83373	1	1	190	8	Why does this entire chapter have a strong focus on Arctic sea ice, with no mention of Antarctic sea ice? This is a major deficiency for those in the Southern Hemisphere, where sea ice change is of considerable and wide-ranging importance.. [Robert Massom, Australia]	Noted. Arctic sea ice has been a poster child of global climate change, which is why it is highlighted in this global chapter. Antarctic sea ice is covered in Ch09.
45529	1	1	190	9	I would like to take this opportunity to thank the people involved in collecting the material for, designing and writing this chapter for having done an excellent job. The importance of this piece cannot be overstated and it is interesting and pleasurable to read as well. [Leonard Borchert, France]	Noted, thank you very much'!
32687	1	1	190	55	It is suggested that PMIP modeling be mentioned as well, and that its importance in future modeling and the increased accuracy of CMIP6 predictions be noted. (The Paleoclimate Modelling Intercomparison Project (PMIP) was established in the 1990s (Joussaume and Taylor, 1995) to understand the response of the climate system to different climate forcings and feedbacks. Through comparison with observations of the environmental impact of these climate changes, or with climate reconstructions based on physical, chemical or biological records, PMIP also addresses the issue of how well state-of-the-art numerical models simulate climate change. To achieve these goals, PMIP has actively fostered paleoclimatic data syntheses, model-data comparisons and multi-model analyses. PMIP also provides a forum for discussion of experimental design and appropriate techniques for comparing model results with paleoclimatic reconstructions. Five different periods have been designed to contribute to the objectives of the sixth phase of the Coupled Model Intercomparison Project (CMIP6) : the millennium prior to the industrial epoch (past1000), the mid-Holocene, 6,000 years ago (midHolocene); the Last Glacial Maximum, 21,000 years ago (lgm); the Last Interglacial, 127,000 years ago (lig127k) and mPWP, the mid-Pliocene Warm Period, 3.2 million years ago (midPliocene-eoi400). These climatic periods are well documented by paleoclimatic and paleoenvironmental records, with climate and environmental changes relevant for the study and projections of future climate changes. Analyses of the individual periods, across all the periods and comparisons with other CMIP6 simulations, will allow examination of relationships between forcings of different nature and amplitude and climate responses, and comparison of the processes involved in these responses. New foci will be put on the role of the ice-sheet and of its feedbacks with the atmospheric and	Noted. This chapter is about projections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33017	1	1	190	55	It is suggested that PMIP modeling be mentioned as well, and that its importance in future modeling and the increased accuracy of CMIP6 predictions be noted. (The Paleoclimate Modelling Intercomparison Project (PMIP) was established in the 1990s (Joussaume and Taylor, 1995) to understand the response of the climate system to different climate forcings and feedbacks. Through comparison with observations of the environmental impact of these climate changes, or with climate reconstructions based on physical, chemical or biological records, PMIP also addresses the issue of how well state-of-the-art numerical models simulate climate change. To achieve these goals, PMIP has actively fostered paleoclimatic data syntheses, model-data comparisons and multi-model analyses. PMIP also provides a forum for discussion of experimental design and appropriate techniques for comparing model results with paleoclimatic reconstructions. Five different periods have been designed to contribute to the objectives of the sixth phase of the Coupled Model Intercomparison Project (CMIP6) : the millennium prior to the industrial epoch (past1000), the mid-Holocene, 6,000 years ago (midHolocene); the Last Glacial Maximum, 21,000 years ago (lgm); the Last Interglacial, 127,000 years ago (lig127k) and mPWP, the mid-Pliocene Warm Period, 3.2 million years ago (midPliocene-eoi400). These climatic periods are well documented by paleoclimatic and paleoenvironmental records, with climate and environmental changes relevant for the study and projections of future climate changes. Analyses of the individual periods, across all the periods and comparisons with other CMIP6 simulations, will allow examination of relationships between forcings of different nature and amplitude and climate responses, and comparison of the processes involved in these responses. New foci will be put on the role of the ice-sheet and of its feedbacks with the atmospheric and	Note. This chapter is about projections.
33027	1	1	190	70	accuracy and validity of climate models outcomes, under various scenarios, are strongly laid on the availability and sufficiency of data. West asia and central aasia is suffered from limited or lach of adequate data.attention should be made , relative adequacy of data in east asia and south east asia not dominate on west asia and central asia output interpretation. [Sahar Tajbakhsh Mosalman, Iran]	Noted. Regional aspects are covered in later chapters.
32697	1	1	190	70	accuracy and validity of climate models outcomes, under various scenarios, are strongly laid on the availability and sufficiency of data. West asia and central aasia is suffered from limited or lach of adequate data.attention should be made , relative adequacy of data in east asia and south east asia not dominate on west asia and central asia output interpretation. [sadegh zeyaeyan, Iran]	Noted. Regional aspects are covered in later chapters.
4529	1	1			Where in this chapter do authors present model hindcasts for the pre-industrial 2000 and 10,000 years? Use e.g. the composite global records of PAGES 2k 2013 and Marcott et al. 2013 for a start. [Sebastian Luening, Switzerland]	Noted. This chapter is about projections.
96347	2	0	4		For readability reasons please include abbreviations of recursory terms (e. g. GSAT, GMSL, SIA) in the "Table of Contents" [Nicole Wilke, Germany]	Rejected. Would require repeated extra pages.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105563	4	16	4	55	Some time the period taken into account is Since 1700, sometimes 1750, end of the 19th century … it can be confusing [Maxime Debret, France]	Noted. Unclear what comment refers to.
105561	4	21	4	21	The terms "likely" is not strong enough and depend of the scale you are dealing with. Since the late 19th century of course the increase of ERF is accelarating !! [Maxime Debret, France]	Noted. Unclear what comment refers to.
105565	4	33	4	35	The percentage are related to which baseline ? What is the reference ? [Maxime Debret, France]	Noted. Unclear what comment refers to.
104605	4	54	5	4	The text indicates that it relies upon AOGCMs, ESMs and "other types of models where appropriate". Yet the subsequent text inexplicably ignores two very important papers (and I am sure you know what I am about to say), namely Christy and McNider (2017) and Lewis and Curry (2018). The text should be altered, perrhaps in the lines relating to this comment. It should say something like: "Two notable studies by Christy and McNider (2017) and Lewis and Curry (2018) arrive at ECS values at the lowest end of our projected range, or slightly below. These results were arrived at either using a more constrained sulfate aerosol forcing, after Stevens (2015), or factoring out variations external to satellite sensed temperatures such as El Nino, other large scale circulation features such as the Atlantic and Pacific oscillations, and significant volcanism. These should be considered as providing a defensible lower bound for GSAT around the year 2100. Policymakers would be prudent if they applied considerable forebearance pending the scientific community's resolution as to whether these empirical approaches are superior to or inferior to the CMIP6 model suite. In essence, they are data-driven models. The fact that the CMIP6 models whose ECS exceeds those in CMIP5 also overpredict warming in recent decades when run in historical mode argues that these two results should be heavily weighted by policymakers." There may be other places in the overall text to insert this notation and the implications of these results should also prompt some small changes in the SPM chapter. Leaving reference to these papers out of the WG1 report is an invitation for withering criticism and subsequent public questioning of the motives of the WG1 leadership. [patrick Michaels, United States of America]	Noted. The ECS assessment is performed in Ch07; the result of this assessment has entered the assessment of GSAT change performed here. Furthermore, the GSAT assessment has been informed by recent papers constraining future GSAT change by past observations. No change needed.
11081	4	L39	4	L39	SR1.5 should be explained in parentheses to improve readability, like, IPCC Special Report on Global Warming of 1.5°C (SR1.5). [Wen Wang, China]	Accepted and implemented.
96349	5	1	9	24	Abrupt climate change / tipping points are discussed in section 4.7.3, and should please be included in the executive summary. [Nicole Wilke, Germany]	Rejected. Ch4 provides the table for integrating abrupt climate change across the WGI report. Thus, each chapter, not Ch4, includes important aspects of abrupt change in its Executive Summary.
71919	5	1	9	24	Many paragraphs of the ES have long, convoluted, hard to read sentences. The ES needs a major rewrite. [John Church, Australia]	Accepted. Language has been simplified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71921	5	1	9	24	There needs to be more informaton on projections for the range of scenarios for all Properties (global mean temperatures are reasonably well covered). There also need to be more informaton on key proerties where there is a longer term commitment - e.g. sea level and ocean acidification. [John Church, Australia]	Taken into account. More information is added for all other properties.
9779	5	3	5	6	please include conversion factors between 1850-1900 and 1950-2014 up here. I would personally prefer the healdine numbers all be with respect to 1850-1900, but you at least need to make this conversion easy. [Robert Kopp, United States of America]	Rejected. This difference is the result of a complex assessment process, which does not fit into the chapeau and anyhow falls under the mandate of Ch02 (see Cross-Chapter Box 2.3).
19825	5	3	5	7	When reading these lines and discovering the table of contents, the title of chapter 4 is baffling. What is meant by "near term information"? Please explain. As it is, perhaps removing the end of the title would be a simple solution to reconcile the title and the contents. [philippe waldteufel, France]	Rejected. The chapter title has been approved by the IPCC plenary and cannot be changed. Near term is explained on line 2 of the chapter.
96351	5	3	13	5	Please include definition of "long-term" (specify period 2081-2100). [Nicole Wilke, Germany]	Accepted. The definition of 'mid-term' and 'long-term' is added.
9657	5	4	5	4	The 1995-2014 reference period is different from the 2000-2009 period used in Chapter 3. This results in inconsistent use of periods in the SPM. I would strongly recommend that the same period is used to report past changes and future projections. [Olivier Boucher, France]	Noted. The 1995–2014 is the reference period for the recent past across WGI report.
104607	5	7	5	7	The following needs to be inserted in the text: "Note that we have purposefully ignored the Russian climate model INM-CM4.8 because we find its low ECS of 1.82degC unacceptable because it is not consistent with the drastic emissions cuts needed to comport with the other CMIP6 models". [patrick Michaels, United States of America]	Rejected. We haven't ignored the INM-CM4.8.
106267	5	8	5	9	The "Shared Socioeconomic Pathways" (SSPs) are different from the quantified emissions scenarios used in ScenarioMIP CMIP6 and here. SSPs are often used as a shorthand for these new scenarios, but this is imprecise. SSPs come without a predefined climate outcome. This can be made more specific by simply referring to them as SSP-based scenarios. If not solved here, it will have to be tackled at the level of the Synthesis Report when the original use and concept of the SSPs in WG3 will potentially clash with the short-hand use for scenarios in WG1. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Cross-chapter BOX 1.5 in chapter 1 introduces the way WGI uses SSPs and the five main scenarios including RCPs: SSP1-1.9, SSP1-2.6, SSP2-4.5, SSP3-7.0 and SSP5-8.5. The treatment of scenarios is coordinated across WGs.
51089	5	8	5	15	Please clarify that the scenarios used here are of GHG concentrations as opposed to emissions. This is a key distinction that appears to have been lost both in this chapter and the TS and SPM. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Some emissions-driven projections are included in the FGD. The ES is not the place for this detail, but Section 4.2.2 now includes this information.
18957	5	8		15	Are you not assessing the literature around these scenarios (and maybe other?) at all? Given that that is te main purpose of IPCC it'd be worh mentioning why you only assess projections and not the literature if that is what you do. Given that the next paragraphs discuss multiple lines of evidence it does not seem to be the case that you only look at CMIP though. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. Assessment of literatures is also the crucial part in the chapter. The paragraph means the model projection results assessed are mainly from SSPs in CMIP6 but also includes RCPs in CMIP5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98977	5	9	5	9	In that climate intervention is being much more discussed as an approach to limit warming and this is not generally included in these scenarios, I'd suggest adding a phrase at the end of the sentence to the effect that "; these scenarios do not consider proposed climate interventions to counter global warming by, for example, stratospheric aerosol injection and other unprecedented and as yet unproven approaches." I would think that this point really needs to be made--I personallly think the approaches would work but not at all clear that they might ultimately get approved. [Michael MacCracken, United States of America]	Rejected. This assessment would go beyond the scope of this chapter.
50727	5	10	5	10	Is there a reason for these SSP and RCP pairings? Some explanation here would be useful or signposting to Box SPM2 - are they thought to be socioeconomic scenarios that are centrally closest to the RCPs, for example? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. These particular scenarios have been picked by CMIP6. Sentence revised to make it clearer that this chapter builds on the research in CMIP6.
89837	5	12	5	14	Please quantify "higher" and "more pronounced" [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Comment misplaced (page 6, not 5). The suggestion has not been followed, because many reviewers requested less detail in the ES.
71261	5	14	5	14	Meaning of "two generations" is unclear. It may be better to confirm which scenario in CMIP6 is compared to the scenario in CMIP5. [Kenji Taniguchi, Japan]	Rejected. "generations of models" is standard terminology. In this ES chapeau, more detail cannot be accommodated.
50729	5	14	5	15	Where appropriate, this chapter also assesses results from CMIP5, which used scenarios based on Representative Concentration Pathways (RCPs); the differences between the two scenario sets matter less than the differences between two generations of models.' - does this mean in terms of the temperature outcome, impacts exposure, or both? It would be useful to clarify this here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section 4.6.2 has been re-orientated to look at the difference more explicitly. In this ES chapeau, more detail cannot be accommodated.
11267	5	19	5	19	Abbreviation of GSAT should appear here [Masahiro Watanabe, Japan]	Rejected. We try to keep the use of acronyms in the ES to a minimum. GSAT is no longer used in the FGD ES.
132187	5	19	5	21	Why use the period 1995-2014 as reference? This corresponds approximately to 2005, i.e. 15 years before present. For projections, it seems fine to use the present-day, i.e. a time period centered over 2020 as reference (this would be the time frame 2010-2030). [Sonia Seneviratne, Switzerland]	Noted. It's the WGI-agreed-upon period for the recent past, chosen because it's the last 20-year period of the CMIP6 historical simulations.
34875	5	19	5	29	Detailed Comments by SOD Chapter – Chapter 4: It is welcome that the SOD recognises that uncertainties in projections for 2018-2100 relate to uncertainty in ECS and TCR. The corollary is that any projections beyond 2100 to 2300 better belong to the realm of science fiction. Please see general comment #15 above. [Jim O'Brien, Ireland]	Noted. The logic behind the comment is not supported by published literature.
15485	5	19	5	29	Many of the SSP scenarios are linked to RCP scenarios by the radiative forcing in 2100, e.g. SSP1-2.6 vs RCP2.6, SSP2-4.5 vs RCP4.5, SSP5-8.5 vs RCP8.5. It is recommended to supplement this section with the temperature change between the reference periods in AR5 and AR6, i.e. 1986-2005 and 1995-2014, to facilitate comparison of projections, especially projections for the end of this century. [SAI MING LEE, China]	Noted. Section 4.6.2 has been re-orientated to look at the difference more explicitly. In this ES chapeau, more detail cannot be accommodated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19367	5	19	5	29	I strongly encourage the authors to replicate this section showng the projections and ranges for warming relative to the preindustrial era (1850-1900) as this is the timescale reference most policy makers and everyday people are used to thinking about. The information relative to the recent past is importannt, but without the context of the numbers relative to preindustrial, I believe many policymakers/laypeople will be confused. It might also be worth noting in the text that when we talk about 1.5C or 2C in the Paris agreement, that that is relative to preindustrial. [Lia Cairone, United States of America]	Taken into account. No doubt that warming relative to 1850--1900 is important, and the assessment of whether and when warming levels are crossed is part of the ES. However, future warming relative to what we have experienced in the recent past also contains important information, and the uncertainty of this assessment is smaller. A balance had to be found between comprehensiveness and conciseness.
35089	5	19	5	44	a percentage estimate of warming over land vs warming over oceans would be welcome here [Baylor Fox-Kemper, United States of America]	Noted. This is taken up later in the ES; it would be too much information to include it here.
66983	5	19	5	44	It might be helpful / better to discuss risk of exceeding other (higher than 1.5°C) thresholds. For instance, GSAT warming in SSP5.8-5 will exceed +4°C before 2100 with very high probability, and could even exceed +6°C also in 2100 (given the assessed ranges for 2081-2100). In my view, discussing such high-risk but still plausible future climate is of high interest. More generally, providing numbers relative to 1850-1900 could be useful here (in many applications, eg impact studies, the pre-industrial reference is still used). [Aurélien Ribes, France]	Taken into account. No doubt this information would be valuable; however, we had to balance this issue against other material competing to make it into the ES under very tight length constraints.
54947	5	19	5	49	The ordering and wording here is a bit of an impediment to clear communication. The first bullet refers to warming at the end of the 21 century relative to 1995-2014; the second bullet then refers to warming in the near term (2021-2041) but is referenced to 1850-1900; the third bullet then jumps back to end of centrury and 1995-2014 reference. We would encourage the authors to think about an overall 'storyline' and organizing the bullets in a way that is easier to follow and less likely to cause confusion or misunderstanding. A particular concern is the potential for the second bullet to be misunderstood since it refers to the 1.5C temperature threshold, but rather than a message about limiting warming to that level (as in SR1.5), this is talking about the likelihood of passing through it. It would be good to make that distinction more explicit. [Nancy Hamzawi, Canada]	Taken into account. Thank you very much for these valuable pointers, which we have tried to follow in the revision.
9659	5	20	5	20	I would suggest to report the warming level for a period centered in 2100 rather than the 2081-2100 period. This is often misinterpreted as the 2100 warming level, but it is not for the highest scenarios such as SSP585. [Olivier Boucher, France]	Rejected. Most projections end in year 2100; only a few simulations for only a few scenarios would be available for implementing this suggestions.
89839	5	21	5	25	Please quantify "larger" [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Comment misplaced and refers to page 4-6. The request for more quantification had to be balanced against the desire for simplification expressed by other reviewers, as well as space constraints.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127395	5	25	5	26	The sentence beginning "Including lines of evidence in addition to the projection simulations..." is very poorly worded and much too long. A suggested rewrite would be to say something like "New and substantial research has introduced additional lines of evidence to conclusions from climate simulations used in previous IPCC assessments. These lines of evidence have led to a reduction in the range of uncertainty and an increase in confidence in projections of temperature change." [Trigg Talley, United States of America]	Taken into account. We agree that the proposed sentences are easier to read than the SOD text when viewed in isolation. However, the connection to the previous sentence, which introduces the new lines of evidence, is lost by not starting the sentence with them. The paragraph has been re-written.
71207	5	25	5	27	Including lines of evidence in addition to the projection, simulations have been possible through substantial research progress since previous IPCC assessments and have both reduced the assessed uncertainty ranges and increased the confidence in them [Michael Mugarura, Germany]	Rejected. Inserting the comma would distort the meaning.
98967	5	27	5	29	I think there needs to be a phrase put at the front of this finding saying something like: "While the amounts of CO2 and other GHG emissions are the most important factor in leading to different projections of the temperature increases likely to occur by 2100, the uncertainty ranges for each scenario for the period 2081-2100 ...." It just needs to be very clear that the key reason for the warming by 2100 depends on our choices and not scientific uncertainties or model shortcomings or variations among models. [Michael MacCracken, United States of America]	Taken into account. Comment 127397 has been followed, which achieves this clarification with fewer words.
18959	5	27		29	I assume this means apart from scenario uncertainty? [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Indeed this is meant.
127397	5	28			Add "for each scenario" after "uncertainty ranges". [Trigg Talley, United States of America]	Accepted and inserted.
52953	5	31	5	32	near-term has been already defined in the first parapraph [Hervé Douville, France]	Noted. Repeated for clarity and self-containedness.
98969	5	31	5	34	This is a very long and complex sentence for such a critical finding. I would suggest breaking it into two sentences, the first stating something like: "A 1.5 C increase in globally averaged surface air temperature relative to preindustrial is now projected as likely to be reached by 2040, and as early as 2030 if emissions continue to increase at high rates." Then have a second sentence with the rest of the details. It would be nice to somehow say that current warming is a bit over 1 C and that the 1.5 C value is the aspirational goal of the Paris Accord, but that would complicate the key finding by too much. There points, however, should likely be made in the rest of the paragraph. [Michael MacCracken, United States of America]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
34601	5	31	5	36	It seems to me these two sentences should be reversed in order of appearance. The real key message is that the best estimate for reaching 1.5C of warming is 2030 (medium confidence). This is particularly important to emphasize because SR1.5 had an estimate that is twice as far into the future (i.e., 2040). [Russell Vose, United States of America]	Taken into account. There have been conflicting comments on whether to emphasize the central estimate or whether to omit it altogether and only give ranges. We have kept the current ordering, because the assessment of the ranges is more robust.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9661	5	31	5	36	I am not sure how to read this. Is it only "very likely" that 1.5°C relative to 1850-1900 can be reached with SSP585 ? Is it not "absolutey certain" ? Or does the "very likely" refer to the 2021-2042 period? "very likely" and "likely" are also weak statements for SSP370 and SSP245. [Olivier Boucher, France]	Taken into account. The finding applies to crossing 1.5°C in the near-term period 2021--2040. Paragraph has been revised for greater clarity.
26819	5	31	5	38	This statement is difficult to understand: Is it only "very likely" that 1.5°C relative to 1850-1900 can be reached with SSP5-8.5 ? Is it not "absolutey certain" ? Or does the "very likely" refer to the 2021-2042 period? "very likely" and "likely" are also weak statements for SSP3-7.0 and SSP2-4.5. [Eric Brun, France]	Taken into account. The finding applies to crossing 1.5°C in the near-term period 2021--2040. Paragraph has been revised for greater clarity.
34877	5	31	5	44	The SSP8.5 scenario, with an estimated 1000ppm CO2 by 2100 is totally unrealistic and should be dropped. Please see general comments #2 and #3. [Jim O'Brien, Ireland]	Rejected. The scenarios per se are used as input here and are not assessed in this chapter.
127399	5	31	5	44	The level of detail in these lines is excessive for an executive summary, and the wording is repetitive (variants of the term "reaching 1.5°C" is repeated four times in 12 lines). Too many numbers. Key messages should be delivered concisely. [Trigg Talley, United States of America]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
6643	5	31	5	44	The above two comments apply to this paragraph. Moreover, the statement that the dominant cause of the change from 2040 to 2030 in the date for reaching the 1.5ºC level is the revised estimates of historical temperature change is questionable. Aside from the issue of how the targets of the Paris Agreement should be interpreted, on page 36 of this chapter the increase in estimate of the temperature change from 1850 to the recent reference period of 1995-2014 is stated to be about 0.1ºC, which is equivalent to about five years of warming at the rate observed for the past forty years. So that leaves another five years to be explained by the faster warming rate of the CMIP6 models compared with the CMIP5 models. The change in observational estimates does not appear to be the dominant factor. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Statement has been corrected as indicated in comment.
54951	5	31	9	23	there are many instances of likelihood and confidence language used in the same sentences. The IPCC uncertainty guidance makes clear that this should generally not be the case, and it tends to be confusing for the reader.The guidance is that if a likelihood qualifier is used, high or very high confidence is implied and should not be stated. [Nancy Hamzawi, Canada]	Accepted. All the instances of likelihood language combined with confidence statements have been removed from the ES, except for medium confidence.
28819	5	31			Suggest: "In the near-term period 2021–2041, a warming level of 1.5°C in globally averaged surface air temperature, relative to the 1850–1900 period, is very likely..." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
96353	5	32			We are very concerned that climate projections are used to analyze period 2021–2041 (we suppose it should read 2021-2040) which starts at present day. We urge the authors to use initialized climate forecasts for this period instead. In addition, a 20 years period is actually too short for deriving the state of climate change. If the authors consider these comments invalid, an explanation must please be included in the report. [Nicole Wilke, Germany]	Taken into account. The initialized forecasts, which are available only for a decade and not for entire near-term period, have been used in the assessment. This is now explicitly spelled out in the ES. Justification for using a twenty-year period is now include in the chapter text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54945	5	34	5	35	It would be helpful to footnote this statement that the best estimate for reaching global warming of 1.5C is around 2030 neglecting the influence of natural variability, to explain what is meant by the phrase "neglecting the influence of natural variability". This is a conclusion of particular interest to many and ensuring it is properly understood is critical. [Nancy Hamzawi, Canada]	Taken into account. The FGD assessment does include the uncertainty from internal variability; statement no longer applicable.
21615	5	35	5	35	My feeling is that the date for crossing 1.5 needs to be given a likely range. Furthermore that range should combine with uncertainy in the observed change to date arising from chapter 2 with the model and scenario based uncertainty arising from chapter 4. That may well lead to an asymmetric and quite broad range but given that this is going to be testable in fairly short order I think possibly it is better to be too broad than too narrow in this regard. [Peter Thorne, Ireland]	Taken into account. The new assessment takes all known sources of uncertainty into account.
65685	5	35	5	35	Suggest rephrasing the statement "… neglecting the influence of natural internal variability." to " .. assuming no changes in the influence of natural internal variability." for clarity. [Kushla Munro, Australia]	Taken into account. The SOD statement was meant as stated, not as suggested. The FGD assessment does include the uncertainty from internal variability; statement no longer applicable.
18961	5	35			You don't neglect natural variability, but when assuming no major volcanic erruption or so is happening it's 2030. I'd rephrase to something like, "baring no major cooling influence from...".. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The statement was about internal variability, not naturally forced variability. The FGD assessment does include the uncertainty from internal variability; statement no longer applicable.
28821	5	35			this also doesn't account for unrepresented natural forced variability such as enhanced volcanic activity or solar brightness [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Such a statement has been added.
9663	5	36	5	38	This is a useful statement (basically we have to do an awful lot of mitigation plus be lucky to stay under 1.5°C) but the statement is very hard to read and understand. This is the sort of IPCC parlance that we should now avoid. [Olivier Boucher, France]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
26821	5	36	5	38	The message is unclear, the sentence too complex, but it is a very important statement. It should be clarified to fully reach the targetted audiance [Eric Brun, France]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
50731	5	37	5	38	to later than during the near-term' - How many years later than does this refer to, e.g. 2030? Please specify if available. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
99595	5	38	5	38	Delete "during" [Stefan Sobolowski, Norway]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
15183	5	39	5	42	The comparison to SR1.5 here is important but not clearly worded. The key point is that the best estimate is ten years earlier, because of updates to the observational temperature record. After that, you can say that the ranges overlap. [Simon Donner, Canada]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
96355	5	39			For consistency reasons it may be helpful to adapt the verb "is" to "will be" in: "[…]of when 1.5 °C is reached encompasses […]" in order to be consistent with line 41: "[…] relative to 1850-1900 will be […]" [Nicole Wilke, Germany]	Taken into account. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65681	5	41	5	43	'However, the best estimate for when a global warming level of 1.5°C relative to 1850–1900 will be reached is assessed here in Chapter 4 to be about ten years earlier than the best estimate of the SR1.5 (medium confidence). The dominant cause of this re-assessment is the provision of enhanced estimates of the historical observational record.' Suggest the authors provide more clarity about these enhanced estimates of the historical record so that the reader can understand why there is such a large difference since the 2018 assessment, particularly as the revision appears to derive from the application of an energy-balance model ('emulator') rather than a analysis of historical data. [Kushla Munro, Australia]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
31539	5	41	5	44	This paragraph is very hard to follow. Suggest attempt to rewrite to be clearer, and in particular line 36-38 [Jean-Baptiste SALLEE, France]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
45457	5	42	5	42	I think the reference to chapter 4 is unnecessary and slightly confusing here. [Leonard Borchert, France]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
2357	5	43	5	43	what does "enhanced estimates" mean here? Is it that there is more data since SR1.5 was released or that problems in the dataset were corrected? I think it would be better to clarify this to avoid confusion. [Vaishali Naik, United States of America]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
21617	5	43	5	43	Enhanced has value judgement undertones. Better to say updated estimates of the historical observed change to date or similar here. [Peter Thorne, Ireland]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
84215	5	43	5	43	what are "enhanced estimates"? Do you mean more? More robust? More accurate because more observations are now available? [Annalisa Cherchi, Italy]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
114423	5	43	5	43	re "enhanced estimates": You may consider adding some more concrete info on this (if space…) [Jan Fuglestvedt, Norway]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
132425	5	43	5	44	It's not clear what "provision of enhanced estimates of the historical observational record" means. Do you mean a larger amount of historical warming than in AR5? [Kyle Armour, United States of America]	Accepted. Paragraphs on globally averaged surface air temperature change have been re-arranged and modified for clarity and concision.
31541	5	46	5	49	Maybe it would be worth saying here that those high ECS models have ECS outside the likely range of chap 7, if that is the case. And maybe stating that those models have very unlikely warming rates, so that links straigthforwardly to the next point. [Jean-Baptiste SALLEE, France]	Noted. This assessment is done in Ch07 and is hence not repeated here in the Ch04 ES. Unlikely warming rates are mentioned a few lines later.
79673	5	46	5	49	I suggest to specify that it is the 5-95% range. Also, the fact that no independence wieighting is currently applied in chapter 4 could be questionable if all CMIP6 models (even if they are from the same center) are included for the FGD, as many (in fact almost all) of the high-sensitivity models have close "cousins" and thereby represent a large fraction of the CMIP6 ensemble. This could bias the upper-range bound. We are currently working on a paper with J. Boé where we are trying to quantify this impact, and we hope to submit it within the next few weeks. [Laurent Terray, France]	Accepted (5--95% range). Noted otherwise. Application of Boé's analysis to CMIP5 models in used in Box 4.1; no corresponding analysis has apparently been published for the CMIP6 models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7517	5	46	5	49	Why are you increasing forcing in CMIP6 if CMIP5 was shown to run hot? [Hugh Lefcort, United States of America]	Noted. Unclear what the question is based on.
127401	5	46	5	49	The chapter should be highlighting the assessed projections not the raw material that goes into the assessment. The spread in CMIP6 (given the additional evidence being used to constrain projections) is not needed in the Executive Summary. Delete. [Trigg Talley, United States of America]	Rejected. It is indeed only the assessed GSAT change that is elevated to the SPM, but the question of applicability of CMIP6 models with unlikely high warming to other quantities has been raised numerous times -- also by the author of this comment --, so that the question must be dealt with at TS level and hence in this ES.
50733	5	51	5	51	"CMIP6 models with very unlikely high warming rates" -is there sufficient understanding to justify the use of the qualifier "very unlikely" at this time? It's unlikely compared to the range but it may be that the ESMs are more sophisticated than the models that project a lower ECS etc. Unlikely feels like a conclusive statement and could be taken out of context. As written, it could be taken to suggest that the latest MOHC ESM and probably all/most ESMs are not performing well, which is possibly not the intention here and further analysis is required to determine this. Suggest supporting information is provided or "very unlikely" is deleted or replaced. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The chapter provides very likely ranges for future GSAT change, and some models-- UKESM among them -- fall outside this range. The ES only talks about warming rates, not other uses of the same model.
2125	5	51	5	51	How do we know these are UNLIKELY? It is also obvious these models will be well above the mutlimodel mean by definition so I suggest merging this with the previous statement. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The two paragraphs have been merged and the text revised for greater clarity.
9665	5	51	5	51	How do you define a CMIP6 model with very unlikely high warming rate? Is this one whose ECS is outside the very likely range? [Olivier Boucher, France]	Taken into account, definition now included (GSAT change outside the very likely range). The two paragraphs have been merged and the text revised for greater clarity.
26823	5	51	5	51	How is defined a CMIP6 model with very unlikely high warming rate? Is this one whose ECS is outside the very likely range? [Eric Brun, France]	Taken into account, definition now included (GSAT change outside the very likely range). The two paragraphs have been merged and the text revised for greater clarity.
54949	5	51	5	53	The wording here is very difficult to follow. "very unlikely high warming" is difficult to parse, and the notion that 'models are assessed as storylines', is unclear. [Nancy Hamzawi, Canada]	Taken into account. The two paragraphs have been merged and the text revised for greater clarity.
127403	5	51	5	53	[ENSEMBLES] These lines confuse models with specific simulations. Models have different ECS/TCR, but it is the simulations that have high/low warming rates under different storylines. Are the storylines not being excluded, or the high-ECS models not being excluded? Is this consistent with lines 25-26? [Trigg Talley, United States of America]	Taken into account. The two paragraphs have been merged and the text revised for greater clarity.
34603	5	51	5	55	I think this message gives too much weight to model outcomes that are likely spurious. Furthermore, the statement is basically a tautology saying that models with high warming rates exceed the multi-model mean. I think most readers would already assume that the models with the most warming are be above the mean. [Russell Vose, United States of America]	Rejected. The statement is not tautological but can only be made under the -- justified -- assumption that robust patterns exist that scale well with GSAT change.
9781	5	51	5	55	needs an assessment statement to explain the 'very unlikely' models [Robert Kopp, United States of America]	Taken into account. The two paragraphs have been merged and the text revised for greater clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96357	5	51	5	55	There are CMIP6 models with high warming rates. How well have these models simulated observed warming, in comparison to the others? This information should be added and discussed consistently across chapters 3, 4, and 7 and possibly others - please provide this information one place. [Nicole Wilke, Germany]	Taken into account. The basis for the assessed GSAT change has been stated more explicitly in the new third paragraph of the ES.
98971	5	51	5	55	These sentences are pretty complex, generally involving at least double negatives, etc. I'd urge some attention to making these points in a simpler form, something like: "CMIP models that project unusually large global warming and large and widespread changes in precipitation that are well above the multi-model mean seem likely have a low probability, although such large warming over at least extratropical land areas cannot be excluded (medium confidence)." Then perhaps add a statement explaining why this is the case. [Michael MacCracken, United States of America]	Taken into account. The two paragraphs have been merged and the text revised for greater clarity.
38585	5	51	5	55	Some people might disagree, so I think it is worth registering my support and saying I think this is a sensible way to explain the high-end warming storylines. This text is clear and well written. We made a similar decision to explain Met Office simulations in UKCP18. [David Sexton, United Kingdom (of Great Britain and Northern Ireland)]	Noted, thank you!
18963	5	51			Isn't the finding "CMIP6 models with very high warming rates are assessed to be unlikely" instead ofwhat it currently says? [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text modified.
65683	5	52	5	54	"CMIP6 models consistent with such high warming storylines cannot be excluded" - this appears to be a circular conclusion. The high warming arises *because* those models produce it. Suggest clarification of the CMIP6 models that produce low(er) warming - which also (presumably) cannot be excluded. [Kushla Munro, Australia]	Taken into account. The high-warming storylines are very unlikely but not impossible. The two paragraphs have been merged and the text revised for greater clarity.
79675	5	53	5	55	Would it be possible to also assess if these models simulate a larger polar amplification ? They also probably have large Arctic ocean warming. [Laurent Terray, France]	Accepted, Arctic added.
107025	5	54	5	54	I would remove the confidence statement. In any case, what is the rationale/justification of this level? To me, it is more a fact that high warming cannot be excluded considering all the uncertainties we are dealing with. [Christophe CASSOU, France]	Rejected. The relationship of simulated GSAT change and change of other quantities -- some of which do not scale with GSAT change -- has not been explored much.
84217	5	54	5	54	would it be possible to have numbers for "particularly large changes"? [Annalisa Cherchi, Italy]	Noted. Other review comments have requested less detail in the ES.
11265	5		9		In each Executive Summary statement (at least some of them), please make sure what the new findings are beyond AR5 [Masahiro Watanabe, Japan]	Taken into account. FGD ES strives to make proper connection to previous reports.
2941	6	1	105	50	Chapter 4 should include 32 CMIP6 models results, or as many as possible CMIP6 models results. [Zong Ci Zhao, China]	Noted. All models providing sufficient data have been included.
96359	6	4	6	4	By contrast to what? Please rewrite. [Nicole Wilke, Germany]	Rejected. It is standard English usage that "by contrast" refers to the previous sentence.
34605	6	4	6	5	The second sentence of this key message remains an important point to emphasize even though it was also a conclusion of AR5. Please consider placing this text in bold as well. [Russell Vose, United States of America]	Noted. Since the first sentence contains a substantially new assessment, owing to the availability of large ensembles for estimating internal variability, the current hierarchy has not been changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96361	6	5	6	5	Predictions --> Short term climate predictions. [Nicole Wilke, Germany]	Rejected. The timeframe of the predictions is given in the same sentence.
31535	6	7	6	7	Can that be clarified: the assessed very likely range of what? [Jean-Baptiste SALLEE, France]	Noted. The same sentence specified what is being assessed here.
127405	6	7			What is the assessed range being referenced? [Trigg Talley, United States of America]	Noted. The same sentence specified what is being assessed here.
98973	6	8	6	10	This seems to me an overstatement. First, the sentence should start with "Volcanic eruptions that inject large amounts of sulfur oxides into the stratosphere" and not just say "Volcanic eruptions"--lots of volcanic eruptions don't do such lofting. Second, the phrase "extremely cold individual years" needs context--volcanic eruptions have to be very, very large to get to "extremely cold individual years"--and "extremely cold" compared to what? An eruption would have to be very, very large to take the global average temperature down to the preindustrial levels--Pinatubo did not do that, for example, and many would say that was a very large eruption. In addition, does not Robock argue that volcanic eruptions lead to warmer winters, so the cooling that is large is during the summer and warm season temperatures might drop a lot, but I think the phrase "extremely cold" implies winter would also be much colder, and it is not clear this is the case. I think some reworking is needed here, and likely back in the chapter. [Michael MacCracken, United States of America]	Taken into account. Added that it has to be a large eruption. There is a fair amount of common understanding what is meant by that. The chapter specifies the precise meaning.
114425	6	8	6	10	Very good that you have this reminder at the ES level. [Jan Fuglestvedt, Norway]	Noted, thank you!
50737	6	9	6	9	"Volcanic eruptions increase the frequency of globally extremely cold individual years and the likelihood of decades with cooling trends in globally averaged surface temperature" - suggest this is rephrased to 'likelihood of individual decades with cooling trends compared to the previous decade in globally averaged surface temperature' for clarity - otherwise this sounds like volcanoes could push us into endless cooling. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, "individual" added.
34607	6	12	6	13	I presume this is for surface temperature. You might want to be specific about that point. [Russell Vose, United States of America]	Accepted, "surface" added.
2127	6	12	6	14	This adds confidence statements to likelihood statements. Why not combine into a single statement about the uncertainty to avoid confusion? [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, confidence statement dropped.
98975	6	13	6	13	I would urge changing "Arctic will be" to "Arctic will continue to be" as this exceedance is already clearly the case, and this needs to be acknowledged. [Michael MacCracken, United States of America]	Accepted, thank you.
34609	6	13	6	14	This sentence could be simplified as follows: "It is very likely that the Arctic will warm more than the planetary average." Also, I was a bit surprised to see very likely rather than virtually certain. [Russell Vose, United States of America]	Rejected; specific reference to the surface is needed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127407	6	13			Surely this is true for all decadal periods? (why restrict the claim to 2081-2100?) [Trigg Talley, United States of America]	Taken into account. Due to the very high internal variability in polar regions the warming may be temporarily masked in low emission scenarios. The level "virtually certain" thus only applies to the end of the century.
50739	6	14	6	14	Suggested edit: 'pronounced than the global average' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, thank you.
98979	6	14	6	16	I would like to urge a reworking of this sentence to say somethng like:" The pattern of temperature change varies across seasons with boreal winter cooling not nearly as strong, especially over ocean areas, as in past decades and centuries as a result of reduced formation of sea ice cover not insulating the atmosphere from Arctic Ocean waters. During boreal summer substantial warming is projected over land areas away from the ocean edge where waters do not increase to much above freezing." I don't really think calling the wintertime temperature increase warming really makes much sense given the ocean waters just a meter or two down from the surface stay at the same temperature whether ice is there or not. And, over the summer, the warming is strongest over the land, setting new high temeprature records, etc. Plus explaining the mechanisms always is appreciated. [Michael MacCracken, United States of America]	Rejected. The ES cannot provide information at this level of regional seasonal detail. The ES statement as it stand summarizes the assessment given in 4.5.1.
79677	6	17	6	19	I wonder if the very important role of relative humidity changes on heat stress changes (e.g WGBT) could be specify a bit more precisely with references to specific large-scale regions (see Brouillet, A., & Joussaume, S. (2019). Investigating the role of the relative humidity in the co-occurrence of temperature and heat stress extremes in CMIP5 projections. Geophysical Research Letters, 46, 11435– 11443. https://doi.org/10.1029/2019GL084156) [Laurent Terray, France]	Not applicable. The corresponding section is moved to CH12
102925	6	19	6	19	GSAT acronym has not been defined yet. It is first defined at page 10, line 6. [Philippe Tulkens, Belgium]	Taken into account. Acronym is no longer used in the ES.
71263	6	19	6	19	GSAT is first appearnce here. Is it OK? [Kenji Taniguchi, Japan]	Taken into account. Acronym is no longer used in the ES.
127409	6	19	6	19	First use of acronym GSAT. Define it. [Trigg Talley, United States of America]	Taken into account. Acronym is no longer used in the ES.
87529	6	19	6	19	GSAT acronym has not been defined yet. It is first defined at page 10, line 6. [Valentina Roberta Barletta, Denmark]	Taken into account. Acronym is no longer used in the ES.
77687	6	19			GSAT not defined at start (global surface average temp) [Emer Griffin, Ireland]	Taken into account. Acronym is no longer used in the ES.
107027	6	21	6	21	I would add:"the SAM trend towards positive phases in austral summer…" [Christophe CASSOU, France]	Rejected. This statement refers to projected near-term changes which do not show a trend towards a positive SAM phase - see Figure 4.16.
50741	6	21	6	21	suggest 'In the Arctic, it is very likely…' and remove the later 'Arctic' in the sentence. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement has been rephrased.
45795	6	21	6	21	Remove "tropospheric". [Twan van Noije, Netherlands]	Rejected. This is the core of the statement, in contrast to previous ones, which deal with the surface.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
34611	6	21	6	22	Again, this sentence could be simplified as follows: "It is very likely that the largest tropospheric temperature increase will be in the Arctic." [Russell Vose, United States of America]	Taken into account. The statement has been rephrased.
132427	6	21	6	22	Awkward sentence. Perhaps rephrase as "It is very likely that projected lower-tropospheric and near-surface warming will be, in the long-term, larger in the Arctic than in the global mean (high confidence)." [Kyle Armour, United States of America]	Accepted, the statement is rephrased accordingly.
65687	6	21	6	22	Suggest changing to: "It is very likely that projected long-term tropospheric warming in the Arctic lower troposphere and near-surface will be larger than the global mean" for clarity. [Kushla Munro, Australia]	Taken into account. The statement has been rephrased.
84219	6	23	6	23	"scenarios with atmospheric high CO2 concentration": would it be better instead to include the name of the scenario(s) as done in previous statements? Or refer in general to "high emissions scenarios"? [Annalisa Cherchi, Italy]	Taken into account. The statement has been rephrased.
50743	6	25	6	25	between modelled and observed temperature trends' - are obs indicating warmer or cooler than models for the troposphere? It would be helpful to include this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The model performance for temperature trends is assessed in chapter 3. A reference to the section in chapter 3 has been added.
2129	6	25	6	25	Is there still an issue with model and observed T profiles? I thought this was resolved? [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Recent literature on CMIP6 models shows on average larger tropical upper tropospheric warming trends than observed in satellite records which may be partly related to their surface temperature trends. See, e.g. https://doi.org/10.1088/1748-9326/ab9af7 https://www.pnas.org/content/116/40/19821
127411	6	25			Add "unresolved" before "discrepancies. [Trigg Talley, United States of America]	Accepted.
98981	6	26	6	26	It seems to me that a sentence needs to be added indicating something like: "Such regional amplifications in changes lead to large scale changes in the temperature gradients that contribute to the atmosperic circulation and evolution of the weather, particularly during the winter half of the year, so that the changes in climate resulting from the regional influence can be hemispheric to global in scale." Basically, the sentences there are nice but there is no indication about why this is an important result--there just has to be a concluding sentence. [Michael MacCracken, United States of America]	Rejected. This type of description is too detailed for the ES which focuses on the assessment of changes made in the chapter. More detail on the relevance of atmospheric temperature changes for circulation is contained within the chapter.
9783	6	28	8	22	I would encourage including assessments of changes as a function of GSAT in the ES. [Robert Kopp, United States of America]	Not applicable. We have just assessed precipitation change given warming levels which are already included in the ES.
52959	6	28			Humidity and precipitation? Add a paragraph about projected changes in atmospheric humidity? [Hervé Douville, France]	Rejected. Considering the length of ES, only assessment of precipitation is added.
111409	6	30	6	30	“Annual global-mean land and global-mean ocean” instead of “global-mean land and global-mean ocean” - kindly please verify that this would be correct [Mihaela Caian, Romania]	Accepted. That is correct. It is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79661	6	30	6	34	Presumably these % increases all with respect to each models pre-industrial precipitation levels? It's hard to compare then, as models may have different baseline levels of precipitation to begin with. Why was % increase chosen instead of absolute increases? [Hannah Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. In the paragraph, it specifies that % changes are relative to the 1995-2014 level.
111411	6	30	6	36	Average increases in land precipitation are slightly different to those given in Chapter 8. Is this a result of having used a different set of CMIP6 models? [James Renwick, New Zealand]	Taken into account. It is due to a different set of CMIP6 models. In FGD, Ch8 and Ch4 have consistent results using same set of models.
127413	6	30	6	36	This is too technical for the Executive Summary. Delete and focus on the cross-scenario conclusions that follow in lines 36-41. [Trigg Talley, United States of America]	Taken into account. The statement is revised with reduced complexity.
34613	6	30	6	41	This key message has a lot of numbers, making it a bit hard to follow. Perhaps it could be simplified using the first key message in this chapter (page 5, line 19) as a template. That message summarizes projected changes for temperature, providing the very likely range for the low and high emissions scenarios in the first sentence, then the other scenarios in a following sentence. The multi-model means are excluded. [Russell Vose, United States of America]	Accepted. It is revised consistently with GSAT ES statement.
96363	6	30	6	41	Are there also CMIP6 models that show very unlikely high precipitation changes ? And what is the assessment of those models? They might be more difficult to assess compared to temperature because of lack of observations. And how about models that seem unlikely warm - are they automatically excluded for the assessment of precipitation and other quantities? [Nicole Wilke, Germany]	Taken into account. While global-mean ocean precipitation scales well with GSAT, global-mean land precipitation doesn't. Thus, we use all CMIP6 models' information for the precipitation assessment. 4.8 includes some discussion regarding very high precipitation change.
127415	6	30	6	41	Far too much detail for an Executive Summary. It is repetitive and confusing. The messages could be much simpler. [Trigg Talley, United States of America]	Taken into account. The statement is revised with reduced complexity.
98983	6	31	6	31	I'd urge dropping "under all five SSPs" as extraneous to the sentence. The effect of scenario is covered in the next sentence, so condense the first statement to make it extractable as a clear and understandable finding. [Michael MacCracken, United States of America]	Accepted.
114427	6	31	6	31	I suggest changing to "all the five SSPs considered here" (Since there are many SSPx-y, and since we did not adress all SSP4-x) [Jan Fuglestvedt, Norway]	Accepted.
89277	6	31	6	36	The number of 5–95% range for SSP2-4.5 (1.7–6.4%) is not consistent with the number in Table 4.3 (1.8, 7.7). [Tinghai Ou, Sweden]	Accepted. It is revised.
51091	6	33	6	34	Please clarify here or elsewhere that the the scenarios of GHG concentrations and radiative forcing here are not just dependent on emissions but also on the strength of climate-carbon cycle feedbacks. As an example, concentrations resulting in a radiative forcing of 8.5 Wm-2 by 2100 could result from a lower emissions scenario than SSP5. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Uncertainty in carbon-cycle feedback is not put into the context of uncertainty in climate sensitivity.
69937	6	33	6	36	Some of land precipitation ranges are not consistent with numbers in section 4.3.1.2, especially in lines from 16 to 18. It should be checked. [Young-Hwa BYUN, Republic of Korea]	Accepted. It is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96365	6	33	6	41	Are the model results for precipitation so precise that a precision of one decimal point is justified? Please see also our comment on the Entire Report regarding the accuracy of quantitative information. [Nicole Wilke, Germany]	Taken into account. Quntitative detail has been reduced in the ES. However, always rounding to full numbers would be too imprecise.
107029	6	35	6	50	I would add one sentence about the teleconnection changes for NAM and SAM to support the Technical Summary. [Christophe CASSOU, France]	Rejected. Space limitations and lack of clear tendencies preclude inclusion.
98985	6	37	6	37	Need to change "warming" to "increase" [Michael MacCracken, United States of America]	Taken into account. Sentence no longer appears in ES.
98987	6	38	6	38	I would thing that "unanimously" can be dropped as extraneous. [Michael MacCracken, United States of America]	Accepted.
45459	6	38	6	41	This sentence is not conclusive. A global warming by which year/s? Do the percentage increases in precipitation correspond to the respective warming levels? At which point in time do they occur? [Leonard Borchert, France]	Taken into account. The statement is revised with clearer explanation. Also refer Cross-chapter BOX 11.1 on global warming level for detail information on methodology
89921	6	43	6	43	Please delete "It is virtually certain that"; it is unnecessary. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
132429	6	43	6	43	Perhaps drop the word "projected" here. It is a fact that the projections do this, and you are assessing that that means it is virtually certain that future precipitation changes will as well. [Kyle Armour, United States of America]	Accepted.
98989	6	43	6	43	It seems a bit silly to have the words "It is virtually certain that"--a qualification that is less than a 1 in 100 chance just seems to be overly concerned with statistical details instead of communicating what is going to happen. The "high confidence" at the end of the sentence is sufficient and appropriate. [Michael MacCracken, United States of America]	Accepted.
34615	6	43	6	46	I have low confidence that I understand the first two sentences of this key message. Perhaps they are saying something like this: "While temperature will increase over the entire planetary surface, precipitation will decrease in some areas and increase in others, and the changes will vary by season. Larger temperature increases are associated with more spatially extensive changes in precipitation." Please rephrase to reduce reader ambiguity. [Russell Vose, United States of America]	Taken into account. The statement is revised accordingly.
34617	6	43	6	54	I think some articles are missing from some parts of this text (e.g., I think it should say, "…constrained by THE global energy balance" on line 49). [Russell Vose, United States of America]	Accepted.
18965	6	43		54	This is not exactly the same for extreme precipitation (at least it's more nuanced) so it'd be important to either specify or check with ch 11 that it's actually consistent. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement is for mean change of precipitation. It is specified in the FGD.
84221	6	44	6	46	sentence unclear, the one that follows is clearer to understand. Are they supposed to have the same meaning? [Annalisa Cherchi, Italy]	Taken into account. Since the two sentences are not same, we revised them more clearly.
111879	6	45	6	45	“a larger land area” refers to: “a same-located, but larger area”, or to the global average land area” that “ will experience statistically significant increase” : kindly please verify if a clarification on that would help [Mihaela Caian, Romania]	Taken into account. It means more land areas will experience significant increase than decrease. It doesn't mean same location but larger area. The statement has been revised to give clearer message.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50735	6	45	6	45	As currently worded, this is a little confusing; it leaves the reader wondering "larger than what?" Suggest re-wording to say: "As warming increases, so too will the land area experiencing statistically..." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement is revised.
96367	6	45			Please specify "statistically significant". [Nicole Wilke, Germany]	Rejected. This is standard terminology.
107031	6	46	6	46	Does "likely" correspond to the calibrated language or not? I am not sure here! [Christophe CASSOU, France]	Rejected. The comment seems to be mislocated. There is no "likely" in this sentence.
98991	6	47	6	48	I think it would help to have an explanation about why rainfall over the ocean matters--otherwise I think this statement will be viewed as extraneous information; so, why is this relevant (e.g., indicating reduced clouds so greater solar uptake, altering salininty and so circulation, why?) [Michael MacCracken, United States of America]	Noted. We are mainly focusing on land precipitation since it really matters for society. However, the significant reduction of oceanic precipitation under GHG warming has important implication for atmospheric circulation, cloud, salinity, etc as you mentioned. More explanation is in the main text.
4099	6	48	6	49	delete "(land plus ocean)" as it is not required when referring to a global context. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is added to make the meaning clearer. There had been plenty of misunderstanding in an earlier draft omitting this addition.
50745	6	49	6	51	be constrained by the global energy balance, whereas regional precipitation changes will be dominated by thermodynamic moisture convergence (please could you use simpler language to explain 'thermodynamic moisture convergence) and dynamical processes (high confidence). Precipitation will increase in large parts of the monsoon region, tropics and at high latitudes..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement is revised.
111881	6	50	6	50	“regional precipitation changes will be dominated by thermodynamic moisture convergence and dynamical processes” - suggest to add the aerosols as regional factor, so suggested replacement: “regional precipitation changes will be dominated by thermodynamic moisture convergence, dynamical processes and aerosol regional changes” [Mihaela Caian, Romania]	Taken into account. The statement is revised.
98993	6	50	6	50	These terms are pretty technical and will only be understood by experts; so why not say something like land-ocean temperature contrasts, occurrence of cyclonic systems, etc.--whatever, but something more understandable. [Michael MacCracken, United States of America]	Taken into account. The statement is revised.
2131	6	51	6	51	high latitudes [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
4101	6	51			Bad grammar. Suggest changing to, "Precipitation will increase in large parts of the monsoon regions, tropics and high latitudes". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
111883	6	52	6	52	maybe would be of interest to specify “decrease over the Western Mediterranean”, instead of “decrease over the Mediterranean” (this is in agreement with Fig. 4.12 pg. 154, and also keeps agreement with CMIP5 that showed some increased cyclonic activity in Eastern Mediterranean in summer) [Mihaela Caian, Romania]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98995	6	52	6	52	How about changing to say "in response to changes in atmospheric circulation caused ty greenhouse-gas-induced global warming." [Michael MacCracken, United States of America]	Rejected. Not only atmospheric circulation but also thermodynamic processes play crucial roles on the regional changes, which is already mentioned in the previous sentences.
98997	6	53	6	54	Would it not be better to indicate that "Interannual variability of precipitation wiii increase over many of the world's semi-arid regions as the subtropics expands poleward."? So give a location. Regarding the variability over the mid-latitudes, will the increase be nearly so much given the average is considerabley higher? [Michael MacCracken, United States of America]	Noted. As you mentioned, the increase in interannual variability of precipitation in semi-arid regions is associated with the poleward expansion of the subtropics. However, interannual variability of precipitation is projected to be increased in other land regions as well. Thus, we want to keep the sentence.
96369	6	54			Please specify term resp. temperature increase of a "warmer world". [Nicole Wilke, Germany]	Taken into account. The sentence is revised.
116277	6		6		The statement related to near term uncertainty and possible effect of volcanic eruptions could also be integrated into the likelihood of reaching a certain level of warming on a certain time horizon (with or without this) as a novel aspect. [Valerie Masson-Delmotte, France]	Taken into account. The previous paragraph now includes a statement that it assumes no major volcanic eruption. Any statement modifying the stated likelihoods due to volcanic eruptions would be speculative, however, because they would have to make ad-hoc assumptions of eruption time and magnitude.
2133	7	1	7	7	Smith et al 2019, npj Clim. Atm. Sci. show that the uncertainty in near term rainfall predictions is greatly exagerated by models and that very large ensembles can yield much more certain predictions. The "natural internal variability" shown here is therefore likely to be much smaller in the real world. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Model uncertainty has been added.
130491	7	1	8	55	In excutive summery, I have not seen assessment on biosphere contents, but there are some contents under sub-section in pages 27-31. [Panmao Zhai, China]	Rejected. Space limitations preclude the inclusion of biospheric material in ES.
65689	7	2	7	3	Suggest including a year range for "In the near term" i.e. "In the near term (yyyy-yyyy) ..." [Kushla Munro, Australia]	Accepted
50747	7	4	7	4	"The anthropogenic aerosol forcing decreases in most scenarios, contributing to increasing global-mean surface air temperature decreases in most scenarios" - with an assumed reduction of coal combustion emissions? If so, this needs to be said here, I feel. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Ch6 explains those aspects.
2359	7	4	7	6	Anthropogenic carbonaceous aerosols are projected to increase in SSP3.70 in the near term (up to 2040-50) (See figure 6.4). This sentence needs a caveat. [Vaishali Naik, United States of America]	Rejected. It reads most scenarios not all scenarios.
96371	7	5			As only global-mean land precipitation is mentioned: please include assessment/statement on ocean precipitation. [Nicole Wilke, Germany]	Rejected. We are mainly focusing on land precipitation since it really matters for society.
98999	7	6	7	8	Would it not be more informative to say that the cooling induced by volcanic eruptions tends to shift the mid-latitude storm track equatorward toward where it was before global warming, so sort of reversing subtropical expansion. Saying it this way would help to make clear that the forcing matters and things all make pretty good sense. [Michael MacCracken, United States of America]	Taken into account. Cross Chapter Box 4.1 covers more aspects regarding impacts of volcanoes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4103	7	10	7	17	Several aspects of grammar need changing in this ES statement: "more increase" to "greater increase"; "and weakened North American monsoon" to "and a weakened North American monsoon"; "The near-term changes in global monsooon precipitation…" to "Near-term changes in global monsoon precipitation..." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
34619	7	10	7	17	I recommend deleting the first sentence of this key message. I'm making this recommendation for two reasons. First off, I think very few people think about the "global" monsoon because monsoons are such a regional feature. Secondly, the second sentence of the key message does a much better job of summariaing the projected changes. You can read that sentence and immediately understand how things are going to change and where. [Russell Vose, United States of America]	Rejected. Global monsoon is also one of important global quantities (e.g., Wang and Ding, 2008, Dynamics of Atmospheres and Oceans, 44, 165-183). Thus, we want to keep the first sentence.
50749	7	13	7	13	Suggested edit: 'characterised by a greater increase in the..' instead of 'more increase' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
79679	7	15	7	17	Section 10.6.3 should also be mentioned here. IPO should be replaced by PDV in agreement with Annex VI definitions. [Laurent Terray, France]	Accepted
99001	7	15	7	17	It seems to me that it would be useful to indicate that there is a trend caused by global warming and then on top of this are variations being caused by the internal variability that could temporarily enhance and diminish the change, but the change will continue to intensify as global warming continues--I think this would be more informative than saying "will be affected". [Michael MacCracken, United States of America]	Taken into account; sentence rephrased.
69939	7	19	7	50	I think it would be better to move this part to the next of "Cryosphere and ocean" summary part in order to keep consistency with the writing order for each section ("4.3.2.Cryosphere, Ocean, and Biosphere" and "4.3.3. Modes of Variability" in the text). [Young-Hwa BYUN, Republic of Korea]	Noted. ES statements in large-scale circulation and modes of variability" also include changes in atmospheric circulation (4.3.1). Thus, we want to keep the order.
34621	7	19	7	50	Perhaps there should be a key message at the end of the "Large scale circulation and modes of variability" section saying that there is low confidence about projected changes in most other modes in all SSPs. Something similar to this came up in Chapter 2 -- i.e., most modes of variability exhibit no sustained trends since the 19th century -- and so we added a key message to that effect to keep the reader from having to guess. [Russell Vose, United States of America]	Taken into account. However, assessments of changes in the various modes are heterogeneous, precluding a simple summary statement.
50751	7	21	7	26	It would be useful to describe the Northern and Southern Annular Modes somewhere, and what a decrease or increase in either actually means? Suggest signposting readers to the Glossary here too. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. There is already the technical annex on Modes of Variability (Annex VI).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2135	7	21	7	26	This statement should allow for the fact that the trend is likely to reverse to a decreasing SAM in the near term as models often underestimate changes in the NAM and SAM (Scaife and Smith, Clim. Atm. Sci., 2018) [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There is a lack of literature investigating whether there is a signal to noise paradox in the Southern hemisphere. Literature shows that models can simulate the historical SAM trend within uncertainty from internal variability, e.g. https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2020GL088890 and also section 3.7.2
52955	7	21	7	26	Beyond the SAM, what may be the near-term circulation response to the anticipated phase of the AMV and PDV? [Hervé Douville, France]	Noted. PDV has been shown to influence the pace of global warming, however there is still large uncertainty about PDV evolution in a future climate (even in the near term), mainly because it is still unclear to what extent PDV is externally forced or internally generated. On the other hand AMV predictions are more skilful and there is medium confidence in the predictions of AMV impacts. AMV and PDV predictions are assessed in section 4.4.3
50753	7	23	7	24	Would it be possible to add here which is likely to exert the greater influence on Southern Annular Mode trend in austral summer: stratospheric ozone recovery or increases in other greenhouse gases? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. CMIP6 simulations prescribing separate ozone and GHG external forcings were not performed for the future scenarios, so we are unable to assess this breakdown.
127417	7	25			This is a strange way to say that forced trends are small compared to the internal variability. [Trigg Talley, United States of America]	Taken into account. The sentence is revised.
45797	7	28	7	29	Can this statement be generalized to more general future warming scenarios/projections? [Twan van Noije, Netherlands]	Rejected. Projected Southern hemisphere circulation trends are very scenario specific owing to the interplay between ozone recovery and well-mixed GHG forcing. Therefore this statement needs to be related to specific scenarios.
111413	7	32	7	32	Change "intensify" to "strengthen" [James Renwick, New Zealand]	Accepted
99003	7	32	7	32	How about changing "for high-emission" to "for the higher emission" [Michael MacCracken, United States of America]	Taken into account. It is change from "high-emission scenarios" to "SSP5-8.5"
96373	7	32			Please specify "high-emission scenarios" (as on page 8, line 26: "low-emissions scenarios SSP1-1.9 and SSP1-2.6"). [Nicole Wilke, Germany]	Accepted
89923	7	35	7	35	Please quantify "slightly more positive" [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The NAM change in CMIP6 models has been quantified.
2137	7	35	7	36	if the trend is very small as implied by "slightly more positive" and the internal multidecadal variability is very large, as we know from many studies (e.g. Scaife et al., Clim. Dyn., 2009, then how can it be very likely to become more positive? [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The NAM change in CMIP6 models has been quantified. Also the likelihood statement has been removed.
10973	7	35	7	36	This very confident statement on the NAM could be reconsidered, eg 1) it's vague on the timescale, 2) some large changes in fig 4.33 seem to be ruled out by 'slightly more positive', and 3) the signal-noise issue of Scaife, Smith etc is discussed elsewhere in this chapter, and suggests 'high confidence' might not be justified here. (eg https://doi.org/10.1038/s41612-018-0038-4). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The time period and the index the statement is based on have been specified. The likelihood statement has been removed and the new evidence for the poor simulation of seasonal-to-decadal NAM has been described.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106847	7	35	7	41	I would be less confident in the NAM ES because when all the members available in CMIP6 are taken into account like in the ES, the spread increases and I doubt the forced response falls in the 5-95% confidence range. In addition, the physical processes associated with NAM, and in particular its barotropic feature, will be very likely perturbed in a warming world and therefore the conclusion might depend on the metrics used for assessment (e.g Cattiaux and Cassou 2013, doi :10.1002/grl.50643). In addition when using the NAO index instead of the NAM index, there is no trend. This tends to suggest that the trend of the NAM is dominated by the Pacific and not by the Atlantic as opposed to historical period, thus questioning the true zonal nature of the mode. Lastly, Chap3 assesses the simulation of NAM with medium perfornance because of the signal-to-noise paradox. Therefore, based on all these arguments,I would replace "very likely" by "likely" and "high confidence" by "medium confidence". The period for the ES needs to be specied as well: Here long-term. I would also include the other scenarios in the ES because it is also important to state that no significant changes are expected for lower levels of warming (below 3 degrees?) [Christophe CASSOU, France]	Taken into account. The time period and the index the statement is based on have been specified. The likelihood statement has been removed and the new evidence for the poor simulation of seasonal-to-decadal NAM has been described.
96375	7	35	7	41	"Northern annular mode": This expression is not used in the text, it is always abbreviated, and outside the modelling community NAM is probably not very often used, more known as NAO. Please revise. [Nicole Wilke, Germany]	Noted. The NAM acronym is defined at page 11 line 12. The NAO and the NAM are related but have different global expressions. Of the two, the NAM is the global pattern, which is therefore the most appropriate to refer to in this chapter. It is consistent with the assessment of annular modes in chapters 2 and 3.
79681	7	38	7	39	I would suggest to be a bit more specific than "large internal variability and structural differences among models". Why not cite explicitely the tug of war between Arctic amplification and upper tropospheric warming ? [Laurent Terray, France]	Taken into account. Text has been amended.
99005	7	38	7	39	It seems to me that a major contributor meriting mention is the interaction of the circulation with the large orographic features, so the Himalayan Mountains, Tibetan Plateau, Rocky Mountains, and Greenland, with the zonal movement of atmospheric waves tending to get stuck due to their waves being larger with the reduced equator-pole temperature gradient. It is just hard for the waves to move smoothly, and this is hard to do in models without quite high resolution, and there is variability as early snow cover occurs in some locations. So, it seems to me mentioning both the larger nature of the waves and the obstructive orographic features. [Michael MacCracken, United States of America]	Rejected. Stationary waves affect the atmospheric circulation but they are not explicitly assessed in the chapter.
26825	7	39	7	40	Please specify the direction of the change and if the response is consistent between models in this case. [Eric Brun, France]	Noted. The statement refers to the fact that model projections for the North Atlantic are not robust and that responses in different models can show the opposite sign from one another. Hence it is not appropriate to add a direction of change or consistency statement.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127419	7	40			Why is it that only N. Atlantic storm tracks are to be used for storyline constuction? [Trigg Talley, United States of America]	Taken into account. So far the storylines literature has focused on the European/Mediterranean and Southern hemisphere midlatitudes. Apart from a weakening of the storm track in the Mediterranean, which is consistent across models, uncertainties in the regional European storm track responses are large, hence why storylines are particularly useful there. Wording has been changed to not imply storylines can only be applicable in these regions.
99007	7	43	7	43	Again, saying "virtually certain" seems a useless qualification. Given the content of the next sentence, I am surpursed that the phrase being used is not "very likely" [Michael MacCracken, United States of America]	Rejected. The first sentence is much easier to support than gradual changes in ENSO.
15487	7	47	7	47	Given that 1) not much about extreme El Nino and La Nina is discussed in the main text; 2) operational climate centres around the world normally define El Nino and La Nina events (including extreme events) by SST anomalies of Nino regions rather than rainfall variability, it is suggested to remove the text "which is used for defining extreme El Ninos and La Ninas". [SAI MING LEE, China]	Taken into account. Many recent publications use rainfall variability for the definition of extreme ENSO. Anyway, the sentence is revised accordingly.
81635	7	52	7	52	the last bullet also covers non-cryosphere land. Consider revising the section header [Sönke Zaehle, Germany]	Noted. Complete consistency would require awkward header strategy, though.
18967	7	53		41	It is the only time that there is regional change mentioned in the ES, I think it would be really important to mention above with the rainfall projections asa well that they are very different regionally & that confidence levels are also very different. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Space is very limited, however.
127421	7	54	7	55	Why is this 2081-2100 the time period highlighted? This is going to happen much sooner under most scenarios. [Trigg Talley, United States of America]	Taken into account. The wording has been improved. The first sentence refers to the ensemble and period average sea-ice area being below 10^6 km^2. On the other hand, the second sentence refers to when the sea ice area in individual realizations is first below 10^6 km^2, which indeed happens before 2081-2100 in most scenarios.
9667	7	54	8	1	I am not a specialist of sea ice, but at first sight, this seems inconsistent with the conclusions of Notz et al (GRL, 2020): "In the vast majority of the available CMIP6 simulations, the Arctic Ocean becomes practically sea-ice free (sea-ice area < 1 million km2) in September for the first time before the year 2050 in each of the four emission scenarios SSP1-1.9, SSP1-2.6, SSP2-4.5 and SSP5-8.5 examined here." [Olivier Boucher, France]	Taken into account. The wording has been improved. The first sentence refers to the ensemble and period average sea-ice area being below 10^6 km^2. On the other hand, the second sentence refers to when the sea ice area in individual realizations is first below 10^6 km^2.
26827	7	54	8	1	This statement seems inconsistent with the conclusions of Notz et al (GRL, 2020): "In the vast majority of the available CMIP6 simulations, the Arctic Ocean becomes practically sea-ice free (sea-ice area < 1 million km2) in September for the first time before the year 2050 in each of the four emission scenarios SSP1-1.9, SSP1-2.6, SSP2-4.5 and SSP5-8.5 examined here." [Eric Brun, France]	Taken into account. The wording has been improved. The first sentence refers to the ensemble and period average sea-ice area being below 10^6 km^2. On the other hand, the second sentence refers to when the sea ice area in individual realizations is first below 10^6 km^2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45461	7	54	8	4	If the latter sentence only refers to CMIP6 models, does the former one reference the CMIP5/6 ensemble mean? But the first sentence talks about the SSP scenarios, which have only been around since CMIP6. In other words: is there a difference in the underlying data base between the first and the second sentence? The phrasing suggests there is. [Leonard Borchert, France]	Taken into account. The wording has been improved. Both sentences refer to the CMIP6 models.
99009	7	54	8	4	I don't understand. The second sentence concludes that first-time September ice-free conditions are projected to occur before 2050, but the first sentence says it is only likely in the interval 2081-2100 and says this is high confidence? So, is the difference here that it will take over three decades to go from the first occurrence to being a 100% of the time occurrence. Somehow, with continuing warming going on it is hard to see how there could be such persistence of a bit over 1 M km2 of sea ice. I might also note that all this focus on a little bit more change in September is a bit strange--the key is when the albedo drops in spring from a 70-80% albedo to a 20% or so, and then to the dark ocean value. So, what I really want to know is when the jump is from low ocean heat uptake to high ocean heat uptake--that is what I want to know rather than sea ice cover in September with its low sun angle. And then this heat is carried over into the winter and so providing heat to the polar cold season atmosphere. So, one impact comes from the absence of sea ice during the winter storm season as large waves then get formed and erode coastal regions, and the other is increased eveporation of water to fall as snow over land--and then the third is the warmth contribution to warming and softening and eventually melting the Greenland ice sheet. It would sure be nice if this assessment were to get over talking about September sea ice cover (which I agree some out there make into some sort of tipping point) and talk about the aspects of the change that have some real impacts and may indeed relate to tipping points. Be creative and inventive and scientific instead of just talking about a measure that has falsely been portrayed as incredibly significant. [Michael MacCracken, United States of America]	Taken into account. The wording has been improved. The first sentence refers to the ensemble and period average sea-ice area being below 10^6 km^2. On the other hand, the second sentence refers to when the sea ice area in individual realizations is first below 10^6 km^2.
96377	7	54			The Arctic is regularly referred to as "Arctic Sea" in this chapter (not "Arctic Ocean"). Please ensure consistency. [Nicole Wilke, Germany]	Rejected. It is common to use "sea" when referring to sea ice, and "ocean" when referring to the ocean generally. Complete consistency does not work well in this case.
50755	7	55	7	55	Suggest it would be helpful to mention here that September is when Arctic sea-ice reaches it's annual minimum extent. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This mention has now been made.
34623	8	1	8	4	This sentence be simplified along the following lines: The vast majority of CMIP6 simulations show the first ice-free September will occur before 2050 and before anthropogenic CO2 emissions reach 1000 GtCO2. [Russell Vose, United States of America]	Taken into account. Thank you, this proposed wording works well and has been adopted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2139	8	2	8	2	This statement includes the effects of internal variability in determining the first ice free Arctic date but the earlier statements on exceeding the 1.5 deg threshold do not. This is despite clear evidence from decadal predictions for a much earlier temporary exceedence of 1.5 deg in Smith et al GRL 2018. A symmetric approach to the two threshold exceedances should be taken or it looks biased. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Rejected This point is well taken but perfect symmetry across this chapter, and the report generally, in how threshold crossings are characterized is practically impossible.
6645	8	2	8	2	Stating that the ensemble spread "includes the observational range of uncertainty" is rather weak, as the ensemble spread could be much too large but include the observational spread. Would it be correct to use a word such as "matches" rather than "includes"? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The word "includes" has been changed to "is within". "Matches" does not quite work because the spread across realizations for a given model may be less than the observation uncertainty.
50763	8	3	8	3	...the vast majority of simulations show an ice-free Arctic for the first time in September before 2050' - is it possible to include when before 2050 this occurs for different SSPs? It would be helpful to understand if this timing differs across the range of scenarios. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This level of detail is beyond that of an executive statement.
132431	8	3	8	3	It is strange that Arctic sea ice is assessed relative to cumulative carbon emissions, when all other variables in the chapter are assessed relative to global temperature change. I realize there is a paper that links sea ice to cumulative carbon, but I suggest you instead describe sea ice loss as a function of global temperature for consistency, or else describe all other changes as a function of cumulative carbon as well. The link between sea ice and 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]	Taken into account. Your point is well taken. In the Executive summary statement, the reference to cumulative emissions has been removed.
10033	8	3	8	4	Must be 1000 PgC not GtCO2, we are already at ~2500 GtCO2 of emissions. [Andrew MacDougall, Canada]	Taken into account. Thank you for pointing this out. The units are correct but the change in emissions is between present day and the future. The characterization of sea ice loss in terms of cumulative emissions has been removed.
127423	8	3	85	3	"In a scenario where aerosol injection is used to limit RF at year 2020 levels..." It needs to be clarified that authors mean aerosol injection into the stratosphere (SAI), not "aerosol injection" into clouds (MCB). Really, the same comment applieds to this whole paragraph: be clear where statements are for SAI vs for MCB vs for any mechanism of SRM. [Trigg Talley, United States of America]	Taken into account. Comment applies to page 85, not ES. Clarification added.
9669	8	6	8	8	"very likely" seems a weak statement. What is the physical mechanism that would make sea level rise not to rise throughout the 21st century. Is the "very likely" is warranted by SSP119 not seeing sea level rise continuing towards the end of the century, then this sentence fails to convey the certainty that sea level will rise continuously in the other scenarios. [Olivier Boucher, France]	Accepted and strenghtened.
26829	8	6	8	8	Very likely seems a weak statement [Eric Brun, France]	Accepted and strenghtened.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9785	8	6	8	10	I worry that the very likely ranges are used here without some qualification about the potential for substantially larger amount of GMSL rise. The corresponding statements in chapter 9 highlight (though perhaps inadequately) deep uncertainty; there is no mention of this here. [Robert Kopp, United States of America]	Taken into account. This point is well taken and a cross report effort is done to find ways in which to better communicate deep uncertainty. In this executive statement it is quite clear that the GMSL ranges reported are under the SSP scenarios and these scenarios alone.
34625	8	6	8	10	I like short, pithy key messages, but this may go a tad too far. It might be prudent to included projected GMSL changes for at least one other SSP (presumably a lower one) to give the reader a better feel for the range of possible futures. [Russell Vose, United States of America]	Taken into account. Good point. Results for a low and high SSP are now reported.
89415	8	6	8	10	I would recommend to resolve potential inconsistencies with the estimates given in Chapter 9. [Ricarda Winkelmann, Germany]	Taken into account. Inconsistencies with Chapter 9 have been resolved.
15185	8	6	8	10	The sea-level content in this chapter i strong, but needs to more prominently articulate the limits of model ability to project ice-sheet melt, and the long tail of higher GMSL projections from other sources. This entry in the Executive Summary will lead some to mistakently conclude this is the full range of potential GMSL this century, whereas the text, references and other assessments (like the US NCA, which has an upper bound of 1.3 m, which increased to 2.4 m if ice cliff instability, etc. is considered) present the possibility of higher GMSL this century. I appreciate that the ES language here specifically points to model output. The problem is other estimates are not offered. This is a huge problem. If the IPCC doesn't offer the full range of possibilities, low confidence or not, this chapter will run into the same problem the IPCC has had with interpretation of SLR projections since AR4. [Simon Donner, Canada]	Take into account. This point is well taken and a cross report effort is done to find ways in which to better communicate deep uncertainty. In this executive statement it is quite clear that the GMSL ranges reported are under the SSP scenarios and these s
7519	8	6	8	10	Why report SSP5-8.5 since there is already pushback showing the worst case scenario (business as usual) is too pessimistic and highly unlikely. You should reference the most likely senario. My university STEM students notice this highlighting of worse case scenarios and it makes them cynical toward the entire report. Please help me prevent my students from becoming skeptics. [Hugh Lefcort, United States of America]	Rejected. WGI cannot assess the realism of the scenarios but instead assesses the published literature, which does use SSP5-8.5.
71265	8	6	8	10	No description about regional variations in future GMSL? [Kenji Taniguchi, Japan]	Noted. This is mandate of Chapter 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99011	8	6	8	10	This is a totally inadequate presentation of the risk given the obervations that a number of ice sheet glacial streams are approaching destabilization and how ice shelves are thinning due to ocean warming. 20,000 years ago sea level was down 120 meters when global average temperature was down about 6 C, and back further in history there were essentially no polar ice sheets and sea level was up of order 60 meters when global average temperature was up about 3 C--the equilibrium sea level sensitivity looks to be of order 15-20 meters per degree C. For the 120 centuries from 20ka to 8ka, sea level rose at an average rate of 1m/century as global average temperatures rose at an average rate of 1 C per 2000 years. To suggest that global warming of 1 C already and 1-3 C during the 21st century is going to lead to a total rise of roughly a meter or less just does not seem plausible, and choosing not to mention the risks posed by ice sheet deterioration is simply unacceptable in conveying risk to the public. The start of the chapter also said it was projecting out to 2300, and warming of a few degree C persisting like this is going to cause really significant ice loss (check out the documantary "Chasing Ice" to see how fast change can occur). Just because the effects of moving ice and ice shelf thinning cannot be calculated is not an excuse for not presenting the risk based on the paleoclimatic record. I think this paragraph is simply unacceptable. And covering this very critical issue in 4.5 lines is also simply inappropriate as there are at least technological responses to warming (i.e., air-conditioning) whereas for sea level rise, levees will not be able to be built high enough and in enough locations to prevent very large displacement, especially from the many urban centers built at sea level. This paragraph and presumably the analysis in the chapter from which this came simply have to be completely redone [Michael MacCracken, United States of America]	Noted. These issues are treated in other chapters (e.g. Chapter 2 for paleo analogues and Chapter 9 for deep uncertainty). The statements made here are solely based on results under the SSP scenarios, as is made clear at the beginning of this subsection.
96379	8	6	8	15	Please check these statements about GMSL and AMOC in relation to those in Ch9. For the AMOC, they give different numbers and confidence levels compared to Ch9. [Nicole Wilke, Germany]	Taken into account. Inconsistencies with Chapter 9 have been resolved in due course.
111957	8	7			One could expect the quantification of the share of glaciers and ice sheets melting in the highlight in bold, actually, more than a third due to thermal expansion gives too much uncertainty [Tomas Halenka, Czech Republic]	Taken into account. Good point. The statement has been changed to "with more than two-thirds of that rise due to glacier and ice sheet melt".
31537	8	8	8	9	Two comments: can other scenario be mentioned? Need to discuss with chap 9: while I understand chap 4 wants consistency in providing v. likely range, chap 9 provides likely range [Jean-Baptiste SALLEE, France]	Taken into account. With regards to the first comment we now additionally report of SSP1-2.6 results. With regards to the second comment we coordinate with Ch9 to resolve it.
54953	8	8	8	10	While we understand that the GMSL values included here are the very likely range vs the likely range included in the SPM, it is important that there is consistency within IPCC products in terms of key findings in order to reduce the chance that different values will be cited/used. We will leave it to the authors to decide on the best approach (e.g. include both likely and very likely ranges, switch to likely range etc.) to reduce the chance that different sea level rise projections will be cited. [Nancy Hamzawi, Canada]	Taken into account. Consistency with Chapter 9 has been established.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89925	8	14	8	14	Please quantify the magnitude of potential weakening [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The magnitude of potential weakening is now stated.
21619	8	15	8	15	It is unclear to me what strengthen to historical values means - do you rather mean recover to historical values here? [Peter Thorne, Ireland]	Taken into account. "strengthen" has been changed to "recover".
52957	8	17	8	18	What about the uptake increase relative to the emission increase? Will the land and ocean carbon sink efficiency change depending on the SSP scenarios? [Hervé Douville, France]	Noted. These are important considerations that lie within the mandate of Chapter 9.
50765	8	17	8	22	It would be really useful to also include projections of ocean warming, in the ocean and cryosphere section or the earlier temperature section of the Executive Summary. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is a valid suggestion but space considerations have dictated the prioritization of variables reported in the Executive Summary.
96381	8	18	8	19	Please define a time frame. Like per year, decade, … or cumulative. [Nicole Wilke, Germany]	Taken into account. "net" has been changed to "cumulative" to make this clearer.
99013	8	20	8	21	With there being indications from observations that both the Amazon forest and high polar latitudes may become carbon sources rather than sinks, suggestions that global carbon uptake needs to be explained--where is it happening and how long will it continue to go on. Also, rapid temperature rise tends to stress ecosystems and they become vulnerable to pests and/or burn and the carbon is released whereas it taks very long times for the re-creation of ecosystems with substantial carbon content. There needs to be an explanation of where these increases in carbon uptake will be occurring in ways that pests, moisture stress, etc. are not countering their role in carbon uptake. [Michael MacCracken, United States of America]	Noted. These considerations are beyond the mandate of Chapter 4.
84223	8	20	8	21	would it be possible to add a number for the different increase of carbon uptake over land and over ocean? [Annalisa Cherchi, Italy]	Taken into account. The land values now appear the statement.
84225	8	21	8	22	what happen to ocean pH in ssp1-2.6? It decreases or it remains constant? [Annalisa Cherchi, Italy]	Taken into account. Ocean pH levels decrease and then increase slightly. This is noted now.
35091	8	21	8	22	a range of pH changes would be nice [Baylor Fox-Kemper, United States of America]	Rejected due to space limitations; this is for Chapter 9.
114433	8	26	8	27	This sentcen is quite long. You may consider splitting it. Tthe first part is quite vague; "limit globally averegaed surface warming" compared to what? [Jan Fuglestvedt, Norway]	Taken into account; sentence rephrased.
106269	8	26	8	27	The message that these scenarios will "limit" warming is extremely vague. Can something slightly more precise be formulated, like "will stabilize", "will halt", "will cap"? [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised
106271	8	26	8	36	The ES message applies concepts of "global warming" different from how they are defined in the glossary. For clarity, this could be reworded. For your information, the current glossary definition of Global warming "The estimated increase in global mean surface temperature (GMST) averaged over a 30-year period, or the 30-year period centered on a particular year or decade, expressed relative to pre-industrial levels unless otherwise specified. For 30-year periods that span past and future years, the current multi-decadal warming trend is assumed to continue." [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have not used the term "Global Warming". Further, it is common to use 20-yr period to compute means as can be seen in several instances in the report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108011	8	26	8	53	The difficulty of detecting and quantifying the effect of emissions reductions in the near term presents a substantial risk that must be incorporated into climate decision-making. In particular, the near-term uncertainty in climate trajectory and lagged response for both CO2 mitigation and CDR approaches justifies investigation of climate intervention (particularly SRM) approaches that can rapidly influence the climate system. This is a key and substantive policy-relevant feature of potential SRM approaches, as discussed later in this chapter (AR6 WG1 Ch.4 pg. 91, lines 2-4). [Kelly Wanser, United States of America]	Accepted. The comment is appreciated
50767	8	27	8	27	Suggest edit for clarification: "Emissions reductions as represented in the low-emissions scenarios SSP1-1.9 and SSP1-2.6 are virtually certain to limit globally averaged surface warming in the latter half of the 21st century, compared to higher emissions scenarios.." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; Text is revised
132433	8	27	8	27	To what level of warming are you limiting globally averaged surface warming to? The next paragraph says limiting to 1.5C or 2C, so is that what you mean here as well? Or do you mean limit the rate of warming? [Kyle Armour, United States of America]	Taken into account. We mean the climate response to different scenarios are indistinguishable in the short term. Text is revised
2361	8	27	8	29	The effect of emissions reductions on the warming rate in the near term will also be masked due to opposing effects from reductions in cooling (sulphate) versus warming (methane, ozone, BC) SLCFs (CH 6). The role of SLCFs in the near-term needs to included here. [Vaishali Naik, United States of America]	Accepted. However, in the short term, interannual variability masks the distinction between scenarios, independent of the source of forcing
11085	8	28	8	29	Here it states that "the effect of emissions reductions on the globally averaged warming rate in the near term (2021–2040) will be hard to detect due to masking by natural internal variability (high confidence)". On the other hand, it states (in 5 L31-L34) that "A warming level of 1.5°C in globally averaged surface air temperature, relative to the period 1850–1900, is, in the near-term period 2021–2041, very likely to be reached in scenarios SSP3-7.0 and SSP5-8.5, likely to be reached in scenarios SSP1-2.6 and SSP2-4.5, and more likely than not to be reached in Scenario SSP1-1.9 (high confidence)", which means that low emissions will very likely lead to the increase of warming rate. Both statements are of high confidence. Is there a discrepancy here? [Wen Wang, China]	Taken into account. Here, the concern is the overlap of the PDF distributions but in page 5 L31-34, the means (over 20 or 30 years) are discussed.
127425	8	29			Delete "masking by". It adds nothing. [Trigg Talley, United States of America]	Accepted; Text is revised
50769	8	31	8	31	" effect of emissions reductions on the globally averaged warming rate in the near term (2021–2040) will be hard to detect due to masking by natural internal variability' -is this the only reason or due also to inertia in the climate system and a lifetime of greenhouse gases being emitted now? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In this case, only reason is internal variability as we compare two different scenarios using a large initial condition ensemble. Inertia of the past should be operating on both scenarios and the lifetime of the gases are unchanged

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99015	8	32	8	34	I don't understand the point of this sentence. By saying "individual points" is this just saying that cooling can occur anywhere with no implication meant that it would happen to all points at once or over the globe for some extended period? I just don't understand what the point is. And, it needs to be more clearly indicated that this does not mean an absence of all warming at any location, just that some locations may not be as much warmer than others or may not warm much for a decade or so due to how the circulation is changing, etc. I'd urge some clarification. [Michael MacCracken, United States of America]	Taken into account. We mean "all regions". Text is revised.
9671	8	33	8	33	I think you mean "any point" or "any region" rather than "all points". [Olivier Boucher, France]	Taken into account. It means "all regions" though not simultaneously. Text is revised
114431	8	34	8	34	Insert "global" before "surface" [Jan Fuglestvedt, Norway]	Accepted; Text is revised
99597	8	35	8	35	Delete "in" [Stefan Sobolowski, Norway]	Accepted; Text is revised
114429	8	35	8	35	delete "in" before "about" [Jan Fuglestvedt, Norway]	Taken into account, text is revised.
106273	8	36	8	44	It should be made clear whether this speaks to CDR in general or global net CDR. In the latter case, the assessment seems plausible. However, in the former case, this ES message contradict the general understanding that a reduction in the annual net CO2 emissions will result in an quasi-instantaneous reduction in the rate of warming. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. It is the later case. Text is revised
106275	8	36	8	44	Please specify the extent of this lag (is this 5, 10, 50 or 100 years), and reflect upon it in context of the previous ES message which highlights that the detection time of mitigation benefits for surface air temperature is in about 25–30 years for the global mean. If the anticipated lag is less than 25 years, does that mean it is basically undetectable (and maybe irrelevant)? Clarifying this would be useful. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The lag is decades for some variables, centuries for some variables. Text is revised
50757	8	38	8	41	The key message here is not completely clear. Is it that CDR has a vital role to play in scenarios that limit warming to 1.5 or 2C, but CDR can't be expected to play much of a role in high emissions scenarios? Is this because the scale of viable CDR deployment does not change for high emission scenarios so would have limited effectiveness with greater warming? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text is revised.
111813	8	38	8	44	I think that this para doesn't talk about CDR as such but about net negative emissions delivered with the help of CDR, otherwise it would not make sense to talk about the climate response and termination effects. If this paragraph were an CDR as such then the first sentence should not contain the word "typical", because even to get to net zero you need CDR [Oliver Geden, Germany]	Accepted; Text is revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41395	8	38	8	53	The authors have made a great effort elaborating on CDR and particularly SRM, in the chapter. Please consider elevating more (quantitative) information to the ES, as this issue will be of increasing interest to policymakers. For example, the fact that that a sudden and sustained termination of SRM will not only cause rapid warming, but that it would "increase both land and ocean temperature and precipitation at a rate that far exceeds (> an order of magnitude) that predicted for future climate change without SRM" is key to communicate (verbatum from section 4.6.3.3). [Alexander Nauels, Germany]	Accepted; Text is revised
10245	8	39	8	41	This sentence assumes only 1 CDR option is deployed whereas nearly all experts on CDR/NETs believe that it is much more likely that a portfolio of CDR/NETs options will be deployed. This point is also addressed below in relation to the main text. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, We write "CDR approaches"
132203	8	39	8	41	"However, under high-emission scenarios, model-based assessments suggest that CDR approaches currently considered viable have limited potential in mitigating warming (medium confidence)". This seems like an important statement but I could not find it verbatim in the chapter (I looked for the term "viable") and thus could not find the source. Here and elsewhere: I strongly recommend the authors to ensure that the text of the ES can be found verbatim somewhere in the chapter, e.g. as a summary of given sections or subsections (see approach we have mostly followed in Chapter 11) [Sonia Seneviratne, Switzerland]	Taken into account. Text is revised. This sentence is deleted in the revision
96383	8	39	8	41	The term "limited potential" is not well defined. In fact, falsifying the opposite statement "unlimited potential" as a check reveals that the statement in the current text is rather trivial. It would be very important to find a semantically better defined term to describe the findings from CDRMIP and Chapter 4.6.3.2. Perhaps "low" potential, "small" compared to ... [Nicole Wilke, Germany]	Taken into account. Text is revised. This sentence is deleted in the revision
54955	8	39	8	41	This statement is unclear. Is the intended message that CDR deployed at levels currently considered viable will not significantly reduce global warming from the high levels in high emission scenarios? [Nancy Hamzawi, Canada]	Taken into account. Text is revised. This sentence is deleted in the revision
132189	8	40	8	41	"CDR approaches currently considered viable". This is a bit vague, but seems important. Can you clarify which CDR approaches are meant here? [Sonia Seneviratne, Switzerland]	Taken into account. Text is revised This sentence is deleted in the revision
18969	8	40		41	it would be helpful to say why. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised. This sentence is deleted in the revision
50771	8	41	8	42	Please specify by how long the climate system response lags behind deployment of CDR if possible. Also it would be helpful to briefly explain 'termination effects'. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54957	8	41	8	42	While we leave it to the authors to decide what findings deserve to be elevated to the Executive Summary, it would be of broad interest to include the statement from page 80 of Ch. 4 that "sea level rise will not be reversed by CDR on human timescales and land that is lost to sea level rise will not be retrieved (high confidence). This also adds additional perspective to the sentence about time lags in the climate response to CDR. [Nancy Hamzawi, Canada]	Accepted; Text is revised
26831	8	41	8	42	Some quantitative findings of 4.6 and Figure 4.40 might be reflected here, for example those related to the time lags for GSAT, SLR and Acrtic sea-ice. [Eric Brun, France]	Accepted; Text is revised
26833	8	41	8	42	It is unclear what "lag behind" means. Is it that hte effect will be measurable only after some time? [Eric Brun, France]	Taken into account. Yes, it refers to time lag. Text is revised.
9787	8	42	8	44	"Termination effects of CDR" is jargon - clarify. [Robert Kopp, United States of America]	Taken into account, text is revised.
50759	8	42	8	44	On page 79 rows 3-4 you include a very important point about CDRs and their carbon negativity. We would suggest this is elevated to to both the Executive Summary of Ch.4 and the SPM. Insert new sentence "Uncertainties over their overall lifecycle emission balance raises questions about their carbon negativity." (similar comment made in the SPM) [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Beyond the scope of this section. In the revision, this sentence in the main text is deleted in response to several other comments
50773	8	46	8	46	It would also be relevant to explain here that SRM would not tackle ocean acidification and also would likely impact weather, as well as climate (as mentioned) patterns. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text is revised. The biogeochemical implications of SRM are assessed in chapter 5 and hence we do not make a statement on ocean acidification here.
2363	8	46	8	47	Isn't it a given that any action that will diminish greenhouse gas induced warming will influence climate? I think this sentence needs to be reframed, perhaps as - "Solar radiation modification (SRM) can counteract greenhouse-gas-induced warming and is likely to influence climate at regional spatial scales and seasonal timescales in ways different from that due to greenhouse gases. [Vaishali Naik, United States of America]	Taken into account. Text is revised
132435	8	46	8	47	It seems that the key point to convey is that SRM "can diminish greenhouse-gas-induced warming at the global scale, but is unlikely to be able to reduce the effects of warming at regional scales or at seasonal timescales (high confidence)". [Kyle Armour, United States of America]	Taken into account. Text is revised
9673	8	46	8	48	Rephrase.We know for sure SRM impacts the climate, or we would not consider it. You probably mean it could have "climate impacts" that are different from simply masking the warming effect from GHG, or something like this. [Olivier Boucher, France]	Taken into account, Text is revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99017	8	46	8	49	Several problems here. First, rather than say "diminish", I think it would be better to say "offset" or "counter-balance". Second, I don't understand what the phrase "but is likely to impact climate at regional spatial scales and seasonal timescales"--what does this mean and what is the basis for saying this is important? Regarding the second sentence, what does the phrase "large uncertainties" mean--large compared to what? I'd suggest that the uncertainties associated with SRM, assuming it can be done, are likely less than the uncertainties associated with ongoing warming; after all, the objectiveand effect of SRM is to get back toward global weather conditions and a climate that is in the domain of where we have observational experience and scientific insight whereas going forward to warming of a few degrees more is totally beyond our experience and knowledge (how ice sheets will respond, what extreme weather will be like, etc.); I just do not see how the conclusion is arrived at. If the text is going to say "large" , then context has to be provided and based not just that there have not been enough studies, but that somehow the reprsentations of their process is highly uncertain. Now it may be that we don't know if marine cloud brightening can be done, but if it can, hard to see how uncertainties will be larger than those moving into the future. Not prividing context has been a very common shortcoming of virtually all the assessments that are made, sort of a way of demagouging SRM and this needs to stop. The question is to be evaluated is whether a world with ongoing emissions per one of the emissions scenarios without SRM would be more or less impactful than a world with each of the emissions scenarios and with SRM--and the answer may be different for the different scenarios. The blanket statements here simply do not do justice to what is a very serious issue that policymakers will likely be facing given how slow the effort to reduce	Taken into account, Text is revised
26835	8	46	8	49	This statement should be rephrased because we know for sure that SRM impacts the climate. The point is probably that it could have "climate impacts" that are different from simply masking the warming effect from GHG, or something like this. [Eric Brun, France]	Taken into account, Text is revised
5775	8	46	8	53	This summary is well-written and captures the essence of SRM evidence. I only suggest that SRM can diminish GHG-induced *climate change*, as it could generally bring changes in precipitation, extreme T and P, and tropical cyclone intensity closer to pre-industrial levels as well. [Jesse Reynolds, United States of America]	Taken into account, text is revised
111815	8	46	8	53	It seems that these paragraphs have been written with Stratospheric Aerosol Injection in mind. It would be better to name it explicitly, and mybe also mention Marine Cloud Brightening (and some of its features) because it is quite likely that ongoing experiments in Australia will raise awareness for MCB [Oliver Geden, Germany]	Taken into account, Text is revised. We refer to aerosol based schemes which includes MCB
1823	8	46			Change "can" to "could." Since SRM is impossible today, as the technology does not exist, this statement has to be conditional. If SRM were possible, and could be implemented to produce an aerosol cloud with the desirable properties, then it could cool the climate. But saying that it can is much too strong at this stage. [Alan Robock, United States of America]	Taken into account. Text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41965	8	46			Climate impacts and uncertainties. Marine cloud brightening restores sea surface temperatures towards pre-industrial values. The climate impacts of restoration would tend to be more beneficial than the unbridled changes of not doing it. It is quite true that there are large uncertainties in important climate processes but it is hard to think of any useful technology that did not go through an early period of uncertainty. This argument is being used to block research which might have reduced uncertainty. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Agreed. Text is revised
9789	8	47	8	49	These are 'high confidence' statements about the presence of large uncertainties - I think a more appropriate use of the IPCC confidence language would be to state low confidence in understanding [Robert Kopp, United States of America]	Taken into account, Text is revised
68649	8	48	8	48	"large uncertainties in important climate processes associated with SRM" is not clear. Do you mean processes that lead to climate impacts? [Simone Tilmes, United States of America]	Taken into account, Text is revised
68265	8	49	8	53	While SRM may be the only known way to cool the earth in just a few years (see excerpt from the Climate Science Special Report, Fourth National Climate Assessment (NCA4), Volume I (2017) ("14.3. The role of climate intervention in meeting ambitious climate targets SRM approaches offer the only known CI methods of cooling Earth within a few years after inception. An important limitation of SRM is that it would not address damage to ocean ecosystems from increasing ocean acidification due to continued CO2 uptake. SRM could theoretically have a significant global impact even if implemented by a small number of nations, and by nations that are not also the major emitters of GHGs; this could be viewed either as a benefit or risk of SRM."), cutting SLCPs can cut the rate of warming in half and Arctic warming by two-thirds within a few decades (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.). [Durwood Zaelke, United States of America]	Taken into account, text is revised. The biogeochemical implications of SRM are assessed in chapter 5 and hence we do not make a statement on ocean acidification. The discussion of SLCP is not within the scope of SRM discussion
132191	8	51	8	52	There is a high amount of literature on termination effects, why set the first part of the sentence only at "high confidence" and not "likely" or even "extremely likely"? The sudden discontinuation of SRM is by no means an unlikely scenario since SRM requires continuous action (e.g. if the countries responsible for SRM emissions would suddenlty become disfunctional or the SRM infrastructure would be destroyed for some reason, it would take substantial time before SRM could be initiated again). If such a discontinuation were to happen, it is certainly "extremely likely" that temperature would rise suddenly leading to major damages to ecosystems and people (e.g. Trisos et al. 2018, Nat Ecol. Evol.). [Sonia Seneviratne, Switzerland]	Agreed. The text is revised. It is to be noted that our "confidence" assessment in this WG1 report is based climate system response to an abrupt termination, and not on scenario based international governance. Our assessment is of course policy relevant
2365	8	51	8	53	this needs to be updated to say what the AR6 assessed. [Vaishali Naik, United States of America]	Taken into account. The assessment is that a slow ramp down of SRM is likely to minimize the termination shock.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132195	8	51	8	53	Split the sentence in two. First sentence would only address the risks of SRM termination and would be set at "extremely likely". The 2nd sentence would address the possible "gradual phase out scenario" but also mention the difficulty of implementing such a phase out; here is a suggestion: "It is extremely likely that a sudden and sustained termination of SRM would cause a rapid increase in temperature. A gradual phase out of SRM combined with mitigation and CDR is more likely than not to avoid large rates of warming, however the governance required to optimize the SRM phase out together with a choice suitable mitigation and CDR options is likely to be difficult to implement": [Sonia Seneviratne, Switzerland]	Taken into account. Text is revised. However, we do not make any statement on governance which is assessed in WG3.
132193	8	52	8	53	On the other hand, the 2nd half of the sentence seems too confident on a possible management of SRM. It is possible that "a gradual phase out of SRM combined with mitigation and CDR is likely to avoid large rates of warming", but how likely is it that the international community could agree on how to peform this joint "gradual phase out of SRM combined with mitigation and CDR"? [Sonia Seneviratne, Switzerland]	Taken into account. The assessment of whether international community would agree is beyond the scope of the assessment here.
50761	8	53	8	53	In the glossary, "mitigation" is defined as reduction of emissions or enhancement of sinks, so here it would be better to replace "mitigation" with "emissions reduction". [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
41393	8	55	9	23	There is no quantitative information provided in this post-2100 climate change projection section of the ES. An entire section 4.7 is supposed to provide long-term projection information in the chapter. The ES even starts with the statement that 2300 infornation will be provided. None of this has materialised, not a single 2300 estimate can be found in the ES. Long-term information, for example on sea-level rise (commitments), is of critical importance for vulnerable country groups like SIDS and will be requested (also because research is published and available). Please thoroughly revise the long-term, post-2100 section of this ES as well as section 4.7. [Alexander Nauels, Germany]	accepted. More information on this timescale is now provided. SLR is covered in chapter 9
89915	8	55			Being aware that climate change will not cease to be a problem beyond 2100 and given that no matter what the future emission trends in the CO2 emissions is likely to be, the one thing that remains certain is that the impacts from CO2 now and in the future will remain in the climate system for well beyond 2100. Therefore, it is important that CH4 provide information additional relevant information on long-lasting sea level rise due to committed climate change, and impacts beyond 2100. [Joanne Deoraj, Trinidad and Tobago]	taken into account. Sea-level rise projections on this timescale are covered in chapter 9
21621	8	55			I would have expected in this segment of the ES to see statements on long-term SLR commitment and long-term ice-sheet commitment but maybe these are covered in chapter 9 instead? Regardless, for the SYR one or both of the chapters need to cover this in their ES. [Peter Thorne, Ireland]	taken into account. Sea-level rise projections on this timescale are covered in chapter 9

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100009	8	55			The topic of climate change commitment and long-term changes beyond 2100 is of fundamental importance to SIDS, in particular when it comes to the slow but very long-lasting SLR response. Chapter 4 lacks important commitment/long-term information in the ES, also because available information from process-chapters is not absorbed by the responsible author team (e.g. see very sparse content in section 4.7). Please coordinate with other chapters to strengthen the global climate change commitment and long-term change assessment. [Caroline Eugene, Saint Lucia]	taken into account. Sea-level rise projections on this timescale are covered in chapter 9
84149	8	55			The topic of climate change commitment and long-term changes beyond 2100 is of fundamental importance to SIDS, in particular when it comes to the slow but very long-lasting SLR response. Chapter 4 lacks important commitment/long-term information in the Executive Summary. Please coordinate with other chapters to strengthen the global climate change commitment and long-term change assessment. [Jeffers Cheryl , Saint Kitts and Nevis]	taken into account. Sea-level rise projections on this timescale are covered in chapter 9
11083	8	L2	8	L3	It is questionable to say "internal variability include the observational range of uncertainty", which considers the observational range of uncertainty as part of internal variability. [Wen Wang, China]	Taken into account. The original wording was unclear. The word "includes" has now been changed to "is within".
116289	8		8		The notion of "viable" CDR approaches needs to be defined, consistently with outcomes of SR15 or SRCCL. [Valerie Masson-Delmotte, France]	Taken into account. Text clarified
116291	8		8		A reference to SR15 is also possible regarding the assessment of SRM. [Valerie Masson-Delmotte, France]	Taken into account. Text clarified
16659	9	2	9	2	Since it is expected to be around zero, rather than saying there is low confidence in the sign of ZEC, it would be more positive to phrase it in terms of what we do know. E.g. "The sign is not known, but the magnitude of ZEC is likely less than 0.18 deg {4.7.2.2.1} with medium confidence.". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	accepted. This statement has been revised to stress what we do know and the implications/usage of ZEC
9791	9	2	9	6	It'd be nice to have an assessed likelihood statement about the ZEC, rather than just saying there is low confidence in what actually seems to be a fairly narrow range. (Low confidence in the sign seems justified; low confidence in the magnitude seems contradicted by the cited range) [Robert Kopp, United States of America]	accepted. This statement has been revised to stress what we do know and the implications/usage of ZEC
132437	9	2	9	6	Does the Zero Emissions Commitment quantify warming after all emissions cease, or just CO2 emmisions. Perhaps clarify, or better yet discuss both possible interpreteations. [Kyle Armour, United States of America]	accepted. Just co2. text has been clarified
54959	9	2	9	6	From previous assessments, the main message to Policymakers from zero emission commitment experiments has been that global temperature remains at about peak levels for millennia. It would be helpful to confirm that this is still true before articulating details about small increases or decreases in global temperature in the near-term following a zeroing of emissions. [Nancy Hamzawi, Canada]	taken into account. Assessment of near term is of particular relevance now due to the use of ZEC in assessing remaining carbon budgets

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18971	9	2		6	It would be helpful to say that a commitment larger than 0.x can be excluded. Rather than saying magnitude is completely uncertain. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	accepted. This statement has been revised to stress what we do know and the implications/usage of ZEC
99019	9	3	9	3	It need to be clarified if it is just the ceasing of all CO2 emissions or is the casing of all relevant GHG and aerosol emissions. And is this just fossil fuel emissions of CO2 that cease or also the ceasing of emissions from destruction of the biosphere, and also cut off of CO2 (and CH4) emissions from permafrost thawing and of clathrates. It is just not clear what is being cut off and what is meant by "Zero Emissions"--what about ongoing land cover change, etc. a full explanation is needed. [Michael MacCracken, United States of America]	taken into account. Just CO2. That is made clear here now. These other clarifications are important, but not in the ES. See section 4.7
10035	9	4	9	5	Update numbers to ZECMIP final results [Andrew MacDougall, Canada]	accepted. FGD has been updated with available model results
111817	9	8	9	8	Better to use "threshold" instead of "goal", because 2C is not a temperature goas in the context of the Paris Agreement (it's part of a goal range of 1.5-2C) [Oliver Geden, Germany]	taken into account. Phrasing has been clarified.
106277	9	8	9	10	This is imprecise. Some scenarios do indeed include a peak and decline in atmospheric CO2 concentrations in order to peak and decline global mean temperatures. However, this statement is too generalizing. A scenario that reaches net zero CO2 emissions, equally intends to peak and decline atmospheric CO2 concentrations, but aims at the same time to stabilize warming instead of temporarily overshooting it. Please correct. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text has been clarified
45463	9	8	9	14	I think that without telling the reader in which year the CO2 peak occurs in SSP5-3.4-OS, this paragraph does not convey much useful information. [Leonard Borchert, France]	taken into account. Limited detail can be provided in the ES. These details are clearly explained in the text

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68267	9	8	9	14	Any overshoot will cause some irreversible impacts, including SLR and glacial and ice sheet melt that will not be reversed when the overshoot is corrected. Tokarska K. B., et al. (2019) Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an Overshoot, EARTH'S FUTURE 7:1283–1295, 1283 ("Emission pathways that are consistent with meeting the Paris Agreement goal of holding global mean temperature rise well below 2 °C often assume a temperature overshoot. In such overshoot scenarios, a given temperature limit is first exceeded and later returned to, under the assumption of large-scale deliberate carbon dioxide removal from the atmosphere. Here we show that although such strategy might result in a reversal of global mean temperature, the carbon cycle exhibits path dependence. After an overshoot, more carbon is stored in the ocean and less on land compared to a scenario with the same cumulative CO2 emissions but no overshoot. The near-path independence of surface air temperature arises despite the path dependence in the carbon cycle, as it is offset by path dependence in the thermal response of the ocean. Such behavior has important implications for carbon budgets (i.e. the total amount of CO2 emissions consistent with holding warming to a given level), which do not differ much among scenarios that entail different levels of overshoot. Therefore, the concept of a carbon budget remains robust for scenarios with low levels of overshoot (up to 300 Pg C overshoot considered here) but should be used with caution for higher levels of overshoot, particularly for limiting the environmental change in dimensions other than global mean temperature rise."); Solomon S., et al. (2010) Persistence of climate changes due to a range of greenhouse gases, PROC. NAT'L. ACAD. SCI. 107(43):18354–18359, 18356 ("The transfer of heat from the atmosphere to the ocean's mixed layer (top 100	taken into account. SSP534 is one example for which we have quantitative results

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68269	9	8	9	14	Speed is a key metric, and climate solutions must be measured along this dimension as well as along the conventional metrics. It is important how quickly a climate solution can deliver avoided warming. Molina M., et al. (2009) Reducing abrupt climate change risk using the Montreal Protocol and other regulatory actions to complement cuts in CO2 emissions, PROC. NAT'L. ACAD. SCI. 106(49):20616–20621. Because of their short lifetimes (days to a decade and a half), SLCPs can provide fast mitigation, avoiding warming at 2050 of up to 0.6 ºC, while cutting CO2 can avoid between 0.1–0.3 ºC; at 2100, SLCPs avoid 1.2 ºC warming and CO2 avoids 1.6–1.9 ºC. SLCP reductions are critical for vulnerable areas like the Arctic and because they can slow progression of tipping points and self-reinforcing feedbacks. See 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; Ramanathan and Xu (2010) The Copenhagen Accord for limiting global warming: Criteria, constraints, and available avenues, Proc. Natl. Acad. Sci. 107(18):8055–8062; Ramanathan and Feng (2008) On avoiding dangerous anthropogenic interference with the climate system: Formidable challenges ahead, Proc. Natl. Acad. Sci. 105(38):14245–14250; Report of the Committee to Prevent Extreme Climate Change (Co-Chairs: Ramanathan V., Molina M. L., and Zaelke D.; Authors: Alex K., Auffhammer M., Bledsoe P., Borgford-Parnell N., Collins W., Croes B., Forman F., Gustafsson Ö., Haines A., Harnish R. Jacobson M. Z., King S., Lawrence M., Leloup D., Lenton T., Morehouse T., Munk W., Picolotti R., Prather K. Raga G. B., Rignot E., Shindell D., Singh A. K., Steiner A., Thiemens M., Titley D. W., Tucker M. E., Tripathi S., Victor D., & Xu Y.) (2017) Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change. [Durwood Zaelke,	rejected. Speed of achieving scenarios is better assessed in WG3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66753	9	8	9	14	Any overshoot will cause some irreversible impacts, including SLR and glacial and ice sheet melt that will not be corrected when the overshoot is corrected. Tokarska K. B., et al. (2019) Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an Overshoot, EARTH'S FUTURE 7:1283–1295, 1283 ("Emission pathways that are consistent with meeting the Paris Agreement goal of holding global mean temperature rise well below 2 °C often assume a temperature overshoot. In such overshoot scenarios, a given temperature limit is first exceeded and later returned to, under the assumption of large-scale deliberate carbon dioxide removal from the atmosphere. Here we show that although such strategy might result in a reversal of global mean temperature, the carbon cycle exhibits path dependence. After an overshoot, more carbon is stored in the ocean and less on land compared to a scenario with the same cumulative CO2 emissions but no overshoot. The near-path independence of surface air temperature arises despite the path dependence in the carbon cycle, as it is offset by path dependence in the thermal response of the ocean. Such behavior has important implications for carbon budgets (i.e. the total amount of CO2 emissions consistent with holding warming to a given level), which do not differ much among scenarios that entail different levels of overshoot. Therefore, the concept of a carbon budget remains robust for scenarios with low levels of overshoot (up to 300 Pg C overshoot considered here) but should be used with caution for higher levels of overshoot, particularly for limiting the environmental change in dimensions other than global mean temperature rise."); Solomon S., et al. (2010) Persistence of climate changes due to a range of greenhouse gases, PROC. NAT'L. ACAD. SCI. 107(43):18354–18359, 18356 ("The transfer of heat from the atmosphere to the ocean's mixed layer (top 100	taken into account. SSP534 is one example for which we have quantitative results

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66755	9	8	9	14	Speed is a key metric, and climate solutions must be measured along this dimension as well as along the conventional metrics. The question that needs to be answered is how quickly a climate solution can deliver avoided warming. Molina M., et al. (2009) Reducing abrupt climate change risk using the Montreal Protocol and other regulatory actions to complement cuts in CO2 emissions, PROC. NAT'L. ACAD. SCI. 106(49):20616–20621. It is important to note that SLCPs are a critical part of that solution, and that cutting them can avoid warming at 2050 of up to 0.6 ºC, while cutting CO2 can avoid between 0.1–0.3 ºC; at 2100, SLCPs avoid 1.2 ºC warming and CO2 avoids 1.6–1.9 ºC. SLCP reductions are critical for vulnerable areas like the Arctic and because they can slow progression of tipping points and self-reinforcing feedbacks. See 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; Ramanathan and Xu (2010) The Copenhagen Accord for limiting global warming: Criteria, constraints, and available avenues, Proc. Natl. Acad. Sci. 107(18):8055–8062; Ramanathan and Feng (2008) On avoiding dangerous anthropogenic interference with the climate system: Formidable challenges ahead, Proc. Natl. Acad. Sci. 105(38):14245–14250; Ramanathan, Molina, and Zaelke (2017) Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change. [Kristin Campbell, United States of America]	rejected. Speed of achieving scenarios is better assessed in WG3
99021	9	9	9	12	This is not clear and needs to be clarified. What does "fully reverse" mean--back down to 2 C, 1.5 C, 0.5 C, what? There is an actual difference of views (or at least a real difference in how the decline is stated) with regard to what happens to the CO2 concentration after emissions stop--some argue there will be a relatively quick drop by 75 ppm or so because terrestrial and oceanic uptake rates (including transfer to the deep ocean) will continue for some time while others suggest that those fluxes are driven by the gradients created by just the last few year's emissions and so the atmospheric CO2 level will persist near its maximum level. This point needs some develpment to clear up this pont. And it needs to be added that the increased loadings due to emissions of short-lived species will drop back quickly to, presumably, near preindustrial levels. The finding is about a key point and a fullerand clearer explanation is needed. [Michael MacCracken, United States of America]	taken into account. Evolution of CO2 post cessation is covered in ZEC analysis and reductions do persist.
96385	9	11			It may be helpful to include the specific abbreviation SSP5-3.4-OS in the glossary as the "overshoot"-scenarios have not been specified yet in the glossary (page AG-36, line 47 to page AG-37, line 6). [Nicole Wilke, Germany]	accepted. Yes this is a useful suggestion
9793	9	12	9	13	"Global sea level" -> "global mean sea level" [Robert Kopp, United States of America]	accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35093	9	16	9	18	This ES statement is too technical, whereas one describing increasing confidence in irreversibility would be very welcome in the TS section on tipping points. [Baylor Fox-Kemper, United States of America]	accepted. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there.
54961	9	16	9	23	Many of the messages in this final paragraph are confusing especially given the emphasis on reversibility vs irreversibility: 1. previous assessments have emphasized the irreversibilty of the climate change commitment, not the reversibility of it. Can "reversbility' in sentence 1 be changed to 'irrversibility"?, 2. Can "reduction in uncertainty" (about the need for net-zero CO2 emissions for temperature stabilization be rewritten as "strengthening of the confidence"?, 3. The third sentence is very unclear...particularly in light of the last sentence which confirms centennial scale irreversibility of ocean warming. What is the time scale for sea surface temperature recovery that would then enable recovery of other climate system elements? While scientifically, recovery of some climate system elements is important to know about, the conditions and timescales for such recovery should be made more explicit. [Nancy Hamzawi, Canada]	accepted. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there. Description of ZEC is improved in the 1st paragraph in this ES section (beyond 2100)
89841	9	16	9	23	This paragraph is very confusing for someone not already familiar with the literature. Please state more straightforwardly for a specified timescale (or two) which changes are assessed to be reversible and which are assessed to be irreversible. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	accepted. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there.
9675	9	18	9	19	I see a contradiction between this sentence and the sentence on lines 4-5 of the same page. If the ZEC can be as low as -0.4°C (ie a cooling) then how can we be certain that we need net-zero CO2 emissions for temperature stability? Isn't it possible to have a little bit of residual CO2 emissions while maintaining temperature stability? see eg work by Tanaka and O'Neill (Nature climate change, 2018) that says so. [Olivier Boucher, France]	taken into account. Implications of ZEC are better described. Central estimates of ZEC still taken as zero, and used in chapter 5 for remaining carbon budget assessment. Model uncertainty spans zero, so we could still see a slight cooling, or slight warming following emissions ceasing.
26837	9	18	9	19	There seems to be a contradiction between this sentence and the sentence on lines 4-5 of the same page. If the ZEC can be as low as -0.4°C (ie a cooling) then how can we be certain that we need net-zero CO2 emissions for temperature stability? Isn't it possible to have a little bit of residual CO2 emissions while maintaining temperature stability? see eg work by Tanaka and O'Neill (Nature climate change, 2018) that says so. [Eric Brun, France]	taken into account. Implications of ZEC are better described. Central estimates of ZEC still taken as zero, and used in chapter 5 for remaining carbon budget assessment. Model uncertainty spans zero, so we could still see a slight cooling, or slight warming following emissions ceasing.
106279	9	19	9	21	Please clarify what is meant by "multi-decadal reversibility". That something can be reversed for multiple decades, after an overshoot of multiple decades, within multiple decades… etc. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	accepted. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99023	9	19	9	21	And also the sentence on lines 22-23. It needs to be stated much more clearly that there is no indication that loss of mass from the ice sheets is not reversible over quite long periods. Paleoclimatic data make clear that it taks roughly an order of magnitude longer than ice sheet deterioration, and this needs to be mentioned. [Michael MacCracken, United States of America]	taken into account. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there.
99025	9	21	9	22	It would be helpful to provide an indication of the time scale for the potential recovery. [Michael MacCracken, United States of America]	taken into account. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there.
50775	9	22	9	22	even where temperature stabilizes' - it would be useful to make clear here if this is a temperature level higher than present day levels and if so, what level seems to be the threshold for AMOC recovery. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there.
84227	9	22	9	23	"is further substantiated" by what? [Annalisa Cherchi, Italy]	taken into account. This ES section has been thoroughly revised for clarity of reading. We removed AMOC, sea-level and ice-sheet collapse assessment from section 4.7 as they are covered much more comprehensively in chapter 9, so leave discussion there.
71317	9	43	10	5	Some explanation should be included for "enhanced information on internal variability". [Kenji Taniguchi, Japan]	Noted.
116293	9		9		Please report changes to the assessment of (ir)reversibility compared to AR5 and SR (especially SROCC, ch 6). [Valerie Masson-Delmotte, France]	taken into account. Assessment has been updated and synthesised across WG1 chapter
18975	10	1	11	47	It'd be helpful to discuss the role of literature assessment vs direct model assessment in this chapter. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Noted. As per standard IPCC procedures, all assessment performed here is based on the published literature. This sometimes includes applying published methods to a new model dataset. No change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18339	10	1	11	47	Good to see the discussion on internal variability. However, since current models are unable to predict historical or future evolution of the internal climate variations in the real world, the only thing we can really predict is the externally-forced climate changes in the 20th or 21st century, and that is often the focus of most climate change simulations, such as the CMIP historical and 21st century climate simulations as these simulations started from raondom initial conditions (and thus were not intended to reproduce observed or future internal climate variaitions). In other words, all the CMIP climate simulations are designed only for predicting externally-forced climate changes (when averaged over a large number of ensemble runs), not for predicting the interanl climate evolution in the real world. For that purpose (of predicting forced changes), the uncertainty associated with the internal variability is not an issue, as we can smooth it out by averaging over a large number (>100) of ensemble runs. The internal variability is an issue only if we are trying to predict the actual climate state for a future time period, but that is clearly not the goal of all the CMIP model simulations (as they started from random initial conditions and thus are not designed to predict the evolution of the intenral variiability in the real world). Thus, it would be helpful to make these points clear in section 4.1: 1) All CMIP climate simulations are not designed to reproduce the evolution of the observed or future internal climate variations; 2) the CMIP model simulations need to be averaged over a large number (>100) of ensemble simulations to represent the forced climate change; 3) the spread due to internal variability in CMIP model simulations represents a source of uncerntainty only for predicting future climate states over specific future time periods (for which current models are unable to do), but not for predicting future forced climate change. Also, it is unclear whether the scope of this	Rejected. The comment substantially overstates the issue. While it is true that simulations not initialized from the observed state cannot predict the evolution of internal variability, the multiple representations of internal variability contain a wealth of information, such as how close to observations a simulations should be to be considered consistent with observations (e.g., Notz, Phil. Trans., 2015). Simply averaging over all available realizations of different models confounds internal variability with model differences. The chapter has already in its SOD taken a sophisticated viewpoint on how to avoid this confounding.
18981	10	1	11	50	Warming levels seem not to be assessed at all. It'd be worth at least to highlight how they relate to the scenarios. Give background to what is in the SPM now. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Cross-chapter Box on global warming level in Ch11 explains the point.
21625	10	1			In contravention to most other chapters no graphical table of contents accompanies this chapter's introduction. [Peter Thorne, Ireland]	Accepted. Visual abstract has been added in FGD.
71209	10	3	10	9	This paragragh needs to be written in the past tense instead of future tense. E.g Words like "this chapter will assess" should be written like this chapter assessed ..... [Michael Mugarura, Germany]	Taken into account. Future tense replaced by present tense. Past tense would be wholly inappropriate.
106855	10	3			Chpater 4 is also assessing extensively the MOVs and teleconnections. I think that both should be specifiec/mentionned in this first paragraph. [Christophe CASSOU, France]	Accepted.
21623	10	8	10	8	This is cross-chapter box 2.2 [Peter Thorne, Ireland]	Accepted
18973	10	8		9	why only a subset? [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Other elements are covered in later chapters.
114435	10	13	10	15	You may add a reference to ch1 (Section 1.6) as well as WGIII here. [Jan Fuglestvedt, Norway]	Accepted.
19223	10	17	10	19	Add a reference to chapter 1, section 1.5.3, where GCMs and ESMs are introduced? [Anne-Marie Treguier, France]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114437	10	22	10	24	I suggest adding a ref to section 1.6 here and also checking for consistency. [Jan Fuglestvedt, Norway]	Taken into account; cross-reference to Chapter 1, Section 1.4.3, added.
106857	10	22			I am repeating here a comment I have made for the entire report. I don't understand why the "terminoloy" initial-condition large ensemble" is used in the entire report because those ensembles are just lots of realizations of classical historical/ssp runs. Nothing "new" but only "better". "Large ensemble" stands by itself. In addition, this term is very confusing because it is too close to "initialized simulations" referring to decadal forecast. Even if the "initial-condition large ensemble" term as been used in papers, I would defintely not use it in AR6. [Christophe CASSOU, France]	Rejected. The terminology has become standard use in the published literature, and it is that which IPCC should follow.
79683	10	23	10	25	Add Lehner et al. 2020 : Lehner, F., Deser, C., Maher, N., Marotzke, J., Fischer, E. M., Brunner, L., Knutti, R., and Hawkins, E.: Partitioning climate projection uncertainty with multiple large ensembles and CMIP5/6, Earth Syst. Dynam., 11, 491–508, https://doi.org/10.5194/esd-11-491-2020, 2020. [Laurent Terray, France]	Taken into account. Is it not this reference, though, that should be added but Deser et al. (2020) as the overview of multiple SMILEs.
6647	10	23	10	25	The sentence that spans these lines omits mention of the uncertainty that arises from externally-forced variability, such as arises in particular from volcanic eruptions. It could also though be noted somewhere that a volcano that erupts just before a prediction is made can be a source of predictability, if volcanic aerosol is well initialised and forecast. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Future forced variability now included.
2141	10	28	10	28	Real time multimodel decadal predictions were first produced in Smith et al GRL 2013 [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, reference added.
79685	10	38	10	38	I agree but the "precisely" is strongly depending on the ability of models representing IV correctly (which is not really known and is a knowledge gap in chapter 10) [Laurent Terray, France]	Accepted and rephrased.
26839	10	38	10	38	"precisely" might not be the right word. "Can be better estimated" would be more appropriate. Also even though the large ensemble are very interesting and provide new information, they do not sample all the scale of variability that can affect the climate trajectory. So it s not a diagnosis of the internal variability, but an estimation of it (I.E statistical approach) [Eric Brun, France]	Accepted and rephrased.
127427	10	39			The CESM-LE in Kay et al, 2015 only sampled IC uncertainty in the atmosphere, without changing the state of the ocean. Given that the ocean state is not known with precision, the resulting range of variability will be an under-prediction of reality. [Trigg Talley, United States of America]	Noted.
50777	10	41	10	41	near term' - while this along with mid-term and long-term are defined in Box SPM2 and the Glossary, it would also be very helpful to reiterate these definitions at the start of this chapter which focuses on projections and near term climate information. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Near term is already defined in Section 4.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6649	10	41	10	41	"including the one that will unfold in reality" is incorrect wording. What will unfold in reality will not be one of the realizations in a large initial-condition ensemble. It may be something similar to one or more of the realizations, but will not be identical to one of them. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The text does not imply the actual realization is simulated in the ensemble.
2143	10	43	10	43	While the internal variability is now recognised it has also now been shown that it is too large a proportion of the variance in climate models in some regions. See Eade et al GRL 2014, Scaife and Smith, Clim. Atm Sci. 2018, Smith et al Clim. Atm. Sci. 2019 for this very important caveat. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Noted. While the quantification of the predictable part of variability is an important topic, it is too specialized to be brought up here in the introduction.
96387	10	46	10	53	It is surprising that this paragraph only refers to the AR5 and seems to ignore the SR1.5 that also addressed CDR and SRM. Please refer also to the SR1.5. [Nicole Wilke, Germany]	Taken into account; sentence reworded. Note that the SR1.5 did not assess these particular aspects dealt with in this paragraph.
50779	10	47	10	47	Typo: 'CDR' not 'CRD' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
127429	10	47	10	47	Acronym is misspelled (CRD vs CDR). [Trigg Talley, United States of America]	Accepted.
127431	10	48	10	48	SRM is used in a new way (M for "modification" rather than "management"). Should be noted here so it can be connected to previous literature. [Trigg Talley, United States of America]	Noted. This is properly discussed in the section.
114439	10	50	10	51	You may separate more clearly between emergence of a human warming signal and the signal of mitigation efforts. [Jan Fuglestvedt, Norway]	Taken into account. We have reduced the reference list so that they only refer to the mitigation benefits.
71127	10	50	10	51	Another relevant study that could be cited here is Ciavarella et al. (2017): Ciavarella, A., Stott, P. & Lowe, J.: Early benefits of mitigation in risk of regional climate extremes. Nat. Clim. Change 7, 326–330 (2017). [Andrew King, Australia]	Accepted. Inserted.
50781	10	51	10	51	assumed mitigation' - does mitigation here mean GHG emissions reduction or mitigation of warming through emissions or warming reduction from SRM or CDR? Please specify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Mitigation replaced by emissions reductions.
55497	10	52	53		Spring et al., submitted (included in TEXT). Check publication date [Maria del Pilar Bueno Rubial, Argentina]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
116295	10		10		I suggest a cautious use of the term "plausible" when describing scenarios (possible?) given current debates on the plausibility of RCP8.5 (to coordinate x WG). [Valerie Masson-Delmotte, France]	Taken into account. Plausible replaced by possible.
114441	11	1	11	4	You can also say that this chapter provides input to WGIII as well as WGII (but please check how much direct use there is in WGII, to be sure) [Jan Fuglestvedt, Norway]	Taken into account; WGIII added.
50783	11	2	11	2	Suggest it would be helpful here to make the point that magnitudes of warming are typically known after averaging over observations over 20 years. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This would be too much detail for an introduction; furthermore, the chapter also covers 1.5°C of warming being exceeded in single years.
96389	11	3			Please include "global" within "Special Report on 1.5 °C global warming" [Nicole Wilke, Germany]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19827	11	9	11	13	P11 L9-13: while e.g. GSAT, SIA, are indicators, the situation is different for e.g. AMOC. Concerning AMOC, monsoon, NAM/SAM, ENSO, the indicators can only be indexes for these phenomena. [philippe waldteufel, France]	Rejected. Indices for global phenomena can be viewed as indicators for large-scale changes.
50785	11	10	11	10	Is there a reason for why global ocean precipitation is not included in the list of assessed global climate indicatiors for this chanpter? It would helpful to explain if so. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Our focus is on land rather than global mean or global ocean precipitation because land precipitation has greater societal relevance.
114443	11	13	11	13	you may insert "changes in" before "GSAT" [Jan Fuglestvedt, Norway]	Accepted
96391	11	16	11	24	Please explain why the IPCC has chosen periods as short as 20 years for an assessment of climate change. This seems inconsistent with the WMO-definition of climate, i.e. 30 years at least, please see https://library.wmo.int/doc_num.php?explnum_id=4166, P 11, Ch 2, paragraph 3, last sentence "...the period from 1961 to 1990 has been retained as a standard reference period for long-term climate change assessments." Such an explanation might be useful for the glossary as well, e.g. under reference periods. [Nicole Wilke, Germany]	Using 20-year periods goes back to AR4 and acknowledges that 30-year averages of the recent past would average over substantially changing states. Twenty years are hence an accepted compromise between resolution and noise.
106859	11	18	11	18	Cross-Reference to Cross-Chapter Box 2.2 and Technical Annex shoud le added for the MoVs . "important" is too vague. I would say "… and the selection of modes of variability and associated teleconnection assessed across the entire report (X-Chapter Box 2,2 and TA"). [Christophe CASSOU, France]	Accepted
19829	11	23	11	27	The reader may be assumed to know already about the 3 selected periods for AR6/WG1. They are defined in the Box SPM.2, Table TS.4, cross-section box 1, and repeated several more times prior to chapter 4. [philippe waldteufel, France]	Noted.
111887	11	26	11	26	“it is during the mid-term that differences between .. scenarios ..emerge against internal Variability” .. wonder if it would it be appropriate to add: “that also led selecting the internal variability cut-off to 20 years for the near-term“ (-mentioned at line 16, pg 11) [Mihaela Caian, Romania]	Noted. Gist of comment unclear.
114445	11	34	11	34	Insert "climate" before "implications" to distinguish from WGII and III [Jan Fuglestvedt, Norway]	Accepted
114447	11	37	11	37	I think TSU would like to avoid using "target" and rather use "goal" [Jan Fuglestvedt, Norway]	Accepted
127433	11	37	11	37	There are no targets specific in the Paris Agreement, but rather a long-term temperature goal. Please use language actually stated in the decision text, and be consistent with how the WGI and specific chapters discuss the temperature goal (i.e., page 4-49, line 20, "global temperature goal"). [Trigg Talley, United States of America]	Accepted
2901	11	50	13	45	It should mention how well the CMIP6 simulated the key features of the 20th century and provide a synthesis table. [Zong Ci Zhao, China]	rejected. This is covered extensively in chapter 3 and we point that out

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18341	11	52	13	45	I think it would be helpful to make it clear in section 4.2 that all the current projections of future climte change often focus on the externally-forced response to future GHG emissions and other man-made changes. They are not intended to project the actual climate state for a future time period that would include internal climate variations, as current climate models are unabble to predict future evolutions of the unforced internal climate variations. Furthermore, many studies have failed to recognize that their difference maps between a future 20-30year period and a current 20-30year period may contain both forced response and random internal variations as they are often derived from a small number (<50) of ensemble runs, yet such difference maps are often incorrectly interpreted as purely due to the response to future external forcing. This is especially problematic for local and regional preciptiation change which contains large interal variability (Dai and Bloecker 2019), and in regional climate downscaling that is often based on a few ensemble runs as discussed in Dai et al. (2017). Refs cited: Dai, A., R.M. Rasmussen, K. Ikeda, and C. Liu, 2017: A new approach to construct representative future forcing data for dynamic downscaling. Climate Dynamics, DOI: 10.1007/s00382-017-3708-8 Dai, A., and C.E. Bloecker, 2019: Impacts of internal variability on temperature and precipitation trends in large ensemble simulations by two climate models. Climate Dynamics, 52, 289–306. https://doi.org/10.1007/s00382-018-4132-4. [Aiguo Dai, United States of America]	taken into account. This chapter assess initialised predictions as well as projections. Text clearly discusses sources of uncertainty
12195	11	52	23	45	Why is not most of this section placed near the beginning of Ch.3, rather than here? Most of the caveats apply to the historical runs as well. [Bryan Weare, United States of America]	rejected. We need enough detail here for chapter 4 to stand-alone
11269	11	55	11	55	AOGCM has been defined on p.10 l.17 [Masahiro Watanabe, Japan]	accepted. Text has been tidied accordingly
116299	11		11		A visual abstract of the chapter and links to other chapters would be really helpful. What about introducing longer term (beyond 2100) as well? [Valerie Masson-Delmotte, France]	Accepted. A visual abstract is added and introduction of long-term is added.
68651	12	2	12	2	remove "here" [Simone Tilmes, United States of America]	accepted. Done
84229	12	9	12	9	the word done is misspelled [Annalisa Cherchi, Italy]	accepted. Done
102927	12	9	12	9	dome → done [Philippe Tulkens, Belgium]	accepted. Done
79687	12	9	12	9	dome -> done [Laurent Terray, France]	accepted. Done
132439	12	9	12	9	"dome" should be "done". I expect you got > 300 comments for this one typo. [Kyle Armour, United States of America]	accepted. Done. Mercifully only 12 comments on this :-)
71267	12	9	12	9	typo? (dome) [Kenji Taniguchi, Japan]	accepted. Done
50787	12	9	12	9	Typo: dome' > 'done' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Done
79499	12	9	12	9	has been done(comment by: sahar.maleki@ut.ac.ir) [Hanieh Zargarlellahi, Iran]	accepted. Done
32691	12	9	12	9	has been done [sadegh zeyaeyan, Iran]	accepted. Done
87537	12	9	12	9	dome → done [Valentina Roberta Barletta, Denmark]	accepted. Done
33021	12	9	12	9	has been done [Sahar Tajbakhsh Mosalman, Iran]	accepted. Done
127435	12	9			Typo. "dome" should be "done". [Trigg Talley, United States of America]	accepted. Done

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79689	12	11	12	11	I suggest to replace "true" by "observed" [Laurent Terray, France]	accepted. this was a quote from chapter 3, but has disappeared from the SOD (it was in the FOD). Text updated
45465	12	15	12	21	Only mentioning some of the MIPs shown in table 4.1 in the text seems odd. I'd prefer explaining all or none. [Leonard Borchert, France]	reject. This table is not an exhaustive list of MIPs, but lists the ones used in chapter 4 (and where/why they are used)
42941	12	15	12	21	Why is HighResMIP not included? Ocean bias is reduced in eddy permitting (0.25 degree) compared to eddy-parameterized ocean models (1 degree), that are used in CMIP6. Bock, L., A. Lauer, V. Eyring, M. Schlund, M. Barreiro, N. Bellouin, C. Jones, G. A. Meehl, V. Predoi, and M. J. Roberts, 2019: Quantifying progress across different CMIP phases with the ESMValTool. Submitted to J. Geophys. Res. [Rein Haarsma, Netherlands]	Taken into account. This table lists MIPs used in chapter 4 and has been updated accordingly.
45799	12	15	12	21	Future projections focusing on the attibution to changes in aerosols have also been run under DAMIP. Shouldn't these be included here? [Twan van Noije, Netherlands]	Taken into account. This table lists MIPs used in chapter 4 and has been updated accordingly.
18977	12	15		16	I'm very happy to reead that you also use 1850-1900 as a reference as that allows to assess warming levels as well as scenarios. It'd would be good to mention this in the scopeing seection above and explicitely address warming levels there. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text clarified - we point to 4.6.1 which assesses patterns of climate change at warming levels
71269	12	16	12	29	Various MIP is necessary? [Kenji Taniguchi, Japan]	reject. This table is not an exhaustive list of MIPs, but lists the ones used in chapter 4 (and where/why they are used)
50789	12	18	12	19	Why are C4MIP and LUMIP absent from Table 4.1? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Table has been updated. C4MIP is now used (emissions driven runs). LUMIP not currently used in this chapter
96393	12	19			Please verify: "LUMIP" is not mentioned in Table 4.1. [Nicole Wilke, Germany]	taken in to account. LUMIP is not used in this chapter
104609	12	24	12	29	The Decadal Climate Prediction Project (DCPP) is a CMIP6-endorsed MIP (https://www.wcrp-climate.org/modelling-wgcm-mip-catalogue/modelling-wgcm-cmip6-endorsed-mips) and is utilized in 4.4.1 as well as Box 4.1, Figure 1. Therefore it should be included in Table 4.1. (The reference is Boer et al., 2016.) [William Merryfield, Canada]	Accepted. DCPP is included in the Table 4.1 in the final publication.
106093	12	26	12	26	Why isn't DCPP mentioned in the table f.1? [Noel Keenlyside, Norway]	Accepted. DCPP is included in the Table 4.1 in the final publication.
24015	12	26	12	26	DCPP experiments are also analysed in this chapter [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. DCPP is included in the Table 4.1 in the final publication.
106861	12	26	12	28	DCPP is missing in the table. [Christophe CASSOU, France]	Accepted. DCPP is included in the Table 4.1 in the final publication.
79663	12	26	12	29	Table 4.1: surely DCPP needs to be in this table? [Hannah Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. DCPP is included in the Table 4.1 in the final publication.
52961	12	26			What about LUMIP and the global climate response to LUC (although it may be mostly a regional issue)? [Hervé Douville, France]	Not applicable. Output from LUMIP is not used in Chapter 4.
7453	12	27	12	28	DCPP is missing (Boer et al., 2016) [Wolfgang Müller, Germany]	Accepted. DCPP is included in the Table 4.1 in the final publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44979	12	50	13	47	It would be great if a comparative analysis including also RCMs not only GCMs. I am not sure if it is the best place to put comments about the GCM-RCM multi-model ensembles here, but you may think about it. For a sample study please check Fernandez et al., 2019. "Consistency of climate change projections from multiple global and regional model intercomparison projects", Climate Dynamics, 52, 1139-1156. [Mustafa Tufan Turp, Turkey]	reject. This chapter covers global metrics of climate change
79691	13	4	13	4	I suggest "for robustly separating and quantifying" [Laurent Terray, France]	accepted. Done
84231	13	12	13	20	not clear how PPE information/results are used in the chapter (not mentioned in section 4.1) [Annalisa Cherchi, Italy]	taken into account. Text updated to reflect approaches used in chapter 4
114449	13	12	13	20	PPE: Please coordinate and check consistency with Ch1 [Jan Fuglestvedt, Norway]	taken into account. Text updated to reflect approaches used in chapter 4
38587	13	19	13	19	Please also cite Piani et al (2005; https://www.researchgate.net/publication/228354568_Constraints_on_climate_change_from_a_multi-thousand_member_ensemble_of_simulations), and Sexton et al (2012; https://link.springer.com/article/10.1007%2Fs00382-011-1208-9), the latter being based on the only mathematially rigorous framework for handling weighting including structural uncertainty that has been proposed to date (Rougier (2007; (https://link.springer.com/article/10.1007/s10584-006-9156-9). It is the PPE that allows one to do this because it is possible to use mathematics to define structural uncertainty for a single modelling framework such as a PPE. Maybe this aspect should also be mentioned in contrast to l.8 on this page about lack of optimality, which is the case for a multi-model ensemble. [David Sexton, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. Text updated to reflect approaches used in chapter 4
18979	13	19			say what an "emergent constrain" is given it's a fairly new idea compared to other jargon used in the section. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. Text refers to chapter 1 where this is explained
79693	13	22	13	23	There are many papers showing that the challenge is also present on long-time scales (at regional scales and/or considering hydrological or circulation variables) [Laurent Terray, France]	accepted. Text clarified
127437	13	22	13	25	The timescale for detection of a forced signal is not fixed since it depends on the magnitude of the forcing as well as the internal variability. General statements about detectability need to allow for this. For instance, there is no question that Pinatubo is easily detectable in stratospheric variables without having to wait for 'a few decades'. [Trigg Talley, United States of America]	accepted. Text clarified
21627	13	22	13	45	This summary is missing the use of single model large ensembles to explore unpredictable forcing futures such as plausible volcanism and I would suggest adding brief text to this end citing Bethke et al. And yes, this comment is uncomfortably close to being self-serving. [Peter Thorne, Ireland]	accepted. Text clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24017	13	22	13	45	Some discussion of the fact that climate models severely underestimate the predictable signal would be relevant here. For example, on decadal timescales, the predictable signal of the NAO is an order of magnitude too small in climate models, so that taking the models at face value greatly overestimates the role of internal variability. This model error is seen on seasonal, interannual and decadal timescales and there is evidence that longer term responses to external forcings are also affected. [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been revised and the discussion suggested has been included.
45467	13	29	13	29	I do not think that “therefore” is good phrasing here. The potential of large ensembles to quantify uncertainty due to internal variability does not originate from their more frequent use since AR5. This is, however, what the phrasing suggests. [Leonard Borchert, France]	accepted. Done
79695	13	31	13	31	I would avoid using "internal noise", better to keep the same term "internal variability" throughout. I would also suggest to replace "thereby extract …internal noise" by "achieve a more robust estimation of individual model forced response" [Laurent Terray, France]	accepted. Done
110831	13	31			I'd like to suggest adding after "signal from the internal noise" the following ", which can be calibrated against observational data to improve the reliability of probabilistic regional climate projections over the near and mid-term, i.e. 2021-2060 (O’Reilly et al., 2020);" with O'Reilly, C. H., Befort, D. J., and Weisheimer, A. (2020): Calibrating large-ensemble European climate projections using observational data, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-6, under revision. The reason is that there is emerging literature that uses large ensembles to calibrate the spread of the projections using observations as constraints. [Francisco Doblas-Reyes, Spain]	taken into account. Text has been revised
84233	13	33	13	34	syntax of the sentence needs to be adjusted [Annalisa Cherchi, Italy]	Corrected
79665	13	33	13	34	"An alternative approach .... given model is stochastic physics". Typo. Perhaps "An alternative approach .... given model using stochastic physics" [Hannah Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Corrected
127441	13	33	13	34	Sentence is poorly worded. [Trigg Talley, United States of America]	Corrected
104685	13	33	13	39	The discussion of the benefits of stochastic physics for increasing variability between ensemble members might (1) mention if it has been employed in any CMIP6 models, and (2) in addition to its usefulness for representing uncertainty on seasonal time scales, mention any potential benefits for decadal predictions as well. [William Merryfield, Canada]	Taken into account (1) we have included this information (2) the impact of stochastic physics in probabilistic decadal predictions has not been assessed.
104611	13	33	13	45	The portion of the discussion on stochastic plysics that covers positive impacts on model performance seems better suited for section 1.5.3 or 1.5.4 addressing model formulation and techniques. [William Merryfield, Canada]	taken into account. Text has been updated to reflect approaches used in chapter 4

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127439	13	33	13	45	Delete. No model with a stochastic parameterization (which is not that new as a concept) replicates the spread of structural variability across the multi-model ensemble or a PPE. They are just different models. This is thus irrelevant for the needs of this section. [Trigg Talley, United States of America]	Agreed that a single run of a model with stochastic physics is still a single realisation. However the large initial condition ensembles with a single climate model, introduced immediately preceding this section, have limited use if the internal variability in that model is a poor representation of the true internal variability of the climate system. Stochastic physics has been shown to improve systemic biases in variability in a way which has not been achieved through incremental improvements to deterministic parametrisation. In the light of these considerations we decided to include this discussion in the FGD as well. However this has been shortened.
70877	13	33	13	45	I think it's extremely confusing to suggest that stochastic physics represents a different approach to combine internal variability and structural uncertainty. If the physics is truly stochastic (in contrast to PPE), then this is just a different sort of parameterization, is it not? It still should be treated as a single model. Thus, a large ensemble run with stochastic physics should be pretty much the same as a large initial-condition ensemble, in the sense that the ensemble members can be treated as exchangeable. The point about stochastic physics improving systematic biases is all very well, but a single run of a model with stochastic physics is still only one realization. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Agreed that a single run of a model with stochastic physics is still a single realisation. However a large ensemble with a single climate model has limited use if the internal variability in that model is a poor representation of the true internal variability of the climate system. Stochastic physics has been shown to substantially improve systemic biases in internal variability, for example in ENSO, in a way which has not been achieved through incremental improvements to deterministic parametrisations. Nevertheless we have improved the confusing wording at the start of the section, and suggested to split the paragraphs on Single Model Initial condition large ensembles and stochastic physics.
111885	13	36	13	36	Kindly please verify if “representing uncertainty in initialised ensembles” should not be replaced with “representing structural uncertainty in initialised ensembles” [Mihaela Caian, Romania]	Accepted - corrected
106863	13	36	13	36	I would add this paper Batte and Doblas-Reyes (2015, https://doi.org/10.1007/s00382-015-2548-7) in addition to Weisheimer et al. (2014) [Christophe CASSOU, France]	Accepted
79667	13	38	13	45	I don't think Christensen and Berner 2019 says this as such. I'd suggest merging this sentence with the previous, and moving Christensen and Berner 2019 down to line 45, since it concerns the theory for changing Climate Sensitivity through stochastic parametrisations. [Hannah Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
4105	13	39	13	43	The statement here is vague. What are the impacts for near-term projections and why are they important? Are they thought to be more realistic, are they better constrained or is there greater spread, etc.? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	accepted - The statement has been deleted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87941	13	47	17	2	There has been a lot of discussion about the misuse of RCP8.5 and it should be mentioned in this chapter. Examples: Hausfather & Peters Nature https://www.nature.com/articles/d41586-020-00177-3 Burgess et al https://osf.io/preprints/socarxiv/ahsxw/ The issue is that RCP8.5 is not at all a "business-as-usual" (no mitigation) scenario, so it should not be used as a contrast to "mitigation" scenarios. [Ross McKitrick, Canada]	reject. WG1 does not critique scenarios. We do not refer to it as business-as-usual
87943	13	47	17	2	More generally, historical comparison of the range of emission/concentration scenarios to observed CO2 accumulation shows that the "no mitigation" scenario is rght at the low end of the projection range. On this see Figure S4 in the supplement to [Hausfather, Z., Drake, H., Abbott, T. and Schmidt, G. (2019) Evaluating the performance of past climate model projections. Geophysical Research Letters doi: 10.1029/2019GL085378] It's a really remarkable graph and you should look at it, here is the link: https://agupubs.onlinelibrary.wiley.com/action/downloadSupplement?doi=10.1029%2F2019GL085378&file=grl59922-sup-0001-2019GL085378-SI.docx. It contrasts the CO2 concentration forecasts from a wide range of past IPCC assessments (and other studies) against the observed concentrations. Since the 1970s through to today, there have regularly appeared batches of forecasts that spread upwards like a series of sideways-V patterns, reflecting high, medium and low projections. The observations have always tracked the low end of the V. Since they accumulated over an interval during which there was no mitigation policy, the historical implication is that the "no mitigation" scenario is at the low end of the various ranges put forward, going right up to the present. [Ross McKitrick, Canada]	Rejected. Assessment of previous reports and families of scenarios is covered in chapter 1 (section 1.6.1.3). Figure 1.27 shows that what you say here is not the case for past generations of scenario families (IS92, SRES etc) for CO2 emissions.
115191	13	48	14	10	This section should clarify better how CO2 is used as an input in the CMIP6 experimental design - the simulations were driven by CO2 concentrations, not emissions, and so they explore the climate responses to a concentration and radiative forcing pathway, as opposed to the response to an emissions scenario including carbon cycle responses and their uncertainties. [Richard Betts, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. Scenarios also include radiative forcing from aerosols, but not (for example) land use change. CO2 can be both emissions or concentration driven and both are used. 4.2.1 describes the methodology and 4.3.1 presents results
89845	13	48	14	32	This paragraph assumes "scenarios" are synonymous with "socio-economic or emissions scenarios". However, Sutton & Hawkins (ESD, 2020) argue that this report could usefully consider climate response scenarios as well as socio-economic scenarios. (see: https://www.earth-syst-dynam-discuss.net/esd-2019-88/) [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. Text has been revised
51095	13	50	14	10	Please clarify here that in the SSP scenarios, changes in GHG are represented in terms of concentrations not emissions - this is important information for understanding whether the projections capture the contribution of uncertainties in climate-carbon cycle feedbacks to the overall uncertainties in the response of the climate system to emissions scenarios. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. CO2 can be both emissions or concentration driven and both are used. 4.2.1 describes the methodology and 4.3.1 presents results

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127443	13	52			Radiative forcing "by well-mixed GHGs". [Trigg Talley, United States of America]	taken into account. Text lists the mechanisms which affect radiative forcing
77693	13				The RCP scenarios in CMIP5 did not include socio economic pathways (SSPs). It is good there is a section at the start of the chapter explaining scenarios, and referencing section 1.6, which describes the set of SSPs and timeline of scenarios in detail [Emer Griffin, Ireland]	taken into account. No action required
127445	14	6			"required" should be "asked". [Trigg Talley, United States of America]	accepted. Done
96395	14	10			Please define "very long-term climate change" (the term has not been mentioned yet in the introduction in page 11, lines 41-43) [Nicole Wilke, Germany]	accepted. Definition added in 4.1
106865	14	14	14	14	Background aerosol is not precise enough: this should be "Backgroud natural aerosols (mineral dust, sea-salt)" to make a clear difference with anthropogenic aerosols which come from scenario. [Christophe CASSOU, France]	accepted. done
45801	14	14	14	15	"Background aerosols are ramped down (...)" It seems this statement applies to the background tropospheric aerosols, but it is unclear what the background aerosol component consists of in models with prescribed or interactive aerosols. If this ramping down of the tropospheric background is indeed part of the CMIP6 protocol, I wonder how many models have actually implemented it in this way, and for which aerosol components. [Twan van Noije, Netherlands]	taken into account. We clarify that we refer to background aerosols. We do not attempt to document individual model implementations of the protocol
2367	14	14	14	18	Would be useful to have a reference here similar to the Mathes et al paper for solar forcing for projections [Vaishali Naik, United States of America]	Accepted. Matthes includes solar up to 2300
127447	14	17			How does this "avoid uncertainty" in unknowable volcanic forcing? That uncertainty is irreducible. Rather, this tries to avoid artifacts in the experimental design that could confuse the near-term projections. [Trigg Talley, United States of America]	accepted. Text has been updated to stress it is to avoid inconsistent treatment between models
106867	14	20	14	22	CO2 and non-CO2 GES concentrations are also different between RCP and ssp. Not only the regional forcings. This should be also mentioned. [Christophe CASSOU, France]	Accepted. This is a very important point, and the differences are significant. Section 4.6.2 covers this in more detail, and the assessment is reflected in our ES
66549	14	22	14	23	The SSPs and RCPs are not necessarily comparable on a global scale. The forcing scenarios behind the "nominal forcing levels" such as SSP2-4.5 and RCP4.5 are sometimes very different (different evolution of GHG levels) implying that resulting climate projections based on these different scenarios can lead to large differences in results even if the nominal radiative forcing is the same. For instance, Wyser et al (2020) shows that the EC-Earth model commonly used in CMIP5 and CMIP6 get a much stronger climate change signal when forced by the new CMIP6 SSP-forcing compared to the corresponding RCPs. This difference in forcing has a strong impact in addition to changes in climate sensitivity in this model. Wyser, K., Kjellström, E., Königk, T., Martins, H. and Doescher, R., 2020. Warmer climate projections in CMIP6: the role of changes in the greenhouse gas concentrations from CMIP5 to CMIP6. Environ. Res. Lett., 15, 054020, DOI: 10.1088/1748-9326/ab81c2. [Kjellström Erik, Sweden]	Accepted. This is a very important point, and the differences are significant. Section 4.6.2 covers this in more detail, and the assessment is reflected in our ES

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45803	14	25	14	25	This limitation of the RCPs is discussed in a paper by Chuwah et al. It would be appropriate to include a reference to that paper here: Chuwah, C., et al., 2013: Implications of alternative assumptions regarding future air pollution control in scenarios similar to the Representative Concentration Pathways, Atmos. Environ., 79, 787-801, https://doi.org/10.1016/j.atmosenv.2013.07.008. [Twan van Noije, Netherlands]	accepted. done
2369	14	26	14	29	Would be useful to cite chapter 6 here for more information on SLCF projections [Vaishali Naik, United States of America]	accepted. Link has been made
114955	14	27	14	27	Update of the refernce needed (Gidden et al. 2019: https://www.geosci-model-dev.net/12/1443/2019/) [Zbigniew Klimont, Austria]	accepted. done
19831	14	37	14	42	This paragraph recognizes that the 4.2.3. title is somewhat misleading. Only one among the three information sources is specific of near term. [philippe waldteufel, France]	Rejected. All three sources of information mentioned contribute to near term (even if not all them are specific for near term).
79697	14	44	14	45	It seems to me that one cannot really say that D&A methods correct systematic model biases in general (for instance the classical regression-based approach assumes that the spatial pattern of the forced response as simulated by models is correct and that uncertainty is only on the amplitude. This is likely to be in many cases a very strong assumption). Then they derive scaling factors to reach "consistency" between observations and models, that has then to be confirmed by a residual consistency test. I would rephrase by focusing more on consistency rather than "correcting for systematic model biases". [Laurent Terray, France]	Taken into account - Text revised following the suggestion.
52963	14	44	14	46	D&A methods are rather use to correct for spurious sensitivities to the radiative forcings and may be more efficient for constraining long-term climate change given the lesser contribution of natural variability. [Hervé Douville, France]	Taken into account - The statement has been revised.
90671	14	44	14	48	Shiogama et al. (2016) cited in line 48 should be moved to line 44 (after Stott et al. 2013). [HIDEO SHIOGAMA, Japan]	Accepted
127449	14	45			Add "attempt" before "to correct". [Trigg Talley, United States of America]	Accepted
132441	14	47	14	47	Here and throughout the chapter: at times you say "global surface air temperature" while at other times you say "global surface temperature". Are these always the same things (GSAT)? Perhaps check for consistency in language throughout. [Kyle Armour, United States of America]	Accepted - consistency checked
19833	14	52	15	1	While this may seem of marginal importance, a comment explaining why climate predictions initialized from the observed climate state are not called climate forecasting would be welcome. [philippe waldteufel, France]	Rejected - Definition of climate prediction is given in the glossary.
116301	14		14		The use of sensitivitiy tests ot explore the possible effect of future volcanic eruptions to complement SSPs could be introduced in section 4.2.2. [Valerie Masson-Delmotte, France]	taken into account. Volcanic eruptions covered in a cross-chapter box 4.1.

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106079	15	3	15	19	there is no recognition that non-linearity can reduce forecast skill, in either initialisation approach. It is also important to realise that drift might be related to the different climate sensitivty of model and observations, e.g., Chikamoto, Y., A. Timmermann, M. J. Widlansky, S. Zhang, and M. A. Balmaseda, 2019: A Drift-Free Decadal Climate Prediction System for the Community Earth System Model. Journal of Climate, 32, 5967-5995. [Noel Keenlyside, Norway]	Taken into account - The section has been revised and this suggestion is included.
84235	15	3	16	5	this part is rather long and detailed. It could be eventually reduced and focused on the minimum of the information needed [Annalisa Cherchi, Italy]	Taken into account - Text is revised and shortened as much as possible.
7455	15	4			Ocean only state has been assimilated in the MPG CMIP5 decadal predictions experiments (Müller et al., 2012). Müller, W. A., J. Baehr, H. Haak, J. H. Jungclaus, J. Kröger, D. Matei, D. Notz, H. Pohlmann, J.-S. von Storch, and J. Marotzke, 2012: Forecast skill of multi-year seasonal means in the decadal prediction system of the Max Planck Institute for Meteorology. Geophys. Res. Lett., 39, L22707, doi:10.1029/2012GL053326. [Wolfgang Müller, Germany]	Taken into account - this section has been revised and your suggestion included.
4641	15	10	15	10	model (Smith et al., 2013a; Bellucci et al., 2015b; Kröger et al., 2018) --> model (Smith et al., 2013a; Bellucci et al., 2015b; Kröger et al., 2018, Nadiga et al., 2019). [Balasubramanya Nadiga, United States of America]	Accepted
79699	15	10	15	10	I suggest to cite this paper as well, as it purely focuses on drift: Sanchez-Gomez, E., C. Cassou, Y. Ruprich-Robert, E. Fernandez, and L. Terray, 2016: Drift dynamics in a coupled model initialized for decadal forecasts. Climate Dynamics, 46, pp. 1819-1840, doi: 10.1007/s00382-015-2678-y [Laurent Terray, France]	Accepted
45469	15	10	15	12	A reference illustrating how the drift induced by full field initialization can obscure the predictable signal (e.g. Kroger et al., 2018) would be welcome. [Leonard Borchert, France]	Accepted
2145	15	14	15	14	suggest adding after the references: "…but has the disadvantage that the model state is then further from the real world from the start of the prediciton". [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
4643	15	16	15	16	Teng et al., 2017 --> Teng et al., 2017, Nadiga et al., 2019 [Balasubramanya Nadiga, United States of America]	Accepted
2147	15	25	15	25	Knight et al was superseded by Skilful predictions of the winter North Atlantic Oscillation one year ahead. Dunstone N. et al, 2016. Nat. Geosci., 9, 809-814, doi:10.1038/ngeo2824. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
106869	15	25	15	25	I would add: " … Knight et al, 2014) or hybrid relaxation combining surface and tri-dimensional restoring as function of ocean basins and depth (Sanchez et al. 2016, https://doi.org/10.1007/s00382-015-2678-y), to sophisticated…" [Christophe CASSOU, France]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106081	15	25	15	26	The Norwegian Climate Prediction model has been pioneering EnKF initialisation, and would be appropriate to cite corresponding papers: Counillon, F., I. Bethke, N. Keenlyside, M. Bentsen, L. Bertino, and F. Zheng, 2014: Seasonal-to-decadal predictions with the ensemble Kalman filter and the Norwegian Earth System Model: a twin experiment. Tellus Series a-Dynamic Meteorology and Oceanography, 66; Counillon, F., N. Keenlyside, I. Bethke, Y. Wang, S. Billeau, M. L. Shen, and M. Bentsen, 2016: Flow-dependent assimilation of sea surface temperature in isopycnal coordinates with the Norwegian Climate Prediction Model. Tellus A, 68, 32437. [Noel Keenlyside, Norway]	Accepted
4645	15	26	15	26	assimilation methods such as the ensemble Kalman filter (Msadek et al., 2014 --> assimilation methods such as the ensemble Kalman filter (Nadiga et al., 2013, Msadek et al., 2014 ... Reference: Nadiga, B. T., Casper, W. R., & Jones, P. W. (2013). Ensemble-based global ocean data assimilation. Ocean Modelling, 72, 210-230. [Balasubramanya Nadiga, United States of America]	Accepted
7457	15	26			EnKF has also been tested in the MPG System (Brune et al., 2018). Brune, S., A. Düsterhus, H. Pohlmann, W. A. Müller, J. Baehr, 2018: Time dependency of the prediction skill for the North Atlantic subpolar gyre in initialized decadal hindcasts. Clim. Dyn., 51, 1947-1970. doi:10.1007/s00382-017-3991-4 [Wolfgang Müller, Germany]	Accepted
106871	15	32	15	32	Remove Cassou et al. 2018 reference. Not relevant here. [Christophe CASSOU, France]	Accepted
52965	15	36	15	37	Bias corrections (BC) are also needed for projections. The specificity of climate predictions is that BC are indeed lead-time dependent. [Hervé Douville, France]	Noted. No comparable methods exist for projections, owing to lack of verification cases.
7459	15	36	15	45	With respect to the bias adjustment, the extension of the posteriori calibration method to decadal climate predictions has been a benchmark (Pasternak et al., 2018). This method adjusts mean bias, trend and spread of decadal prediction by using a parameterized statistical model. Pasternack, A., J. Bhend, M. A. Liniger, H. W. Rust, W. A. Müller, and U. Ulbrich, (2018): Parametric Decadal Climate Forecast Recalibration (Deforest 1.0). Geosci. Model Dev., 11, 351-368 [Wolfgang Müller, Germany]	Accepted
106083	15	36	15	45	Again, there is no mention of the impacts of model errors on forecast skill [Noel Keenlyside, Norway]	Taken into account - The section has been revised.
106873	15	37	15	37	I would add "… to isolate the predicted climate anomalies and the phase of the decadal modes of variability" [Christophe CASSOU, France]	Accepted
106875	15	42	15	42	I would add:"…further aspects of decadal predictions may be biased, such as the Modes of Variability (ENSO, NAO, etc.) upon which drift patterns are projectng (Sanchez et al. 2016, https://doi.org/10.1007/s00382-015-2678-y), and additional…". This helps really introduce the MoVs as a cross-cutting issue. [Christophe CASSOU, France]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106877	15	50	15	50	Unclear: what does "sign and phase of future climate variability" mean? I would say "the ability to predict the sign and phases of the main modes of decadal variability and their regional fingerprint through teleconnection". [Christophe CASSOU, France]	Accepted
105451	15	55	15	55	Another more recent way of assessing decadal predictions is the following: Analyse how well a specific mechanism is represented at each lead times. This provides a new method to assess forecast skill, which is a more process-based or mechanistic way of assessing skill. One example of this is given in Mohino et al. (2016, https://doi.org/10.1007/s00382-016-3416-9) [Helene R. Langehaug, Norway]	Taken into account in the revised text
45471	16	7	16	11	This sentence is unnecessarily convoluted. I think there might be a comma missing somewhere. As it stands, this is difficult to digest. [Leonard Borchert, France]	Accepted and corrected
127451	16	12	16	16	These lines suggest that most of the rest of this section is irrelevant. If the actually initialization only helps for a year (despite the potential existing for a greater role) over what one would get with a free running model with the same forcing, why is this taking up any space in a report looking at decadal scale projections? [Trigg Talley, United States of America]	Rejected - The sentence is referred to SST on a global scale. In the following paragraph skill of initialised predictions over different regions is assessed.
105453	16	14	16	15	Also an observation based study in the North Atlantic - Arctic region demonstrates that mechanisms describing internal variability can provide predictability several years ahead (Årthun et al., 2017, https://doi.org/10.1038/ncomms15875). [Helene R. Langehaug, Norway]	Accepted
79669	16	19	16	22	I found this sentence hard to follow - I wasn't sure what the approach was alternative to. Does the following sentence still keep the meaning? "Although the skill added by initialization tends to be modest, particularly over land and at longer lead times, assessing the relative skill of initialized and uninitialized predictions in forecasting observed variability suggests the value added by initialization may be greater than previously thought (Scaife and Smith, 2018; Kushnir et al., 2019; Smith et al., 2019c)." [Hannah Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence has been removed.
105455	16	20	16	22	Please rephrase as it is unclear what is meant by 'an alternative approach'. [Helene R. Langehaug, Norway]	The sentence has been removed.
42705	16	20			'an alternative approach' – it would be useful to provide a short description of the important features of this alternative approach. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	The sentence has been removed.
106879	16	21	16	21	"An alternative approach" is too vague. Say more precisely what it is because this is new literature and clearly an added value compared to AR5 [Christophe CASSOU, France]	The sentence has been removed.
24019	16	22	16	22	I don't think Scaife and Smith 2018, and Kushnir et al, 2019, are relevant to this statement [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	The sentence has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106085	16	25	16	25	The earlier reference to this is missing: Keenlyside, N. S., M. Latif, J. Jungclaus, L. Kornblueh, and E. Roeckner, 2008: Advancing decadal-scale climate prediction in the North Atlantic Sector. Nature, 453, 84–88. [Noel Keenlyside, Norway]	Accepted, reference added.
105457	16	26	16	27	The work by Matei et al. (2012, https://doi.org/10.1175/JCLI-D-11-00633.1) suggest attribution of skill to a successful initialization of the AMOC. [Helene R. Langehaug, Norway]	Taken into account - AMOC related skill is in CMIP6 is included
45473	16	26	16	31	I think it should be made clear here that the North Atlantic subpolar gyre is of such great interest to the decadal prediction community, because that is where most of the improvement from initialization is commonly found (e.g. Brune & Baehr, 2020; 10.1002/wcc.637). This is currently not made explicit. [Leonard Borchert, France]	Taken into account - The text says explicitly that much of the predictability is associated with the North Atlantic subpolar gyre. The reference is included.
7461	16	31	16	32	Some more publications are available indicating that there is temperature skill over continental regions. Here are some suggestions for: (Europe) Müller, W. A., J. Baehr, H. Haak, J. H. Jungclaus, J. Kröger, D. Matei, D. Notz, H. Pohlmann, J.-S. von Storch, and J. Marotzke, 2012: Forecast skill of multi-year seasonal means in the decadal prediction system of the Max Planck Institute for Meteorology. Geophys. Res. Lett., 39, L22707, doi:10.1029/2012GL053326., (East Asia) Monerie, Paul-Arthur & Robson, Jon & Dong, Buwen & Dunstone, Nick. (2017). A role of the Atlantic Ocean in predicting summer surface air temperature over North East Asia?. Climate Dynamics. 10.1007/s00382-017-3935-z. , (Eurasia) Wu, B., Zhou, T., Li, C., Müller W. A. and J. Lin (2019), Improved decadal prediction of Northern-Hemisphere summer land temperature . Climate Dynamics, doi.org/10.1007/s00382-019-04658-8. [Wolfgang Müller, Germany]	Taken into account - New references are included.
19513	16	31	16	32	A significant improvement in temperature skill is also found over some land regions including Europe and the Middle East. This sentences is unclear, what is meaning of temperature skill? [Hamideh Dalaei, Iran]	Taken into account. Temperature skill is the skill in predicting GSAT.
32689	16	31	16	32	Add "include Iran Plateau" after Middle East. Because a lot of evidence show that significant improvement in temperature skill also found over Iran Plateau. It can mention to I. R. of Iran Meteorological Organization reports, Alizadeh et al. 2019 https://doi.org/10.1016/j.dynatmoce.2019.101105 , and a lot of references [sadegh zeyaeyan, Iran]	Rejected - The paper suggested refers to seasonal predictions
33019	16	31	16	32	Add "include Iran Plateau" after Middle East. Because a lot of evidence show that significant improvement in temperature skill also found over Iran Plateau. It can mention to I. R. of Iran Meteorological Organization reports, Alizadeh et al. 2019 https://doi.org/10.1016/j.dynatmoce.2019.101105 , and a lot of references [Sahar Tajbakhsh Mosalman, Iran]	Rejected - The paper suggested refers to seasonal predictions
106087	16	34	16	46	here you are missing reference to the trans-basin mode and its multi-year predictability: Chikamoto, Y., A. Timmermann, J.-J. Luo, T. Mochizuki, M. Kimoto, M. Watanabe, M. Ishii, S.-P. Xie, and F.-F. Jin, 2015: Skilful multi-year predictions of tropical trans-basin climate variability. Nature Communications, 6, 6869. [Noel Keenlyside, Norway]	Taken into account - reference added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7471	16	34	16	51	I wonder whether it is worth mentioning here that other dynamical quantities in the atmosphere exhibit predictive skill on decadal timescales. Among others are extra-tropical storm-tracks and storm density (Kruschke et al., 2016, Schuster et al., 2019), blockings (Schuster et al., 2019, Athanasiadis et al,. 2020), and QBO (Pohlmann et al, 2013, Scaife et al., 2014, Pohlmann et al., 2019). Schuster, M., J. Grieger, A. Richling, T. Schartner, S. Illing, C. Kadow, W. A. Müller, H. Pohlmann, and U. Ulbrich (2019); Improvement in the decadal predictions skill of the northen hemisphere extra-tropical circulation through increased model resolution. Earth Syst. Dynam., 10, 901–917, doi.org/10.5194/esd-10-901-2019. Pohlmann, H., Müller, W. A., Bittner, M., Hettrich, S., Modali, K., Pankatz, K., Marotzke, J. (2019). Realistic quasi-biennial oscillation variability in historical and decadal hindcast simulations using CMIP6 forcing. Geophysical Research Letters, 46, 14,118–14,125. doi.org/10.1029/2019GL084878. Scaife, A. A., M. Athanassiadou, M. Andrews, A. Arribas, M. Baldwin, N. Dunstone, J. Knight, C. MacLachlan, E. Manzini, W. A. Müller, H. Pohlmann, D. Smith., T. Stockdale, and A. Williams, 2014: Predictability of the Quasi-Biennial Oscillation and its Northern Winter Teleconnection on Seasonal to Decadal Timescales. Geophys. Res. Lett., 41, pp 1752–1758. Kruschke et al, 2016, Pohlmann et al., 2013 are already cited, Athanasiadis see comment above. [Wolfgang Müller, Germany]	Taken into account - Your suggestion is taken into account in the revised version of the section.
106881	16	39	16	39	I would add:" due to is dependence on predicatble variations in North Atlantic SST through teleconnection (Martin and Thorncroft 2014a , Technical Annex VI)" [Christophe CASSOU, France]	Accepted
45475	16	40	16	44	The potential of ensemble size increase for decadal prediction was recently illustrated by a study demonstrating for the first time skillful decadal predictions of extremely warm summers over land (Borchert et al., 2019; 10.1029/2019GL085385). I think these results should be added here. [Leonard Borchert, France]	Accepted
96397	16	40			For readability reasons please include the meaning of the abbreviation "SST", as it is mentioned for the first time in this chapter. [Nicole Wilke, Germany]	Rejected. The acronyms SST was unfolded at line 23 page 4-15
127453	16	44	16	46	This cannot possibly be true as a general statement (and if were true would suggest that *all* perfect model assessments of predictability should be trashed). Rather, these papers have highlighted a very specific case where this (very interestingly) seems to be true. [Trigg Talley, United States of America]	Taken into account - The statement is revised to make it more specific

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70879	16	44	16	46	This statement needs to be weakened (or caveated, if that's a word), because as written it sounds like the deficiency is universal. Most of the evidence appears to be for the NAO. For the SAM, for example, the predictable signal in models seems to be consistent with nature (Byrne et al. 2019 doi: 10.1029/2018JD030173; and note that Seviour et al. 2014, which had claimed a deficiency for another forecast system, is re-interpreted in Byrne et al. as showing no deficiency). This is particularly important for this chapter, given the central role of the SAM projections in this chapter. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The statement is revised to make it more specific
106883	16	46	16	46	I would add this reference: Strommen and Palmer (2018), https://doi.org/10.1002/qj.3414, which tackles the issue of signal to noise paradox. [Christophe CASSOU, France]	Accepted
105459	16	48	16	49	This is also shown for the winter sea ice in the Arctic/Barents Sea (Onarheim et al., 2015, https://doi.org/10.1002/2015GL064359; Dai et al., 2020, https://doi.org/10.1007/s00382-020-05196-4). [Helene R. Langehaug, Norway]	Accepted
17031	16	48	16	51	As natural disasters like hurricanes are on the rise, resulting in loss of human lives and large-scale property damages, it would be highly beneficial if feasible guidelines are provided, so that international rescue organizations could achieve higher efficiency in human rescue. The guidelines must stress on the importance of using 'smart drones' maneuvered by 'Artificial Intelligence' to reduce the risk of sending human rescue operators. Forest fires are also causing large-scale property damages in many countries. Here also it would be highly beneficial if feasible guidelines are provided highlighting the importance of using "Internet of Things" to capture forest data and to predict the wildfire even before it happens. It has been successfully done in Chile by Entel's DataRobot. [Ravi Amblee, United States of America]	Rejected - This suggestion is off topic with respect to the matter covered in this sub-section
68103	16	50	16	50	Citing a recent study by Solaraju-Murali et al., 2019** could be relevant for drought assessment along with Chikamoto et al., 2017. This study assesses the skill of decadal prediction systems at forecasting the seasonal evolution in drought conditions using proxy drought indices at multi-annual timescale. This assessment presents high skill in predicting summer drought conditions over several regions across Europe. **Solaraju-Murali, B., L.-P. Caron, N. González-Reviriego and F.J. Doblas-Reyes (2019). Multi-year prediction of European summer drought conditions for the agricultural sector. Environmental Research Letters, 12, 124014, doi:10.1088/1748-9326/ab5043. [Balakrishnan Solaraju Murali, Spain]	Accepted
2149	16	50	16	50	Smith et al 2010 also showed decadal predictability of Atlantic hurricanes: Skilful multiyear predictions of north Atlantic hurricane frequency. Smith D.M., R. Eade, N.J. Dunstone, D. Fereday, J.M. Murphy, H. Pohlmann and A.A. Scaife 2010. Nat. Geosci., 3, 846-849, DOI: 10.1038/NGEO1004. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106089	16	50	16	51	Here is a more recent reference related to biogeochemistry, that indicates the BGC can be initialised largely from the physics. Fransner, F., F. Counillon, I. Bethke, J. Tjiputra, A. Samuelsen, A. Nummelin, and A. Olsen, Ocean biogeochemical predictions -initialization and limits of predictability, Frontiers Marine Science, accepted, 2020 [Noel Keenlyside, Norway]	Accepted
7463	16	50			Paxian et al estimated the prediction skill for droughts in the MPG model based on a standardized precipitation index. Paxian A., M. Ziese, F. Kreienkamp, K. Pankatz, S. Brand, A. Pasternack, H. Pohlmann, K. Modali, B. Früh, 2019: User-oriented global predictions of the GPCC drought index for the next decade, Meteorologische Zeitschrift, Vol 28 No. 1, 3 – 21 [Wolfgang Müller, Germany]	Accepted
106885	16	53	16	53	I would say: "In summary, despite challenges (Cassou et al. 2018, Kushner et al. 2019), there is high confidence that info…. On a global scale and large regions over multiannual-to-decadal scale though teleconnections. [Christophe CASSOU, France]	Taken into account - The statement is revised according to suggestions combined with other comments.
4635	16	53	16	54	1. The bar is being set low when it comes to evaluating the utility of initialized predictions as they presently stand. Any measure of initialized predictions has to be gauged in comparison to that of uninitialized predictions. The statement does not make this clear. 2. Measuring initialized predictions against uninitialized predictions is important because unlike the latter, the former has many problems associated with initialization. Chief among them is initialization-related drift associated with full field initialization and a majority of the models use full field initialization. The initialization-related drift in turn necessitates statistical post-processing techniques before the initialized predictions show any skill at all. 3. Presently it is often the case that improvement in skill provided by initialized predictions (with IC ensembles) over that provided by uninitialized predictions (again with IC ensembles) for most models is small and depends strongly on the post-processing method used. 4. Currently, post processing methods are optimized to maximize skill over the verification period and given the shortness of this period, out-of-sample testing is either completely absent or there is very little of it. While realizing the level of skill seen over the verification period in actual predictions over the near-term future is never guaranteed , lack of out-of-sample testing of the post-processing methodology reduces the chances of realizing the level of skill seen over the verification period. 5. Detailed analysis of the physics and dynamics of initialization-related drift as in the CNRM-CM5 model by Sanchez-Gomez et al. (Sanchez-	Rejected - Thank you for reporting this. In the sub-section these points have been addressed and the final assessment takes these points into consideration.
19835	16	53	16	54	This concluding paragraph suggests that "initial conditions" for example might be a more accurate title for the present 4.2.3 section [philippe waldteufel, France]	Rejected - The section addresses the various sources of predictability in the near-term.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127455	16	53	16	54	This seems to be contradictory to lines 12-16 on the same page. [Trigg Talley, United States of America]	Taken into account - The statement is revised according to suggestions combined with other comments.
106091	16	53	17	2	This section fails to discuss the issue of model error impacting forecast skill. There should be some discussion of it. It should also be discussed that these predictions remain at fairly primative stage, and in particular most models are initialized using ad hoc schmes. [Noel Keenlyside, Norway]	Taken into account - The section has been widely revised.
24021	16	53	17	2	This does not really summarise the impacts of initialisation very well. The clearest improvements through initialisation are seen in the North Atlantic and related impacts such as hurricane frequency, Sahel and European rainfall. [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The statement is revised according to suggestions combined with other comments.
110833	17	2			Decadal predictions can also be used to reduce the uncertainty of projections beyond decadal time-scales, e.g. by constraining near-term projections using decadal predictions as in Befort et al. (2020). Such constraints aim to combine information from the initialised predictions with the forced-only projections within a seamless framework. Befort, D. J., O'Reilly, C. H. and Weisheimer, A. (2020). Constraining Projections using Decadal Predictions. Geophys. Res. Lett., under review, comments received with minor revisions [Francisco Doblas-Reyes, Spain]	Taken into account - The section has been revised and this suggestion included.
71129	17	5	18	37	Further discussion of pattern scaling for global warming levels under transient warming could refer to Seneviratne et al. (2016): Seneviratne S I, Donat M G, Pitman A J, Knutti R and Wilby R L 2016 Allowable CO2 emissions based on regional and impact-related climate targets Nature 529 477–83 and King (2019): King, A. D. (2019). The drivers of nonlinear local temperature change under global warming. Environmental Research Letters, 14(6), 064005. As briefly mentioned, under transient climate changes the ensemble-mean approximately follows pattern scaling, but individual model departures from pattern scaling can be substantial and here temperature changes and those of other variables (including precipitation) are inextricably linked. Currently this section refers to pattern scaling of temperature and precipitation separatly without suggesting there is a link. [Andrew King, Australia]	Taken into account. We are making use of Seneviratne et al. (2016) in Section 4.6.1. It is not essential for discussing methodologies in Section 4.2.4, but in terms of the results it yields. Thank you for pointing out the paper by King et al. (2019). This is used in the updated text to point out the differences in regional patterns of change that exist across GCMs at a given level of GSAT.
132443	17	11	17	13	I have not seen the "time-slab" terminology before. Is this a way of saying "time period"? [Kyle Armour, United States of America]	Taken into account. Yes, we refer here to a future period/epoch, and have updated the 4.2.4 text to make this clear.
104615	17	11	17	13	Need to reconcile use of "time-slabs" here with "time slices" on p. 4-11 line 30. [William Merryfield, Canada]	Taken into account. We have updated the 4.2.4 text to make it clear that the concepts of future epochs/time-slabs/time-periods are all equivalent in terms of the patter-scaling discussion.
19225	17	12	17	12	Why AOGCMs and not ESMs in this context? [Anne-Marie Treguier, France]	Taken into account. Both AOGCM and ESM projections are relevant here, and we have updated the text accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106887	17	17	17	17	I would replace "as a function of the increase in GSAT" by "as a function of warming levels" to use consistent terms. [Christophe CASSOU, France]	Taken into account. We have retained the use of "GSAT" in this context, since we want to make it clear that use GSAT rather than GMST to calculate the level of global warming. However, the sentence has been updated to make it clear that in terms of the pattern scaling analysis, "an increase in GSAT" is equivalent to a "level of global warming".
132445	17	21	17	36	I think a simple explanation of pattern scaling is needed here since it is such a key concept for this chapter. That is, something along the lines of the pattern of warming remaining approximately similar at a range of warming levels and across emissions scenarios. I also found this description of the methodology too technical and confusing. The explanation of methodology on page 70, lines 20-37 is a better description. [Kyle Armour, United States of America]	Taken into account. Some simplification were introduced to the discussion of the methodology, and 'pattern scaling' is clearly defined in the text.
70881	17	28	17	29	Why does pattern scaling require assuming that internal variability is independent of external forcing? That is not obvious to me. It might affect the uncertainty estimates, but I can't see how it negates the basic assumption. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This statement is relevant only to the traditional approach to pattern scaling, which assumed a spatial pattern of change that is constant over time. See the second paragraph of Section 4.2.4.
106889	17	31	17	31	The sensitivity of the teleconnection and MOVs to mean backgroud state should be mentioned here as a strong limitation of pattern scaling: it is an example of interaction between internal variability and forced reponse, Drouard and Cassou (2019, https://doi.org/10.1175/JCLI-D-18-0803.1) can be cited for ENSO and there are many references as well in this paper. This issue is also addressed and evidence is provided IN TS (Figure TS34). [Christophe CASSOU, France]	Taken into account. Many thanks for pointing out this references, which has been taken up in 4.2.4 to emphasize that internal variability places limits on the use of traditional linear pattern scaling.
132447	17	33	17	33	Check for consistency throughout on whether it's "sea ice extent" or "sea-ice extent", etc. [Kyle Armour, United States of America]	Accepted. We have updated the 4.2.4 text to refer to "sea-ice" rather than "sea ice".
70883	17	40	17	43	Certain key aspects of the slow response have been shown to be non-proportional to GSAT in a first-order way, namely midlatitude circulation (Ceppi et al. 2018 doi: 10.1175/JCLI-D-17-0323.1) and the associated precipitation changes (Zappa et al. 2020 doi: 10.1073/pnas.1911015117), because of different timescales of the SST response to warming. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. These papers and their findings are discussed in some detail in section 4.6.1.3.
52967	17	45	17	46	Add a reference to PDRMIP, for instance Richardon et al. (2018)? [Hervé Douville, France]	Accepted. References to PDRMIP have been added to the text.
19227	17	45	17	47	These two sentences are difficult to understand. Rephrasing the beginning would help. "for precipitation change, there is suppression" is difficult to grasp when reading it the first time. What is the difference between "supressed precipitation change" and "near zero adjustement"? [Anne-Marie Treguier, France]	Taken into account. The text was rewritten to convey its meaning more clearly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11271	17	46	17	46	Better to cite one more reference: Chadwick, R., Ackerley, D., Ogura, T.,& Dommenget, D. (2019). Separating the influences of land warming, the direct CO2 effect, the plant physiological effect, and SST warming on regional precipitation changes. Journal of Geophysical Research: Atmospheres, 124, 624–640. https://doi.org/10.1029/2018JD029423 [Masahiro Watanabe, Japan]	Rejected. Many thanks for pointing out this reference, but we have strengthened this section using additional references more directly aimed at disentangling the fast and slow response (PDRMIP based studies).
127457	17	47	17	47	What is meant by "solar forcing"? Would SRM be considered solar forcing? Aren't there fast, patterned, responses to SRM (e.g., ITCZ shifts? Changes in land vs ocean precipitation patterns from Marine Cloud Brightening?). [Trigg Talley, United States of America]	Rejected. SRM is not considered in this context, but its affects are discussed separately in Section 4.6.3.
77689	17	54	55	9	pg 17 Herger et al. (2015) pg 55 in Collins et al. (2013) [Emer Griffin, Ireland]	The study by Collins et al. (2013) has already been used in the text.
116303	17		17		Is it possible to develop a discussion of implications of regional changes in land use and SLCF regional forcing for pattern scaling? [Valerie Masson-Delmotte, France]	Not applicable - This comment belongs to sub-section 4.2.4.
2151	18	7	18	7	"…or degenerate responses which produce the same pattern for different forcing factors (e.g. Kidston et al, 2015)" Kidston J., A.A.Scaife, Steven C. Hardiman, Daniel M. Mitchell, Neal Butchart, Mark P. Baldwin and Lesley J. Gray 2015. Stratospheric influence on tropospheric jet streams, storm tracks and surface weather. Nature Geoscience, 8, 433-440, doi:10.1038/ngeo2424. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is not clear whether the suggested reference conveys this point. Also see the text in section 4.6.3.
116305	18	23	18	23	The representation of aerosol forcing in models needs further developments here and also in chapter 3 (currently missing for model data and model model comparisons) building on recent literature. [Valerie Masson-Delmotte, France]	Taken into account. Chapter 4 has extended its discussion of these aspects in Section 4.6.2.
127459	18	23			Delete ", even for the same SSP". There is no situation where the aerosol treatment would be identical across all models for a specific SSP alone. [Trigg Talley, United States of America]	Rejected. This is the point we want to make - even when for the same SSP prescribed forcing, the effective aerosol forcing differs from one model to the next.
79701	18	28	18	29	I agree but the number of models is not constant across GWLs and is going to be much smaller for the highest GWLs which has to be mentioned as a caveat of the method [Laurent Terray, France]	Taken into account. We regard this is a minor practical limitation given the substantial size of the multi-model CMIP6 ensemble, and this is pointed out in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18343	18	40	19	51	Again, I think one needs to be very clear regarding what the uncertainty is for. If it is for the prediction of the actual climate state for a specific future time period, then it will include the uncertainty due to our inability to preduct the future internal climate evolution. But all CMIP climate change simulations are not designed to simulate future climate states (as they started from random initial conditions). Rather, they are designed to simulate forced changes after averaging over a large number of ensemble runs. For such a purpose (of the CMIP climate simulations), the internal variability is not a source of uncertainty! Also, the following study is highly relevant to this section: Dai, A., and C.E. Bloecker, 2019: Impacts of internal variability on temperature and precipitation trends in large ensemble simulations by two climate models. Climate Dynamics, 52, 289–306. https://doi.org/10.1007/s00382-018-4132-4. [Aiguo Dai, United States of America]	Taken into account. We now refer to the reference. Even if the projections are initialized and not expected to match the manifestation of internal variability, individual realizations will still differ from the future actual state due to internal variability. This is why internal variability is referred to as a source of uncertainty in much of the assessed literature
2899	18	40	19	52	CMIP6 should compare with CMIP5. Does CMIP6 decrease the uncertainties more than CMIP5? [Zong Ci Zhao, China]	Take into account. The two are not directly comparable due to different scenarios but we now refer to Lehner et al. 2020 who compared the sources of uncertainties across the two CMIP experiments.
117255	18	43	18	43	correct section in Chapter 1 is 1.4.3 [Maisa Rojas, Chile]	Accepted and corrected.
70547	18	43			I think this reference should be 1.4.3, not 1.4.2. [Gillett Nathan, Canada]	Accepted and corrected.
89847	18	44	18	44	Please replace "due to" with "related to". "due" implies a causality and is not appropriate. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and implemented.
70549	18	47			I think this reference should be 1.4.3, not 1.4.2. [Gillett Nathan, Canada]	accepted
106891	18	52	18	52	I would add the reference Vial et al (2018, https://doi.org/10.1029/2018GL078558) to illustrate the uncertainties in the forced response due to the phase of internal variablity , here associated with AMOC in this paper. [Christophe CASSOU, France]	This is a highly interesting study but too specific for this very general discussion of uncertainties. It should be referred to in a more detailed assessment of the relevant regional changes
26841	18	55	18	55	The linkage between this last sentence and the paragraph is unclear. The paragraph is about findng the different sources of uncertainties not on criteria to reduce uncertainties [Eric Brun, France]	Taken into account. The last sentence is deleted.
70885	18	55	19	1	This discussion is a bit rosy-tinted, in comparison with Chapter 1 where it is much more nuanced. Suggest a reference back to Chapter 1, and probably also to the recent paper of Hall et al. (2019 doi: 10.1038/s41558-019-0436-6) which is quite balanced. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Sentence referring to emergent constraints is removed.
132449	19	1	19	1	I suggest citing an emergent constraint review paper here (e.g., doi 10.1038/s41558-019-0436-6, 10.1007/s40641-015-0027-1), rather than these specific papers that may not be the best examples of the methodology. [Kyle Armour, United States of America]	Sentence referring to emergent constraints is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45539	19	3	19	17	Methodological advances have been made recently for better quantifying the relative part of internal variability in multi-model ensembles, even with a single run (Hingray et al., 2019), and even if these ensembles are incomplete (Evin et al., 2019). Hingray, B., Blanchet, J., Evin, G., Vidal, J.-P. (2019) Uncertainty component estimates in transient climate projections. Climate Dynamics 53, 2501–2516, https://doi.org/10.1007/s00382-019-04635-1 Evin, G., Hingray, B., Blanchet, J., Eckert, N., Morin, S. Verfaillie, D. (2019) Partitioning uncertainty components of an incomplete ensemble of climate projections using data augmentation. Journal of Climate, 32, 2423–2440, https://doi.org/10.1175/JCLI-D-18-0606.1 [Jean-Philippe Vidal, France]	Taken into account. The references are now referred to.
17005	19	10	19	10	GSAT (Lehner et al., 9999). [Sergio Aquino, Canada]	Corrected
19837	19	12	19	17	What is the meaning of internal variability? When focusing on the influence of humanity on climate change, internal variability is another name for noise, and it is of major importance to determine whether a signal can be detected in spite of this noise (and of weaknesses of the detecting devices). If one is however interested on projecting future climate with no emphasis on the anthropogenic influence, then it is different, because internal variability is the climate… [philippe waldteufel, France]	Noted. We refer to internal variability in the way it used in much of the assessed literature.
79671	19	15	19	15	Please also cite "Christensen, Berner and Yeager, "The value of initialisation on decadal timescales: state dependent predictability in the CESM Decadal Prediction Large Ensemble", in review, J. Climate." which assesses the state dependent limits of decadal predictability. [Hannah Christensen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. References added
26843	19	15	19	16	This might appear a little bit optimistic. If long term internal variability is not represented (long term due to ocean for example) even large ensemble cannot capture it and thereby quantify it. Idem if the internal variability is induced by ice-sheet instabilities not represented in most climate models [Eric Brun, France]	Taken into account. The statement is rephrased.
79703	19	15	19	17	I agree indeed but it need to be mentioned that the "accurate quantification" is subject to the ability of models to simulate the right amount and properties of internal variability. This perspective paper should also be cited : Deser, C., F. Lehner, K. B. Rodgers, T. Ault, T. L. Delworth, P. N. DiNezio, A. Fiore, C. Frankignoul, J. C. Fyfe, D. E. Horton, J. E. Kay, R. Knutti, N. S. Lovenduski, J. Marotzke, K. A. McKinnon, S. Minobe, J. Randerson, J. A. Screen, I. R. Simpson and M. Ting, 2020: Insights from earth system model initial-condition large ensembles and future prospects. Nat. Clim. Change, doi: 10.1038/s41558-020-0731-2 [Laurent Terray, France]	Taken into account. The statement referred to the fact that internal variability can be better quantified within individual models whereas it remains uncertain in reality.
24023	19	15	19	17	The role of internal variability will be overestimated if models underestimate the forced signals [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement is rephrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21629	19	16	19	16	Is accurately the right word here? It can be unambiguously quantified for sure but the quantity is still fundamentally predicated upon an assumption vis-à-vis the ultimate adequacy of the model which, tautologically speaking, is unprovable. So I think accurate is too strong an assertion here. [Peter Thorne, Ireland]	Taken into account. The statement is rephrased.
70887	19	16	19	16	"can be accurately quantitied" is a bit sweeping. I suppose you mean "in principle", i.e. given a sufficiently large ensemble? [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement is rephrased.
132451	19	19	19	20	Is it really impossible to attahc probabilities to future emissions scenarios? There are papers that do just this and seem very reasonable to me, e.g., doi: 10.1038/NCLIMATE3352 [Kyle Armour, United States of America]	Taken into account. Statement is removed and referred to the assessment in chapter 1.
127461	19	19	19	20	This line is ripe for misquotation. Uncertainty can of course be quantified (and the assessment does that well). Do the authors simply mean to say that scenario choices are not probabilistic? In which case just say "It is impossible to attach reliable probabilities to societal decisions (Schneider, 2001)". [Trigg Talley, United States of America]	Taken into account. Statement is removed and referred to the assessment in chapter 1.
114451	19	19	19	28	I support addressing this issue of plausibility etc here. But I suggest you contact WGIII authors from the xWG team on scenarios in order to get help with updates literature and consistency check and ref to what WGIII is doing. This does not need much space and will be a very valuable contibution to this chapter on scenarios. WIth the broad set of scenarios adressed (which has been subject to discussion in media already) this text can be a very useful clarification. [Jan Fuglestvedt, Norway]	Statement is removed and referred to the assessment in chapter 1.
21631	19	19	19	28	Future uncertainty also arises from unpredictable changes in solar and, in particular, volcanic forcings. [Peter Thorne, Ireland]	Taken into account. This is now referred to.
89849	19	19			It is too strong to say forcing uncertainty cannot be quantified. There is a plausible range of possibilities. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Statement is removed and referred to the assessment in chapter 1.
68653	19	20	19	20	Doesn’t this also include decisions on applying climate intervention methods? If so, it should be added in this paragraph. [Simone Tilmes, United States of America]	Taken into account. Statement is removed and referred to the assessment in chapter 1.
26845	19	34	19	34	wa suggest to add "when driven by large and increasing radiative forcing" after "horizons" . This would not be the case for long simulation of the last millennium [Eric Brun, France]	Rejected. This statement is clearly written in the context of future projections
7905	19	38	19	38	Please add the following sentence in line 38: "In addition, Sanjuán et al (2020) propos to include the net carbon dioxide emission due to the Portland cement production in the next generation of models in order to minimize the uncertainties found I the current models: Sanjuán, M.Á.; Andrade, C.; Mora, P.; Zaragoza, A. Carbon Dioxide Uptake by Cement-Based Materials: A Spanish Case Study. Appl. Sci. 2020, 10, 339. https://doi.org/10.3390/app10010339 Andrade C, Sanjuán MA. Updating Carbon Storage Capacity of Spanish Cements. Sustainability 2018;10:4806. https://doi.org/10.3390/su10124806 [Miguel Angel Sanjuán, Spain]	Rejected. This statement is too specific for the general discussion of uncertainties presented here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26847	19	40	19	40	It should also be stated somewhere that adding degrees of freedom in models naturally increases uncertainties.... then reducing uncertainties is not only having smaller error bars it is also having a better understainding of the relative contributions of different factors of feedbacks [Eric Brun, France]	Taken into account. It is not always correct that adding processes increases uncertainties as there are also compensating effects.
19839	19	40	19	42	The end of this sentence lacks meaning [philippe waldteufel, France]	Accepted. Sentence rephrased
42943	19	40	19	42	This sentence is hard to follow. Please rewrite [Rein Haarsma, Netherlands]	Taken into account. Sentence rephrased
127463	19	40	19	43	These sentences are confusing, and the Lehner et al reference needs an update. What does "at the expense of radiative forcing" mean? Do you mean that, in CMIP6, climate response and model uncertainty is larger but scenario uncertainty is smaller? [Trigg Talley, United States of America]	Taken into account. Sentence rephrased
44981	19	40	19	46	I guess the reference year for both "Lehner et al., 9999" and "Tokarska et al. 9999" will be 2020. Please check the published years throughout the text. [Mustafa Tufan Turp, Turkey]	Accepted. Corrected
106893	19	41	19	41	I don't understand this sentence "the climate response or model uncertainties in CMIP6 is larger than in CMIP5 at the expense…." [Christophe CASSOU, France]	Taken into account. Sentence rephrased
2371	19	41	19	42	missing 'of' after expense [Vaishali Naik, United States of America]	Taken into account. Sentence rephrased
17009	19	42	19	42	GSAT (Lehner et al., 9999). [Sergio Aquino, Canada]	Accepted. Corrected
17007	19	44	19	44	GSAT (Lehner et al., 9999). [Sergio Aquino, Canada]	Accepted. Corrected
17011	19	46	19	46	Tokarska et al., 9999 [Sergio Aquino, Canada]	Accepted. Corrected
52969	19	48	19	49	as well as groundwater feedbacks in most models (e.g., Smerdon 2017)? [Hervé Douville, France]	Taken into account and rephrased
96399	19	48	19	50	This sentence is difficult to read due to double negation "are not or only partially accounted for […] including missing lad-ice feedbacks […]". If possible, please adapt to positive statement. [Nicole Wilke, Germany]	Taken into account and rephrased
21633	19	48	19	51	Again, uncertain futures of natural forcings fall into this category. [Peter Thorne, Ireland]	Taken into account and added
42945	19	48	19	51	Deep uncertainties from unresolved physical processes, should also be mentioned. [Rein Haarsma, Netherlands]	Taken into account. Discussion has been expanded.
127465	20	25	20	25	Should be "fraction of models ARE broken". [Trigg Talley, United States of America]	Not applicable since sentence has been rephrased
67845	20	25	20	27	There is a need for consideration on some uncertainties (e.g. overestimation of model agreement and results of low-quality data) and how to improve the results. [Ruandha Agung Sugardiman, Indonesia]	Taken into account. Forcing and intermodel dependence are assessed elsewhere in the report. A quality statement has been inserted to address the issue of low quality data/ results
7245	20	25	20	27	How to account for this uncertainty (e.g. overestimation of model agreement and a low-quality results)? It is advised to also to discuss a way forward to tacke this uncertaintes issues and how to improve the robustness of the result. [Asaad Irawan, Indonesia]	Taken into account. Forcing and intermodel dependence are assessed elsewhere in the report including section 4.2.5 in this chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127467	20	28	20	30	The sentence could be improved to avoid misunderstanding. The phrase as currently written "mean change of a climate variable" is referring to a change in a variable's (spatial, temporal, or ensemble) mean value, and the chapter is noting that these quantities would not be sensitive to a change in the (spatial or temporal) variance of the variable. Please clarify what authors meant. [Trigg Talley, United States of America]	Not applicable. Sentence was rephrased and comment no longer applicable
114455	20	32	20	49	I think some coordination with WGII may be needed here [Jan Fuglestvedt, Norway]	Taken into account. The mapping and stippling is coordinated across WG1 with feedback from WG2
96401	20	34	20	34	Please state how the standard deviation of internal variability is calculated; time frame, daily/yearly values an so on, all models equally, model weighting… [Nicole Wilke, Germany]	Taken into account. Method is more clearly defined in the revised version.
42707	20	34			two standard deviations of internal variability' – can this be made more explicit? E.g. When expressing differences in 20-year means, it is the standard deviation of 20-year means from the historical control runs that is being used. There was some confusion about this point following AR5 and it would be helpful to be explicit as to what is done. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Method is more clearly defined in the revised version.
96403	20	35	20	37	The sentence is unclear, please rewrite. [Nicole Wilke, Germany]	Taken into account. Method is more clearly defined in the revised version.
104617	20	36	20	36	"model mean" should be "model mean change" [William Merryfield, Canada]	Accepted
52971	20	39	20	42	What about masking (no shading) the latter areas since it may be misleading to suggest that there will be no significant change while most individual models say the opposite but disagree about the sign and it may be wiser to highlight that we just don't know what the response will be? [Hervé Douville, France]	Taken into account. This point is referred to in more detail taking into account the manuscript by Zappa et al. (2021)
127469	20	39			"is used to interpret" should be "is used to interpreting" or "has experience interpreting". [Trigg Talley, United States of America]	Accepted. Corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70949	20	42	20	45	We are submitting a manuscript (G Zappa, E Bevacqua and TG Shepherd: "The real mean signal to noise of multi-model climate change projections") to Int. J. Climatol. in which we propose a new methodology to diagnose the robustness and magnitude of future projected changes from multi-model ensembles. Rather than basing the stippling and hatching of spatial maps on the signal to noise of the multi-model mean response - as it is standard in the IPCC, and in this chapter - our proposed approach evaluates the mean forced signal-to-noise of the individual model responses. This enables us to make statements on regions where a large future change compared to year-to-year variability is plausible, regardless of whether the mean signal is robust across the ensemble. While previously proposed alternative approaches were also able to discriminate between regions with a small response from those with large uncertainty, this method has the benefit of being as simple, and with as few free parameters, as the standard IPCC approach, while explicitly providing information that is relevant for risk assessment, i.e. the potential for a large change. For mean precipitation changes, we find that the majority (58% in surface area) of the unmarked regions and part (18%) of the hatched regions from the AR5 hid climate change responses that are on average large compared to the year-to-year variability. Based on the newer CMIP6 ensemble, we identify that a considerable risk of large annual-mean precipitation changes, despite the lack of a robust projection, exists over 21% of the global land area, mostly including Central America, Northern South America (including the Amazon), Central and West Africa (including parts of the Sahel) and the Maritime continent. You may contact g.zappa@isac.cnr.it for the submitted version of this paper. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The new paper is discussed in detail in the revised section and Cross-Chapter Box Atlas.1
105461	20	51	21	3	This is an important paragraph, please expand on how model projections are constrained (taking into account both model dependence and model performance). [Helene R. Langehaug, Norway]	Taken into account. This is the focus of Box4.1. which immediately follows 4.6.2
96405	20	53	21	1	We suggest to name the most important quantities that are now more physically consistent in the model (is it aerosol forcing? Ch3 P 4 L 5-7 state that no progress is found between CMIP5 and CMIP 6 regarding surface temperature pattern and biases. There seem to be major inconsistencies in the assessment or disagreement between authors of different chapters regarding the performance of CMIP6 models. Please revise. [Nicole Wilke, Germany]	Taken into account. This statement is coordinated with CH3
106323	21	5	23	43	I commend the authors for taking into account mutliple lines of evidence in their assessment of projections of future warming. One risk of this approach is that CMIP6 models are being dismissed as being unreliable. This is incorrect of course, but the ensemble they provide through CMIP6 is simply not fit for the purpose of being a formal distribution. Somewhere (and maybe it is already somewhere) a clear explanation should be provided of what CMIP6 model results do bring and where the strength of the CMIP6 ensemble lies. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FGD text has been checked to ensure that the continued use of CMIP6 is properly justified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115187	21	7	21	7	This discussion of uncertainty in the projections does not include uncertainties in climate-carbon cycle feedbacks, presumably because of the experimental design of using concentration-driven projections. This should be clarified here as it is not immediately obvious. [Richard Betts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Emissions-driven simulations are now included in Section 4.3.1, including a new figure, and the discussion of climate-carbon feedbacks as a source of uncertainty has been expanded in Section 4.2.5.
2933	21	7	23	46	This box should provided the names of 29 CMIP6 modles or Box4.1 Figure 1 should provide 32 CMIP6 results. [Zong Ci Zhao, China]	Rejected. The names of the models used in the figures will be included in figure data tables after finalising FGD. Listing them in the main text would make it unreadable.
45477	21	10	21	10	It is not clear to me what “defined the ensemble” is supposed to mean. Please word this more clearly. [Leonard Borchert, France]	Taken into account. Sentence rephrased.
127471	21	10	21	10	The phrase "and defined the ensemble" is confusing. Better to say "and used as part of the ensemble for evaluation of a scenario". [Trigg Talley, United States of America]	Taken into account. Sentence rephrased.
111959	21	15			scenario uncertainty declared above (p. 19, l. 19) it cannot be quantified …, which is in good accordance with its definition in Chap. 1, actually, choice of the radiative forcing scenario itself should not be considered as scenario uncertainty. [Tomas Halenka, Czech Republic]	Accepted. Section 4.2.5 has been modified to make it clear that the conversion from concentrations to ERF contributes to model uncertainty. No change here.
127473	21	18			Trends "in some variables". (not all) [Trigg Talley, United States of America]	Taken into account. Sentence rephrased.
26849	21	24	21	24	The" uncertainty quantification" should be more explicit as as it is done in the text above [Eric Brun, France]	Taken into account. Sentence rephrased.
127475	21	24	21	25	[PROGRESS] This was a choice made by the report's authors; it was not "necessary". And given the revisions to that calculation based on needed corrections to the global mean temperature observations that were known about at the time, it may have been premature. [Trigg Talley, United States of America]	Taken into account. Sentence rephrased.
96407	21	27	21	29	The sentence is difficult to understand - please clarify in a simpler way what is compared with what. [Nicole Wilke, Germany]	Taken into account. Sentence rephrased.
45805	21	42	21	42	Change "model independence" to "model dependence". [Twan van Noije, Netherlands]	Rejected. Independence goes in the same direction as performance in their effect on the weights and is hence preferred.
26851	21	45	21	46	i.e without using the results of the projections? This is unclear. The text is too complex in the next 2 pargraphs and it might be useful to rephrase it using the figure and explaining what is done [Eric Brun, France]	Noted. Intent of the comment unclear. The text seems straightforward enough. No change.
127477	21	46	21	46	[ENSEMBLES] Delete "yet". It is not at all clear that any universal robust method will ever exist. Assuming that it will emerge is wishful thinking. [Trigg Talley, United States of America]	Taken into account. Sentence rephrased.
89851	21	47	21	49	This statement is at odds with the assessment of GSAT projections so apparently applies only to projections of other variables, in which case they will be inconsistent with the GSAT projections. Please clarify extremely carefully. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The statement refers to a UNIVERSAL approach. No change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127479	21	48	21	49	[ENSEMBLES] This choice by the authors is very problematic. The range of CMIP6 models is not centered on the most likely assessed range of ECS. There is a significant number of models that exceed this on the high end, and only two that are lower. Simply using the range as if it was symmetric gives an upwards bias to most changes. [Trigg Talley, United States of America]	Taken into account. Unlike what seems to be implied in this comment, other quantities do not necessarily scale simply with GSAT change. The implication of the separate GSAT assessment for other quantities has been described more extensively in the FGD.
18985	21	51	22	3	It would be good to include in this box a cross-ref to the regional chapters & make the point that for everything regional/local combining differet lines of evidence including obs is essential. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The subject matter covered here is complementary to the regional chapters. It would not be efficient use of space to cover methodological aspects not applied here.
127481	21	52	21	52	[ENSEMBLES] The CMIP6 ensemble range is a moving target and will still be so when this report is finalized. Quantitative statements need to be qualified. [Trigg Talley, United States of America]	Noted. The diagnosed CMIP6 ensemble properties have largely stabilised by the FGD.
132453	21	54	22	1	Be sure to reference Chapter 7 here. [Kyle Armour, United States of America]	Accepted. Section 4.2.5 has been modified to make it clear that the conversion from concentrations to ERF contributes to model uncertainty. No change here.
11277	21		22		Please state clearly in this Box or 4.3 how the GSAT change in SSP simulations has been adjusted by referring to the ECS/TCR estimates in Chap 7. At l.12-18 on p.23, you explained the ECS/TCR ranges from Section 7.5 have been converted to GSAT ranges using an emulator, but did not discuss much how much the emulated ranges (as well as the central value) are different from those directly obtained from CMIP6 SSP runs. [Masahiro Watanabe, Japan]	Taken into account. The procedures have been explained in great detail both here (in principle) and in Section 4.3.4 (in practice). In the FGD, revision has been made for further clarity.
17013	22	14	22	14	(Maher et al., 2018, 9999a, 9999b; Deser et al., 9999; Lehner et al., 9999). [Sergio Aquino, Canada]	Taken into account. Production issue.
11275	22	14	22	52	Published year of some references (Maher et al., Tokarska et al., Smith et al., Sospedra-Alfonso and Boer) is missing [Masahiro Watanabe, Japan]	Taken into account. Production issue.
50791	22	15	22	16	Missing word:/'…transient climate response (TCR) are based on..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It's a list; the other items do not constitute complete sentences either.
114457	22	15	22	26	It shoudl be it clear to what extent the same emulators are used across chpater and if not, how consistency is secured. [Jan Fuglestvedt, Norway]	Taken into account. This has now been clarified through cross-chapter activities.
66985	22	15	22	26	The description is clear in terms of the GHG response, but reconstruction of GSAT response also requires to incorporate other forcings. How is it done? Do you assume that forcings efficacies are all equal to 1? [Aurélien Ribes, France]	Noted. Indeed that is the case.
23473	22	16	22	16	It is unclear why Collins (AR5) is referenced here. The assessment of ECS that is used in this chapter is purely that from AR6 (Chapter 7). I feel that Collins et al should be removed otherwise this is misleading. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference to AR5 Ch12 deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23475	22	16	22	16	It would be good to name the 4 main lines of evidence, rather than just saying "multiple lines of evidence". i.e. say "from process understanding, warming over the instrumental record, paleoclimates, and emergent constraints (Table 7.13)". This highlights that these lines of evidence are independent, and reminds the reader that CMIP6 models themselves are NOT part of this assessment, so there is no circularity. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and replaced, thank you.
87805	22	16	22	17	Please note that this sentence is confusing, as both ECS and TCR are originally defined in terms of GSAT in 1pctCO2 only simulations, so why do they need to be converted to GSAT? Or is this sentence referring to the observation-based estimates of TCR and ECS -then the input temperature could be already converted to GSAT without the need of an emulator? Please clarify. [Katarzyna Tokarska, Switzerland]	Noted. It is the ranges that are being converted, as the sentence says. No changes.
41397	22	17	22	26	The introduction of the EBM emulator is an important addition for the efforts to expand the lines of evidence underlying the CMIP6 assessment. This is a major step forward compared to AR5. However, the IPCC WGI reader is not familiar with the application of this Held 2010 type 2-layer EBM and may always think of the MAGICC/FaIR models when the term emulator is used. Please consider revising the term "emulator" to "energy balance emulator" or "EBM emulator" or similar to avoid confusion. This also holds for the important box figure where my emulator confusion was triggered first. [Alexander Nauels, Germany]	Noted. The use of emulators has been explained more systematically across the FGD. The SOD sentence referred to here was accurate; no change
26853	22	19	22	20	shoudn't uncertainties on f be considered in the reasoning? (or the pdf). The text should clearly state that this f2 is now well known (so indicate the error bar is a way to do it). Otherwise the distribution of f should be considered to produce the full range of plausible estimates [Eric Brun, France]	Noted. The method has not been changed, because although it is less comprehensive than what is suggested here, it is the simplest and hence the most transparent one.
11273	22	20	22	26	Please use the symbols same as in Chap 7: f_2x -> DF_2xCO2, l->a [Masahiro Watanabe, Japan]	Accepted and implemented.
50793	22	21	22	21	Please spell out what EBM stands for. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and implemented.
96409	22	21			For readability reasons please include the meaning of the abbreviation "EBM", as it is mentioned for the first time in this chapter. [Nicole Wilke, Germany]	Accepted and implemented.
26855	22	25	22	26	GSAT projections have been reconstructed from emulators and ECS/TCR ranges estimated in Chapter 7. Has it been verified that a GSAT projection from a climate model whose TCR and ECS are within the very likely ranges of Chapter 7 falls in the very likely range obtained from the emulators? Given the complexity of the procedure involved in 4.3.4 this is an indispensible sanity check. [Eric Brun, France]	Noted. This has been checked and confirmed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127483	22	30	22	30	[ENSEMBLES] "effectively the same model" needs qualifying. Two models with the same (or nearly the same) atmospheric model may have very similar atmospheric feedbacks, but different ocean components might make ocean impacts and changes very different. Similarly, the same ocean model has very little impact on the feedbacks in different atmospheric components. This is not a sufficient method to calculate the effective degrees of freedom in the ensemble. [Trigg Talley, United States of America]	Noted. This, however, is exactly the way in which Maher et al. (2020) have indeed re-sampled the models. No change.
79705	22	33	22	34	It is likely that accounting for a priori independence will not change the CMIP6 GSAT range. However large-scale regional changes can be much more sensitive to this. We are working on this with J. Boé, hope we can submit the paper very soon [Laurent Terray, France]	Noted. Inquiry with the reviewer has established that no such paper will be forthcoming in time for the acceptance cut-off. No change.
42709	22	35			'performance in simulating the past' – should it be mentioned here that some models were trained to reproduce the past record. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This level of detail would go too far in this summary. No change.
42711	22	35			Can 'a-posteriori independence based on a comparison with observations' be given a fuller explanation? What does it mean? [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This level of detail would go too far in this summary. No change.
87807	22	37	22	38	Please note that Brunner et al. (2020) also uses a similar method and arrives at similar conclusions. Perhaps it would be good to include a citation to that study too. "Reduced global warming from CMIP6 projections when weighting models by performance and independence." Lukas Brunner, Angeline G. Pendergrass, Flavio Lehner, Anna L. Merrifield, Ruth Lorenz, and Reto Knutti Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-23, 2020 [Katarzyna Tokarska, Switzerland]	Taken into account. Brunner et al. has been added.
12197	22	43			"kriging" refers to a simple interpolation method in most of the literature. [Bryan Weare, United States of America]	Noted. No change.
132455	22	46	22	46	I suggest citing an emergent constraint review paper here (e.g., doi 10.1038/s41558-019-0436-6, 10.1007/s40641-015-0027-1), rather than these specific papers that may not be the best examples of the methodology. Be sure to reference Chapter 7 at the end of this paragraph as well. [Kyle Armour, United States of America]	Taken into account. Brient review has been added. Chapter 7 as a reference might be misleading, because the GSAT assessment occurs not there but in 4.3.4.
17015	22	52	22	52	Boer, 9999 [Sergio Aquino, Canada]	Taken into account. Production issue.
116307	22		22		What about insights from paleo simulations (eg Eocene simulations, https://www.nature.com/articles/s41558-020-0764-6)? [Valerie Masson-Delmotte, France]	Accepted; thanks for pointer.
96411	23	4	23	4	These quantities are also assessed in Ch9, there is overlap between these chapters which please needs to be resolved, in particular in case of inconsistencies - please check. [Nicole Wilke, Germany]	Noted. Comment is puzzling since Ch09 does not assess internal variability in GSAT. No change.
26857	23	4	23	9	This should be written more simply, it is unclear why some of the large ensemble are isolated and the other 2 in the same sentence. It looks complex when the numbers and size of the ensemble could be given in one sentence with all 4 large ensembles [Eric Brun, France]	Noted. The large ensembles are used in different ways here, which is why they are not grouped together. No change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127485	23	4			The CANESM5 model whose LE is specifically used on Box 4.1. Figure 1 has a Gregory sensitivity of 5.6°C, well outside the assessed range. How does it make sense to use it in this figure? [Trigg Talley, United States of America]	Noted. CanESM5 is the largest ensemble in CMIP6; as the caption states, it is only its IV that has been plotted, not its forced response. No change.
17017	23	9	23	10	Figure 10 1:. [Sergio Aquino, Canada]	Noted. Meaning of comment completely unclear.
87809	23	12	23	13	Please note that this sentence is confusing, because the original ECS and TCR definitions are defined in 1pctCO2 simulations, and not SSP simulations that are subject to CO2 and non-CO2 forcing. Please clarify. [Katarzyna Tokarska, Switzerland]	Noted. It is the ranges that are being converted, as the sentence says. No changes.
87811	23	12	23	13	this is unclear how ECS contributes to the transient warming? Isn't the transient response dominated by TCR, since we are not at equilibrium state? Please clarify. [Katarzyna Tokarska, Switzerland]	Noted. Late-21st century warming correlates better with ECS than does TCR and than expected by many. No change.
114459	23	13	23	13	How sensitive is this to the choiec of SSP2-4.5 for this? [Jan Fuglestvedt, Norway]	Noted. SSP2-4.5 has been the standard scenario for the near-term predictions; No large sensitivity to scenario expected here.
71923	23	17		26	I think these plots sould be extended to 2300 so that the reader can immediately see the short and long term implications, rahter than having separate figures later. I also suggest the the beyond 2100 timescale is discussed immediately after the projections to 2100 rather than lateer in the chapter - the issues affecting projections to 2100 and 2300 are very similar and it should result in a more concise and informative discussion. [John Church, Australia]	Rejected. The long-term treatment has been expanded in FGD, but folding it in here would overburden the box.
26859	23	18	23	18	This is a very interesting new addition on the way to discuss future climate projections. However, there are a lot of assumptions in these constraints. The full reasoning should be easy to understand without digging into chapter 7. Also whatever is done to improve or correct the CMIP6 results, this is done without considering all possible feedbacks acting in the Earth Climate System, which should be added somewhere as an additional uncertainty. There is risk here to be overconfident in the likely (or very likely range) of the projections. A complete estimates should also consider model content and complexity, which is difficult since there is not necessarily a straihtforward relationship between model content/complexity and goodness of fit with observations or between model content/complexity and the estimated ECS and TCR. Also this discussion and all the reasoning to find the most likely range should not mask that these are only statistical estimates and there is still a non negligible chance to be outside the uncertainty range. The lower probability, but high risk cases should be better highlited in the chapter. [Eric Brun, France]	Noted. The assessment is based on the reasoning laid out in great detail here and reflects the authors' scientific judgement. No change.
6651	23	34	23	34	HadCRUT5 is not "observations". It is a dataset derived using observations and a number of assumptions, and includes estimated values for grid boxes that contain no usable observations. "observations" should be changed to "an observationally-based estimate" if only HadCRUT5 is shown, though please see comment 5. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and implemented.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87813	23	48	23	48	It would be helpful to discuss time-series together with trends for each component of the climate system. The current structure of this chapter (especially sections 4.3 and 4.4) is very repetitive and inconsistent. It is unhelpful to the readers to flip through many pages and several figures, to compare trends with time-series of the same variable. Furthermore, the figures showing time-series sometimes contain different amount of simulations as the figures of trends of the same variables. It would be helpful to see the trend plot as another panel on the same figure that shows the time series. It is also confusing that this section contains the same sub-section titles for discussing time-series and trends. Such respective sections should be merged, to keep the text shorter and consistent. Currently, this chapter is difficult to follow as related sections are scattered around. [Katarzyna Tokarska, Switzerland]	Noted. After much discussion, the chapter team decided early on to not organize the chapter on a variable by variable basis (i.e. variable X dealt with its entirety, then variable Y, and so on). Rather, the team decided on using Section 4.3, and Figure 4.1 in particular, to illustrate in a holistic way the connected changes in the main key indicators across the main realms in the climate system. Subsequent sections go into more detail, e.g., showing trends, pattern changes, etc. This approach leads to some repetition which we are striving to minimize. To help better guide the reader through this structure we have introduced a visual table of contents.
21639	23	50	24	2	While this text is nice I'm not sure it adds terribly much and if space is tight it could easily be deleted without negative impact. Section 4.1 already did a good overall signposting and I'm not sure you need this or other within section signposts in addition. [Peter Thorne, Ireland]	Rejected. We feel that there is enough distance between Section 4.1 and 4.3 that this additional signposting would be welcomed by the reader.
18987	23	50	24	12	refer to chapter 11 for projection of changes in global extreme indices [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and included.
18991	23	50	24	12	Here, or somewhere else in chapter 4 information on how time horizon projections can be transferred to warming levels (and for what variables & in particular for which not) and vice versa [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section 4.6.1 discusses the patterns of climate change for specific warming levels. It is also possible to visualize the times of warming level exceedances in Fig. 4.1 (see axis in the right), and relate these to the time horizons consider in the report.
127487	23	50	24	12	[ENSEMBLES] The use of the term 'assessment' in this paragraph (and later in this section) might lead the reader to assume some QC has been applied to the CMIP6 ensemble. But this is not the case, the referenced figures are simply the CMIP6 ensemble without any further processing. [Trigg Talley, United States of America]	Rejected. Not all of the variables considered in Section 4.3 are based solely on the CMIP6 archive. GSAT, for example is assessed using multiple lines evidence; and all the variables are evaluated in terms of CMIP6 (and other) simulations together with the expert judgement of the author team.
74207	24	4	21	4	it is unclear to what the percentage number relates to. One could be the wrong impression that it is to the ocean inventory. [Christoph Völker, Germany]	Not applicable. The page and line ranges do not makes sense.
50795	24	8	24	8	SSP2-4.5 (middle-of-the-road)' - this description could perhaps be misinterpreted to mean SSP2-4.5 represents a scenario with lowest mitigation effort. This could be reframed to something like 'medium mitigation pathway'. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is standard terminology.
127489	24	10	24	12	Use parentheses consistently (e.g., also around 1850-1900). "is also assessed" should be "are also assessed." [Trigg Talley, United States of America]	Accepted.
69941	24	12	24	12	It needs spacing between "1850-1900" and "is" [Young-Hwa BYUN, Republic of Korea]	Accepted.
71271	24	17	24	17	In the caption, it is better to add explanation of dashed line in Fig. 4.1. [Kenji Taniguchi, Japan]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41399	24	17			Please consider also adding observational records here for validation, similar to the SPM. If it remains impossible to show total SLR under SSP1-19 (it shouldn't be), please specify in the caption why. Also, it remains unlcear to me why you focus on SSP1-26 instead of SSP1-19. SSP1-19 is the only PA consistent pathway and therefore of utmost policy relevance. Policy makers would want to know the uncertainty ranges of SSP1-19 projections much more than SSP1-26. The readers would at least have to know about the TIER1/2 rationale responsible for limited SSP1-19 availability. This comment holds for all similar chapter figures. [Alexander Nauels, Germany]	Taken in account. 1) Earlier on, the decision was made by the chapter team to not include observations in this figure, or in any of the other figures of the chapter. 2) SSP1-1.9 dis now shown. 3) After much discussion between the chapter, the decision was made to highlight with shading SSP1-2.6 (as a low emission scenario) and SSP3-7.0 (as a high emissions scenario). SSP5-8.5 has been deemed highly unlikely, and SSP1-1.9 has too few simulations to obtain robust uncertainties.
19841	24	23	24	23	"r1" is only defined later, in subsection 4.3.1.1 [philippe waldteufel, France]	Accepted.
127491	24	24			Given the small number of ensemble members, isn't the t-test distribution more appropriate? Thus the 1.645 factor should be larger (~1.75 or so depending on the number of members) in each SSP. This follows for many other figures as well. [Trigg Talley, United States of America]	Accepted.
84237	24	31	24	32	this sentence is quite generic here [Annalisa Cherchi, Italy]	Rejected. Links to relevant chapters are essential.
106907	24	35			I think that all the figures given in tables in this section are misleading because, while the Ensemble Mean might be a good estimate of the forced signal, the likely range might be very much polluted by internal variability, especially at near term and for low-level warming scenarios. The likey range in that case should not be interpreted as the likely range of the forced reponse. Therefore the nature of uncertainties given in parenthesis is very different accross this period/scenario matrix, being dominated by internal variability at near-term and in low-warming scenarios, and on the other hand being dominated by model uncertainties for later leadtime and strong warming levels. I think that we could do much better in AR6 using all possible members and also technics to quantify both sources uncertainties as a function of leadtime and scenarios. The actual formulation is too me conservative and to say it in a provocative way, can be interpreted as a simple buisiness as usual way to update IPCC reports. [Christophe CASSOU, France]	Rejected. There is nothing misleading here in that we have been very open about our one-model one-realization approach which has been discussed and justified in Box 4.1. We note that the one-model one-realization approach was also used in the WGI AR5 Report.
106909	24	35			For communication issue, it would be great to recall and state clearly at the beginning of the paragrah or somewhere else in a visibile and prominent place in the Chapter, that what is shown here through ensemble mean is an estimation of the forced response and not what the observed climate is expected to follow. In the general public, the confusion is almost systematic and climate-deniers regularly used this trick (see for instance the crasy buzz about the last hiatus). [Christophe CASSOU, France]	Rejected. In all cases we show or quote ensemble means and 5-95% uncertainty ranges, and we discuss the role of internal variability (and other sources of uncertainty) early in the chapter (see Section 4.2.5) and throughout
71927	24	37	25	40	I take it, and I think it should be made clear, that no account of the higher climate sensitivity of the CMIP6 models is considered here. I presume this will also apply to many other subsections. [John Church, Australia]	Rejected. All available CMIP6 models have been utilized. We see no need to specifically call out the higher sensitivity models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106897	24	37			This section gives the estimations of the expected changes for atmospheric fields based on the figures drawn specifically for the report. There are very very few references provided in this section which is essential for the report. The lack of reference is a major concern for me as we are doing an assessment of literature.I would strongly recommend to add the latest references available because our assessment must be anchored to published papers. A related question: how confidence levels are estiablished based on the sole figures provided drawn specifically for the report? [Christophe CASSOU, France]	This is an introductory/overview section describing the latest projections of a selection of key indicators of climate change. Subsequent sections and chapters undertake a detailed assessment of the literature. The figures in this section are straightforward calculations based on the CMIP6 archive. As in the AR5, we define the "likely" range as the 5-95% range across the simulated values. This definition accounts for the fact that the 5-95% range is probably an underestimate of the true uncertainty.
106895	24	39	24	39	Add concentration after GHGs because it is misleading and could be intepreted as emission. [Christophe CASSOU, France]	Accepted.
11507	24	39	24	40	"The AR5 assessed from CMIP5 simulations that GSAT will continue to rise over the 21st century if GHGs continue increasing." While the sentence is not wrong as such, and makes sense with respect to what follows, I think that the overall assessment of AR5 that temperature would continue to rise with increasing GHGs was based on more than just CMIP5 simulations. [Gerhard Krinner, France]	Accepted.
15489	24	39	24	49	The temperature projection presented in AR5 is Global Mean Surface Temperature (GMST), not Global Surface Air Temperature (GSAT) (Ref.: AR5 WGI SPM, P.21, Table SPM.2). In particular, the 5-95% projection ranges for the four RCP scenarios refer to GMST instead of GSAT. Please revise. [SAI MING LEE, China]	Rejected. The numbers quoted here are from Chapter 12 of the AR5 WGI Report where GSAT not GMST is assessed.
45483	24	39	25	40	There is no explicit discussion of the different warming projections between CMIP5 and CMIP6 here. Should they be briefly discussed? [Leonard Borchert, France]	Accepted.
127493	24	39			"assessed" should be "concluded". [Trigg Talley, United States of America]	Rejected. That was their assessment.
50797	24	43	24	43	Was it 'GSAT' in the AR5, or GMST? It would be helpful to report both values here for completeness. If possible please could you also include observed global surface temperature rise relative to the pre-industrial (for improved global policy relevance)? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. GSAT was reported in the AR5.
96413	24	51	24	56	For continuity's sake in AR5 and AR6 the same reference periods should please be used, preferably 1961-1990. When introducing new reference periods, please provide an "offset" so that users can convert between reference periods. [Nicole Wilke, Germany]	Rejected. It was a cross report decision to use these new time periods. Quoting results for both times periods for all quantities assessed would be extremely cumbersome.
127495	24	51			[ENSEMBLES] The authors need to include a statement here related to the date that the CMIP6 ensemble was sampled (since this will continue to change over time). [Trigg Talley, United States of America]	Noted. A full accounting of the date and model files has been provided during the corrigendum/copyedit stage.
45807	24	52	24	53	I wonder what the advantage is of using the r1 member only. Wouldn't it be more representative to use for each model the average over all available members? [Twan van Noije, Netherlands]	Noted. As discussed in Box 4.1 there is not an established approach. For simplicity and for consistency with the AR5 Report we elected the one-model one-realization approach.
45479	24	53	24	53	Can it be assumed to be clear to every reader what an “r1” simulation is? [Leonard Borchert, France]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127497	24	53			[ENSEMBLES] Some care needs to be taken with the selection procedure for the CMIP6 runs. The additional varying parameters (p and f in particular) may be of relevance. Where models have a variation of these, some thought needs to be given, perhaps in consultation with the model group, which one (or more) simulations should be included. For instance, if f2 runs have improved forcing, r1i1p1f2 might be the better ensemble member. Differences in p-value may be worth including as separate members in the selection. [Trigg Talley, United States of America]	Taken into account. Selection has been clarified.
41401	24	54	24	56	Given the shift to historical GSAT and the new reference period, the reader would not only want to see the projections relative to "pre-industrial" in the figure but also in the table. In fact, judging from the way this topic is currently dealt with in the SPM, it is possible to become very problematic as the new oberved GSAT record plus the projections presented here, in particular for SSP1-19 and SSP1-26, will have lost the line of sight to AR5, and therefore the state of knowledge informing the Paris Agreement. Please coordinate closely with SPM authors on how to put these new (and 0.1 degC warmer) projections (rel to 'pre-industrial') into a PA context. [Alexander Nauels, Germany]	Taken into account. Our Chapter is presently overlength and it would be hard to justify doubling the size of Table 4.2. Instead we are now providing the GSAT offset that can be added to obtain anomalies relative to 1850-1900.
115193	25	3	25	7	It's good to see the timing of peaks in CO2 concentration mentioned here. Please include a figure showing the pathways of CO2 concentrations used in the projections. So far the report only shows emissions pathways, which is not the whole story. [Richard Betts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is done elsewhere in the Report.
11509	25	3	25	9	This paragraph is a bit painful to read, and the data are given in Table 4.2. Do you really need to state these numbers here? [Gerhard Krinner, France]	Rejected. Agreed, but we think it is important to summarize the anomalies displayed in Table 4.2 and put them into the context of the underlying CO2 scenarios.
11511	25	3	25	19	Readers might want to interpret the 5-95% range as the very likely range. Maybe, to prevent confusion, you could write explicitly (again) here why this is just a 5-95% model range with a confidence statement, not an assessed likelihood? [Gerhard Krinner, France]	Accepted.
50799	25	4	25	4	5-95% range - is this the very likely range? If so please specify this here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is the likely range.
114461	25	12	25	20	I automatically expected a table with numbers relative to 1850-1900. I suggest you say that a atble with such number will come later, and add a ref to that. [Jan Fuglestvedt, Norway]	Taken into account. Our Chapter is presently overlength and it would be hard to justify doubling the size of Table 4.2. Instead we are now providing the GSAT offset that can be added to obtain anomalies relative to 1850-1900.
87815	25	14	25	14	Table 4.2. Please clarify and explicitly indicate in the caption that these are GSAT values? Also, please clarfiy how many CMIP6 models were used in each simulation to calculate the mean (or refer to the relevant section in the technical summary) [Katarzyna Tokarska, Switzerland]	Accepted.
21641	25	14	25	17	Should the caption note the chapter 2 assessed change between 1850-1900 and the modern baseline for completeness? [Peter Thorne, Ireland]	Noted. We don’t see the relevance here, but this is a topic covered in Subsection 4.3.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12199	25	14	25	19	As with Table 4.6 this table needs estimates of the year in which the 1.5, 2 and 3 are exceeded. Only then is the corresponding text clear. [Bryan Weare, United States of America]	Noted. Instead of significantly increasing the size of Table 4.2 we have used the text to indicate these years of exceedance.
84239	25	14	25	20	for the values in Table 4.2 and reported in the text it should be specified how many models have been used for the estimates [Annalisa Cherchi, Italy]	Accepted.
71925	25	14		18	Add the projections to 2300 as another line. [John Church, Australia]	Noted. This is dealt with in Section 4.7.
41403	25	14			As these estimates are crucial input for the SPM, information has to be given on the 1850-1900 to 1995-2014 ("pre-industrial") warming that has to be added to the provided projections. These combined estimates will be under extreme scrutiny from governments and it should be very clear why projections have changed and how to relate them to the AR5 (informing the Paris Agreement). [Alexander Nauels, Germany]	Accepted. The translation value is now provided in the text and caption Fig. 4.1.
42713	25	14			Could an additional table be included that provides, as best can be done, a comparison of surface air temperature projections with AR5 results? Even accepting the different scenarios, because of the different reference periods used in AR5 and AR6 this is not trivial. Users will end up trying to do this themselves and it's better that the report provides a consistent and definitive comparison for the same reference period. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This chapter is well over length and while this comment is well taken we just do not have the space for additional table.
106899	25	23	25	23	I am afraid that "unamimity" is a too strong adjective because the internal variabiity is not correctly assessed when using only one member while internal variability is very important for low warming levels. I would strongly recommend to use all the members avaliable for scenario and provide a likelyhood statement based on the calibrated language of IPCC. [Christophe CASSOU, France]	Rejected. There are strong drawbacks to the procedure proposed by the reviewer, such as inhomogeneity of the sampling procedure. The sentence as stated is robust.
69943	25	23	25	26	I think It would be better to understand sentences if the years when GSAT anomalies reach to 1.5℃/2.0℃/3.0 warming relative to pre-industrial condition(1850-1900 average) are also added into Table 4.2 [Young-Hwa BYUN, Republic of Korea]	Noted. This information already exists in Subsection 4.3.6.
21643	25	23	25	31	I think a more thorough assessment of this key policy question is required than is presently undertaken here. The time to crossing and probability of crossing thresholds is key. Uncertainty ranges need to be calculated and these should include the uncertainty in where we stand today arising from the chapter 2 assessment. [Peter Thorne, Ireland]	Noted. This information already exists in Subsection 4.3.6.
106901	25	23	25	31	I may have missed it but if not, how is defined the threshold on exceedance? First year? Average over a temporal window? [Christophe CASSOU, France]	Noted The methodology in the caption to Table 4.3.
106905	25	23	25	40	Could any link be made here with DCPP results? Smith et al. (2019) should be cited. In any case, decadal forecast should be mentioned/assessed here. [Christophe CASSOU, France]	Noted. This is the subject of Section 4.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50803	25	28	25	31	"In summary, we conclude that within the near-term (2021–2040) or mid-term (2041–2060), global temperature rise simulated by the CMIP6 models exceeds 1.5°C relative to 1850–1900 under most of the SSPs considered here, and above 3.0°C under the highestforcing scenario" - this final sentence in the paragraph is important and policy relevant - can it be emboldened and perhaps given a separate paragraph? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
106903	25	28	25	31	The sentence "in summary…" is very awkward and vague while it is important statement. I would completely rephrase it to be more precise. In addition, does the confidence level apply for both conclusions? Same remark for precipitation later in the following section. [Christophe CASSOU, France]	Noted.
45481	25	28	25	31	Is there a difference between the temperature increases for the near-term and mid-term time windows that is worth mentioning here? [Leonard Borchert, France]	Taken into account. Since the assessment of GSAT change is based not only on CMIP6, the difference between periods should not be expanded upon here.
127499	25	28			[CONFIDENCE] These are odd statements to be "concluding". They are simply arithmetic on the CMIP6 models, there is no concluding to be done. The answer (given the specific selection procedure for the simulations) cannot be otherwise. The authors should simply state the result. Thus delete "we conclude that". [Trigg Talley, United States of America]	Taken into account; sentence rephrased.
50805	25	36	25	36	Suggested addition to clarify warming is relative to the global average: that the Arctic region will warm about twice as much as the average over land..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
71273	25	38	25	38	Minimum for SSP5-8.5 is 3.1 degC in Table 4.2. [Kenji Taniguchi, Japan]	Accepted.
79707	25	38	25	40	Numbers 3.2°C and 14.6°C are not exactly the same as in table 4.2 . I do not really understand why there is a "high confidence" statement at the end of the paragraph. [Laurent Terray, France]	Accepted. 1) the number have been corrected. 2) the confidence statement has been removed since the likely range is quoted.
50801	25	Table 4.2	25	Table 4.2	Is there a reason for the commas between the numbers in brackets, rather than hyphens (this is also in the SPM)? Please clarify that these numbers reflect the 5-95% ranges of CMIP6 temperature projections. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. 1) Using hyphens introduces a problem when negative numbers are involved (not in this table, but in other tables in this section). Besides which, this was the presentation used in the WGI AR5 Report. 2) The caption clearly states that these are 5-95% ranges.
127501	26	3	26	6	Indicate whether the increase is relative to PI or begining of the RCP to facilitate comparison with AR6 numbers. [Trigg Talley, United States of America]	Accepted.
52183	26	5	26	8	Paragraph mention changes in the suitable areas for agriculture and mention increases in the Northen high latitud areas by 16% or 5.6 million km2. However in therms of loss of agriculture areas in tropical regions it doesn't mention any percentage or area. We recommend this information to be included for deeper understanding of the diferences. [Maritza Jadrijevic Girardi, Chile]	Rejected. This sort of impact information is not in the mandate of Chapter 4.
74347	26	11	26	11	It is more interesting to show figures of land precipitation from several regional as an example [Yulizar Yulizar, Indonesia]	No applicable Regional maps of land precipitation are shown later in the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17019	26	11	26	11	(see Figure 4.1:) [Sergio Aquino, Canada]	No applicable Regional maps of land precipitation are shown later in the chapter.
89279	26	15	26	18	The number of 5–95% range for SSP1-2.6 (0.8–5.6%) is not consistent with the number in Table 4.3 (0.7, 5.6). [Tinghai Ou, Sweden]	Accepted.
79709	26	16	26	16	range 0.9-4.2% does not correspond to the numbers in table 4.3 [Laurent Terray, France]	Accepted.
71275	26	16	26	18	For SSP1-1.9, range of precipitation variation is 0.6-4.8 % in 2081-2100 (Table 4.3). In 2021-2041, 0.9-4.2%. For SSP1-2.6 in 2081-2100, 0.7-5.6 % in Table 4.3. [Kenji Taniguchi, Japan]	Accepted.
127503	26	17			[CONFIDENCE] The use of "high confidence" classifiers for arithmetic is odd. Given the selection of CMIP6 runs, the 5-95% range of metric X in an SSP is an exactly known quantity. Statements of what the CMIP6 models show should not be caveated in this way. [Trigg Talley, United States of America]	Accepted. Good point. The confidence classifiers have been removed.
132457	26	20	26	21	This is an awkward sentence. Perhaps drop "unanimity across" and it will be fine. [Kyle Armour, United States of America]	Rejected. We do not see this sentence as awkward.
21645	26	20	26	29	As written it is unclear whether the numbers in this paragraph equate to the projected change at the time of crossing such thresholds or rather are just the averages of those runs exceeding such thresholds. This presumably matters hugely for interpretation. I am assuming from the table caption that this is somehow value at / near time of threshold exceedance but that should be made clearer accordingly in the text either way. [Peter Thorne, Ireland]	Accepted. Good point. These are averages only over the model where 2.0C is exceeded.
104619	26	20	26	29	This paragraph does not clearly convey that the stated changes relate to the time of exceedance for the various temperature thresholds. Recommend therefore replacing "since 1850–1900" in line 22 with "from 1850–1900 until the time of exceedance". [William Merryfield, Canada]	Accepted.
71277	26	26	26	26	"3.5%" -> "3.9%" ? Or the value in Table 4.3 (for SSP1-1.9, dT>2.0degC) is incorrect? [Kenji Taniguchi, Japan]	Accepted.
104621	26	26	26	29	The last sentence of this paragraph seems to indicate that the times of 1.5°C and 3.0°C exceedance always occur within the near-term (2021–2040) or mid-term (2041–2060). However, Fig. 4.1a indicates this is not the case, particularly with respect to the 3.0°C threshold. [William Merryfield, Canada]	Accepted. This sentence is unclear and unnecessary and has been removed.
45485	26	26	26	29	The way the concluding sentence of this paragraph is phrased suggests a sudden increase in precipitation when a certain temperature rise threshold (1.5 or 3.0 degrees) is crossed. Surely, this is not what's meant here. Please rephrase to avoid ambiguity. [Leonard Borchert, France]	Accepted. This sentence is unclear and unnecessary and has been removed.
11279	26		27		I think you should assess not only the precipitation change (in mm/day or %) across SSP scenarios but also the hydrological sensitivity (in %/K) using all SSPs, so that readers can compare your assessment with AR5 (Figs. 12.6 and 12.7). [Masahiro Watanabe, Japan]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32693	27	1	190	55	It is suggested that the following sections be included in the self-regulation systems headings: If a large volume of the Earth's ice melts, it will deactivate the ocean cycle, and as a result, heat will not be transferred to higher latitudes, which will cause the northern latitudes to cool and thus start a cold period in the world. [sadegh zeyaeyan, Iran]	Rejected. This is outside the mandate of Chapter 4.
33023	27	1	190	55	It is suggested that the following sections be included in the self-regulation systems headings: If a large volume of the Earth's ice melts, it will deactivate the ocean cycle, and as a result, heat will not be transferred to higher latitudes, which will cause the northern latitudes to cool and thus start a cold period in the world. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. This is outside the mandate of Chapter 4.
132459	27	4	27	12	Reference Chapter 8 for an explanation of these patterns. [Kyle Armour, United States of America]	Accepted.
65693	27	4	27	12	Suggest including projections for the southern hemisphere. Currently only northern hemispheric projections are provided. [Kushla Munro, Australia]	Rejected. Space limitations have prevented us from detailing the precipitation changes over the Southern Hemisphere. Subsequent chapters do though provide this level of regional information.
84241	27	4	27	24	the rationale of fig 4.2 and relative assessment is not clear. Why selection of these two regions? [Annalisa Cherchi, Italy]	Rejected. The rationale is to contrast to regions of opposing change. We think this is clear as written. Note the addition of a new sentence at the end of this paragraph that makes the rationale even clearer.
106911	27	5	24	5	Why medium confidence? Justication to choose the levels of confidence are missing. [Christophe CASSOU, France]	Accepted. This has been changed to "high confidence" to reflect the likely range that has been established.
28823	27	5			The larger increase in precipitation over land than ocean is dominated by rapid atmospheric adjustments with the amplified land response offset by a slower response to warming than over the ocean as determined by idealised modelling (e.g. Samset et al. 2017 Clim. Atmos. Sci.). This will need to be made clearer in 8.2.1 so that we are consistent with this chapter. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
106913	27	6	27	6	Add extratropics in the text to be consistent with Figure 4.2 [Christophe CASSOU, France]	Accepted. That you for noticing this. "extratropical" has been added
21647	27	6	27	9	This sentence makes no sense as written and requires revision [Peter Thorne, Ireland]	Accepted. That you for noticing this. "global" has been removed.
17021	27	8	27	8	(Figure 4.2:; high [Sergio Aquino, Canada]	Accepted
52973	27	11	27	12	, thereby highlighting the potential limitations of pattern scaling for regional hydrological changes (see also Section 8.5.3). [Hervé Douville, France]	Accepted. Good point and addition.
11513	27	27	28	40	It's perfectly fine that the cryosphere is reduced to Arctic sea ice here. It might be useful, however, to add one little sentence that projections for other cryosphere elements (Antarctic sea ice, ice sheets, glaciers, snow, permafrost) are given in Ch 9. (Similar for other ocean and BGC variables and the near-term section) [Gerhard Krinner, France]	Noted. This subsection focuses on limited number of key indicators of global climate change. Chapter 9 deals with glaciers and ice sheets and implications on global sea level.
111961	27	27	31	36	In the Section titled Cryosphere, Ocean, and Biosphere one should miss the projections for glaciers and ice-sheets, actually, one of key factors for sea level projections, which are here [Tomas Halenka, Czech Republic]	Noted. This subsection focuses on limited number of key indicators of global climate change. Chapter 9 deals with glaciers and ice sheets and implications on global sea level.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127505	27	31	27	35	Rather than "for RCPxy", it should be "under RCPxy". [Trigg Talley, United States of America]	Accepted.
39105	27	31	28	2	Update SIA assessment with findings from Notz et al 2020. Notz, Dirk, et al. "Arctic Sea Ice in CMIP6." Geophysical Research Letters (2020): e2019GL086749. [Ola Kalen, Sweden]	Accepted.
45487	27	32	27	34	I think it should be "...range from 43% for RCP2.6 to 94% for RCP8.5… 8% for RCP2.6 to 34% for RCP 8.5..." [Leonard Borchert, France]	Accepted.
84243	27	35	27	35	to what percentage does correspond the drop of "below 1 million km**2"? In previous lines informtion are given in % and are not easily comparable with this number here. [Annalisa Cherchi, Italy]	Noted.
52975	27	43			solely according to their simulated trends, especially if there is no successful attribution of the observed trend to the GHG forcing.? [Hervé Douville, France]	Noted.
52977	27	44			Do you mean "magnitude" and/or "spread" instead of "quality"? [Hervé Douville, France]	Accepted and modified. . Good point.
84245	27	46	27	46	I don't see the expansion of the acronym SIA [Annalisa Cherchi, Italy]	Rejected. Defined on line 4 of Introduction.
50807	27	49	27	50	The definition of 'ice free' here is included quite far into the chapter. A definition is also not included in the SPM. Please include the definition of this from the outset of the Chapter/SPM. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
52023	27	49	28	16	The terminology used for an "ice-free" Arctic in this section is not consistent. On P27L49 you say "Arctic is considered ice-free with coverage below 1 million km2" but on p28L16 "near ice-free Arctic" is used. I recommend to change both of these to "practically ice-free" to be consistent with the terminology used in the SIMIP paper and in Chapter 9. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you for this suggestion.
52027	27	54	27	54	The language used here ("simulations show an ice-free Arctic in September before 2050") makes it sound like the Arctic will be permanently ice-free in September once it crosses that threshold. In fact the models show considerable inter-annual variability and so only predict an 'occasional' ice-free Arctic. In the SIMIP paper - and associated media reports - we have said that "Arctic sea ice will likely disappear occasionally in summer even before 2050", which might be more accurate wording to use here. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you for the suggested wording change.
132461	27	54	27	55	It is strange that Arctic sea ice is assessed relative to cumulative carbon emissions, when all other variables in the chapter are assessed relative to global temperature change. I realize there is a paper that links sea ice to cumulative carbon, but I suggest you instead describe sea ice loss as a function of global temperature for consistency, or else describe all other changes as a function of cumulative carbon as well. [Kyle Armour, United States of America]	Accepted.
45489	27	54	27	55	Surely, the amount of cumulative anthropogenic CO2 emissions needed to melt September Arctic sea ice also depends on the emission intensity, i.e. how quickly that level of emissions is reached? I am not convinced the information about the cumulative emission 'target' for an ice free Arctic is helpful here. [Leonard Borchert, France]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17023	27	55	27	55	(SIMIP Community, 9999) [Sergio Aquino, Canada]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
116309	27		27		It could be good to better link the model evaluation to outcomes of chapter 3 rather than duplicating efforts. References to SROCC as also missing in section 4.3 (what differs? Why?) [Valerie Masson-Delmotte, France]	Rejected. This paragraph is specific to model subsetting which needs to be assessed in this section where model subsetting is not employed. Chapter 3 has only one sentence on this subject and so duplication is not a major issue. On the second point, a reference to the SROCC has been added.
79501	27				It is suggested that the following sections be included in the self-regulation systems headings: If a large volume of the Earth's ice melts, it will deactivate the ocean cycle, and as a result, heat will not be transferred to higher latitudes, which will cause the northern latitudes to cool and thus start a cold period in the world.(comment by: sahar.maleki@ut.ac.ir) [Hanieh Zargarlellahi, Iran]	Rejected. This is outside the mandate of Chapter 4.
84247	28	1	28	1	would it be possible to estimate (have a number) for what "much lesser degree" means? [Annalisa Cherchi, Italy]	Rejected. These numbers are readily apparent from the Table.
132463	28	1	28	2	I don't think it's true that March SIA decreases "to a much lesser degree" than September SIA in the future. Table 4.4 shows comparable declines in March and September, with March SIA decreasing by even more in some scenarios and time periods. [Kyle Armour, United States of America]	Taken into account. Thank you for spotting this. It should have been mentioned that this statement refers the percentage change.
102929	28	5	28	5	Table 4.4 For some SSP the Arctic Sea Ice will disappear in September. In the table, the results are give in Area-unit, and the ensemble standard deviation shows an internal, with negative area. I understand the mathematical meaning, but looks simply wrong. [Philippe Tulkens, Belgium]	Accepted. 5-95% ranges based on percentiles are now used thereby solving the issue of "negative" ice area.
87539	28	5	28	5	Table 4.4 For some SSP the Arctic Sea Ice will disappear in September. In the table, the results are give in Area-unit, and the ensemble standard deviation shows an internal, with negative area. I understand the mathematical meaning, but looks simply wrong. [Valentina Roberta Barletta, Denmark]	Accepted. 5-95% ranges based on percentiles are now used thereby solving the issue of "negative" ice area.
12201	28	5	28	11	This important table does not not convey at all well the substantial decrease in September from either preindustrial or 95-14, given the dramatic recent drops. As it is, it suggests that there is no change in the September near-term. As with other tables this one should show differences from the 95-14 average with a way of noting that ice free is represented by a decrease of say 3 based on Fig. 4.1c. [Bryan Weare, United States of America]	Rejected. We would prefer to display absolute values rather then differences in order for consistency with Fig. 4.1c.
6653	28	5	28	12	Arctic sea-ice area cannot be negative, yet there are negative entries in Table 4.4. The calculation of some of the 5% values must be inappropriate, as it gives unphysical results. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. 5-95% ranges based on percentiles are now used thereby solving the issue of "negative" ice area.
52025	28	7	28	10	I find the fact that the parentheses contain areas below zero for September rather odd. Given that sea ice cannot be negative, does it make sense to use a Gaussian 90% CI here? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38315	28	7	28	11	The change in sea ice area can be negative, but the minimum area itself can only be zero and cannot be negative. In Table 4.4, there are several negative values in the estimated Arctic sea ice area. It is suggested to modify the statement or give explanations. [Yaming LIU, China]	Accepted. 5-95% ranges based on percentiles are now used thereby solving the issue of "negative" ice area.
130501	28	7	28	11	In table 4.4, Arctic sea ice area projections are negative in many places. Why? [Panmao Zhai, China]	Accepted. 5-95% ranges based on percentiles are now used thereby solving the issue of "negative" ice area.
84249	28	7	28	12	would it be possible to have the number of models used for the estimates given in Table 4.4 within the table? [Annalisa Cherchi, Italy]	Accepted. The number of models are shown in Fig. 4.1.
12203	28	7	29	13	This paragraph is of little value with no citations and no conclusions concerning the "advances" [Bryan Weare, United States of America]	Accepted.
46591	28	10	28	10	It might be helpful to add the number of available models for each scenario (n=…) given that there probably is quite a large spread in available models [Dirk Notz, Germany]	Accepted. The number of models are shown in Fig. 4.1.
104625	28	15	28	28	Regarding "much-reduced likelihood of a near ice-free Arctic Ocean during summer for a global warming compared to pre-industrial levels of 1.5°C compared to 2.0°C", a degree of confidence should be assigned to this significant and well-established result. [William Merryfield, Canada]	Accepted. Good point.
45491	28	17	28	17	I would try to avoid the double “compared”. Replace the latter one with “than for”? [Leonard Borchert, France]	Accepted.
104623	28	18	28	23	It should be clarified what is meant by the model being observationally constrained, considering that the lowest observed sea ice extents until 2019 have been (3.57, 4.17) million km**2 in (2012, 2007), whereas in Fig. 4.3 the ensemble mean is about 2.5 in this time frame, with some ensemble members at 1.5 or lower. (This is in accordance with a known bias toward low Arctic sea ice in the model considered.) [William Merryfield, Canada]	Accepted.
106915	28	23	28	26	I would be more quantitative here and add figures to illustrate what "significantly higher" means. If possible, I would express this important assessment in a calibrated language (likelihood/confidence), because if it is highly significant, confidence is therefore high and can be provided. [Christophe CASSOU, France]	Accepted. Good point.
132465	28	26	28	26	I recommend avoiding the word conservative since it's not clear whether this means that the estimate may be an overestimate or underestimate, depending on how the word is meant. I suggest saying explicitly which you mean here. [Kyle Armour, United States of America]	Noted.
54963	28	26	28	28	While the authors are correct that published estimates of an sea ice loss under 1.5C and 2.0C are possibly conservative, this is because sea ice in those models is generally not sensitive enough to global warming (see, e.g., Chapter 9, p. 43, line 23. The fact that future aerosol decline will lead to decreased sea ice is irrelevant in this context, as that effect works through its impact on surface temperatures. The question addressed here is sea ice loss under 1.5C and 2.0C global warming, and the pathway towards that state (whether or not that includes changes to aerosols) is irrelevant. [Nancy Hamzawi, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6655	28	33	28	33	In estimates made from observational data, sea-ice extent and sea-ice area are two different quantities. Table 4.4 refers to sea-ice area, and the paragraph after it discusses sea-ice cover, which I would interpret to mean area. But the caption of Figure 4.3 begins "Arctic sea ice extent". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The table is for sea ice are and the figure is for sea ice extent.
104627	28	34	28	35	The caption to Fig. 4.3 states "black curve is the average over twenty simulations following historical forcings to 2015 and RCP8.5 extensions to 2100", but the RCP8.5 extension is actually dark red. [William Merryfield, Canada]	Accepted. Thank you for noticing this. This has been corrected.
107233	28	43	29	27	The fact that when CO2 levels rose from 307 ppmv (ninety years ago) to 413 ppmv (now), it caused no detectable acceleration in the rate of sea-level rise at the highest-quality long measurement sites, means that the projections from these various approaches are worthless. "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..." Burton (2012) doi:10.1007/s11069-012-0159-8 https://sealevel.info/1612340_Honolulu_Wismar_Stockholm_vs_CO2_annot3.png [David Burton, United States of America]	Noted. This is beyond the scope of Chapter 4.
127507	28	43	29	39	This section should restrict itself to the thermosteric aspects of the CMIP6 ensemble. The other components are assessed in very different ways and the total is discussed in Chapter 9. Partially including the totals here is unsatisfying and confusing. [Trigg Talley, United States of America]	Taken into account. However, it would be confusing to offer seemingly different assessments from Chapter 9. Instead, the references to Chapter 9 have been made more explicit.
104483	28	45	29	1	4.3.2.2 Global Mean Sea Level: "The AR5 assessed from CMIP5 process-based simulations that it is very likely that the rate of GMSL rise during the 21st century will exceed the rate observed during 1971–2010 for all RCP scenarios due to increases in ocean warming and loss of mass from glaciers and ice sheets (Church et al., 2013)."....=> I suggest to extend this short statement into a paragraph summarizing future mass losses from the ice sheets and relevant drivers (Ch9). Detailed assessment of SMB and surface air temperature changes is provided in the Atlas (supported by Interactive Atlas). A reference will be useful here. [Irina Gorodetskaya, Portugal]	Taken into account. This is really the job of Chapter 9 and we now make a point of referencing Chapter 9 more strongly than we already have in this subsection.
21649	28	45	29	5	This paragraph should also characterize the principal SROCC findings which nudged these estimates considerably higher. [Peter Thorne, Ireland]	Taken into account. This is done in Chapter 9, and we would rather not duplicate the information here.
96415	28	45	29	5	Here the SROCC results needs to be added. The higher bound of 1.07m of the RCP8.5 scenario compared to AR5 due to progress in AA ice sheet knowledge was one of the main results of the SROCC, in unison with the numbers given here. (Confirming SROCC or based on the same projections?) [Nicole Wilke, Germany]	Taken into account. This is done in Chapter 9, and we would rather not duplicate the information here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79087	28	45	29	5	GMSL projections in SROCC differ slightly from AR5, due to the Antarctic contribution. For completeness, I think the SROCC numbers should be in here, either alongside or replacing the AR5 numbers. [Aimee Slangen, Netherlands]	Taken into account. This is done in Chapter 9, and we would rather not duplicate the information here.
127509	28	45			The AR5 GMSL projections only used CMIP5 for the thermal expansion. [Trigg Talley, United States of America]	Noted.
50809	28	Table 4.4	28	Table 4.4	2041-60 row, September, 3 right-most cells: the CMIP6 multi-model averages here do not match the definition of 'ice free' and appear to contradict the text on pg 27 'For CMIP6 models...the vast majority of simulations show an ice-free Arctic in September before 2050.' Please explain why this doesn't appear to be the case in Table 4.4. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text referred to models in better agreement with recent observations than the entire ensemble. While formally consistent with the table, this was misleading. Text has been shortened to avoid this issue.
96417	29	3	29	5	Some of the projected numbers differ from the numbers given in Ch9 in the ES. Please revise. [Nicole Wilke, Germany]	Taken into account; numbers consistent with Chapter 9.
9795	29	7	24	13	This paragraph about the different approaches used in the literature to proejct GMSL made sense in the context of chapter 9, but seems a bit free floating here -- hard to know what to take form it in isolation. Is it the right methods summary paragraph to include here? [Robert Kopp, United States of America]	Noted. In the interest of brevity, we do not delve deeper here but instead refer to Chapter 9.
67847	29	7	29	8	It is stated that" There have been significant modelling advances since the IPCC AR5, with most sea-level projections falling into one of three categories" It is advised to have brief descriptions of those three categories, to have a better understanding of the discussed paragraph. [Ruandha Agung Sugardiman, Indonesia]	Noted. The categories follow immediately after. No change.
7247	29	7	29	8	It is stated that" There have been significant modelling advances since the IPCC AR5, with most sea-level projections falling into one of three categories" Thus It is advised to have brief descriptions of those three categories. It may to have a better understanding on the discussed paragraph. [Asaad Irawan, Indonesia]	Noted. The categories follow immediately after. No change.
96419	29	7	29	8	We suggest to name the most significant modelling advances that influenced sea level. [Nicole Wilke, Germany]	Rejected in the interest of brevity. Chapter 9 deals with this extensively.
15929	29	15	29	27	The text should make clear that these are not equilibrium sea level rises and the text would be stronger and clearer with a statement on the equilibrium levels along with further long term projections of sea level rises for each SSP and the rates of change. Commentary should also be made that the rate of change of sea level rise will increase with respect to time, as is evident from the table. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been added to the end of this paragraph to bring out most of these points.
9797	29	15	29	27	this section may give the mistaken impression that the GMSL values are directly derived from CMIP6 models [Robert Kopp, United States of America]	Accepted. Good point. Text modified to ensure that there is not this mistaken impression.
9803	29	15	29	27	This paragraph needs language similar to that about higher warming rates simulated by more sensitive CMIP6 models on page 35, 33-37. [Robert Kopp, United States of America]	Rejected. We would rather leave it to Subsection 4.3.4 to comment generally on the implications of the high ECS models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50811	29	15	29	27	The projected GMSL results here do not appear to match that provided in the related sections in the SPM (C.2.5). Is this difference due to the long-term (2081-2100) projection reported here, vs projections to 2100 reported in the SPM? Please clarify and ensure consistency in GMSL change projections across the report. It would also be helpful to explain why the SSP5-8.5 likely range is lower and narrower compared to the SROCC (AR6 SSP5-8.5 to 2100 is 0.60-0.90m by 2100, whereas the SROCC RCP8.5 is 0.61-1.10m for 2100). Lines from Ch 9, lines 30-42 could be added here to highlight the uncertainty in higher emission scenarios and unpack the reasons behind this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Consistency has been assured. On the other points, a reference to Chapter 9 Section 9.6.3 has been provided.
79089	29	15	29	27	Why is this 5-95% rather than the likely range? [Aimee Slangen, Netherlands]	Taken into account. We are know providing the true likely range.
39107	29	15	29	38	Sea Level anomalies for total 2081-2100: values in Table 4.5 are the same (except SSP1-2.6) as in Box SPM.2, Table 2, but time for Box SPM.2 is 2100, not 2081-2100. Check for consistency. [Ola Kalen, Sweden]	Taken into account. Consistency has been assured.
111963	29	18		27	More elaboration on contribution of melting glaciers and ice sheets [Tomas Halenka, Czech Republic]	Rejected. This is under the mandate of Chapter 9 which we refer to in the text..
69175	29	22	29	22	The sea level anomalies relative to 1995-2014 over the long-term under SSP1-2.6 given on page 29, line 22 is inconsistent with the numbers in Table 4.5. Suggest correction. [Kaoru Magosaki, Japan]	Accepted.
79711	29	22	29	22	range 0.05-0.13 m does not match the numbers in table 4.5 [Laurent Terray, France]	Accepted.
71279	29	22	29	22	Range of GMSL variation is wrong rof SSP1-2,6, long-term (0.29-0.71 in Table 4.5). The value in L.22 is result for SSP3-7.0, near-term. [Kenji Taniguchi, Japan]	Accepted.
15491	29	22	29	22	The long-term projection range 0.05 m - 0.13 m for SSP1-2.6 does not tally with what is shown in Table 4.5 (0.29 - 0.71). Please check and revise as appropriate. [SAI MING LEE, China]	Accepted.
71931	29	22		23	These numbers are inconsistent witht the table - seems to be some mistake. [John Church, Australia]	Accepted.
9799	29	25	29	27	The statement about > 1/3 of GMSL rise being due to TE undercounts the tail risk associated with ice sheet projections -- since this is not assessed in this section, and the assessment may change, I would be careful about the statement about the prooprtion of GMSL rise due to TE [Robert Kopp, United States of America]	Noted. The thermosteric component is no longer assessed in this chapter.
69177	29	30	29	39	"2081-2100" in the 4th line of the Table 4.5 seems to correctly be "2021-2040". [Kaoru Magosaki, Japan]	Noted. The thermosteric component is no longer assessed in this chapter.
69179	29	30	29	39	There are some inconsistencies between Table 4.5 in Chapter 4 and Table 9.6 in Chapter 9 (e.g. the levels of the future GMSL rise under SSP1-2.6, the levels of near-term GMSL rise under other scenarios). What is the causes of that? If these are from the same data, correction should be made. [Kaoru Magosaki, Japan]	Taken into account. Harmonization has been achieved.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42559	29	30	29	39	Inconsistencies among Table 4.5 (Ch4) and Table 9.6 (Ch9, p.96). The contents of both tables require careful re-examination. Period average total sea level anomalies (multi-model means) and 90% uncertainty range limits for 2081-2100 (Table 4.5, row 3) are numerically equal to „median“ and „very likely“ range limits for 2100 (Table 9.6, row „Total (2100)“) for scenarios SSP2-4.5, SSP3-7.0 and SSP5-8.5. 1) Same values can hardly hold for 2100 and! 2081-2100. 2) Deviation in terms: multi-model „mean“ vs. „median“. 3) 90% range in Table 4.5 is defined as +/-1.645*StdDev. In other SLR-related instances, at any rate in the context of the steric component (cf. Ch9, p92, lines18-23 and my comment #11), this range is awkwardly enough considered a „likely“ (66%) rather than a „very likely“ range. In Table 9.6, however, the numerically same range limits, are claimed to constitute the „very likely“ range. 4) If defined as a Gaussian +/-1.645*StdDev, how come the range is not symmetric about the mean/median? 5) Everything said also applies to row „Total 2041-2060“ (Table 4.5) and row „Total medium term (2050) (Table 9.6). 6) Finally, note that 1.st cell in 4th row of Table 4.5 should read „Steric: 2021-2040“. [Sabine Hüttl-Kabus, Germany]	Taken into account. Harmonization has been achieved.
71929	29	30		39	In the table, it should be thermosteric. Add the projections to 2300 here and also the rate of rise over 2081-2100. Add discussion of the longer term projections to this subsection. Should specifically note that this is the 95% model range as in the AR5, which the AR5 assessed as likely. Is this the assessed likely or very likely range? [John Church, Australia]	Taken into account. 1) Thermosteric values have been removed; 2) Section 4.7 deals with projections past 2100. A pointer has been introduced. 3) This is the likely range.
15493	29	32	29	38	Re: Table 4.5. The projection period 2081-2100 in the first row of the Steric component appears to be a typo error. Please revise. It is recommended to supplement this table with the global mean sea level change between the reference periods in AR5 and AR6, i.e. 1986-2005 and 1995-2014, to facilitate comparison of projections, especially projections for the end of this century. [SAI MING LEE, China]	Taken into account. 1) the thermosteric values have been removed. 2) The first paragraph of this subsection deals with the AR5 values. Space limitations prevent us from expanding the table.
41405	29	32			SSP1-19 GMSLR projections must not remain unavailable for the Final Draft, please complement with methods like updated Kopp et al projections used in Chapter 9. [Alexander Nauels, Germany]	Taken into account. Table no longer included since it would duplicate too much from Chapter 9.
11287	29	42	30	34	Please explain the mechanisms by which the AMOC weakens in response to the GHG forcing. [Masahiro Watanabe, Japan]	Noted. This is the territory of Chapter 9.
83093	29				Table 4.5. Here and elsehwere - I think we need to take care to use the same nomenclature for the different GMSL components across the report. I believe that Ch9 has largely adopted the terminology put forward by Gregory et al (2019): https://link.springer.com/article/10.1007/s10712-019-09525-z [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83095	29				Section 4.3.2.2 I think it would be helpful to briefly explain that global thermosteric sea-level change is the only GMSL component that comes directly from CMIP simulations, and also to note the close correspondence to total ocean heat uptake. I don't see any direct reference to Ch9 for methods on deriving the GMSL projections presented in this section. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the interest of brevity, the treatment has not been deepened here; clear reference now to Chapter 9.
33025	30	1	190	55	It is suggested that the following sections be included in the self-regulation systems headings:If carbon is increased so much that its concentration causes negative feedback and does not allow the sun's rays to enter the earth, it will cause cooling on the planet. Therefore, the increase in carbon causes some heat and has a negative feedback from a certain level. Is this included in the scenarios and the limit of increasing carbon to create a negative feedback is clear? [Sahar Tajbakhsh Mosalman, Iran]	Rejected. This is outside the mandate of Chapter 9.
32695	30	1	190	55	It is suggested that the following sections be included in the self-regulation systems headings:If carbon is increased so much that its concentration causes negative feedback and does not allow the sun's rays to enter the earth, it will cause cooling on the planet. Therefore, the increase in carbon causes some heat and has a negative feedback from a certain level. Is this included in the scenarios and the limit of increasing carbon to create a negative feedback is clear? [sadegh zeyaeyan, Iran]	Rejected. This is outside the mandate of Chapter 9.
50815	30	2	30	2	Suggest that "lighter and more stable " is replaced with "less dense and therefore more stable" for accuracy [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
106917	30	2	30	2	Reference clearly needed here or at least a reference to Chap9 should be added. What is also missing as a driver of future AMOC is the freshwater input from Greenland which also stabilizes the water column [Christophe CASSOU, France]	Accepted.
132467	30	4	30	7	I am having trouble following this sentence. Hopefully now that more models are available, you can explain how AMOC changes for the different SSPs separately. [Kyle Armour, United States of America]	Noted. While output from a few more models have become available, the result remains that overall weakening is scenario independent.
102931	30	4	30	9	The spread of the model prediction is quite high, and even if averaging the results would make the weakening of the AMOC the results, given that there is not even agreement on the sign, the conclusion of a “monotonic weakening” sounds extreme. [Philippe Tulkens, Belgium]	Rejected. "Monotonic" refers to the model mean values which indeed show a robust monotonic decrease (except for SSP1-1.9 where too few simulations are available). In the text we do highlight the large variation in AMOC strength across the individual simulations.
84251	30	4	30	9	but the spread is huge even if the number of models considered is small [Annalisa Cherchi, Italy]	Noted.
9801	30	4	30	9	It seems weird to make a project based on pooling results across emissions scenarios. [Robert Kopp, United States of America]	Accepted. Pooling is no longer done.
79713	30	4	30	9	I suggest to include the Roberts et al. paper in final revision stage to JAMES which looks at MOC projections with both LR and HR versions of a set of HighResMIP models. I suggest also to extensively refer to Chapter 9 for more detailed analysis of the mechanisms and make sure that there is consistency between the executive statements of the two chapters [Laurent Terray, France]	Taken into account. To avoid undue duplication with Chapter 9,the assessment has not been expanded, though.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7809	30	4	30	9	I assume this is to 2100? Need to state this. Also, from the figure, is seems that this analysis is based on 6 models only. Are the increases all due to one model? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
87541	30	4	30	9	The spread of the model prediction is quite high, and even if averaging the results would make the weakening of the AMOC the results, given that there is not even agreement on the sign, the conclusion of a “monotonic weakening” sounds extreme. [Valentina Roberta Barletta, Denmark]	Accepted and modified.
19843	30	4	30	23	The striking fact here is that AMOC's evolution seems, according to figure 4.4, nearly independent of the scenarios. Would there be a way to reconcile this with the strong dependency upon scenarii (in other words basically CO2 emissions and global warming) observed on so many physical indicators, including the high attitude temperature and precipitation mentioned here. In any case, such a behaviour deserves to be commented. This lack of dependency on anthropogenic global change is less striking however when one considers the very large dispersion of the (small number of) simulations. While nobody will contest the correctness of averaging procedures, the conclusion with medium confidence as to AMOC weakens (weakens monotonically!) seems bold [philippe waldteufel, France]	Accepted.
31533	30	5	30	7	Is that independent of scenario ? [Jean-Baptiste SALLEE, France]	Noted. This correct.
2153	30	8	30	8	surely not monotonically? [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
106919	30	8	30	8	"Monotonically" is not correct as some models in CMIP6 undergo strong decadal variability on top of negative trends. This decadal variability is visible when taking several members for a given scenario and it could overcome the forced signal especially in the near and mid-term. Anyway the assessment on AMOC futures outcomes is too weak to me in the actual version and should be considerably improved by including more models and by deepening the interpretation of the models behavior. It is clear from circles that the response is very much model dependent and not much scenario dependent. Any literature on AMOC future in CMIP6 ? Based on the actual assessment, I wouldn't even have chosen the medium level for confidence but the low one. In addition, would it be possible to write the conclusion in a calibrated language? I would have written something like "it is more likely than not that AMOC will decline over the 21st century (low confidence)". References are clearly missing here and there are also may papers based on CMIP5 and published between since 2013 which need to be assessed! If it is not done extensively in Chap4, which I understand, a cross-reference to Chap9 is mandatory. [Christophe CASSOU, France]	Taken into account. The number of models has been increased and a further discussion on scenario independence can be found in Chapter 9 Section 9.2.3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45493	30	19	30	21	I understand the AMOC index is here defined to make sure model-dependent latitudes of maximum AMOC are accounted for. I suggest, however, to introduce a maximum latitude as well, to ensure that possible unrealistic anomalies close to the pole are not considered. Also, why is 30N chosen as the minimum latitude? This explicitly excludes the location of the RAPID array, the only long-term observation system of AMOC in existence. Would it be worth trying to include (or even explicitly select) the latitude of 26.5N here to enable a direct comparison to recent observations? [Leonard Borchert, France]	Accepted. We are now using 26.5N.
102933	30	26	30	34	This block complements the results discussed above (Page 30, lines 4-9) for different models, but what the two blocks refer to is hard to follow: Rewrite making clear what are the different models, and what are the conlcusions For the Atlantic Meridional Overtutning Circulation. [Philippe Tulkens, Belgium]	Rejected. We agree that the two blocks of text are complementary, however we also feel that the text makes clear that these are different experiments (transient versus stabilization) and models (CMIP5 versus CMIP6).
50813	30	26	30	34	It would be useful to include further discussion of AMOC projections which is an important future impact that should also be included in the SPM (currently only discussed in C.6.3 in the context of abrupt collapse). Pending increased availability of CMIP6 results it would be helpful to elevate information on AMOC projections to the SPM and explain here how the latest results compare to AR5 and SROCC projections. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Inclusion of the AMOC information is under consideration by the SPM author team. A detailed comparison of the CMIP6 projections against the AR5 and SROCC can be found in Chapter 9 Section 9.2.3.
7811	30	26	30	34	The description of the scenarios here is unclear. In particular the AMOC continues to decline for 5-10 years after what? I would think it likely that the AMOC response would vary depending on how quickly the stabilisation was reached. Also there is similar discussion in section 4.7.1.5. That location seems to be a better place to include the discussion of AMOC during stabilisation runs. There also seems to be similar discussion in 4.7.2.2.4. Please check that these are all consistent and don't overlap unless necessary [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
87543	30	26	30	34	This block complements the results discussed above (Page 30, lines 4-9) for different models, but what the two blocks refer to is hard to follow: Rewrite making clear what are the different models, and what are the conlcusions For the Atlantic Meridional Overtutning Circulation. [Valentina Roberta Barletta, Denmark]	Rejected. We agree that the two blocks of text are complementary, however we also feel that the text makes clear that these are different experiments (transient versus stabilization) and models (CMIP5 versus CMIP6).
127511	30	30			How is CanESM5 "nearly independent" of CanESM2? This does not seem likely for any reasonable interpretation of the words. Delete. [Trigg Talley, United States of America]	Accepted.
2155	30	34	30	34	Some discussion of common errors in ocean state that can prevent model AMOC from collapsing may be needed here?e.g. Hawkins, E., Smith, R. S., Allison, L. C., Gregory, J. M., Woollings, T. J., Pohlmann, H. and de Cuevas, B. (2011) Bistability of the Atlantic overturning circulation in a global climate model and links to ocean freshwater transport. Geophysical Research Letters, 38 (10). L10605. ISSN 0094-8276 [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is outside the scope of this chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87817	30	37	30	37	It would be helpful to discuss time series and trends in the same section, as otherwise, some information is repetitive. Also, respective figures of time series and trends could be merged together, as it is easier to interpret trends when seeing the time series. (e.g. Fig. 4.15 is easier to interpret when directly compared with Fig. 4.5 that contains the same type of information). [Katarzyna Tokarska, Switzerland]	Rejected. This would go against the well thought out rationale for the structure of the entire chapter.
87819	30	37	30	37	Please note that Fig. 4.5 contains the same information as Figure 5.25 in CH5, and related Figure 5.23. Perhaps it would be good to keep only one version of this figure in CH5 to avoid repetition, and for consistency? Also, there the model spread is important to discuss when talking about land carbon uptake (that varies widely across models even within each scenario), so I would suggest to remove this section and refer to Ch5 instead to avoid repetition. [Katarzyna Tokarska, Switzerland]	Rejected. There is a strong desire to retain Fig. 4.5 in Chapter 4 since it is felt that changes in land and ocean carbon fluxes are a key indictors of global climate change, the subject of this section. However, this should not prevent Chapter 5 from delving into the mechanisms and implications. It should also be noted that unlike Fig 5.25, Fig. 4.5 presents 5-95% ranges, and on all of the core SSPs, not just SSP1-2.6 and SSP5-8.5.
87821	30	37	30	37	This section could be merged with section 4.4.2.2., to avoid repetition. Also, it would be easier to understand Fig. 4.15 if it was near (or even a bottom panel) of Fig. 4.5, since they show the same type of information. [Katarzyna Tokarska, Switzerland]	Rejected. This would go against the well thought out rationale for the structure of the entire chapter.
52979	30	37			In addition to the absolute values, also assess potential changes in the % of CO2 emissions which are absorbed by ocean and land respectively? [Hervé Douville, France]	Noted. Good point, but this lies more under the mandate of Chapter 5.
19845	30	44	30	56	There is no discussion about the figures quoted for accumulated carbon fluxes. On the other hand, when the conclusion lines 55-56 mention the "net ocean and land carbon flux", what does it mean? For the fluxes (expressed in PgC/yr), this conclusion contradicts figure 4.5. One would of course like to re-examine the implications of these simulations with respect to the atmospheric CO2 concentrations for various scenarios. Is it possible for the text to include some comments on this issue, or at least to give a reference to relevant parts of the WG1 report? They are not easy to find. [philippe waldteufel, France]	Accepted. Agreed, this was a confusing paragraph.
15931	30	45	30	56	There needs to be clarification about the carbon fluxes - I assume that the quoted flux for the different SSPs refers to the carbon leaving the atmosphere by going into the various reservoirs such as biomass and the ocean. In reality carbon will out-gas from these, so the permanently sequestered carbon will be much lower. So along side these quoted figures for the flux, it would be sensible to give clarification of updated estimates of permanent carbon sequestration.Furthermore, there will be some maximum rate of carbon flux, and there is no mention of this. Has this been included in the models, and if so, how has it been determined? [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These issues are dealt with in Chapter 9.
81627	30	46	30	49	In Chapter 5, ocean C fluxes are shown to stop increaseing and even start to decrease past 2080 even for SSP5. Consider therefore rephrasing "increases continuously through most of the 21st century" to reflect the stabilisation or decline of the ocean sink in the latter quarter of the 21st century [Sönke Zaehle, Germany]	Rejected. We feel that the phrase "most of" suffices to make the point.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87823	30	49	30	50	Please note that this notation may be confusing, as flux is usually expressed in PgC/year, and here is presented the change in the ocean and land carbon storage. Please use terminology consistent with CH5, as in Fig. 5.25. therein. [Katarzyna Tokarska, Switzerland]	Accepted. Agreed, this was a confusing paragraph.
81625	30	50	30	50	A focus of Chapter 5 will be the model spread. For consistency, I would recommend to add ± 1 SD uncertainty ranges to htese numbers [Sönke Zaehle, Germany]	Taken into account. 5-95% ranges are now quoted.
102935	30	51	30	55	It would be important to separate the cumulative flux between the 1850 to present and present to 2100 periods. The difference in cumulative sink can only be attributed to future differences, but the total includes the influence of the past. The concluding sentence of the paragraph is not supported by the values as presented. [Philippe Tulkens, Belgium]	Accepted. Agreed, this was a confusing paragraph.
102937	30	51	30	55	It is unclear whether the "land carbon flux accumulated" refers to the total terrestrial carbon flux (which should translate to carbon stock changes on land - any lateral transfers to oceans) or only to the amounts resulting from vegetation models (natural response to environmental change), excluding management effects. If it includes management, then it should be clearly stated and the values saparated for all scenarios (preferably in a table), as management effects and natural response are likely to follow significantly different trajectories. If the values include only natural response, then the text should emphasize that management impacts are not included and the conclusions should be changes accordingly. In that case, no conclusions should be presented regarding "land carbon flux accumulated", as management (e.g., more intensive forest harvest, extensive soil disturbance) can counteract and reverse any accumulation due to natural response. [Philippe Tulkens, Belgium]	Noted. This is more in the scope of Chapter 5.
87825	30	53	30	53	the responses among the models differ quite a bit for the land carbon uptake, so is the ensemble mean response representative? Perhaps it would be good to show the model spread and comment on the outlier behavior too (e.g. UKESM and CanESM5 show quite different values for land carbon uptake in SSP 2-4.5) . Also, Chapter 5 already covers this discussion in depth, so maybe this paragraph should be removed to avoid repetition with CH5. [Katarzyna Tokarska, Switzerland]	Taken into account. 1) We feel that a >10 model ensemble mean is probably representative and that calling out a particular model as an outlier would problematic. 2) We are now quoting 5-95% ranges. 3) We feel that surface carbon flux is a key indicator of global climate change and so fits within the intent of this section.
81629	30	54	30	54	A focus of Chapter 5 will be the model spread. For consistency, I would recommend to add ± 1 SD uncertainty ranges to htese numbers [Sönke Zaehle, Germany]	Taken into account. 5-95% ranges are now quoted.
87827	30	55	30	55	this the net ocean and carbon flux referring here to flux in PgC/yr or cumulative land and ocean carbon storage in PgC? [Katarzyna Tokarska, Switzerland]	Accepted. Agreed, this was a confusing paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102939	30	55	30	56	The concluding sentence is not supported by the evidence presented and the opposite seems more likely. For oceans, it is reported above (lines 48-49) that the atmosphere-to-ocean flux would decrease continually under the lower emissions scenarios. For land, the numbers presented only present totals for 1850 to 2100, which does not allow any comparison even with past fluxes, let alone trends (increasing or decreasing) over the 21st century. Furthermore, net atmosphere-to-land carbon fluxes depend fundamentally on management (which can dominate over natural response of vegetation). Lower emission scenarios often rely on a high deployment of bioenergy, which can lead to the reduction of carbon stock on existing forest. [Philippe Tulkens, Belgium]	Accepted. Agreed, this was a confusing paragraph.
19229	30	55	30	56	Would there be another word for "accumulated carbon flux" (= stock?)? Does "net carbon flux" in the last sentence refer to "accumulated flux"? The wording should avoid confusion between the conclusion text and the figure (which shows the fluxes). [Anne-Marie Treguier, France]	Accepted. Agreed, this was a confusing paragraph.
114463	30	55	30	56	maybe I misunderstand, but it does not look like this in fig 4.5b [Jan Fuglestvedt, Norway]	Accepted. Agreed, this was a confusing paragraph.
12205	30	55	30	56	This statement is not at all consistent with Fig. 4.5b, which suggests near-zero fluxes towards the end of the century for two of the scenarios [Bryan Weare, United States of America]	Accepted. Agreed, this was a confusing paragraph.
79503	30				It is suggested that the following sections be included in the self-regulation systems headings:If carbon is increased so much that its concentration causes negative feedback and does not allow the sun's rays to enter the earth, it will cause cooling on the planet. Therefore, the increase in carbon causes some heat and has a negative feedback from a certain level. Is this included in the scenarios and the limit of increasing carbon to create a negative feedback is clear? (comment by: sahar.maleki@ut.ac.ir) [Hanieh Zargarlellahi, Iran]	Rejected. This is outside the mandate of Chapter 9.
87829	31	3	31	4	Figure 4.5. It is unclear what is meant by the net land carbon uptake rate? ie. the atmosphere-land carbon flux, defined as NBP, does it include land use change? (which differs among the different SSP scenarios)? I would suggest keeping the terminology consistent with CH5. Also, this figure is redundant, given similar figures 5.23 and 5.25 in Chapter 5, so maybe consider removing it here to avoid duplication. [Katarzyna Tokarska, Switzerland]	Accepted. Agreed, this was a confusing paragraph.
102941	31	5	31	5	Why is the “uncertainty shading only shown for two scenarios? For “low forcing” scenarios the long term uptake could be compatible with 0. [Philippe Tulkens, Belgium]	Noted. It was a chapter decision to show shading for only two scenarios so as to simplify the presentation.
87545	31	5	31	5	Why is the “uncertainty shading only shown for two scenarios? For “low forcing” scenarios the long term uptake could be compatible with 0. [Valentina Roberta Barletta, Denmark]	Noted. The shading is not added to all scenarios to avoid clutter.
93407	31	17	31	20	It needs to be specified in this sentence that the projections relate to the period between 1986–2005 and 2081–2100. [Carles Pelejero, Spain]	Accepted.
19847	31	29	31	36	In figure 4.6, perhaps the "no units" label for the scale of ordinates can be omitted. [philippe waldteufel, France]	Rejected. We prefer to be specific on the units or lack of in this case.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52981	31	39			Would it be feasible to adopt a more ambitious and systematic framework across most chapters (especially 2, 3 and 4), where changes in variability would be assessed in terms of both spatial and temporal structures? For instance, what about a possible eastward shift in the projected PNA (e.g., Zhou 2014)? [Hervé Douville, France]	Noted. This particular section is dedicated to index changes while subsequent sections deal with pattern changes.
52983	31	43	31	45	, still using simple geographical indices and thereby assuming no change in the spatial structure of these modes of variability. (if you keep this approach, you may want at least to highlight that it is a severe assumption that is used here for the sake of simplicity?) [Hervé Douville, France]	Noted. This particular section is dedicated to index changes while subsequent sections deal with pattern changes.
44983	31	48	34	44	This part should be discussed in-depth. Especially, for the SAM, it could be discussed specific to IOD, MJO, and AAO instead of overall as the SAM. [Mustafa Tufan Turp, Turkey]	Noted. Subsequent sections and chapter are more in-depth.
106927	31	48			In the current SOD, it is impossible to discriminate in the modes of variability changes what is due to the change in the variability itself and/or in the mean state (forced response) which could then project on one phase of the mode of variability. To go further, I would suggest to add one panel to the current figure showing the evolution of the stddev of the NAM computed over a 30-year running window (similar to figure 4.17). The combination of the two metrics would provide a complete information on how modes are changing and would also address the changes in teleconnection in a more practical way for users. Indeed decision-makers/policy-makers are interested by interannual variability over such a temporal window in addition to mean state changes. To help quantify/understand mean-state vs variability contribution in NAM and SAM total changes, the running correlation between the two zonal-averaged indices used to define the modes could be also computed to check if the true see-saw nature of the variability stays the same in a warming climate. [Christophe CASSOU, France]	Noted. These points are well taken, however this chapter is already over length and adding an additional figure and text along these lines is not feasible.
102943	31	50	31	50	Please redefine the acronym, write the Northern Annular Mode in the title [Philippe Tulkens, Belgium]	Accepted.
87547	31	50	31	50	Please redefine the acronym, write the Northern Annular Mode in the title [Valentina Roberta Barletta, Denmark]	Accepted.
106921	31	54	31	54	Check the definition of the NAM index. Wrong here as it uses difference in normalized zonal average and it is an adimensional quantity. [Christophe CASSOU, France]	Rejected. This index definition is not wrong, Here we feel we have to be consistent with the definition of the NAM as defined in the AR5 and in the cited literature.
106923	32	5	32	5	Over the historical part, NAM and NAO are indeed very much correlated but the changes for the future is not. I have produced the equivalent of Fig.4.7 but for the NAO and there is absolutely no trend. I will send the figure to the Chapter 4 Las. Therefore I would be very cautious. [Christophe CASSOU, France]	Accepted.
102945	32	7	32	20	This is an interesting contextualization, but it is the only place in this chapter where a phenomenon is so clearly explained. There is a certain non-unifomity in the level of details in the description of the results. [Philippe Tulkens, Belgium]	Rejected. See response to comment #1092.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87549	32	7	32	20	This is an interesting contextualization, but it is the only place in this chapter where a phenomenon is so clearly explained. There is a certain non-unifomity in the level of details in the description of the results. [Valentina Roberta Barletta, Denmark]	Rejected. We feel that this is an appropriate level of detail for this particularly important mode of variability.
79715	32	12	32	15	does this underestimaton of multidecadal variability apply to all seasons ? [Laurent Terray, France]	Noted. This is a good question, but is beyond the level of detail expected of this chapter, in particular.
2157	32	14	32	14	references for stratosphere: Scaife A.A., J.R. Knight, G.K. Vallis, C.K. Folland 2005.A stratospheric influence on the winter NAO and North Atlantic surface climate.Geophys. Res. Let., 32, L18715. Manzini E., A. Karpechko, J. Anstey, M.P. Baldwin, R.X. Black, C. Cagnazzo, N. Calvo, A. Charlton-Perez, B. Christiansen, Paolo Davini, E. Gerber1, M.Giorgetta, L.Gray, S.C. Hardiman, Y.-Y. Lee, D.R. Marsh, B.A. McDaniel, A. Purich, A.A. Scaife, D. Shindell,S.-W. Son, S. Watanabe and G. Zappa, 2014.Northern winter climate change: Assessment of uncertainty in CMIP5 projections related to stratosphere-troposphere coupling. J. Geophys. Res., 119, doi:10.1002/2013JD021403. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The focus is on new literature.
90827	32	17			"Little evidence for a significant role" could be supported by study "Anthrogenic forcing of the northern annular mode in CCMVal -2 models" and "Projected effects of declining anthropogenic aerosols on the southern annular mode" [Vivien How, Malaysia]	Rejected. Here we are briefly summarizing some information from Chapter 3 that is relevant to the projections. Greater detail and additional references are provided in Chapter 3.
52223	32	22	33	7	the NAO+ Weather Regime shows a positive trend in frequency in winter in CMIP6 models. The trend is robust in the multi-model mean for ssp370 and ssp585 scenario, moderately for ssp245. Paper to be submitted soon: F. Fabiano, P. Ghinassi, V. Meccia, S. Corti (9999) – A regime view of future circulation changes in mid-latitudes in CMIP6 models [Fabiano Federico, Italy]	Taken into account. Paragraph has been modified.
127513	32	24			Given the results in Chapter 3 suggesting that models with a top above the stratopause have much better winter variability in the NAM, does it make sense to group all models together in this assessment? If there is a difference in trends between these two groups of models it should be noted (and if not, that too is interesting). [Trigg Talley, United States of America]	Noted. These are valid considerations but are out of the scope of this particulate subsection and better addressed in the subsequent "mechanistic" chapters.
79717	32	27	32	31	Is it possible to dismiss sampling issues for the differences between the different scenarios ? In other words do we have a good explanation for a more positive NAM only for the two high emission scenarios ? [Laurent Terray, France]	Taken into account. With the addition of even more model simulations (now 35 for SSP5-8.5) we can see that the NAM remains slightly more positive which is consistent with earlier CMIP5 studies and the AR5.
50817	32	27	32	31	It would be helpful to explain here if a more positive wintertime NAM projected under higher emission scenarios also strengthens westerly jets and whether this causes these move north or south. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Beyond the scope of the subsection.
106849	32	36	32	42	The definition of the NAM/SAM index is wrong and does not follow the ones in the cited papers. The difference is between normalized zonal mean SLP and nomalization has been omitted in the current definition. Correct computations are provided in the Technical Annex and Chap3. [Christophe CASSOU, France]	Rejected. This index definition is not wrong, Here we feel we have to be consistent with the definition of the NAM as defined in the AR5 and in the cited literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106925	32	47	33	7	I would add the Cattiaux and Cassou (2013, https://doi.org/10.1002/grl.50643)'s reference that illustrates the changes in the barotropic nature of NAM which helps discriminate sea-ice versus tropical warming effect on one hand and the sensitivity to the forcing scenario on the other hand. [Christophe CASSOU, France]	Rejected. We feel that the existing references suffice.
6657	32	50	32	50	Would "opposing" be better than "opposite"? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
21655	32	51	32	51	Should also refer to cross-chapter box 10.1 here? [Peter Thorne, Ireland]	Accepted.
132469	32	56	32	56	Drop the word "which" here. [Kyle Armour, United States of America]	Rejected. Existing wording is correct.
52985	33	4			Also Oudar et al. 2020 (based on both CMIP5 and CMIP6 models, https://doi.org/10.1029/2019GL086695) [Hervé Douville, France]	Accepted. The Oudar et al. reference has been added to the FGD.
52987	33	14	33	16	although projected changes in the stratospheric polar vortex remain highly uncertain and poorly understood in both CMIP5 (Wu et al., 2019) and CMIP6 (Ayarzagüena et al., 2020) models. [Hervé Douville, France]	Rejected. This point is well taken, however in this paragraph we are focused on areas of improvement going from CMIP5 to CMIP6.
21657	33	16	33	16	There is no closing summary given on the NAM which places it at odds with all remaining segments of section 4.3 [Peter Thorne, Ireland]	Noted. Progress has mainly been on identifying why no robust changes is being simulated, precluding a simple summary statement.
50819	33	44	33	46	Please specify how many models followed this recommended approach. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We feel that this level of detail is unwarranted here.
65691	33	44	33	46	Suggest clarification. Are CMIP6 models capturing realistic stratospheric ozone trends or was this just a recommendation? [Kushla Munro, Australia]	Noted. This question is more in the scope of Chapter 3.
127515	33	48	33	55	[PROGRESS] What was learned in AR6 compared to AR5? New info? More/less confidence? More/less likely? [Trigg Talley, United States of America]	Rejected. Space limitations have limited the level of detail that we can go into here. Subsequent chapters delve deeply.
2159	33	49	33	49	This change of the SAM trend appears toi have occurred already:Banerjee, A., Fyfe, J.C., Polvani, L.M. et al. A pause in Southern Hemisphere circulation trends due to the Montreal Protocol. Nature 579, 544–548 (2020). https://doi.org/10.1038/s41586-020-2120-4 [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
116311	33		33		I would suggest that indices are used consistently in ch 2, 3, 4, please check (here described for SAM). [Valerie Masson-Delmotte, France]	Rejected. Here we feel we have to be consistent with the definition of the NAM as defined in the AR5 and in the cited literature.
50821	34	1	34	3	Can this summary statement be an emboldened, separate para at the end? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We feel that the present placement of this sentence is appropriate.
19849	34	1	34	3	As shown by figure 4.7, the projections from 2020 until 2100 suggest quite similar trends for NAM and SAM. This looks like the main feature: as soon as the impact of deep ozone depletion in the SH disappears, the behaviour of both NH and SH modes becomes similar. This is not reflected in the "summarizing" sentence, which highlights the recent past, whereas it is expected it should rather consider the projections for times to come. [philippe waldteufel, France]	Rejected. The reviewer makes a valid observation regarding the differing evolutions of the NAM and SAM anomalies. However, for brevities sake we prefer to not draw this point out explicitly. We also note that the sumary statement does not highlight the recent past. Rather, it only refers to the future under the highest emissions scenario.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106929	34	6			I don't understand Fig.4.8. What is plotted? Standard deviation computed each year from monthly mean for each model and then averaged? Spread between model for each year? If so, using 8 models for such a computation in ssp1-1.9 is meaningless and even 20 realizations is not a lot. But more importantly, I don't see what can be drawn from this Figure. Why not doing the same for SST as for precipitation in Fig. 4.17 ? This is a relevant metric to evaluate the changes in variability and this is what is investigated here. [Christophe CASSOU, France]	Not applicable. The method has been changed. .
52989	34	6			could also include the IOD given the multiple interactions between the Pacific and Indian Ocean variability => replace by Indo-Pacific variability? [Hervé Douville, France]	Rejected. Space limitations preclude is discussing the IOD here.
14463	34	8			Recommend replacing “quasi-periodic” by “irregular” as “quasi-periodic” has a very specific dynamical meaning that is not accurate here. [Malte Stuecker, United States of America]	Accepted.
90055	34	14	34	15	ENSO variability in the 21st century is of low confidence due to "strong component of natural internal variability. It must be clarified, if this is the case even at the end of the century time-slabs as ENSO impacts have wide-ranging consequences to many countries. Particularly, in the light of ENSO variability being very likely assessed to remain the dominant mode. In the linked figure 4.8, SSP1-1.9 with 8 models does not match with the others, maybe removed. [Govindarajalu Srinivasan, Thailand]	Rejected. This statement reflect current understanding notwithstanding the relatively low number of models that contributed to SSP1.1.9.
52991	34	15			and a lack of model consensus? [Hervé Douville, France]	Rejected. This statement reflect current understanding notwithstanding the relatively low number of models that contributed to SSP1.1.9.
70293	34	17	34	30	But, we know from the literature that just because ENSO SSTs do not increase in variance, this does not mean that the teleconnections of ENSO (which is the main reason people care about ENSO) do not change. In fact, there is an abundance of literature from CMIP5 that shows that this does change along with CMIP6 model anaylsysis presented in the technical summary (see page 99 of the SOD techniical summary) that shows the smae thing exists in CMIP6. Not reporting this clearly here will lead to confusion, especially within groups that focus on climate impacts. [Shayne McGregor, Australia]	Noted These issues are discussed in subsequent sections.
70295	34	17	34	30	I can see that the is a discussion of the impacts of background state changes on ENSO in sections 4.4.3.2 and 4.5.3.2, but probably the most important place they should be presented (as much of the research is pinned to the21st century) but it is not presented. I also think that the great analysis that has been presented in the TS (page 99) could and should be brought in here. Also, teleconnections are the reason why ENSO is of such interest, so I beleive that there should at least be some text pointing to where ENSO changing teleconnection are discussed in AR6 (which is currently in the TS). [Shayne McGregor, Australia]	Noted. These issues are discussed in subsequent sections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65695	34	17	34	30	Suggest including considerations of other ENSO indices, which offer more insight into the impact of rising greenhouse gases on ENSO diversity, ENSO-driven precipitation, and amplitude. For example results from the following: - Wang, G., W. Cai, and A. Santoso, 2020: Stronger Increase in the Frequency of Extreme Convective than Extreme Warm El Niño Events under Greenhouse Warming. J. Climate, 33, 675–690, https://doi.org/10.1175/JCLI-D-19-0376.1 - Cai, W., Wang, G., Dewitte, B. et al. Increased variability of eastern Pacific El Niño under greenhouse warming. Nature 564, 201–206 (2018). https://doi.org/10.1038/s41586-018-0776-9 - Power, S., Delage, F., Chung, C. et al. Robust twenty-first-century projections of El Niño and related precipitation variability. Nature 502, 541–545 (2013). https://doi.org/10.1038/nature12580 [Kushla Munro, Australia]	Rejected. This section is simply intended to highlight the main indicators of global climate change which we believe is accomplished using this well known index.
26861	34	21	34	21	It should consider Ciais paper showing that consistency increases between model results when the definition of the Niño box is adapted to each model, so as to reduce the effect of model biases in the comparison [Eric Brun, France]	Noted. This is a valid point but for ease of computation and for simplicity this approach was used here.
102947	34	23	34	23	Please shortly explain why ENSO 3.4, and why a 3rd order polynomial (no citations given) [Philippe Tulkens, Belgium]	Not applicable. The method has been changed.
87551	34	23	34	23	Please shortly explain why ENSO 3.4, and why a 3rd order polynomial (no citations given) [Valentina Roberta Barletta, Denmark]	Not applicable. The method has been changed.
52993	34	25	34	26	also highlight this in the figure caption or remove the curve for the SSP1-1.9? [Hervé Douville, France]	Not applicable
132471	34	27	34	29	Do we really have high confidence that the amplitude of ENSO will not change over the 21st century? I think this is how this sentence will be read. I think you instead mean something like "the amplitude of ENSO does not robustly change in CMIP6 projections over the 21st century, leading to low confidence in how ENSO amplitude may change in the future." [Kyle Armour, United States of America]	The increasing amplitude ENSO precip in CMIP5, CMIP6 and new results since AR5, for example from large Initial conditions are good evidence considered our revision
104631	34	27	34	29	Regarding "it is very likely that the amplitude of ENSO variability does not robustly change over the 21st century (high confidence)", it needs to be made clearer that this statement applies to the absence of a consensus among models, in accordance with lines 44-45 on p. 4-7: "There is no consensus from models for a systematic change in amplitude of El Niño–Southern Oscillation sea". It should avoid any suggestion that there is high confidence that ENSO variability will not change in nature, because ENSO variability does either increase or decrease robustly in some individual models, and it cannot be determined with great confidence which models is most realistic in this respect. [William Merryfield, Canada]	Taken into account. Revision has taken consideration of this and conclusive statement in revised text. For example the increasing amplitude ENSO precipitation which is consistent in CMIP5, the available CMIP6 models alongside the same evidence from results based on large initial conditions is good basis for assessment opinion.
50823	34	27	34	30	Can this summary statement be an emboldened, separate para at the end? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42715	34	27			There is little discussion of the results shown in figure 4.8. The overall lack of trend in std in clear from the figure but the future projections do seem to have periods considerably higher and lower than those in the historic period. Some further discussion of this figure would be useful, even if to say these differences cannot be regarded as significant and the reasons why. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Good points.
52995	34	29	34	30	update the list of CMIP5 studies (e.g., Chen et al., 2017) [Hervé Douville, France]	Taken into account. More citation has been added.
104633	34	33	34	42	It is very difficult to draw any conclusions from this Fig. 4.8 by eye. It woud help, for example if mean amplitudes for each scenario and 20-year evaluation period, ideally with confidence intervals, could be indicated to the right of the panel somewhat as in Fig. 4.4. [William Merryfield, Canada]	Rejected. As noted in the revised test, it is apparent from the figure that there is no obvious long-term trend in ENSO amplitude.
87553	34	45	24	45	Use the extended names in the titles [Valentina Roberta Barletta, Denmark]	Accepted, title expanded.
102949	34	45	24	45	Use the extended names in the titles [Philippe Tulkens, Belgium]	Accepted, title expanded.
89855	34	45	35	18	I think the method here is basically good, but it could be explained more clearly. Were the Jimenez-de-la-Cuesta and Mauritsen results used directly? [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been modified for clarity and precision, including references.
114465	34	45	37	3	This section is very important and could be highlighed to stand out more clearly. Not sure how, but with more references in text and perhaps a better title that better reflects that this addresses changes over the century. [Jan Fuglestvedt, Norway]	Taken into account, subsection is now more clearly highlighted through visual abstract and through revised title.
114467	34	45	37	3	The use for emulator based on assessment of ECS i ch7 and multiple lines of evidence is strengthening the report. This will be an important point in the communication of AR6 WGI findings and needs to be lifted to the higher summaries. [Jan Fuglestvedt, Norway]	Accepted and followed through via ES, TS, and SPM.
69945	34	45	37	3	Related to 4.3.1.1, I think it would be better to combine this section to 4.3.1.1 because only GSAT was explained in this section and there are no further assessments for precipitation and other variables. Also, main summary for temperature assessment in the future in pase 5 is told based on this section, not on the 4.3.1.1, but it may make confusion over numbers of future GSATs described in the executive summary if there is only very simple look at section 4.3.1.1. and 4.3.4. [Young-Hwa BYUN, Republic of Korea]	Taken into account. There is much merit to moving 4.3.4 to near 4.3.1. However, there are also counterarguments, which in the view of the author team have prevailed: 4.3.1 is straightforward, whereas 4.3.4 is sophisticated and complex. We have therefore decided to keep the SOD structure but have more clearly signposted the structure, including a new visual abstract.
66987	34	45	37	3	I struggled a little bit to follow organization of Section 4.3. I suggest moving subsection 4.3.4 closer to the rest of the discussion on GSAT could make this section easier to follow. [Aurélien Ribes, France]	Taken into account. There is much merit to moving 4.3.4 to near 4.3.1. However, there are also counterarguments, which in the view of the author team have prevailed: 4.3.1 is straightforward, whereas 4.3.4 is sophisticated and complex. We have therefore decided to keep the SOD structure but have more clearly signposted the structure, including a new visual abstract.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71933	34	45			Very nice to see the progress in the inclusion of the wider information rather than just depending on the CMIP6 model results. The assessed ranges should be compared with the AR5 ranges - I take it the means and ranges are very close except that here they are the very likely range compared to the likely range in the AR5. [John Church, Australia]	Accepted. Broad comparison to likely range in AR5 added.
79721	34	47	34	55	some of the text has already been introduced in sectin 4.3, perhaps it is possible to reduce it. [Laurent Terray, France]	Taken into account. Text has been deleted.
96421	34	47	34	55	This text is doubled, it is also found in the introduction to the chapter. [Nicole Wilke, Germany]	Taken into account. Text has been deleted.
45495	34	50	34	50	The narrowing of the uncertainty range from 5-95% to 17-83% to account for potential uncertainty not represented in the model range seems arbitrary. Is there a good reason for this decision? [Leonard Borchert, France]	Noted. This was a decision by the AR5 author team.
127517	34	53			[PROGRESS] "necessary and justified" is too much. The decisions of the AR5 authors could have been different, and the justification used was not universally agreed on. [Trigg Talley, United States of America]	Taken into account. Text has been deleted to avoid duplication. Response has been given in Box 4.1.
70311	34		34		The overlap of content between sections 4.3.3.2, 4.4.3.2, and 4.5.3.2 is interesting. I can see that the authors have tried to not be repetative by gradually adding information as you progress through sections, but this does not make total sense as if I were to only read section 4.3.3.2, I get a very different understanding of what will change. I beleieve that this section should contain all information of projected changes. Then to avoid overlap, mabe the later sections can simply focus on what is different during the earlier periods when compared to this earlier section which details 21st century changes. [Shayne McGregor, Australia]	Taken into account. We have tried to follow the suggestion, dealing with the tension between avoiding repetition, keeping the mentioned subsections consistent, and keeping the time-stratified sections reasonably self-contained.
87831	35	8	35	9	Please note that Brunner et al. also arrives to similar conclusions, so it would be good to include this citation: "Reduced global warming from CMIP6 projections when weighting models by performance and independence." Lukas Brunner, Angeline G. Pendergrass, Flavio Lehner, Anna L. Merrifield, Ruth Lorenz, and Reto Knutti Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-23, 2020 [Katarzyna Tokarska, Switzerland]	Accepted and included.
132473	35	8	35	9	Jimenez-de-la-Cuesta and Mauritsen (2019) shouldn't be cited here I think, since that is not used as a constraint on CMIP6 projections. That publication is used extensively in Chapter 7 to help assess ECS and TCR ranges, which then informs the emulator projections. [Kyle Armour, United States of America]	Accepted and deleted.
50825	35	11	35	12	Does this mean no palaeo data was used? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. "Recent" has been added to line 8. Paleo-information enters the Ch07 assessment of ECS and TCR, but it would distract strongly if mention of paleo-information was added to this paragraph.
50827	35	20	35	20	As per earlier comment, it would be helpful to include temperature rise relative to the pre-industrial too. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106931	35	20	35	20	For the very likely range affected by internal variability, I would use all available members to get a robust statistics . I would also add somewhere "providing unpredictable major or recurrent volcanic eruptions". [Christophe CASSOU, France]	Taken into account (volcanoes). However, IV is estimated using large ensembles.
79723	35	23	35	25	It is correct for the median/mean and upper bound but there are quite large differences for the lower bound. It would be interesting to 1) redo the 3 analysis with a similar or quasi-similar and more complete set of models 2) to possibly comment on the lower bound difference [Laurent Terray, France]	Taken into account. FGD uses updates on estimates and uses language accordingly.
66993	35	23	35	25	I agree that results are "consistent", particularly for the upper bound of projected warming ranges. Lower bounds, however, seems a bit more dependent on the method -- which might deserve a few words. In the latest submitted version of Ribes et al., we've tried to tackle this issue a bit. [Aurélien Ribes, France]	Taken into account. FGD uses updates on estimates and uses language accordingly.
50829	35	27	35	27	It would be useful to add a sentence here: 'By the middle of the century, the very likely temperature range of the highest and lowest emissions pathways are almost completely distinct.' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. SOD already expressed exactly this; has been re-formulated to make statement more succinct.
26863	35	29	35	30	Model ECS is certainly an important factor for the differences in model spread between CMIP6 and CMIP5. The chapter should also discuss that in addition to this spread the storyline of green house gases, aerosol and landuse is different between CMIP5 RCP and CMIP6 SSP for a given radiative forcing. For example SSP5-8.5 provides hihgher temperature in 2100 than RCP8.5. This is seen in integrated model results ONeil et al 2016, and certainly some modeling groups tested RCP and SSP model response with the same model version. The differences are not necessarily in the same direction for all pairs of RCP and SSP. Is it discussed somewhere in the chapter? It should, as the way it could affect the model spread. [Eric Brun, France]	Taken into account. The FGD assessment is now more nuanced concerning the causes of the higher GSAT spread in CMIP6 than in CMIP5. The material has been moved from 4.3.4 to 4.3.1, where the CMIP6 results are discussed. Furthermore, the completely re-vamped Section 4.6.2 now has an explicit comparison between RCPs and SSPs.
96423	35	29	35	34	No reason is given why the "very unlikely" modelled warming rates cannot be ignored. The text here gives not more detail than the statement in the ES. The scientific reasoning for not ignoring them needs please to be added in this assessment or the assessment needs revision. Please explain why they are judged "very unlikely" and why they are still considered useful. [Nicole Wilke, Germany]	Taken into account. The text states the simple logical conclusion that very unlikely is different from impossible. Text has been rephrased.
34879	35	29	35	37	It is good to see that the SOD admits that the higher warming rates of CMIP6 models compared to CMIP5 are unlikely to happen. Please see general comments #2 and #3 above. [Jim O'Brien, Ireland]	Noted.
71281	35	29	35	37	It may be better to move this paragraph after Fig.4.9. [Kenji Taniguchi, Japan]	Taken into account. This material has been moved to 4.3.1., because it is about the CMIP results and not the assessed ranges.
127519	35	32	35	33	Update references. [Trigg Talley, United States of America]	Taken into account. This material has been moved to 4.3.1., because it is about the CMIP results and not the assessed ranges.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79725	35	33	35	35	it is not clear to what extent this whole section is providing any rationale for these likelihood and confidence statements [Laurent Terray, France]	Taken into account. The FGD assessment is now more nuanced concerning the causes of the higher GSAT spread in CMIP6 than in CMIP5. The material has been moved from 4.3.4 to 4.3.1, where the CMIP6 results are discussed. Furthermore, the completely re-vamped Section 4.6.2 now has an explicit comparison between RCPs and SSPs.
50831	35	34	35	34	The higher warming rates simulated by the more sensitive CMIP6 models are assessed unlikely to occur but cannot be excluded' - please could you explain why, is this if observations track the models well? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text states the simple logical conclusion that very unlikely is different from impossible. Text has been rephrased.
12207	35	34	35	37	This needs a citation [Bryan Weare, United States of America]	Taken into account. The text states the simple logical conclusion that very unlikely is different from impossible. Text has been rephrased.
132475	35	42	35	52	Is it not possible to apply the different constraints to the same set of models? It seems messy that some of the differences are coming from model selection. [Kyle Armour, United States of America]	Taken into account. The approaches have been updated by applying them to a more uniform set of models and to more scenarios. But since the assessment must rely on the published papers, there is a limit to how much harmonisation has been possible.
106933	35	42	35	54	For clarity and to really see the changes, instead of 3 panels, I would draw one single panel with all the GSAT estimation together [Christophe CASSOU, France]	Rejected. Choices must be made, and the current assembly is considered the most effective to make the steps visible.
42717	35	42			Figure 4.9 strikes me as being very important in coming to the top-level global warming projection in the report. As this method is a departure from previous reports, I'd suggest that this figure should be included in the SPM. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The figure is included in the TS; the SPM does not provide enough space for this technical discussion.
87555	36	4	36	6	This notes that is methodological, should be anticipated in the methods section, where the differences between AR5 and this work are explained. [Valentina Roberta Barletta, Denmark]	Rejected. This is a crucial aspect of the Ch04 assessment, and information must be kept reasonably local. Without the concrete results at hand, the method could not be communicated comprehensibly.
102951	36	4	36	6	This notes that is methodological, should be anticipated in the methods section, where the differences between AR5 and this work are explained. [Philippe Tulkens, Belgium]	Rejected. This is a crucial aspect of the Ch04 assessment, and information must be kept reasonably local. Without the concrete results at hand, the method could not be communicated comprehensibly.
66989	36	4	36	8	Note that several studies, eg, Ribes et al., are based on HadCRUT4-CW (not HadCRUT5). The revision in historical warming can also be partly due to the shift from GMST to GSAT. [Aurélien Ribes, France]	Taken into account. The FGD assessment is based on Cross-Chapter Box 2.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41407	36	4	36	11	The combination of historical warming and projected warming appears to become the most worrying issue of AR6 WGI. Currently, it's a mess, not only because of the usual new reference periods, the GMST correction compared to AR5, and introduction of historical GSAT, but also, and most importantly, because the line of sight to the IPCC assessment informing the Paris Agreement is completely lost. Why do you cite GMST again here (HadCRUT5), while there has been a lot of effort going into providing historical GSAT warming for consistency? GSAT historical warming of 0.91 degC is provided in the SPM. As you rightly point out, these estimates matter a great deal, for policy makers in particular. This whole issue should to be revisited across chapters by all the authors involved. [Alexander Nauels, Germany]	Taken into account. The FGD assessment is based on Cross-Chapter Box 2.3.
96425	36	5	36	5	In the individual chapters different data sets are used to calculate temp. difference since 1995-2014. This needs explaining. Please see also our comment on the entire report regarding the lack of consistency across chapters. [Nicole Wilke, Germany]	Taken into account. The FGD assessment is based on Cross-Chapter Box 2.3.
6659	36	6	36	7	HadCRUT5 is just one of several observationally based datasets. It is the newest, and possibly therefore the best, but it is so new it has not at the time of writing this comment been made publicly available, and so has not been subjected to independent scrutiny. Please see the last part of comment 157. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FGD assessment is based on Cross-Chapter Box 2.3.
79727	36	8	36	17	I am not sure that Ribes et al. 9999 should appear after "emulator" as the Ribes Method has nothing to do with an emulator (it is a kriging approach, very similar to a one-step Kalman-filter approach). Note that the Ribes et al. 2020 paper reference (still in revision at this time I believe) has now a different title than the one in the chapter 4 reference list [Laurent Terray, France]	Taken into account. There was an "and" missing in the SOD text, introduced during production by TSU. Ribes et al. reference has been updated.
132477	36	9	36	14	I have not read Ribes, but it is unclear here whether the emulator results you're referring to are from that paper or from your own analysis based on the emulator methodology you describe above. [Kyle Armour, United States of America]	Taken into account. There was an "and" missing in the SOD text, introduced during production by TSU. Ribes et al. reference has been updated.
45497	36	13	36	14	This sentence, particularly the “Table 4.6...” half sentence, reads weirdly. I suggest rephrasing. [Leonard Borchert, France]	Taken into account. This text has been deleted.
50833	36	19	36	23	These are two important sentences detailing projected timing of reaching 1.5C - could they be emboldened, in their own paragraph? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Emboldening sentences here would go counter to agreed-upon formatting, but the text has been revised for greater clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102953	36	19	36	32	General comment on need to better explain near-term warming in the context of SR1.5 findings. This chapter (and also the SPM) needs a clearer, consolidated explanation of the near-term prospects for crossing the 1.5°C temperature threshold and how to interpret such information. At present it is challenging to piece together the messages from this text, combined with Ch2.3 and SR1.5. It is therefore necessary to have a single place in the report to explain and synthesise this question, encompassing the following: - SR1.5 chiefly referred to GMST, whereas AR6 refers chiefly to GSAT - How to now interpret the SR1.5 message that warming has already reached 1°C above pre-industrial levels and is like to reach 1.5°C in 2030-52; - Clarify the text in the parapraph selected, which is quite confusing. It states for example, that the best estimate for when a level of 1.5°C will be reached is about ten years earlier than the best estimate of the SR1.5, but then states that the broader estimate is still in line with SR1.5, and refers to both 2030 and 2040 as the best estimate witin the same paragraph. - How policymakers should interpret crossing of the 1.5°C threshold, noting that policymakers are likely to notice crossing of the threshold using a GMST series (for example the WMO state of the climate), and this could happen in the 2020s (though possibly later since GMST is a cooler metric than GSAT?). - Brief synthesis of what crossing the threshold in the near-term means for meeting the goal of limiting warming to 1.5°C by the end of the century (noting that mitigation pathways are principally within the scope of WGIII). [Philippe Tulkens, Belgium]	Taken into account. The text has been thoroughly revised; the observational aspects of historical warming have been updated in Cross-Chapter Box 2.3, in close alignment with the revisions to Section 4.3.4.
34881	36	19	36	37	The SOD conclusion that GSAT (apart from internal variability) will reach 1.5°C by 2030 is to be seriously questioned scientifically as well as whether it amounts to political scaremongering. How can GSAT possibly increase by 0.4°C (assuming that the current figure is 1.1°C, which is an artificial figure) within one decade? Please see general comments #1, #2 and #3 above. [Jim O'Brien, Ireland]	Noted. This assessment is based on multiple lines of evidence..
19851	36	19	36	37	How do this likelihood and confidence statements compare with the conclusions reached in subsection 4.3.1.1 concerning GSAT? This is a quite logical question, which deserves to be answered here. The tables 4.2 and 4.6 are built in such a way that they do not help much. [philippe waldteufel, France]	Taken into account. Both 4.3.1.1 and 4.3.4 have been revised, to make the connection between them clearer, and duplication has been removed.
21661	36	19	36	37	Given the high policy relevance of when we may breach these thresholds in the context of the Paris agreement a more quantitative assessment of these timings is warranted and necessary. This should include uncertainty in where we are today arising from the chapter 2 assessment. But equally note that this is discussed both before and then two pages later in almost the exact same manner. See suggestion in another comment to integrate these better in a (cross-chapter) box. [Peter Thorne, Ireland]	Taken into account. The FGD assessment explicitly includes the uncertainty of the historical warming, as well as that from internal variability. Greater coherence with the Ch02 assessment is reached through Cross-Chapter Box 2.3. The duplication in Section 4.4.1.1 has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50835	36	25	36	25	later than during the near-term for scenario SSP2-2.6' - please specify by how much later. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence no longer appears in the FGD.
66991	36	28	36	32	I think the discrepancy with SR1.5 is at least partly related to the choice of using GMST (in SR1.5), vs GSAT (in AR6). This could be said explicitly. Of course using GSAT ensures better consistency with many other indices! [Aurélien Ribes, France]	Taken into account. The FGD best estimate of historical GMST increase is the same as for GSAT (see Cross-Chapter Box 2.3).
132479	36	30	36	31	It's not clear what "provision of enhanced estimates of the historical observational record" means. Do you mean a larger amount of historical warming than in AR5? [Kyle Armour, United States of America]	Taken into account. The sentence no longer appears in the FGD.
6661	36	30	36	32	Please see comment 159 on an earlier occurrence of this questionable statement. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The FGD text contains a more nuanced analysis of the change in timing.
21659	36	31	36	31	Per comment on the ES enhanced is value-laden here and should be replaced with a different wording. [Peter Thorne, Ireland]	Taken into account. The sentence no longer appears in the FGD.
45499	36	31	36	31	Is “re-assessed” the correct word here? [Leonard Borchert, France]	Taken into account. The sentence no longer appears in the FGD.
16521	36	34	36	52	If the chemistry models are correct in simulating an increase in OH of 8% (previous paragraph), then presumably the inversions in this paragraph would be very different? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is more relevant comment to Chapter 6. Chapter 6 notes that there is conflicting information from global ESMs, CCMs and observational inversions regarding changes in OH over the 1980 to 2014 period.
50837	36	36	36	36	Suggested edit: 'more likely than not to be missed in the mid-term period..' (remove additional 'not') [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence no longer appears in the FGD.
114469	36	40	37	3	Use ranges for timig of hitting thresholds, rather than single years? [Jan Fuglestvedt, Norway]	Taken into account. Alas, the ranges are so asymmetric, and the degree of asymmetry so strongly depends on the scenario, that the central values must also be given. But text has been revised for clarity.
106283	36	40	37	3	Only providing the ranges can avoid reader focussing on the single year estimates provided here. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Alas, the ranges are so asymmetric, and the degree of asymmetry so strongly depends on the scenario, that the central values must also be given. But text has been revised for clarity.
19853	36	40	37	3	In this Table 4.6 some figures indicated for SSP-1-1.9 are puzzling because they correspond to more warming than SSP1-2.6. Please comment. [philippe waldteufel, France]	Taken into account. The FGD table no longer shows this phenomenon, presumably owing to the more even use of CMIP6 models and scenarios across the updates of the underlying papers.
9705	36	42	36	52	I understand internal variability is not considered here, but maybe it should, especially for the second half of the Table because the 1995-2014 may have been colder than it could have. [Olivier Boucher, France]	Taken into account. The FGD assessment explicitly includes the uncertainty of the historical warming, as well as that from internal variability. Greater coherence with the Ch02 assessment is reached through Cross-Chapter Box 2.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9707	36	42	36	52	I have a hard time reconciling table 4.6 with material presented elsewhere in this report. Specifically I am surprised by the fact that SSP119 does not cross the 2°C threshold at the 95% level. This seems in contradiction with statements made in Chapter 3 (likely range of human-induced warming is 0.8 to 1.4°C in 2010-2019 relative to 1850-1900) and chapter 4 (SSP119 very likely to be 0.3 to 0.9 °C warmer in 2081-2100 relative to 1995-2014. Admittedly the two present-day periods are not the same (an issue I flagged earlier), but combining the [0.8-1.4°C] likely range and the [0.3-0.9°C] very likely range results in a much larger probability of crossing the 2°C threshold. Given that SSP119 overshoots in some models, the probability of crossing the threshold may even be higher. See this simple python script: import numpy as np nbpt=10000 t1 = np.random.normal(1.1, 0.3, nbpt) print('T1 likely percentiles=',np.percentile(t1,17.0),np.percentile(t1,83.0)) t2 = np.random.normal(0.6, 0.3/1.645, nbpt) print('T2 very likely percentiles=',np.percentile(t2,5.0),np.percentile(t2,95.0)) t=t1+t2 print('T1+T2 very likely percentiles=',np.percentile(t1+t2,5.0),np.percentile(t1+t2,95.0)) print('T1+T2 2oC corresponds to percentile=',np.count_nonzero(t1+t2>2)/float(nbpt)*100.) [Olivier Boucher, France]	Taken into account. Diagnosing anthropogenic historical warming is tantamount to our best understanding of what caused this warming. Our tools include climate model simulations of the historical period, which invokes uncertainties in addition to observational uncertainties. Historical warming can hence be more tightly constrained through observations than through our interpretation of its causes. In addition, it is very unclear how the uncertainties in attributed historical warming are correlated with the uncertainties in the projections. Therefore, we use the observed historical warming to anchor projected change relative to 1995--2014 back to change since 1850--1900 (see Cross-Chapter Box 2.3), and the uncertainty ranges of the threshold-crossing times are not as large as suggested in this comment.
26865	36	42	36	52	Table 4.6 is difficult to reconcile with material presented elsewhere in this report. Specifically it is surprising that SSP1-1.9 does not cross the 2°C threshold at the 95% level. This seems in contradiction with statements made in Chapter 3 (likely range of human-induced warming is 0.8 to 1.4°C in 2010-2019 relative to 1850-1900) and chapter 4 (SSP1-1.9 very likely to be 0.3 to 0.9 °C warmer in 2081-2100 relative to 1995-2014. Admittedly the two present-day periods are not the same, which is an issue for the concisness of the report, but combining the [0.8-1.4°C] likely range and the [0.3-0.9°C] very likely range results in a much larger probability of crossing the 2°C threshold. Given that SSP1-1.9 overshoots in some models, the probability of crossing the threshold may even be higher. [Eric Brun, France]	Taken into account. Diagnosing anthropogenic historical warming is tantamount to our best understanding of what caused this warming. Our tools include climate model simulations of the historical period, which invokes uncertainties in addition to observational uncertainties. Historical warming can hence be more tightly constrained through observations than through our interpretation of its causes. In addition, it is very unclear how the uncertainties in attributed historical warming are correlated with the uncertainties in the projections. Therefore, we use the observed historical warming to anchor projected change relative to 1995--2014 back to change since 1850--1900 (see Cross-Chapter Box 2.3), and the uncertainty ranges of the threshold-crossing times are not as large as suggested in this comment.
26867	36	42	36	52	Table 4.6 : Internal variability seems not considered here, but maybe it should, especially for the second half of the Table because the 1995-2014 may have been colder than it could have. [Eric Brun, France]	Taken into account. The FGD assessment explicitly includes the uncertainty of the historical warming, as well as that from internal variability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41409	36	42			You have to be clear here which historical warming you are assuming, given the GMST/GSAT confusion across chapters. These estimates appear to be consistent with the SPM, hence 0.91 degC historical GSAT warming between 1850-1900 and 1995-2014. Please add this information in the caption. [Alexander Nauels, Germany]	Taken into account. The FGD assessment explicitly includes the uncertainty of the historical warming, as well as that from internal variability. Greater coherence with the Ch02 assessment is reached through Cross-Chapter Box 2.3.
116313	36		36		Could the assessment of when levels of warming are reached also include the outcome of simulations with random volcanic forcing and their implications? It would be good to have everything at the same place. [Valerie Masson-Delmotte, France]	Taken into account. However, this would require a combination of the approach to assessed GSAT with additional simulation of random eruptions, and this information has simply not been available.
87557	37	1	37	1	Please explain also the meaning of a range with one number and one n.a. [Valentina Roberta Barletta, Denmark]	Noted. The n.a. is explained in the caption.
102955	37	1	37	1	Please explain also the meaning of a range with one number and one n.a. [Philippe Tulkens, Belgium]	Noted. The n.a. is explained in the caption.
50839	37	1	37	2	Please explain why the near term temp rise is lower for SSP1-2.6 than for SSP1-1.9. Is this a near-term component of aerosol reduction necessary for the level of decarbonisation necessary to hit an 1.9RCP? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A pointer has been added.
44985	37	1	37	2	Left border of the figure is not solid. [Mustafa Tufan Turp, Turkey]	Noted. Unclear what is meant.
24025	37	6	40	20	I would suggest making more use of initialised predictions for the coming decade, since these forecasts are of widespread interest and can be assessed against observations much earlier than longer term projections. Perhaps multi-model ensemble mean maps of temperature and precipitation for the coming decade could be presented? [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Particular attention has been paid to including results from DCPP initialised predictions in the FGD. However, we don't include any additional figure on that.
7465	37	6	47	44	In Section 4.4. (e.g. 4.4.3 NAO, PDV and AMV) initialized prediction publications are extensively used to explain the near-term climate change. I wonder whether initialized decadal climate forecasts and skill maps are useful in 4.4, such as for temperature and precipitation maps. There is high confidence of using decadal forecasts for such parameters (at least for temperature), which may support the authors intention to make an estimate for near-term climate. [Wolfgang Müller, Germany]	Taken into account. Particular attention has been paid to including results from DCPP initialised predictions in the FGD. However, we don't include any additional figure on that.
19855	37	8	37	8	What is meant by "assessed"? In this section it probably means issuing confidence and likelihood statements. [philippe waldteufel, France]	Noted. Assessment is used in its standard interpretation.
19857	37	8	37	11	Chances are this section will partly repeat information already given elsewhere, for example in the previous section, which considers the whole near- mid- long-term span. Admittedly, the outline highlights near term; still, repetitions should be avoided. [philippe waldteufel, France]	Accepted, repetition has been deleted.
52997	37	18			GSAT subsection could be merged with 4.3.4 [Hervé Douville, France]	Taken into account. Repetitions have been deleted, but some material sits better in this separate subsection.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106941	37	18			I would add one additional diagnostic in this section related to the probability of hiatuses at near-term. I think that it is an important issue to specifically address following the big buzz hiatus had after AR5. I would suggest to provide a figure showing the pdf of the GSAT trend over 2020-2040 for all the scenarios using all members to show the range of outcomes and have an objective and quantitative evaluation of the probability for slowdown/acceleration of warming. This would be a nice follow-on to the Cross-Chapter Box 3.1 and this would clearly state that modulation of the trends due to natural variability is not a surprise, well assessed and not hidden (important for communication issue). Link to PDV and AMV would a cherry on the cake. How about a panel b) with an x-y graphe (x=AMV, y=PDV) with a color code for the amplitude of the trend (1 dot, one member)? I can help with this if you decide to follow my suggestion. [Christophe CASSOU, France]	Noted. Figure 4.39 later on does a fair amount of the communication. Since space is tight and there is Cross-Chapter Box 3.1 already, any discussion of future hiatus/surge periods are best placed in that box.
15495	37	20	37	24	The temperature projection presented in AR5 is Global Mean Surface Temperature (GMST), not Global Surface Air Temperature (GSAT) (Ref.: AR5 WGI SPM, P.18, Section E.1). Please revise. [SAI MING LEE, China]	Rejected. As AR5 Section 11.3 shows, it is GSAT that is being used. However, AR5 had not been as consistent in its terminology as AR6 is; hence this misleading labelling in the SpM. .
127521	37	20		27	[PROGRESS] The AR5 assessment of near term GSAT was too low mainly through the use of HadCRUT3 (which had unrealized low biases) to calibrate the CMIP5 ensemble. The assessment here should acknowledge the new assessment is higher and explain why. [Trigg Talley, United States of America]	Taken into account. The comparison against AR5 has been deepened.
84253	37	36	38	24	not clear here how do the results from DCPP differ from projections [Annalisa Cherchi, Italy]	Noted. DCPP results are shown together with projections in Figure 4.10 and are put into context in the text. No action item discernible no change.
106103	37	36	41	55	The biggest concern I have with this chapter is the lack of use of DCPP data from CMIP6. This entire section written about near-term changes and there is little results from the new DCPP experiments. This is odd, especially with the conclusion earlier that initialisation adds skill. [Noel Keenlyside, Norway]	Taken into account. Particular attention has been paid to including results from DCPP initialised predictions in the FGD.
116315	37		37		I suggest to report a range of years, not a single year. [Valerie Masson-Delmotte, France]	Taken into account. The table now only contains 20-year ranges for threshold-crossing times.
12209	38	1	38	9	Very little here refers directly to the very important Fig.4.10 [Bryan Weare, United States of America]	Not applicable. The figure is removed.
96427	38	1			We kindly ask the authors to relate the climate change signal of AR6 to the one of AR5, discussed on page 4-37 line 20, i.e. to the reference period 1986 - 2005. [Nicole Wilke, Germany]	Taken into account. The comparison against AR5 has been deepened.
106095	38	6	38	6	There are 10+ prediction systems contributing to the DCPP, yet here you only present 2 DCPP models. This needs to be updated. [Noel Keenlyside, Norway]	Taken into account. By the time of the SOD, only 3 DCPP forecasts were available, for 2019--2028, and they are all shown in Figure 4.10. The FGD includes additional forecasts now available.
106097	38	6	38	6	Section 4.2.3 does not provide a "detailed assessment of the performance and skill of initialized decadal predictions" from the recent DCPP. In fact there are basically no results from the DCPP in this chapter. This needs updating. [Noel Keenlyside, Norway]	Taken into account. Text modified. Particular attention has been paid to including results from DCPP initialised predictions in the FGD. However, we don't include any additional figure on that.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50841	38	11	38	20	This is a repetition of p4-36 lines 19-31. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The repetition is deleted and the sentences are revised considerably.
21663	38	11	38	24	This is almost line for line the third time this has been covered. These assessments should be reconciled, placed together and highlighted. The use of a box given the policy relevance of this question may be warranted and, given the need to ingest inputs from chapter 2 on observational uncertainty and other chapters on emulators, constraints etc. may arise to a cross-chapter box on when we may cross certain temperature thresholds. [Peter Thorne, Ireland]	Taken into account. Text has been removed here.
71131	38	11	38	24	There are relevant papers examining timing of reaching the 1.5C and 2C thresholds that could be referred to here. The influence of the decadal variability in the Atlantic (Smith et al. 2018) and the Pacific (Henley and King 2017) is substantial with respect to the timing of these thresholds being reached under continued emissions scenarios. Henley B J and King A D 2017 Trajectories toward the 1.5 °C Paris target: Modulation by the Interdecadal Pacific Oscillation Geophys. Res. Lett. 44 2017GL073480 Smith D M et al 2018 Predicted chance that global warming will temporarily exceed 1.5 °C Geophys. Res. Lett. 45 11895–903 [Andrew King, Australia]	Taken into account. Text has been removed here. Discussion and annual exceedance has been added to 4.3.4.
87559	38	13	38	13	Stating that for scenario SSP1-1.9 The 1.5 warming level is more likely than not to be reached should be explained wrt the entry in table 4.6 with a range (number, n.a), see previous comment. [Valentina Roberta Barletta, Denmark]	Taken into account. Text has been completely re-written.
102957	38	13	38	13	Stating that for scenario SSP1-1.9 The 1.5 warming level is more likely than not to be reached should be explained wrt the entry in table 4.6 with a range (number, n.a), see previous comment. [Philippe Tulkens, Belgium]	Taken into account. Text has been completely re-written.
108117	38	15	38	15	Instead of the term “bias-corrections” I suggest to use the term “bias adjustments”, which is explained in Chapter 10 Section 10.3.1.4.2 and used in Chapter 2, 8, 10 and 12. Probably in the case of climate predictions, the term “bias correction” is more appropriate. [Claas Teichmann, Germany]	Not applicable. Unclear which text this applies to.
127523	38	29			[PROGRESS] Perhaps the AR5-assessed GSAT projection should be plotted as well, so that it would be clear what is different in this assessment. [Trigg Talley, United States of America]	Rejected. There is no AR5-assessed GSAT projection available. The assessed GSAT projection in AR6 is based on synthesizing existing methods and literature. However, the new Figure 4.3.5 in FGD provides comparison between RCPs in AR5 and SSPs in AR6.
41411	38	29			There appears to be an error in this fugure. While you are saying that you are using GSAT, in the caption you provide 0.86 degC historical warming which isn't GSAT but HadCRUT5 GMST. The rather sloppy treatment of this highly policy relevant issue of combining historical and projected warming is worrying. Please correct the figure and add 0.91 degC historical GSAT warming for the secondary y-axis. Apologies in advance if i got this wrong. This would still show though that there needs to be more diligence applied in this context. [Alexander Nauels, Germany]	Not applicable. Figure 4.10 in the SOD is removed in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6663	38	34	38	35	HadCRUT5 is not "observations". Please see comment 163. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure 4.10 in the SOD was removed in the FGD. However, please note that we use "observations-based estimate" for HadCRUT5 in Box 4.1 Figure 1 in the FGD.
127525	38	38	38	43	Given the claims earlier in the chapter that the DCPP efforts only provide a single year of improved skill over the free running models, it's unclear what benefit is gained by using them in this figure. In the global mean there is no additional info beyond the standard CMIP6 ensemble. [Trigg Talley, United States of America]	Not applicable. Figure 4.10 in the SOD is removed. However, this comment is relevant to the BOX4.1 Figure 1 where the DCPP results are shown. Section 4.2.3 also discusses that the value added by initialization may be greater than previous thought. Section 4.4.1.1 assesses that forecasts initialized from recent observations simulate GSAT changes for the period 2019-2028 relative to the recent past that are consistent with the assessed very likely range in annual-mean global surface temperature. The author team thinks this is also important information to deliver.
127527	38	50	39	4	It is noticeable that the spatial pattern of near-term warming is uniformly positive, which is different to the pattern seen in recent decades which shows cooling in the Southern Oceans and south of Greenland. It is possible that unresolved excess melting from both ice sheets (and ice shelves) is affecting these local ocean areas. Since most (all?) of the CMIP6 ensemble being assessed here are assuming a static ice shelf, they will underestimate the net freshwater flux. To the extent that the ice sheet mass loss is anthropogenic, this may be a negative feedback that is not incorporated into the models, and thus the spatial patterns shown in 4.11 will overestimate (specifically) the warming in the Southern Oceans. [Trigg Talley, United States of America]	Noted. Those are very good comments. Chapter 9 discusses those aspects and limitations of the state-of-the art models in CMIP6 particularly on resolving the Southern ocean processes.
50843	38	52	38	52	largest warming occurs in winter' - in reference to the Arctic, should this be summer? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The artic warming is larger in boreal winter than summer. Please refer Figure 4.12.
106935	38	52	38	52	I would say: "this shows with very high confidence that the largest warming…": confidence state added here. [Christophe CASSOU, France]	Not applicable. This is a good suggestion but the confidence level on spatial patterns of surface warming is given in the summary statement in Section 4.5.1.1.
50845	38	56	38	56	Suggested addition: 'trajectories are for well-mixed..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Authors mean "the trajectories for well-mixed GHGs".
84255	38	56	39	1	sentence unclear, better to rephrase [Annalisa Cherchi, Italy]	Rejected. Authors decided to keep the sentence.
87561	39	9	39	9	In the caption DJF and JJA should be described, since in the text the season names are used, not the month names. And the caption here is not the same as in page 153. [Valentina Roberta Barletta, Denmark]	Taken into account. The caption in the text is now same as the caption below the Figure. Authors decided to keep DJF and JJA in the figure caption since they are also mentioned in other parts of the text.
102959	39	9	39	9	In the caption DJF and JJA should be described, since in the text the season names are used, not the month names. And the caption here is not the same as in page 153. [Philippe Tulkens, Belgium]	Rejected. Authors decided to keep DJF and JJA in the figure caption since they are also mentioned in other parts of the text.
127529	39	9	39	14	Repeat convention for stippling and hatching in caption. [Trigg Talley, United States of America]	Accepted. All captions for map figures are considerably revised including detail explanation on stippling and hatching.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79729	39	11	39	11	Is internal variability derived based on the 22 control simulations ? [Laurent Terray, France]	Not applicable. The internal variability is derived based on the models being available. For example, control simulations from 39 and 37 models were used to estimate internal variability for SSP1-2.6 and SSP3-3.7, respectively, in Figure 4.12 which is the SOD Figure 4.11.
50847	39	19	39	26	It would be helpful to add details here about the forcing of aerosols relative to GHGs and their relative time-scales. Otherwise it feels like the issue of short-term warming from a reduction in dirty fuel combustion is being swept under the carpet. The trade off of temp rises in the short and long-term needs to be clarified to policy-makers. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Thank you very much for the good point. The details about the forcing of aerosols relative to GHGs and their relative time-scales are well covered in Chapter 6 and Section 4.4.4 in Chapter 4 also discusses some of aspects.
132481	39	21	39	21	I have not heard the term "spatial efficacies" before, and suggest not using it here. You can just say what you mean "The change in surface temperature per unit ERF for CO2..." [Kyle Armour, United States of America]	Rejected. The term is widely used and the definition of special efficacies is provided in the sentence which is "the change in surface temperature per unit ERF".
19231	39	28	39	31	Is it possible to name a few of these regions, and refer to the hatched areas in figure 4.11? [Anne-Marie Treguier, France]	Rejected. As chapter 4 focuses on global change or large-scale changes, regional details are not included. Detail regional information can be found in chapters 11, 12, and Atlas.
127531	39	34	40	20	[PROGRESS] What about confidence limits (likely, low or high, etc.)? What was learned since AR5? What has changed in AR6? [Trigg Talley, United States of America]	Taken into account. The summary statement including confidence level is added in 4.4.1.3. The changes in AR6 are consistent with AR5. Here is the summary statement: Consistent with AR5, we conclude that projected changes of seasonal mean precipitation in the near term will increase at high latitudes. Near-term projected changes in precipitation are uncertain mainly because of natural internal variability, model uncertainty, and uncertainty in natural and anthropogenic aerosol forcing (medium confidence).
11091	39	36	39	37	It is questionable to say "much of the non-robustness in near-term projections is attributable to natural internal variability". The aims of projection is to predict the variability in the future. Much of the non-robustness in near-term projections comes from the limitation of our understanding of natural internal variability. [Wen Wang, China]	Taken into account. The sentence was removed and the role of internal variability on the near-term precipitation change was more clearly stated in the FGD.
127533	39	37	39	37	If the phrase "mean precipitation" is referring to the "global average of precipitation", authors should use the latter phrase. There are too many other ways to interpret the word 'mean' (for example, as used on line 45). [Trigg Talley, United States of America]	Taken into account. The sentence is revised accordingly. Section 4.4.1.3 mainly discusses changes in annual mean and seasonal mean precipitation.
50849	39	37	39	39	Section 4.4.1.3 is missing information on projected changes in precipitation extremes, this would be helpful to include here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The projected changes in precipitation extremes are well covered by Chapters 8 and 11.
127535	39	38	39	38	What does "considerably small" mean? Please rephrase. [Trigg Talley, United States of America]	Taken into account. The sentence is revised accordingly. AR5 assessment and advances since AR5 on near-term precipitation changes are included.
28825	39	38			remove "considerably" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. It is deleted and the sentence is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79731	39	41	39	43	I suggest to refer to sections 10.4.2 and 10.6 for illustrative examples of the influence of internal variability on projections at egional scale (worth checking also Chapter 8). [Laurent Terray, France]	Taken into account. We include cross-referencing to Chapter 10 Section 10.4.3 and Chapter 8 Section 8.4.
19859	39	41	39	46	While the information on line 43 is valuable, is it likely that somebody interested in internal variability at long term will look for it in the "near term global climate change" section? Similarly, the second sentence seems to confirm that on the long term natural variability, although large, does not forbid detection of anthropogenic changes in precipitation, even on regional scale. Several readings however are needed to extract this conclusion [philippe waldteufel, France]	Taken into account. The sentences were removed in the FGD and the revised text focuses on aspects of internal variability and anthropogenic forcing on near-term precipitation change.
52999	39	44	39	46	What is the practical relevance of this statement? Large ensembles are useful to quantify internal variability, but they do not necessarily agreed about the forced precipitation response and do not address the decadal predictability of precipitation. Emphasize a stronger signal-to-noise ratio for projected changes in extreme precipitation? [Hervé Douville, France]	Taken into account. The sentence is revised emphasizing the changes in internal variability not mean state.
127537	39	45	39	45	This whole paragraph is confusing. It starts by saying changes are attributable to internal variability, and change is small. Then it says that uncertainty remains significant through the end of the century. Then it ends by saying the changes in decadal precipitation are distinguishable. If the signal-to-noise ratio is small and uncertainty is high, then how can changes be distinguishable? Please clarify. [Trigg Talley, United States of America]	Taken into account. The whole paragraph is considerably revised with summary of AR5 assessment as a start and discussing advances since AR5 on near-term precipitation changes. The role of internal variability and anthropogenic aerosol forcing is more clearly stated.
106939	39	46	39	46	I think that a confidence statement can be added for the summary sentence of the paragraph. I would put high confidence. [Christophe CASSOU, France]	Rejected. Sentence describes statement of fact, citing literature, rather than formal assessment.
42719	39	46			When discussing the relationship of the anthropogenic signal to decadal variability a caution is required not to understate the importance of the climate change signal. For example, consider a region in figure 4.12 where the 2021-40 mean change is enhanced by 20% and that the std of 20 year means in this region has a slightly higher magnitude. This region would be hatched in figure 4.12. This does not mean that the climate change signal is not important compared to variability because the probability of getting a 20-year period with high precip will be considerably enhanced over the historical period. Basically, the baseline for the variability in raised. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Displaying robustness and uncertainty in the future climate change has been carefully revised across the WGI report. You can find the detail information on displaying robustness and uncertainty in maps across the WGI report from Cross-Chapter Box Atlas.1. Associated with your point, Chapter 8 uses the less strict threshold for displaying robustness for precipitation change so that the forced signal is not undermined.
127539	39	48	39	56	Many messages in this paragraph are confusing -- first citing many papers indicating that aerosol impacts regional patterns of precipitation, but later closing the paragraph by saying there is low to medium confidence in the impacts of aerosols on precipitation. Are authors saying the papers indicating impacts are wrong? Work is needed to clarify and harmonize the messages. [Trigg Talley, United States of America]	Taken into account. The paragraph is considerably revised with more clarification on the role of internal variability and anthropogenic aerosol forcing in near-term precipitation changes. Authors don't mean that papers are wrong. However, we assess that because of large uncertainty in the aerosol radiative forcing and the dynamical response to the aerosol forcing there is medium confidence in the impacts of aerosols on near-term projected changes in precipitation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50851	39	51	39	51	aerosol changes' - please specify if these are assumed to be increases or decreases. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Section 4.4.4 and Chapter 6 discuss the trajectory of future aerosol forcing under the five SSP scenarios in detail.
79733	39	54	39	56	I suggest to add: ...in the aerosol radiative forcing "and model forced response" [Laurent Terray, France]	Accepted. The sentence is revised as follows: Because of the large uncertainty in the aerosol radiative forcing and the dynamical response to the aerosol forcing there is medium confidence~.
106937	39	55	39	55	What the rationale for "low-to-medium"? I would pick either "low" or "medium". [Christophe CASSOU, France]	Accepted. Changed to "medium confidence".
116317	39		39		The discussion about pattern of ERF needs to be combined with the section on pattern scaling. [Valerie Masson-Delmotte, France]	Taken into account. Section 4.2.4 includes the discussion, particularly for precipitation.
87563	40	3	40	3	Same as before, define DJF and JJA. And this version of the caption (ll 3-8) I more expanded than the one at page 154. [Valentina Roberta Barletta, Denmark]	Rejected. Authors decided to keep DJF and JJA in the figure caption since they are also mentioned in other parts of the text.
102961	40	3	40	3	Same as before, define DJF and JJA. And this version of the caption (ll 3-8) I more expanded than the one at page 154. [Philippe Tulkens, Belgium]	Rejected. Authors decided to keep DJF and JJA in the figure caption since they are also mentioned in other parts of the text.
127541	40	3	40	8	The caption on this page is inconsistent with the caption on page 4-154. [Trigg Talley, United States of America]	Accepted. The caption is revised considerably and consistent with the caption under Figure 4.12.
70963	40	13	40	20	We are submitting a manuscript (G Zappa, E Bevacqua and TG Shepherd: “The real mean signal to noise of multi-model climate change projections”) to Int. J. Climatol. in which we propose a new methodology to diagnose the robustness and magnitude of future projected changes from multi-model ensembles. Rather than basing the stippling and hatching of spatial maps on the signal to noise of the multi-model mean response - as it is standard in the IPCC, and in this chapter - our proposed approach evaluates the mean forced signal-to-noise of the individual model responses. This enables us to make statements on regions where a large future change compared to year-to-year variability is plausible, regardless of whether the mean signal is robust across the ensemble. While previously proposed alternative approaches were also able to discriminate between regions with a small response from those with large uncertainty, this method has the benefit of being as simple, and with as few free parameters, as the standard IPCC approach, while explicitly providing information that is relevant for risk assessment, i.e. the potential for a large change. For mean precipitation changes, we find that the majority (58% in surface area) of the unmarked regions and part (18%) of the hatched regions from the AR5 hid climate change responses that are on average large compared to the year-to-year variability. Based on the newer CMIP6 ensemble, we identify that a considerable risk of large annual-mean precipitation changes, despite the lack of a robust projection, exists over 21% of the global land area, mostly including Central America, Northern South America (including the Amazon), Central and West Africa (including parts of the Sahel) and the Maritime continent. You may contact g.zappa@isac.cnr.it for the submitted version of this paper. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reference is added in Chapter 4 and the proposed method is evaluated as one of methods displaying robustness of future change in Cross-Chapter Box Atlas.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28827	40	13			RCP4.5 or 7.0 may be more relevant than 8.5 to policymakers making planning decisions [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. All map figures in FGD show changes under SSP1-2.6 and SSP3-7.0 (instead of SSP5-8.5).
106943	40	20	40	20	Can anything be added from the DCPP runs? [Christophe CASSOU, France]	Not applicable. The useful skill for decadal precipitation changes by DCPP is just found over specific regions such as Sahel and some part of Europe associated with the Atlantic multi-decadal variability. Those aspects are assessed in Section 4.4.3.5. Differently from global surface temperature, the state-of-the-art models have difficulties in capturing the recent decadal trend in global mean precipitation.
4107	40	23			The subsection on near-term monsoon projections is well written and it is encouraging that links have been made to the preceding global monsoon assessments in Chapters 2 and 3, including for the assessment statements. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In 4.4.1.4, we included signpost to Annex V on the definition of global monsoon and linkage between CHs 2. 3. and 4. For the linkage between CHs 2. 3. and 4. Technical Summary Box TS.13 on monsoon synthesized past to future changes in global monsoon.
11087	40	25	40	25	After "The global monsoon", add a "(GM)". In addition, IPO is explained at Line 32-33, but AMV is not exlpained. [Wen Wang, China]	Not applicable. The sentence was removed in the FGD.
84257	40	25	40	27	I would move this sentence at the end of the section, using it as potential link to regional monsoons assessment in chapter 8 [Annalisa Cherchi, Italy]	Accepted. The sentence is revised so that the definition of global monsoon is consistent with Annex V and Chapter 8 Section 8.3.2. The cross-referencing to the Annex and Chapter 8 is added.
17929	40	25	40	40	Here as in other chapters discussed above, the same "global monsoon" statements are made that highlight an end of 20th century increase at the expense of explaining the post-World War II decrease partially attributable to the unique combination of aerosols and greenhouse gases, at least insofar as pertains to the West African monsoon. This to say, once more that the variability is not all natural, and associated with multi-decadal patterns of oceanic variability such as the IPO or AMV. Another way of looking at this is to say that IPO and especially AMV are not all natural, not in the second half of the 20th century. [Alessandra Giannini, France]	Not applicable. The role of IPO or AMV on the global monsoon changes during the second half of the 20th century is mainly assessed in Chapters 2 and 3. The aspect of past global monsoon change in the Chapter 4 SOD was deleted in the FGD. You can find assessment on the interplay between internal variability such as IPO and AMV and the GHG forcing in Chapter 10.
106945	40	25	40	40	IPO should be replaced by PDV [Christophe CASSOU, France]	Accepted. IPO is replaced by PDV across the chapter.
53001	40	25			What is a "local" monsoon? [Hervé Douville, France]	Not applicable. It meant 'regional' monsoon. "local" monsoon is deleted from the sentence.
4109	40	29	40	30	Change, "that affect the decadal changes of GM" to "that affect decadal changes of the GM" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. 4.4.1.4 was considerably revised and the sentence was deleted in the FGD.
4111	40	30			Remove "the" before "global monsoon precipitation". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. 4.4.1.4 was considerably revised and the sentence was deleted in the FGD.
50853	40	32	40	32	Please spell out 'AMV' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence is removed. However, 'AMV' is spelled out when it appears first in 4.2.3.
4113	40	33			Remove "the" before "global monsoon precipitation". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. 4.4.1.4 was considerably revised and the sentence was deleted in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4115	40	34			While there is evidence that the recovery trend of GMP "can be explained by the phase change of AMV", it has to be acknowledged somewhere that the positive trend could also be explained by the switch over between aerosol forcing and GHG, which must happen at some point. For example, note the Kitoh et al. (2013) diagrams (also in AR5) in which the forced (i.e. MME-mean) signal switches from a negative to a positive trend at some point in the early 21st century. In a CMIP ensemble of historical and future scenarios, there is no in-phase arrangement of internal variability and thus some component of a negative-to-positive trend reversal must be arising from external forcing. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We incorporated this point in 4.4.1.3 (precipitation). Since aerosol forcing affects precipitation over most of land regions not just global monsoon region. We included the following aspects that in addition to the response to GHG forcing, forcing from natural and anthropogenic aerosols exert impacts on regional patterns of precipitation and aerosol changes induce a drying in the SH tropical band compensated by wetter conditions in the NH counterpart. We also assessed that because of the large uncertainty in the aerosol radiative forcing and the dynamical response to the aerosol forcing there is medium confidence in the impacts of aerosols on near-term projected changes in precipitation.
115525	40	36	40	36	Here are many statements about monsoon precipitation, which means `mean precipitation' – however the mean precipitation might change a bit (or might even remain constant) but the intensity of precipitation could change dramaticically, with dry periods and floods. The latter is what matters to people not the mean precipttation. This should be reflected in the section and actually throughout the report. [Rolf Müller, Germany]	Noted. Those are very good comments and Chapters 8 and 11 assess changes in intensity and duration of precipitation. Chapter 4 mainly focus on global or large-scale mean changes.
4117	40	40			I suggest replacing "projection" with "near-term projection" to emphasize that this subjection is all about the near term. There may be readers who jump straight to the particular subsection of interest (e.g. monsoon) and miss the context of the wider section. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. 4.4.1.4 was considerably revised and the sentence was deleted in the FGD.
11089	40	43	40	45	There must be some references to support the statement "the basic pattern of wet regions including global monsoon regions tending to get wetter and dry regions tending to get drier is apparent". In fact, dry northwestern China get significantlt wetter in the last several decades. [Wen Wang, China]	Taken into account. The statement is basically from the AR5. In 4.4.1.3, we assessed that the 'wet get wetter, dry get drier' might not hold especially over subtropical land regions. In the subsection, we further discussed more detail mechanisms for near-term precipitation influenced by not only GHG forcing but also aerosol forcing and natural variability.
53003	40	46	40	47	higlighting both strong model uncertainties and … [Hervé Douville, France]	Not applicable. The sentence is removed.
116319	40		40		Coordination is needed to improve coherency on monsoon across chapters especially here with ch 2 and 3. [Valerie Masson-Delmotte, France]	Accepted. The treatment on global monsoon has been better coordinated across chapters particularly using the new Annex IV on monsoon.
104635	41	10	41	13	It states here that global monsoon precipitation "tends to increase by 1-4%" in 4.13a. However, 4% in represents extreme values (signal+positive noise) of the interannually varying curves, whereas average anomalies over 2081-2100 which are much more representative will be closer to 2%, even for SSP5-8.5 [William Merryfield, Canada]	Taken into account. The sentence is revised using multi-model mean change and 5-95% range. The 5-95% range is much larger for the previous range. For SSP5-8.5, the multi-model mean (5-95% range) of global land monsoon precipitation change is 1.9% (-0.8 ~ 5.2%).
4119	41	11			It would be worth clarifying the near term (i.e. 2021-2040) for the reader who may only look at this subsection. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The definition of the near term is introduced at the beginning of Section 4.4, which should be enough.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4121	41	12	41	17	Can changes to global monsoon circulation metrics be related to projections of the Hadley or Walker circulations likely to be discussed in Chapter 8? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Yes, they can. However, we didn't include detail assessment on the linkages of global monsoon circulation to Hadley or Walker circulation since there are not enough literature to discuss their linkages and how they change in response to GHG forcing.
11093	41	17	41	19	When saying "near-term changes in global monsoon precipitation and circulation are likely to be dominated by the effects of natural internal variability", is it possible to give a definition of natural internal variability? The term "natural internal variability" or "internal variability" is a widely used but not clearly defined in the chapter. [Wen Wang, China]	Not applicable. Glossary provides the definition of internal variability or natural variability used across the WGI report. "Internal variability" is defined as "fluctuations of the climate dynamical system when subject to a constant or periodic external forcing (such as the annual cycle).
50855	41	17	41	19	It would be helpful to embolden the final summary sentence and include as a separate paragraph. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The separate summary sentence is added.
26869	41	17	41	19	A cross reference to monsoon in chapter 8 could be welcomed, to help maintain consistent monsoon assessment between chapters [Eric Brun, France]	Accepted. The cross reference to Chapter 8, Section 8.3.2 is added.
80633	41	17	41	19	There is likely an additional effect on the Asian monsoon from scenario differences in SLCF emissions. I don't know if you want to treat this here, in the SLCF section, or refer to Ch6 or Ch8, but there is at least a paper in discussion here that tackles the issue in CMIP6: Wilcox et al. 2020, ACP, https://doi.org/10.5194/acp-2019-1188 (Reviews are positive, the paper should be accepted well in advance of the deadline.) [Bjorn Samset, Norway]	Taken into account. Wilcox et al. (2020) is added and the effect on regional precipitation change is added in 4.4.1.3.
104485	41	22	42	30	4.4.2 Cryosphere, Ocean, and Biosphere: this section seems to be incomplete. Regarding the Cryosphere part, I suggest including the near-real term projections in the Greenland and Antarctic ice sheet mass loss (with reference to Ch9 and the Atlas). [Irina Gorodetskaya, Portugal]	Noted. Given space limitations, Chapter 4 has had to make many hard decisions on what can be presented. Ice sheet mass loss is covered in Chapter 9.
74251	41	26	41	26	O(1D) has been called O1D before (page 30, line 17); try to be consistent [Christoph Völker, Germany]	Noted. This comment seems to be referring to another sections of the chapter.
50857	41	26	41	26	Suggested addition for clarification: ' below 1 million km2' (i.e. become 'ice free')..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
96429	41	26	41	44	Here again the SROCC results are not reported. It should be made clear that much of the progress since AR5 was already stated in the SROCC. The focus in the AR6 should be on the progress since the SROCC. That refers to all results regarding ocean and cryosphere. [Nicole Wilke, Germany]	Accepted
104637	41	28	41	31	The statement "it is very likely that different trajectories of the near-term evolution of anthropogenic forcing cause distinctly different likelihood ranges for very low sea-ice coverage to occur over the next two decades" is at best weakly supported by Fig. 4.1 and Table 4.4. [William Merryfield, Canada]	Accepted. The references to Fig, 4.1 and Table 4.5 are misplaced here which is a discussion about AR5 simulations and some particular studies related to them.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132483	41	31	41	32	Arctic sea ice loss is not most directly described in terms of cumulative carbon emissions. It is more directly described in terms of global or hemispheric temperature. As I noted above, it seems strange to describe sea ice in terms of cumulative carbon when no other variables in the chapter are described this way. [Kyle Armour, United States of America]	Taken into account. The reference to cumulative emissions has been removed.
50859	41	33	41	36	RCP2.6 and RCP8.5 are reported here, it would also be useful to report projections for a trajectory we are globally closer to currently based on emissions projections and current climate policies. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We can only access what has been reported in the literature. Note that the sea ice figures in this chapter show results from all five priority SSPs.
19861	41	47	41	51	One wonders what motivates this (infrequent) choice of running simulations for 3 durations. What are the WG1 scientists looking for? [philippe waldteufel, France]	Noted. These are *not* three different simulations. These are three different trend lengths ending in the near-term. This allows us to judge how robust the near-term trends are to a change in the length of period.
104639	41	49	41	51	Although it's true that internal variability may mask the importance of anthropogenic forcing on the evolution of Arctic sea-ice cover in the near-term, the spreads in Fig. 4.14 represent inter-model differences, which can be substantial, in addition to internal variability. [William Merryfield, Canada]	Noted. Good point. As it happens though this sentence has been removed.
106947	41	51	40	53	I don't understand the last sentence. What does "respectively" stand for? [Christophe CASSOU, France]	Taken into account. This sentence has been rewritten for clarity.
19865	42	1	42	10	On one hand, average velocities of area decrease appear on figure 4.14 quite close for every SSP and duration. On the other hand, the percentage of trends running against the average does depend on the duration. Is this explained by internal variability? [philippe waldteufel, France]	Noted. As the length of period over which the trends are computed increases, the uncertainty, due to internal variability, also decreases. This is now better explained in the revised text.
6665	42	3	42	3	106 should presumablby be 106. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
53005	42	3			Figure 4.14 could also provide an estimate of internal variability only (grey bar) using piControl simulations + add the legend for the different SSPs. This comment also applies to Figure 4.15 and 4.16 [Hervé Douville, France]	Taken into account. 1) It is not the intent of this figure to decompose the trend uncertainty into internal variability and model uncertainty. 2) The SSPs are now labelled.
87833	42	13	42	13	This section 4.4.2.2. contains very similar information to section 4.3.2.4 and Fig. 4.5 is presenting the same information as Fig. 4.15. It would be helpful to merge these two sections to avoid repetition or clarify what the differences are. Also, this section is repetitive with chapter 5, which provides more detailed discussion, so consider removing this section from here to avoid duplication. [Katarzyna Tokarska, Switzerland]	Rejected. 1) The information in this figure is much more detailed and focused on the near-term than in Fig. 4.5. 2) The information here, quantifying near-term carbon storage projections is not treated in Chapter 5.
19863	42	15	42	16	The reader of the WG1 report know all this by heart! [philippe waldteufel, France]	Noted.
87565	42	15	42	18	Thise lines are almost the same as in 4.3.2.4 [Valentina Roberta Barletta, Denmark]	Rejected. The information here and there is very different.
102963	42	15	42	18	Thise lines are almost the same as in 4.3.2.4 [Philippe Tulkens, Belgium]	Rejected. The information here and there is very different.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81631	42	15	42	20	Given that AR5 did not assess near-term changes, there is no need to repeat stuff that was said already on page 30 [Sönke Zaehle, Germany]	Noted.
381	42	20	40	20	remove period before reference [Wolfgang Obermeier, Germany]	Noted.
87835	42	23	42	24	It would be useful to keep Figures 4.5 and 4.15 together or next to each other, as they present almost the same type of information. [Katarzyna Tokarska, Switzerland]	Rejected. This information in these two figures, and their intent, are very different.
19867	42	23	42	30	No comment on figure 4.15b (land uptake)? No assessment either? [philippe waldteufel, France]	Accepted.
81633	42	28	42	29	Is the use of the word uncertainty warrented here, given that the high variance at 10 year time-scale compared to 30 year time-scale reflects mostly the large interannual variability of the land carbon balance (see Chapter 5.2.1.4), and is not directly related to ESM uncertainty, Maybe worth to at least mention the large terrestrial variability here? [Sönke Zaehle, Germany]	Accepted.
50861	42	28	42	30	Should this be 'land carbon uptake potential'? The actual uptake is influenced by land use policy etc. too. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. While this is a good point, but for consistency with the rest of the report we prefer to stick with the current wording.
53007	42	29	42	30	What about possible changes in the efficiency of these sinks (e.g., Green et al., 2019)? Why no parallel subsection in Section 4.5? [Hervé Douville, France]	Rejected. We do not feel that this level of discussion is warranted here.
87567	42	30	42	30	what is the conclusion for the land carbon uptake, in terms of of probability? [Valentina Roberta Barletta, Denmark]	Accepted.
102965	42	30	42	30	what is the conclusion for the land carbon uptake, in terms of of probability? [Philippe Tulkens, Belgium]	Accepted.
93657	42	34	42	34	Using novel decadal prediction experiments from NCAR, Athanasiadis et al. (2020) found remarkable skill in reproducing the observed multi-annual variations of wintertime blocking frequency over the North Atlantic and of the North Atlantic Oscillation (NAO) itself. This result is partly due to the large ensemble size, that allows the predictable component of the atmospheric variability to emerge from the background chaotic component. ref. Athanasiadis, P.J., Yeager, S., Kwon, Y. et al. Decadal predictability of North Atlantic blocking and the NAO. npj Clim Atmos Sci 3, 20 (2020). https://doi.org/10.1038/s41612-020-0120-6 [Stefano Materia, Italy]	Rejected. The section is about ocean and land carbon uptake, not the NAO.
87837	42	36	42	36	it seems that is is not the net uptake, but the flux (uptake rate). It would be good to keep the terminology consistent. [Katarzyna Tokarska, Switzerland]	Accepted. "Uptake" has been changed to "flux".
106951	42	45			Teleconnections are not assessed at all in this section devoted to MOVs even if the word appears in titles of the section. I would recommend to add few sentences for each modes to explain what the implications of the changes for the modes are at regional scale through teleconnections. What does it mean in terms of uncertainties? When applicable, I would make cross-reference to regional chapters . If nothing could be added because of lack of literature or editorial choices, I would remove the word in the title. [Christophe CASSOU, France]	Taken into account - 'teleconnection' is removed from the subtitle.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106953	42	45			Except for ENSO and NAM/SAM, I would considerably shorten the subsection devoted to the other modes. There is not much to say/literature to assess for near-term especially for the interannual tropical modes of variability. I would therefore combine the modes (except ENSO, NAM, SAM staying separated) into 2 subsections: 1. decadal modes of variability (including AMV and PDV) and 2. Interannual tropical modes of variability (including IOB, IOD, Trop ATL modes). I would shorten the description of the modes themselves and add what their future evolution (no changes most of the time) implies for variability at interannual scale over continents through teleconnections and implies in terms of uncertainties at near-term. [Christophe CASSOU, France]	Taken into account - However some reviewers suggested an extension of the sections that in your view should be shortened. We have taken action to include more information and shrink the text when possible.
79735	42	47	42	49	need to check whether this is really done in an exhaustive manner in these chapters [Laurent Terray, France]	Taken into account. Cross-references are now more specific.
106949	42	52			Why medium confidence? Considering the text just after and the fact that there is no literature assessed (which is an issue in this section), I would put "low" confidence. Why showing MAM and SON because those seasons are never commented? I would be more specific and concentrate on winter and summer consistently with Chap3 and also with the rest of diagnostics provided in Chap4 and limited to DJF and JJA. I would use all the available members for the figure. [Christophe CASSOU, France]	Noted. The medium confidence statement summarises the assessment from AR5 not this assessment. The figures show all seasons for completeness and for back comparison with earlier studies. The approach throughout the chapter is to use the first realisation (r1) from each model and this is kept consistent for the modes of variability.
87569	42	54	42	54	I advice to always use the extended name in the section/subsection titles. [Valentina Roberta Barletta, Denmark]	Taken into account. Changed
102967	42	54	42	54	It is advised to always use the extended name in the section/subsection titles. [Philippe Tulkens, Belgium]	Taken into account. Changed
7983	42	54	43	10	Jensen, A.D.; Lupo, A.R.; Mokhov, I.I.; Akperov, M.G.; Sun, F. The dynamic character of Northern Hemisphere flow regimes in a near term climate change projection. Atmosphere 2018, 9, 27. - this work demonstrates in a an RCP 4.5 scenarios using CMIP 5 - that the frequency of the transition from zonal to meridional flow regimes (and vice versa) remains the same as current. This has implications for weather forecasting - i,e, the forecasting wall suggested by Lorenz (1963) and others since doesn't change. This supports the work of Deser et al. 2012 Barnes, E.A.; Polvani, L.M. CMIP5 projections of Arctic amplification, of the North American/North Atlantic circulation, and of their relationship. J. Clim. 2015, 28, 5254–5271. In particular on Line 1-2. [Anthony Lupo, United States of America]	Noted. This part of the subsection has been changed in the FGD. Therefore the reviewer's suggestion is not applicable
127543	42	54	43	10	Jensen et al. (2018) demonstrate, in an RCP 4.5 scenario using CMIP 5, that the frequency of the transition from zonal to meridional flow regimes (and vice versa) remains the same as current. This has implications for weather forecasting -- i.e., the forecasting wall suggested by Lorenz (1963) and others since doesn't change. This supports the work of Deser et al. (2012). In particular on lines 1-2. Citation: Jensen, A.D.; Lupo, A.R.; Mokhov, I.I.; Akperov, M.G.; Sun, F. The dynamic character of Northern Hemisphere flow regimes in a near term climate change projection. Atmosphere 2018, 9, 27. [Trigg Talley, United States of America]	Noted. This part of the subsection has been changed in the FGD. Therefore the reviewer's suggestion is not applicable

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127545	42	55	43	26	Connect to other sections treating NAM and SAM (e.g., Section 4.3). Identify what differs here (broken down by season). [Trigg Talley, United States of America]	Noted. Here the near-term changes are considered (see the title of the section), while in section 4.3.3 an. overview of the projected changes of the annular modes over the XXI century is provided
116321	42		42		Where are projections of Antarctic sea ice discussed? [Valerie Masson-Delmotte, France]	Noted. These projections are discussed in Chapter 9 to which we have now included a pointer.
79737	43	1	43	1	I suggest to be a bit more specific than "large response uncertainty", for instance with references to the "tug of war" already cited previously in the chapter [Laurent Terray, France]	Rejected. "Large response uncertainty" captures more aspects of uncertainty than simply the tug of war between upper and lower tropospheric temperature gradients.
79739	43	6	43	6	which are the physical processes you are referring to here ? It is not clear what "above" is referring to. [Laurent Terray, France]	Taken into account. Clause removed in FGD.
53009	43	6	43	7	replace "interannual (…) forcing" by "internal variability"? [Hervé Douville, France]	Taken into account. Text updated.
104645	43	7	43	8	A tendency for positive near-term change in the NAM is also evident in fall, particularly considering the narrower confidence intervals. [William Merryfield, Canada]	Taken into account. Text now specifies fall, winter and spring in the FGD.
79741	43	7	43	10	this tendency is not so clear for ssp 126 and 245 when looking at Figure TS35 (check consistency in the number of model used) [Laurent Terray, France]	Taken into account. The high emission scenarios for which this statement applies have been specified.
19869	43	7	43	10	These lines are dedicated to readers endowed with particularly excellent eyesight! Less lucky people, however, are only able to detect from figure 4.16 that in both hemispheres the spread of projected anomalies is particularly large in local winter. Is there an explanation? [philippe waldteufel, France]	Noted. Internal variability in the NAM is largest in the winter, which contributes to this spread. The text has been reordered to emphasise the MMM change is small compared to the intermodel spread
84259	43	13	43	23	fig. 4.16 has a legend incomplete. Each bin is a scenario but it is not specified which one (even if it could be deduced) [Annalisa Cherchi, Italy]	Noted. This format is used in figures throughout the chapter and the colour coding per scenario follows the TSU style guide so can be traced.
15187	43	28	44	27	This text of GMSL is missing an assessment of the limitations of models of ice sheet melt and the implications for GMSL projections. It is absolutely vital for governments that the scientific community explain that the GMSL modelling estimates presented here do not represent the full range of possibilities. Otherwise, this assessment will fall into the same GMSL projection communications trap as previous assessments. [Simon Donner, Canada]	Not applicable. This comment is misplaced. GMSL is not discussed in this section. Unfortunately, we cannot identify what it refers to.
7467	43	34			Athanasiadis et al 2020 also published a work about decadal NAO predictability. Athanasiadis, P. J., S. Yeager, Y.-O. Kwon, A. Bellucci, D. W. Smith, and S. Tibaldi (2020). Decadal Predictability of North Atlantic Blocking and the NAO. NPJ Climate and Atmospheric Science, in press. [Wolfgang Müller, Germany]	Taken into account. Reference added in FGD.
87571	43	37	43	37	I advice to always use the extended name in the section/subsection titles. [Valentina Roberta Barletta, Denmark]	Taken into account. Changed
102969	43	37	43	37	It is advised to always use the extended name in the section/subsection titles. [Philippe Tulkens, Belgium]	Taken into account. Changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70893	43	48	43	51	The causal link between a cessation of the trend in polar vortex breakdown and the cessation of the summertime SAM trends has been demonstrated using a purely observational analysis by Saggioro and Shepherd (2019 doi: 10.1029/2019GL084763) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Citation added in FGD
99589	43	51	43	51	Add a recent review paper on the topic after "… Screen et al., 2018a": Cohen et al., 2020. Reference: Cohen, J., X. Zhang, J. Francis, T. Jung, R. Kwok, J. Overland, T. Ballinger, U.S. Bhatt, H. W. Chen, D. Coumou, S. Feldstein, D. Handorf, G. Henderson, M. Ionita, M. Kretschmer, F. Laliberte, S. Lee, H. W. Linderholm, W. Maslowski, Y. Peings, K. Pfeiffer, I. Rigor, T. Semmler, J. Stroeve, P. C. Taylor, S. Vavrus, T. Vihma, S. Wang, M. Wendisch, Y. Wu, and J. Yoon, 2020: Divergent consensus on the influence of Arctic Amplification on mid-latitude severe winter weather. Nature Climate Chang, 10, 20-29. doi:10.1038/s41558-019-0662-y. [Xiangdong Zhang, United States of America]	Rejected. This section refers to the Southern hemisphere whereas this paper addresses NH change.
87573	43	56	43	56	even here, JJA, DJF not defined, even if trivial. [Valentina Roberta Barletta, Denmark]	Taken into account. Specific reference to austral winter and summer added. DJF, JJA etc are defined earlier in the chapter.
102971	43	56	43	56	even here, JJA, DJF not defined, even if trivial. [Philippe Tulkens, Belgium]	Taken into account. Specific reference to austral winter and summer added. DJF, JJA etc are defined earlier in the chapter.
79743	44	3	44	3	What does significant mean in that context ? Is it statistical significance of a trend or decadal change or the exceedance of a value (~2) of signal to noise ratio. If the latter, I 'd suggest using "detectable" instead or explicitely refer to the exceedance of a given signal to noise ratio threshold [Laurent Terray, France]	Taken into account. Changed to detectable
70895	44	4	44	6	Ceppi and Shepherd (2019 doi: 10.1029/2019GL082883) show that the effect of GHG increases on the summertime SAM is substantially mediated by the GHG-induced delay in stratospheric vortex breakdown, and that there is a large spread in climate model responses in this respect. So the dependence is not just on GHG forcing scenario, and there is no reason to expect an exact balance between GHG and O3 forcing. [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Ceppi and Shepherd (2019) focus on the long-term trend and this study is discussed in section 4.5.3. The wording used does not imply an exact balance of the forcings merely that they oppose one another.
87575	44	8	44	20	There is no conclusion clearly stated, as in most of the sections [Valentina Roberta Barletta, Denmark]	Taken into account. An assessment statement has been added.
102973	44	8	44	20	There is no conclusion clearly stated, as in most of the sections [Philippe Tulkens, Belgium]	Taken into account. An assessment statement has been added.
50863	44	12	44	12	typo: 'less than half the number of…' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This text does not appear in the FGD.
26871	44	32	44	34	It is not clear why this study is singled out here. There are many studies that assessed this question and which conclude differently. [Eric Brun, France]	Noted. The study was first to suggest the nonlinear change of ENSO SST variability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79745	44	42	44	48	I think that the way this diagnostic is performed (the use of normalization) leads to an artificial increase of variance in the out of sample period and an artifical decrease in the reference period. See Sippel et al for instance: Sippel, S., Zeischler, J., Heimann, M., Otto, F.E.L., Peters, J. and Mahecha, M.D. (2015) Quantifying changes in climate variability and extremes: Pitfalls and their overcoming. Geophysical Research Letters, 42(20) [Laurent Terray, France]	Noted. The use of normalization is due to considerable inter-model diversity in simulating ENSO amplitude (e.g., Bellenger et al., 2014). This diversity can lead to misinterpretation of the results based on a multi-model average.
39075	44	42	44	48	Is there a specific reason to use the Niño 3 region here, while the Niño 3.4 is used in other plots (e.g., Fig 4.8)? [Federico Serva, Italy]	Taken into account. For consistency, Niño 3.4 region is now used in Fig. 4.17
65697	44	53	45	4	Suggest possible inclusion of the evidence of changes to ENSO extremes (see references 1-2 above). Another useful reference is: Yeh, S.-W., Cai, W., Min, S.-K., McPhaden, M. J., Dommenget, D., Dewitte, B., … Kug, J.-S. (2018). ENSO atmospheric teleconnections and their response to greenhouse gas forcing. Reviews of Geophysics, 56, 185–206. https://doi.org/10.1002/2017RG000568 [Kushla Munro, Australia]	Noted. Yeh et l paper refers to ENSO and its teleconnections response to global warming over the XXI century. This subsection is devoted to near-term changes.
104651	44	55	44	55	The reference to rainfall variability over Niño3.4 region is inconsistent with the Fig. 4.17 caption which indicates Niño3. [William Merryfield, Canada]	Taken into account. For consistency, Niño 3.4 region is now used in revised Fig. 4.17
53011	44	55	44	56	even after detrending the data (also specify that precipitation data have been detrended in the caption of Fig. 4.17?) [Hervé Douville, France]	Taken into account-Detrended result retains same the same assessment opinion
53013	45	2	45	4	This is a misleading statement about the forced component of ENSO variability only. You may want to specify "forced change" here and add the following sentence: Yet, near-term changes in ENSO properties can arise from natural variabilty but are poorly predictable at the decadal timescale (e.g., Wittenberg et al., 2014) [Hervé Douville, France]	Taken into account. Sentence rephrased.
104653	45	2	45	4	This sentence comes across as a bit self-contradictory with respect to rainfall variability. [William Merryfield, Canada]	Taken into account. It is revised accordingly.
65699	45	3	45	3	Suggest clarification of: "ENSO" -> "ENSO-driven" [Kushla Munro, Australia]	Accepted
14467	45	9	45	43	To be consistent with the other chapters, I recommend mentioning here the remote role that the Atlantic can have in causing PDV. Relevant references: Cai et al. 2019 (DOI: 10.1126/science.aav4236) and McGregor et al. (2014), Recent Walker circulation strengthening and Pacific cooling amplified by Atlantic warming, Nature Climate Change, DOI:10.1038/nclimate2330 [Malte Stuecker, United States of America]	Noted. Thank you for the suggestion. However, considering the limitation in the number of pages and the aim of the chapter, i.e. the evolution of PDV in the near-term, we decided of not including further material on this respect. Also, the role of the Atlantic in causing PDV is treated in Chapter 3 and in Annex IV.
7469	45	20	45	26	Boer and colleague recently published a work on the predictability of the PDO. Boer, G. J., and Sospedra-Alfonso, R. (2019). Assessing the skill of the Pacific Decadal Oscillation (PDO) in a decadal prediction experiment. Climate Dynamics, 53, 5763–5775. https://doi.org/10.1007/s00382-019-04896-w [Wolfgang Müller, Germany]	Accepted - This reference has been included

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65703	45	29	45	31	Suggest including additional knowledge: Recent experiments have provided some indication that the South Pacific is a key region for Tropical Pacific decadal variability, while the North Pacific has more of an impact on interannual timescales. See: - Liguori, G., & Di Lorenzo, E. (2019). Separating the North and South Pacific Meridional Modes contributions to ENSO and tropical decadal variability. Geophysical Research Letters, 46, 906– 915. https://doi.org/10.1029/2018GL080320 - Chung, C.T.Y., Power, S.B., Sullivan, A. et al. The role of the South Pacific in modulating Tropical Pacific variability. Sci Rep 9, 18311 (2019). https://doi.org/10.1038/s41598-019-52805-2 [Kushla Munro, Australia]	Accepted - The suggested references have been included and some text added.
21665	45	37	45	38	This is less a prediction than a hindcast and will be even more the case by the time the report is published. Perhaps as such belongs in chapter 3 rather than chapter 4? [Peter Thorne, Ireland]	Noted. Thank you. However, considering that the literature is not abundant in this respect and that the paper in question contains a prediction, we decided to document this as such in the report.
53015	45	38			"was" predicted for years 2013-2022 (Meehl et al., 2016). Is this prediction supported by the 2013-2020 observations? [Hervé Douville, France]	Taken into account - This prediction is partly supported. However, we have to wait for the end of 2022 to properly evaluate its skill.
116325	45	40	45	43	Refer to CCB on hiatus here [Valerie Masson-Delmotte, France]	Accepted. Now there is a reference to CCB3.1
21667	45	40	45	43	This is covered in some depth by cross-chapter box 3.1 and it is questionable whether this duplication should remain. If it does at least cross-reference to the box. [Peter Thorne, Ireland]	Accepted - We referred to the Cross-Chapter Box 3.1
65701	45	42	45	42	Suggest an additional reference: Bordbar, M.H., England, M.H., Sen Gupta, A. et al. Uncertainty in near-term global surface warming linked to tropical Pacific climate variability. Nat Commun 10, 1990 (2019). https://doi.org/10.1038/s41467-019-09761-2 [Kushla Munro, Australia]	Accepted - reference is included
79747	45	43	45	43	not sure this is a peer-reviewed paper, I suggest to check [Laurent Terray, France]	Accepted - This was removed.
84261	45	46	46	6	this assessment about Indian Ocean modes is quite poor and does not consider CMIP6 or DCPP recent simulations (as done for example for the other modes) [Annalisa Cherchi, Italy]	Taken into account. Unfortunately the literature coverage of near term evolution of IOD and IOB is very limited. However the sub-section has been updated and new available literature added.
76849	45	48	46	6	Worth noting the findings from following paper on the emergence of an equatorial mode of climate variability in the Indian Ocean that could be an important source in the assessment and management of climate related risks in the Indian Ocean Rim countries and larger Southeast Asia. Reference: P. N. DiNezio, M. Puy, K. Thirumalai, F.-F. Jin, J. E. Tierney, Emergence of an equatorial mode of climate variability in the Indian Ocean. Sci. Adv. 6, eaay7684 (2020) [Sandeep Sahany, Singapore]	Rejected - This sub-section is related to near-term changes in IOD- and IOB. The results of the paper are related to changes in the second half XXI century under GHG forcing.
99413	45	49	45	50	Several studies have shown that IOD and ENSO coincide during several years but are not 'closely-related-to' but are two independent modes of variability which interact and affect each other. Meanwhile IOB is closely related to ENSO. [Vikas Kumar Kushwaha, India]	Taken into account. The text has been changed to include your suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3697	45	51	45	51	A recent review review brings together the complex interactions between the IOD and ENSO. It is not simply a one-way effect. https://science.sciencemag.org/content/363/6430/eaav4236 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sub-section is revised.
4123	45	54	45	55	Why does the mean-state tend towards positive IOD? Does this relate to a slow-down of the Asian monsoon circulation/Somali Jet and thus reduced upwelling? (Can you link to Chapter 8?) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We cross-checked with Ch.8
53017	45	54	46	2	also quote Li et al. (2016) suggesting that the projected IOD-like warming pattern is not necessarily reliable given the strong and common biases of CMIP5 models? [Hervé Douville, France]	Taken into account. The sub-section has been revised to partially incorporate your suggestion
127547	45	54	46	6	Conclusions in this paragraph are puzzling and difficult to follow. Results are not robust and no new studies have been done for AR6. Why do authors conclude that the many features mentioned in the paragraph that "relationship observed in the current climate to persist in the near-term future"? Wouldn't it be fairer to say "our report can make no assessment of whether these features will change in the future (high confidence)" or "there is low confidence in projections of indian ocean teleconnection change"? [Trigg Talley, United States of America]	Taken into account - The sentence has been in part removed.
3699	46	1	46	1	But work has since been done which shows a signficiant response of extreme positive IODs (like the one which has played a role in both the East African flooding and Australia wildfires of 2019/2020), with a doubling in frequency at 1.5degC above pre-industrial. There is model agreement on this. https://www.nature.com/articles/s41467-018-03789-6 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Noted - The description these findings are referred in section 4.5.3 (mid to long term changes)
3701	46	2	46	2	There have been a number of interesting papers in recent years on the Indian Ocean warming and dipole. I have linked to these in a recent online article I wrote https://futureclimateafrica.org/news/has-climate-change-played-a-role-in-the-exceptional-eastern-africa-2019-short-rains/ [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Thanks a lot. Taken into account - However the literature linked is not specific for near-term changes, which is what is assessed in this sub-section
4125	46	3	46	6	The sentence here is extremely clunky and should be revised. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This suggestion the sentence has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18345	46	9	46	52	Many modeling studies show that the recent AMO or AMV cycles are partially forced by volcanic and anthropogenic aerosols. Because the future evolution of the VA and AA won't be the same as in recent decades, future AMV will not only depend on how the internal component of the current AMV envolves, but also depend on how future VA and AA forcing may occur. The historical VA and AA forcing not only caused an AMO-like oscillation in the North Atlantic, but also in other ocean basins, as shown by Qin et al. (2020a). How to seperate and quantify the internally-generated and externally-forced components in North Atlantic SST multidecadal variations is still a challenge. We attempted to address this issued in Qin et al. (2020b). Refs cited: Qin, M., W. Hua, and A. Dai, 2020: Aerosol-forced multi-decadal variations across all ocean basins in models and observations since 1920. Science Advances, accepted. Qin, M., A. Dai, and W. Hua, 2020: Quantifying contributions of internal variability and external forcing to Atlantic multidecadal variability since 1870. Science Advances, submitted. [Aiguo Dai, United States of America]	Taken into account- The suggested literature is scrutinized and.
106099	46	14	46	14	There are much earlier references on this topic. I think the following should be cited: Zhang, R., T. L. Delworth, and I. M. Held, 2007: Can the Atlantic Ocean drive the observed multidecadal variability in Northern Hemisphere mean temperature? Geophysical Research Letters, 34, L02709. [Noel Keenlyside, Norway]	Noted. Thank you. However, considering the limitation in the number of pages we hade decided to limit the number of citations here to the most recent ones and refer to annex IV.2.7.
111113	46	17			You might want to add O'Reilly, C. H., Zanna, L., & Woollings, T. (2019). Assessing External and Internal Sources of Atlantic Multidecadal Variability Using Models, Proxy Data, and Early Instrumental Indices. Journal of Climate, 32(22), 7727-7745. [Francisco Doblas-Reyes, Spain]	Noted. Thank you. However, considering the limitation in the number of pages we hade decided to limit the number of citations and refer to annex IV.2.7. and Section 3.7.7
45501	46	19	46	23	I think a clearer differentiation between the subpolar branch of AMV, thought to be unforced, and the subtropical branch of AMV, thought to be forced, might be in order here. [Leonard Borchert, France]	Rejected - Here we refer to the assessment in AR5 where this differentiation is not apparent.
73891	46	25	46	36	A robust relationship between AMV and temperature over Mediterranean and Western Europe is demonstrated in a multi-model study based on DCPP simulations. The study also shows that models give an inconsistent response of the Atlantic jet in winter. The manuscript is in review, a copy is available through the TSU Document Management System and through the following link https://drive.google.com/file/d/1qUc6Yy2tB_2zyeKq7RgTyp9XOphl-xxB/view?usp=sharing [Paolo Ruggieri, Italy]	Taken into account - This suggestion has been partially considered in the revised version and the reference added.
2161	46	27	46	27	A forecast for declining AMV was recently made by: Forecast cooling of the Atlantic subpolar gyre and associated impacts. Hermanson L., R. Eade , N.H. Robinson , M.B. Andrews , J.R. Knight, A.A. Scaife and D.M. Smith, 2014. Geophys. Res. Lett., 41, 5167-5174, doi:10.1002/2014GL060420. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This suggestion has been included in the revised version

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105465	46	27	46	28	Recent results from a single-model ensemble shows that "interannual-to-decadal predictability of North Atlantic SSTs depends strongly on the strength of subpolar ocean heat transport at the start of a prediction, indicating that physical mechanisms need to be taken into account for actual temperature predictions" (copied directly from the abstract in Borchert et al., 2018, https://doi.org/10.1175/JCLI-D-17-0734.1). [Helene R. Langehaug, Norway]	Taken into account - This suggestion has been considered in the revised version.
105463	46	28	46	29	This skill is neither translated to the ocean regions further north towards the Arctic Ocean, i.e., the Nordic Seas and the Barents Sea (Langehaug et al., 2017, https://doi.org/10.1007/s00382-016-3118-3), although observation based data and ocean models forced by a realistic atmosphere demonstrate a mechanism that can provide skill to these regions (Årthun et al., 2017; Langehaug et al., 2018, https://doi.org/10.1007/s00382-018-4184-5). [Helene R. Langehaug, Norway]	Accepted - references are included and the text updated accordingly.
45505	46	31	46	36	I feel that the study by Borchert et al. (2019; 10.1029/2019GL085385) that showed a robust link between AMV and the probability of occurrence of extremely warm summers on the Northern Hemisphere could be a good addition to this passage. They also discuss the value of their findings for decadal prediction. [Leonard Borchert, France]	Accepted - reference is included and the text updated accordingly.
53019	46	31			Also quote Qasmi et al. (2017)? [Hervé Douville, France]	Accepted
45503	46	32	46	32	Omit the “()” around “Simpson et al”. [Leonard Borchert, France]	Accepted
105467	46	34	46	34	It is also shown using reanalysis data, a mechanism through which ocean temperature controls the variability and provides predictability of European SAT (Årthun et al., 2018, https://doi.org/10.1002/2018GL077401). [Helene R. Langehaug, Norway]	Accepted - reference is included and the text updated accordingly.
73893	46	34	46	42	The impact of the AMV on the Eurasian hydrological cycle is assessed in this study provided by Nicoli' et al, using the DCPP model framework. The manuscript is in review, a copy is available following this link: https://drive.google.com/file/d/1pBXJCkILWi8-zAq88cB9MIjDwnhQi87j/view [Paolo Ruggieri, Italy]	Accepted - reference is included and the text updated accordingly.
104655	46	37	46	44	Should note here or in 4.2.3 [William Merryfield, Canada]	Taken into consideration- The sub-section has been reshaped.
24027	46	42	46	42	Sheen et al 2017 is relevant here [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you. The section has been in part reshaped. The comment is not applicable.
24029	46	42	46	44	The WMO multi-model ensemble (www.wmolc-adcp.org) predicts wetter than average conditions over the Sahel consistent with a warmer north than south Atlantic [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Thank you. Taken into account - However only references to peer reviewed literature have been included.
7813	46	46	46	52	I don't see how the varied AMOC-AMV relationship across models and role of ocean dynamics lead to low confidence in AMOC impacts but high confidence in AMV skill itself. Surely if the forecast can capture the AMV but not the impacts then that is a problem with ocean-atmosphere coupling/impacts of SST on the atmosphere? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The statement doesn't imply high confidence in AMV skill itself, rather high confidence that the AMV skill over 5-8 year lead time is improved by using initialised predictions (compared to non-initialised ones).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45507	46	46	46	52	The controversy about the active role of the ocean in generating AMV should receive a bit more attention here. It is unclear from the current phrasing, why an inactive role of the ocean for AMV should decrease the confidence in AMV predictions. Furthermore, evidence is increasing that the ocean does indeed have an active role in generating AMV (e.g. Zhang et al., 2019, 10.1029/2019RG000644), and appears to be particularly important to set the characteristic time scale of AMV (e.g. Oelsmann et al., 2020, 10.1029/2020GL087259). While the immediate response of the ocean to atmospheric forcing shows a similar pattern as AMV (Clement et al., 2015, 10.1126/science.aab3980), atmospheric forcing does not appear to be capable of producing this characteristic time scale (Zhang et al., 2016, 10.1126/science.aaf1660). Since the importance of AMV for decadal prediction lies in its oceanic contribution due to the predictable oceanic memory, and that memory appears for the aforementioned reasons to be important to set the time scale of AMV, I think the confidence in predictions of AMV impacts could be characterized as higher than low (albeit not too high due to the ongoing scientific discussion). [Leonard Borchert, France]	Taken into account. The statement is reconsidered in the light of recent studies.
132485	46	50	46	50	Perhaps cite Wills et al. 2019 (doi: 10.1175/JCLI-D-18-0269.1) here as well. [Kyle Armour, United States of America]	Not applicable - This part has been removed
53021	46	50	46	51	even using empirical prediction schemes (e.g., Suckling et al., 2017)? [Hervé Douville, France]	Not applicable. This part has been removed
79749	46	50	46	51	Is it low confidence in the predictions of AMV impacts or in the AMV prediction itself (some studies claim the AMV is mostly forced which imply a certain level of predictability). I suggest to reformulate the confidence statement. [Laurent Terray, France]	Taken into account. The statement is reconsidered in the light of recent studies.
24031	46	50	46	51	I'm not sure which AMV impacts you mean here. I think we have medium to high confidence in predictions of Sahel rainfall and hurrican numbers. [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement is reconsidered in the light of recent studies.
79751	47	1	47	44	I feel that this sub-section could be a bit enhanced by a more thorough treatment of the teleconnection aspects and also the interplay between forced changes and AMV-related changes on the AMM for instance. Reference could also be made to section 10.4.1.2.1 [Laurent Terray, France]	Taken into account - This suggestion has been considered in the revised version and new literature included.
21669	47	1			In chapters 2 and 3 these modes come earlier in the order. For the reader it would be good to see consistency in the order in which modes of variability are assessed across 2-3-4. [Peter Thorne, Ireland]	The order of the modes have been changed as suggested
114471	47	47	50	42	I think this section needs to clarify better the distinction beween precursor and secondary species [Jan Fuglestvedt, Norway]	Taken into account. Note many of the details of individual SLCFs are removed in FGD so this issue is less prominent.
114473	47	47	50	42	The section also has interfaces with several other parts of the report, and coorindiation is needed with ch6, 5, and 10. [Jan Fuglestvedt, Norway]	Taken into account. Coordination has been improved with ch6, 5 and 10 in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45809	47	47			Section 4.4.4: The description of the response to SLCFs in this section is rather selective. [Twan van Noije, Netherlands]	Taken into account. The section has been amended in the FGD to focus on the total SLCF effect on climate in SSPs, and refers to Chapter 6 for more detailed breakdowns of SLCF effects on climate.
2373	47	49	47	49	The acronym SLCF is already defined on pages 11 (line 19) and 14 (line 24). Can be eliminated from here. [Vaishali Naik, United States of America]	Accepted. Removed.
116327	47	49	47	49	"Mitigation of CO2 is linked with mitigation of SLCF" is to be nuanced (building on ch 6). Improved assessment of interplays of a changing climate with air quality with ch 6 is needed (here, also for TS/SPM). Regarding aerosols, what about dust in a changing climate, where is this assessed in the AR6 WGI? [Valerie Masson-Delmotte, France]	Taken into account. The sentence has been revised in the FGD. Dust in a changing climate is not within the remit of Ch4.
15933	47	49	47	50	The statement: "Mitigation of long-lived GHGs is also associated with mitigation of short-lived climate forcers (SLCF) (methane, aerosols, ozone) (see also Sections 6.6.3 and 6.6.4)." is incorrect. Emission of methane from the terrestrial sources such as melting permafrost, vegetation decay, etc. is independent of fossil fuel emissions. While methane emission increases have been associated with the fracking, it is not established what fraction of the total this contributed to. Other gases such as HCFs an SF6 are also largely independent of fossil fuel use. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been revised in the FGD.
50865	47	51	47	51	emission reductions aimed at decreasing local air pollution could have a near-term impact on climate' - suggest this is rephrased to: 'a near-term warming impact…' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed.
50867	47	54	47	54	modest influence on warming levels' - please state if the influence on warming is positive or negative, i.e. by adding 'modest positive influence…' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence changed.
114475	48	1	48	2	unclear sentence [Jan Fuglestvedt, Norway]	Taken into account. Sentence re-phrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127549	48	1	51	1	This document may have been written a year ago; however, there is a vital opportunity to discuss the ""NATURAL"" experiment of wide-scale reductions in short-lived climate forcing. ""[SLCF] could increase global mean precipitation (2-4.6%; low confidence; Richardson et al., 2018; Samset et al., 2018; Takahashi et al., 2018). Future tropospheric ozone radiative forcing is likely to be determined by an interplay between changes to tropospheric ozone precursor emissions."" Now that the world is living this natural COVID19-induced SLCF, authors should update this section. It is so important that release of the report should even be delayed to make sure high confidence on what removal of SLCF did during early/mid 2020 can be assessed. If this opportunity is missed, Governments will have to wait 7-8 years for AR7 . [Trigg Talley, United States of America]	Take in to account. The impact of COVID-19 is discussed.
50869	48	4	48	4	Suggested reordering for clarity: ' Mitigation of CH4 combines rapid temperature reduction effects..'? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is removed from the FGD.
76837	48	4	48	6	Some of the statements made in this sentence require additional explanation and a citation. What is the scale of the 'very strong [CO2] mitigation required to have clearly detectable effect in the near-term'? [Nathan Borgford-Parnell, Switzerland]	Taken into account. This sentence is removed from the FGD.
15935	48	4	48	6	The statement: Mitigation of CH4 combines rapid effects on surface temperature with long term gains, while mitigation of CO2 stands out as the most efficient, both in the short and longer term," neglects the logarithmic relationship between CO2 forcing and atmospheric concentration. So, even if atmospheric concentrations could be reduced, and there is no evidence yet that this can be done, then the initial reduction in radiative forcing will be lower than that which will be achieved by a similar reduction at a much later date when atmospheric concentrations are at or near to pre-industrial levels. Consequently, any reductions in either CO2 emissions, or atmospheric CO2 concentrations are unlikely to reduce radiative forcing sufficiently to overcome the warming effects of interlocking feedback mechanisms. Thus, mitigation of CO2 is not likely to be the most efficient in the short term. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is removed from the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
76839	48	5	48	6	What does “most efficient, both in the short and longer term” mean in this context? Is it most efficient because shorter-lived climate forcers do not have the same long-term impact on the climate? Solomon et al (2013) stated that “[C]limate change that takes place due to increases in carbon dioxide concentration is largely irreversible for 1,000 years after emissions stop. Following cessation of emissions, removal of atmospheric carbon dioxide decreases radiative forcing, but is largely compensated by slower loss of heat to the ocean, so that atmospheric temperatures do not drop significantly for at least 1,000 years.” Note also the text at 4-102 line 48-53. This finding would appear to indicate that mitigating CO2 emissions is not an effective or efficient way to reduce anthropogenic warming in the short-term. (Solomon S. et al. (2009) Irreversible climate change due to carbon dioxide emissions, Proc. Natl. Acad. Sci. USA 106:1704-1709, 1704) [Nathan Borgford-Parnell, Switzerland]	Taken into account. This sentence is removed from the FGD.
50871	48	8	48	8	Rapid elimination of sulphate aerosols (SO2), which contributes a negative radiative forcing in the present, would likely cause an increase in GSAT of a few tenths of a degree'a few tenths of a degree' - please specify by how much a few tenths of a degree means in context of the different scenarios. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is removed from the FGD.
50873	48	9	48	9	'this could be tempered by reduction in methane emissions' - these gases/species have different sources though? This is only the case if CH4 and SO2 are considered together and from the same sources. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is removed from the FGD.
114957	48	9	48	10	The formulation "could be tampered" originates from SR1.5 report but there seem to evidence building, or say model simulations, showing that it can actually offset by methane mitigation, e.g., https://www.geosci-model-dev-discuss.net/gmd-2019-375/, and more recent results from aerchemmip might be showinfg the same. If confirmed, the language-conclusion should be adapted respectively [Zbigniew Klimont, Austria]	Taken into account. This sentence is removed from the FGD.
16661	48	12	48	23	Note that the GSAT changes for figure 4.18 explicitly exlcude the methane mitigation effects of SSP370-lowNTCF. The methane effects are included in Nicholls et al. 2020 (their supplementary figure S6) for the scenario they label SSP370-lowNTCF-gidden. This scenario shows > 0.5K cooling (compared to SSP370) by the end of the century. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. GSAT changes due to all SLCFs (incl. methane) are now included in FGD.
50875	48	16	48	16	more realistic scenarios for current air quality legislation targets and the maximum feasible reduction of SLCFs from anthropogenic sources based on current technological options indicate combined climate and air quality benefits on decadal and longer timescales' - is there a possibility that the additional warming from a reduction in cooling aerosols in the short term causes overall warming to pass a critical threshold for climate change? If so, please could you add some information on this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is removed from the FGD. It is discussed in more detail in chapter 6.

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80631	48	16	48	16	A second relevant reference here, in addition to Shindell and Smith, could be Aamaas et al. 2019, ACP, https://doi.org/10.5194/acp-19-15235-2019 [Bjorn Samset, Norway]	Noted. This sentence is removed from the FGD.
2375	48	16	48	19	The purpose of this sentence is not clear and begs the question what is the reason for differences. Possibly because of differences in the assumed trajectories of emissions in the scenarios. I would suggest delete or provide an explanation. [Vaishali Naik, United States of America]	Taken into account. This sentence is removed from the FGD.
32965	48	16	48	23	This section compares the impact of methane and BC reductions in the older UNEP/Shindell studies with the SSP3-7.0-lowNTCF vs SSP3-7.0 and states that the "estimated additional warming...is much lower than an earlier estimate". This is not a like with like comparison, however, as the lowNTCF scenario reduces all NTCFs including cooling aerosols whereas the UNEP/Shindell scenarios looked at the controls on the warming NTCFs alone (methane and BC-related measures with a net warming impact), so naturally the lowNTCF scenario would see a smaller value. This should be rewritten. The UNEP Emissions Gap Report from 2017 included a reanalysis of a scenario with warming SLCFs only reduced, as in the earlier UNEP/Shindell work, and found a fairly similar result (though with a larger methane and smaller BC-related component) so I don't know of any reason to believe the older estimates were off. [Drew Shindell, United States of America]	Taken into account. This sentence is removed from the FGD.
45811	48	19	48	21	Please clarify that the methane concentrations are not reduced in the SSP3-7.0-lowNTCF experiment. [Twan van Noije, Netherlands]	Taken into account. This has been clarified.
69947	48	19	48	23	I think more references (or sources of figure) should be mentioned for Figure 4.18. Collins et al.(2017b) only tells experimental design of AerChemMIP. [Young-Hwa BYUN, Republic of Korea]	Taken into account. Citation to Allen et al (2020) added.
2377	48	19	48	23	Careful! The AerChemMIP SSP3-70-lowNTCF simulation assumed SLCF emissions to follow the lowNTCF scenario but allowed methane concentrations to follow the SSP3-70 trajectory. So the near term warming is the combined effect of reducing aerosols and increasing methane. It would be useful to exactly quantify "few tenths of a degree" as the change in the near term GSAT relative to present day (1995-2014). This statement "but this difference is smaller than the likely GSAT range of the SSP3-7.0 projections based on the CMIP6 model spread." is misleading as presumably there is spread in the SSP3-70-lowNTCF projections as well. Figure 4.18 only shows the model spread in SSP3-70, why not also include spread in SSP3-70-lowNTCF? [Vaishali Naik, United States of America]	Taken into account. The SSP3-70-lowNTCF scenario has been clarified. The temperature change has been quantified (citing also Allen et al., 2020). The spread in SSP3-70-lowNTCF is omitted as it is hard to visualise the figure with overlapping uncertainty ranges.
41413	48	20	48	20	Please provide the full term for the NTCF abbreviation here. This holds also for the caption and legend of Figure 4.18. Non-specialist readers will not be able to easily comprehend the figure otherwise. [Alexander Nauels, Germany]	Taken into account. This has been clarified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
76841	48	25	48	29	The opposite can be equally true. The co-emission of CO2 from major sources of SLCFs would also mean that policies aimed at reducing SLCF emissions could implicitly capture some CO2 reductions. While it is true that some SLCF mitigation technologies would not simultaneously address co-emitted CO2 - properly designed climate policies would presumably incentivize mitigation options which achieve maximum simultaneous mitigation of all climate forcing co-pollutants. For the purposes of a WG1 report, does it matter if the necessary CO2 mitigation comes from decarbonization efforts or as a co-benefit of SLCF mitigation? [Nathan Borgford-Parnell, Switzerland]	Taken into account. This sentence is removed from the FGD. It is discussed in more detail in chapter 6.
16663	48	25	48	33	The roles of SLCFs compared to LLGHGs can be distinguished, SSP370-lowNTCF scenario was specifically designed to do that. The activity data (including energy production methods, and transport) is exactly the same as in SSP370, the only difference is the techological controls - e.g. particle filters and catalytic converters. And for methane the assumption of better methane capture from fossil fuel exploration, piplines, landfill, cattle sheds etc. See Gidden et al. for more details. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph is removed from the FGD.
2379	48	25	48	33	This aspect is better discussed in chapter 6 (see section 6.5.3). I think this paragraph can be eliminated. [Vaishali Naik, United States of America]	Taken into account. This sentence is removed from the FGD.
45813	48	25	48	33	The ideas expressed by Pierrehumbert (https://doi.org/10.1146/annurev-earth-060313-054843) on the comparison of the effect of SLCF versus CO2 mitigation could also be mentioned in this context. [Twan van Noije, Netherlands]	Noted. This paragraph is removed in the FGD.
114959	48	29	48	33	I believe I made comments to this statement in FOD review. I believe, this is only true if one assumes that all 2C compatible scenarios include eradication of solid biomass for cooking and heating (one of the biggest sources of anthropogenic BC but mostly assuming that there is no net CO2 from this source). I am not sure if all of them do that but I think adding here "...consistent with meeting the 2C target and assuming eradiation of solid biomass use for cooking and heating, additional...." and then at the end "...implict CO2 controls" is not correct from this perspective as a policy of non-biomass cooking is not CO2 control, and so the end of the sentence shall be adapted too. [Zbigniew Klimont, Austria]	Taken into account. This sentence is removed from the FGD.
50877	48	32	48	33	Suggested addition for clarification: 'Nevertheless, other approaches may aim to tackle poor air quality through legislation, which could reduce the abundance of SLCFs and exert an additional positive influence over global temperature in the short-term.' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This sentence is removed from the FGD.
2381	48	51	48	53	Is there really low confidence in the larger sensitivity of surface temperatures to aerosol emissions in NH and over land (regions with high emissions)? See papers by Shindell et al, Westervelt et al., Samset et al, PDRMIP papers and many others. Also see https://www.nature.com/articles/s41467-018-05838-6. [Vaishali Naik, United States of America]	Noted. This paragraph is removed in the FGD.

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2383	48	53	48	56	I don't understand what is meant by "knock on effects". Projected global reductions in aerosol emissions will have impacts everywhere, unless the point is that differences in projections of regional aerosol emissions may be regionally varying impacts. [Vaishali Naik, United States of America]	Taken into account. This sentence is removed from the FGD.
16665	49	2	49	5	All the determinants of ozone forcing are listed here. So it is *certain* that the ozone forcing is determined by these - as what else is there? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This paragraph is removed in the FGD.
2385	49	2	49	5	Future tropospheric ozone forcing is better discussed in section 6.6. [Vaishali Naik, United States of America]	Taken into account. This sentence is removed from the FGD.
45815	49	2	49	5	Part of the mentioned changes are feedbacks, and should therefore not be included in the (effective) tropospheric ozone forcing. For instance, the changes to the lightning NOx production described by Banerjee et al. and Finney et al. are mainly a response to future surface warming. Also, part of the changes in the stratosphere-troposphere exchange of ozone are a response to surface warming (Iglesias-Suarez et al.), and is therefore a feedback. Please correct this. [Twan van Noije, Netherlands]	Taken into account. This sentence is removed from the FGD.
114477	49	8	49	8	Can you explain this better? [Jan Fuglestvedt, Norway]	Taken into account. Sentence simplified in FGD to only mention uncertainty in anthropogenic aerosol ERF.
2387	49	9	49	10	Also see chapter 6 section 6.6. [Vaishali Naik, United States of America]	Taken into account. Reference to chapter 6 added in FGD.
37905	49	11	49	11	I hope to add one more uncertainty in the climate impacts of SLCF in the future, (iv) nonlinear interaction among the SLCFs (Dobricic et al., 2019; Ming and Ramaswamy, 2009; Shim et al., 2019). - Nonlinear impacts of future anthropogenic aerosol emissions on Artic warming (Dobricic et al., 2019) - Nonlinear climate and hydrological responses to aerosol effects (Ming and Ramaswamy, 2009) - Effects of anthropongenic and natural forcings on the summer temperature variations in East Asia during the 20th century (Shim et al., 2019) [Junhee Lee, Republic of Korea]	Rejected. Several of these studies refer to regional effects of SLCFs which are beyond the scope of chapter 4.
16667	49	11	49	14	It is not just that the methane shortwave forcing was underestimated previously, it was completely missing! [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Underestimated changed to "neglected" in FGD.
2389	49	12	49	22	These are discussed in chapters 6 and 7. I am wondering if these chapters could be cited rather than elaborating here. One big uncertainty in the climate response to SLCFs in the future comes from biogeochemical feedbacks (see section 6.3.6). [Vaishali Naik, United States of America]	Taken into account. Cross referencing to chapters 6 and 7 has been improved in these sentences.
132487	49	14	49	14	ERF is already defined above. [Kyle Armour, United States of America]	Taken into account. Removed in FGD.
45817	49	14	49	16	The studies by Banerjee et al. and Finney et al. describe changes in lightning NOx production in response to future climate change. This is (mostly) a feedback (i.e. a response to surface warming), which does not contribute to future ozone radiative forcing. Please correct this. [Twan van Noije, Netherlands]	Taken into account. This sentence is removed from the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15937	49	17	49	18	The statement: "If the recent observed growth in methane emissions were to continue until 2100, it would add an additional 0.5 Wm-2" is misleading by starting with the word "if" which implies that the observed growth is not going to persist. As a minimum there should some qualifying statement about how likely it is that methane emissions could be brought under control. Better is to have acknowledgement of the risk that melting of subsea permafrost could lead to rapid and large scale emissions that are currently unquantifiable. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is removed from the FGD.
53023	49	17			what about the SSPs? [Hervé Douville, France]	Taken into account. More information on the role of SLCFs in the SSPs is added in the FGD.
50879	49	19	49	19	...compared to the RCP2.6 scenario' - does this mean an additional 0.5 WM-2 is not expected under RCP2.6 because the observed growth in methane emissions is not expected to continue to 2100? Please clarify this, and if CH4 emissions did add 0.5 WM-2 to RCP2.6 whether this would mean it wouldn't be broadly consistent with the Paris Agreement goal. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence is removed from the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54545	49	22	49	35	There is a gap in the report which could well be addressed in this paragraph, namely advances since AR5 in the understanding of the influence of volcanic eruptions on climate. Some notable examples: - Schurer et al., 2013 found that "volcanic forcing is the dominant driver of forced variability in preindustrial SATs" from 850-1950 CE -Schurer et al., 2014: Concluded that "solar forcing probably had a minor effect on Northern Hemisphere climate over the past 1,000 years, while, volcanic eruptions and changes in greenhouse gas concentrations seem to be the most important influence over this period." - Sigl et al., 2015: "large eruptions in the tropics and high latitudes were primary drivers of interannual-to-decadal temperature variability in the Northern Hemisphere during the past 2,500 years." and "Overall, cooling was proportional to the magnitude of volcanic forcing and persisted for up to ten years after some of the largest eruptive episodes." - McGregor et al. (2015): "Climate simulations using single and cumulative forcings suggest that the ocean surface cooling trend from 801 to 1800 CE is not primarily a response to orbital forcing but arises from a high frequency of explosive volcanism. Our results show that repeated clusters of volcanic eruptions can induce a net negative radiative forcing that results in a centennial and global scale cooling trend via a decline in mixed-layer oceanic heat content." Schurer, A. P., Hegerl, G. C., Mann, M. E., Tett, S. F. B. and Phipps, S. J.: Separating Forced from Chaotic Climate Variability over the Past Millennium, J. Clim., 26(18), 6954–6973, doi:10.1175/JCLI-D-12-00826.1, 2013. Schurer, A. P., Tett, S. F. B. and Hegerl, G. C.: Small influence of solar	Taken into account. References are added.
2391	49	22	49	35	Also see papers coming out of VOLMIP http://www.volmip.org/index.php?id=publications [Vaishali Naik, United States of America]	Taken into account. Related references are added.
114479	49	23	49	23	I suggest adding "or a series of eruptions" [Jan Fuglestvedt, Norway]	Taken into account. We also mentioned a decadal to multi-decadal sequence of small-to-moderate volcanic eruptions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54547	49	23	49	24	The impact of the small-to-moderate eruptions was a pretty big issue in the aftermath of the AR5, and probably deserves a more substantial description, with a more representative sampling of the literature, including e.g., papers by Solomon et al., 2011, Fyfe et al., 2013 and Schmidt et al., 2018: Solomon, S., Daniel, J. S., Neely, R. R., Vernier, J. P., Dutton, E. G. and Thomason, L. W.: The Persistently Variable "Background" Stratospheric Aerosol Layer and Global Climate Change., Science, 333(6044), 866–70, doi:10.1126/science.1206027, 2011. Fyfe, J. C., Gillett, N. P. and Zwiers, F. W.: Overestimated global warming over the past 20 years, Nat. Clim. Chang., 3(9), 767–769, doi:10.1038/nclimate1972, 2013. Schmidt, A., Mills, M. J., Ghan, S., Gregory, J. M., Allan, R. P., Andrews, T., Bardeen, C. G., Conley, A., Forster, P. M., Gettelman, A., Portmann, R. W., Solomon, S. and Toon, O. B.: Volcanic Radiative Forcing From 1979 to 2015, J. Geophys. Res. Atmos., 123(22), 12,491-12,508, doi:10.1029/2018JD028776, 2018. [Matthew Toohey, Canada]	Taken into account. Related references are added.
54549	49	29	49	30	These concrete numbers are representative only for the magnitudes of eruptions which were included in the simulation. Progress since AR5 using climate proxies and model simulations supports the possibility of even stronger and longer-lasting climate perturbations from stronger eruptions or clusters of eruptions, e.g., Northern European cooling of -2C after the 536/540 CE double event. Refs: Sigl, M., Winstrup, M., McConnell, J. R., Welten, K. C., Plunkett, G., Ludlow, F., Büntgen, U., Caffee, M., Chellman, N., Dahl-Jensen, D., Fischer, H., Kipfstuhl, S., Kostick, C., Maselli, O. J., Mekhaldi, F., Mulvaney, R., Muscheler, R., Pasteris, D. R., Pilcher, J. R., Salzer, M., Schüpbach, S., Steffensen, J. P., Vinther, B. M. and Woodruff, T. E.: Timing and climate forcing of volcanic eruptions for the past 2,500 years, Nature, 523, 543–549, doi:10.1038/nature14565, 2015. Büntgen, U., Myglan, V. S., Ljungqvist, F. C., McCormick, M., Di Cosmo, N., Sigl, M., Jungclaus, J., Wagner, S., Krusic, P. J., Esper, J., Kaplan, J. O., de Vaan, M. A. C., Luterbacher, J., Wacker, L., Tegel, W. and Kirdyanov, A. V.: Cooling and societal change during the Late Antique Little Ice Age from 536 to around 660 AD, Nat. Geosci., 9(3), 231–236, doi:10.1038/ngeo2652, 2016. Toohey, M., Krüger, K., Sigl, M., Stordal, F. and Svensen, H.: Climatic and societal impacts of a volcanic double event at the dawn of the Middle Ages, Clim. Change, 136(3–4), 401–412, doi:10.1007/s10584-016-1648-7, 2016. [Matthew Toohey, Canada]	Taken into account. Related references are added.
54551	49	30	49	32	Not clear if "multi-decadal" refers to the length of the reconstructions or some aspect of their variability. Plus this statement is missing supporting citations. [Matthew Toohey, Canada]	Taken into account. We clarify this sentence, references are also added.

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33207	49	32	49	35	Zuo et al. (2019a and b) revealed the dyring of climatologically wet regions and wetting of climatologically dry regions following tropical volcanic eruptions. I suggest adding these 2 references here. References:Zuo, M., T. Zhou, and W. Man, 2019: Hydroclimate Responses over Global Monsoon Regions Following Volcanic Eruptions at Different Latitudes. Journal of Climate, 32, 4367-4385.doi:10.1175/jcli-d-18-0707.1 Zuo, M., T. Zhou, and W. Man, 2019: Wetter global arid regions driven by volcanic eruptions.	Taken into account. References are added.
50881	49	32	49	45	Would CO2 from large (Pinatubo scale or larger) eruptions potentially contribute to a longer-term warming impact? It would be helpful to state here if so. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Compared with the anthropogenic carbon dioxide emission, the magnitude of carbon dioxide produced by volcanic eruptions is small. The impact of sulphate aerosols plays a dominant role. So the effect of the carbon dioxide produced by volcanic eruptions is not taken into account here.
54553	49	33	49	33	The references here are all model studies I believe. Understanding of the impact of volcanism on precip has also been advanced through analysis of proxy data, e.g., Rao et al. (2017): Rao, M. P., Cook, B. I., Cook, E. R., D'Arrigo, R. D., Krusic, P. J., Anchukaitis, K. J., LeGrande, A. N., Buckley, B. M., Davi, N. K., Leland, C. and Griffin, K. L.: European and Mediterranean hydroclimate responses to tropical volcanic forcing over the last millennium, Geophys. Res. Lett., 44(10), 5104–5112, doi:10.1002/2017GL073057, 2017. [Matthew Toohey, Canada]	Rejected. Precipitation responses on a global scale are concerned here.
2393	49	37	49	37	Remove "generally" to avoid ambiguity. Volcanic forcings were turned off CMIP5 future simulations. [Vaishali Naik, United States of America]	Accepted. Removed.
99401	49	37	49	40	A relevant addition here could be that high-latitude volcanic activity can be accelerated by thinning of ice sheets under anthropogenic warming (Tuffen, 2010), an uncertainty that to my knowledge has not yet been considered in studies of long-term climate projections that include volcanic forcing. Tuffen, H. (2010). How will melting of ice affect volcanic hazards in the twenty-first century?. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 368(1919), 2535-2558. See also: Maclennan, J., Jull, M., McKenzie, D., Slater, L., & Grönvold, K. (2002). The link between volcanism and deglaciation in Iceland. Geochemistry, Geophysics, Geosystems, 3(11), 1-25. [Herman Fuglestvedt, Norway]	Rejected. Several of these studies are beyond the scope of chapter 4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12219	49	37	49	49	This paragraph does a poor job of explaining the important concept of the infleunce of volcanism in the future. From the legend of Fig. 4.19 it appears that in the VOLC one model was run multiple times with volcano strength and timing quasi-randomly distributed. How were the magnitude and timing set? I do not see the value of means of random volcanoes, rather for a fixed set of volcanoes for a model with varying initial conditions. This would give a much better idea of the influence of volcanisim. If this is not possible, then it is much better to focus on a small number of realistic futures to understand the short and middle term influence of volcanoes. Is this really the only study of this type? [Bryan Weare, United States of America]	Noted. The detailed experiment design can be referred to Bethke et al., 2017. There is no enough space to give a detailed description here. The focus here is the possible effect of random volcanic forcing on future climate projections. Due to the unpredictability of volcanic eruptions, possible scenarios need to be considered as much as possible. A fixed set of volcanoes for a model with varying initial conditions emphasize on the role of internal variability, which is beyond the scope of Chapter 4.
45509	49	38	49	42	I think the work of Swingedouw et al. (2015, 10.1038/ncomms7545) that illustrated how volcanic eruptions could be a pacemaker for AMOC and therefore North Atlantic climate and its predictions should be referenced here. This paper would add an interesting twist to the existing argument on volcanic influence on decadal predictions. Also, Haustein et al. (2020, 10.1175/JCLI-D-18-0555.1) could be worth considering with an argument along similar lines. [Leonard Borchert, France]	Taken into account. Related references are added.
54555	49	39	49	39	There have also been substantial advances in understanding the impact of volcanism on seasonal-decadal timescales, e.g., Swingedouw et al. (2017): Swingedouw, D., Mignot, J., Ortega, P., Khodri, M., Menegoz, M., Cassou, C. and Hanquiez, V.: Impact of explosive volcanic eruptions on the main climate variability modes, Glob. Planet. Change, 150, 24–45, doi:10.1016/J.GLOPLACHA.2017.01.006, 2017. [Matthew Toohey, Canada]	Taken into account. Related references are added.
114481	49	44	49	44	But this depends on teh size of the volcanoes, so it woudl be good if yo say something about that. [Jan Fuglestvedt, Norway]	Taken into account. More details can be found in Bethke et al. 2017, there is no enough space here to go into more detail.
114483	49	46	49	49	It is a bit uncelar hwo much of this is assessment by the authors and how much is review. [Jan Fuglestvedt, Norway]	Noted. The conclusions here can be referred to Bethke et al. 2017. Reference: Bethke, I., Outten, S., Otterå, O. H., Hawkins, E., Wagner, S., Sigl, M., et al. (2017). Potential volcanic impacts on future climate variability. Nat. Clim. Chang. 7, 799–805. doi:10.1038/nclimate3394.
114485	49	48	49	48	Can you say something more about the potential implication sof the last point made in this para ? (An is this para fully consitent with 4.2.4.?) [Jan Fuglestvedt, Norway]	Taken into account. We clarify this point in more detail.
50883	49	48	49	49	Suggested edit for clarification: 'likelihood of individual decades with negative GSAT trend, compared to previous decades; and..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We clarify this point in more detail.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127551	50	15	50	20	This would SEEM to be the place to look at some of the new VolMIP simulations for CMIP6. Not all models exhibit ENSO+ behavior post-eruption. And some paleo evidence is equivocal. Well, Zuo et al. (2018) looked at CESM, but this paper looked at BOTH GISS-E2 and CESM and it was first. And this is an IPCC, not CESM report. Citation: Colose, C.M., A.N. LeGrande, and M. Vuille, 2016: Hemispherically asymmetric volcanic forcing of tropical hydroclimate during the last millennium. Earth Syst. Dyn., 7, 681-696, doi:10.5194/esd-7-681-2016. [Trigg Talley, United States of America]	Taken into account. References are added.
106955	50	15	50	29	There is a lot of litterature on the impact of the volcanic eruption on the NAO and there is no reason why these papers are not assessed here since volcanic forcing on ENSO is presented in this section. I would suggest to cite a review paper by Swingedouw et al. (2017, https://doi.org/10.1016/j.gloplacha.2017.01.006) where many references can be found herein and the Menegoz et al (2018, https://doi.org/10.1007/s00382-017-3986-1) paper emphasing the need for large ensemble to extract the forced volcanic signal on the NAO that could even be conditionnal to the AMV phase. [Christophe CASSOU, France]	Taken into account. The main concern here is tropical SST variability, and the volcanic impacts on historical climate is covered in other chapters (Chapter 3 and 8).
14465	50	15			typo: “El Niño” instead of “El Nino” [Malte Stuecker, United States of America]	Accepted. Revised.
24033	50	17	50	17	Also Hermanson et al 2020, 10.1029/2019JD031739 [Doug Smith, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; while the text in question has been shortened, the reference has been added to 4.3.6-
50885	50	25	50	26	The El Nino and postive Indian Ocean Dipole events are associated with large tropical eruptions - suggest this sentence is written the other way round. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, revised.
53025	50	28	50	29	may deserve a confidence statement? [Hervé Douville, France]	Taken into account. Added.
71935	50	45			I would suggest putting Regional in the title to more clearly ditinguish it from the previous section. [John Church, Australia]	Rejected. This section assesses the global pattern and not the regional details
106957	50	49	50	56	This paragraph is very important and should be put upfront at the very beginning section 4.3 or section 4,4. [Christophe CASSOU, France]	Rejected. The sequence of sections is the result of long considerations of structural coherence by the chapter team.
79753	50	51	50	52	perhaps add that also the possible interaction/rectification between these two factors [Laurent Terray, France]	Taken into account. Interaction is referred to.
53027	50	54	50	56	and may therefore underestimate climate change in areas with strong intermodel spread. [Hervé Douville, France]	Noted. This is discussed in previous sections.
79755	50	56	51	2	this differs from what is in the caption of figure 4.21. I suggest to put the correct definition only in the caption, no deed to repeat it in the text. [Laurent Terray, France]	Taken into account. Text is rephrased.
19253	51	1	51	1	One standard deviation, rather than two? [Anne-Marie Treguier, France]	Rejected. 2 standard deviations correspond to a harmonized agreement across chapters.
87577	51	16	51	21	caption of fig 4.21 is more complete than the corresponding one on page 163. [Valentina Roberta Barletta, Denmark]	Taken into account. Caption is corrected
102975	51	16	51	21	caption of fig 4.21 is more complete than the corresponding one on page 163. [Philippe Tulkens, Belgium]	Taken into account. Caption is corrected
127553	51	16	51	21	Inconsistent captions here and below the figure. [Trigg Talley, United States of America]	Taken into account. Caption is corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71937	51	16		21	Suggest add a diagram of humidity change here since the next few paragrtaphs emphasise its importance and because it is an important impact for drought, fires, etc. [John Church, Australia]	Taken into account. Relative humidity is shown in Fig.4.25
106959	51	26	51	26	I would add: "… in any given model and in observations, can be…" [Christophe CASSOU, France]	Rejected. This projection chapter is focusing on models not observations
79757	51	26	51	29	This has already been mentioned several times in this chapter, there is no need to repeat it here I think [Laurent Terray, France]	Taken into account. Statement is removed here.
19871	51	26	51	37	It is necessary that this subsection comments and interprets the striking behaviour of every projection in the northern Atlantic, as shown by figure 4.21. [philippe waldteufel, France]	Taken into account. The local cooling is referred to and a reference to the corresponding section in CH9 is added
106961	51	30	51	30	I would add: "… the more the sign and amplitude of the regional…" [Christophe CASSOU, France]	Accepted
79759	51	32	51	34	Yes, very important to stress that. Perhaps it should even be highlighted more by being put at the beginning of section 4.5 [Laurent Terray, France]	Taken into accounted. The difference between raw model and assessed GSAT is highlighted at the ES level.
84263	51	40	52	15	these arguments are considered also in section 8.2.1 for consequences in annual mean precipitation changes [Annalisa Cherchi, Italy]	Noted
106963	51	41	51	41	Is "virtual certain" necessary? Could it be simply stated as a fact? Where is the uncertainty? [Christophe CASSOU, France]	Noted. Virtually certain is kept because the statement is about the future.
127555	51	41			There is also good paleo-climate evidence the land-ocean warming difference is consistent with model predictions (Schmidt et al, 2014, doi:10.5194/cp-10-221-2014; Izumi et al. , 2013, doi:10.1002/grl.50350). [Trigg Talley, United States of America]	Taken into accounted. References to paleo evidence is added.
42947	51	48	53	51	The role of the lapserate feedback by Brogli et al 2019 and Kroner et al 2017 should also be mentioned here. [Rein Haarsma, Netherlands]	Taken into account. References to Mediterranean amplification are added
106965	51	50	51	53	I would add a confidence level here, I guess "high confidence"? [Christophe CASSOU, France]	Accepted
116331	51		51		For the land ocean contrast, a reference to insights from paleo simulations (ch 3) could be made. [Valerie Masson-Delmotte, France]	Accepted. The revised text refers to CH3
19873	52	11	52	11	Is it appropriate to speak here of uncertainty? While there are mechanisms able to explain why continental surfaces become warmer, these mechanisms as well as surface properties are not uniform. [philippe waldteufel, France]	Noted. The differences contribute to the response uncertainty across models
70403	52	14	52	15	“sensitive to characteristics of land surfaces that are challenging to model, including stomatal conductance and soil moisture (Berg et al., 2016).” An additional reference is Zarakas et al. (in revision, doi:10.31223/osf.io/emgxb) which calculates the surface temperature change and land ocean temperature contrast due to plant physiological responses to increasing CO2. [Abigail Swann, United States of America]	Taken into account. References added
2163	52	18	52	18	No reference to PAMIP? [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References to PAMIP added
106967	52	19	52	19	Could "very likely" be upgraded to "virtually certain" like for the land-sea contrast? [Christophe CASSOU, France]	Because variability is large and the statement applies to all SSP scenarios including all low emission scenarios, "very likely" is more appropriate

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127557	52	20	52	21	[PROGRESS] Can authors state in this section whether the improvement in understanding since AR5 has resulted in a change in projections of the amplitude change or the mechanisms governing the amplification? Or whether any theories have been discounted as being important? What has been learned since the last report? [Trigg Talley, United States of America]	Taken into account. Progress since in AR5 is more clearly mentioned but very briefly because space is limited.
77691	52	30			warming in the Arctic will continue to be amplified [Emer Griffin, Ireland]	Accepted
65179	52	32	52	32	Beer et al 2020 (doi: 10.1029/2019GL086706) also suggested a role for changes in oceanic heat flux to contribute to Arctic amplification [Mark England, United States of America]	Taken into account and reference added
102977	52	33	52	33	Polar amplification mentions: Arctic amplification (see Chapter 7 Section 7.6.2), but that chapter concerns Physical descriptions of metrics. Polar amplification is under Chapter 7 Section 7.4.4.1 [Philippe Tulkens, Belgium]	Reference to chapter 7 is updated
19875	52	34	52	34	"to" lacking before "play" [philippe waldteufel, France]	Accepted
127559	52	36	52	37	Sentence is a little awkward. Suggest a rewrite. [Trigg Talley, United States of America]	Accepted
79761	52	39	52	39	Perhaps also moisture transport in addition to heat (e.g Woods and Caballero 2016) [Laurent Terray, France]	Noted
79763	52	41	52	43	I would also suggest to briefly mention the theory put forward by S. Lee and co-workers (see A theory for polar amplification from a general circulation perspective. Asia-Pac. J. Atmos. Sci., 50, 31–43, doi:10.1007/s13143-014-0024-7 for the general idea). Interaction between tropical diabatic heating and Arctic climate could also deserve a few lines. [Laurent Terray, France]	Taken into account but space is limited
65181	52	49	53	3	Polvani et al 2020 (doi: 10.1038/s41558-019-0677-4) suggest that ozone depleting substances contributed to Arctic amplification over the second half of the twentieth century. It is likely therefore, with current and projected reductions in CFCs that this contribution will decrease in the coming decades. [Mark England, United States of America]	Accepted
80001	53	2	53	3	Other forcing agents have also been found to strongly contribute to Arctic Amplification; one example are ozone depleting substances (CFCs) - see Polvani et al., 2020; DOI:10.1038/s41558-019-0677-4. This paper received a lot of media attention and even though the potential model-dependency of this result has not been tested yet, it would be good to briefly mention this study here. [Gabriel Chiodo, Switzerland]	Accepted
50887	53	17	53	17	current climate models which lack dynamic ice…' - please specify if this is the case for the majority of models, and where ice sheet dynamics are included, what these models show. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account but individual model configurations are not discussed here
127561	53	17			Also Rye et al., 2020 (doi:10.1029/2019GL086892). [Trigg Talley, United States of America]	Accepted
79765	53	22	53	30	I would suggest to add a few lines about proposed mechanisms and processes underlying these seasonal amplitude changes for each of the large-scale regions mentioned here [Laurent Terray, France]	Rejected. Discussion of regional processes does not fit in global chapter and often there is a lack of literature discussing the detailed processes

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65183	53	22	53	30	This paragraph is correct but there is an important role for internal variability in these projections which make the changes in the amplitude of the seasonal cycle more robust in some regions (Siberia, Western Europe/Mediterranean, North Africa) than others [Yetella and England 2018 doi: 10.1029/2018JD029066]. This paper shows, for example, that with only five ensemble members one cannot detect a change in the seasonal cycle amplitude over much of North America by the end of the century because of small signal to noise ratio. [Mark England, United States of America]	Taken into account. The role of internal variability is explicitly mentioned in the revised text
19877	53	28	53	28	The title of (Sanchez and Simon) in the reference list is "Transcriptional control of kidney cancer"; this might be interpreted as a trap to check whether reviewers take a look at this list. Or a private joke? [philippe waldteufel, France]	Accepted. Reference is corrected.
53029	53	29	53	30	What are the related key findings? [Hervé Douville, France]	Taken into account. Findings are now specifically mentioned
19293	53	42	54	41	A pointer to the chapter on extremes could be useful in that section on changes in temperature variability. [Anne-Marie Treguier, France]	Accepted
127563	53	42			This section doesn't mention jet stream stability changes -- and the impact on temperature variability at mid-latitudes. See, e.g., Francis and Vavrus, 2015 (doi:10.1088/1748-9326/10/1/014005). Perhaps this is a transient feature of a changing climate, but, at least now, the day-to-day temperature variability can be extremely large because of jet stream changes. If these changes are tied to decreased meridional temperature gradients, then this process needs mentioning here. [Trigg Talley, United States of America]	The potential circulation changes in the mid-latitudes are assessed in CH10. The corresponding section is referred to but the circulation changes are far more uncertain than the gradient changes.
111415	53	49	53	53	Consider including the findings of Rehfeld, K., Hébert, R., Lora, J. M., Lofverstrom, M., and Brierley, C. M.: Variability of surface climate in simulations of past and future, Earth Syst. Dynam., 11, 447–468, https://doi.org/10.5194/esd-11-447-2020, 2020 [James Renwick, New Zealand]	Taken into account and reference added
50889	53	51	53	52	global mean temps tend to decrease in a warmer world as a result of reduced albedo variability in high latitudes resulting from melting snow and ice…' - it would be helpful to include here how model experiments compare with observations. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The signal is not yet detectable in observations, at least there is no literature supporting this statement a detected signal in observations.
71283	54	12	54	12	"daily" -> "diurnal"? [Kenji Taniguchi, Japan]	Taken into account. Daily here refers to day-to-day and not diurnal. This is clarified
12227	54	12	54	13	High latitudes of the SH seem to have reduced variability in both seasons in Fig. 4.23. [Bryan Weare, United States of America]	Taken into account. This is correct but we have not assessed the robustness across a large set of models yet.
79767	54	13	54	13	this reference could be added here: T. Schneider et al. JCLIM 2016 DOI: 10.1175/JCLI-D-14-00632.1 [Laurent Terray, France]	Taken into account. Reference added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70897	54	14	54	17	This hypothesis has been substantiated by the recent analysis of Tamarin-Brodsky et al. (2020 doi 10.1038/s41561-020-0576-3) using a Lagrangian tracking methodology, which furtherrmore shows that in winter across NH midlatitudes the cold extremes reduce more than the warm ones, relative to the new climatology (hence there is a change in skewess) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference to Tamarin-Brodsky is added
132489	54	14	54	19	Perhaps something could be said here assessing the literature claiming more mid-latitude cold extremes with polar amplification. This is a hot topic right now, and readers may wonder whether why that mechanism is not discussed here. I personally think the model projectiions (and your assessment) is correct, but it would probably be good to say somethign about that debate, either here or [Kyle Armour, United States of America]	Taken into account. Reference added to the corresponding discussing the related literature. The thermodynamic contribution to the variability reduction tends to be more robust than the changes in atmospheric circulation
106969	54	14	54	19	I would suggest to add the Cassou and Cattiaux (2016, https://www.nature.com/articles/nclimate2969) reference showing clear shortening of the winter season and expansion of the summer season associated with a the reduction of snow cover in spring over Europe. [Christophe CASSOU, France]	Noted. The reference is added in the assessment of the seasonality.
70899	54	34	54	41	The recent analysis of Tamarin-Brodsky et al. (2020 doi 10.1038/s41561-020-0576-3) using a Lagrangian tracking methodology shows that in central Europe during summer, the magnitude of the cold extremes (relative to the new climatology) increase just as much as the magnitude of the warm extremes, because of advection from the colder Atlantic ocean [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference to Tamarin-Brodsky is added
79769	54	38	54	38	I would also suggest adding this reference which will help making links with WG2: Bathiany S, Dakos V, Scheffer M, Lenton TM. Climate models predict increasing temperature variability in poor countries. Sci Adv. 2018;4(5):eaar5809. Published 2018 May 2. doi:10.1126/sciadv.aar5809 [Laurent Terray, France]	Taken into account. Reference added .
79771	54	44	54	44	it seems strange that this important section has no references to papers published after AR5, there must be some out there ! [Laurent Terray, France]	Taken into account. There are relatively few new papers on projected upper air temperature trends since AR5, but some more references are added.
68779	54	46	55	35	The modelled projections for tropospheric temperatures might be tempered with analysis provided in Christy and McNider 2017 (DOI:10.1007/s13143-017-0070-z) that suggests a comparison between observed lower tropospheric temperature rise is half that projected by CMIP-5 models.It might also be acknowlegded that Figure 10.SM.1(b) from AR5, Ch 10 Supplementary Material (p. 10SM-6) shows significant departures between modelled and observed atmospheric temperatures. If the CMIP-6 models correct for this then this should be stated. Otherwise this discrepancies should be acknowledged. - Sean Rush, New Zealand [sean rush, New Zealand]	Noted. The comparison of model and observed tropospheric temperature trends over the historical is assessed in section 3.3.1.2. The assessment of future temperature trends in CH4 refers to the challenges in representing tropospheric temperature trends even at the ES level.
45511	54	47	54	47	I think grammatically it should be "… and it was very likely that..." [Leonard Borchert, France]	Accepted. Corrected
45513	54	48	54	48	Again, I think it should be "… large warming was likely in the tropical upper troposphere…" [Leonard Borchert, France]	Accepted. Corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19879	54	54	55	35	The tropopause ought to be indicated on these figures 4.24. In addition to the warming above the Antarctic, several features deserve comments, such as the latitudinal profile of the cooling in the stratosphere, with the lowest limit of the cooled zone right on the equator. [philippe waldteufel, France]	Rejected. The tropopause height was not available from all models used in the figure. The structure of stratospheric cooling has been discussed in relation to the brewer Dobson circulation strengthening.
87579	55	1	55	1	(fig 4.24). This caption is more complete than the one at page 166. [Valentina Roberta Barletta, Denmark]	Taken into account Captions have been harmonized.
102979	55	1	55	1	(fig 4.24). This caption is more complete than the one at page 166. [Philippe Tulkens, Belgium]	Taken into account Captions have been harmonized.
45515	55	6	55	6	Omit the ":" after "Figure 4.24". [Leonard Borchert, France]	Accepted. Corrected
76851	55	17	55	19	"Annual wet-day precipitation totals, as well as average wet-day precipitation intensity, have increased significantly, accompanied by a significant increase in the frequency and intensity of precipitation extremes in Singapore." (1) Cheong, W. K., Timbal, B., Golding, N., Sirabaha, S., Kwan, K. F., Cinco, T. A., et al. (2018). Observed and modelled temperature and precipitation extremes over Southeast Asia from 1972 to 2010. Int. J. Climatol. 38, 3013–3027. doi:10.1002/joc.5479. (2) Li, X., Wang, X. and Babovic, V. (2018), Analysis of variability and trends of precipitation extremes in Singapore during 1980–2013. Int. J. Climatol, 38: 125-141. doi:10.1002/joc.5165 [Sandeep Sahany, Singapore]	Noted. The chapter deals with global large-scale climate indicators. Chapter 10 and the Atlas assess regional changes. The comment also seems out of place.
84265	55	18	55	18	what "regions"? Whole sentence is not fully clear [Annalisa Cherchi, Italy]	Taken into account. This sentence has been changed and regions is no longer used.
50891	55	20	55	20	adiabats' - please explain this term. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence has been changed and adiabats is no longer used.
50893	55	21	55	23	CMIP6 models overestimate tropical upper tropospheric warming since 1979 and that this is caused by an overestimate of the SST trend during this period' - please clarify the impact of this on CMIP6 estimates of historical warming. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence has been changed and the reference to overestimated SST trends has been removed since it is assessed in detail in Chapter 3 including implications for historical CMIP6 warming. A citation to section 3.3.1.2 has been retained in 4.5.1.2
84267	55	25	55	35	this paragraph is totally without references/citations either to figures or to published works [Annalisa Cherchi, Italy]	Noted. There are few studies on projected atmospheric temperature changes since AR5 that are relevant to this section.
45517	55	38	55	38	This is the first time I consciously read a "we" in the IPCC report. Is that how it's supposed to be? Sounds a bit to paper-y in my opinion. Consider rephrasing. [Leonard Borchert, France]	Taken into account. Changed to 'It is assessed….'
87581	55	48	55	48	title is "Near surface humidity" but in the text it is always referring to relative humidity, apart sporadic mentions… [Valentina Roberta Barletta, Denmark]	Taken into account. This subsection assesses near-surface relative humidity. It is now clarified in the text.
102981	55	48	55	48	title is "Near surface humidity" but in the text it is always referring to relative humidity, apart sporadic mentions… [Philippe Tulkens, Belgium]	Taken into account. This subsection assesses near-surface relative humidity. It is now clarified in the text.
84269	55	48	56	24	theoretical framework for water cycle constraints assessed in sections 8.2.1 and 8.2.3 and it should be referenced [Annalisa Cherchi, Italy]	Accepted. Cross-chapter references are added.
71941	55	48	57	46	Suggest moving this subsection to immediately after the greater warming over land subsection. [John Church, Australia]	Rejected. The sequence of sections is the result of long considerations of structural coherence by the chapter team; thus it's not changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53031	55	48			Add a final summary which could be also uplifted in the ES? [Hervé Douville, France]	Added
106971	55	48			A summary paragraph is missing at the end of this subsection similarly to what is provided for section 4.5.1.2 [Christophe CASSOU, France]	Accepted. Added
132491	55	50	55	51	My understanding is that RH is expected to remain nearly constant with warming in the free troposphere, but not at the near-surface. In fact, the rest of this paragraph describes how RH is not constant over land or oceans. [Kyle Armour, United States of America]	Taken into account. This sentence has been reorganized to focus on the reductions in near-surface land RH.
71939	55	54		55	Hmmm? In the greater land warming section it was argued that the land was warmer because of reduced humidity and now it is argued that the land is less humid because it is warmer. This sounds circular - the argument needs to be clearer [John Church, Australia]	Taken into account. There are positive feedbacks between the greater land warming and reduction in land RH. This sentence has been rephrased.
130493	56	3	56	13	I am afraid this part overlaps with other chapters. [Panmao Zhai, China]	Taken into account. Cross-chapter and cross-section references are added.
19881	56	6	56	10	This whole subsection is not well written; the lines selected here illustrate this assessment. The 1st sentence is obscure. The second one, to become acceptable, need that a "of" is added before "soils". The last one (lines 9-10) is void of any information; it just contributes to inflate the SOD. [philippe waldteufel, France]	Taken into account. The sentences have been rephrased.
70405	56	7	56	8	“However, the changes in land RH are also strongly influenced by evapotranspiration, which is suppressed by the drying soils under climate change” Evapotranspiration is also influenced by stomatal conductance, and the direct influence of plant responses to CO2 (rather than soil drying) can be seen in C4MIP experiments in which only the land surface sees increasing CO2. About half of the change in RH over land is attributable to plant responses to CO2 (Swann et al. 2016, Lemordant et al. 2018). Given the large contribution of stomatal closure for modulating near surface RH over land it should be mentioned as first order along with soil moisture. [Abigail Swann, United States of America]	Accepted. Plant responses are mentioned, and references are added.
28829	56	13		19	Could also refer to Chapter 8.2.2.1/Figure 8.4 and check for consistency. The main reason for the lack of wet gets wetter, dry gets drier over land is that long-term P-E is positive and expected to become more positive as moisture transport amplifies. More detailed assessment of spatial thermodynamic gradients and feedbacks however drive decreases in some regions and also it is recognised that during the dry season, P-E can be negative and amplified as greater moisture divergence dries soils. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cross-chapter reference added.
132493	56	16	56	16	"high" should probably be "higher" in both instances here. [Kyle Armour, United States of America]	Corrected.
19883	56	17	56	20	Is one to understand that the " wet-gets-wetter, dry-gets-drier" principle can only be applied over the oceans? [philippe waldteufel, France]	Taken into account. The " wet-gets-wetter, dry-gets-drier" principle mainly applies over the ocean.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3703	56	30	56	32	Surely the one coherent region of land showing a significant increase should be mentioned? The Greather Horn of Africa in JJA. This is also one of the main tropical region with a signficiant increase in DJF (fig4.27) and annual (fig4.35) rainfall. The two results presumably not unrelated. Increasing annual rainfall in the region, as shown by CMIP6, is supported by my recent study using a state-of-the-art convection-permitting model for the region, thereby providing an additional line of evidence for the result. https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-19-0328.1?mobileUi=0 [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. However, space constraints have precluded the inclusion of this exception to the general tendency.
50895	56	30	56	32	Re significant decrease in relative humidity in the mid-latitude southern hemisphere - please specify if this is in general, or in summer or winter. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and specified
84271	56	35	56	45	labels above panels to be adjusted (2080-2100 repeated twice, while 2040-2060 is missing) [Annalisa Cherchi, Italy]	Accepted and Adjusted.
87583	56	37	56	43	This caption is much more extensive than the one at page 167. And the caption mentions predictions for 2041-2060, but those are not in the figure. [Valentina Roberta Barletta, Denmark]	Accepted and Adjusted.
102983	56	37	56	43	This caption is much more extensive than the one at page 167. And the caption mentions predictions for 2041-2060, but those are not in the figure. [Philippe Tulkens, Belgium]	Accepted and Adjusted.
71285	56	48	57	36	Necessity of description of heat index here. It should be put in Chapter 12? [Kenji Taniguchi, Japan]	Not applicable. The part is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79051	56	48	57	36	I have a few concerns over the general coverage of heat stress. First, this term is not (so far as I can tell) defined anywhere – yet it can mean different things. It seems to be used here very generally to refer to "how hot it feels" from the combined effects of atmospheric sensible and latent heat. This needs to be stated, and it needs to be clarified that changes in radiation are not being discussed in this section. For some help setting the context, I suggest either consulting Matthews (2018) or Buzan and Huber (2020). Second, the text needs to differentiate between increases in the *frequency* of values over thresholds, and absolute changes in the magnitude of metrics. Generally, low-latitude environments should expect to see a much larger increase in threshold exceedances because a high proportion of values already lay very close to these critical levels. *Separately* those regions are *also* expected to see some of the largest absolute changes in humid heat metrics. The latter is because projected changes in moist enthalpy/equivalent temperature are larger hot/humid climates due to the water vapour feedback. For help making these points, I suggest consulting Matthews et al. (2017) [who also looked at heat stress as a function of global warming amounts]; Matthews et al. (2018); and Mora et al. (2017). References Buzan, J.R. and Huber, M., 2020. Moist heat stress on a hotter Earth. Annual Review of Earth and Planetary Sciences, 48. Matthews, T.K., Wilby, R.L. and Murphy, C., 2017. Communicating the deadly consequences of global warming for human heat stress.	Not applicable. The part is removed.
28831	56	48			I was slightly surprised to see heat stress in this section despite the dependence on humidity due to the dominating effects of temperature changes rather than humidity and wind speed changes [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part is removed.
132495	56	56	57	2	Wet-bulb temperature is an important concept. Is it worth defining it here, even qualitatively, and saying why it's relevant? [Kyle Armour, United States of America]	Not applicable. The part is removed.
116333	56		56		I see a potential for improved x chapter coordination on land processes coupling energy, water and carbon fluxes (in relationship with the section on terrestrial aridity and implications) to develop a clear message (also for TS-SPM integration). [Valerie Masson-Delmotte, France]	Taken into account. Cross chapter references are added.
116335	56		56		Heat stress needs to be addressed also in ch 2 and 3 for coherency of structure. [Valerie Masson-Delmotte, France]	Not applicable. The part is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79053	57	13	57	17	The text about wet-bulb exceeding 35C will now need to be updated with the results of Raymond et al. (2020), who found that this threshold has been exceeded already (they also provide a comprehensive assessment of historical changes). References Raymond, C., Matthews, T. and Horton, R.M., 2020. The emergence of heat and humidity too severe for human tolerance. Science Advances, 6(19), p.eaaw1838. [Tom Matthews, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part is removed.
79055	57	17	57	18	The text comparing southern Europe/Mediterranean to Southwest Asia and the North China Plain needs to change. The latter experience far more significant humid heat; the processes that generate high humid heat may be similar between locations, but the severity of the threat is not. I also think the Persian Gulf should be included amongst the examples. It experiences the most severe humid heat on Earth (Raymond et al., 2020; Pal and Eltahir, 2015). References Raymond, C., Matthews, T. and Horton, R.M., 2020. The emergence of heat and humidity too severe for human tolerance. Science Advances, 6(19), p.eaaw1838. Pal, J.S. and Eltahir, E.A., 2016. Future temperature in southwest Asia projected to exceed a threshold for human adaptability. Nature Climate Change, 6(2), p.197. [Tom Matthews, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part is removed.
50897	57	20	57	20	Typo: 'eastern United States…' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part is removed.
50899	57	22	57	22	urban humidity deficit' - it would be useful to clarify if this is this relevant only in cities far from the coast. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79057	57	24	57	27	This text could do more to recognise themes from earlier in the chapter, and to recognise earlier work. The near constancy of relative humidity under climate warming was noted erlier in the text, and under concurrent temperature increases, it is self-evident that humid heat ("heat stress") will increase faster than air temperature. Delworth (1999) demonstrated this long before the more subtle and nuanced messaging from Li et al. (2018) – the study referenced in the text. References Delworth, T.L., Mahlman, J.D. and Knutson, T.R., 1999. Changes in heat index associated with CO2-induced global warming. Climatic Change, 43(2), pp.369-386. Li, J., Chen, Y.D., Gan, T.Y. and Lau, N.C., 2018. Elevated increases in human-perceived temperature under climate warming. Nature Climate Change, 8(1), pp.43-47. [Tom Matthews, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part is removed.
9805	57	24	57	30	See also Li et al 2020, doi:10.1088/1748-9326/ab7d04, for changing WBGT extremes as a function of GSAT [Robert Kopp, United States of America]	Not applicable. The part is removed.
79773	57	24	57	30	These two recent references are also very relevant here: Brouillet, A., & Joussaume, S. (2019). Investigating the role of the relative humidity in the co-occurrence of temperature and heat stress extremes in CMIP5 projections. Geophysical Research Letters, 46, 11435– 11443. https://doi.org/10.1029/2019GL084156 and A. Brouillet has another paper that is I believe almost accepted in Climatic Change [Laurent Terray, France]	Not applicable. The part is removed.
19885	57	28	57	30	Impacts or hazards are not to be considered in chapter 4, according to the outline. [philippe waldteufel, France]	Not applicable. The part is removed.
79059	57	32	57	36	The description of wet-bulb temperature changes seems to be very incomplete; the figure also needs to be improved. Without the addition of zonal means (and with the coarse graduated colour scheme), it is difficult to see the apparent amplification of changes in high latitude northern regions (emphasised in the text). Equally striking, and of far greater significance (because of how hot and humid it is already), are the large changes projected for the Amazon Basin; North Africa; and the Indus Basin. Please consider reorienting the text to consider changes that are of most consequence for *heat stress*. Also consider changing the plot so that the colour ramp starts at 0C. I do not see any blue (cooling) in these panels! [Tom Matthews, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part is removed.
12235	57	33			In Fig. 4.26 I see neither distinct seasonal or regional charactristics; most differences are only one color code away, which could be minute. Unlike most other plots in this chapter, there is no measure of significance. [Bryan Weare, United States of America]	Not applicable. The part is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21671	57	36	57	36	There isn't an obvious concluding statement to this section in contrast to almost all other sections within the chapter. This is a pity because a key finding on heat stress should be considered for elevation to the ES and from there onto the TS and SPM given the societal implications. [Peter Thorne, Ireland]	Not applicable. The part is removed.
87585	57	47	58	36	This full page deals with heat stress and wet bulb temperature. It could deserve its own subsection. [Valentina Roberta Barletta, Denmark]	Not applicable. The part is removed.
102985	57	47	58	36	This full page deals with heat stress and wet bulb temperature. It could deserve its own subsection. [Philippe Tulkens, Belgium]	Not applicable. The part is removed.
84275	57	49	59	48	section 4.5.1.4 could be reduced and referenced to chapter 8 for the theoretical understanding of precipitation change [Annalisa Cherchi, Italy]	Accepted and implemented.
84273	57	51	57	53	sentence to rephrase as it contains repetitions [Annalisa Cherchi, Italy]	Accepted. Sentence is rephrased
12237	58	6			"significant" should not be used in this context [Bryan Weare, United States of America]	Accepted.
74349	58	9	58	11	Is it similar meaning with the Clausius-Clapeyron rate, where increasing the temperature by 1 degree Celcius will increase the water holding capacity in the atmosphere by 7%? [Yulizar Yulizar, Indonesia]	Taken into account. The relationship has been clarified more explicitly.
28841	58	11			~2%/oC or make consistent with earlier values in the chapter [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Global mean precipitation increases approximately 2–3% per °C of GSAT warming
132497	58	13	58	14	Perhaps cite doi: 10.1029/2008JD010561, 10.1007/s00382-018-4359-0, and 10.1029/2010JD013949 here as well, which provide a perspective from the surface energy budget. Also, some of the text of the paragraph repeats on page 59, lines 6-14. [Kyle Armour, United States of America]	Accepted. Relevant references are included in the assessment. Repeated text has been removed.
53033	58	14	58	15	although land surface processes and feedbacks may also play a significant role (e.g., Chadwick et al. 2017, Richardson et al. 2018)? [Hervé Douville, France]	Taken into account. Discussion on regional processes are addressed in Chapter-8, hence removed from this section.
28833	58	19			More precisely an amplification of P-E patterns and the dry drier does not simply apply over subtropical land as discussed on p.56 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
106973	58	27	58	28	Is the Hadley cell widening significant in both hemispheres? Can differences be made between the southern and northern cells? For southern one, I would add this reference: Jebri et al. (2019, https://doi.org/10.1175/JCLI-D-19-0304.1) [Christophe CASSOU, France]	Rejected. The HC widening is not globally uniform, thus can not assess the impact of NH and SH HC widening to tropical precipitation change.
53035	58	33			Section 8.2.1 [Hervé Douville, France]	Accepted. Corrected the subsection details
28835	58	33			Section 8.2.1 is the correct section in Chapter 8 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the subsection details.
53037	58	38	58	40	This PDRMIP result must be reconciled with the ScenarioMIP results summarized in Section 4.3.1.2 [Hervé Douville, France]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28837	58	41			Additionally, fast adjustments to radiative forcings that more rapidly warm the land than ocean also contribute to the total increases in precipitation that counteract and appear to dominate the larger land than ocean precipitation response shown in Table 4.3 with the help of reduced aerosol cooling effects (refer to 8.2.1 which will also need to be updated/corrected to be clearer about the expected effects on transient precipitation responses) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Modified the text accordingly.
106975	58	43	58	43	Add the type of scenario or warming level considered here. [Christophe CASSOU, France]	Accepted. Based on RCP4.5 and RCP8.5 scenarios.
87587	58	47	58	49	these lines repeat, for the general precipitation patterns, what already said in lines 18-20 for thermodynamic response only. This section ie very explanatory, but it could be harmonized, collecting the common background info and stating more clearly each of the specific aspect for which the predictions are given. [Valentina Roberta Barletta, Denmark]	Accepted. Section reframed.
102987	58	47	58	49	these lines repeat, for the general precipitation patterns, what already said in lines 18-20 for thermodynamic response only. This section ie very explanatory, but it could be harmonized, collecting the common background info and stating more clearly each of the specific aspect for which the predictions are given. [Philippe Tulkens, Belgium]	Accepted. Section reframed.
28839	58	50			However, signals of wet get wetter, dry get drier do emerge when sampling and following wet and dry regions in space and time (Chou et al. 2013 Nature Geosci. http://dx.doi.org/10.1038/NGEO1744; Liu & Allan 2013 ERL http://dx.doi.org/10.1088/1748-9326/8/3/034002; Polson & Hegerl 2016 GRL http://onlinelibrary.wiley.com/doi/10.1002/2016GL071194/abstract) This observation can be explained by considering thermodynamic gradients (Byrne & O'Gorman, 2015 J. Clim http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-15-0369.1) and that dry seasons can exhibit negative P-E (Kumar et al. 2015 GRL http://onlinelibrary.wiley.com/doi/10.1002/2015GL066858/abstract) - see 8.2.2.1 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Paragraph modified.
26873	59	3	59	4	This paragraph and the previous one refer to chapter 8. The material between the two chapters shold be revisited to avoid too much overlap and provide condensed and well focussed assessements in the two chapters. [Eric Brun, France]	Accepted. Paragraphs are re-written accordingly.
79775	59	4	59	4	I suggest to also mention/refer to the illustrative examples of section 10.4.2 and 10.6 [Laurent Terray, France]	Accepted
53039	59	4			Section 8.2.2 [Hervé Douville, France]	Accepted. Included the subsection details in the text.
53041	59	16			See also Box 8.2 [Hervé Douville, France]	Accepted
53043	59	19	59	20	What about assessing such changes in the large ICE , as done for tas in Fig. 4.23? This would be consistent but not redundant with CH8 which emphasizes that precipitation variability can increase more strongly than mean precipitation but does not use the large ICE to support this statement. [Hervé Douville, France]	Taken into account. Large ICE are shown for temperature assessment. For precipitation assessment we can refer to Chapter-8.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23261	59	19	59	20	Suggest to note that precipitation variability would increase across different timescales. Suggest to modify the statement as: "Precipitation variability is projected to increase over a majority of global land area over a wide range of timescales in response to warming (Pendergrass et al., 2017)." [Wenxia Zhang, China]	Accepted. Modified the sentence
23263	59	22	59	28	Would it be useful to briefly compare the projection uncertainty between CMIP6 and CMIP5, and to note the possible role of the wider range of climate sensitivity in CMIP6 than CMIP5? This is discussed in Lehner et al. (2020). [Lehner, F., Deser, C., Maher, N., Marotzke, J., Fischer, E. M., Brunner, L., Knutti, R., and Hawkins, E.: Partitioning climate projection uncertainty with multiple large ensembles and CMIP5/6, Earth Syst. Dynam., 11, 491–508] [Wenxia Zhang, China]	Accepted. Discussed in Section 4.4.1.3
6667	59	23	59	23	"exceeds" should be "exceeding" [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
87589	59	23	59	23	exceeds → exceeding [Valentina Roberta Barletta, Denmark]	Accepted
102989	59	23	59	23	exceeds → exceeding [Philippe Tulkens, Belgium]	Accepted
127565	59	23	59	23	"with change exceeds" (grammar error). [Trigg Talley, United States of America]	Accepted. Corrected the text.
50901	59	23	59	23	Suggested edit: 'with change exceeding..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text.
6669	59	26	59	26	"in the CMIP6" should be "by the CMIP6". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
4127	59	27			Change "and high latitudes" to "and at high latitudes" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected accordingly.
84277	59	31	59	40	fig 4.27 partially overlaps with fig 8.31. A possibility to avoid overlapping is to have annual means in fig 4.27 and seasonal means in fig 8.31 [Annalisa Cherchi, Italy]	Rejected. Based on FOD comments and also to maintain consistency in Chapter-4 assessment , we would like to retain seasonal patterns of precipitation change.
84279	59	31	59	40	labels above panels to be adjusted; caption reports annual mean but labels indicate seasonal means [Annalisa Cherchi, Italy]	Accepted. Figure caption is modified.
87591	59	33	59	38	The caption is different from that at page 169. This one is correct. Always remember DJF, JJA. [Valentina Roberta Barletta, Denmark]	Accepted. Figure caption is modified.
102991	59	33	59	38	The caption is different from that at page 169. This one is correct. Always remember DJF, JJA. [Philippe Tulkens, Belgium]	Accepted. Figure caption is modified.
53045	59	33			Overlap with Fig. 8.15. CH4 could show precipitation changes in mm/day and CH8 in % for the sake of complementarity? [Hervé Douville, France]	Rejected. Percentage change in precipitation will give better representation of the change rather than showing actual values.
87593	59	43	59	48	These conclusions are clear, but the previous discussion is difficult to follow, it makes the conclusion seem not very logically related to the previous long discussion. [Valentina Roberta Barletta, Denmark]	Taken into account; text modified for greater clarity.
102993	59	43	59	48	These conclusions are clear, but the previous discussion is difficult to follow, it makes the conclusion seem not very logically related to the previous long discussion. [Philippe Tulkens, Belgium]	Taken into account. Text modified for greater clarity.
50903	59	47	59	47	large parts of themonsoon regions..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4129	59	47	59	48	Several grammatical errors here. Change "of monsoon region" to "of the monsoon regions"; change "and high latitude" to "and high latitudes"; change "in Mediterranean" to "in the Mediterranean". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Modified the text.
84281	59	53	59	53	"global monsoon", the word monsoon should be singular (it contains the regional monsoons) [Annalisa Cherchi, Italy]	Accepted. Corrected the text.
84283	60	2	60	4	issues about regional monsoons (i.e. onset dates) could be given just in ch 8 (no need to repeat them here) [Annalisa Cherchi, Italy]	Accepted. Deleted the text
83851	60	2	60	4	Citation Missing: Sabeerali and Ajayamohan (2017) Full Reference: C. T. Sabeerali and R. S. Ajayamohan, 2017, On the shorteening of Indian Summer Monsoon in a warming scenario, Climate Dynamics, doi: 10.1007/s00382-017-3709-7 [Ajaya Mohan Ravindran, United Arab Emirates]	Not applicable. Suggest to cite in ch 8, where the regional monsoons are assessed
106977	60	3	60	3	"Monsoon onset dates are likely to become earlier or not to change" does not mean anything to me. Is likely corresponding to the calibrated langage? [Christophe CASSOU, France]	Accepted. deleted
19887	60	10	60	11	Sentences such as the present one, beginning by " Since the AR5, there has been considerable progress..." or similar terms, are becoming familiar throughout this section 4.5: examples are found on P56 lines 52-54, or P58 lines 6-7 and 47-48. Very well. In the case of precipitation however, figure 3.10 illustrates how the climate models pain to get rid of a spurious double ICTZ. Both for near-term and long term, there is no indication in the present chapter that the situation has improved for projections. Therefore, some humility remains recommended! [philippe waldteufel, France]	Taken into account, sentence rephrased.
87595	60	10	60	24	These lines, and even some of the previous ones, are a summary of AR5 results. The focus should be to express the state of the art prediction based on what is curretly known, both from AR5 and older and new works. Some of the sections, like here, seems to focus on different assessments/works' conclusions, without harmonizing them. [Valentina Roberta Barletta, Denmark]	Taken into account, text modified and assessment deepened.
102995	60	10	60	24	These lines, and even some of the previous ones, are a summary of AR5 results. The focus should be to express the state of the art prediction based on what is currently known, both from AR5 and older and new works. Some of the sections, like here, seems to focus on different assessments/works' conclusions, without harmonizing them. [Philippe Tulkens, Belgium]	Taken into account, text modified and assessment deepened.
6671	60	11	60	12	This sentence needs attention. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, sentence rephrased.
4131	60	12			Sentence contains two projects/projections. Suggesting changing wording to, "...for simulating GM properties projects that under the RCP4.5 scenario the NH monsoon precipitation will increase much larger than its SH counterpart..." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text.
127567	60	13	60	13	"increase much larger" (grammar error). Change to "increase more strongly". [Trigg Talley, United States of America]	Accepted, Corrected the text

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4133	60	13			Change "due to increase" to "due to an increase" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text.
111417	60	14	60	15	Replace "against stabilization of troposphere" with "countered by stabilization of the troposphere" [James Renwick, New Zealand]	Accepted, Corrected the text
4135	60	15	60	16	Change "plays more important role on regional differences" to "plays a more important role in regional differences" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text.
53047	60	17	60	18	, but may still contribute to offset a significant fraction of the thermodynamic component of precipitation change (Sooraj et al., 2014). [Hervé Douville, France]	Accepted. Corrected the text. Reference is needed
83853	60	17	60	18	Doesn't fit the context. Discussion here is on global monsoons. Suggested to move the Asian monsoon part to the relevant chapter (Section 8.4.2.4.1) [Ajaya Mohan Ravindran, United Arab Emirates]	Accepted. Deleted the text
23265	60	17	60	18	Suggest to provide a brief physical explanation in this sentence as: "In the Asian monsoon regions, the monsoon circulation slows down at a much lower rate than in the other monsoon regions, associated with the opposite changes in the meridional temperature gradient in the lower and upper troposphere (Endo and Kitoh, 2014; Endo et al. 2018)." [Endo H, Kitoh A, Ueda H. A unique feature of the Asian summer monsoon response to global warming: The role of different land–sea thermal contrast change between the lower and upper troposphere[J]. Sola, 2018, 14: 57-63.] [Wenxia Zhang, China]	Taken into account, sentence rephrased.
64849	60	18	60	18	To explore a mechanism of the unique response of the Asian monsoon circulation, i.e., a weaker slowing down of the atmospheric circulation, Endo et al. (2018) analyzed AMIP-type experiments with CMIP5 multi-models and indicated that CO2-induced enhancement of the land-sea thermal contrast and the resultant atmospheric circulation changes are the most influential in the South Asian monsoon among various regional monsoons, which suggests an important role of the land warming on the Asian monsoon response to global warming. Therefore, I would like to suggest that this paper could be cited here like the following. “, which is associated with a Eurasian continent warming and the resultant atmospheric circulation changes (Endo et al., 2018).” Endo, H., A. Kitoh, and H. Ueda, 2018: A unique feature of the Asian summer monsoon response to global warming: The role of different land-sea thermal contrast change between the lower and upper troposphere. SOLA, 14, 57–63, doi:10.2151/sola.2018-010. [Hirokazu Endo, Japan]	Taken into account. move the Asian monsoon part to the relevant chapter (Section 8.4.2.4.1) as suggested by other reviewers
23267	60	20	60	22	This statement is also supported by recent literatures based on CMIP5 and CMIP6, e.g., Zhang et al. (2019) and Chen et al. (2020). [Zhang W, Zhou T, Zhang L, et al. Future Intensification of the Water Cycle with an Enhanced Annual Cycle over Global Land Monsoon Regions[J]. Journal of Climate, 2019, 32(17): 5437-5452. /// Chen Z, Zhou T, Zhang L, et al. Global land monsoon precipitation changes in CMIP6 projections. Geophysical Research Letters, 2020, submitted.] [Wenxia Zhang, China]	Accepted. References added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4137	60	22			It would be clearer to write "but is partly offset by reduced convergence" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text
23271	60	24	60	24	Apart from the seasonal mean monsoon rainfall, the seasonality of global monsoon rainfall is projected to enhance in response to warming, featuring a greater wet-dry season contrast (Lee and Wang 2014; Zhang et al. 2019). It would be useful to note the projected changes in monsoon seasonality. [Lee, J. Y., and B. Wang, 2014: Future change of global monsoon in the CMIP5. Climate Dyn., 42, 101–119. /// Zhang W, Zhou T, Zhang L, et al. Future Intensification of the Water Cycle with an Enhanced Annual Cycle over Global Land Monsoon Regions[J]. Journal of Climate, 2019, 32(17): 5437-5452.] [Wenxia Zhang, China]	Accepted. Corrected the text. References added
4139	60	24			Has El Nino been used as shorthand for ENSO, or does the finding here only apply to the warm phase of ENSO? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Corrected to "ENSO".
4141	60	26			Suggesting rephrasing this to, "…it is also suggested that the sensitivity of land monsoon precipitation to warming increases slightly…" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text
4143	60	27			Change, "no sensitivity" to "no such sensitivity" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text
84285	60	32	60	32	no need to specify "aggregated over all monsoon systems" [Annalisa Cherchi, Italy]	Accepted. Corrected the text
4145	60	33	60	34	The sentence here should also refer back to the near-term monsoon discussion in Section 4.4.1.4. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It does so by referring to the figure.
104665	60	33	60	34	The stated stronger increase in monsoon precipitation over 2021–2040 for SSP1-2.6 than for SSP5-8.5 strikes one as counterintuitive. Could it at least partly be due to considering different ensembles of models between the two scenarios? In any case, should 2021-2040 changes even be discussed in section 4.5? [William Merryfield, Canada]	Accepted. Deleted the text in near term projection
50905	60	36	60	36	precipitation index is projected..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text
4147	60	36			Change "precipitation index projected" to "precipitation index is projected" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text
127569	60	41	60	41	Sentence needs work (subject, verb, object). [Trigg Talley, United States of America]	Take into account, sentence rephrased.
4149	60	41	60	42	Several grammatical errors here. Change "patterns in monsoon rainfall" to "patterns of monsoon rainfall"; change "the north-south asymmetry" to "a north-south asymmetry"; change "more increase" to "greater increase"; change "the east-west asymmetry" to "an east-west asymmetry". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected the text

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4151	60	41	60	44	Another relevant study pertaining to longitudinal asymmetry, but in the near/mid-term to 2050, is that of Wilcox et al. (2019) "Accelerated increases in global and Asian summer monsoon precipitation from future aerosol reductions", https://doi.org/10.5194/acp-2019-1188. The large diversity in aerosols emissions pathways over Asia gives rise to very different sulphate loadings over India and China. Depending on the SSP chosen, there are different trajectories for the monsoon. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added.
53049	60	41			no pattern here, just timeseries? [Hervé Douville, France]	Taken into account, sentence rephrased.
23273	60	43	60	44	The projected weakened North American monsoon is explained in Pascale et al. (2017), suggesting the role of regional sea surface warming pattern. I suggest to cite this paper in this sentence. [Pascale S, Boos W R, Bordoni S, et al. Weakening of the North American monsoon with global warming[J]. Nature Climate Change, 2017, 7(11): 806-812.] [Wenxia Zhang, China]	Accepted. Reference added
83855	60	44	60	44	Typo, Wang et al. 9999a, 9999b [Ajaya Mohan Ravindran, United Arab Emirates]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
21673	60	44	60	44	Again, this section in contrast to almost all others ends without some form of new summary assessment finding. [Peter Thorne, Ireland]	Accepted. New summary assessment finding is added
79777	60	49	60	53	I suggest also to refer to the blocking and storm-track model assessment done in section 10.3.3.4 [Laurent Terray, France]	Accepted, the text now includes a reference to section 10.3.3.4.1
12241	61	1	61	9	Given these conclusions it would be far better to show zonal averages, perhaps separated for land and sea, instead of the hard to interpret Fig. 4.28 [Bryan Weare, United States of America]	Taken into account - The text has been revised to better describe the regional SLP changes.
106979	61	1	61	9	There is no literature assessed for SLP in this section even if there are a lot of studies since AR5 which investigated the changes in circulations. No confidence statement about the expected changes in SLP are provided. This subsection needs to be considerably improved. [Christophe CASSOU, France]	Taken into account. The section is expanded with more references and a better description of the spatial pattern in the SLP response and its links to precipitation changes.
74351	61	2	61	3	I would like to suggest to add the relationship with increasing or decreasing the sea level pressure with the pattern of projection in precipitation [Yulizar Yulizar, Indonesia]	Taken into account. The section is rephrased and the links with the spatial pattern in precipitation changes highlighted.
53051	61	7	61	9	No stippling => low confidence? [Hervé Douville, France]	Taken into account. The lack of stippling in SSP5-2.6 is considered in the assessment of the confidence in the projections.
111419	61	8	61	9	Replace "in the absence of a larger global warming signal" with "which is projected to over-ride the relatively weak global warming signal" [James Renwick, New Zealand]	Taken into account. The section is substantially rephrased
87597	61	14	61	19	Caption is different from the one at page 171. [Valentina Roberta Barletta, Denmark]	Taken into account. The captions have been made consistent
102997	61	14	61	19	Caption is different from the one at page 171. [Philippe Tulkens, Belgium]	Taken into account. The captions have been made consistent

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106981	61	24			Does it make sense to show the changes in zonal mean wind in annual mean and not by season, all the more that most of the text and following figures describes seasonal changes and clearly state that annual mean may masks the patterns of changes? I would provide the DJF and JJA season on the plot, instead of the annual mean. [Christophe CASSOU, France]	Taken into account. In the revised version of the section this option is considered.
111421	61	25	62	20	Some inconsistencies with Chapter 8, section 8.4.2.8. For the Northern Hemisphere, it would be good to mention the decreased surface N-S temperature gradient vs the increased tropopause-level gradient. [James Renwick, New Zealand]	Taken into account. In the revised version of the section this option is considered.
12243	61	30	61	41	Given this text says that the annual means mask the changes, why not redo Fig. 4.29 to be DJF and JJA? Again the hatching in Fig. 4.29 completely obscurs the imporant features. [Bryan Weare, United States of America]	Taken into account. In the revised version of the section this option is considered.
53053	61	30			Figure 4.29 could rather show DJF and JJA? [Hervé Douville, France]	Taken into account. In the revised version of the section this option is considered.
87599	61	36	61	36	Is this conclusion based on a single work not yet published? [Valentina Roberta Barletta, Denmark]	Taken into account. Additional references to support this statement have been added. Moreover the study is now published.
102999	61	36	61	36	Is this conclusion based on a single work not yet published? [Philippe Tulkens, Belgium]	Taken into account. Additional references to support this statement have been added. Moreover the study is now published.
87601	61	46	61	51	Caption is different from the one at page 171. [Valentina Roberta Barletta, Denmark]	Taken into account. The captions have been made consistent
103001	61	46	61	51	Caption is different from the one at page 171. [Philippe Tulkens, Belgium]	Taken into account. The captions have been made consistent
52225	62	1	62	20	for the North Atlantic in winter, the tendency is for a squeezing of the jet rather than a shift. See Peings (2018) (https://doi.org/10.1088/1748-9326/aacc79) and Oudar (2020) (https://doi.org/10.1029/2019GL086695). Also, this section should mention the tug-of-war between tropical and polar influences as at page 32 [Fabiano Federico, Italy]	Taken into account. In the revised version of the section this option is considered.
132499	62	1	62	20	I was expecting to see an assessment of projected changes in SH storm track here, and the relative roles of greenhouse-induced-warming and ozone recovery. This comes later in the section on SAM, but are these two sections really separable like this? At the very least, perhaps point readers to the SAM section (4.5.3). [Kyle Armour, United States of America]	Taken into account. In the revised version of the section this option is considered.
83857	62	2	62	2	NH Summer also shows a poleward shift of westerlies. Full Reference: S. Sandeep and R. S. Ajayamohan, 2015, Poleward shift in Indian summer monsoon low level jet stream under global warming, Climate Dynamics, doi: 10.1007/s00383-014-2261-y [Ajaya Mohan Ravindran, United Arab Emirates]	Rejected. This does not appear to be relevant to this section on extra-tropical circulation, though it would be appropriate to the section on the monsoon.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2165	62	13	62	13	An important omission is the role of tropical rainfall changes on the extratropical jets and storm track. E.g.: Scaife A.A. et al., 2017.Tropical Rainfall, Rossby Waves and Regional Winter Climate Predictions. Quart. J. Roy. Met. Soc., 143, 1-11. DOI: 10.1002/qj.2910. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paper has been considered in the overall revision of the section.
106983	62	17	62	20	The low confidence statement for the poleward shift of the wind in the Northern Hemisphere in winter is inconsistent with the high confidence statement about the changes of the NAM, because we know that both are physically connected. This is another reason to downgrade the confidence level of the NAM statement to "medium". [Christophe CASSOU, France]	Approved, the text has been modified to make the confidence statements consistent. The confidence on the NAM response has been reduced.
80753	62	22	62	38	This section is highly unbalanced btw reducing GHG emissions and CDR. The section argues more for the latter, starting from 2025, despite its low potential, its high cost and the unlikely scaling-up of existing technologies in the next 5 years. Reducing emissions is unfairly treated in the overall section. [Yamina Saheb, France]	Not applicable. This comment seems misplaced. We cannot identify what it refers to.
2167	62	33	62	33	This section is dependent on the models accurately simulating the split between internal and forced variability which may not be the case: Scaife A.A. and D. Smith, 2018. A Signal to Noise Paradox in Climate Science. Clim. Atm. Sci., 1, 28, 10.1038/s41612-018-0038-4. [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The impact of this issue on the assessed confidence of projected NAM/NAO changes has been considered in the revision.
53055	62	47			also Oudar et al. (2020, https://doi.org/10.1029/2019GL086695) based on both CMIP5 and CMIP6 models [Hervé Douville, France]	Taken into account. The reference has been included.
70901	62	51	62	51	Mindlin et al. is now published (2020): doi: 10.1007/s00382-020-05234-1 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
116337	62		62		The introduction to storylines is also done in chapter 1 (please check). What is missing here are examples of storylines, or aspects of ch 4 assessment that could be complemented by storylines. [Valerie Masson-Delmotte, France]	Taken into account. In the revised version of the section this is revised to be consistent with chapter 1.
18993	63	1	63	37	Extratropical cyclones are assessed extensively in chapter 11.7.2 this should be referred here & also the conclusions put different emphasis which should be carefully checked for consitensy. I was surprised to find this section here. It might not be necessary. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text in the two chapters has been better coordinated in the revision aiming for more consistency and complementarity between the two chapters.
87603	63	1	63	37	this section mostly summarizes the CMIP5 results, the new CMIP6 model results are only mentioned in two lines. Is the contribution from those model so irrelevant for the extratropical cyclones future evolution? [Valentina Roberta Barletta, Denmark]	Noted. Not many CMIP6 studies on circulation change were available at the time of writing of the SOD. The relevant studies are now included in its revision.
103003	63	1	63	37	this section mostly summarizes the CMIP5 results, the new CMIP6 model results are only mentioned in two lines. Is the contribution from those model so irrelevant for the extratropical cyclones future evolution? [Philippe Tulkens, Belgium]	Noted. Not many CMIP6 studies on circulation change were available at the time of writing of the SOD. The relevant studies are now included in its revision.
53057	63	9	63	10	see also Section 8.4.2.8 [Hervé Douville, France]	Taken into account. The two sections have been better coordinated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11255	63	13	63	14	The citation should be Chang, 2018 instead of Kar-Man Chang, 2018 [Edmund Kar-Man Chang, United States of America]	Accepted. Reference changed
42949	63	15	63	16	Bomb cyclone deepening rates display strong dependence on horizontal model resolution. Influence of Model Resolution on Bomb Cyclones Revealed by HighResMIP-PRIMAVERA Simulations" by Jiaxiang, Gao et al. Accepted ERL [Rein Haarsma, Netherlands]	Taken into account. This paper has been cited in the revision.
42951	63	20	63	24	HighResMIP similations provide extra insight. For example Gao et al: Influence of Model Resolution on Bomb Cyclones Revealed by HighResMIP-PRIMAVERA Simulations" by Jiaxiang, Gao et al. Accepted ERL [Rein Haarsma, Netherlands]	Taken into account. The impact of model resolution on cyclones has been discussed in the revised text.
104667	63	27	63	28	It's stated that "CMIP5 models indicate that the frequency of intense ETCs will increase in the SH (Chang, 2017). The wind speeds associated with ETCs are therefore expected to intensify..." However it does not necessarily follow that wind speeds associated with ETCs intensify, if the frequency of less intense ETCs were to increase faster than the fequency on intense ETCs for example. Therefore recommended deleting "therefore" in this sentence. [William Merryfield, Canada]	Accepted.
19889	63	35	63	36	There is indeed a simple and strong argument favouring an increase in the frequency of rainy storms, while the arguments relative to other properties may be strong but are less straightforward. Still, the poleward move illustrated on figure 4.31 seems also based on a simple and strong argument. [philippe waldteufel, France]	Noted. While the increase in rainfall is based on strong physical arguments, the poleward shift of storm tracks remains partly based on heuristic arguments and affected by larger uncertainties across models.
52229	64	3	64	7	The Scandinavian Blocking regime shows a decreasing trend in ssp585 (robust) and ssp370 (medium) simulations, see Fabiano et al. (9999) as in comment 1. This is consistent with the decrease of winter blocking over Europe. [Fabiano Federico, Italy]	Rejected. The manuscript suggested wasn't accepted by the cut off deadline
10975	64	8	64	8	Should this be 'projected decrease'? [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Agreed - Corrected
4173	64	16	64	20	The results of Kennedy et al. (2016) are dependent on model and the intensity of perturbation experiments. For example, the tropical pertubation in their experiments is excessively strong while the dominating warming occurs in the Arctic. Moreover, their conclusions are focus on East Atlantic-Euro blocking while there is a increasing blocking frequency over BDL sea and Euro-Asia continent in their experiments. Hence, the author may display more views of the blocking response to warming climate. For example, Barens and Screen (2015, "The impact of Arctic warming on the midlatitude jet-stream: Can it? Has it? Will it?") elaborated three distinct questions about Arctic affecting jet activity. Results of Luo et al. (2018, "Changes in Atmospheric Blocking Circulations Linked with Winter Arctic Warming: A New Perspective"; 2019, "Weakened Potential Vorticity Barrier Linked to Recent Winter Arctic Sea Ice Loss and Midlatitude Cold Extreme") indicated the Arctic warming can recede the background PV gradient to modulate the regional blocking lifespan and frequency. [Wenqi Zhang, China]	Rejected. The topics highlighted are part of the discussion in Box 10.1, which is referred here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132501	64	19	64	20	Ithink more needs to be said here about why this is controversial, and about your assessment of the potential for Arctic warming to increase blocking frequency at mid-laitudes. Is there an assessment on this? Readers will wonder. [Kyle Armour, United States of America]	Rejected. This is extensively discussed, as referred, in Cross-Chapter Box 10.1
12247	64	23	64	24	"shows a remarkable decrease" should be something like "suggests a decrease" given that the error bars overlap. [Bryan Weare, United States of America]	Rejected. The decrease is clear and the negative trend exhibited by the models over the period 1950-2100 is significant at 5% confidence level.
106985	64	24	64	27	I would state "low confidence" instead of "at most medium" which is not clear. I would add results from HighResMIP scenario if possible in Fig. 4.32. I think that resolution is an important matter when assessing blocking changes. [Christophe CASSOU, France]	Taken into account - The statement has been revised.
67849	64	25	64	27	The paragraph refers to the high emission sceanrio (RCP 8,5). Is there any research/reference related to low or medium emission scenario? A full set of scenario may lead to a throough interpretion of the effect of ocean acidification. [Ruandha Agung Sugardiman, Indonesia]	Rejected. Figure 4.32 shows four different SSPs.
7249	64	25	64	27	The paragraph refers to the high emission sceanrio (RCP 8,5). Is there any research/reference that related to the low or medium emission scenario? A full set of scenario may lead to a through interpretion of the effect of Ocean acidifications. [Asaad Irawan, Indonesia]	Rejected. Figure 4.32 shows four different SSPs.
53059	64	32			Figure 4.32: For the sake of consistency with CH3, what about using the same definition as in Fig. 3.16 and the whole longitudinal distribution? This would also allow to document a possible eastward shift in the winter blocking activity in the NH. [Hervé Douville, France]	Rejected. The blocking index used to compute this figure is the same as that used to produce figure 3.16 (i.e. Davini and D'Andrea 2020). Figure 4.32 provides a clear picture of the 1) model bias 2) changes in blocking frequencies in 4 different SSP scenarios.
87605	64	33	64	34	This caption is correct, the one at page 174 has the (a) and (b) reference in the caption wrong. [Valentina Roberta Barletta, Denmark]	accepted. Corrected
103005	64	33	64	34	This caption is correct, the one at page 174 has the (a) and (b) reference in the caption wrong. [Philippe Tulkens, Belgium]	Noted
18041	64	44	64	44	For Section 4.5.2 Please consider introducing a section on Deoxygenation. Oxygen loss belongs alongside the discussion of temperature and acidification including for deep waters. [Lisa Levin, United States of America]	Rejected. Deoxygenation is assessed in Chapter 5 (section 5.3.3.2). We decide not to include deoxygenation here.
71943	64	44	65	28	No Figures for this section - suggest at least need a zonal average picture of warming and acidification. [John Church, Australia]	Taken into account. Figures for zonal mean ocean pH are added. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.
21681	64	44			This section is very very distinct from all other parts of 4.5 and significant efforts to reorientate this text to better jive with remaining parts of section 4.5 would be hugely beneficial. It just looks like ocean has been utterly hollowed out and right now the chapter would be better saying nothing. [Peter Thorne, Ireland]	Taken into account. This section is re-written.
130495	64	46	64	46	Please consider change "ocean temperatures" to "ocean heat content". [Panmao Zhai, China]	Taken into account. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21677	64	46			Section lacks an opening statement characterising AR5 / SROCC findings [Peter Thorne, Ireland]	Taken into account. This section is re-written. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.
69919	64	47	64	54	Figure 4.33: There is no prior evidence in the text for the uneven behavior of NAM during JJA. The explanation of less positiveness of NAM as compared to other seasons in all the different scenarios would reinforce the understanding about the teleconnection in the Northern hemisphere and its linkage to other teleconnection as well. [SAHIL SHARMA, India]	Taken into account. Paragraph modified to arrive at a clearer assessment. (Refers to page 65, not 64).
19295	64	48	64	55	Why isn't there a stronger statement about ocean heat content? SROCC SPM says "it is virtually certain that the global ocean... and has taken up more than 90% of the excess heat ..." [Anne-Marie Treguier, France]	Taken into account. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap
68781	64	48	65	11	This section should acknowledge the work of Gebbie and Huybers 2019 whose work suggests that climate models are projecting higher levels of warming than what will actually occur because they are failing to account for deep ocean warmth that is actually warming the surface from below. They estimate that 25% of recent warming may be attributable to deep ocean warmth that is not accounted for in IPCC models. Gebbie and Huybers, “The Little Ice Age and 20th-century deep Pacific cooling” Science 363, 70–74 (2019).https://science.sciencemag.org/content/363/6422/70.full - Sean Rush, New Zealand [sean rush, New Zealand]	Rejected. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap
50907	64	48	65	11	Is there a reason why results for future ocean temperature rise are not provided here? Modelled/predicted results for ocean heat uptake would be valuable information to include. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.
132533	64	52	64	53	This statement is at odds with Chapter 3 who state that this is extremely likely. However, even that seems at odds with Chapter 9 (page 22, lines 33-35) who state that it is virtually certain that anthropogenic forcing has caused the observed increase in OHC in the upper and intermediate ocean (where most of the OHC changes are observed). [Kyle Armour, United States of America]	Taken into account. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.
104669	64	55	64	55	It appears that Section 9.2.3.3 should in fact be Section 9.2.1.4. [William Merryfield, Canada]	Taken into account. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.
21679	65	1	65	12	Section text is completely out of keeping with remaining subsections of 4.5. There is discussion of processes and even of idealized experiments. But there is zero analysis of the scenarios and no discussion of medium to long-term projections. Section needs to be completely rewritten to instead focus upon these for intra-chapter consistency. Present text arguably better belongs in chapter 9. [Peter Thorne, Ireland]	Taken into account. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132503	65	2	65	2	This would better read "heat uptake appears to track what one would expect from the uptake of a passive tracer, with the exception of the North Atlantic Ocean where AMOC changes play an important role" [Kyle Armour, United States of America]	Taken into account. This section is rewritten. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap.
87607	65	6	65	6	diapycnically → dyapycnally ? [Valentina Roberta Barletta, Denmark]	Taken into account. This section is rewritten.
103007	65	6	65	6	diapycnically → dyapycnally ? [Philippe Tulkens, Belgium]	Taken into account. This section is rewritten.
21685	65	15			This section as written has no projection component at all so is out of scope of the section. [Peter Thorne, Ireland]	Taken into account. Figures for zonal mean ocean pH are added. Projections of long-term ocean thermal properties are assessed comprehensively in Chapter 9, and are not covered here to avoid overlap
36411	65	17	65	28	This paragraph should include some discusion the Arctic Ocean which is decreasing in aragonite saturation state more rapidly than the Southern Ocean (see Steinacher et al., 2009; Zhang et al., 2020). [Adrienne Sutton, United States of America]	Taken into account. Discussion on Arctic ocean acidification has been added.
30597	65	17	65	28	This paragraph should include some discusion the Arctic Ocean which is decreasing in aragonite saturation state more rapidly than the Southern Ocean (see Steinacher et al., 2009; Zhang et al., 2020). [nina bednarsek, United States of America]	Taken into account. Discussion on Arctic ocean acidification has been added.
50909	65	17	65	28	It would be helpful if projected ocean acidification change under different scenarios could be included here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Zonal mean figure of ocean pH change has been added.
6673	65	18	65	18	What is meant by the words "climate change" in this line. If "climate change" means the change in the variables that define the Earth's climatic state, then ocean acidification will depend on future climate change, particularly the change in concentration of carbon dioxide. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section is rewritten.
18043	65	19	65	20	Please make sure that your use of the term deep ocean in multiple places is consistent with the definition of deep ocean given earlier as 2000-6000 m. Waters from 700-2000 m are called intermediate. [Lisa Levin, United States of America]	Taken into account. Text is revised.
84287	65	31	69	44	section 4.5.3 has some repetitions with section 4.4.3: some statements at the beginning of subsections are somehow repeated. Distinction between near term and then mid-long term is maybe too subtle for this topic and available related literature [Annalisa Cherchi, Italy]	Taken into account - The Section is revised ad overlaps with section 4.4.3 and other chapter minimised.
107003	65	31			Like for the near term section, I would combine AMV and PDV in one "decadal modes and teleconnection" section and IOB/IOD/Atl modes in one "Tropical modes and teleconnection". Again the assessment of teleconnection is clearly a gap in the current version and implications for regional climate should be also better emphasized. [Christophe CASSOU, France]	Taken into account. As for section 4.4.3 the revised version of 4.5.3 is designed to minimise overlaps and include overlooked material.
127571	65	38	65	38	[PROGRESS] Add a few sentences summarizing whether AR6 knowledge and/or confidence levels differ markedly from AR5, and, if so, how and why. [Trigg Talley, United States of America]	Rejected. This summary can be found in section 4.3.3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19891	65	38	65	42	Please refer to projections shown on figure 4.7 and comment. While they seem compatible, figure 4.33 and this section propose a rather different picture for the SAM. [philippe waldteufel, France]	Noted. Figure 7 shows yearly SAM values for DJF whereas Figure 4.33 shows 20-year mean anomalies for 2081-2100. Hence the uncertainty ranges in Fig 4.33 are smaller than in Fig 4.7.
106987	65	38			See ealier comment on NAM definition which should be corrected. See earlier comment on the level of confidence. The latter is missing here for near-term anyway and should be "medium". [Christophe CASSOU, France]	Not applicable. The NAM definition is consistent with the Technical Annex MOV.
87609	65	40	65	40	NAM → Northern Annular Mode [Valentina Roberta Barletta, Denmark]	Noted. NAM has been defined in 4.3.3 and it is used in 4.4.3.
103009	65	40	65	40	NAM → Northern Annular Mode [Philippe Tulkens, Belgium]	Noted. NAM has been defined in 4.3.3 and it is used in 4.4.3.
106991	65	40			Changes in teleconnections are not assessed at all for NAM/NAO in the chapter but are assessed in TS (figure TS35). This is a gap because there is litterature addressing this issue showing that teleconnections over Europe are reduced because of the reduction of snow-ice feedback in winter. As stated for ENSO, teleconnections at large should be assessed as much as possible because those can be interpreted as the transfer function from large-scale to regional changes. They are also very important to better understand the nature of uncertainties at regional scale. [Christophe CASSOU, France]	Rejected. Teleconnections for MOV are not assessed and are beyond the scope of the chapter.
52227	65	47	65	54	As in comment 1: robust positive trend in NAO+ in ssp585, see Fabiano et al. (9999) [Fabiano Federico, Italy]	Taken into account. Paragraph modified to arrive at a clearer assessment.
2169	65	53	65	53	It might be good to frame the changes in NAM and SAM by also quoting interannual standard deviation [Adam Scaife, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Where hPa NAM/SAM changes are given in the text these are compared to the interannual standard deviations.
87611	66	1	66	1	SAM → Southern Annular Mode [Valentina Roberta Barletta, Denmark]	Noted. SAM has been defined in 4.3.3 and it is used in 4.4.3.
103011	66	1	66	1	SAM → Southern Annular Mode [Philippe Tulkens, Belgium]	Noted. SAM has been defined in 4.3.3 and it is used in 4.4.3.
87613	66	30	66	30	There is no clear statement about the prediction and confidence level for NAM and SAM. [Valentina Roberta Barletta, Denmark]	Taken into account. Statement included
103013	66	30	66	30	There is no clear statement about the prediction and confidence level for NAM and SAM. [Philippe Tulkens, Belgium]	Taken into account. Statement included
87615	66	35	66	40	The caption is not very informative. The colors (models) are not mentioned, and it does not provide any relevant information on what the graph tells. [Valentina Roberta Barletta, Denmark]	Taken into account and corrected
103015	66	35	66	40	The caption is not very informative. The colors (models) are not mentioned, and it does not provide any relevant information on what the graph tells. [Philippe Tulkens, Belgium]	Accepted. Corrected
53061	66	35			Figure 4.33: Also show (grey bars) an estimate of internal variability only using piControl simulations and add the legend for SSPs? [Hervé Douville, France]	Taken into account. This has not been added to Fig. 4.33 to avoid it becoming overcrowded, but where hPa NAM/SAM changes are given in the text these are compared to the interannual standard deviations.
53063	66	45			could deserve one or two figures for the sake of consistency with CH3 (cf. for instance parallel to Fig.3.35a-b and Fig.3.36a but showing historical versus future under SSP8.5)? [Hervé Douville, France]	Rejected. Fig 4.8 and Fig 4.17 already show Nino3.4 and Nino precipitation indices over the 21st century.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106989	66	45			Changes in teleconnections are not assessed at all in this section devoted to ENSO even if the word appears in the title of the section. There are a lot of literature about this issue and an entire subsection was provided in AR5 (shift of the Aleutian Low etc.). This is clearly a gap in SOD. I would suggest to add the Drouard and Cassou (2019, https://doi.org/10.1175/JCLI-D-18-0803.1) paper and check herein for relevant references. Changes in ENSO teleconnection is extremely relevant for regional chapters and is accordingly addressed in Technical Summary. [Christophe CASSOU, France]	Taken into account. A paragraph on teleconnections has been added, but "teleconnections" removed from subsection title not to create false expectations. Length limitations preclude deeper assessment.
42721	67	3			Figure 4.16 appears to be incorrect – should it be figure 4.8? [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
53065	67	8			ENSO characteristics (e.g., spatial structure, frequency, amplitude, but also seasonality and life cycle) [Hervé Douville, France]	Taken into account; text has been re-structured.
42953	67	31	67	33	These sentence do not add much information to the near section paragraph. [Rein Haarsma, Netherlands]	Rejected. This comment appears to refer to another section than stated.
106995	67	47	67	47	Cross reference to Chap3 should be used for model evaluation of the PDV. [Christophe CASSOU, France]	Accepted. - Reference to Chapter 3 Added
12249	68	1	70	47	Given the lack of figures and significant conclusions, this section shoud be drastically shortened. [Bryan Weare, United States of America]	Taken into account - This section is revised to add overlooked material/citations and shortened when advisable.
53067	68	3	68	4	also Geng 2019 [Hervé Douville, France]	Accepted -Added reference
106997	68	6	68	6	"More frequent" should be replaced by "higher frequency" [Christophe CASSOU, France]	Accepted Changed as suggested
106993	68	10	68	13	Cross reference to Chap3 should be used instead of a specific paper to assess the human influence on PDV. [Christophe CASSOU, France]	Not applicable. The sentence has been removed.
132505	68	10	69	12	I'm having trouble understanding this sentence. Also, specify which century you are referring to. [Kyle Armour, United States of America]	Taken into account - the sentence was redundant and it has been removed.
53069	68	15	68	16	although there is growing evidence that global warming could shorten the PDV lifespan and suppress its amplitude in high-emission scenarios? [Hervé Douville, France]	Taken into account. Final statement changed accordingly.
127573	68	17			Some comments should be added related to the possible development of an Indian Ocean El Niño (DiNezio et al, 2020, 10.1126/sciadv.aay7684). [Trigg Talley, United States of America]	Rejected due to space limitations.
21691	68	19			This IOB/IOD segment felt a bit internally inconsistent and like it lacked a real synthesis. For example at one point it argues for a 'decrease in IOB frequency' but then states that no new studies suggest such a thing despite having two paragraphs earlier cited just such a study. Some efforts to better synthesise here would be advisable. [Peter Thorne, Ireland]	Taken into account. Inconsistency in the assessment of IOB/IOD has been improved and medium confidence is given for the projected changes in IOD
21687	68	37	68	38	I'm not sure this makes logical sense. How can a mode show decreased frequency? Given states of the mode could e.g. a decrease in frequency of magnitude of the positive phase but the mode itself is a continuum so is ever present? [Peter Thorne, Ireland]	Accepted. The statement is removed.
21689	68	42	68	43	Would surely be better to refer to other chapters assessment of ENSO change rather than a single paper here? [Peter Thorne, Ireland]	Taken into account. Paragraph modified.
42955	68	42	68	45	Is this not in contradiction with page 67 line 31-33?? [Rein Haarsma, Netherlands]	Accepted. The statement is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87617	68	48	68	48	A conclusion is drawn with “high confidence”, but all the discussion above is mostly based on old (prior 2015) works. [Valentina Roberta Barletta, Denmark]	Taken into account. Medium confidence is given to projected IOD changes
103017	68	48	68	48	A conclusion is drawn with “high confidence”, but all the discussion above is mostly based on old (prior 2015) works. [Philippe Tulkens, Belgium]	Taken into account. Medium confidence is given to projected IOD changes
45531	68	53	69	14	I think it is worth pointing out here that a recent paper (Mann et al., 2020, 10.1038/s41467-019-13823-w) showed that model simulations appear to insufficiently simulate much of the observed multidecadal climate variations, also AMV. This has profound consequences for projections of AMV using those models, and our understanding of forced vs. unforced North Atlantic climate variability. [Leonard Borchert, France]	Rejected. Model deficiency in simulating AMV are discussed in Chapter 3.7.7. Here we refer to this chapter section.
53071	68	54			"coming decades" are rather the topic of Section 4.4? [Hervé Douville, France]	Noted - rephrased to avoid confusion
105469	69	1	69	4	Model simulations over the last millenium give insight into the AMO/AMV variability seen in paleo-observations: similar variations to dense overflow water from the Nordic Seas (Lohmann et al., 2015, https://doi.org/10.5194/cp-11-203-2015). [Helene R. Langehaug, Norway]	Noted. Thank you. However the topic of the paper suggested is not very relevant here, this is relevant in chapter 2 and in the Modes of Variability Annex IV.
106999	69	10	69	10	It is not because the pattern of SST may not change that the teleconnection remains the same because of non-linearity of the processes at work. See ENSO teleconnections for instance whereas SST variability per se does not change. I would remove this sentence and I consider this as a knowledge gap. [Christophe CASSOU, France]	Accepted.- Agreed. The part of sentence including "teleconnections" has been removed
53073	69	10	69	14	May deserve a stronger coordination with CH9 to assess how the AMOC-AMV link may change with global warming (e.g. Hand 2020)? [Hervé Douville, France]	Taken into account. Consistency and coordination with chapter 9 is checked on this topic
53075	69	16	69	17	"AMV is unlikely to change in the future" (low confidence)? [Hervé Douville, France]	Accepted- low confidence
104671	69	24	69	24	The use of "likely" in its calibrated sense (i.e. italicised) does not appear to be appropriate in this sentence which refers to a technical or human rather than scientific issue. Recommend therefore replacing "likely" with "probably" to avoid any such ambiguity. [William Merryfield, Canada]	Accepted - Thank you. The text is changed following your suggestion
107001	69	24	69	24	Likely should be reserved for calibrated language. Misleading here and should be replaced. [Christophe CASSOU, France]	Accepted - Adverb replaced
7815	69	37	69	37	These references are not really suitable since they are talking about whether the AMOC has changed in the past. Better to refer to the earlier section showing weakening in CMIP6 models [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - References removed and section 4.3.2.3 is included.
45519	69	37	69	37	Omit the “(“ before “Robson et al”. [Leonard Borchert, France]	Not applicable - references removed.
87371	69	37	69	38	The reference to Caesar et al. (2018) and Robson et al. (2014a) are far from being the best since these authors do not much discuss the projections but present state of the AMOC. Bakker et al. (2016) might seem more appropriate for instance. (for complete references see IPCC SROCC chapter 6.7). [Didier Swingedouw, France]	Accepted - References removed and section 4.3.2.3 is included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106101	69	42	69	44	A recent multi-model study clearly concluded that the TA teleconnection to the Pacific will weaken under global warming, because of a stabalisation of the atmosphere. Jia, F., W. Cai, L. Wu, B. Gan, G. Wang, F. Kucharski, P. Chang, and N. Keenlyside, 2019: Weakening Atlantic Niño–Pacific connection under greenhouse warming. Science Advances, 5, eaax4111. [Noel Keenlyside, Norway]	Accepted. The suggested reference is considered.
114487	69	47	73	52	I got the feeling that you repeated a bit more than needed here, and tgat you could use more references to what is shown earlier in teh chapter [Jan Fuglestvedt, Norway]	Taken into account. Text has been revised.
106851	69	49			It could be interesting to address in section 4.6.1 the changes of the MoV and associated teleconnection as a function of warming levels. This could be done for ENSO, NAM and SAM. This would be very much relevant for policy makers and for the TS. [Christophe CASSOU, France]	Rejected. Please see the detailed discussion in section 4.6.1.3, which assesses that only a small fraction of the spread in the shift of the Northern and Southern hemisphere midlatitude circulation is explained by GSAT at a fixed time-horizon, which (in combination with several other factors) implies that an epoch analysis to assess midlatitude atmospheric circulation changes and related annular modes of variability is of limited value. Moreover, we assess in section 4.3.3.2 that there is no robust consensus in terms of change in the amplitude of ENSO during low mitigation 21st century futures. Consequently, we do not undertake an analysis of changes in ENSO as a function of levels of global warming.
21693	69	49			I wonder whether this description of approach should be explicit what the basis for assigning change since 'pre-industrial' is based upon. I am assuming it is using the chapter 2 assessed change between 1850-1900 and the modern reference period? Is it using the best estimate only or the range? And are model simulations then rebased to be this value at the modern reference period? For reproducibility and transparency I think these details need to be included in 4.6.1 in the FGD. [Peter Thorne, Ireland]	Not applicable. Firstly, no, section 4.6.1 does not make use of the Chapter 2 assessed range of temperature increase between 1850-1900 and the modern reference period. Rather, for each GCM projection that contribute to the 4.6.1 analysis, the emergence of a specific level of global warming is calculated relative to the relevant model simulation of 1850-1900 GSAT. The methodology is described in some detail in lines 20-37 on page 70 of the SOD. The FGD also refers the reader to cross-chapter box 11.1, in which the methodology to calculate the emergence of levels of global warming is demonstrated graphically.
11281	69		73		For assessing regional temp/precip/circulation changes, SST pattern change especially in the tropical Pacific would be one of crucial factors. Since AR4 (Fig. 10.16), AR5 did not assess the Pacific SST pattern change if it is more 'El Nino-like' or 'La Nina-like'. In AR6, I think it's important to discuss the SST pattern change in the projections, by referring to the historical changes, in a separate subsection in 4.6.1. [Masahiro Watanabe, Japan]	Rejected. The proposed modification does not fit into the systematics chosen by the authors.
127575	70	6	70	7	What thresholds? There should be a qualifier in front of this word. Even so, if the meaning is the temperature goal, then it should be stated as such. [Trigg Talley, United States of America]	Accepted. We have updated this sentence to make it clear that we are referring to the 1.5 degrees C and 2 degrees C levels of global warming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132507	70	9	70	37	This is better written than the discussion of pattern scaling on page 17. But a simple description of what is meant by "pattern scaling" would be welcom here. It is an important concept to convey for this section. Something along the lines of what is said on page 74, lines 16-19. [Kyle Armour, United States of America]	Rejected. The pattern scaling methodology we use is described in lines 20-25 on page 70 of the SOD, it is mentioned specifically that the time-shift approach is applied in this section. This methodology does not rely on linearity (the line 16-19 discussion on page 74 of the SOD does not apply to the time-shift method we apply in 4.6.1). Pattern scaling is discussed in section 4.2.4, as well as the various methodologies available to calculate such patterns (including the time-shift method). We are referring to this section in 4.6.1 and for space considerations will not repeat definitions and methodology discussions in any detail, in 4.6.1.
11403	70	10	70	12	“These include performing model simulations under stabilisation scenarios designed to achieve a specific level of global warming (e.g. Dosio et al., 2018; Kjellström et al., 2018; Mitchell et al., 2017), ...” These three studies are not based on stabilisation scenarios, but RCP scenarios (RCPs 4.5 and 8.5; 8.5; 2.6 and 4.5 respectively. What these studies do is that they look at certain warming levels when they appear in the used scenarios. More correct would be to describe them as analysis of epochs identified within transient simulations. [Strandberg Gustav, Sweden]	Accepted. These references are indeed for studies based on transient simulations, and are now being referred to as such in the FGD text.
7417	70	15	70	15	Seneviratne et al., 2018 citation, please indicate 2018a or 2018b [Geremy PANTHOU, France]	Accepted. Text updated to indicate that we are referring here to Seneviratne et al. (2018b).
11405	70	16	70	18	“These different methodologies are discussed in some detail in Section 4.2.5 (see also James et al., 2017) and generally provide qualitatively consistent results regarding changes in the spatial patterns of temperature and rainfall means and extremes at different levels of global warming.” I don’t find this discussion in 4.2.5. It should also be note that it’s complicated to look at extrems with these kinds of approaches since the occurrence of extremes within a SWL is dependent on the underlying scenario. See Lars Bärring and Gustav Strandberg 2018 Environ. Res. Lett.13 024029. [Strandberg Gustav, Sweden]	Rejected. The bulk of evidence indicates that regional changes in many types of extremes respond quasi-linearly with global mean temperature, generally irrespective of emission scenarios (also see Section 11.2.5 in Chapter 11; Cross-Chapter Box 11.1;). The argument of Barring and Strandberg (2018) is mostly based on pointing out that trends exist within transient scenarios, when multi-decadal periods centred around the emergence of a specific GWL are considered. However, when considering the average attributes of extremes for a GWL defined in this way, then those attributes scale quasi-linearly with the GWL (SR15 Ch3; Seneviratne et al., 2016; Wartenburger et al., 2017; Matthews et al., 2017; Tebaldi and Knutti 2018, Sun et al., 2018a, Kharin et al., 2018, Beusch et al., 2019).
96431	70	16			Methodologies as "pattern scaling" are described in section 4.2.4. Please verify reference to Section 4.2.5 mentioned here. [Nicole Wilke, Germany]	Accepted. The text has been changed to indicate that we are referring to 4.2.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106285	70	20	73	52	For consistency with the WG1 core set of scenarios it would be important to also include SSP1-1.9 values here. If less simulations are available for the scenario, this can be communicated as well and the implications of it for the comparability of results can be highlighted. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Whilst we appreciate the notion of extending the analysis to SSP1-1.9, the Chapter 4 and 11 methodology relies on the CMIP6 Tier 1 simulations only (these exclude SSP1-1.9). The reason for this is that all participating models in ScenarioMIP of CMIP6 are obliged to undertake the Tier 1 simulations, allowing for equal weighting in the pattern scaling analysis in terms of the models considered per SSP. Since there is extensive evidence that for the variables we undertake pattern scaling for the emission scenarios do not substantially impact on the patterns of change, our assessment is that including SSP1-1.9 in the analysis will not significantly change the findings.
6675	70	24	70	24	This explanation is not very clear. What does "the first year" refer to when discussing a plot of 20-year moving averages? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The explanation is in lines 20-23 on page 70. We first construct a time series of 20-year moving averages of GSAT, and the first year in this time series that exceeds a given temperature threshold is the year in question. We added a cross-chapter box (Cross-Chapter Box 11.1), in which this methodology is demonstrated graphically, and which should help to clear up any remaining confusion.
87619	70	34	70	37	The last sentence does not have the main verb. [Valentina Roberta Barletta, Denmark]	Rejected. The sentence is long but otherwise reads well.
103019	70	34	70	37	The last sentence does not have the main verb. [Philippe Tulkens, Belgium]	Rejected. The sentence is long but otherwise reads well.
11407	70	43	70	44	It is also important to acknowledge the differences between RCPs at the same SWL (Lars Bärring and Gustav Strandberg 2018 Environ. Res. Lett.13 024029.) [Strandberg Gustav, Sweden]	Rejected. The sensitivity of pattern scaling at different GWLs to different mitigation scenarios is discussed in the previous paragraph. Also see Cross-Chapter Box 11.1, in which the use of GWLs for pattern scaling analysis is discussed in the context of the climate response not being in equilibrium. It should also be noted that Barring and Strandberg (2018) largely focusses on discussing warm and cold extremes in the context of different GWLs, but the methodology used is different to that applied in Chapters 4 and 11. Barring and Strandberg point out that in transient scenarios trends exist in a multi-decadal period used to represent a specific GWL, and that these trends are stronger in low mitigation scenarios, thereby affecting the analysis of extremes. Chapters 4 and 11, however, calculate the average attributes of variables at GWLs, and show that these scale quasi-linearly with GWL, largely independent of RCP/SSP (Cross-Chapter Box 11.2; SR15 Ch3; Seneviratne et al., 2016; Wartenburger et al., 2017; Matthews et al., 2017; Tebaldi and Knutti 2018, Sun et al., 2018a, Kharin et al., 2018, Beusch et al., 2019).
132509	70	50	70	50	Does this section refer to GSAT or GMST? I think it is GSAT, but you should specify. [Kyle Armour, United States of America]	Rejected. See line 25 of page 70, which makes it clear that the analysis is GSAT-based.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19893	70	50	70	50	Section 4.6.1.1, actually, is concerned with surface temperature, as far as one can see [philippe waldteufel, France]	Rejected. See line 25 of page 70, which makes clear that the analysis of levels of global warming is based on GSAT.
12251	70	55			Given in Fig. 4.34 the relatively weak measure of robustness of two thirds of models having the same sign, one cannot say that there are "robust" increases over both land and sea [Bryan Weare, United States of America]	Accepted. There was an error in the Figure caption, and in fact robustness of the signal is defined in this section and Figures for regions where the multi-model mean change exceeds two standard deviations of pre-industrial internal variability, and where at least 90% of the models agree on the sign of change.
50911	71	3	71	3	projected for land regions' - please specify here that this means larger in comparison to ocean regions. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We are referring here to the increase with respect to the level of global (GSAT) warming, which clearly implies that land regions warm faster than oceanic regions - no reason to mention this explicitly.
107005	71	4	71	4	I would provide the figure of the amplifcation factor as a function of global warming [Christophe CASSOU, France]	Rejected. We appreciate the suggestion, but the spatial maps presented do provide a means of viewing the amplification factor for different regions for a specific level of global warming. Given that the Chapter is already exceeding its length limits, we have opted not to generate an additional graphic.
45521	71	12	71	12	Singular for “phenomena” is “phenomenon”. Since the word “phenomena” references one phenomenon here, it should be “phenomenon”. [Leonard Borchert, France]	Accepted - correction made.
132511	71	13	71	14	Be sure to cite Chapter 7 here as well. [Kyle Armour, United States of America]	Accepted - reference to Section 7.4.4.1 added.
114489	71	16	71	16	I think you ned to insert "approximatley" before "Linearly" [Jan Fuglestvedt, Norway]	Accepted - change made as suggested.
114491	71	22	71	24	I think you dont need to discuss mechansims here; better refer back to eralier parts [Jan Fuglestvedt, Norway]	Rejected. Our preference is to briefly refer to mechanisms, with references to the sections where these are discussed in more detail.
50913	71	25	71	25	either 'temperature' or 'tend' needs an 's' at the end of it [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - suggested change implemented.
79779	71	31	71	31	I think that the caption here does not match the one that is indicated just below the figure on page 4-176 (different meaning of hatching etc …) [Laurent Terray, France]	Accepted. This is the correct heading, and has replace that on page 4-176.
96433	71	34	71	37	Figure 4.34, legend: The legend does not correspond to the legend below the figure itself (page 4-176, lines 7-8): Sentence on "Stippling" is missing; measure of robustness ("at least 90% of the models agree on [...]") does not correspond to statement on "cross-hatching" ("two-thirds of the models agree [...]"). Please verify. [Nicole Wilke, Germany]	Accepted. This is the correct heading, and has replaced that on page 4-176.
18995	71	45	73	18	This seems a rather random selection of regions. What is the rational for assessing changes for some regions but not others? Are the results preesnted here, in particular the confidence levels consistent with more systematic assessment of regional changes in chapters 11 & 12? [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There are no overlaps with Chapter 11, since precipitation extremes as a function of the level of global warming, are discussed in Ch-11. We have taken care that the discussion on regional changes in 4.6.1.2 are consistent with those in Chapters 10&12. Moreover, our focus is on highlighting precipitation changes that amplify with increasing level of global warming, focusing on broad geographical regions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6677	71	49	71	49	"increase" should be "increases". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
53077	71	53			also Fläschner et al. 2016 [Hervé Douville, France]	Accepted. The reference is added
116341	71		71		What is the confidence associated with projections of Antarctic polar amplification, given outcomes of chapter 3, and also insights from paleoclimate simulations for Antarctic climate? [Valerie Masson-Delmotte, France]	Taken into account. We have revised the text, which now states that Antarctic amplification (which is defined as SH high latitude warming, compared to the change in GSAT, see Table 4.2) is unlikely to occur at all levels of global warming of 1-4 degrees C that manifest during the 21ste century. This is consistent with the assessment of Section 7.4.4.1, which discusses in detail the capability of CMIP6 models to represent polar amplification for different paleoclimates, such as the LGM, MPWP, and early Eocene.
45523	72	6	72	6	It should be “The fast responses are forcing-dependent...” [Leonard Borchert, France]	Accepted. The sentence is reframed accordingly
70903	72	6	72	7	It is not correct to say that the slow precipitation response scales with GSAT: see Zappa et al. (2020 doi: 10.1073/pnas.1911015117) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
87621	72	9	72	9	contrained → constrained. [Valentina Roberta Barletta, Denmark]	Accepted
103021	72	9	72	9	contrained → constrained. [Philippe Tulkens, Belgium]	Accepted
53079	72	9			"constrained" [Hervé Douville, France]	Accepted
87623	72	15	72	21	Caption is more extended than that at page 177. [Valentina Roberta Barletta, Denmark]	Taken into account. Captions have been made consistent.
103023	72	15	72	21	Caption is more extended than that at page 177. [Philippe Tulkens, Belgium]	Accepted. Both figure captions are now the same. (Please note that in the published report the figures will be integrated in the text and therefore the caption will only feature once).
96435	72	16	19		Figure 4.35, legend: The legend does not correspond to the legend below the figure itself (page 4-177, lines 3-6): Sentences on "Stippling" and "Hatching" are missing: "Stippling indicates regions where the multi-model […]. Hatching indicates regions where [...]". [Nicole Wilke, Germany]	Accepted. Both figure captions are now the same. (Please note that in the published report the figures will be integrated in the text and therefore the caption will only feature once).
4153	72	34	72	35	Another relevant study here that could be considered is that of Chevuturi et al. (2018): Projected changes in the Asian-Australian monsoon region in 1.5°C and 2.0°C global-warming scenarios. https://doi.org/10.1002/2017EF000734 [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
130497	72	36	72	36	strange expression: "dangerous extrem precipitation events". [Panmao Zhai, China]	Accepted. The discussion on extreme precipitation events are removed from this section.
53081	72	42	72	43	Too general statement which may not be valid in many regions or for large ensembles? [Hervé Douville, France]	Accepted. Regional assessments are removed from Section 4.6.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127577	72	43	72	43	[PROGRESS] Add a few sentences summarizing whether AR6 knowledge and/or confidence levels differ markedly from AR5, and, if so, how and why. [Trigg Talley, United States of America]	The assessment is consistent with AR5 finding that global mean precipitation will increase with increased global mean surface temperature. However, this section assess that with increase in level of global warming, regional patterns in precipitation vary considerably. it is very likely that with increase warming, a larger land area is projected to experience statistically significant increase in precipitation.
70905	72	43	72	45	This is also shown in Mindlin et al. (2020: doi: 10.1007/s00382-020-05234-1) to be robust across storylines of change in these SH regions [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The relevant reference is included in the assessment.
79781	72	44	72	45	I would suggest to specify if it is austral or boreal winter. [Laurent Terray, France]	Accepted
79783	72	47	72	48	I would be a bit cautious here, for instance for SW Australia, section 10.4.1 suggests that the forced response cannot really expain the decreasing rainfall trend [Laurent Terray, France]	Taken into account; sentence no longer appears.
19895	72	51	73	18	Figure 4.36 is very nice: in particular, it provides a clear feeling about what is meant by "dry gets drier, wet gets wetter", and the extent up to which this is correct. The legend should indicate the difference between the top and bottom lines of the figure (or alternatively remove one of those lines…) [philippe waldteufel, France]	Taken into account. Figure caption has been modified.
87625	72	53	72	53	Caption in more extended than the one at page 178 [Valentina Roberta Barletta, Denmark]	Taken into account. Captions have been made consistent.
89691	72	53	72	53	Indicate how you define "significant" [Kirsten Zickfeld, Canada]	Taken into account. Figure caption has been expanded to explain significance shown in fig.
103025	72	53	72	53	Caption in more extended than the one at page 178 [Philippe Tulkens, Belgium]	Accepted. Both figure captions are now the same. (Please note that in the published report the figures will be integrated in the text and therefore the caption will only feature once).
96437	72	53	72	56	Figure 4.36, legend and graphics in bottom row (page 4-178): Please specify description on top and bottom row (i. e. does the bottom row correspond to the ocean area fraction?). Please label the axes in the bottom row accordingly (i. e. "ocean area fraction")? [Nicole Wilke, Germany]	Taken into account. Caption has been made consistent with page 178
53083	72	53			suppress first occurrence of "precipitation" from the caption, specify that shaded areas represent the 5-95% confidence interval? [Hervé Douville, France]	Taken into account; duplication removed.
96439	72	56	73	1	Figure 4.36, legend: The legend does not correspond to the legend below the figure itself (page 4-178, lines 3-6): Sentence is missing: "The solid line illustrates the CMIP6-multi model mean and the shaded […]." [Nicole Wilke, Germany]	Accepted. Both figure captions are now the same. (Please note that in the published report the figures will be integrated in the text and therefore the caption will only feature once).
104679	73	6	73	6	The sentence "It is virtually certain that average warming will be higher over land than over the ocean." appears out of place in this section on precipitation. [William Merryfield, Canada]	Taken into account. The assessment is made based on Figure 4.36

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53085	73	6	73	8	Remove the first sentence and rephrase the second one, for instance as: "Precipitation variability in most climate models is projected to increase over most areas in response to global warming, including over land." [Hervé Douville, France]	Taken into account. The sentence is reframed
50915	73	7	73	7	in most climate models increases over…' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
50917	73	11	73	11	increase or decrease in precipitation' - if information is avaiable it would be helpful to specify here the proportion of land likely to experience an increase or decrease in precipitation. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence is reframed
114493	73	15	73	18	this is a repetition, but OK, with a slightly different angle. I guess this is considerd by teh authors. [Jan Fuglestvedt, Norway]	Taken into account; text restructured to avoid duplication.
50919	73	16	73	16	Suggest adding ' In regions where precipitation on land is expected to rise, increase will be higher…' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence no longer appears.
96441	73	17			Linking word is missing: "[…] compared to 1.5°C and? 2°C of warming […]." Please verify. [Nicole Wilke, Germany]	Accepted. Corrected the sentence
53087	73	50	73	52	may be also true for changes in regional monsoon circulations and would deserve a warning about the limitations of the method for both tropical and mid-latitude precipitation changes given the possible contribution of changes in large-scale circulation? [Hervé Douville, France]	Rejected. We don't have sufficient literature to assess the changes in regional monsoon circulation as a function of increasing level of global warming.
41415	74	1	74	17	The content presented in this subsection does hardly cover the highly policy relevant issues implied by the heading. In fact, the current content much rather appears to be an extension of the previous pattern scaling subsection. Only the path dependence/overshoot issues are covered to some degree atm. If you read the first paragraph after reading the subsection heading, it should become clear to the authors that the content is detached from what the section is supposed to be covering in detail. 1.5/2 degC are of utmost importance in the context of the Paris Agreement. No assment at all is provided on 1.5 degC. This subsection has to be thoroughly revised. [Alexander Nauels, Germany]	taken into account, implications of overshoot and comparison with mitigation scenarios have been better developed
9807	74	1	74	33	The National Academies of Sciences (2016, doi:10.17226/21898) introduced another policy-relevant metric, the Initial Pulse Adjustment Timescale, defined as the time to peak warming after the injection of a small pulse of CO2 on top of a baseline scenario. IT would be useful to have an IPCC assessment of this metric. [Robert Kopp, United States of America]	taken into account. Content of this section is revised
114505	74	1	75	17	From the title of Section 4.7.4 I expected to find more about feedbacks triggered during overshoot. [Jan Fuglestvedt, Norway]	taken into account, implications of overshoot and risk have been better developed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19897	74	3	74	6	Again, these lines raise hesitation about what is meant in this report by "assessment". On line 6, the report does assign at least some likelihood and/or confidence statements; on line 3, it does not, and assessing might then mean reporting on, commenting, discussing… This is not simply an editorial issue. We are considering here an assessment report, hence asking for a clear understanding of what assessment means appears legitimate. Incidentally, the group "assess" is counted 4760 times in the report SOD. [philippe waldteufel, France]	taken into account, elements of assessment have been made clearer
89693	74	8	74	8	Replace "carbon" with "CO2" (the TCRE is only defined for CO2) [Kirsten Zickfeld, Canada]	accepted
106287	74	10	74	10	If you want to be more precise, the section is Section 5.5. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	accepted
114495	74	13	74	14	Make it more clear that this is your assessment; not only a reflection of what the literature is saying [Jan Fuglestvedt, Norway]	taken into account, elements of assessment have been made clearer
132513	74	21	74	24	The discussion of time-dependent feedbacks seems out of place here. It is related to the change in SST pattern (failure of pattern scaling) over time, but would take quite a bit more explanation to make this connetion. Why not just say that the SST pattern changes over time, with the slow emergence of Southern Ocean warming being a nice example. This has implicatoins for climate sensitivity in Chapter 7, but that seems not relevant here. [Kyle Armour, United States of America]	taken into account, those statements sit better elsewhere, and have been removed here
132515	74	26	74	28	Isn't the key point to emphasize here that the differences in spatial pattern between scenarios and warming levels relatively small compared to the common projected pattern of warming itself? This is a nice analysis, but I see it as confirming that pattern scaling works to a large degree, except over the Southern Ocean. [Kyle Armour, United States of America]	not applicable, this figure is no longer presented
89695	74	30	74	33	Explain why the temperature patterns differ. Due to different comination and regional patterns of forcing? [Kirsten Zickfeld, Canada]	not applicable, this figure is no longer presented
93863	74	30	74	33	It would be very useful to have explanations for the noted differences between the changes in temperature and precipitation for the two assessed pathways. [Quentin Lejeune, Germany]	not applicable, this figure is no longer presented
96443	74	30	74	33	Fig. 4.37 shows higher warming in most regions for 2 degrees under SSP5-8.5 vs SSP1-2.6. Since the intuitive response would be more of a warming in the scenario with slow changes (due to more of the committed warming being realized with the system being closer to equilibrium) a short explanation should be given. [Nicole Wilke, Germany]	not applicable, this figure is no longer presented
50921	74	31	74	32	Over many land areas, SSP5-8.5 has more warming than SSP1-2.6 for the same global GSAT change of 2 degrees. Is this due to fewer aerosols assumed to be emitted with strong decarbonisation or a triggering of land feedbacks or some kind of land-use change? Please explain the reason(s) for this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	not applicable, this figure is no longer presented
114499	74	38	74	43	figure 4.37: I think you should avoid TAS in figure. [Jan Fuglestvedt, Norway]	not applicable, this figure is no longer presented

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114501	74	38	74	43	re "deviation of patterns of climate change at a common level of warming achieved by two different pathways." It could be useful with a few words about what the differences are in the pathways (aerosols, GHG….) [Jan Fuglestvedt, Norway]	not applicable, this figure is no longer presented
89697	74	43	74	43	"path dependence" is misleading here. The difference is not only the result of different trajectories of forcing, but also different forcing combinations. [Kirsten Zickfeld, Canada]	not applicable, this figure is no longer presented
114503	74	48	74	49	Re "While emissions…… are still possible…" I think this needs more nuances. (We dont know if this is possible technlogically, econmically etc.) [Jan Fuglestvedt, Norway]	accepted. Phrase removed
89701	74	48	74	49	Most scenarios consistent with limiting warming 1.5, 2 degress by 2100 involve overshoot (SSP1-1.9, SSP1-2.6) [Kirsten Zickfeld, Canada]	accepted
21695	74	48	74	49	Does this still hold true given the shifts in our understanding of warming to date articulated in chapter 2 and noted frequently earlier in this chapter? [Peter Thorne, Ireland]	taken into account. Assessment has been checked across chapter findings
12261	74	48	75	5	This very important section has not been well handled at all. The important conclusion is there is a hysteresis, but that is hardly evident from the text or Fig. 4.38. "overshoot" is not the relevant concept. The mean numbers in lines 2-3 are difficult to interpret. I also question the small uncertainty given to precip given the large variability evident in Fig. 4.38.One cannot discern either the 2081-2100 or earlier equal CO2 period from Fig. 4.38 [Bryan Weare, United States of America]	taken into account, figure has been clarified
50923	74	53	74	53	SSP5-34-OS overshoot scenario - it would be helpful to inlcude here what this scenario results in in terms of a peak temp rise (and when) based on median climate sensitivity. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	taken into account - more scenario details have been given
19899	74	53	75	1	The specific case of sea level needs to be commented. [philippe waldteufel, France]	rejected. This is already discussed
89711	74	53	75	2	There is a body of literature on the climate effects of overshoot that is not cited here, e.g. Tokarska et al., 2015, ERL; Tokarska et al., Earth's Future, 2019; Mathesius et al., 2015, Nat. Clim Change, Li et al., GRL, in revision (see unpublished papers on DMS). [Kirsten Zickfeld, Canada]	accepted, literature coverage has been substantially increased
87839	74	53	75	5	Please note that these conclusions depend on the level of overshoot. It is essential to clarify. Also, it would be helpful to cite other recent papers that explore climate response to overshoot scenarios using different models, for example: Tachiiri et al., 2019. Effect on the Earth system of realizing a 1.5 °C warming climate target after overshooting to the 2 °C level. ERL. and: Tokarska et al. 2019. Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an Overshoot. AGU Earth's future. [Katarzyna Tokarska, Switzerland]	accepted, literature coverage has been substantially increased
89699	74	55	74	55	"are not reversible": is this only due to a lagged response, or also aymmtery in forcings? E.g. non-CO2 forcings are likely different in 2020 and 2100. [Kirsten Zickfeld, Canada]	taken into account - more scenario details are described

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50929	75	1	75	3	Suggest that this statement could be made clearer if the projected estimate is captured with the corresponding variable in lines 54-55 (pg 74), e.g. GSAT (0.36±0.34 K) etc. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted, text has been clarified
53089	75	1			Figure 4.38: How many models? [Hervé Douville, France]	accepted, this is detailed in the figure data statements
87841	75	3	75	4	Please clarify to which variables this sentence is referring to, and at what level of overshoot? Also, please note that surface warming is reversible for low levels of overshoot (up to 200-300 PgC), e.g. see Tokarska et al. 2019. Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an Overshoot. AGU Earth's future. [Katarzyna Tokarska, Switzerland]	taken into account, text has been clarified
106289	75	3	75	5	It would be essential to clearly state that this assessment is conditional on concentrations declining, as these runs are concentration driven. Given the large uncertainties in the carbon-cycle response (see Section 5.4 in Chapter 5) the swift reversal of concentrations is not a given after a high overshoot. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	taken into account, text has been clarified
21697	75	4	75	5	This feels a bit of a dangerous statement to make without including at least some references to back it up. Is it supported by literature? If so please cite. If not then carefully consider this text. If instead it is backed by the analysis in the chapter at least be explicit and note how although also have regard to the scope that this is an assessment and not performing fundamentally new and novel science, obviously. [Peter Thorne, Ireland]	taken into account, text has been clarified
116343	75	5	75	5	There are aspects here that could be relevant for the TS-SPM (on irreversibility- reversibility at global - regional scales). Please expand the corresponding chapter finding. [Valerie Masson-Delmotte, France]	accepted, summary statement in ES
46593	75	12	75	12	Please show sea-ice area (not extent) in panel c for consistency with the remainder of this chapter, and chapter 2 and 9 [Dirk Notz, Germany]	accepted, figure has been clarified
111819	75	20	75	20	on terminology: this section is actually not on CDR as such, but 'only' on CDR leading to net negative emissions (not to the still very high volumes of CDR needed to offset residual emissions to get to net zero). I wonder if that could be reflected in the section title. I don't have a specific proposal to make. Maybe "net negative emissions" would work in the section title, but probably only if you change "mitigation" as well [Oliver Geden, Germany]	Taken into account. Thanks for the excellent suggestion. The text and title are revised.
114509	75	20	88	17	This section adresses xWG topics. I suggest coordination across all three WGs on terminolgy here. The xWG team on SRM can be very useful here. And more use of the SR1.5 and SRCCL. I also miss more assessment of the literature by the author team; and not just referring to what single papers have found [Jan Fuglestvedt, Norway]	Taken into account. Thanks for the excellent suggestions. The final draft addresses all these issues.
89703	75	22	75	22	Mitigatin versus large-scale intervention: This categorizaton is inconsistent with the definiton of mitgattion in the WGI glossary ("A human intervention to reduce emissions or enhance the sinks of greenhouse gases"). [Kirsten Zickfeld, Canada]	Accepted; the text is revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111821	75	22	75	26	I wonder if you really need refer to the "geoengineering" terminology here since the IPCC doesn't use it anymore since SR1.5 (you mention it later, but you are kind of reintroducing the impression that this is an appropriate term). You could even do with subsuming CDR and SRM under "climate intervention" and just describe what CDR and SRM do. In my view, this would be consistent with the "new" IPCC logic of treating them strictly seperately [Oliver Geden, Germany]	Taken into account. Since the term "geoengineering" has been used in the literature extensively in the past, we are only recollecting it here. We do not use the term hereafter in the entire body of the text. In the revision, CDR and SRM are treated separately.
112097	75	24	75	24	The EuTRACE assessment of 2015 (see reference at end) should also be cited here as a key defining reference document for the topic. It was the first (and still only significant) international assessment assessment of the topic that was conducted, augmenting the numerous national assessments, and included a broader scope on societal issues than the NRC assessment. Further, only citing a UK and US report risks reinforcing the perception of the IPCC as being Anglo-dominated. Citation: Schäfer, S.; Lawrence, M.; Stelzer, H.; Born, W.; Low, S.; Aaheim, A.; Adriázola, P.; Betz, G.; Boucher, O.; Carius, A.; Devine-Right, P.; Gullberg, A. T.; Haszeldine, S.; Haywood, J.; Houghton, K.; Ibarrola, R.; Irvine, P.; Kristjansson, J.-E.; Lenton, T.; Link, J. S. A.; Maas, A.; Meyer, L.; Muri, H.; Oschlies, A.; Proelß, A.; Rayner, T.; Rickels, W.; Ruthner, L.; Scheffran, J.; Schmidt, H.; Schulz, M.; Scott, V.; Shackley, S.; Tänzler, D.; Watson, M.; Vaughan, N. (2015) The European Transdisciplinary Assessment of Climate Engineering (EuTRACE): Removing Greenhouse Gases from the Atmosphere and Reflecting Sunlight away from Earth. Funded by the European Union's Seventh Framework Programme under Grant Agreement 306993. [Mark Lawrence, Germany]	Accepted. Excellent suggestion. Text is revised. All references are updated where required.
50927	75	25	75	29	It is strange that the Glossary of SR1.5 is referenced, but not the Glossary of the AR6. Where relevant it would be useful to also include references to the AR6 Glossary. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; the text is revised. AR6 Glossary is referred in the FGD
127579	75	26	75	30	As the paragraph takes the space to discuss usage of the term "geoengineering", it seems awkward to not explain the term "Solar Radiation Management" instead of the new term "Solar Radiation Modification". It is understandable to choose a less loaded phrase, but it should at least be explained once. [Trigg Talley, United States of America]	Taken into account; text is revised. Solar radiation management is now mentioned.
96445	75	28	75	28	SRM is limited to changes in shortwave radiation budget here, but p. 81 l. 26-27 clarify that SRM refers to thermal radiation management as well (which is good, for completeness). So please change here to include longwave radiation management under "SRM" or "RM". [Nicole Wilke, Germany]	Taken into account; the text is revised to also include the some long wave altering options.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112099	75	28	75	29	SRM is a misnomer, since it does not only refer to techniques modifying the shortwave radiation budget, but also includes Cirrus Cloud Thinning (CCT), later in Table 4.7, which modifies the longwave radiation budget. Here it is enough to include at the end of the sentence "...and is also widely used to include further related techniques, especially cirrus cloud thinning, which modifies the longwave radiation budget". This is elaborated on in the comments on section 4.6.3.3. [Mark Lawrence, Germany]	Taken into account; the text is revised to also include some long wave altering options
5777	75	29	75	33	It is indeed best to consider SRM and CDR separately and to avoid the word "geoengineering" [Jesse Reynolds, United States of America]	Taken into account. SRM and CDR are treated separately in the revision and the use of the term "geoengineering" is avoided.
114531	75	29	87	33	Maybe this paper is of relevance: Detecting sulphate aerosol geoengineering with different methods. https://www.nature.com/articles/srep39169 [Jan Fuglestvedt, Norway]	Accepted; Text is revised. The revised text cites this reference.
114507	75	31	75	31	Re "overlap": Not clear what is meant here. [Jan Fuglestvedt, Norway]	Taken into account; text is revised. The revised text confirms with the AR6 convention that CDR is a form of mitigation.
111823	75	31	75	31	CDR and mittigation: that's tricky, and as the sentence stands, you should add some words on the overlap - or delete part of the sentence that mentions the overlap [Oliver Geden, Germany]	Taken into account; comment is highly appreciated. text is revised. The revised text confirms with the AR6 convention that CDR is a form of mitigation.
89705	75	31	75	32	"While there is some overlap …". CDR is part of mitigation according to the WGI glossary. [Kirsten Zickfeld, Canada]	Taken into account; Text is revised. The revised text confirms with the AR6 convention that CDR is a form of mitigation.
71211	75	31	75	33	I have a concern with the differentiation between mitigation CDR and SRM. What is the temporal scope consideration in this separation? The CO2 amounts in the atmosphere might at one point in the past have accumulated because of anthropogenic activites and poor mitigation accounting. Not puting these into consideration might be an oversight. I think this needs to be considered or a qualifier of the differentaition needs to be inserted. [Michael Mugarura, Germany]	Taken into account. Text is revised. The revised text confirms with the AR6 convention that CDR is a form of mitigation.
112109	75	31	75	33	The clear distinction here between mitigation and CDR (in contrast to some other previous IPCC publications) is very appreciated and encouraged to be kept up throughout the revision process. [Mark Lawrence, Germany]	Accepted; Thanks for the appreciative comments; Text is revised. The revised text confirms with the AR6 convention that CDR is a form of mitigation.
89707	75	34	65	34	SSP1-2.6 could mentioned in addition to or instead of RCP2.6 [Kirsten Zickfeld, Canada]	Taken into account; The text is revised. SSP1-26 is discussed in the CDR section 4.6.3.2
68655	75	34	75	34	Why not add SSP5-34-OS here as well, since it includes the largest negative emissions? [Simone Tilmes, United States of America]	Taken into account; the text is revised. The link between CDR and SSP5-34-OS is discussed in the CDR section 4.6.3.2
96447	75	34	75	34	Shouldn't RCP2.6 be referred to as SSP1-2.6, Box SPM.2 uses the SSPx-y nomenclature for scenarios only. Or is indeed a reference to the AR5 scenario intended here? [Nicole Wilke, Germany]	Accepted. Text is revised. If RCP is used, it does refer to the previous class of scenarios. SSP1-2.6 is discussed in the CDR section 4.6.3.2
111825	75	34	75	34	If you talk about CDR as such (as you clearly do here), then any strong mitigation scenario contains CDR (to offset residual emissions). If you talk about net negative, then you should explicitly say so [Oliver Geden, Germany]	Taken into account. Negative and net emission are discussed in detail below in section 4.6.3.2 where climate response to CDR is discussed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93865	75	34	75	36	This sentence is simplistic and misleading; there are many land-use pathways that are compatible with RCP2.6 or even more stringent mitigation targets (such as RCP1.9), including pathways without BECCS. The pathway called RCP1.9 'Low Energy' in SRCCL (equivalent to LED in SR15) for example does not use BECCS at all, which implies that this can also be the case for some RCP2.6 scenarios if stringent criteria on e.g. energy demand are verified. [Quentin Lejeune, Germany]	Accepted; the text is revised. A detailed discussion of CDR options is provided in Chapter 5 which is referred in this section.
106291	75	34	75	38	Another good line of evidence to support this statement are the four illustrative pathways presented in the IPCC SR1.5 (SPM and Chapter 2). Even the scenario that explicitly excludes contributions from CCS (and thus BECCS) a substantial contribution from land-based removals has to be considered. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; Text is revised. A detailed discussion of CDR options is provided in Chapter 5 which is referred in this section
15939	75	34	75	38	"Most strong-mitigation scenarios assume CDR in addition to emissions reductions; for example, RCP2.6 explicitly includes direct CDR from around 2025 onward and achieves net negative emissions by 2070 through a combination of bioenergy and carbon capture and storage (van Vuuren et al., 2011a). Similarly, the emission scenario of SSP1-1.9 is characterized by a rapid decline to zero and a long period of negative emissions for CO2 (O'Neill et al., 2016; Rogelj et al., 2018a)." This statement makes no consideration of the thermal efficiency of a BECCS power plant. Our assessment is that the thermal efficiency of a power plant can be no more than 8% and once other energy requirements are taken into account such as separation of the CO2 from the exhaust stream and normal process losses, then the thermal efficiency is most likely to be negative. Under these circumstances, it is highly unlikely that BECCS will play any significant role in the global energy supply or in solutions to remove CO2 at scale from the atmopshere. See https://cop23.unfccc.int/documents/65014 [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; Text is revised. A detailed discussion of CDR options is provided in Chapter 5 which is referred in this section. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68271	75	34	79	38	As written, this implies that BECCS is the only way to achieve negative emissions in RCP2.6. This is a critical error and must be corrected. BECCS is not carbon negative in the near-term because bioenergy leaves a carbon deficit for several decades to a century—far longer than the window of a decade or two available for slowing feedbacks and avoiding crashing through the 1.5C guardrail. See, e.g., IPCC AR5 WG III (2014) 11.13.4 GHG emission estimates of bioenergy production systems ("The combustion of biomass generates gross GHG emissions roughly equivalent to the combustion of fossil fuels. If bioenergy production is to generate a net reduction in emissions, it must do so by offsetting those emissions through increased net carbon uptake of biota and soils…Hence, the total climate forcing of bioenergy depends on feedstock, site-specific climate and ecosystems, management conditions, production pathways, end use, and on the interdependencies with energy and land markets…For example, in the specific case of existing forests that may continue to grow if not used for bioenergy, some studies employing counterfactual baselines show that forest bioenergy systems can temporarily have higher cumulative CO2 emissions than a fossil reference system (for a time period ranging from a few decades up to several centuries"). Subsequent analysis since AR5 further strengthens the case that bioenergy is not carbon neutral in the critical next decade or two. Danielle Venton, Core Concept: Can bioenergy with carbon capture and storage make an impact?, PNAS (2016); Mary S. Booth, Not carbon neutral: Assessing the net emissions impact of residues burned for bioenergy, ENVIRON. RES. LETT. 13 (21 February 2018); Sterman J. D., et al. (2018) Does replacing coal with wood lower CO2 emissions? Dynamic lifecycle analysis of wood bioenergy, ENVTL. RESEARCH LETTERS 13(015007):1–10, 1 ("We simulate substitution of wood for coal in power	Taken into account; the text is revised. A detailed discussion of CDR options is provided in Chapter 5 which is referred in this section. All references are updated where required.
89709	75	35	75	35	unclear what is meant by "direct" CDR [Kirsten Zickfeld, Canada]	Accepted. "direct" is deleted in the revision
130503	75	35	75	35	direct CDR? [Panmao Zhai, China]	Accepted. "direct" is deleted in the revision
50931	75	37	75	37	long period' - please specify how long. 'rapid decline to zero' - please specify when this occurs. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The text is revised. C emissions become zero by 2050 and emissions negative in the 2nd half of this century (see section 4.6.3.2 on CDR)
111827	75	40	75	43	Maybe better just to mention CDR and SRM, instead of subsuming them under "climate internvention" (which is, in a way, the NAS' term for geoengineering) [Oliver Geden, Germany]	Accepted; text is revised extensively for FGD.
111829	75	41	75	41	you might add the chapter numbers for WG3: 12 + 14 [Oliver Geden, Germany]	Accepted and Taken into account: Text is revised. Chapter numbers are provided.
2395	75	42	75	43	ERFs due to aerosols in SRM are discussed in section 6.3.7 [Vaishali Naik, United States of America]	Taken into account; Text is revised. The relevant section in Chapter 6 is now referred in the SRM section 4.6.3.3
71945	75	46	78	39	An important concept missing here is that the oceans, glaciers and ice sheets will all be out of equilibrium and will contribute to ongoing ocean warming and sea level change for decades to centuries. An important aspect of this is that later/slower mitigation leads to larger commitments to sea level rise, as well as possible threshold crossing. [John Church, Australia]	Accepted. This is indeed a very good comment. This lag in sea level rise is discussed in the CDR subsection 4.6.3.2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21699	75	49	75	52	The logic behind this assertion is insufficiently clear to me as it stands. The statement is probably correct but even after thinking about it for some time its not clear to me that it follows from the argument given here. I think some more detail is required to be much clearer to the reader here. [Peter Thorne, Ireland]	Taken into account; the text is revised extensively for the FGD.
89713	75	50	75	50	Matthews et al 2009 should be included among the "classics". [Kirsten Zickfeld, Canada]	Accepted; the text is revised. All references are updated where required.
111831	75	50	75	52	you should clarify whether you are talking about net zero CO2 or GHGs (I guess the former, which would not even be 2C compatible) [Oliver Geden, Germany]	Accepted; thanks for pointing this out. It should be GHGs. The text is revised.
68657	75	51	75	52	"avoid serve warming thresholds" net-zero emissions by 2100 may still reach many serve warming thresholds, so this is not really correct. [Simone Tilmes, United States of America]	Taken into account; the text is revised in response to this and similar comments. Sentence is deleted
89715	75	51	75	52	It should be added that warming threshold exceedance could be avoided by net negative emissions. [Kirsten Zickfeld, Canada]	Taken into account; the text is revised extensively for the FGD. Sentence is deleted in revision
50925	75	51	75	52	What is meant by "severe warming thresholds"? This phrase is not in the glossary. Does it mean "tipping points"? (which is in the glossary) or "higher temperatures"? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; text is revised extensively for the FGD. Sentence is deleted in revision
106293	75	54	76	2	The validity of this statement would depend on the rate of emissions reductions. For example, consider the extreme case of CO2 emissions dropping to zero (a shut-down experiment). Concentrations would start decling immediately, and absent a substantial ZEC also warming will "peak" or better, stabilize. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; the text is revised extensively for the FGD
106295	75	54	76	2	"peak temperature" requires a more specific description. If CO2 emissions become net zero, temperature is expected to stablize. In that case "peak" is not easily understood. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; the text is revised for clarity
34883	75	54	76	14	The SOD notes that even with severe CO2 mitigation, global CO2 levels will continue to rise for decades. Please see general comment #14 above. [Jim O'Brien, Ireland]	Taken into account. Text is revised extensively for the FGD.
87843	76	4	76	5	The first sentence in this paragraph is convoluted, and it is unclear what it means. Please note that in very ambiguous mitigation scenarios with net-negative emissions, the carbon sinks reverse and outgassing is happening. It would be good to either clarify what levels of mitigation this sentence is referring to. [Katarzyna Tokarska, Switzerland]	Taken into account; the text is revised extensively for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15941	76	4	76	5	The statement: "Mitigation would reduce CO2 emissions, but atmospheric CO2 concentrations would continue to increase as long as emissions exceed removal by sinks (Millar et al., 2017)." should be qualified with some estimation of the expected rates of removal by sinks. For example, when applying the the average rate of removal of CO2 over the last 4 Mililankovitch cycles to the current high levels of CO2, indicates that it will take approximately 250,000 years for CO2 to return to the upper peak levels of the paleoclimate records. Alternatively, the AR5 estimate of permanent CO2 sequestration at 1 petagramme of CO2 per year could be quoted. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; the text is revised extensively for the FGD.
87849	76	4	78	38	The paragraphs on internal variability, time of emergence and detection of mitigation do not fit well into the section about climate response to zero emissions and CDR. Please note that some models in ZEC-MIP and CDR-MIP intercomparisons do not include atmospheric dynamics or internal variability, so the discussion of internal variability here seems out of place. It would be helpful to move the paragraphs on internal variability into a separate section. Currently, the discussion of internal variability breaks the flow of discussing the climate response to zero-emission ZEC and CDR scenarios and intercomparison. [Katarzyna Tokarska, Switzerland]	Taken into account. Accepted. Text is revised extensively for FGD. Section 4.6.3.1 is devoted to the discussion on the emergence of benefits of emission reductions. Section 4.6.3.2 discusses the climate response to CDR options. Commitment is no longer discussed in this subsection.
87845	76	6	76	7	It is unclear how this sentence differs from ZEC described a few lines below. Please note that the section below concludes that temperature may decline or increase after emissions are stopped. Please clarify to avoid contradiction. [Katarzyna Tokarska, Switzerland]	Taken into account; the text is revised extensively for the FGD. Commitment no longer covered in this subsection.
87847	76	7	76	11	This sentence is confusing and may seem to contradict the conclusions from ZEC MIP discussed on p.91, line 17, which say that "it is very likely that atmospheric CO2 concentrations will decline for decades of CO2 emissions cease" but temperature response to ZEC is model dependent. Perhaps consider re-writing this section or moving the ZEC section (p.90) up here, to make sure that the conclusions are consistent. [Katarzyna Tokarska, Switzerland]	Taken into account; the text is revised extensively for the FGD. Commitment no longer covered in this subsection.
89717	76	8	76	9	"would decrease only slowly": it even increases in some models (MacDougall et al., 2020, Biogeosciences; Fig. 4.43) [Kirsten Zickfeld, Canada]	Taken into account; text is revised. All references are updated where required.
106297	76	12	76	14	With the ZECMIP results having been published now, it makes sense to replace this reference with MacDougall et al (2020) Biogeosciences. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; commitment no longer covered in this subsection.
114511	76	16	76	27	I feel this is repetition. It woudl be good if you coudl make it more clear what you want to say here. [Jan Fuglestvedt, Norway]	Accepted; text is revised.
21701	76	20	76	22	I thought you had said earlier that Arctic would be effectively sea-ice free by mid-century irrespective of scenario. If so, clearly both these statements cannot simultaneously be true. Suggest cross-check for consistency the various arctic sea-ice sections. [Peter Thorne, Ireland]	Taken into account; topic no longer covered here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89719	76	22	76	36	There is some overlap with the material assessed in section 5.6.1 [Kirsten Zickfeld, Canada]	Taken into account; topic no longer covered here.
96449	76	26			Please specify to what "probability" is referred to: "probability of drought" or "probability of drought deficits". (Please rephrase, if correct: "[…] the probability of drought and of precipitation deficits in some regions […]"?) [Nicole Wilke, Germany]	Taken into account; topic no longer covered here.
68659	76	29	76	44	I would suggest switching the content in this paragraph around, and first discuss the RCP8.5 vs. RCP4.5 studies and then the 1.5 and 2.0C studies. Else it sounds like RCP4.5 is a viable option compared to RCP8.5, but this is not the case. RCP4.5 is still not an option in terms of climate impacts, reaching tipping points etc. [Simone Tilmes, United States of America]	Accepted, warming levels no longer covered here.
9809	76	29	76	44	See also Li et al 2020, doi:10.1088/1748-9326/ab7d04, for changing WBGT extremes as a function of GSAT [Robert Kopp, United States of America]	Taken into account, extremes no longer covered here.
89721	76	51	76	52	Give reference period for temperature anomalies [Kirsten Zickfeld, Canada]	Taken into account, these numbers have been deleted here.
50933	76	54	76	54	20 years of emissions reductions' - please explain here how much emissions are reduced by over this period. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, extremes no longer covered here.
114513	76	55	76	55	What kind of mitigation is referred to here? Possible to specify (CO2, Ch4, aerosols….) [Jan Fuglestvedt, Norway]	Taken into account, extremes no longer covered here.
116345	76		76		There is a need to develop this section (climate response to mitigation) in the TS, building on outcomes of multiple chapters. It could also be nice to better quantify the type of rate of reduced CO2 emissions linked with discernable effects over a certain time interval (more quantitative information on the size of a discernable signal) (I have in mind temporary reductions such as in 2020 compared to sustained, reduced emissions). [Valerie Masson-Delmotte, France]	Accepted. Excellent point. Text is revised. Here, we have assessed the response over decadal scale for reductions in CO2 emissions over decades. We believe the climate change signal due to CO2 emission reduction in 2020 would not be detectable. The Cross-Chapter Box on covid-19 would address this comment.
106299	77	8	77	23	This section should probably be updated with any research looking at indications in a slowdown in CO2 concentration increase due to COVID-19 lockdown emission reductions in 2020. If half a year of emission reductions would be detected in the observations CO2 concentration record, some of the statements will have to be updated. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, Text is revised. A new Cross-Chapter Box on COVID-19 discusses the change in CO2 and other effects. However, the COVID related CO2 decrease and the global mean temperature change is likely to be undetectable against internal variability
34885	77	8	77	23	The SOD similarly notes that it will be impossible to quantify any effects of mitigation, which again points to adaptation being the key climate strategy going forward. Please see general comment #14 above. [Jim O'Brien, Ireland]	Taken into account. Text is revised. Our discussion of "delay" should not be interpreted as "never". Policy prescription regarding mitigation vs adaptation is beyond the scope of IPCC assessments
2397	77	10	77	13	Detecting the climate response to mitigation is additionally complicated due to changes in forcings with opposing signs (e.g., aerosols vs. ghgs) [Vaishali Naik, United States of America]	Taken into account. Text is revised
68661	77	12	77	12	change "know" to "observe" [Simone Tilmes, United States of America]	Accepted; text is changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127581	77	16	77	17	Why are these scenarios referred to as "counterfactual"? The term connotes different meanings to different audiences and may be considered as "not factual". Suggest additional context or language describing these scenarios. [Trigg Talley, United States of America]	Taken into account, counterfactual scenario refers to a scenario that has not happened. The text is revised
89723	77	17	77	18	"median time f detecttion": at which confidence level? [Kirsten Zickfeld, Canada]	Taken into account, it is at 5% level. Text has been simplified.
53091	77	22	77	23	May be useful to add: "The stronger the mitigation is, the earlier the detection of the benefits will be."? [Hervé Douville, France]	Taken into account. Text is revised
9811	77	25	77	35	There are some subtle points here that are omitted in the executive summary and caused confusion there. [Robert Kopp, United States of America]	Taken into account. Details are usually not provided in Executive summary.
87627	77	25	78	49	This section on the difficulty in detecting the short term effect of mitigation in very interesting. Care must be taken to the cited papers: most of them are still not published. And I seems somewhat full of repetitions, given that it is often written reporting results from specific works, instead of describing the state-of-the-art picture using published studies as support. [Valentina Roberta Barletta, Denmark]	Taken into account, Thanks for the appreciative comments, text is revised and now assesses rather than quotes the publications.
103027	77	25	78	49	This section on the difficulty in detecting the short term effect of mitigation in very interesting. Care must be taken to the cited papers: most of them are still not published. And it seems somewhat full of repetitions, given that it is often written reporting results from specific works, instead of describing the state-of-the-art picture using published studies as support. [Philippe Tulkens, Belgium]	Taken into account, Thanks for the appreciative comments, text is revised and now assesses rather than quotes the publications.
53093	77	33	77	35	Why? This is a signal to noise ratio issue and the observational constraints do not necessarily lead to increase the signal. [Hervé Douville, France]	Taken into account. Sentence has been cut.
87851	77	37	77	54	Please note that another recent study would be relevant here and could be cited: McKenna et al., Stringent mitigation substantially reduces risk of unprecedented near-term warming rates. in review at NCC [Katarzyna Tokarska, Switzerland]	Accepted, text is revised. McKenna now assessed.
17029	77	39	77	39	Maher et al., 9999a; Samset et al., 9999; 39 Spring et al., 9999) [Sergio Aquino, Canada]	Taken into account, All references are updated where required.
89725	77	49	77	54	Unclear how the analysis by Samset et al. differs from that by Friedlingstein & Tebaldi [Kirsten Zickfeld, Canada]	Taken into account, the Samset et al. analysis investigate the effect of individual climate forcers, text is revised.
96451	77	49			It is not clear, whether "time of detection" (e.g. as mentioned in lines 25 and 32) and "time of emergence" are meant interchangeably resp. synonymously. Please clarify and include a glossary entry for "emergence". [Nicole Wilke, Germany]	Taken into account, emergence now used throughout.
68663	77	51	77	51	Samset et al., 9999 [Simone Tilmes, United States of America]	Taken into account, 9999 means a submitted paper; Text is revised. All references have been updated where required.
130499	78	1	78	1	"SMILES"? Why brings this Acronym? Only used once. [Panmao Zhai, China]	Accepted; text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12265	78	1	78	10	This section does a very poor job of explaining the concepts or results in Fig. 4.39. Many questions abound. The text mentions large ensemble runs for six models, but results for only two are shown. "initial condition ensemble" is not defined. I am guessing this is not the initial condition, but that of the earlier part of the simulations. It is unclear what max and min trends are that are plotted. Noone should have to go to an unpublished paper to have these and other important questions answered. The conclusion in line 9 is pretty meaningless. How much does internal variabilty influence relevant variables? If in doubt, leave it out. [Bryan Weare, United States of America]	Taken into account. Initial condition ensemble refers to an ensemble of simulations which start from different model initial conditions. Min and max surface temperature trends from these suite of simulations is shown. Figure Caption is revised now. Text is revised extensively.
45525	78	14	78	14	Omit the “.” after “climate variability”. [Leonard Borchert, France]	Taken into account, Text is revised.
89727	78	15	78	15	"certainty": 100% confidence interval? [Kirsten Zickfeld, Canada]	Taken into account, It is 95% confidence level. Text is revised
114515	78	22	78	26	This para contains important info, and needs to be reflected at higher levels. [Jan Fuglestvedt, Norway]	Taken into account. Comment highly appreciated.
130505	78	22	78	26	Useful information for "near-term information", which could be used in Excutive summery. [Panmao Zhai, China]	Taken into account; subsection reflected in ES.
50935	78	23	78	23	faster rate than before' - please clarify if this means faster than previously understood/projected. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text revised
107007	78	23	78	23	"at a faster rate than before": too vague. When? [Christophe CASSOU, France]	Taken into account, text revised
53095	78	25	78	26	Yet, the earlier the implementation is, the more efficient the mitigation will be. Any delay in the detection of the short-term benefits due to internal variability will be compensated for by a catch-up phenomenon in the medium term and does not in any way prevent the long-term objective of reduced global warming at the end of the 21st century. [Hervé Douville, France]	Taken into account. Text is revised
53097	78	31			Figure 4.39: shift longitudes by 180°? [Hervé Douville, France]	Accepted. Figure revised.
111833	78	42	78	42	At least here you should clearly say "to net negative emissions" (maybe followed by "through CDR"), because that's waht you are mainly talking about [Oliver Geden, Germany]	Taken into account. Text is revised. Title changed to "4.6.3.2 Climate Response to Mitigation by Carbon Dioxide Removal"
41417	78	42	87	33	The hard work that has gone into the CDR and SRM sections is much appreciated, this will be very useful. Please apply subsection numbering though in order to more easily find the relevant parts in the TOC (remove introductions and in-section headings without numbering). [Alexander Nauels, Germany]	Taken into account. Text is revised. Subsection numbering is not provided as we feel it is not needed
107009	78	42			I would conclude the CDR section by a summary paragraph with a clear message about the assessment of CDR. I would conclude based on the balance of evidences listed here that CDR would induce more negative effects than positive effects based on what I am reading. In any case, whatever the conclusions be, a clear message from AR6 should come out from this section. [Christophe CASSOU, France]	Taken into account. The revised final draft is expected to have clear message on CDR
81061	78	44	79	29	For the reader, cross referencing between this CDR section and the one in ch5 (section 5.6.2: biogeochemical responses to carbon dioxide removal) would be helpful. Ch5 will do the same. [canadell pep, Australia]	Taken into account; in the revised text section 5.6.2 is explicitly referred

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12271	78	44	80	33	The 4xCO2 more than 100 year time frame of Fig. 4.40 is inappropriate here. Is there no model output to a more realistic and comprehensible 2xCO2 and decadal time scale? Fig. 4.40 is not well described in the text. There are no estimates of how much delay there is in a return. This 1%/year is not "abrupt". The "nonlinearity" of the temperature response is not clear. Overall this section is misses the mark. [Bryan Weare, United States of America]	Taken into account. The scenario described is not unrealistic. In high emission scenarios such as SSP5-8.5, the projected CO2 levels are about 1100 ppm. Also, these are idealized experiments that help to get the science. Agreed that 1% /year is not abrupt, it is ramp up simulation. The nonlinearity of the temperature response is discussed in the context of cumulative carbon in section 5.6. The relevant subsection of Chapter 5 with more details is cited in the revision
68665	78	45	78	46	it would be useful to repeat briefly what options are considered, e.g., are industrial approach considered? [Simone Tilmes, United States of America]	Accepted, the text is revised, Some examples of CDR options are now discussed in the introductory paragraph of section 4.6.3.2.
127583	78	47	78	48	It would be useful after the sentence reading "In this subsection, ..." to add a bit more detail about what is not being considered (e.g., biogeochemistry, or secondary physical impacts). For example, an implementation of biofuel production large enough to be a strong CO2 sink might have ecosystem impacts, or effects on land albedo, or soil moisture and latent heat fluxes, etc., and these effects are not being considered in the first studies of CDRMIP. These things are mentioned on page 4-80, but it might be worthwhile to provide a hint at the beginning of the section that these issue exist and will be discussed later. [Trigg Talley, United States of America]	Taken into account. As discussed in the introductory paragraph of section 4.6.3.2, only the climate system responses are discussed. More elaborate discussion on CDR is provided in section 5.6 where is referred in this section.
40959	78	48	78	50	The glossary definitions distinguish between negative emissions for CO2 alone vs. for multiple greenhouse gases. The term 'negative greenhouse gas emissions' is used to refer to removal of multiple GHGs, while CDR only refers to removal of CO2. 'Net negative CO2 emissions' is the equivalent of 'Net negative greenhouse gas emissions' but only for CO2. The term 'Net emissions' isn't defined in the glossary but if it were, it would be the balance for multiple GHGs rather than just CO2. [TSU WGI, France]	Taken into account, text is revised for clarity throughout the section 4.6.3
115425	78	48	78	51	The term "negative emissions" or "Net negative emissions" is confusing and unscientific. If gases are emitted, they can not be negative. Therefore these expressions shouldn´t be used by a serious body as IPCC. Also, the use of CDR to englobe all kinds of carbon removal, including natural ones, such as forests and wetlands is also confusing. CDR in all litterature before SR1.5 and still in most litterature today is referred to and associated with TECHNOLOGICAL removals and withg geoengineering. Those promoting geoengineering are pleased that a body as IPCC confuses natural cycles with technological risky proposals, such as all geoengineering tecniques. PLEASE remove the use of these terms for natural carbon sinks. They are different in all senses and it is not helpful for neihter researchers or policy makers [SILVIA RIBEIRO, Mexico]	Taken into account. Text is revised extensively in response to this and several other comments on terminology. However, we retain and "negative emissions" and "negative net emissions" in the text as these are widely used now across the IPCC WGs now. CDR and negative CO2 emission refer to CO2 removals and not emissions.
12267	78	48			"negative net emissions" [Bryan Weare, United States of America]	Taken into account, it refers to CO2 removal from the atmosphere and this is widely used terminology and hence retained
93867	78	51	78	52	The maximum potential of CDR varies according to what? This sentence is not clear. [Quentin Lejeune, Germany]	Taken into account, text is revised for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112101	78	51	78	52	"the maximum potential of CDR varies (high confidence; see Chapter 5)"; what is meant as high confidence here - the maximum potential (which is absolutely wrong since that's highly uncertain), or simply that it varies (correct, but what does it vary acfross - technique, time, study)? Needs to be made more explicit. [Mark Lawrence, Germany]	Accepted, text is revised for clarity.
114517	78	52	78	52	Reference to SRCCL? [Jan Fuglestvedt, Norway]	Accepted, text is revised. All references are updated where required.
89729	78	52	78	52	Delete confidence statement as CDR potentials are not assessed in this chapter [Kirsten Zickfeld, Canada]	Accepted, text is revised
10247	78	53	78	53	This reference is not relevant to the text. Should it be the van Vuuren et al (2016) reference already in the reference list? [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text is revised. All references are updated where required.
114519	78	53	78	55	Unclear [Jan Fuglestvedt, Norway]	Accepted, text is revised
50937	78	56	78	56	expected to precede net negative emissions' - if possible please include by typically how long this is expected. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text is revised. This depends of the anthropogenic emissions and negative emission pathways as explained in the revision
12269	79	1	79	3	This needs a citation [Bryan Weare, United States of America]	Taken into account, text is revised extensively. The later sentence is deleted in the revision. All references are updated where required.
116369	79	1	79	4	Please check carefully if the following statement, "it should be cautioned that none of the CDR proposals has been proven to work in reality, especially at large scale, and their overall lifecycle emission balance raises questions about their carbon negativity" is the expression of the conclusion of an assessment (as done in SRCCL, also building on experiences of reforestation or afforestation), or is the expression of an opinion. [Valerie Masson-Delmotte, France]	Taken into account, text is revised. This sentence is deleted in the revision.
111835	79	1	79	4	Not sure whether this applies to large-scale afforestation [Oliver Geden, Germany]	Accepted, text is revised extensively. The later sentence is deleted in the revision.
111837	79	1	79	15	Not sure if you need to reflect on the status of CDR techniques since this is done extensively in ch5 [Oliver Geden, Germany]	Taken into account, Text is revised and discussion is shortened.
114521	79	2	79	2	Re "....should be cautioned that none of the CDR proposals has been proven to work in reality....": I dont think this is a valid statement. Needs reformulation and nuances in light of the various types of CDR. And I suggest coorination with WGIII [Jan Fuglestvedt, Norway]	Accepted, thank you. The text is revised. This sentence is deleted in the revision.
89731	79	2	79	2	"Has been proven to work in reality": I don't think this is correct as atated. The CO2 sequestration potential of natural methods such as afforestation, soil carbon sequestration has been proven to work. What has not been proven is that these methods can be scaled up. [Kirsten Zickfeld, Canada]	Accepted, the text is revised and is more nuanced now. This sentence is deleted in the revision.
96453	79	2	79	2	This statement needs to be more nuanced please. Plenty of CDR proposals have been proven to work in reality (i.e. observations show a CO2 sequestration) when you think of land-based CDR such as re/afforestation, with more mixed evidence also agricultural management changes such as no-/low-till, but also DACCS. [Nicole Wilke, Germany]	Accepted, the text is revised and is much more nuanced now. This sentence is deleted in the revision.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112103	79	2	79	3	"However, it should be cautioned that none of the CDR proposals has been proven to work in reality, especially at large scale" - here supporting references should be included, especially to the major reviews by Lawrence et al. (Nature Communications, 2018) and Nemet et al. (Environ. Res. Lett., 2018) (it is astounding that these reviews, along with the companion papers to Nemet et al. by Minx et al. and Fuss et al. are not cited at all here, and only Fuss et al. is cited in Chapter 5; these are relatively grave omissions since they provide very thorough overviews of the topic of climate geoengineering) [Mark Lawrence, Germany]	Accepted, the text is revised and is much more nuanced now. This sentence is deleted in the revision. All references are updated where required.
106301	79	2	79	4	This statement is unsupported by evidence and is only acceptable if CDR is explicitly defined. Afforestation and forest management are CDR measures that have been deployed, and some at large scale. The statement should be made sufficiently specific to avoid confusion in this regard. This is particularly confusing as in paragraphs further below, the term CDR is used to refer to both BECCS and afforestation/forest management when discussing the application of CDR in SR1.5 scenarios. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the text is revised in response to this and other similar comments. This sentence is deleted in the revision.
50939	79	2	79	4	"cautioned that none of the CDR proposals has been proven to work in reality" Does this include afforestation? If not this could be rephrased as: "Afforestation may be impermanent, and other CDR proposals are technologically immature, and unproven at large scale." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text is revised the revised text is more nuanced. This sentence is deleted in the revision.
93869	79	2			"none of the CDR proposals has been proven to work in reality, especially at large scale": this phrase is unclear and could prove very misleading, it needs to be revised. What are the mentioned "proposals"? Many CDR methods listed in Table 5.9 do work in the real world in the sense that their "technology" can already achieve long-term removal of carbon from the atmosphere. [Quentin Lejeune, Germany]	Accepted, the text is revised and is much more nuanced now. This sentence is deleted in the revision.
89733	79	3	79	4	carbon negativity: references missing [Kirsten Zickfeld, Canada]	Taken into account, text is revised. This sentence is deleted in the revision.
111853	79	6	79	15	If you keep this, it would be good to highlight how substantial the gross CDR numbers in 1.5C scenarios are (not only reporting the net negatives). Median across all scenarios was 730 Gt in 21st century (SR1.5, ch2) [Oliver Geden, Germany]	Accepted, and taken into account, text is revised extensively.
115427	79	9	79	11	These paragraphs are a good example of the previous comment. in SR 1.5 there is one scenario that does not include any geoengineering option - as it refers to afforestation, forest restoration, etc. But the confusion in the IPCC use of the terms have been useful for those promoting all kinds of geoengineering to say that IPCC is promoting CDR = geoengineering in ALL scenarios, whcih is not true [SILVIA RIBEIRO, Mexico]	Accepted, text is revised extensively for clarity.
112105	79	10	79	13	"the use *of* CDR", "can vary from *a* couple of" - add the text in **, and generally a language edit will be needed (presumably done in the technical review phase) [Mark Lawrence, Germany]	Accepted. Thanks. The text is revised for clarity

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106303	79	11	79	11	The reference to SR1.5 Chapter 2 is incorrect here: Rogelj, J., Shindell, D., Jiang, K., Fifita, S., Forster, P., Ginzburg, V., Handa, C., Kheshgi, H., Kobayashi, S., Kriegler, E., Mundaca, L., Séférian, R., Vilariño, M.V., 2018. Mitigation pathways compatible with 1.5°C in the context of sustainable development, in: Flato, G., Fuglestvedt, J., Mrabet, R., Schaeffer, R. (Eds.), Global Warming of 1.5 °C: An IPCC Special Report on the Impacts of Global Warming of 1.5 °C above Pre-Industrial Levels and Related Global Greenhouse Gas Emission Pathways, in the Context of Strengthening the Global Response to the Threat of Climate Change, Sustainable Development, and Efforts to Eradicate Poverty. IPCC/WMO, Geneva, Switzerland, pp. 93–174. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the text is revised extensively. All references are updated where required.
112107	79	11	79	13	it is noted that the SR1.5 says CDR would need to be available at scale well before 2050 to contribute to the 1.5C goal. Two major reviews (Nemet et al., Environ. Res. Lett., 2018; and Lawrence et al., Nature Communications, 2018) that considered scale-up timelines were published shortly before the SR1.5, but not in time for full consideration in the SR1.5. Both conclude that it is highly unlikely that any CDR technique can be implemented at scale before 2050 (noting that for net afforestation to be at scale, gross afforestation would have to exceed still existing gross deforestation levels). This should be included: "In stark contrast, two extensive reviews (Nemet et al., 2018; Lawrence et al., 2018) conclude that it is highly unlikely that any CDR technique can be implemented at scale before 2050." (It is also worth mentioning that this timeline is also discussed in the companion reviews by Minx et al. and Fuss et al., so they could also be cited here, but the topic is particularly focused on in Nemet et al.) [Mark Lawrence, Germany]	Accepted. The text is revised extensively. All references are updated where required.
50947	79	12	79	12	suggested edit if accurate: 'socially acceptable CDR becomes feasible and available at a global scale well before..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
89735	79	12	79	12	"will be difficult to realize": statement implies value judgment that is out of place here [Kirsten Zickfeld, Canada]	Accepted, text is revised.
106305	79	12	79	13	This statement is too generalizing. Scenarios with no or limited overshoot in the SR1.5 can also be realised without this condition, noting that forest management is an option that is readily available. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text is revised for FGD.
50941	79	13	79	13	"a couple" is slightly colloquial and may not translate well into other languages; suggest replace with "approximately two" if that is what is meant. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text is revised
106307	79	13	79	14	The primary source for this statement is also IPCC SR1.5 Chapter 2. Waisman et al puts insights from that report in a broader context. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; text is revised for FGD. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96455	79	17	79	22	"such as RCP8.5 (Keller et al., 2014; González et al., 2018)" -- the equivalent study to Gonzalez (which is ocean-based CDR in the MPI-ESM (while Keller used an EMIC)) for land-based CDR is Sonntag et al., 2016 10.1002/2016GL068824). It should be added to "the effectiveness of a specified amount of CO2 removal is small for large background CO2 concentrations as CO2 radiative forcing scales with the logarithm of CO2 concentration" that "although the efficiency of a given CDR measure, such as reforestation, to take up CO2 may increase with higher CO2 concentrations (Sonntag et al 2016)". Some methods are highly non-linear with the scenario. [Nicole Wilke, Germany]	Accepted. Text is revised extensively for FGD. All references are updated where required.
96457	79	17	79	22	The term "limited" [cooling] is not well defined. In fact, by trying to falsify the opposite statement "unlimited cooling" as a check, the statement in the current text seems trivial. It would be very important to find a semantically better defined term to describe the findings from CDRMIP and similar findings. [Nicole Wilke, Germany]	Accepted. Text is revised extensively for FGD.
87855	79	17	79	23	Please make sure that this section is consistent with Chapter 5 5.6.2.1.2: Effectiveness of CDR. Currently, some text here somewhat contradicts Chapter 5 findings, which conclude that earth system response to CDR has been extensively studied among many more studies (not listed here, but cited in CH5). [Katarzyna Tokarska, Switzerland]	Taken into account. The text is extensively revised now. It is made consistent with chapter 5 and many more modelling studies on net negative CO2 emissions are cited now.
114523	79	17	79	29	I find this para somewhat unclear. [Jan Fuglestvedt, Norway]	Taken into account. In response to this and other comments, this para is revised and we hope the revised para is clearer.
87863	79	17	79	49	It would be helpful to summarize the climate responses to CDR based on different studies using different models, to make this section more comprehensive, and not only focused on one study of Keller et al. 2018. There is a large body of literature of exploring earth system response to CDR that arrives at similar conclusions and is not even mentioned here or referred to (e.g. Jones et al. 2016 ERL, Tokarska and Zickfeld 2015, ERL). Please see Chapter 5 that describes studies beyond the CDR-MIP. [Katarzyna Tokarska, Switzerland]	Taken into account. Text is revised. We hope the revised text is more comprehensive. All references are updated where required.
87853	79	18	79	19	This first sentence is not true, as there is a number of studies that looked at the CDR response for all RCP scenarios (e.g. MacDougall 2013 GRL; Zickfeld et al. 2016, ERL), for RCP 2.6 (e.g. Jones et al. 2016, ERL), or idealized emission scenarios that entail plausible rates of CO2 emission reduction to stabilize global mean temperature at 1.5C or 2.0C level (e.g. Tokarska and Zickfeld, 2015, ERL), or idealized scenarios (Schwinger and Tjiputra, 2019, GRL). This entire paragraph gives a false impression that CDR MIP is one of the first efforts to look at the earth system response to CDR, while, in fact, there are plenty of studies that arrived at similar conclusions. Please consider re-writing, and for consistency with Chapter 5 that mentiones a lot more studies. [Katarzyna Tokarska, Switzerland]	Accepted, Text is revised extensively for FGD. All references are updated where required.
89737	79	20	79	20	Define effectiveness [Kirsten Zickfeld, Canada]	Taken into account. This sentence is removed in the revised draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89739	79	20	79	22	Include reference (e.g. Zickfeld et al., 2020) [Kirsten Zickfeld, Canada]	Taken into account, text is revised. All references are updated where required.
87857	79	22	79	23	This sentence may sound controversial, as several previous studies lead to consistent conclusions regarding the climate impacts and efficiency of artificial carbon dioxide removal from the atmosphere - see Tokarska and Zickfeld 2015, ERL; and Jones et al. 2016 ERL, for example, and the earlier studies cited therein. Those studies already showed the reduced efficiency of carbon sinks under negative emissions, and outgassing when the emissions are net-negative. [Katarzyna Tokarska, Switzerland]	Accepted. Good comment. Text is extensively revised for FGD. All references are updated where required.
89741	79	23	79	23	"efficacy": I suggest to be consistent in the termonology and use the term "effectiveness" throughout (unless you are referring to something different with "efficacy"). [Kirsten Zickfeld, Canada]	Accepted, text is revised
50943	79	24	79	25	"initiated to explore the potential of impacts, potential and challenges of CDR". To avoid using the word "potential" in 2 different ways, suggest this could be rephrased to say: "initiated to explore the impacts, potential and challenges" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text is revised.
89743	79	25	79	29	It should be pointed out that the CDR needed to achieve a 1%/yr decline in CO2 concentration largely exceeds the rates of removal deemed peasible. [Kirsten Zickfeld, Canada]	Taken into account, we use the term "idealized" which indicates that it may not be realistic. Idealized studies help to understand the science
89745	79	25	79	50	Given the unrelaistic amount of CDR implied in the CDR-MIP 1% ramp-up, ramp-down experiments, I question that these experiments are suitable to assess the climate effects of CDR. Scenarios with relaitic amount os removal e.g. SSP1-2.6 in my view are better suited to investigate the climate response to CDR. In the CDRMIP framework, the 1%/yr experiments were intended to investigate the reversibility of changes in climate system components rather than the climate response to CDR. [Kirsten Zickfeld, Canada]	Taken into account. Good point. Text is revised. The ramp down phase in CDRMIP represents the net negative CO2 emissions. The revised text and the subsection title clarifies this important point. The other papers that investigate the effect of net negative emissions are discussed in the revised text.
50945	79	28	79	28	The use of the word "mitigation" is inconsistent with the definition in the glossary. Suggest replace with "emissions reduction". [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, Text is revised
87859	79	28	79	29	Please note that other studies that arrive at similar conclusions using different CO2 emission scenarios (and perhaps less idealized than 1pct CO2 only runs). e.g. Jones et al. 2016 ERL; Tokarska and Zickfeld 2015, ERL. Also, a reference to Chapter 5 would be useful here, where the climate-carbon response to CDR is discussed in detail. [Katarzyna Tokarska, Switzerland]	Taken into account. Text is revised. All references are updated where required.
82083	79	29	79	29	It is stated the the CDRMIP experiment CO2 ramp-down could be due to mitigation or CDR or a combination of both. It is not clear to me how such a large ramp down could be due to mitigation alone. [David Keller, Germany]	Taken into account. Excellent point. Text is revised.
96459	79	40			Figure 4.40, legend: Please verify nomenclature of sea level: "f) thermostatic sea level" ("thermosteric" according to glossary; please see glossary entry, page AG-43, line 51-52). [Nicole Wilke, Germany]	Taken into account, caption and text revised to clarify that it is thermosteric sea level that we have shown in the figure. Glossary is also referred to in the revision.
87861	79	46	79	46	Rather than referring to models "non-CMIP", it would be better to call them EMICs? [Katarzyna Tokarska, Switzerland]	Taken into account. Non-CMIP6 refers to both EMICs and CMIP5 models. Text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111839	79	53	80	33	Again, this is mainly about "net negative emissions" not about CDR, should be highlighted in the subsection title [Oliver Geden, Germany]	Taken into account, text is revised. We agree and add a sentence that explicitly states that the ramp down phase represents the net negative CO2 emission scenario.
116347	79		79		The section on CDR needs to build more on SRCCL and be coordinated with WGIII [Valerie Masson-Delmotte, France]	Taken into account, Text is revised extensively in response to this and several other comments
50953	80	1	80	1	response to idealised CDR deployment' - and/or emissions reductions? It's a combination of CDR and mitigation that pulls atmos CO2 down. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Good point. Text is revised.
90829	80	3			Additional info "The effects of MCB on seasonal polar temperatures and heat flux"? [Vivien How, Malaysia]	Taken into account. Comment seems out of context. This probably refers to line 3 page 83. The discussion on MCB is revised.
89747	80	4	80	5	"lag behind the deplyment of CDR": as stated earlier in the section, CDR deplyoment does not necessarily result in CO2 decline, therefore replace with "lag behind the decline in CO2". [Kirsten Zickfeld, Canada]	Taken into account, Thanks for the comment. Text is revised.
96461	80	4			Please specify whether this section only refers to "thermosteric" sea level. [Nicole Wilke, Germany]	Taken into account, Text is revised. Yes, this refers to only thermosteric sea level
9813	80	6	80	6	"global sea level" -> "global mean sea level" [Robert Kopp, United States of America]	Accepted. Text is revised. Thanks
17035	80	6	80	6	It would be beneficial if guidelines are provided as to how and what alternatives to be explored when land and properties are lost, so that research organizations, insurance companies and entrepreneurs could focus on those alternatives. The guidelines must stress on the importance of low-cost relocation programs that includes low cost housing built by '3D printing' machines and 'robotic' technology to drive down the cost and to ease the burden on the economy. [Ravi Amblee, United States of America]	Accepted but this is beyond the scope. Impact assessments are not made in this section
50949	80	6	80	10	There are important points here not currently reflected in the SPM, i.e. that sea level rise will not be reversed by CDR on human timescales and land that is lost to sea level rise will not be retrieved (high confidence). Suggest it would be helpful to highlight in the SPM (section D2) that CDR may not remove some significant risks, and links to necessary adaptation. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now included in the Executive summary. The decision to take this message to SPM depends on the SPM writing team
9815	80	9	80	14	This is consistent with ch 9, but be aware that an assessment of the reversibility of GMSL rise is made in 9.6.3.5 ["Thus, the few studies conducted to date suggest that geoengineering may be effective at reducing the yet-to-be-realized sea-level commitment but is ineffective at reversing GMSL rise (low confidence). "] [Robert Kopp, United States of America]	Taken into account, text is revised to cite this section. Thanks for providing this information on the X-chapter issue.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7817	80	10	80	14	This is very misleading. Most of the models in Fig 4.40 show an overshoot. The one that does not could show changes after the simulation stopped. In Sgubin many models did not show an overshoot because the simulation only ran until GHG were reversed, and from Fig 4.40 you can see that the overshoot often happens after this.Other relevant papers explaining reasons for an AMOC overshoot are Jackson et al, 2015DOI 10.1007/s00382-013-1842-5 and Wu et al 2011 DOI: 10.1029/2011GL049998. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised. All references are updated where required.
53099	80	18			Also refer to Section 8.2.1 [Hervé Douville, France]	Taken into account, text is revised. This sentence is deleted in the revision.
89749	80	31	80	32	"coolong effectivess… is found to be less". Include reference. [Kirsten Zickfeld, Canada]	Taken into account. Text is deleted in revision
87865	80	36	80	36	It would be helpful if this paragraph provided a distinction between a direct carbon dioxide removal from the atmosphere (e.g. direct air capture) what has much fewer impacts on land and ocean ecosystems, vs. other methods that have more impacts for ecosystems (e.g. land and ocean-based solutions). Different CDR methods have a different level of interference with the ecosystems and impacts, and this should be clarified. Currently, this paragraph is mixing up different technologies, which each has very different impacts on ecosystems. [Katarzyna Tokarska, Switzerland]	Rejected. Beyond the scope of this subsection. A detailed discussion of the various CDR options is discussed in section 5.6 as indicated in the revised text
112111	80	37	80	52	This paragraph is a selection of various relevant points that are individually correct, but not the most significant ones to cover under this topic. It does not represent the extensive literature well. The authors should carefully read the companion reviews by Minx et al., Fuss et al. and Nemet et al. in Environ. Res. Lett. (2018) and the review by Lawrence et al. (Nature Communications, 2018), which give a more comprehensive coverage of the cooling potential and side effects, and use these and the references therein to structure this better and focus on the most important points to bring across. [Mark Lawrence, Germany]	Taken into account, text is revised extensively for FGD. All references are updated where required.
106309	80	38	80	39	The primary source for this statement is also IPCC SR1.5 Chapter 2. Waisman et al puts insights from that report in a broader context. Also SR1.5 SPM Figure 3b illustrates this. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised extensively for FGD. All references are updated where required.
114525	80	39	80	39	More references than this is needed. [Jan Fuglestvedt, Norway]	Taken into account. The sentence is removed in revision. Text is revised extensively. All references are updated where required.
114527	80	39	80	40	What is this based on? The paper you cite in the sentence before? And the "very likeley" indicate that you have done an assessment here. I think a much stronger basis is needed here, and again, some collaboration with WGIII coudl be useful. Alternatively, leave out any assessment of the feasibility of CDR [Jan Fuglestvedt, Norway]	Taken into account. Text is revised extensively for FGD. Only a confidence level is now given.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83713	80	39	80	44	Please clarify whether forestry is included in CDR. If so, this section should also highlight the potential co-benefits (i.e. forestry can enhance water quality and biodiversity). The authors could also consider including the pros and cons of seaweed as a CDR (as this has also garnered interest/research). [Dan Zwartz, New Zealand]	Rejected, As discussed in several instances in this section, an exhaustive discussion of CDR methods, potential and side effects are discussed in section 5.6. This section assesses only the climate response to CDR
10249	80	40	80	44	As with the comment above to page 8, this sentence assumes only 1 CDR option is deployed whereas nearly all experts on CDR/NETs believe that it is much more likely that a portfolio of CDR/NETs options will be deployed. This needs to be recognised in the report and modelling of multiple CDR options needs to be recommended. I suggest inserting the following new text after this sentence to deal with this point "However, it is widely recognised by relevant experts that a portfolio of CDR/NETs options is more likely to be deployed that a single option e.g. Caldeira et al (2004) and Project Drawdown (2020) Key Insight 2". References: Caldeira et al. (2004) Ch 5 'A Portfolio of Carbon Management Options' In 'The Global Carbon Cycle: Integrating Humans, Climate, and the Natural World', Editors: C.B. Field and M.I. R. Raupach, Island Press and National Research Council. (2015) Climate Intervention: Carbon Dioxide Removal and Reliable Sequestration. Washington, DC: The National Academies Press. https://www.researchgate.net/publication/258432218_A_Portfolio_of_Carbon_Management_Options and https://islandpress.org/books/global-carbon-cycle. Project Drawdown (2020) The Drawdown Review 2020. Climate Solutions for a New Decade. A Project Drawdown Publication, 104 pp. https://www.drawdown.org/drawdown-framework/drawdown-review-2020 [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and Taken into account. Text is revised extensively for FGD. All references are updated where required.
50955	80	41	80	41	as currently deemed possible' - e.g. with bioenergy growth constrained by land and water availability? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the specific study cited here, Sahara and the Australian deserts are afforested to avoid land availability issues. Yes, the afforestation and other scenarios considered in the cited study are possibly unrealistic. Text is revised
87867	80	44	80	44	Please note that several other studies look at impacts of CDR methods - see Chapter 5. It is unfair to cite only one reference. [Katarzyna Tokarska, Switzerland]	Accepted, text is revised. Many studies are now cited. Section 5.6 is also cited here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106189	80	44	80	44	Talking about side-effects of CDR here, it would be good to put these not just into a climate stabilisation contaxt but an Earth system context. Heck et al., Nature Climate Chang,8, doi.org/10.1038/s41558-017-0064-y, 2018 show that biomass-based CDR can help maintain the climate planetary boundary (Rockström et al., 2009; Steffen et al., 2015) but implies transgressing further or putting more pressure on other land-related planetary boundaries; if all planetary boundaries of the planetary boundaries are respected, there is no or only a small remaining potential for tCDR Reference to this would put the climate discussion of this chapter into the wider context of an Earth system discussion (cf. Anthropocene). [Wolfgang Lucht, Germany]	Taken into account, text is revised. However, the discussion on planetary boundaries beyond the scope here. Scope is limited to climate response to CDR. All references are updated where required.
89751	80	44	80	47	Side effects of CDR methods are assessed in section 5.6 [Kirsten Zickfeld, Canada]	Accepted, side effect are not assessed in the revision
53101	80	45			availability of land and water? [Hervé Douville, France]	Accepted, text is deleted in the revision.
103029	80	46	80	47	sentence reads '... the related impacts on terrestrial ecosystems in the case ocean alkalisation are some of the side effects.' Add the word 'of' before ocean. [Philippe Tulkens, Belgium]	Taken into account, Text is deleted in revision
103031	80	46	80	47	sentence reads '... the related impacts on terrestrial ecosystems in the case ocean alkalisation are some of the side effects.' Correct 'alkalisation' to 'alkalinisation'. [Philippe Tulkens, Belgium]	Taken into account, Text is deleted in revision.
103033	80	46	80	47	sentence reads '... the related impacts on terrestrial ecosystems in the case ocean alkalisation are some of the side effects.' Aren't there side effects on marine ecosystems? [Philippe Tulkens, Belgium]	Accepted, thanks. Text is deleted in revision
12275	80	49	80	50	conversition of forest to cropland is not afforestation. [Bryan Weare, United States of America]	Accepted, text is revised
50951	80	49	80	52	Suggest this should say ' increased albedo' as the conversion is from from forest to cropland? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, thanks. Text is revised
5651	80	49	80	52	Please check and correct: afforestation is e.g. the conversion of cropland to forest, not, as you write here, the other way round. [Joachim Rock, Germany]	Accepted, thanks. Text is revised
83717	80	54	81	9	More explanation of what is meant by terminating the CDR would be helpful. [Dan Zwartz, New Zealand]	Taken into account, Text is revised.
87871	80	54	81	9	Please note that this paragraph ignores an entire body of literature on earth system responses to net-negative emissions. It would be good to cite papers from different groups, in addition to the Keller et al. study, especially that other studies use different models and negative emission pathways, to provide a more balanced view. Please see that Chapter 5 cites many other studies. [Katarzyna Tokarska, Switzerland]	Taken into account, Text revised. This section discusses specifically the termination effects of CDR and we assess only papers that have investigated the termination effects. All references are updated where required.
111841	80	54	81	9	Again, this is mainly about "net negative emissions" not about CDR, should be highlighted in the subsection title [Oliver Geden, Germany]	Taken into account. As stated, Termination effects have been assessed in the literature only for scenarios where there is CDR but no net negative emissions. Text on termination is revised
87869	80	55	81	2	Please note that this is not true, as earlier studies considered net-negative emissions - e.g. Jones et al. 2016 ERL; Tokarska and Zickfeld. 2015 ERL and Tokarska et al. 2019 Earth's future; and MacDougall et el. 2013 GRL. [Katarzyna Tokarska, Switzerland]	Accepted. The text is revised. This section discusses specifically the termination effects of CDR and we assess only papers that have investigated the termination effects. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106311	81	3	81	4	Please clarify "termination". [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text changed to "termination of CDR".
50957	81	4	81	6	This sentence is not clear - should it say: 'are too small to be detected when CDR is terminated? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text is revised. Comparison is to the rate of warming under SRM termination
19233	81	4	81	6	"increases in global mean warming rates are too small": what is the meaning of this sentence? [Anne-Marie Treguier, France]	Taken into account, text is revised. Comparison is to the rate of warming under SRM termination
54965	81	4	81	6	This sentence about the effects of terminating CDR seems to be missing something. Should it say that the global mean warming rates following termination are too small to be detected? (otherwise "too small" doesn't make sense). [Nancy Hamzawi, Canada]	Taken into account, text is revised. Comparison is to the rate of warming under SRM termination
89753	81	5	81	5	Unclear what is meant by "too small" [Kirsten Zickfeld, Canada]	Taken into account, text is revised. Comparison is to the rate of warming under SRM termination
82085	81	5	81	5	It is stated that "the increase in global mean warming rates following termination are too small". I'm not sure that "too" is the right word here. I think that "very" or "relatively" would be better, although this does not apply for all CDR methods examined in Keller et al., 2014 as termination of artificial upwelling resulting in a large increase in warming. Perhpas this should be clarified. [David Keller, Germany]	Taken into account, text is revised.
93411	81	6	81	6	The ref González et al should be Ferrer González et al [Carles Pelejero, Spain]	Taken into account, Last name is always used to cite references. We find this paper is cited as González et al in other papers too. All references are updated where required.
132517	81	7	81	9	I do not understand how the cessation of CDR could produce a similar rate of warming as produced by a cessation of SRM. If you cease CDR, don't you just start slowly growing atmospheric CO2 concentrations again? What am I missing? Are there any references for this claim? [Kyle Armour, United States of America]	Taken into account. This particular cited study uses rather larger addition of ocean alkalinity and stops the addition in 2070 but emissions follow RCP8.5 scenario. Text is revised
50961	81	12	81	12	Add '(SRM)' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. 'SRM' is added.
9817	81	12	87	33	This section reads quite differently from much of the chapter and to some extent reads like a very long cross-chapter box. [Robert Kopp, United States of America]	Taken into account. We have substantially revised this section. Sections on SRM and CDR appear of out of place. However, this is how the scope for Chapter 4 has been defined by WG1.
114529	81	12	87	33	Sometimes it is a bit uncelar whether you use RF or temp when you talk about offsetting warming. [Jan Fuglestvedt, Norway]	Taken into account. We have revised the text. It generally refers to offsetting of temperature unless otherwise specified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112261	81	12	87	33	In Section 4.6.3.3 ("Climate Response to Solar Radiation Modification"), including in Table 4.7, one SRM scheme is missing that has recently been studied, including one climate-model based assessment, namely "Arctic Ice Management". The climate-model based study (Zampieri and Goessling 2019: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019EF001230) found that, with Artic Ice Management, it is in principle possible "to keep the late-summer sea ice cover at the current extent for the next ~60 years", but the effect was found to be strongly confined to the Arctic. This and the fact that the late-summer Arctic cooling is accompanied by a winter Arctic warming implies a very small effect on annual-mean GSAT of about -0.02K, despite the significant intervention in the ice-albedo feedback. The corresponding global-mean solar radiative forcing was found to be -0.08 W/m**2. [Helge F. Goessling, Germany]	Taken into account. In the revised text, we add a sub-section briefly discussing surface and the regionally focused high latitude schemes, including the 'Arctic Ice Management' scheme. All references are updated where required.
80025	81	12	87	33	The whole section on SRM (and/or the cited papers in there) often compares impacts of SRM to a “low CO2 world”. However, impacts of SRM should always be compared to future climate change scenarios without SRM. That is the only appropriate comparison, because in a low-CO2 world we would not need any SRM, but we want to know it we would be better off in a world with SRM and climate change or in a world with climate change only. [Gabriel Chiodo, Switzerland]	Taken into account. In the revised text, we generally compare the world with SRM to the world with high-CO2 and no SRM. The figure in this section does compare SRM effects relative to high CO2-world
112115	81	12	87	33	I could not find anything about the timescales to potential full-scale implementation of SRM in this entire section. This is a very important point that should be included, because otherwise the "rapid response" point that is made gives the misimpression that it could also be implemented quickly. Numerous studies give evidence that for a variety of reasons, an implementation at scale before 2050 is highly unlikely to be acheived. An assessment of this issue is provided in Lawrence et al. (Nature Communications, 2018). [Mark Lawrence, Germany]	Taken into account. This section mainly assesses climate response to SRM. The timescale of potential SRM implementation is beyond the scope of this assessment. However, the timescale of cooling due to abrupt turning on of SRM is discussed. All references are updated where required.
107011	81	12			I would conclude the SRM section by a summary paragraph with a clear message about the assessment of SMR. I would conclude based on the balance of evidences listed here that SRM would induce much more negative effects and open the world to major thread. A clear message from AR6 should come out from this section. [Christophe CASSOU, France]	Taken into account. A summary paragraph is added. Our assessment is based on available literature.
2399	81	14	81	14	The "introduction" subheading is not necessary. [Vaishali Naik, United States of America]	Accepted. The subheading is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112113	81	15	81	15	It is acknowledged in this paragraph that the definition given here of SRM makes it a misnomer, since it leaves out CCT, but then the use of SRM is simply perpetuated. It should at least also be acknowledged that functional alternative terminology has been proposed and is in use in various contexts, e.g., "Radiative Forcing Geoengineering" used in the review by Lawrence et al. (Nature Communications, 2018), as well as "SRM and related techniques" and simply "Radiation Management", used in the EuTRACE and German BMBF assessment reports [Mark Lawrence, Germany]	Taken into account. We are aware of different definitions of SRM. Here we define SRM in a way that is consistent with SR1.5. We have revised the paragraph that introduces SRM.
5779	81	15	81	16	Consider defining SRM as having the aim of reducing *climate change*, as SRM could generally bring changes in precipitation, extreme T and P, and tropical cyclone intensity closer to pre-industrial levels -- albeit imperfectly -- as well. [Jesse Reynolds, United States of America]	Taken into account. Corresponding text is revised.
5781	81	15	81	16	This definition is somewhat contradicted by the inclusion of cirrus cloud thinning in SRM, at Ch 4 p. 81, lines 22-27 [Jesse Reynolds, United States of America]	Taken into account. We follow the definition of SRM used in SR1.5. To avoid confusion, We have revised the paragraph that introduces SRM.
40955	81	15	81	16	The glossary definition just says"reducing warming" rather than "reducing GHG-induced warming". This needs to be consistent. Let the TSU know if you wish to add "GHG-induced" to the glossary definition, but note that black carbon also causes warming. [TSU WGI, France]	Taken into account. Text is revised in response to this comment and several other similar comments
50959	81	15	81	27	SRM is defined here as a modification of short wave radiation, however, lines 25 to 27 (p81) contradict this, saying the definition of SRM includes longwave radiation modification,and that this usage is consistent with SR1.5. However, in SR1.5 glossary (bottom of p158), SRM is defined only in terms of short wave radiation. Please ensure that AR6's glossary is consistent with that of SR1.5, and terms used throughout as defined in the glossaries, so SRM refers to shortwave modifications. Then, cirrus cloud thinning (longwave radiation modification) can be discussed as an extra technique, which is not strictly SRM. Table 4.7 would then need a different title, unless cirrus cloud thinning is removed from the table. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We agree that cirrus cloud thinning is not strictly SRM, but our SRM definition is consistent with what is in SR1.5. SR1.5 uses SRM to refer to all direct interventions on the planetary radiation budget, including cirrus cloud thinning. (Chapter 1 of SR1.5, p70-71). Also, the table in SR1.5,which summarizes the main characteristic of SRM, includes cirrus cloud thinning. To make this point clear, we have revised the paragraph that introduces SRM.
68667	81	16	81	16	the ~2% decrease, as stated by the Royal Society, has been revisited in Kravitz et al., 2014. A range of decrease can be given instead based on 12 GeoMIP models. [Simone Tilmes, United States of America]	Taken into account. As indicated, it is only approximate. Text is revised. All references are updated where required.
80005	81	16	81	18	This statement could be misleading in the sense that it gives the reader the impression that the goal of radiation management must be to re-create a preindustrial climate or offset the warming of a doubling of CO2. However, it will not be possible to re-create a preindustrial climate and offsetting the warming of a doubling of CO2 should not be strategy behind climate engineering. This must be stated somewhere (e.g. Keith & MacMartin, 2015). Same applies for table 4-7 on page 4-82. [Gabriel Chiodo, Switzerland]	Taken into account. We agree that the goal of SRM is not to offset 2xCO2 warming. We have revised the text and table. All references are updated where required.
96463	81	17	81	18	Please specify reference : "[…] to offset all of the warming from a doubling of CO2" - "relative to an assumed high-GHG-world" (please compare page 85, line 16)? [Nicole Wilke, Germany]	Taken into account. In the revised text, we have re-structured corresponding statements and added corresponding references.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26875	81	18	81	18	We suggest to add "concentration" after "doubling of CO2" [Eric Brun, France]	Taken into account. Text is revised.
19463	81	18	81	21	It is important to emphasize that efficacy and side effects are dependent on the deployment scenario. Everybody is aware that the cost and benefit of mitigation depends on how much GHG emissions are reduced. But that's not appreciated for SRM and this point should be clarified. [Masahiro Sugiyama, Japan]	Taken into account. We have re-structured the corresponding discussion.
5783	81	22	81	27	This inclusion of cirrus cloud thinning in SRM somewhat contradicts the definition of SRM given above and in the glossary. [Jesse Reynolds, United States of America]	Taken into account. We agree that cirrus cloud thinning is not strictly SRM, but we ensure that SRM definition is consistent with what is in SR1.5. SR1.5 uses SRM to refer to all direct interventions on the planetary radiation budget, including cirrus cloud thinning. (Chapter 1 of SR1.5, p70-71). Also, the table in SR1.5,which summarizes the main characteristic of SRM, includes cirrus cloud thinning. To make this point clear, we have revised the paragraph that introduces SRM.
115549	81	24	81	24	suggest replacing could be achieved by has been suggested to be achievable or similar [Rolf Müller, Germany]	Taken into account. We have re-structured corresponding discussion and revised text.
1825	81	24			Change "could" to "might" We have not idea how easy it would be to do. [Alan Robock, United States of America]	Taken into account. We have re-structured corresponding discussion and revised text.
80007	81	25	81	27	I would clearly separate techniques that try to alter the shortwave radiation from the ones that try to alter the longwave radiation as impacts and effects on climate clearly differ. Using the term solar radiation management for cirrus cloud seeding is simply wrong. I would refer to all techniques (solar and longwave radiation management techniques) with the term RM (radiation management) or RE (radiation engineering), without specifying any further. This avoids confusion. [Gabriel Chiodo, Switzerland]	Taken into account. We agree that cirrus cloud thinning is not strictly SRM, but we ensure that SRM definition is consistent with what is in SR1.5. SR1.5 uses SRM to refer to all direct interventions on the planetary radiation budget, including cirrus cloud thinning. (Chapter 1 of SR1.5, p70-71). Also, the table in SR1.5,which summarizes the main characteristic of SRM, includes cirrus cloud thinning. To make this point clear, we have revised paragraph that introduces SRM.
80003	81	28	81	28	injections of sulphate aerosols have been proposed for the stratosphere, not the troposphere. Please replace “troposphere” with “stratosphere” [Gabriel Chiodo, Switzerland]	Taken into account. Some regional options by Mike MacCraken (2016) do propose injection into the Arctic troposphere. Text is revised to make it clear.
108013	81	32	81	36	The Stjern 2018 study cited is not the most applicable GEOMIP experiment to the assessment of MCB. Stjern 2018 evaluates G4CDNC, which increases cloud droplet number concentration by 50% in all low clouds. A better study to cite is Ahlm 2017, which analyzes the G4-Sea Salt experiment, which more closely represents proposed MCB interventions. Ahlm, Lars, Andy Jones, Camilla Weum Stjern, Helene Muri, Ben Kravitz, and Jón Egill Kristjánsson. "Marine cloud brightening–as effective without clouds." Atmospheric Chemistry and Physics 17 (2017): 13071-13087. http://dx.doi.org/10.5194/acp-17-13071-2017 [Kelly Wanser, United States of America]	Taken into account. The citation of Ahlm (2017） is added in the subsection on MCB. All references are updated where required.
108015	81	32	81	36	for the MCB entry in the SRM table: Neither of the cited studies support the 75% of ocean area claim. [Kelly Wanser, United States of America]	Taken into account. In the revised table, we replace the column 'Potential for countering a warming from a doubling of CO2' to 'radiative forcing potential'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41967	81	33	82	1	Table 4.7 column 3 says that marine cloud brightening needed to offset double CO2 would require treatment of nearly 75% of the ocean area. This is much too high. Jones Haywood and Boucher in https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008JD011450 showed that just 3.3% of the earth's surface most suitable area would offset 0.97 watts per square metre. If marine cloud brightening was used in parallel with CO2 reduction, let alone CO2 removal it would not be necessary to offset a doubling. Where did the 75% come from? What else did the 75% person write? Should it have been 7.5% for a more likely amount of cooling? [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised. In the revised table, we replace the column 'Potential for countering a warming from a doubling of CO2' to 'radiative forcing potential'. The discussion on MCB is expanded is now expanded and revised. All references are updated where required.
41969	81	33	82	1	Column 4 of table 4.7 says that there are uncertain regional changes in precipitation patterns but column 5 cites Stjern et al. https://www.atmos-chem-phys.net/18/621/2018/acp-18-621-2018-supplement.pdf. which shows that these are beneficial. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised. All references are updated where required.
41971	81	33	82	1	Column 4 of table 4.7 on page 81 also mentions salt fallout on land. The amount of salt put in to the atmosphere from the sea is difficult to measure and so there is a wide range of estimates. Recent ones are clustered around 6 billion tonnes of salt per year see for example Grini at doi.org/10.1175/1520-0442(2002)015%3C1717:MTACOS%3E2.0.CO;2. This rate is about 200,000 kilograms a second with a very wide range of drop sizes, in enormous quantities during hurricanes. The salt content of spray from one vessel is about one kg per second. If Twomey and Köhler are right about spray quantity and nucleation and we have mono-disperse spray with drop diameters in the sweet spot in the right places, the amount of salt needed to offset the present world thermal damage since preindustrial times is about 300 kilograms a second. We want to work in the pristine air of mid-ocean so much of the salt that we produce will fall back to the sea. However some will reach land. There is already lots of spray from waves breaking on beaches and much of this will blow inland. Lots of salt is spread on land by tidal surges of hurricanes. The salinity of the oceans has remained stable over millions of years because salt which comes ashore is washed back down rivers by rain. There are a few places, in Saudi Arabia and bits of Australia with levels of rainfall which are so low that salt has been building up. Some salt from marine cloud brightening will also get to such places. But there is so much there already that more will not make any difference. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The assessment of sea salt deposition on land is highly uncertain. We removed this statement in the table and just briefly mention it in the text. All references are updated where required.
4155	81	33	82	2	Inclusion of monsoons (and other phenomena) in this summary table of potential impacts of SRM and SAI is a very useful collation of information for policymakers and interested scientists and is a good advance over previous reports. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text in the table is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112119	81	33	82	2	Maximum expected acheivable values based on a scientific review of the literature are given in Lawrence et al. (Nature Communications, 2018). It would be much more consistent to use that review as a self-consistent primary source for the values in the table, and to list and discuss different impressions where the IPCC authors find them (especially based on newer literature) [Mark Lawrence, Germany]	Taken into account. In the revised table, we replace the column 'Potential for countering a warming from a doubling of CO2' to 'radiative forcing potential', and assess the cooling potential of each SRM option based on available literature. All references are updated where required.
19465	81	35	81	35	Related to the point above, caution should given to indicate that SRM doesn't have to be used at the full scale. [Masahiro Sugiyama, Japan]	Taken into account. In the revised table, we replace the column 'Potential for countering a warming from a doubling of CO2' to 'radiative forcing potential', Also, in the revised text, we discuss climate response when SRM is not implemented at the full scale.
127585	81	35	81	35	It is worth pointing out in the Table 4.7 caption that the side effects column is listing features of the climate system where there are likely to be differences with present day, or pre-industrial climates, but that these effects are frequently much smaller than the effects on the same features by GHG forcing in the absence of SRM. Perhaps it would be useful to distinguish key side effects where the impact is certainly worrying (like increased UAV at the surface), from those where there is some ambiguity (e.g., monsoon precipitation). [Trigg Talley, United States of America]	Taken into account. In revised Table, we changed the title of the column to 'key climate and environmental effects' to reflect these points in a more neutral way.
127587	81	35	81	35	The list of key side effects is problematic, particularly for SAI. The entry is very one-sided. For example, the change in ratio of direct and diffuse light can actually increase crop yields (e.g., Pongratz et al., 2012), and the cooling can decrease heat and water stresses and so increase crop yields (Pongratz et al., 2012; Parkes et al., 2015). Similarly, although geoengineering does change monsoons, those changes are much much smaller than the changes to the monsoons in a BAU scenario (e.g., Nalam et al, 2018). Very little detail can be provided in a table, but it needs to be done in a more neutral way, and the current entries are not balanced. Chapter authors Bala and Kravitz should be able to straighten this out. [Trigg Talley, United States of America]	Taken into account. In revised Table, we changed the title of the column to 'key climate and environmental effects' and discuss these effects in a more neutral way. All references have been updated where required.
17033	81	35	81	35	Table 4.7: Column: "Key side effects" : "potential decrease in crop yields" Comment: It would be highly beneficial for the farmers who would suffer from decreased crop yields, if feasible guidelines are provided, to develop "vertical farming" which can survive independent of rainwater. [Ravi Amblee, United States of America]	Taken into account. In revised Table, we write "changes in crop yield". Providing guidelines such as vertical farming is beyond the scope of this section.
96465	81	35	81	35	Please use primary literature for land-based SRM: Davin et a al 2014 www.pnas.org/cgi/doi/10.1073/pnas.1317323111 investigated albedo-effects of no-till agriculture; this study also showed effects from altered evapotranspiration fluxes. [Nicole Wilke, Germany]	Taken into account. References are updated when required

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96467	81	35	81	35	Tab. 4.7, "Whitening the roofs": it is unclear where the "max potential RF about -0.1 W m-2" comes from -- this value does not seem to be contained in the reference (Seneviratne et al 2018a). It further seems more appropriate to cite the original literature rather than the reference to a review paper (in particular if the values are not derived from the review paper). Also, some key literature is missing from the review paper, e.g. Zhang t al, 2016 doi:10.1088/1748-9326/11/8/084014, who quantify the potential of global white roofs to mitigate global warming, so exactly what Tab. 4.7 is about. [Nicole Wilke, Germany]	Taken into account. The table is revised and references are updated when needed.
26877	81	35	81	35	Table 4.7 We suggest to add "concentration" after "doubling of CO2" in the first raw [Eric Brun, France]	Taken into account. We changed the heading of this column to 'radiative forcing potential'
96469	81	35	81	36	Tab. 4.7: The table's key side effects are mostly formulated in a neutral way ("changes") -- apart from the crop yield issue, where the table says explicitly "decrease". The cited Proctor et al study conclude that SRM would "attenuate little of the global agricultural damage from climate change", the other two earlier studies investigating explicitly SRM effects on crop yields concluded "that solar radiation management would have little impact on rice production in China but could increase maize production" (Xia et al 2014 https://doi.org/10.1002/2013JD020630) and that SRM "in a high-CO2 climate generally causes crop yields to increase, largely because temperature stresses are diminished while the benefits of CO2 fertilization are retained." (Pongratz et al 2012 DOI: 10.1038/NCLIMATE1373). So overall, though all authors mention that methods have caveats and that regionally and for specific crop types yield losses may occur, all available studies find beneficial rather than detrimental effects on crop yields with SRM compared to unabated climate change. We thus suggest a neutral formulation: "changes in crop yields". [Nicole Wilke, Germany]	Taken into account. In the revised Table, we changed the heading of the column to 'key climate and environmental effects' , and discuss SRM effects in a more neutral way. We now write 'changes in crop yields'. All references are updated where required.
68669	81	36	81	36	Table 4.7: Stratospheric Aerosols Injections: change to Stratospheric Aerosol Interventions; add to second row, second column: and climate feedbacks; 4th column, second row: "potential decrease in crop yields" needs to be clarified, because there is also potential increase in crops depending on regions. [Simone Tilmes, United States of America]	Taken into account. The use of 'stratospheric aerosol injection' is consistent with most available literatures. we changed 'potential decrease in crop yields' to 'changes in crop yields'.
127589	81	36	81	36	In Table 4.7, where did the assessment come from that MCB could only offset 2xCO2 warming "if nearly 75% of ocean area is seeded"? Neither of the studies cited (Latham et al., 2012; Stjern et al., 2018) show this. Rasch et al. (2009), for example, show that seeding 70% of the ocean would "overshoot" offsetting 2xCO2. [Trigg Talley, United States of America]	Taken into account. In the revised table, we replace the column 'Potential for countering a warming from a doubling of CO2' to 'radiative forcing potential', and assess the cooling potential of each SRM option based on available literature. All references are updated where required.
80009	81	36	81	36	The source of uncertainty here is the type of aerosol (i.e. CaCO3. Al2O3, SO2, H2SO4), and in second order importance, the location (latitude, longitude, altitude) and temporal (e.g. seasonal) characteristics of the emission scenario (e.g. Vattioni et al., 2019). [Gabriel Chiodo, Switzerland]	Taken into account. Text is revised. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80011	81	36	81	36	SAI/Key Side effects: Changes to Precipitation patterns/hydrological Cycle (Tilmes et al., 2013) [Gabriel Chiodo, Switzerland]	Taken into account. Text is revised. All references are updated where required.
1827	81	36			Change "Mt. Pinatubo" to "1991 Pinatubo" The Pinatubo volcano has erupted many times, so you have to specify which eruption you are referring to. And delete "Mt." which also means megaton and so is confusing, as you use it with two different meanings here. Or use "1991 Mount Pinatubo" [Alan Robock, United States of America]	Taken into account. We revised the table and changed the content of corresponding column. In the text, we use 'Mount Pinatubo in 1991".
2409	81		87		I think the organization of this section needs to be made consistent with other sections in the chapter. Some sub-sections within this section read more like a review rather than an assessment. There are very few overall confidence statements especially for the assessment of individual SRM approaches (e.g., SAI, CCT etc). [Vaishali Naik, United States of America]	Accepted. Text is revised extensively for FGD.
112117	82	1	82	1	In table 4.7, the statement that "RF of several W/m2 *is* achievable" for Ocean Albedo Increase is utterly misleading. This *might* be acheivable if some technology is found that actually works as envisioned and only tested in models and very small-scale experiments. This kind of over-confident language shows up throughout the section and should be watched for carefully during revisions. [Mark Lawrence, Germany]	Taken into account. Text is revised.
114533	82	7	82	7	Re "imprecise": I think you can say along which dimensions; space, time, climate vaiable [Jan Fuglestvedt, Norway]	Taken into account. This sub-section is re-written.
68671	82	9	82	9	"would be closer": This statement depends on the variables considered and also on how SRM is done. If it is not performed to keep interhemispheric temperatures from changing, or only applied to one hemisphere, larger shifts in rainfall can occur than without SRM. [Simone Tilmes, United States of America]	Taken into account. This sub-section is re-written.
108017	82	10	82	11	Uncertainties in SRM-related processes are common to processes related to climate sensitivity more generally. SRM-related processes are not unique, unusual, or uncommon - instead, they are simply "normal" climate processes that have been identified as targets for manipulation. Limitations in current understanding of these climate processes in the context of SRM should not be distinguished or assessed differently than they are assessed in the context of human influences on the climate system more broadly, especially on the physical science basis that is the proper domain of WG1. While there are governance and political challenges to SRM, these should be discussed as separate from physical process-level uncertainty. Proposed edit: There are large uncertainties in understanding of SRM-related climate processes, though these uncertainties also affect all climate projections more generally." [Kelly Wanser, United States of America]	Taken into account. Text is revised as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80013	82	11	82	11	The timescale of potential SRM scenarios is dependent on the scenario under consideration. SRM could also only be applied to reduce the rate of global warming. Furthermore, how long does “long-term” mean? Especially relative to the “millennial timescale for the lifetime of atmospheric CO2” which is mentioned in the first part of this sentence. In addition, SRM requires simultaneous deployment of CDR techniques and inevitably also large-scale CO2 mitigation. SRM without CO2 mitigation and without deployment of CDR should not be considered at all, given the large “termination” effect. All in all, the connection between SRM and CDR should probably be highlighted here. [Gabriel Chiodo, Switzerland]	Taken into account. Text is revised. The assessment here focuses on physical climate response to SRM and CDR. The assessment of deployment scenario is beyond scope.
2401	82	11	82	13	Not clear if the chapter/section references are for this draft of AR6 or are referring to AR5. Given that this sentence belongs in a paragraph summarizing AR5 assessments, I am assuming that this is AR5 and if so, I find this very confusing. [Vaishali Naik, United States of America]	Taken into account. Text is revised.
50963	82	12	82	13	"mitigation" is not being used in accordance with the glossary definition. It should be replaced with "emissions reduction". [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
80015	82	13	82	15	This statement is wrong. There is evidence that SRM will contribute to mitigate atmospheric carbon and therefore also ocean acidification. The three mechanisms involved are (1) carbon cycle feedback and (2) reduced permafrost melting due to reduced surface temperatures and (3) reduced energy sector emissions (Keith et al, 2017). [Gabriel Chiodo, Switzerland]	Taken into account. Text is revised. All references are updated where required.
41973	82	14			Chapter 4 page 82 line 14 says that solar radiation management will do nothing about ocean acidity. This is true but irrelevant. It does nothing for many other problems such COVID 19, gold shortage and Scottish World Cup football performance. Carpenters are allowed more than one tool in their box. Should we reject a brilliant proposal to reduce ocean acidity because it does not also moderate hurricanes or save Arctic ice? We need to use all the tools in harmony. This is not a TV game show with a single winner. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. is revised.
71213	82	19	83	17	Though the idea of discussing the possibilities of SRM is good, there is need to give a recommendation going forward as to whether there is further need to invest in SRM research or to just focus on a few plausible SRM interventions. [Michael Mugarura, Germany]	Not applicable. This discussion is beyond the scope of this chapter. As is well known, IPCC assessment is only policy relevant and not policy prescriptive
5785	82	19	83	17	A key conclusion of SR1.5 regarding SRM was: "with high agreement that [SAI] could limit warming to below 1.5°C" (p. 350). This seems relevant, as the first question one should ask of a proposed response to climate change is, "Would it work, at least in a gross sense?" [Jesse Reynolds, United States of America]	Taken into account. Text is revised.
50965	82	1st row of t	82	2nd row of	"nearly 75 % of ocean area is seeded" - is it possible to comment on the feasibility of stratocummulus being induced over 75% of the ocean? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised. we replace the column 'Potential for countering a warming from a doubling of CO2' to 'radiative forcing potential', and assess the cooling potential of each SRM option based on available literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50967	82	Table 4.7	82	Table 4.7	Table 4.7 'Whitening the roofs' column: 'potential changes to urban climate' - please could you clarify if this is this a side effect or if regional cooling is the intention of this measure? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised. In the revised table, we lumped together surface-based albedo modification approaches and revised the corresponding text. In the revised Table, we changed the heading of the column to 'key climate and environmental effects' and discuss these effects in a more neutral way.
108019	83	2	83	2	Stjern 2018b study cited is not the most realistic assessment of MCB. A better study to cite is Ahlm 2017, which analyzes the G4-Sea Salt experiment, which more closely represents proposed MCB interventions. Ahlm, Lars, Andy Jones, Camilla Weum Stjern, Helene Muri, Ben Kravitz, and Jón Egill Kristjánsson. "Marine cloud brightening–as effective without clouds." Atmospheric Chemistry and Physics 17 (2017): 13071-13087. http://dx.doi.org/10.5194/acp-17-13071-2017 [Kelly Wanser, United States of America]	Taken into account. Corresponding discussions are revised and Ahlm et al. (2017) is cited in appropriate place.
96471	83	3	83	3	We kindly ask the authors for clarification what is meant by the term "human natural ecosystems". [Nicole Wilke, Germany]	Taken into account. We re-organize this subsection and corresponding texts are removed.
108021	83	7	83	10	This sentence is not reflective of the SR1.5 SRM assessment, and the changes are overly perjorative in nature. In particular, the word "legality" is never used in the report in reference to SRM. The conclusions of the SR1.5 assessment should be incorporated verbatim, including the confidence measures. The two candidates are as follows: "Overall, the combined uncertainties surrounding the various SRM approaches, including technological maturity, physical understanding, potential impacts, and challenges of governance, constrain the ability to implement SRM in the near future." (SR1.5 Ch.4, pg.352) "Some recent model-based analysis suggests SRM would be effective but that it is too early to evaluate its feasibility. Even in the uncertain case that the most adverse side-effects of SRM can be avoided, public resistance, ethical concerns and potential impacts on sustainable development could render SRM economically, socially and institutionally undesirable (low agreement, medium evidence)." (SR1.5, Ch.4, pg. 317) [Kelly Wanser, United States of America]	Taken into account. We revised the text and deleted the word 'legality'. All references are updated where required.
108023	83	7	83	10	While it is reasonable to report the conclusions of the SR1.5 assessment, the proper scope of WG1 is the physical science basis. There are substantial governance and political challenges to SRM, but these should be discussed as separate from questions of physical effectiveness, and ideally left to other WGs as appropriate. On that basis, the relevant conclusion from SR1.5 is as follows: "While theoretical developments show that SRM is technically feasible,... global field experiments have not been conducted and most of the knowledge about SRM is based on imperfect model simulations and some natural analogues." (SR1.5, Ch.4, pg. 351-352) [Kelly Wanser, United States of America]	Taken into account. Text is revised. The governance and political challenges are discussed in WG3 report. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108025	83	10	83	12	This sentence ("Against this background…") is meant to report the conclusions of SR1.5 at the time it was written, but the wording implies this is the current assessment of the AR6 WG1 authors. The connection should be clarified by combining this sentence with the previous, or making the verb passive or past tense, to achieve consistency with the tense of the rest of the paragraph: "...the existing literature only supported..." or "...the existing literature would only have supported..." [Kelly Wanser, United States of America]	Taken into account. Text is revised
127593	83	20	83	40	The english in this paragraph is pretty rough, and needs tidying, one example is, "at the time writing this report". [Trigg Talley, United States of America]	Taken into account. Text is revised.
1839	83	20			There is one subject missing from the scope, which is the engineering. You do not mention that it is covered in another part of the IPCC report, so it has to be covered here. This subject is whether SRM is technically possible. It is impossible today, as the technology does not exist. This has to be clearly stated here. The technique which seems the easiest to potentially do is stratospheric aerosols, and there have been estimates of the cost. See Robock (2020), which summarizes those studies. Even though it may be possible to design airplanes to emit gases or particles, we still cannot be confident that we can create aerosols with the desired properties. And any of these designs need to be tested, during which we would learn a lot, and have surprises. A recent paper by Keith shows that space-based techniques are impractical and too costly for the coming decades. MCB remains to be shown as pratical. We do not know enough about the cloud microphysics or regional climate to be able to effectively and safely even design an emission strategy. So please make clear that these ideas are very speculative, and no matter what models tell us about the climate impacts, even if we could agree on a plan for stratospheric injection, the world needs to realize that it is a hypothetical idea and cannot safely address global warming by itself. We can't wait to ramp up mitigation. Robock, Alan, 2020: Benefits and risks of stratospheric solar radiation management for climate intervention (geoengineering). The Bridge, 50, 59-67. [The Bridge is the quarterly peer-reviewed magazine of the National Academy of Engineering. The entire issue of the journal is available online at https://www.nae.edu/228883/Spring-Bridge-Issue-on-Engineering-and-Climate-Change . The issue is about engineering and climate change, and you can read the Editor's Note starting on p. 5 on	Taken into account. In the revised text, we clearly state that this section assesses the global and large-scale physical climate system response to SRM based on theoretical and modelling studies. The assessment of technical feasibility and engineering aspects is beyond the scope of this report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127591	83	20			GeoMIP does expressly look at SRM as a mitigating strategy. However, the mechanism by which most models do this is the model's code for implementing volcanoes. As such, efforts (+ results) such as VolMIP, SSiRC, VolRes -- and even the last millennium volcano studies -- should indeed be covered in this section. It is important to note all the vagaries and uncertainties in the stratospheric response, circulation, and chemistry. It is also worth mentioning here the distinction between models that idealize the SRM -- i.e., they impose non-interactive aerosols in the stratosphere that do not interact with the chemistry and are not beholden to the background circulation -- and those models that account for atmospheric chemistry. [Trigg Talley, United States of America]	Taken into account. Text is revised. As the scope here is climate response to a portfolio of SRM options, we do not discuss volcano studies and aerosol chemistry in detail. However, we do mention volcanoes are analogues for SAI, and we discuss interaction between aerosol and stratospheric chemistry and circulation.
115429	83	21	83	21	delete "widely studied" There are niot "widely studied" SRM technqiues, as all have been studied by a small group of scientists, many of whcih promote geoengineering even as a personal commercial actvitiy. In this paragraph, the impression given is that this a well studied technique, while in reality is nothing but modelling and theorectical studies by a small group of scientists [SILVIA RIBEIRO, Mexico]	Taken into account. Text is revised.
127595	83	21	83	38	[PROGRESS] The section felt quite scattered and sort of wordy. There is lots of good info within the whole SRM section, but It could be revised to more crisply deliver messages. Lines 21-32 probably belong in the introduction paragraph of Section 4.6.3.3. A further restructing would then use "assessment of SRM in AR5" and "assessment of SRM" to discuss what was learned in AR5 and SR1.5. Then start this section by saying that what has been learned for AR6 used simulations involving CMIP5, and GEOMIP6 simulations and individual studies. Then perhaps be explicit about where progress has been made (3 or 4 areas), namely : (1) simultaneous targetting of climate goals, (2) optimization of source emissions to achieve climate goals, (3) large ensemble studies, and (4) more in-depth individual investigation of particular SRM strategies or processes. [Trigg Talley, United States of America]	Taken into account. This section is largely rewritten.
111843	83	23	83	23	Delete "ethics" since this will not be part of WG3, ch14 [Oliver Geden, Germany]	Taken into account. 'ethics' is discussed in WR2 report (Chapter 16). Text is revised.
108027	83	26	83	38	This paragraph should be rewritten to emphasize the material developments since AR5. Both GeoMIP and single-model studies have made significant advances in physical process realism (e.g, fully coupled aerosols as in GLENS and GeoMIP6-sulfur), single-model large ensembles to uncover robust regional effects, and plausible deployment strategies (e.g., targeted and regional climate goals). [Kelly Wanser, United States of America]	Taken into account. This paragraph is revised.
2403	83	28	83	30	Will there be GeoMIP analysis available for this report? Can this statement be updated? [Vaishali Naik, United States of America]	Yes, this section assesses GeoMIP analysis. Text is revised.
50971	83	29	83	29	at the time of writing..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115431	83	34	83	38	delete "widely studied" There are niot "widely studied" SRM technqiues, as all have been studied by a small group of scientists, many of whcih promote geoengineering even as a personal commercial actvitiy. In this paragraph, the impression given is that this a well studied technique, while in reality is nothing but modelling and theorectical studies by a small group of scientists [SILVIA RIBEIRO, Mexico]	Taken into account. Text is revised.
108029	83	41	83	52	This discussion of efficacy is misplaced, misleading, and perjorative. The concept of "efficacy" as discussed in the papers cited is highly technical and not tied to layperson concepts of "efficiency" or "feasibility", or "effectiveness". This paragraph should either be deleted, moved, or clarified to introduce the reader to the specific definition of efficacy used in the literature - "relative change in temperature per unit change in RF caused by modifying albedo versus CO2". Without this definition, the concept of efficacy is not defined enough to understand the discussion in this paragraph without referencing the cited papers. Furthermore, the cited literature is more tightly tied to questions of model tuning & diagnosis, climate feedbacks, and equilibrium climate sensitivity than SRM per se, and its inclusion here is not germane to the question of SRM process uncertainty or society-relevant questions of whether "SRM could work". [Kelly Wanser, United States of America]	Taken into account. In the revised text, we clarify the meaning of "efficacy" and briefly assess different forcing efficacy between SRM and CO2 forcing.
50969	83	42	83	43	For clarity, suggest replacing "SRM" with "SRM-induced radiative forcing" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
114535	83	42	83	43	I think you need to add "in terms of RF" when you say that "the SRM required …. is larger" even if you refer to CO2 RF in the end of the sentcen. [Jan Fuglestvedt, Norway]	Taken into account. Text is revised.
132519	83	42	83	43	This shoud probably read "… that the ERF of SRM required to offset a CO2-induced global mean temperature increase is larger than the CO2-induced ERF" [Kyle Armour, United States of America]	Taken into account. Text is revised.
1837	83	42			"the SRM required" What does this mean? How is it measured? What is the metric? [Alan Robock, United States of America]	Taken into account. Text is revised.
50973	83	44	83	44	Suggest this is amended to say: 'of solar forcing is modelled to be less than…' - unless actual interventions have been made and can be confirmed. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
68673	83	47	83	47	"stratospheric ozone changes", change to "stratospheric ozone and water vapor changes" [Simone Tilmes, United States of America]	Taken into account. Text is revised.
80017	83	47	83	47	Incorrect citation; G.Chiodo, 2016 should be: Chiodo and Polvani (2016) [Gabriel Chiodo, Switzerland]	Accepted. Citation is corrected.
100769	83	47	83	47	The citation should be Chiodo and Polvani, 2016 instead of G. Chiodo. Chiodo, G. and L.M. Polvani, 2016: Reduction of Climate Sensitivity to Solar Forcing due to Stratospheric Ozone Feedback. J. Climate, 29, 4651–4663, https://doi.org/10.1175/JCLI-D-15-0721.1 [Juan Antonio Añel, Spain]	Accepted. Citation is corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108031	83	48	83	50	Neither of the cited papers (Duan 2018, Krishna-Pillai 2019) discuss comparative efficacy between SAI and MCB or CCT. [Kelly Wanser, United States of America]	Taken into account. The text is revised.
111845	83	50	84	4	there should be emerging literature around recently conducted MCB experiments over the Great Barrier Reef [Oliver Geden, Germany]	Taken into account. The discussion of MCB is revised. We have cited literature on this suggestion of protecting GBR using MCB.
132521	83	52	83	52	Drop "during slow feedback". I don't know what that means and you don't need it. Be sure to cite doi: 10.1002/2017MS001096 and 10.1175/JCLI-D-18-0843.1 here, which I think are the definitive papers relating SST patterns to feedback differences. [Kyle Armour, United States of America]	Taken into account. In the revised text, we drop the discussion on the technical details of different forcing efficacy. All references are updated where required.
127597	84	1	84	20	The paragraph is somewhat unbalanced and simplistic in making probablistic confidence statements (although there are no statements in this section documenting how these statements are quantified). Those statements that include confidence and likelihood characterizations in the current draft repeatedly document the certainty that SRM is not a perfect counteragent to GHG forcing (which I agree with). And other statements point out certainty of certain negative aspects of SRM. But to be provacative, how about including a few statements with similar confidence/likelihood characterizations about aspects of SRM that might be viewed positively (but I think are not contentious)? For example: 1. It is virtually certain that SRM, if implementable, has the potential to markedly and rapidly diminish some effects of global warming from increased GHG concentrations on many global and regional climate features over large areas of the planet. 2. Many recent studies indicate that it is very likely that SRM strategies simultaneously reduce impacts from global warming on many climatological features (e.g., temperature, precipitation, and heat and water stresses to ecosystems) over many regions, but the compensation is clearly not perfect and studies indicate there will be residual impacts on features, so negative consequences from unexplored impacts on the Earth system (unknown unknowns) may still outweigh the positive aspects discussed here. [Trigg Talley, United States of America]	Taken into account. This section is re-structured and text is revised.
96473	84	1	84	20	This paragraph "Climate Response to SRM" has the same title as 4.6.3.3, though it is a subset only. The next 3-4 paragraphs are really subsections of the paragraph "Climate Response to SRM"; this should please be reflected in the structure. [Nicole Wilke, Germany]	Taken into account. Text is revised.
12277	84	1	84	20	The rather dull results in Fig. 4.41 are hardly discussed. [Bryan Weare, United States of America]	Taken into account. Text is revised.
50975	84	1	85	18	It would be helpful if this discussion about the effects of the different technologies followed the descriptions of the technologies, ie suggest that this section is moved to after page 86 row 28. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have re-structured this section and revised text correspondingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5789	84	1	85	18	A key message is missing (although I am not sure where it should be in this subsection): models consistently indicate that SRM at a moderate intensity would bring all regions' climate closer to preindustrial conditions. [Jesse Reynolds, United States of America]	Taken into account. Text is revised and this message is included.
50979	84	2	84	2	e-folding'? Please explain this term. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the revised text, e-folding is a standard term that means an increase or decrease by a factor of e
108033	84	3	84	5	"Highly idealized" is a term with perjorative intent. It does not appear elsewhere in AR6 WG1. [Kelly Wanser, United States of America]	Taken into account. Text is revised.
108035	84	5	84	7	The term "idealized" is not warranted or accurate, given the sophistication of the model ensembles in the two papers cited. In particular, Kravitz 2017 uses the GLENS ensemble, which includes multiple members with fully coupled interactive chemistry at high resolution. [Kelly Wanser, United States of America]	Taken into account. Text is revised. All references are updated where required.
108037	84	5	84	10	The imperfect compensation of SRM on GHG forcing lacks a discussion of relative magnitude of residuals in comparison to unmitigated GHG pathways. In particular, "substantial" implies that the residuals are large relative to some climactic or societally-relevant baseline. and "completely reverse" implies an unrealistic standard that is not applied to any other mitigation or adaptation approach in WG1 AR6. In addition, the assessed confidence of "virtually certain" is not cited nor supported by surrounding citations. The two sentences should be combined: "2016). Idealized modelling studies have also consistently shown that SRM has the potential to markedly and rapidly diminish the effect of increasing GHG on global and regional climate (Irvine et al., 2016; Kravitz et al., 2017b), though there could be substantial residual climate change at the regional scale (high confidence) (Kravitz et al., 2014). [Kelly Wanser, United States of America]	Taken into account. Text is revised. All references are updated where required.
132523	84	8	84	8	I suggest citing doi: 10.1002/2015GL064314 here as well. Also on page 85, line 13 where discussing resitual effects on polar regions. [Kyle Armour, United States of America]	Taken into account. Text is revised. References are updated when required.
68675	84	10	84	10	"SRM related climate process": aerosol-cloud interactions impact the climate, but count aerosol microphysics as climate processes? [Simone Tilmes, United States of America]	Taken into account. Text is revised
108039	84	10	84	12	Sentences should be combined to make it clear the uncertainties affect both SRM and climate projections. "There are also large uncertainties in important SRM-related climate processes such as aerosol microphysics and aerosol-cloud-radiation interaction and hence the level of understanding is low, though it should be noted these processes uncertainties affect all climate change projections, not just those involving SRM. Aerosol radiative forcing uncertainty remains as the largest contributor to the overall ERF uncertainty since 1750 (WG1 Ch.2, pg.25, lines 35-36, WG1 Ch.7, 8)". [Kelly Wanser, United States of America]	Taken into account. Text is revised accordingly. All references are updated where required.
2405	84	12	84	12	a relevant section(s) could be cited here. [Vaishali Naik, United States of America]	Taken into account. Text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108041	84	12	84	19	These three sentences are redundant. One should be deleted. The "By design" is most confusing and most redundant. [Kelly Wanser, United States of America]	Taken into account. Text is revised.
68677	84	13	84	13	"substantial differences": suggest to change to "in part significant differences", for example GLENS impacts only show significant differences in some and not all regions and seasons. [Simone Tilmes, United States of America]	Taken into account. Text is revised.
50983	84	15	84	19	The text (p.84 rows 15-19) says this Figure (4.41) shows patterns relative to both high and low-CO2 worlds but this is difficult to understand in Figure 4.41 - please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
45527	84	16	84	19	There are two instances of "(Figure 4.41:)" here, both of which are not correct. Please check. [Leonard Borchert, France]	Taken into account. Text is revised.
108043	84	16	84	20	"Design" is used two ways, and inconsistently here. First, GEOMIP scenarios are perscribed to balance SRM and CO2 forcings within the context of each models' climate sensitivty - they not "designed", either in the sense of representing an explicitly chosen global temperature goal or spatiotemporal pattern. The second use of the term "design" is more appropriate, and most relevant to the paragraph theme. [Kelly Wanser, United States of America]	Taken into account. Text is revised.
41975	84	16			.Chapter 4 page 84 line 16 refers to figure 4.41 (at the end of the chapter) showing the effects of each of three SRM approaches to offset the same amounts of CO2-induced global warming. But most of the modelling of marine cloud brightening has been done with spray patterns chosen for the convenience of comparing climate models rather than finding the best way to exploit the technology. The models for marine cloud brightening sprayed steadily through the year usually between latitudes 45 N and 45 S. This ignores the short life of spray, giving a high-frequency response and low phase-shift desirable in control systems, and also the agility of spray vessels to allow tactical control of ocean regions and seasons of the year. It is like locking the steering for a road test. In particular, vessel movements allow control of the temperature difference across oceans which is known to have larger effects on precipitation than actual temperature values. It is not possible to show all the desirable features of different spray patterns in a single figure. A different message comes from Stjern et al. at https://www.atmos-chem-phys.net/18/621/2018/acp-18-621-2018-supplement.pdf. The first part of the reference is given twice on chapter 4 page 135 but the second entry does not include the " –supplement" suffix for the later paper with better figures. This shows in figure S4 the mean precipitation changes of nine leading climate models following an increase of condensation nuclei concentration by 50% in regions of low level marine cloud. This would increase precipitation by small but useful amounts in drought-stricken regions. The main precipitation reductions are all over the sea. Figure S1 shows particularly strong temperature reductions up to 4 K over the Arctic and smaller but fairly even temperature reductions elsewhere. These two figures alone would convince decision makers that	Taken into account. The MCB discussion is further expanded and revised. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68679	84	19	84	19	"Also, the patterns of climate response… ": add "and impacts depend on the baseline scenario chosen, as well as the climate targets (Tilmes at al., 2020). (ESD, accepted), and with that on the amount of required injections. [Simone Tilmes, United States of America]	Taken into account. Text is revised.
50977	84	25	84	37	Fig 4.41- please clarify the meaning of PiControl and the Geomip scenarios G1 and G4cdnc here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure caption is revised.
11415	84	25	84	37	While it is mentioned in the text, many people only read figure captions, and it is highly misleading to present the figure as if it is "the" response to each method, rather than one possible response. More generally, the entire section on SRM might be easier to present if it was stated very explicitly up front, rather than buried in a sentence several paragraphs in, that assessing SAI or MCB or CCT is very different from assessing the response to future (well-mixed) GHG because they aren't "one thing" and so there is no such thing as "the" climate response. Probably the over-compensation of global mean precip relative to global mean temperature is the ONLY robust statement that can be made. [Douglas MacMartin, United States of America]	Taken into account. Text and Figure caption are revised.
68681	84	42	84	42	GLENS simulations demonstrated that global precipitation over land does not have to change, depending on the application. SRM applications alone lead to the trade-offs, but other changes in future emissions may counter this effect. A clarification would help. [Simone Tilmes, United States of America]	Taken into account. Text is revised.
5787	84	42	84	43	"a trade-off between reversing temperature and precipitation change" is not quite right, as it implies that decision-makers could get only one or the other. Instead, as noted below, SRM overcompensates P relative to T. So there would be a trade-off only beyond fully compensating T. Below that level of SRM, there would *not* be a trade-off. [Jesse Reynolds, United States of America]	Taken into account. Text is revised.
108045	84	42	84	56	This paragraph is more productively framed in terms of SRM choices and implementation strategies. Without a "trade-off" framing, the results of the studies discussed are mutually inconsistent and confusing. To address this issue, the following sentence could be added at the beginning: "The specific climate response to SRM depends greatly on the details of implementation and must be assessed in context with the explicit or implicit climate "goals" or "targets" of each study." [Kelly Wanser, United States of America]	Taken into account. This paragraph is revised.
19467	84	45	84	47	The authors say, "If SRM was used to fully offset GHG-induced global mean warming." They also have to say, "SRM can be used moderately," which would greatly reduce a myrid of SRM side effects. [Masahiro Sugiyama, Japan]	Taken into account. Text is revised.
68683	84	47	84	47	note, Tilmes et al., 2013 used idealized solar dimming experiments [Simone Tilmes, United States of America]	Taken into account. Text is revised. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11417	84	47	84	47	This is not true. The conclusion regarding global mean precipitation is well substantiated and follows from physics, but there is no basis for claiming "a substantial reduction in rainfall in the tropical monsoonal regions", as that depends on model and approach. (For a counter-example, for example, see Kravitz et al 2019, Comparing surface and stratospheric impacts of geoengineering with different SO2 injection strategies. Journal of Geophysical Research: Atmospheres, 124. https://doi.org/10.1029/2019JD030329; at least for India, the precipitation increases; we also have submitted paper that you can't cite due to cut-off date, but shows a way to deploy that would not reduce India precip, so it is 100% certain that the sentence as written is not true.) Perfectly fair if you include some qualifier like "in some simulations", or maybe even "in many, but not all simulations,..." [Douglas MacMartin, United States of America]	Taken into account. Text is revised. There would be a reduction in "mean" tropical rainfall when solar insolation is uniformed reduced to offset all of global mean warming. All references are updated where required.
11419	84	53	84	54	For P-E in more current models and simulations (noting that Irvine et al 2019 is for a solar reduction), see Cheng et al 2019, Soil moisture and other hydrological changes in a stratospheric aerosol geoengineering large ensemble. Journal of Geophysical Research: Atmospheres, 124. https://doi.org/10.1029/2018JD030237. Also Irvine and Keith, https://doi.org/10.1088/1748-9326/ab76de, which I think was submitted before the literature cut-off date (Dec 31 2019?) [Douglas MacMartin, United States of America]	Taken into account. These citations are added. All references are updated where required.
50981	84	55	84	55	"CO2-induced changes in extremes in temperature and precipitation and tropical cyclone intensity are also likely to be reduced by SRM" - it would be useful here to clarify that this is a statement based on model simulations only(?) and that observations have not yet confirmed this. While SRM side effects are mentioned elsewhere it is important to avoid the risk of model-based statements such as this being taken out of context from this report. It would be useful to highlight that there is further information on side effects in the report. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
108047	85	1	85	5	The cited paper for sea-ice extent is not the most realistic and recent result. Jiang 2019 simulates the same scenario, but finds that maintaining 2020 temperature levels maintains sea ice throughout the simulation. The sentence should be rewritten: "In a scenario where stratospheric aerosol injection is used to limit surface temperature at year 2020 levels and average spatial temperature patterns, SRM largely maintains the high-latitude sea ice in both hemispheres, though residual seasonal temperature changes have a significant effect on the seasonal cycle of sea ice change (Jiang 2019)". Jiang, Jiu, et al. "Stratospheric Sulfate Aerosol Geoengineering Could Alter the High Latitude Seasonal Cycle." Geophysical Research Letters (2019). https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL085758 [Kelly Wanser, United States of America]	Taken into account. This paragraph has been rewritten. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2407	85	3	85	5	Understanding of the reponsee to aerosol injection is highlighted here…how is this different from what is being discussed in section on SAI beginning on line 22? [Vaishali Naik, United States of America]	Taken into account. This paragraph is re-structured.
11421	85	3	85	5	Um… but see also Kravitz et al 2017 and Tilmes et al 2018, where aerosol injection maintained 2020 levels and *increased* september sea ice extent. So this sentence as written is extremely misleading. Again, this is why it is important to better integrate the observation that the response to SAI (or MCB or CCT) is not "one thing" to be discovered, but depends on how you do it. [Douglas MacMartin, United States of America]	Taken into account. This paragraph is re-structured. All references are updated where required.
127599	85	5	85	18	[CONFIDENCE] It might be worth pointing out that all conclusions within this block of lines are a result of one (or at most two) model studies and therefore there is low confidence in the robustness of their conclusions. It is also important to emphasize that multi-model and large ensemble studies would be useful to test the robustness of many of these conclusions. [Trigg Talley, United States of America]	Taken into account. This paragraph is re-structured.
46595	85	6	85	6	I find the experssion "sea-ice loss is stabilized" unclear. Maybe better to say "additional sea-ice loss can be avoided" or the like [Dirk Notz, Germany]	Taken into account. This paragraph is re-structured and text is revised.
108049	85	7	85	13	The term "highly uncertain" is not justified by the cited papers, which all show a consistent effect of SRM on AMOC - all three show that AMOC is strengthened by SRM against a background of weakening circulation on GHG pathways without SRM. The consistency of results in recent, full-featured models do not justify a characterization of AMOC changes under SRM as "more uncertain" than they are in all other climate scenarios. [Kelly Wanser, United States of America]	Taken into account. This paragraph is re-structured and text is revised.
50993	85	9	85	13	Should this sentence on regional applications of MCB have a confidence statement assigned to it? In an analogous way to the following sentence. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph is re-structured and text is revised.
11423	85	11	85	11	Need to replace "would" with something more like "in one particular study". Taking an identical strategy in CESM2(WACCM6) rather than CESM1(WACCM) does not lead to this outcome. So the answer is not "would", but rather, we don't know. Tilmes et al (currently still under review for ESD; https://doi.org/10.5194/esd-2019-76) was submitted before literature cut-off date too, and has the relevant plots. [Douglas MacMartin, United States of America]	Taken into account. This paragraph is re-structured and text is revised. All references are updated where required.
68685	85	11	85	12	Other model simulations with a different model have shown that an acceleration of the AMOC may not occur with SRM (e.g., Tilmes et al., 2020). Therefore, one needs to point out that these results are from single model studies. [Simone Tilmes, United States of America]	Taken into account. This paragraph is re-structured and text is revised. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1835	85	15	85	16	"by offsetting only half of the GHG-induced global warming" There are several articles that found this before Irvine et al. (2019): Jones, Andy, Jim M. Haywood, Kari Alterskjær, Olivier Boucher, Jason N. S. Cole, Charles L. Curry, Peter J. Irvine, Duoying Ji, Ben Kravitz, Jón Egill Kristjánsson, John C. Moore, Ulrike Niemeier, Alan Robock, Hauke Schmidt, Balwinder Singh, Simone Tilmes, Shingo Watanabe, and Jin-Ho Yoon, 2013: The impact of abrupt suspension of solar radiation management (termination effect) in experiment G2 of the Geoengineering Model Intercomparison Project (GeoMIP). J. Geophys. Res. Atmos., 118, 9743-9752, doi:10.1002/jgrd.50762. Kravitz, Ben, Alan Robock, Simone Tilmes, Olivier Boucher, Jason M. English, Peter J. Irvine, Andy Jones, Mark G. Lawrence, Michael MacCracken, Helene Muri, John C. Moore, Ulrike Niemeier, Steven J. Phipps, Jana Sillmann, Trude Storelvmo, Hailong Wang, and Shingo Watanabe, 2015: The Geoengineering Model Intercomparison Project Phase 6 (GeoMIP6): Simulation design and preliminary results. Geosci. Model Dev., 8, 3379–3392, doi:10.5194/gmd-8-3379-2015 MacMartin, D. G., Wang, W., Kravitz, B., Tilmes, S., Richter, J. H., & Mills, M. J. (2019). Timescale for detecting the climate response to stratospheric aerosol geoengineering. Journal of Geophysical Research: Atmospheres, 124, 1233–1247. doi:10.1029/2018JD028906. Tilmes, S., B. M. Sanderson, and B. C. O'Neill (2016), Climate impacts of geoengineering in a delayed mitigation scenario, Geophys. Res. Lett., 43, 8222–8229, doi:10.1002/2016GL070122. [Alan Robock, United States of America]	Taken into account. The text is revised. However, the suggested references are not directly relevant to the specific discussion of "offsetting only half of the GHG-induced warming". All references are updated where required.
19469	85	15	85	18	The dependence of SRM efficacy/side effects on the magnitude of deployment should not be mentioned as an afterthought. It should be used as a framing for the whole section on SRM. It doesn't mean that SRM can be used only moderately. A rogue state could use it at a full scale. But that's a societal dimension. In SRM, societal choice and natural science impacts are closely tied together. A better framing is required for the SRM section. See http://www.nature.com/doifinder/10.1038/nclimate2493 and http://dx.doi.org/10.1080/14693062.2017.1323721 [Masahiro Sugiyama, Japan]	Taken into account. This paragraph is re-structured. Framing of SRM in WG1 is made such that the science of the climate system response to SRM is primarily assessed. Less focus is given to the scenarios. All references ware updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108051	85	15	85	18	This paragraph is misleading and mischaracterizes the literature. There are substantial numbers of SRM modeling scenarios that limit warming to 1.5, as mentioned earlier in this chapter and in SR1.5 Ch.4, in addition to holding temperature at various arbitrary levels based on ensemble spin-up averages. In all three scenarios (halfing warming, limiting to 1.5, holding some arbitrary value) , results show a decline in extremes versus unmitigated GHG pathways. Omitting this consistent agreement despite scenario and model variation is substantively misleading. See for example: Pinto, Izidine, et al. "Africa's Climate Response to Solar Radiation Management With Stratospheric Aerosol." Geophysical Research Letters 47.2 (2020): e2019GL086047. https://doi.org/10.1029/2019GL086047 Curry, Charles L., et al. "A multimodel examination of climate extremes in an idealized geoengineering experiment." Journal of Geophysical Research: Atmospheres 119.7 (2014): 3900-3923. Ji, Duoying, et al. "Extreme temperature and precipitation response to solar dimming and stratospheric aerosol geoengineering." Atmospheric Chemistry and Physics (Online) 18.PNNL-SA-132309 (2018). [Kelly Wanser, United States of America]	Taken into account. This paragraph is re-structured and suggested references are added . Also, The assessment here is on the science of climate response to SRM. Less focus is given to the scenarios. All references are updated where required.
5791	85	15	85	18	This short paragraph would be better placed above, at p. 84 between lines 47 and 48. [Jesse Reynolds, United States of America]	Taken into account. This paragraph is re-structured.
11425	85	15	85	18	I don't think "typically" is warranted any more; that would have been accurate for AR5 but is pretty dated now. I could list plenty of studies that look at holding temperatures constant at some particular level (2020, or 1.5C…), plus things like limiting rates of change. I think what is needed somewhere is a more nuanced paragraph on possible scenarios (including use for overshoot, or limiting rates of change), and the Irvine et al reference is fine for talking about that particular scenario. [Douglas MacMartin, United States of America]	Taken into account. This paragraph is re-structured. The assessment here is on the science of climate response to SRM. Less focus is given to the scenarios of implementation.
115433	85	15	85	18	Delete these lines,a s they are not enough substantied by independent scientists. There are very few studies, all speculative and from scientists biased towards SRM on the use of "moderate" SRM, as proposed by Irvine. And the proposal / conlusions in the cited article ignore many other aspects that will still remain, such as ozone depletion, acid rain, ecosystem impacts, agricultural impacts, aesthetics, ethics, and other unknowns. To have a better picture, please take into consideration the article Robock, Alan, 2020: Benefits and risks of stratospheric solar radiation management for climate intervention (geoengineering). The Bridge, 50, 59-67. http://climate.envsci.rutgers.edu/pdf/RobockBridge.pdf. Specially see table 2 [SILVIA RIBEIRO, Mexico]	Taken into account. This paragraph is re-structured. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50989	85	18	85	18	Suggest this summary statement on the efficacy of SRM and other impacts aslo recognises those climate impacts that the technique does not ameliorate/address - i.e. sea level rise and changes to oceanic pH. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph is re-structured.
80019	85	21	85	47	The SAI technique described in this section only focuses on the emission of sulfate aerosols or/and its precursors. In the whole section on SRM it is only shortly mentioned in table 4-7 that there are also other potential aerosol species such as calcium carbonate or alumina which could be emitted for the purpose of SRM. I think this section should be written in more general terms, not only focusing on sulfate aerosols, but also the type of emitted aerosol species should be added as a main source of uncertainty (line 32-33) together with the heterogeneous reactions and reaction rates of these particles. I think there should be at least 2-3 sentences about SAI using solid aerosols. It should be stated that these solid aerosols show the potential of showing less ozone depletion and less stratospheric warming while simultaneously showing a larger efficacy (i.e. larger radiative forcing per emitted mass) (Blackstock et al., 2009; Ferraro et al., 2011; Jones et al., 2016; Keith et al., 2016; Keith, 2010; Pope et al., 2012; Teller et al., 1996; Weisenstein et al., 2015). [Gabriel Chiodo, Switzerland]	Taken into account. In the revised text, we expanded the discussion of SAI using non-sulfate aerosols. All references are updated where required.
68687	85	23	85	23	"reduction of stratospheric ozone" change to "reduction of stratospheric polar ozone". [Simone Tilmes, United States of America]	Taken into account. Text is revised.
11427	85	23	85	23	re ozone, I think it would be appropriate to include in brackets either "(for sulfate aerosols)" or "(depending on the aerosol material)" and cite David Keith's PNAS paper on calcite. (Noting that more recent research suggests calcite wouldn't increase ozone, but at least wouldn't lead to destruction.) [Douglas MacMartin, United States of America]	Taken into account. Text is revised. All references have been updated where required.
11429	85	23	85	23	Re monsoon precipitation, as commented before this is simply not true. (We also have other simulations that were not submitted before the literature cut-off date, but demonstrate how one could increase monsoon precipitation - so I know it is not a true statement.) If you want to list side effects that are robust, list (i) stratospheric heating, which would likely have surface climate effects (e.g. Isla Simpson et al, 2019 in JGR), (ii) increased ratio of diffuse to direct sunlight, as two examples... [Douglas MacMartin, United States of America]	Taken into account. Text is revised. All references have been updated where required.
6679	85	26	85	26	The global temperature did not drop by 0.5ºC in 1992. So would it be appropriate to write "would have caused a global cooling of about 0.5ºC in 1992 had an El Niño event not warmed the atmosphere at the same time"? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the text. All references are updated when required
68689	85	26	85	26	Please, check with last IPCC report for a more recent number(0.3degrees instead of 0.5degrees). [Simone Tilmes, United States of America]	Taken into account. We have revised the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68691	85	26	85	29	Volcanoes are not suited as analogues for climate impacts of SRM, since they erupted abruptly and at random locations, which results in very different climate effects than targeted and continuous injections. What we can learn from larger and smaller volcanoes is how well models represent microphyscial processes like aerosol formation and coaguation, chemical interactions, and movement of injected plumes. [Simone Tilmes, United States of America]	Taken into account. Text is revised.
108053	85	29	85	32	This paragraph obfuscates the extent and degree that the volcanic forcing historical record constrains estimates of the climate response to SAI, to prejudicial effect. Confidence assessments about the effectiveness of SAI approaches should be compatible with broader confidence assessments for volcanic forcing processes. Sentence should read: "Emergent constraints (see Chapter 1 and 5) that relate the climate system response to volcanic eruptions can be used to reduce uncertainty of the land surface temperature response to SAI. For example, by incorporating the observed influence of recent volcanic eruptions on land temperatures, a recent study shows that CMIP5 models overestimate the land surface warming per unit change in surface solar radiation due to stratospheric aerosols by ~20% (Plazzotta et al., 2018)." [Kelly Wanser, United States of America]	Taken into account. Text is revised accordingly.
108055	85	32	85	34	This sentence mischaracterizes the uncertainty of SAI-related processes by stating they are "large", without referencing or justifying this assessment in context of the uncertainty of broader climate projections. This sentence should import the assessment language for volcano and aerosol radiative processes discussed elsewhere in AR6 WG1. [Kelly Wanser, United States of America]	Taken into account. Text is revised.
108057	85	34	85	37	This assessment omits contradictory evidence about the stated relationship between SAI efficacy at higher injection rates and atmospheric loadings. In particular, the highest-quality evidence with physically-plausible chemistry-coupled ESMs show that forcing is effectively linear with injection rate up to very high loadings of 50 teragrams of SO2 per year. The confidence assessment of "likely" misharacterizes the literature. A more appropriate assessment would be "as likely as not". Please cite: Tilmes, Simone, et al. "CESM1 (WACCM) stratospheric aerosol geoengineering large ensemble project." Bulletin of the American Meteorological Society 99.11 (2018): 2361-2371. [Kelly Wanser, United States of America]	Taken into account. This paragraph is revised. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108059	85	34	85	37	The use of the words "efficacy" and "efficiency" in this sentence are not precisely defined enough to avoid misleading non-expert readers, and not tied to layperson concepts of "efficiency" or "feasibility", or "effectiveness". This paragraph should read "It is likely that SAI will reduce radiative forcing 35 expressed as the ratio between sulphate aerosol forcing and injection rate, which is sensitive to the location 36 of injection, decreases with injection rate of SO2 as larger particle concentrations in the background leads to 37 larger size particles and lower efficiency (Niemeier and Timmreck, 2015; Kleinschmitt et al., 2018). [Kelly Wanser, United States of America]	Taken into account. text is revised. All references have been updated where required.
80021	85	34	85	37	This statement is not true for all types of SAI. This statement only applies for SAI by emission of SO2. This should clearly be stated. The direct emission of sulphate aerosols (accumulation mode sulfate aerosols) for example show the opposite effect (e.g. Vattioni et al., 2019). Also, this statement does not apply for SAI by emitting solid particles. Additionally, the cited statement above, which only applies for SAI by emission of SO2, was also shown by Vattioni et al., 2019. Also, the efficacy in not only dependent on the location but also on the timing of the emissions (e.g. seasonal or pulsed emissions). [Gabriel Chiodo, Switzerland]	Taken into account. Text is revised. All references are updated where required.
50991	85	36	85	36	as larger particle concentrations in the background..' - is it possible to specify the size of these larger particles? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised. Larger in this context refers to volcanic size particles with a radius of about 0.5 micron.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68273	85	39	85	47	Stronger language than “alter the stratospheric ozone concentrations” is needed here: SRM proposals that would use sulfate emissions to cool the planet risk destroying stratospheric ozone. WMO, et al. (2019) Scientific Assessment of Ozone Depletion: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 6.16 (“Column ozone changes as the result of stratospheric aerosol geoengineering therefore depends on the injection amount, timing (ODS loading), and injection strategy (influencing aerosol size and location; Appendix 6A). Relatively small and constant injections of 2.5–4 Tg S yr−1 between 2020 and 2070, which would result in 0.5°C of surface cooling, are calculated to lead to an approximately 4% reduction in the global stratospheric column ozone for 2020 and only 1% reduction by 2070 (Pitatry et al., 2014; Xia et al., 2017). Much larger injection amounts that would lead to a surface temperature cooling of around 2°C in 2040–2050, based on a single model study, would result in reductions in column ozone of 28–40% in October over Southern Hemisphere (SH) high latitudes and 8–18% for NH high latitudes in March, with varying values depending on the injection altitude (Tilmes et al., 2018). Injections closer to the tropopause cause a stronger dynamical response and could result in up to an 8% increase in column ozone in NH winter mid- and high latitudes. A single modeling transient simulation based on RCP8.5 greenhouse gas forcings with continuously increasing SO2 injections between 2020 and 2099 and decreasing ODSs would result in approximately constant change in column ozone in high polar latitudes (20–23% in October over the SH and 10–12% in March over the NH polar latitudes) and slightly larger (3–5%) column ozone values compared to non-geoengineering conditions for tropics and winter northern mid-latitudes by the end of the 21st century (Richter et al., 2018).”). [Durwood Zaelke, United States of America]	Taken into account. This paragraph is re-structured and text is revised. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68275	85	39	85	47	This section should include ice albedo modification SRM. See, e.g., Leslie Field et al., Increasing Arctic Sea Ice Albedo Using Localized Reversible Geoengineering, Earth's Future (May 2018) (describing a process to slow the melting of the polar ice caps through the deployment of floating sand, made of silica, that floats atop polar ice, increases its albedo, and is designed to degrade over time). Other researchers propose using wind power to pump water to the surface of the Arctic. They calculate that adding about 1 meter of thickness over 10% of the Arctic Ocean, at an estimated cost of $50 billion/year, would offset decreases in ice thickness observed since 2000. "Our analysis so far shows that artificial thickening of the ice can counteract a roughly 1°C temperature increase across the Arctic." Desch et al., Arctic Ice Management, Earth's Future (19 December 2016). Another group of scientists have proposed slowing melting through geoengineering polar glaciers. See Moore et al., Geoengineer polar glaciers to slow sea-level rise (Comment), Nature (14 March 2018). They propose three methods to achieve this: A) removing the subglacial stream under Antarctica that acts as a lubricant to speed up flow of the ice into the ocean, B) blocking warm water from reaching glaciers by constructing 100-meter-high walls with sloping sides at its edge, and C) artificially pinning ice shelves that hold back glaciers by constructing berms and islands. The authors note that more research is needed to establish the scientific validity of these projects. A strategy to stabilize the West Antarctic Ice Sheet would use massive amounts of energy to pump nearby seawater, produce snow, and deposit the snow around the Pine Island and Thwaites glaciers. The plan would utilize an estimated 12,000 wind turbines to generate 145 GW of power needed to save the ice sheet, and thereby avoid 3 meters of sea level rise. See Johannes Feldmann et al. (2019), Stabilizing the West Antarctic Ice Sheet by surface	Taken into account. In the revised text, we add a paragraph briefly discussing these ice albedo modification SRM options. All references are updated where required.
11431	85	42	85	42	I think it would be worth adding Simpson et al 2019 to this list (", J. Geophysical Research A. 124, 2019. doi:10.1029/2019JD031093) as an explicit study on how stratospheric heating could impact precipitation. [Douglas MacMartin, United States of America]	Taken into account. Text is revised and Simpson et al. (2019) is added and discussed.
68693	85	43	85	45	Simpson et al, 2019 has also shown that this effect is causing a reduction in the Indian monsoonal precipitation. [Simone Tilmes, United States of America]	Taken into account. Text is revised. All references are updated where required.
50985	85	45	85	45	What is "meridional temperature"? Should this be "meridional temperature gradient"? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
67689	85	45	85	45	it should be Richter et al., 2018 (Jadwiga is her first name) [Karen Rosenlof, United States of America]	Accepted. Reference is corrected.
11433	85	46	85	47	I'm surprised that you only have "medium confidence" that a positive number has a different sign than a negative number, i.e. that warming would offset cooling. Really, I don't think you even need to articulate a confidence in that. [Douglas MacMartin, United States of America]	Taken into account. Text is revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108061	85	50	86	4	This treatment of Marine Cloud Brightening is far too short given the extensive literature on this SRM approach, especially given the relative space devoted to Cirrus Cloud Thinning. Proposed text: The next most-studied SRM approach to offset climate warming is Marine Cloud Brightening (MCB), where the primary proposal as a climate response is to disperse small particles (80-100nm) of salt generated from sea water into the base of marine stratocumukus clouds in regions of high susceptibility to increase their alebdo (2009 Jones, et al., other). The natural analogue for this effect is the impact on cloud albedo of particulates in pollution, or the “cloud-aerosol effect” which is understood to produce a substantial but uncertain cooling effect on climate. “There is high confidence that aerosols and their interactions with clouds have offset a substantial portion of global mean forcing from well-mixed greenhouse gases. They continue to contribute the largest uncertainty to the total [Radiative Forcing] estimate. “ (IPCC 5th Assessment, 2013, Summary for Policymakers p. 13-14.) The more specific natural analogue for MCB are reflective, persistent “ship tracks” observed after the passage of a sea-going vessel emitting combustion aerosols into susceptible clouds. These ship tracks have a large local forcing effect, and estimates of net negative forcing from existing global shipping activity are substantial, though with wide confidence intervals reflecting process and model uncertainty (Diamond 2019, Gryspeerdt 2019). Global assessments of the potential for MCB approaches suffer from a lack of research in deployment strategies, but have bounded radiative forcing potential between- 0.6 and -4 W/m^2 per year (Stjern 2018, Jones 2009), including the potential for MCB to offset a doubling of CO2 (Jones	Taken into account. This sub-section is substantially expanded and revised. All references are updated where required.
108063	85	50	86	4	This section lacks a discussion of natural analogues of proposed MCB mechanisms with large radiative impact as an important constraint on uncertainty and potential effectiveness. Two papers on ship tracks provide important evidence for the plausibility of proposed MCB mechanisms and significance of the potential influence on climate. Diamond, Michael, Hannah Director, Ryan Eastman, Anna Possner, and Robert Wood. "Substantial Cloud Brightening from Shipping in Subtropical Low Clouds." (2019). https://doi.org/10.1002/essoar.10501145.1 Gryspeerdt, Edward, Tristan WP Smith, Eoin O'Keeffe, Matthew W. Christensen, and Fraser W. Goldsworth. "The Impact of Ship Emission Controls Recorded by Cloud Properties." Geophysical Research Letters 46, no. 21 (2019): 12547-12555. https://doi.org/10.1029/2019GL084700 [Kelly Wanser, United States of America]	Taken into account. The analogue of ship tracks is discussed in the revised text. All references are updated where required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127601	85	50	86	4	Just as volcanic eruptions are a good proxy for understanding the potential efficacy of SAI, ship tracks are a good proxy for understanding the potential efficacy of MCB. Two recent studies (Toll et al., 2019, Nature, doi:10.1038/s41586-019-1423-9; Diamond et al., 2020, AGU advances, doi:10l1029/2019AV000111) have demonstrated the potential efficacy of aerosol injection into low marine clouds to increase cloud albedo using ship tracks as proxies. The importance of these two studies is that (a) they are based on observed, not modeled, responses, (b) they are based on statistical analyses, not one-off case studies, and (c) they indicate that the Twomey effect dominates the cloud response to the addition of aerosol. This is quite important, because one of the proposed reasons why MCB might not be effective is if cloud fraction/LWP decrease in response to higher CDNC. These two studes show that the increased CDNC effect is not significantly offset by a decrease in LWP. Why is the assessment of SRM mechanisms presented here based solely on the GeoMIP model results? Also include other studies -- in particular those that are observationally constrained (given how poorly global models represent clouds and aerosol-cloud interactions). [Trigg Talley, United States of America]	Taken into account. These two studies are discussed and text is revised. All references are updated where required.
112121	85	50	86	4	This section is much more poorly written than the sections on SAI and CCT and should be revised up to that standard (it only cites two relevant but not seminal or most important papers and misses the alternate name Marine Sky Brightening); the section on this topic in the review by Lawrence et al. (Nature Communications, 2018) would provide a helpful shortcut for this for the IPCC authors. [Mark Lawrence, Germany]	Taken into account. This sub-section is substantially expanded and revised.
50987	85	51	85	51	At the start of the paragraph on Marine Cloud Brightening, please could you add a sentence to explain what it is, as included for other SRM technologies. This could be: "Marine Cloud Brightening is the addition of small aerosols, such as sea salt, to cloud formation locations to increase the reflectivity of those clouds to incoming shortwave solar radiation" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sub-section is substantially expanded and revised.
127603	85	51	85	52	The cited Stjern et al. paper made the point repeatedly that MCB forcing will cause relatively modest changes in the precipitation pattern (see the abstract documenting weak decrease globally and 1% percent increases over land, and see page 631 summary sentence indicating "slight" precipitation increases). Secondly, RMS and correlation statistics indicate that the SAI and MCB simulation are almost indistinguishable. If these differences are truly significant, present statistical tests demonstrating this result. Also MCB appears to return the temperature field (RMS and Correlation values) to a state more like the control than does SAI. So there is little evidence that the stronger local forcing imposed by MCB would produce larger climate changes than SAI in temperature or precipitation. [Trigg Talley, United States of America]	Taken into account. This sub-section is substantially expanded and revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114537	85	51	85	53	Is this the assessment of the author team? Shoud not be based on a single paper. I guess you simply refer to the findings in teh cited paper. [Jan Fuglestvedt, Norway]	Taken into account. This sub-section is substantially expanded and revised.
41977	85	51			Chapter 4 page 85 line 51 mentions 'highly heterogeneous' as if this was a bad thing. We could argue that the heterogeneous, alternating movements of the steering wheel of a road vehicle are very useful for keeping it in the correct position along a winding road. The drought reductions and evenly spread temperature reductions in Stjern et al. 2018, which should seem extremely attractive to any decision maker, are despite the heterogeneous movement of clouds. Chapter 4 page 86 line 41 mentions global mean precipitation meaning sea and land. It would be much more useful to study regional and seasonal results on land. Few people will care about precipitation reduction over the sea and these few can be provided with water from desalination plant. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sub-section is substantially expanded and revised.
108065	85	53	85	54	This sentence mischaracterizes the uncertainty of MCB-related processes by stating they are "high", without referencing or justifying this assessment in context of the uncertainty of broader climate projections. Specifically, the climate response to anthropogenic aerosols through both direct and indirect mechaisms are consistently assessed as large and negative, though within a large range reflecting process and modeling uncertainty (AR6 WG1 Ch.2,7,8). The process-level mechanisms behind both phenomena are highly related, and constraining them would improve both assessments of MCB mechanims and of future climate trajectories more broadly (Wood 2017). Research to reduce uncertainty of MCB effects improve understanding of both current human influences through anthropogenic aerosols, and essential climate processes affecting fundamental estimates of climate sensitivity. This sentence should import the confidence assessment language for aerosol and cloud radiative processes discussed elsewhere in AR6 WG1. Wood, Robert, Thomas Ackerman, Philip Rasch, and Kelly Wanser. "Could geoengineering research help answer one of the biggest questions in climate science?." Earth's Future 5, no. 7 (2017): 659-663. https://doi.org/10.1002/2017EF000601 [Kelly Wanser, United States of America]	Taken into account. This sub-section is substantially expanded and revised. All references are updated where required.
1833	85	53	85	54	Specify which are the recent studies. [Alan Robock, United States of America]	Taken into account. This sub-section is substantially expanded and revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108067	85	54	86	1	This sentence mischaracterizes the proposed mechanisms underlying the effective radiative forcing of MCB. In particular, the relative contribution of the direct scattering effect is due to prescribed mechanisms in the cited modeling scenario, specifically, GeoMIP G4-sea salt, which increases the mass of salt aerosols according to the pre-existing size distribution already represented in each model. This mischaracterizes the MCB mechanism as first proposed by Jones 2009 and the Marine Cloud Brightening Project at University of Washington since then. MCB proposals have consistently emphasized the importance of specified areosol size distributions to increase the reflectivity of marine clouds without inducing counterproductive precipitation - see Neukerman 2014. The study (Stjern 2018) cited to support the assessment of large changes in regional precipitation from MCB approaches uses a model configuration (GeoMIP G4 CDNC) chosen for model simplicity, not proposal realism or physical mechanism plausibility. Implementing this scenario would be infeasible to implement, both practically and in terms of energetic requirements. Jones, Andy, Jim Haywood, and Olivier Boucher. "Climate impacts of geoengineering marine stratocumulus clouds." Journal of Geophysical Research: Atmospheres 114.D10 (2009). Neukermans, Armand, et al. "Sub-micrometer salt aerosol production intended for marine cloud brightening." Atmospheric research 142 (2014): 158-170. [Kelly Wanser, United States of America]	Taken into account. This sub-section is substantially expanded and revised. All references are updated where required.
11435	85	55	85	55	Missing the citation, Alm et al if I recall right. [Douglas MacMartin, United States of America]	Taken into account. This sub-section is substantially expanded and revised.
127605	85	55	86	1	No reference(s) are provided to support the assertion that "Recent studies suggest that the direct scattering effect of sea salt aerosol might play an important role in the potential of MCB through sea spray injection." This may be based on modeling studies where, for example, sea salt aeorosol was added to the entire ocean area or to entire latitude bands (30N-30S). Such model experiments are interesting and useful sensitivity studies, but in no way represent how MCB would actually be deployed -- i.e., in regions with a high fraction of stratocumulus clouds. Other studies have shown that, in more realistic implementation, the direct effect would not be insignificant but would be small compared to cloud forcing. This is really not what should be highlighted about MCB in the mere five sentences given to describing this potential SRM mechanism. [Trigg Talley, United States of America]	Taken into account. This sub-section is substantially expanded and revised. References are provided in the revised text.
116349	85		85		outcomes of ch 3 on model evaluation, ch 7 on RF and feedbacks, and chapter 4 on confidence in projections, need to be used for the assessment of SRM . [Valerie Masson-Delmotte, France]	Taken into account. Relevant assessment from these chapters are used in the revised text.
87629	86	1	86	1	sea spay → sea spray' [Valentina Roberta Barletta, Denmark]	Accepted, text is revised.
114539	86	1	86	1	spay --> spray [Jan Fuglestvedt, Norway]	Accepted, text is revised.
103035	86	1	86	1	sea spay → sea spray' [Philippe Tulkens, Belgium]	Taken into account, text is revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50995	86	2	86	2	missing word: 'associated with warmer..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text is revised.
9819	86	7	86	27	the description of CCT suggests that it operates primarily on the planet's thermal emission, and therefore it is not, strictly speaking, best classified as 'solar radiation management' [Robert Kopp, United States of America]	Taken into account. We follow the definition of SRM used in SR1.5 that include CCT as part of SRM. To avoid confusion, We have revised the paragraph that introduces SRM.
112123	86	7	86	27	This section is pretty good, but is missing several key points. In particular: "thinning" does not only imply reducing the lifetime and coverage but also the radiative thickness; CCT itself does not *cause* an increase in precipitation (if it were implemented without any increase in CO2, then precipitation would decrease), but in combination with increasing CO2 it *results in* a precipitation amount that is greater than any shortwave technique results in for the same temperature decrease (this needs to be described more accurately); parts of the paragraph are redundant (also with lines 47-49 later on, same page); and "has been found to be detectable" is the same over-confident language as noted in another comment (this needs to be caveated with "in model simulations" or "likely") [Mark Lawrence, Germany]	Taken into account. Text is revised.
127607	86	10	86	10	CCT would be equally effective if it reduced cirrus cloud opacity rather than amount. [Trigg Talley, United States of America]	Taken into account. Text is revised.
50997	86	22	86	22	Does "unperturbed low-CO2 climate state" mean pre-industrial climate? Or something else? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
51003	86	22	86	43	On rows 22 and 43 we find statements that CCT will increase precipitation, but the references supporting the apparently similar statements are different. Please clarify whether the two statements are saying the same thing or should be different, and be consistent in the use of references. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised to make the point clear.
50999	86	24	86	24	Please clarify what "increased CO2 concentrations" here is relative to. This is currentlly confusing because the previous sentence refers to a low-CO2 state. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised.
11437	86	24	86	26	The second half of this sentence is unrelated to the first half. And the first half is problematic; re ozone, either MCB or maybe stratospheric aerosols other than sulfate also would not affect ozone, but more importantly there is no evidence that CCT has reduced side effects on the hydrological cycle except in the global mean - but no-one experiences the global mean. Without more research we don't know whether it has higher or lower *regional* precipitation effects. [Douglas MacMartin, United States of America]	Taken into account. This sub-section is revised and this sentence is removed.
51001	86	25	86	25	Is there a word missing after "Ozone"? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sub-section is revised and this sentence is removed.
96475	86	25			Please verify, why the reduced side effects on the hydrological cycle mentioned here are included in Table 4.7, but the effects on the "Stratospheric Ozone" also mentioned in the text are not included in Table 4.7 (page 82, CCT). [Nicole Wilke, Germany]	Taken into account. Both the text and the Table have been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15943	86	31	86	49	The IPCC report up to this point has made no assessment of the safe rise in global temperatures before irreversible effects become unavoidable in the long term, yet it is the safe rise in global temperatures that forms the strategic framework for SRM deployment. We demonstrated in our submission to the UN Talanoa Dialogue (prepared in co-operation with MacCraken et al) that the maximum long term safe temperature rise can be no more than 0.5degC above the 1880-1910 base line, and this is roughly commensurate with the global temperatures circa 1980 when interacting and amplifying feedback mechanisms were first observed. A return to these safe conditions is impossible without SRM. Hence, this section should make reference to the maximum long term safe and sustainably temperature rise, rather than implicitly refer to the politically set temperature targets of 1.5degC and 2degC. See https://cop23.unfccc.int/documents/65014 [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	To define what is a 'safe' level of global temperature in the long term is beyond the scope of this Chapter. The assessments here are policy relevant but not policy prescriptive.
68697	86	38	86	38	Not clear what “large”means. Significant, or large compared to not applying SRM? [Simone Tilmes, United States of America]	Taken into account. Text is revised.
11439	86	38	86	39	would read better to put in context; "However, while smaller than the changes with equatorial injection (Kravitz et al 2019),…" [Douglas MacMartin, United States of America]	Taken into account. The content in the sub-section 'Strategically designed SRM' is merged into other sub-sections.
68695	86	41	86	49	Peakshaving experiment could be mentioned as a strategic approach of SRM applications. [Simone Tilmes, United States of America]	Taken into account. The content in the sub-section 'Strategically designed SRM' is merged into other sub-sections and text is revised.
53103	86	45	86	49	Moreover, and although this is mainly the topic of WG3, finely tuned SRM strategies open a Pandora's box and raise the question of defining what is an optimum global climate and who should be the main beneficiaries (any reference?). [Hervé Douville, France]	Taken into account. There is no discussion of optimal climate here. The content in the sub-section 'Strategically designed SRM' is merged into other sub-sections and text is revised.
114541	86	52	87	10	This paper may be relevant here: Detecting sulphate aerosol geoengineering with different methods. https://www.nature.com/articles/srep39169 [Jan Fuglestvedt, Norway]	Taken into account. Corresponding discussion is added. All references are updated where required.
127609	86	52	87	10	The recent study by Diamond et al. (2020; AGU advances, doi:10l1029/2019AV000111) provides information on time scales of detectability of marine cloud brightening specifically that could be included here. [Trigg Talley, United States of America]	Taken into account. The suggested reference is cited in the revised text in the subsection on MCB. The Diamond et al paper discusses the detectability of cloud properties and radiative forcing due to MCB at local scales, which is not directly relevant to the discussion here.
80023	86	52	87	10	I think this paragraph needs a note about detectability of SRM activity other than via temperature change. As written in the paragraph the detection requires a large temperature signal , or it will take very long (> two decades) to be detected. This motivates finding other ways to detect SRM activity. This could be done through a LIDAR network that monitors stratospheric aerosol burden or long-time measurements of stratospheric temperatures (via airplanes and balloons). Today, it would not be possible to immediately detect SRM activity, which is a risk on its own. [Gabriel Chiodo, Switzerland]	Taken into account. Corresponding text is added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11441	86	53	86	54	Unclear what a "full test" is… something of Pinatubo scale might answer questions about global mean temperature response, but not the regional response, which would require more; someone reading this sentence might infer that one could learn the regional response from a Pinatubo-scale "test". (And the stratospheric aerosol properties might be resolvable with something smaller than a Pinatubo.) There is some discussion of this in multiple papers, though not clearly articulated in any single one; for an old one there's MacMynowski et al 2011 (“Can we test geoengineering?” Royal Soc. J. Energy & Environmental Science, 4(12), pp 5044-5052, 2011. (doi: 10.1039/C1EE01256H) [Douglas MacMartin, United States of America]	Taken into account. Text is revised.
68699	86	53	86	55	1. Cooling was only 0.3C from Mt Pinatubo, the smaller effect is due to the slow response of the ocean to a short-term cooling event. 2. Continuous injections of sulfur will lead to a larger cooling because sea-surface temperatures will adjust within a few years. There is a large range of efficiency in different models, providing only one number for the cooling (0.5C) and citing one paper is not sufficient. Gving a range of different temperature responses would be recommended. [Simone Tilmes, United States of America]	Taken into account. Text is revised .
127611	86	53	86	55	Given that eruptions smaller than Pinatubo are routinely detected in stratospheric and tropospheric temperatures, the idea that any SRM test would need to be Pinatubo-sized seems exaggerated. A vertical fingerprint of change should be detectable at much smaller values. Thus, the very likely designation seems over confident. [Trigg Talley, United States of America]	Taken into account. Text is revised. The discussion mainly focusses on the detection of climate response to SRM.
127613	86	53	86	55	"Hence, a full test of the climate system response to SRM will very likely require an SRM forcing of the size produced by the 1991 Mount Pinatubo eruption which produced global mean cooling of about 0.5K (Robock et al., 2010)." Is this statement actually for SAI -- not for SRM by any mechanism? Take care when referring to results that were for studies of SAI specifically, rather than for SRM more generally. [Trigg Talley, United States of America]	Taken into account. Text is revised to refer to SAI in specific.
41979	86	54			Chapter 4 page 86 line 54 refers to the need for very large forcing of stratospheric sulphur to avoid masking by internal variability. For marine cloud brightening very small energy changes, far below the contrast detection threshold of the human eye, can be detected by superposition of satellite images. Papers demonstrating examples of this are available. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text for this sub-section is revised. This sub-section assesses detectability of global and regional climate response to SRM based on existing literature.
6681	86	55	86	55	The above comment applies here also. Why is the reference here to Robock et al. (2010) when the reference to the same point on Page 85, line 26, is to Soden et al.(2002)? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Here we cite Robock et al. (2010) for SAI Detectability. In the SAI section, we cite Soden et al. (2002) for climate effect of 1991 Mount Pinatubo eruption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116351	86		86		For section 4.6 on detectability of responses to emission reductions, or to SRM, could it be possible to also consider ocean heat content, in addition to surface warming? (also building on insights from the effect of past eruptions on ocean heat) (see ch 3) [Valerie Masson-Delmotte, France]	Taken into account. Discussion is based on what is available in the literature.
51005	87	2	87	2	after one to two decades' - so would it need to be equivalent to Pinatubo erupting ever year for 2 decades? Or just an amount similar to this eruption and then detection 10 -20 years later? Or equivalent to pinatubo spread over 10 - 20 years and then, eventually, detection? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text is revised to make the message clear.
127615	87	13	87	33	Although there is virtually no doubt that an abrupt termination of SRM would cause a rapid increase in temperature with knock-on effects (IF THE CO2 FORCING WERE LARGE AT THE TIME OF TERMINATION), it is also worth pointing out in the same sentence that there is no identifiable scientific motivation to do this; and, as stated later on lines 25-27, simple strategies could be used to avoid the issue. Furthermore, the effect is not instantaneous and inevitable (relatively rapid societal responses could address this without significant consequences). Put the abrupt change issue into context by indicating that this effect is only important when SRM forcing is large to counter large CO2 forcing, and rephrase the sentences and paragraph to help the reader to understand that this outcome is only relevant in certain contexts. [Trigg Talley, United States of America]	Taken into account. Text is revised to make the message clear.
127617	87	13	87	33	[PROGRESS] Most of these conclusions were made in AR5 and SR1.5. Can authors identify progress on these issues since then? Perhaps the discussion needs to be refactored into various subsections? [Trigg Talley, United States of America]	Taken into account. Text is revised and references are updated
5793	87	13	87	33	Overall, this is a more accurate description of termination that one often sees. As a small point, to be more precise, sudden and sustained termination of SRM would need to occur after a substantially long period of SRM, at a significant magnitude, in order for harmful termination "shock" to occur. [Jesse Reynolds, United States of America]	Taken into account. Text is revised.
41981	87	13	87	33	Chapter 4 page 87 lines 13 onwards discuss abrupt termination resulting in the rapid (10 year) increase in temperatures to the values set by greenhouse gas concentrations. I can agree that abrupt changes are bad but argue that the failure of many other very desirable technologies are serious in much shorter times. For example electricity generation in 20 milliseconds, the internet in two seconds, air traffic control in two minutes, water purification and food distribution in two days. Spray vessels can be repaired or replaced in times much less than 10 years. It would be much more serious if the IPCC passage were to read 'once Event X has started it could never be reversed'. This is actually the case for several climate-related tipping points which marine cloud brightening could prevent. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text in this subsection is revised. The subsection mainly assesses climate response to SRM termination.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71215	87	14	87	17	I am not comfortable when the word "high confidence" is used on a SRM subject which is still highly hypothetical and drown from mainly idealized Scenarios [Michael Mugarura, Germany]	Taken into account. "High confidence" derives from model consistency and our understanding of the physics of the climate system.
15945	87	14	87	33	As noted in the previous section, there are no proven techniques for CDR that are likely to be sufficiently scalable and sustainable to allow the removal of CO2 down to levels that are able to restore the planet's radiative budget and allow resoration of previous Holocene conditions. Likewise, upwards pressure on CO2 is increasing and will likely increase further in the future due to the combination of factors such as population growth, increasing adaption burdens to climate change, and unconstrained arms races. Thus, SRM should be considered as a long term solution and be capable of being deployed by future societies that are otherwise pre-occupied in the struggles to survive against climate change. This dictates the design requirements for SRM solutions. Furthermore, in the hypothetical ideal of fossil fuel being replaced by zero carbon, then the fall in aerosol cooling would necessitate SRM intervention. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The scale issue of CDR is briefly mentioned here.
51007	87	16	87	17	Two of the references supporting this important statement pre-date the AR5; are there more up-to-date references these could be replaced with? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the revised text, updated references are used.
96477	87	21	87	31	Both sentences in line 21 ("Sudden and sustained […]") and 29 ("The large warming rates […]") describe the effects of a "sudden and sustained termination of SRM" on land and ocean temperature velocities. Please clarify the differences of both statements or shorten the text. [Nicole Wilke, Germany]	Taken into account. Text is revised.
1829	87	23	87	24	I don't understand what "Average 5-year summer precipitation trends on land is negative" means. In what scenario? And if this is correct, should you change "is" to "would be?" We found (Trisos et al. 2018) that sudden implementation of SRM in the G4 scenario would produce drying of the Amazon associated with an El Niño pattern in the Pacific in the first decade (Fig. 2e), but this is region specific. We did not find drying over land in the first 5 years (Fig. 1c). The signal is noisy, but there was no negative trend in the first 5 years, averaged over 4 GCMs with 3 ensemble members each. There are regions of drying (Fig. 2f), but the land average is not negative. [Alan Robock, United States of America]	Taken into account. The corresponding discussion is removed.
53105	87	24			is "however" negative (may need an explanation?) [Hervé Douville, France]	Taken into account. Text is revised.
11443	87	26	87	26	And this was explicitly simulated in MacMartin et al 2014..."Solar geoengineering to limit rates of change", Phil. Trans. Royal Soc. A, 372(2031), 2014. doi:10.1098/rsta.2014.0134, so that would be a more relevant reference than the ones you have. [Douglas MacMartin, United States of America]	Taken into account. This reference is included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115435	87	26	87	29	There are no "simple policies" that could be applied to the governance of SRM in any case. Plu the Parker -Irvine article referes to "simple", all other authors on governance of SRM coincide that it is an extremely difficult issue, full of conflict of interests, etc. To suggest that termination shock could be "managed" by simple policies is completely wrong. The management of policies related to cliamte change, without including the many new challenges and variables of SRM and geongineerring has proven extremely difficult at UNFCCC and other UN fora. Please of respect to the seriousness of the challenges, delete the reference to Parker-Irvine article. Futhermore, there are not many doubts related to the termination effects, as line 29 says. Most articles referred to the issue show it would be very negative, with rebound effects and strong negative impacts on biodiversity (Trisos et al, 2018) [SILVIA RIBEIRO, Mexico]	Taken into account. The corresponding discussion and references are removed.
68701	87	27	87	27	Termination may not be so simple to prevent, especially in case of natural disasters. But also polical circumstances may result in at least short term terminations. [Simone Tilmes, United States of America]	Taken into account. The corresponding discussion and references are removed.
111847	87	27	87	27	Not sure if readers will be make sense of "simple policies" [Oliver Geden, Germany]	Taken into account. The corresponding discussion and references are removed.
1831	87	27			"simple policies could prevent SRM termination" is speculation, and should come with a normative assessment. It is easy to think of scenarios when "simple policies" would not work, including threats from nations feeling disadvantaged by continuing SRM (e.g., geoengineering is causing drought or flooding or some other extreme in my country), war, technical breakdowns, and pandemics. IPCC should not claim that SRM termination can easily be prevented as a blanket statement to dismiss the potential danger. [Alan Robock, United States of America]	Taken into account. The corresponding discussion and references are removed.
114543	87	29	87	31	Is this the assessment of the author team? Should not be based on a single paper. I guess you simply refer to the findings in the cited paper. [Jan Fuglestvedt, Norway]	Taken into account. Text is revised.
23259	87	34	87	37	In addition assessed above we suggest that SRM without CDR by AFOLU will occur high acid rain-crisis and sea-acidification rapidly. Thus Reserchs on AFOLU in Desert are more important. [Seokhwan Jeong, Republic of Korea]	This discussion is beyond the scope of this chapter.
34887	87	36	90	12	Climate change projections beyond 2100 better belong to the realm of science fiction until there is a lot better understanding of climate science and observations in the coming decades. [Jim O'Brien, Ireland]	reject. The section is of great interest to many parties

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41419	87	36	92	19	While it is understood that a lot of the long-term, post-2100 information is dealt with in the process chapters, chapter 4 is still the place to compile the global long-term projections. Unfortunately, sections 4.7.1 and 4.7.2 can be regarded as the weakest sections of the entire chapter, despite the great importance of post-2100 projections, in particular when it comes to committed climate impacts, also informing very sensitive policy discussions like loss and damage. Presenting a two-sentence subsection on SLR committments (4.7.2.2.3), for example, cannot be considered a robust assessment of literature since AR5, i am afraid. Many parts of these two sections need serious work to still meet the IPCC assessment criteria. [Alexander Nauels, Germany]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. we clearly signpost to that assessment instead of the short coverage here.
71947	87	41		43	It wou.d still be valuable to examine the GCM results [John Church, Australia]	taken into account. GCM results are used where available
6683	87	42	87	42	The wording "fully complex" is perhaps best avoided, as there is no end to the complexity that might be introduced into a GCM. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Done
41421	87	42	87	42	Please provide information on which EMICs have been used to complement the ESM projections, and why you have not made use of emulators/SCMs for specific processes, like sea-level rise? Given you mainly focussing on RCPs post 2100, there is ample peer-reviewed literature available to complement the current assessment. [Alexander Nauels, Germany]	taken into account. We now make more use of CMIP6 ESMs and emulator approach and rely much less on EMICs
114545	87	46	88	19	Very good that you make these clarifications in teh start. [Jan Fuglestvedt, Norway]	taken into account. Thank you. No action required
89755	87	47	88	12	Check that commitment defimitions are consistent with definitions in WGI glossary. I also sugges to order the types of commitment by relevance to WGI, i.e. constant composition, zero emissions, constant emissions, infrastructure. [Kirsten Zickfeld, Canada]	accepted. Definitions have been harmonised across WG1 through breakout discussions
114547	88	2	88	2	I sugest inserting "atmospheric" before "composition" [Jan Fuglestvedt, Norway]	accepted. done
51009	88	9	88	9	missing word: 'committed us to' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. done
114551	88	20	90	12	Considering climate chnages beyoind 2100 is important and this section is very welcome [Jan Fuglestvedt, Norway]	taken into account. Thank you. No action required
96479	88	22	88	27	The SROCC reported on results till year 2300. We suggest to add here a summary and describe what is new in AR6 compared to SROCC results. [Nicole Wilke, Germany]	accepted. We now leave coverage of sea-level rise and updates since SROCC to chapter 9, and draw on their assessment statements where required
114549	88	23	88	24	I think you can change "less" to "not" [Jan Fuglestvedt, Norway]	accepted. done
96481	88	25	88	26	For readability reasons please include the meaning of the abbreviation "ECP", as it is mentioned for the first time in this chapter. The term "ECPs" is here referred to as "CMIP5 extension scenarios to 2300 and beyond (ECPs)". This is not consistent with the term definition in the glossary (page AG-36, lines 25-26): "Extended concentration pathways describe extensions of the RCPs from 2100 to 2300 that were [...]". Please verify discrepancy. [Nicole Wilke, Germany]	accepted. done

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87873	88	40	88	42	Please note that Tokarska et al. 2016 NCC also look at the carbon cycle responses beyond the year 2100 and perhaphs should be included here. Reference: Tokarska, K., Gillett, N., Weaver, A. et al. The climate response to five trillion tonnes of carbon. Nature Clim Change 6, 851–855 (2016). https://doi.org/10.1038/nclimate3036 [Katarzyna Tokarska, Switzerland]	accepted. Literature coverage has been updated and increased
127619	88	40			Also Rind et al. (2018 ,doi:10.1029/2017JD027149) showed multi-century instability in long runs with extreme forcings that are not visible in the trends to 2100. [Trigg Talley, United States of America]	accepted. Literature coverage has been updated and increased
93415	88	42	88	42	Moore et al., 2018 ref is not in the list. I presume it is Moore, J. K., Fu, W. W., Primeau, F., Britten, G. L., Lindsay, K., Long, M., Doney, S. C., Mahowald, N., Hoffman, F., & Randerson, J. T. (2018). Sustained climate warming drives declining marine biological productivity. Science, 359(6380), 1139–1142. https://doi.org/10.1126/science.aao6379 [Carles Pelejero, Spain]	accepted. Reference corrected
41423	88	50			Why do you present steric SLR only, and only based on one study (Palmer et al 2018)? This cannot be called a robust assessment at all, also when looking at the correponding figure. There are large uncertainties involved in long-term SLR, particularly regarding AIS and GIS responses. Still, this shouldn't lead to simply being quiet about those. Other SLR component responses, like glaciers, can be quantified quite well beyon 2100 (see Marzeion et al etc). SLR is a crucially important, an existential climate variable for particularly vulnerable countries like SIDS. Hence, a lot more effort should to go into this assessment. [Alexander Nauels, Germany]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
96483	88	50			Figure 4.42, legend: As the figure shows simulated climate changes up to 2300 from extensions of the RCPs from 2100 to 2300, and these are named "ECP" in this section 4.7.1 (e. g. ECP2.6, ECP4.5, please see lines 31-38), please consider to use the nomenclature in the legend and in the graphs (graph (a), upper row). The legend would accordingly be completed as follows: "Simulated climate changes up to 2300 from the CMIP5 multi-model ensemble under the four Extended RCP scenarios"). [Nicole Wilke, Germany]	accepted. done
96485	88	52			Figure 4.42, legend, "(d) thermal sea level rise": Please consider to use consistent terms on "thermosteric" sea level (please see glossary entry, page AG-43, lines 51-52, and page AG-44, line 4) [Nicole Wilke, Germany]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
116355	88		88		For aspects related to long term responses, can insights from past warm phases be also combined with the result of projections? [Valerie Masson-Delmotte, France]	accepted. Links to paleo assessment have been made, and are consistent with chapter 2
106313	89	4	89	23	Given that temperature projections in Chapter 4 make use of an emulator, it would be great for cross-chapter consistency if also the impact of SSP1-1.9 extended to 2300 would be considered and presented, possibly with an adjusted confidence statement. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	accepted. We make use of emulators to consider GSAT projections of all the main scenarios
11515	89	14	89	14	"above 20 degrees" -> "above 20°C" (some people could misread this as 20 degrees Fahrenheit, one never knows). Same applies 2 lines further down [Gerhard Krinner, France]	accepted. done

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17027	89	15	89	15	Randerson et al., (2015) [Sergio Aquino, Canada]	not applicable. This reference is already cited here
87875	89	23	89	23	It may be good to refer to Chapter 5 section 5.5.1.2. that discusses limits of TCRE in detail. [Katarzyna Tokarska, Switzerland]	accepted. We cross-reference Ch.5 on TCRE text
41425	89	26	89	35	Not a single post AR5 study is cited in this subsection which is extremely worrying as it is hardly possible that there has been no publications on this important topic since 2013. Please thoroughly revise and update! [Alexander Nauels, Germany]	accepted. Literature coverage has been updated
53107	89	34			Figure 4.42: If the sampling of RCP6.0 cannot be improved, it might be better not to show it with only two models given the potentially misleading comparison with other RCPs? [Hervé Douville, France]	not applicable. we no longer show RCPs at all and now draw on CMIP6 simulations and emulators
41427	89	38	89	45	Also this subsection is incredibly short. Only one study is cited for the assessment of the post-2100 reponse of Artic sea-ice. Please provide more lines of eividence! [Alexander Nauels, Germany]	accepted. Literature coverage has been updated
71949	89	48	90	2	This is inadequate - users need to know the full GMSL, not ust the steric component. [John Church, Australia]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
15189	89	48	90	2	This short section on GMSL to 2300 is far too brief. Is this supposed to be a placeholder, while awaiting new model results? This chapter either needs to offer a full assessment of the research here, or point readers to Chapter 9. [Simon Donner, Canada]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
9821	89	48	90	2	See also the assessment of multi-century and multi-millennial sea-level change in 9.6.3.5 [Robert Kopp, United States of America]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
96487	89	48	90	2	Chapter 4.7.1.4 "Global Mean Sea Level" only contains statements regarding the steric GMSL and nothing on the contribution from polar ice sheets and glaciers. This omission needs to be remedied or clearly stated. Another option is to refer directly to Chapter 9.6.3.5 and include all information there. There seems little value in separating assessments of the steric and other component of SLR beyond 2100. [Nicole Wilke, Germany]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
87143	89	48			We are aware of the large uncertainities that exist with respect to long term SLR projections however we believe that a more in-depth assessment of the topic is needed because of its importance to SIDS especially as it relates to the slow but lasting SLR response. Authors for different chapters need to better coordinate to ensure a more robust picture of the global climate change commitment and long-term change assessment. [Jacqueline Spence, Jamaica]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
100011	89	48			While we acknowledge the large uncertainties involved when it comes to long-term SLR projections, we would also like to call for a more in-depth assessment of long-term SLR, as this topic is of utmost importance to SIDS. This information must not focus only on thermal expansion effects but has to capture all main drivers, including the risk of irreversible processes trigerred in the Polar regions and the SLR potential that comes with these processes. [Caroline Eugene, Saint Lucia]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.

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84151	89	48			Long-term SLR projections is very important to SIDS. While we acknowledge the large uncertainties involved when it comes to long-term SLR projections, more in-depth assessment of long-term SLR would be useful. In this regard, the information must not focus only on thermal expansion effects but also other main drivers, including the risk of irreversible processes triggered in the Polar Regions and the SLR potential that comes with these processes. [Jeffers Cheryl , Saint Kitts and Nevis]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
11447	89	50	90	2	IPCC was appropriately criticized after AR5 for implicitly ignoring the SLR due to ice sheets. This section title is about global mean sea level, but the paragraph is only about the steric component. At an absolute minimum, this needs a sentence that explicitly states that there are multiple contributors to SLR, and we have very high uncertainty on the contribution from ice sheets, but that that contribution could be substantially larger than the steric component. Otherwise this paragraph seems to be deliberately misleading. [Douglas MacMartin, United States of America]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
41429	89	50	90	2	You are presenting the post-2100 global SLR assessment based on a single study that only provides the steric signal?! This can hardly be called a comprehensive assessment. It is true that ESMs generally do not provide post-2100 total GMSLR projections, however EMICs could (in theory). You acknowledge the fact that ESMs cannot always provide the long-term information either (p87 l41) and the community has therefore made great efforts to develop alternative methods, like GCM/ESM emulators that can provide longer-term global information until 2200/2300, see Kopp et al 2014, Nauels et al 2017 just to name a few. These are not the classical "semi-empirical approaches" that have rightly drawn criticism in AR5. And what about the most critical contributions to global sea-level rise? The Antarctic contribution to future global sea-level rise is on everybody's mind when it comes to long-term global projections. Several process-based studies fed by CMIP models have tried to shed light on this topic since AR5. There is not even a single mention of the drivers that will become much more critical than the thermosteric component, not even a reference to chapter 9. The large uncertainties in this context have to be mentioned (and the fact the these uncertainties mostly cover the tails that would make everything much worse). Unfortunately, the authors fail to provide urgently needed SLR information on time scales that are of great societal importance. This section has to be thorougly revised. [Alexander Nauels, Germany]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
51011	89	51	89	51	the steric GMSL' - it would be useful to specify here that these projections include the thermal expansion component of total GMSL rise only and that additional contributions from glaciers and AIS and GIS are projected. For context it would be helpful to mention what proportion of GMSL rise is currently due to the steric component (approx one third?). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41431	90	5	90	12	Also here, please revisit this subsection and provide more lines of (existing) evidence. [Alexander Nauels, Germany]	accepted. We remove the section on AMOC as this is covered much more comprehensively in chapter 9. we clearly signpost to that assessment instead of the short coverage here.
96489	90	7	90	12	In the SROCC one finds a detailed discussion about AMOC and ice sheet changes and whether tipping points exist for both. The relation to the SROCC report is missing here for the AMOC, while ice sheets are not discussed here despite their crucial role for long-term climate development - repeating the main results from SROCC should be feasible. [Nicole Wilke, Germany]	accepted. We remove the section on AMOC as this is covered much more comprehensively in chapter 9. we clearly signpost to that assessment instead of the short coverage here.
127621	90	12			Note that Rind et al. (2018, doi:10.1029/2017JD027149) show interesting very long term shifts in the AMOC under 4xCO2 or RCP8.5 beyond 2100. [Trigg Talley, United States of America]	accepted. We remove the section on AMOC as this is covered much more comprehensively in chapter 9. we forward this study to Chapter 9 and clearly signpost to that assessment instead of the short coverage here.
2903	90	13	90	13	Summary table of main climate changes for the 21st century as projected by CMIP6 should be given. [Zong Ci Zhao, China]	rejected. Not part of this section. The whole chapter deals with climate changes in the 21st century.
114557	90	15	96	17	Some of these concepts are also introduced in ch1. Consistency check between the two chapters (and probably other chapters as well) is needed [Jan Fuglestvedt, Norway]	accepted. These definitions have been revised and agreed following thorough cross-chapter breakout discussions. Glossary is also updated
114559	90	15	96	17	For Irreversibility and tipping points defintions are given in the text. Can you highlight more the different meanings and usages? And being consistent with the glossary [Jan Fuglestvedt, Norway]	accepted. These definitions have been revised and agreed following thorough cross-chapter breakout discussions. Glossary is also updated
44093	90	15			For LDCs, issues related to climate change commitment and long-term changes are of very high importance, in particular regarding the loss & damage discussion. It appears that section 4.7.2 has not received enough attention as subsection are very short and do not provide comprehensive assessments for individual variables. It is important that more quantitative information is elevated to the executive summary of the chapter, so that this information can be potentially utilised in the SPM. Please thoroughly expand the assessment presented in section 4.7.2! [Lamin Mai Touray, Gambia]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
132525	90	17	90	36	This is a strange paragraph since it says almost nothing about the climate change under constant forcing / atmospheric composition. I suggest revising to start with a description of why there is warming in the pipeline with fixed forcing, and perhaps even an estimate of that value based on today's level of energy imbalance (0.8 W/m2) and the value of climate feedback from Chapter 7's assessment of ECS (alpha = 4 W/m2 divided by 3 C). Then the committed warming with constant forcing is just 0.8/alpha = 0.6 C. You could add uncertainties as welll. The paragraph you have written discusses caveats to this estimate coming from time-dependent feedbacks, but that seems like a nuance. [Kyle Armour, United States of America]	accepted. This paragraph deals with climate forcing under idealised fixed forcing, but we have moved the first paragraph of 4.7.2.2 to this section too
11517	90	19	90	23	Although the difference between the Charney ECS and the Earth System ECS is explained in Box7.1 you refer to here, it might be useful to say which ECS you talk about here. Even the LongRunMIP ECS is a Charney Ecs, isn't it? [Gerhard Krinner, France]	accepted. We mention earth-system sensitivity too

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96491	90	19			In the glossary entry on "ECS" the term is referred to a change in the GMST (please see glossary, page AG-10, lines 52-53). In this section, "the change in GSAT" is mentioned. [Nicole Wilke, Germany]	rejected. GSAT is the correct measure of temperature for the definition of ECS and TCR (as laid out in chapter 7, box 7.1)
53109	90	34	90	36	Slower Earth System feedbacks (e.g., soil moisture, vegetation, permafrost) may also contribute to enhance global warming on centennial timescales (low confidence)? [Hervé Douville, France]	accepted. We mention earth-system sensitivity too
51013	90	35	90	35	fast feedbacks' - such as forest dieback? Examples here would be helpful. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Fast feedbacks such as changes in clouds. Forest dieback and ice sheets are now mentioned as not included in ECS
89757	90	41	90	42	Check consistency of definition with glossary [Kirsten Zickfeld, Canada]	accepted. These definitions have been revised and agreed following thorough cross-chapter breakout discussions. Glossary is also updated
132527	90	41	90	43	This is the kind of description I expected to see in the paragraph above. [Kyle Armour, United States of America]	accepted. This paragraph moved to the prior sub-section
114553	90	48	90	52	Can you add nuances about CO2 vs GHG (CO2+ on-CO2) for this concept? [Jan Fuglestvedt, Norway]	accepted. We discuss CO2-only-ZEC and make this explicit
132529	90	48	91	26	I found the discussion of the Zero Emmisions Commmtment (ZEC) to be confusing. More clarity is needed as to whether ZEC is defined with respect to all emisions (including aerosols, methane, nitrous oxide, etc) or whether it is defined with respect to CO2 emissions only (holding all other emissions fixed). This difference matters since a ZEC with a cessation of all emissions leads to substantial transient warming upon emissions cessation; relevant papers are doi:10.1007/s10584-005-9027-9, 10.1029/2010GL045850, and 10.1038/NCLIMATE3357. I suggest that both definitions of ZEC are clearly laid out, quantified, and compared. Also, it should be explained which definition is most relevant for the calculation of TCRE, and why. I would have though the ZEC with respect to all emissions is more relevant, but it looks like ZEC with respect to CO2 only is used here and in Chapter 5. Is the neglect of cessation of other emissions in the ZEC here a caveat for the calculation of TCRE? [Kyle Armour, United States of America]	accepted. We discuss CO2-only-ZEC (as required for carbon budgets) and make this explicit
99367	90	48	190	52	This paragraph can be important to explain the meaining and typical magnitude of ZEC. However, it currently feels unclear. The relation with TCRE is important, as it is because the ZEC is small that cumulative emissions is a useful concept. Please clarify and provide links to other chapters dealing with cumulative emissions and carbon budgets (e.g. section 5.5). In addition, I am wondering if there shouldn't be more explanations on the origin of ZEC and its consequences: if a significant part of it is related to short-lived forcers (especially sulfate aerosols and black carbon), then the change in GSAT could be a temporary overshoot rather than a long term "commitment". Does this have consequences for how the carbon budget is defined? [Philippe Marbaix, Belgium]	accepted. We give more details on ZEC and link to 5.5. The ZEC considered here and used for carbon budgets is the CO2-only one (not including other GHGs or aerosols), and we now make this explicit. The glossary definition is updated to reflect this more clearly
116357	90		90		For aspects related to abrupt change and irreversibility, could insights from paleoclimate be also used, where relevant? [Valerie Masson-Delmotte, France]	rejected. These aspects are already treated in other chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38607	90		99		"The message is not very clear and consitent. The chapter needs revision, because the arguments can not be followed" [Aribert Peters, Germany]	taken into account. Text has been further revised and reviewed internally for clarity
114555	91	1	91	9	Please consider updates in the literature on this topic. [Jan Fuglestvedt, Norway]	taken into account - literature assessed has been updated
106315	91	2	91	3	This description is imprecise. Several of these studies first simulate a clear decline in temperatures and then a rebound on century timescales. It's worthwhile describing this more precisely. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Timescales of response and metrics used have been be clarified
89761	91	4	91	4	Include reference to Ehlert et al., 2017, ERL [Kirsten Zickfeld, Canada]	accepted. Literature is updated
9823	91	11	91	14	clarify whether this is 1000 PgC added *on top of* a 1%/yr increase, or 1000 PgC added via a 1%/yr increase [Robert Kopp, United States of America]	accepted. Text clarified.
10037	91	12	91	12	18 Models [Andrew MacDougall, Canada]	accepted. Details updated with 20 available models at 31 Jan 2021
55499	91	12			MacDougall et al.,Submitted (included in TEXT). Chek publication date [Maria del Pilar Bueno Rubial, Argentina]	accepted. Paper is now published, and citation updated
87877	91	16	91	17	This is an important conclusion, but it seems to contradict several statements in this chapter above that make the opposite conclusion that temperatures will rise after emissions are stopped. Please consider moving this section sooner, and make sure that the conclusions are consistent across this chapter. [Katarzyna Tokarska, Switzerland]	taken into account. This statement is correct, and we have checked for contradictions across the chapter. Our ES makes this clear. The chapter also covers that we may see committed climate changes (esp. sea level) even if GSAT is not committed
89759	91	19	91	19	Calrify how ZEC is calculated. [Kirsten Zickfeld, Canada]	rejected. Method is described in text: 20 year mean GSAT change.
89763	91	20	91	21	Goodwin et al., 2014, Nat. Geosc.; Ehlert et al., 2017, ERL also analyze this balance [Kirsten Zickfeld, Canada]	accepted. Literature has been updated
10039	91	22	92	23	Update numbers to ZECMIP final results [Andrew MacDougall, Canada]	accepted. Details updated with 20 available models at 31 Jan 2021
21703	91	24	91	26	Is it worth pointing out that this is less than interannual (and perhaps even interdecadal) natural variability in GSAT as indicated by observations and ESM control runs? [Peter Thorne, Ireland]	accepted. Done
54967	91	28	91	46	This is not shown in the final, published, version of Sigmond et al, 2020 [Nancy Hamzawi, Canada]	Accepted
11519	91	29	91	33	Strong direct link between Arctic sea ice and cumulative emissions is also suggested by Notz&Stroeve, Science, 2016 [Gerhard Krinner, France]	accepted
19239	91	36	91	39	Are there numbers for the ZEC sea level? some quantification would be useful, if possible. [Anne-Marie Treguier, France]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15191	91	36	91	39	Similar issue here as with 4.7.1.4. This short section on GMSL commitment is far too brief. The long-term commitment to GMSL from present-day and near-term emissions is one of the most important considerations for the world's governments that the IPCC is charged with assessing. If this is brief because the material is covered elsewhere in the report like chapter 9, then make that clear in the text. [Simon Donner, Canada]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
41433	91	36	91	39	Please understand that I am struggling with a two sentence assessment. How can this be comprehensive? And here, despite refraining from using anything else but GCM/ESM/EMIC data, you decide to cite a SCM study based on an ancient 2005 MAGICC version that is not even capturing uncertainties? In our 2019 PNAS study, we have tried to estimate the GMSLR commitment in 2300 following a cessation of current emissions. This study is cited in chapter 9. It seems like there has not been much interaction with Chapter 9 at all, which would be extremely worrying. [Alexander Nauels, Germany]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
71951	91	36		39	Can this be quantitative? [John Church, Australia]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
100013	91	36			A two sentence assessment on global SLR commitment is not satisfactory. There is much more literature available that would allow for a more in-depth and nuanced assessemt. Please provide more quantitative information, and expand the assessment. The entire section 7.4.2 appears to have not received the attention it should, given the importance of the commitment issue. [Caroline Eugene, Saint Lucia]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
84153	91	36			There is much more literature available that would allow for a more in-depth assessment. Please provide more quantitative information and expand the assessment. The entire section 7.4.2 needs greater attention instead of providing just two sentences that speak to the assessment on global SLR commitment. [Jeffers Cheryl , Saint Kitts and Nevis]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
9825	91	37	91	39	See also 9.6.3.5 [Robert Kopp, United States of America]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
89765	91	39	91	39	continued SLR under negative emissions was also shown in Tokarska et al., 2015, ERL. [Kirsten Zickfeld, Canada]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment
96493	91	42			For consistency reasons, please consider to adapt to "North Atlantic Meridional Overturning Circulation" (it is not clear why "North" Atlantic was specifically mentioned). [Nicole Wilke, Germany]	accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51015	91	43	91	43	by 'overshoots' in the context of AMOC, what does this mean? Please explain. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted
7819	91	43	91	44	The first two papers at least show AMOC overshooting when CO2 levels are reduced back to their preindustrial conditions. Terminating emissions means concentraions stay constant.Also in many (possibly all?) models in these papers the AMOC recovers to its preindustrial strength. Other references for this are Wu et al 2015 https://doi.org/10.1007/s00382-014-2302-6 and Sgubin et al 2015 https://doi.org/10.1007/s00382-014-2391-2. There is also similar discussion in section 4.7.2.3 [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	accepted
89767	91	44	91	49	Not direcly relevant to AMOC response to sero emissions. Move to section 4.6.1? [Kirsten Zickfeld, Canada]	accepted
96495	92	1			Figure 4.43, legend: In order to make the figure more comprehensible, please consider to briefly define the "1 % experiment", as explained in the corresponding paragraph (page 91, line 14): "[…] a 1 % per year CO2 increase.". Please use consistent nomenclature for the "1% experiment" or "A1", please also see comment on Table 4.8. [Nicole Wilke, Germany]	accepted. Legend updated
10041	92	11	92	11	Update numbers to ZECMIP final results. Also maybe round to 0.1K precision [Andrew MacDougall, Canada]	accepted. Details updated with 20 available models at 31 Jan 2021
96497	92	12			Table 4.8: Please use consistent nomenclature for the "1% experiment" or "A1", please see our comment on Figure 4.43. [Nicole Wilke, Germany]	accepted. done
71953	92	19	94	1	I think this section is inadequate, particularly for sea level commitments. [John Church, Australia]	taken into account. it was not the intention to marginalise sea-level rise, as this is covered in detail in chapter 9. The short coverage here is removed and the 4.7 introduction points explicitly to the chapter 9 assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132531	92	19	96	7	I find this discussion and classification of reversibility and tipping points to be confusing, The definition of reversibility used is "if the recovery process takes substantially longer than the time it takes to reach that state" and the defniition of a tipping point is "a level of change in the system properties beyond which a system reorganizes, often abruptly, and does not return to the intial state even if the drivers of the change are abated". By these definitions, I would say that sea ice loss is not reversible (because there is a long delay in its recovery with respect to CO2 forcing, coming from the delay in global temperature response to forcing); but that sea ice loss does not have a tipping point since there is no level of change in the system beyond which sea ice will not regrow back to the same state if radiative forcing is returned to a given level (sea ice area is just a function of global temperature after all). Yet, Table 4.10 classifies sea ice as reversible with a tipping point., leaving me quite confused. This is also inconsistent with Chapter 9, who correctly state that there is no tipping point for Arctic sea ice (see their ES point and supporting text on page 44; it apears they use a definition of reversibility that is different from yours as well). Meanwhile, the literature on sea ice classifies it as reversible with no tipping point because the system lacks hysterisis with respect to the forcing. I do not know the literature as welll regarding the other components in Table 4.10, but I worry that the murky defnitions used here will lead to similar confusion for them. We need to come up a definition for tipping points and reversibility that is consistent across the report and consistent with the literature. [Kyle Armour, United States of America]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These have also been checked with WG2 and WG3 and Glossary updated
80635	92	19	99	10	Very nice presentation of irreversible change, tipping points and lphl storylines. We should coordinate the presentation here with section 1.4.5 Abrupt change, tipping points and surprises, notably Figures 1.11 (Illustrating storylines) and 1.13 (Types of tipping point). Overall it seems consistent, but I think we have slightly distinct definitions and wording. Let's sit together at some point (on zoom, I guess...) and go through these sections for consistency. [Bjorn Samset, Norway]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These have also been checked with WG2 and WG3 and Glossary updated
39837	92	21	92	23	"substantially longer" -> SROCC changed this to "signficantly longer". I don't think there's a difference in meaning though, but we should decide whether to use "substantially" or "signficantly". Either way, these are vague words. Does it mean twice as longer or an order of magnitude longer? [TSU WGI, France]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These have also been checked with WG2 and WG3 and Glossary updated
53111	92	24			Replace "some" by "many"? [Hervé Douville, France]	accepted. done
96499	92	24			In order to increase comprehensibility, please include some of the reversible "aspects of the physical climate changes". [Nicole Wilke, Germany]	rejected. Table has been completely revised
87373	92	35	92	35	Sigmund et al. (9999) [Didier Swingedouw, France]	accepted, we have contacted the reviewer to get more details on this reference.
84289	92	37	93	1	not clear from where/what literature does this part of the assessment comes from [Annalisa Cherchi, Italy]	accepted, these terms are undergoing definitions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51017	92	38	92	39	Clarification of whether projected ocean heat content changes are / are not irreversible would be helpful here [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, has been clarified.
89769	92	38	92	39	"mixed layer depths tend to overshoot": unclear what is meant. [Kirsten Zickfeld, Canada]	accepted, it has been clarified that mixed layers tend to deepen more their original values under reversal of surface forcing due to accumulation of heat in the subsurface.
51019	92	39	92	39	Please expand in what 'overshoot' means in this context on ocean heat content [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the table has been completely re-structured.
88169	92	39	92	40	By "frozen soil" are you referring to permafrost or seasonal frost? It is unclear what the source of the comment regarding reversibility is with respect to permafrost (assuming this is what frozen soil means) and its likelihood. If this is related to results from ESM (e.g Boucher et al. 2012), these analysis don't consider that permafrost especially at more southern locations is not in equilibrium with the current climate and formed under colder conditions and is preserved today due to insulation provided by the peat -- we refer to this as ecosystem protected permafrost. The lag time therefore may be quite long and could be century to millenial scale. Also, various other changes may accompany permafrost thaw including changes in drainage and pond formation and also changes in vegetation which will also determine the rate at which permafrost may form. The models also only consider permafrost in the upper few metres (3 m) and don't consider that thaw will continue at greater depth ---- this has an effect on the reversibility over relatively short time scales. [Sharon Smith, Canada]	accepted. Text is clarified
87879	92	40	92	41	Please note that Tokarska et al. 2019 also assess carbon cycle responses to net-negative emission scenarios. It would be good to include these citation here for completness of the assessment of different studies. Reference: Tokarska et al. 2019. Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an Overshoot. AGU Earth's Future. [Katarzyna Tokarska, Switzerland]	accepted. literature updated
51021	92	40	92	41	long lag' mentioned twice - it would be useful to specify typically how long this is. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text clarified
51029	93	1	93	1	Suggested edit if accurate: 'Ice sheet depletion is largely..' (replace 'change' with 'depletion') [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. done
51031	93	2	93	2	Please specify what 'overshoot' means what in this context of ENSO responses [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted, a statement on the increased prevalence of an El Nino like pattern (as opposed to an increased amount of variability) has been specified
19243	93	15	93	15	Table 4.9 legend mentions "reversibility" but some of the examples (ice sheets) have irreversible behavior. Information on boreal forests is missing. Table 4.9 should me more consistent with table 4.10. [Anne-Marie Treguier, France]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87881	93	15	93	15	Table 4.9. Since this table provides a summary of different studies, it is missing the following papers: Global mean surface air temperature - reversible with lag: Tokarska and Zickfeld, 2015 ERL; Jones et al., 2016 ERL; MacDougall 2013 GRL Sea level rise: Irreversible and long timescale of response Tokarska and Zickfeld 2015 ERL Ocean carbon store: Schwinger and Tjiputra, 2019. Ocean Carbon Cycle Feedbacks Under Negative Emissions. GRL. Tokarska et al. 2019. Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an Overshoot. AGU Earth's Future. Land carbon store: Ziehn 2020 An assessment of land-based climate and carbon reversibility in the Australian Community Climate and Earth System Simulator. Mitigation and Adaptation Strategies for Global Change. https://doi.org/10.1007/s11027-019-09905-1 Krause et al. 2018. Large uncertainty in carbon uptake potential of land-based climate-change mitigation efforts. Global Change Biology Tokarska et al. 2019. Path Independence of Carbon Budgets When Meeting a Stringent Global Mean Temperature Target After an	literature updated, and table contents checked with other chapters
18045	93	15	93	15	Please include Ocean (De) Oxygenation in Table 4.9 [Lisa Levin, United States of America]	taken into account. We synthesise here the relevant aspects which are assessed throughout WG1 report
103037	93	15	93	15	Table 4.9: The terms in the column headings should be clarified and perhaps changed. "quantity" should be replaced with "component", "overshoots" shoudl be explained so that the table is self-explanatory. [Philippe Tulkens, Belgium]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments
103039	93	15	93	15	Table 4.9: The title of the table is confusing as it suggests that all the components listed change reversibly. It shoudl be rephrased, e.g. as: "Degree of reversibility of components of the climate system". [Philippe Tulkens, Belgium]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments
103041	93	15	93	15	Table 4.9: Add 'ocean' to 'Mixed layer depth'. [Philippe Tulkens, Belgium]	accepted. done
7821	93	15	93	15	The table says that AMOC overshoots 'under salinification of subtropical surface ocean'. This suggests that this is the forcing being changed (rather than GHG). Maybe say something like: Under reversal of GHG in some models. Also maybe say something about the forcing in the caption - are we assuming reversal of GHG unless specified? [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	accepted, the subset of models responding with salinification and additional freshwater experimental design deriving the results have been specified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89771	93	15	93	15	Table 4.9: Explain what different columns mean. [Kirsten Zickfeld, Canada]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments
93419	93	15	93	15	In Table 4.9, overshoots in 'surface alkalinity' are indicated for the 'ocean biogeochemistry' 'quantity', and the paper John et al 2015 is quoted. Although this paper may be a good reference for 'nitrogen cycle' I don't see it discussing about surface alkalinity. [Carles Pelejero, Spain]	Accepted, the table has been completely re-structured.
96501	93	15	93	16	Tab. 4.9: Jones et al 2009 say on the tropical forest dieback that "recovery is on such a long timescale as to make the die-back effectively irreversible on human timescales of the next 1–2 centuries.". It should please be specified that "lag" in Tab. 4.9 may refer to timescales irrelevant for humans, but it does not seem consistent with P 92 L 21-22 that considered timescales here as one century. [Nicole Wilke, Germany]	accepted. Text is revised and assessment agreed with chapter 5
51023	93	15	94	1	It would be very helpful to include a definition of 'irreversibility' in the description of Table 4.9 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments
51025	93	15	94	1	It is unclear what is meant by 'long timescale' of response - is it possible to quantify this? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted, this has been quantified to occur on longer than centennial timescales,
51027	93	15	94	1	These are important results that synthesise potential reversible and irreversible impacts of components of the climate system - t would be extremely beneficial to highlight these to policymakers in the SPM. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Key ones are highlighted. TS covers these also
88171	93	15			Table 4.9 - Linearly reversible with lag - What time period does this refer to decades, centuries, millenia? Theoretically most things would be reversible with a lag. [Sharon Smith, Canada]	accepted, timescales have been added
88173	93	15			Table 4.9, Permafrost Row (pg 94) - One reference isn't much to conclude permafrost loss is likely reversible on what I assume is relatively short time frame. Permafrost is not in equilibrium with the current climate and permafrost in the discontinuous zone formed under colder conditions and is preserved today due to insulation provided by the peat -- we refer to this as ecosystem protected permafrost. The lag time therefore may be quite long and could be century to millenial scale. Also, various other changes may accompany permafrost thaw including changes in drainage and pond formation and also changes in vegetation which will also determine the rate at which permafrost may form. It is unclear that the ESM analysis on which the results are based consider these processes nor do they consider the deeper heat transfer that occurs. It isn't just a question of GMST changes. [Sharon Smith, Canada]	accepted. Assessment now draws on chapter 5
15947	93	16	93	16	The table should also include subsea permafrost, as found in the East Siberian Sean and the Laptav Sea, and elsewhere in the Arctic region, from which large volumes of methane can be released with irreversible consequences. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. We synthesise here the relevant aspects which are assessed throughout WG1 report

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15949	93	16	93	16	The table should also include the death of the coral reefs, which will be irreversible once the ocean heat content in the surface layers exceeds a threshold amount, and it is already close to this given the current accelerating death rate of the corals. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	rejected. This is an important issue, but will be covered in WG2 report
46597	93		93		Must add "Arctic" to quantity "sea ice" [Dirk Notz, Germany]	accepted
116359	93		93		Can the table build on SROCC ch 6 and provide clarity on changes compared to AR5 and SROCC? Note, a common question is : "is current warming irreversible" (considering ocean heat uptake, etc). The response provided on land surface temperature suggests that it follows forcing, but what about the link to SST and what about the reversibility of CO2 concentration in the atmosphere (the timescale of atmospheric decay was not explicitely addressed in the current WGI SOD, but it was assessed in AR5 ch 6 in a FAQ). [Valerie Masson-Delmotte, France]	accepted. Table is harmonised with other chapters and builds on SROCC table
89773	93		94		Table 4.9: Additional literature on reversibility: Sea surface temperature: Mathesius et al., 2015; Li et al.; GRL, in revision (ms on DMS); ocean heat content/SLR: Tokarsa and Zickfeld, 2015, ERL; land carbon store: Tokarska and Zickfeld, 2015; Tokarska et al., Earth's Future 2019; permafrost: MacDougall et al., GRL, 2013; Ocean carbon store: Tokarska and Zickfeld, 2015; Tokarska et al., Earth's Future 2019; ocean biogeochemistry: Mathesius et al., 2015 also for sea surface ph and deoxygenation, Li et al., GRL, in revision (pH, oxygen at sea surface and depth); ice sheets: MacDougall, GRL, 2013. [Kirsten Zickfeld, Canada]	accepted. literature updated
96503	94	0			Table 4.8, column "Linearly reversible lag": For consistency reasons please adapt to "permafrost" to "permafrost carbon" (as named in page 92, line 40, and Table 4.10). [Nicole Wilke, Germany]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments
96505	94	0			Table 4.8, column "Linearly reversible lag": For consistency reasons within the table, please also indicate likelihood for "Land carbon store" as mentioned in page 92, line 39 (likely). [Nicole Wilke, Germany]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments
14791	94	1	94	1	Table 4.9: the purpose and organization of this table is unclear to the reader, even in light of references to this Table in the text. Suggest removing or clarifying Table purpose/intent. Also - the ice sheet references here are very old - suggest assessing newer literature, i.e. that in Chapter 9. [Jeremy Fyke, Canada]	accepted. The purpose of the table is synthesis of such components from across the assessment by different chapters. These two tables have been merged to make this clearer
19245	94	3	94	4	Table 4.10. Information in table 4.10 should be presented in a way consistent with table 4.9. For example, ice sheets are "irreversible" in table 4.9 and reversible in table 4.10, and no time scale is indicated. [Anne-Marie Treguier, France]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments
19247	94	3	94	4	Table 4.10. why is there no tipping point in sea level rise while there is for the ice sheets? Are not the two related? [Anne-Marie Treguier, France]	accepted. Tables for abrupt and reversible have been merged and contents checked with chapter assessments

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106191	94	7	94	7	The definition of "tipping point' here is cited as Kopp et al., 2016. The landmark publication that largely introduced the term to climate science however clearly is Lenton et al., PNAS, 2008, which includes a well-considered, seminal definition in the supplement (one that does not imply irreversibility and is quite general). [Wolfgang Lucht, Germany]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters
40919	94	7	94	13	The definition for 'tipping point' is currently under discussion, so this text may need to be updated for FGD. [TSU WGI, France]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters
9827	94	7	94	20	This is not, in fact, the definition advocated by Kopp et al 2016 -- we argue that this definition leads to confusion among the general public: "We accordingly rec- ommend that the term tipping point be reserved for Gladwellian critical thresholds, which we define as the critical thresholds exhibited by tipping elements with no significant lag between commitment and realization, and recommend that the generic term critical threshold be used more broadly. " [Robert Kopp, United States of America]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters
35861	94	7	94	30	A much wider look at the literature assessments of tipping points would be helpful here. Some references to tipping points in dynamical systems and hysteresis would build some attention for climate challenges (e.g. https://doi.org/10.1016/j.ymssp.2016.08.025) [Baylor Fox-Kemper, United States of America]	taken into account. literature coverage now points to focus chapters/sections
19241	94	7	94	30	A reference to chapter 1 (section 1.4.5) should be added. The definition in chapter 1 is worded differntly, and references are made to a different litterature. [Anne-Marie Treguier, France]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters
14793	94	7	94	30	Suggest differentiating and clarifying distinction between 'abrupt' and 'irreversible'/'tipping point'. For example, long term ice sheet changes may be more the latter than the former - slow to manifest, but largely irreversible (i.e. via presence of a 'tipping piont'). More specifically, title is called 'abrupt climate change' but the content actually seems to largely discuss irreversible tipping points. [Jeremy Fyke, Canada]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These have beenchecked also with WG2 and WG3. Glossary has beenupdated and text harmonised across chapters
28239	94	7		11	It is not so clear from the text how a tipping point is different from irreversibility. "does not return to its initial state" sounds exactly like irreversibility. In Sect. 1.4.5., a tipping point is defined as a catastrophic bifurcation (although this word is not used there), which is also irreversible. [Sebastian Bathiany, Germany]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28237	94	10	94	11	"The term abrupt refers to changes that occur faster than the rate of change of forcing (Alley et al., 2003)." This definition could be formulated more precisely. The rate of change in the forcing is typically not in the same unit as the rate of change in the system, so the notion that one is "faster" than the other does not make sense. I believe it is more adequate to refer to a "nonlinear response to the forcing" (as is mentioned in Sect. 1.4.5), or a substantial increase in the susceptibility of the system to forcing, or in case of a catastrophic tipping point, a self-propelled transition to a new state. It is the change in the response over time that makes an abrupt change, not the absolute rate of change at one point in time. [Sebastian Bathiany, Germany]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters
51035	94	13	94	15	Section 4.7.3. seems to be missing evidence of what about abrupt changes in climate can inferred from the palaeo record? It would be helpful to add information here about what this line of evidence indicates. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. literature coverage now points to focus chapters/sections
83973	94	17	94	20	After this paragraph It should be inserted a phrase that talks about replacement of fauna after dissolution of calcium carbonate shells of first consumers and damage to the whole marine ecosystems [Marco Tulio Cabral, Brazil]	rejected. This is a WG2 issue
51033	94	17	94	20	If possible, it would be helpful to assign a confidence statement to this sentence highlighting observations of irreversible impacts that may have already occurred, and elevate this to the ES and SPM. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Confidence to be assigned
96507	94	22	94	30	In the SROCC one finds a detailed discussion about AMOC and ice sheet changes and whether tipping points exist for both and whether the changes are reversible. It should be made clear what results have been reported in the SROCC and which findings here are new. [Nicole Wilke, Germany]	accepted - SROCC is now cited
51037	94	23	94	26	suggested edit: '..changes involve AMOC, Greenland and Antarctic ice sheets, permafrost carbon release, methane clathrate liberation, sea ice reduction and changes to hydrological cycles/monsoon circulations. Since the AR5 there has been a recognition that the characteristics abrupt response of tropical and boreal forests are fundamentally..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text has been clarified after coordination with Chapter 9
23309	94	25	94	28	The following paper can also suport the tropical forest dieback: Zeng, Z., et al. (2013). "Committed changes in tropical tree cover under the projected 21st century climate change." Sci. Rep. 3. [Zhenzhong Zeng, China]	accepted. Section points to Chapter 5 for literature background
51039	94	29	94	30	suggested edit: appreciated' > 'identified'? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text has been clarified
116361	94		94		the confidence level associated with each answer in the table needs to be provided (also valid for previous table). Please consider which findings need to be highlighted in the chapter ES and communicated in the TS-SPM (so far, very little related information). [Valerie Masson-Delmotte, France]	accepted. Confidence statements added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83969	94				Please Insert . Coastal bay acidification with decrease in diversity (Eichler et al., 2014). Eichler, P.P.B., et al. (2014) Evaluation of Environmental and Ecological Effects Due to the Accident in an Oil Pipe from Petrobras in Guanabara Bay, RJ, Brazil. Open Journal of Marine Science, 4, 298-315. htt [Marco Tulio Cabral, Brazil]	rejected. Oil spill is not a WG1 climate change issue
83971	94				Please insert Inlet Acidification with faunal replacement (Eichler et al., 2018) Eichler, P.P., McGann, M., Rodrigues, A.R., Mendonca, A., Amorim, A., Bonetti, C., de Farias, C.C., e Sousa, S.H.M., Vital, H. and Gomes, M.P., 2018. The occurrence of the invasive foraminifera Trochammina hadai Uchio in Flamengo Inlet, Ubatuba, São Paulo State, Brazil. Micropaleontology, 64(5-6), pp.391-402. [Marco Tulio Cabral, Brazil]	rejected. This is a WG2 issue
28235	94				Chapter 4.7.3 deals with the same content as Chapter 1.4.5 It might save space and improve the structure to combine them. I am wondering why the topic of abrupt change / tipping points is scattered across several chapters (1, 4, 5, 8). There is some redundancy and also some contradiction in how terms like "abrupt change"or "tipping point" are defined. [Sebastian Bathiany, Germany]	taken into account. Content is coordinated across cross-chapters. Ch.1 provide background, and Ch.4 assemble synthesis of assessments throughout the report
14795	95	1	95	1	Table 4.10: based on the definition of 'tipping point' on page 94 ("…does not return to the initial state even if the drivers of the change are abated…"), how are 'tipping point' and 'irreversible' different? Suggest making text consistent with Table (or vice versa). [Jeremy Fyke, Canada]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These have beenchecked also with WG2 and WG3. Glossary has beenupdated and text harmonised across chapters
35095	95	1	95	5	Some subtlety here on whether sea ice has a tipping point? Chp 9 assesses based on processes that there is no tipping point for Arctic sea ice, and uncertain process representation in Antarctic sea ice precluding a tipping point assessment. So, while the correct locations are in the table, I think the "yes" in the tipping point columns is not appropriate, and a "no" and "maybe" would be better. On the other hand, is there a warming level beyond which there is no more summer Arctic ice? Definitely, but it is not a threshold or feedback-based process it is just a level where the warming is enough (very abstractly, I guess the state change of ice to water is technically a threshold, but you know what I mean--no earth system threshold feedback not a meterials science threshold). [Baylor Fox-Kemper, United States of America]	accepted. Definitions were harmonised across chapters via breakout discussions.
4157	95	1	96	2	Why is table line on monsoons not cross-linked to one of the earlier monsoon sections, e.g. 4.5.1.5? (Although the poleward aspect was not described there.). Can it be linked to the overall increases in GMP? Does that spread poleward rather than longitudinally? Or linked to the expansion of the Hadley cells (e.g. p58 line 27?) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text and table were made consistent across chapter
9829	95	1	96	2	Assessment basis for this table is unclear. [Robert Kopp, United States of America]	rejected. assessment is synthesis of chapters/sections referred to
9831	95	1	96	2	Per Kopp et al 2016, it would be helpful to indicate lag between commitment to state shift and realization of state shift [Robert Kopp, United States of America]	accepted. Definitions were harmonised across chapters via breakout discussions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9833	95	1	96	2	I'm a bit confused by the definition of 'tipping point' you're using -- the text suggests you are using 'tipping point' as synonymous with 'criticla threshold' in the sense recommended by Kopp et al 2016, in which case ice sheets do exhibit critical thresholds [Robert Kopp, United States of America]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters
9835	95	1	96	2	If ice sheets exhibit a tipping point (which they should under the definition I thought you were using) so too should sea level [Robert Kopp, United States of America]	taken into account. Text and table were updated to include relevant assessments from other chapters
104683	95	1	96	2	The indication of a tipping point for Arctic Sea ice appears to contradict p. 9-44 lines 14-15, which state "There is high confidence that no “tipping point” or critical threshold exists in global mean temperature beyond which the loss of summer sea ice becomes self-accelerating and irreversible." In addition, there is no support for the indicated Antarctic Sea Ice tipping point in section 9.3.2. [William Merryfield, Canada]	taken into account. Text and table were updated to include relevant assessments from other chapters
103043	95	3	95	5	Table 4.10: for the Ocean carbon Sink, the chapter/section 5.4.8.4. is given. However, 5.4.8.4. corresponds to Ocean acidification and de-oxygenation. Other sections seem to be more relevant such as for example: 5.4.10. Near-term prediction of ocean and land carbon sinks; 5.2.2.4. Ocean and inland emissions and sinks; 5.4.4. Climate effects on ocean carbon uptake; 5.4.4.2. Biological drivers of ocean carbon uptake. [Philippe Tulkens, Belgium]	taken into account. Text and table have been updated to include relevant assessments from other chapters
103045	95	3	96	5	Table 4.10: for AMOC, under irreversible it is said 'no'. However, Table 4.9 on page 93 states that the AMOC is irreversible under extreme Greenland ice melt. So, it should be changed to 'yes'. [Philippe Tulkens, Belgium]	taken into account. Text and table have been updated to include relevant assessments from other chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88175	95	3			Table 4.10 - Permafrost Ice row- Are you refering specifically to the ground ice within permafrost or permafrost extent in general in this row. Permafrost is a thermal condition (i.e. ground with temperature <0C) and although it usually contains moisture in the form ice, that is not always the case. If you are referring to the ground ice specifically, it could be argued that loss of ice is irreversible with respect to the amount of ice. Some massive ice for example is buried glacial ice that formed during the last glaciation and its loss would not be reversible on shorter time scale if permafrost does reform -- i.e. ice contents would not return to the same amounts. Also as mentioned in earlier comments, permafrost is not in equilibrium with the current climate and in the discontinuous zone formed under colder conditions and is preserved today due to insulation provided by the peat -- we refer to this as ecosystem protected permafrost. The lag time therefore may be quite long and could be century to millenia scale. Also, various other changes may accompany permafrost thaw including changes in drainage and pond formation and also changes in vegetation which will also determine the rate at which permafrost may form. It would also make more sense to refer to permafrost extent shrinking rather than just "near-surface permafrost" as this would include both the lateral and vertical extent. Although the models used only consider the upper 3 m of the ground the overall conclusion is that there will be less permafrost. [Sharon Smith, Canada]	taken into account. Text and table were updated to include relevant assessments from other chapters
15951	95	5	95	5	Comments on the table: 1. It is stated that the "Possible CH4 release from clathrates" is extremely unlikely for large scale releases. However, this does not recognize that significant rises in methane are already being observed in the Barrow Alaska measuring station, and elsewhere across the Arctic region, which must surely indicate that this is far more probable than extremely unlikely. Furthermore, the statement in section 5.4.8.3 that supports it does not refer to the most recent observational evidence. 2. The table and the supporting text does not consider interconnecting effects between these tipping points. Thus, one tipping point being passed can cause other to tipping points to also be triggered and visa versa. This means that if multiple tipping points are exceeded, then the change to the climate system as a whole becomes much quicker and more irreversible. 3. It is not clear what qualifies an Earth System Component to be classified as tipping point or not in the second column. For example, the loss of ice sheets is not listed as a tipping point, but it hard to imagine any circumstance where this would not be a tipping point, nor how it would not lead to a cascade of other tipping points. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. Text and table were updated to include relevant assessments from other chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7823	95	5	95	5	Earlier on in section 4.3.2.3 the change in AMOC over the 21st century was given as much less certain (likely not v likley) since some models show an increase. The message should be consistent [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Assessments were made consistent
7825	95	5	95	5	Why is the AMOC listed as not irreversible? This contradicts Table 4.9. This should be 'maybe/possibly' [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Assessments were made consistent
73901	95	5	95	5	Why are both the monsoon and the El Nino Southern Oscillation not classified as a tipping point? They are both recognised tipping elements in Lenton et al. 2008 and Steffen et al. 2018. Other entires in the tipping point column are confusing for instance why are some referred to as a simple 'yes' but the AMOC is 'possible collapse' and forests 'only if climate threshold is crossed'. Is this not the case for all they will only tip if a threshold is crossed? [Paul Ritchie, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text was clarified
71165	95	6			In Table 4.10 it is listed "Permafrost Ice" This terminology does not exist. It is assumed the authors mean ground ice in permafrost [Lukas Arenson, Canada]	accepted. Text was clarified
51041	95	Table 4.10	95	Table 4.10	Please could you specify here why Arctic Sea Ice is a tipping point. It is reversible; is it due to uncovering soil that could potentially liberate, if it thaws, lots of CH4 and CO2? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text and table were made consistent
46599	95		95		Arctic sea ice has no tipping point, in particular not in summer. I suggest to change the respective wording in the first column to "possibly in winter". Note that Drijfhout et al., 2015, only find a tipping point for the Southern Ocean, as far as I remember [Dirk Notz, Germany]	accepted. Definitions were harmonised across chapters via breakout discussions
46601	95		95		I would argue that our understanding of Southern Ocean sea-ice is too limited to assess whether or not there is a tipping-point. I suggest to change the respective wording to "Possibly" in the "Tipping Point?) column for Antarctic sea ice [Dirk Notz, Germany]	accepted. Definitions were harmonised across chapters via breakout discussions
46603	95		95		I find it highly confusing why it says "no tipping point" for ice sheets. Both the loss of the Greenland ice sheet and of the Antarcgtic ice sheet exhibit clear tipping point behaviour in just about any tipping-point definition that I can think of. Sorry if I might have misunderstood what you refer to here. [Dirk Notz, Germany]	accepted. Definitions were harmonised across chapters via breakout discussions
46605	95		95		As ice sheets exhibit tipping-point behaviour, so should sea-level rise [Dirk Notz, Germany]	taken into account. Text and table were updated to include relevant assessments from other chapters
28241	95		96		Table 4.10: not clear what the distinction is between tipping point and irreversibility (see comment above which refers to the text). Suggestion: could one distinguish abrupt change from irreversible abrupt change in the table? In that case, one should distinguish the loss of Arctic summer sea ice from the loss of Arctic winter sea ice. The mechanisms differ between these cases, and so does the abruptness. When accounting for the "masking" effect of the continents, ice area reduces gradually in summer, but not necessarily in winter (at least it is much more susceptible in winter). [Sebastian Bathiany, Germany]	accepted. Cross-chapter discussions and breakout groups have discussed and agreed definitions. These were checked also with WG2 and WG3. Glossary has been updated and text harmonised across chapters

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19249	96	5	96	7	Some indication of time scales would be useful in these sentences. [Anne-Marie Treguier, France]	accepted. Text was clarified
15953	96	5	96	7	This paragraph needs to be qualified, for example: 1. How did the scale and speed of the abrupt changes in CMIP5 compare to the paleoclimate changes? 2. Which of the tipping points in the table above the paragraph are included in CMIP5? 3. Are outcomes showing observed tipping points in CMIP5 included in the assessments of temperature changes for the various SSPs used in this report? [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text was clarified
51043	96	7	96	7	abrupt changes' - as abrupt as those seen in palaeo-records? And is this seen across all CMIP5 models? It would be helpful to include these details here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Text was clarified
44507	96	10	98	45	Ho does "probability" relate to "likelihood" in the context of this section? Note that in the SPM and in other chapters, the terminology "low-likelihood high-impact storylines" is used. Section 4.8 should be linked (also in terms of langauge use) to the concept of "low-likelihood high-impact storylines". [Jana Sillmann, Norway]	Taken into account. Likelihood is used as agreed on the cross-chapter level.
89853	96	10	99	8	I very much welcome the inclusion of this section. I believe it is a valuable addition to the assessment. Perhaps the other panels in Fig 4.44 should show anomalies with respect to panels a and b? This would emphasise the additional warming compared to the multi-model mean. [Rowan Sutton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The additional panels for temperature now show additional warming beyond the multi-model mean
53113	96	10			What about a two part Section entitled "Low-Probability High-Impact Storylines" with 4.8.1 focusing on "Near-term climate change enhanced by internal variability" (what if we add one or two same-direction standard deviation of internal variability on top of the ensemble mean forced response for instance?) and 4.8.2 focusing on "Long-term Low Probability High-Warming Storylines" (the current Section 4.8)? [Hervé Douville, France]	Taken into account. The potential additional role of internal variability is accounted for in the spatial PDFs. Surprises in the near-term are discussed in the near-term section and in FAQ4.1
15955	96	12	96	18	The discussion about the upper and lower bounds, rather than the likely ranges, should also reference that the distribution of risk is likely to be log normal, with the upper tail extending far longer than the lower tail. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	This paragraph was shortened due to space constraints
53115	96	12	96	27	Remove (rather CH1)? [Hervé Douville, France]	This sentence helps to motivate the following section.
12281	96	12	98	45	This dangerously excludes any comment on the corresponding "low-probability low warming" cases. Critics will say that you are fear mongering. [Bryan Weare, United States of America]	Taken into account. The focus on high warming is clearly motivated in the previous section by the higher associated risk. The fact that low-likelihood low warming" have equal probability is stated in the section but the associated storyline is not specifically assessed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51045	96	17	96	18	suggested edit for clarification: 'deep uncertainties about the model representation of aspects of the climate system, such as tipping points and irreversible components of the climate system.' - otherwise could be quoted out of context in an attempt to undermine the credibility of climate models to represent any/all aspects of the climate system. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence is revised according to suggestion in order to avoid misunderstandings
44505	96	22	96	23	Make sure the definition of risk is consistent what is actually written in CH1, it does not contain the word hazard. In SOD it reads "isks to human and natural systems result from the interactions of climatic impact drivers, such as extreme weather events and sea level rise, with exposure to and vulnerability to those drivers." [Jana Sillmann, Norway]	Taken into account. The term risk is updated and consistent with CH1
117259	96	23	96	24	update correct section and box number [Maisa Rojas, Chile]	Accepted, cross-reference updated.
127623	96	30			They are not designed for probabilistic assessments, full stop. [Trigg Talley, United States of America]	Taken into account. Revised statement is now more explicit
51047	96	33	96	33	suggested edit: 'such storylines, informed, for example, by palaeo observations and 95th percentile-plus tails of model output distributions can be used..' [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Suggestions are added but the sentence is split in two to avoid a too complex sentence
15957	96	35	96	36	The statement "Note that by definition the lower bound of the likely model range (see BOX 4.1:) is equally probable," is misleading. The risk profile is more likely to be log normal than not, so while the probabilities of the tails are equal, it is more likely that the temperature will be much higher than much lower of the mean expected temperature rise. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The statement as it stands only refers to the likelihood of the outcome. The fact that the risk is higher is discussed in the previous sections.
11521	96	35	96	37	Switching from singular to plural within the sentence [Gerhard Krinner, France]	Corrected
83097	96				Section 4.8. It is good to see some discussion of High-Warming Storylines. I think it could be even more useful if the storyline was more comprehensive in terms of variables. Could you show changes in sea-ice and global thermosteric sea-level change from the two models that exceed the likely range warming in figure 4.44? My thinking around high-end storylines is based on the Shepherd et al (2018) paper https://link.springer.com/article/10.1007/s10584-018-2317-9. In my view, it doesn't really make sense to assess these scenarios in the usual IPCC language of "very unlikely" etc. The point is that the probabilities are hard to quantify, but at some level the narrative set of events can't be ruled out. In this sense, one could just present a set of climate indices from one or two models that lie outside the assessed ranges to illustrate how stakeholders might use members of CMIP6 ensemble to explore this space - and test vulnerabilities etc - in a physically consistent representation of the climate system (?). I think this is helpful because it turns the higher ECS models in CMIP6 into an opportunity to make more robust risk-based decisions. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Partly taken into account. Low-likelihood storylines are also assessed in CH9 and the material pulled together in the TS. The use of uncertainty language has been revisited. The process of getting to the storyline is informed by the GSAT assessment which uses uncertainty language. The storylines themselves are not interpreted in a probabilistic way.

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51049	97	1	97	1	upper bound of the assessed very likely range' - I thought we're interested in values above this (as we're looking at the high end) rather than the upper bound of very likely. Who decides that this is the right cut off point to explore the possible impact of extremes? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section also assess warming in models beyond the upper bound of the likely range.
87631	97	5	97	5	Remove "(Figure 4.44:, second row)", it is already said in the same sentence. [Valentina Roberta Barletta, Denmark]	Accepted
103047	97	5	97	5	Remove "(Figure 4.44:, second row)", it is already said in the same sentence. [Philippe Tulkens, Belgium]	Accepted
44509	97	6	97	7	consider to rephrase "while a linear scale has been shown…" [Jana Sillmann, Norway]	Taken into account. Sentence is rephrased
96509	97	6	97	8	Reference is made to section 4.2.4., but link to "linear has been shown to provide an appropriate approximation for changes in temperature patterns at lower levels of warming." has not been found in section 4.2.4. Please verify if the referred section should be 4.6.1.(?) [Nicole Wilke, Germany]	Corrected
17025	97	25	97	25	(Tokarska et al., 9999) [Sergio Aquino, Canada]	Corrected
53117	97	54			but changes in seasonal mean precipitation can be even stronger due to enhanced seasonality in many regions (Box 8.2). [Hervé Douville, France]	Taken into account. Sentence is rephrased
115445	98	32	98	32	EW is not BASED but just INSPIRED by naturally occurring processes [SILVIA RIBEIRO, Mexico]	Not applicable, we do not understand the comment and cannot identify what it refers to.
96511	98	54	98	55	Figure 4.45, legend: Value (°C) for "95% of assessed range" is missing. Please include missing value. [Nicole Wilke, Germany]	The value is given in the corresponding section referred to.
96513	99	1			Please verify if sentence is incomplete: […] changes in four individual model simulations with high GSAT warming showing ??" [Nicole Wilke, Germany]	Sentence is corrected
111423	99	6	99	6	Remove the words "increasing or decreasing". [James Renwick, New Zealand]	Accepted
79785	99	13	99	13	I guess this important section would need to be further developed for the FGD: a few suggestions: model performance (or lack of) and model biases, use of model democracy without acconting for model inter-dependence etc … [Laurent Terray, France]	Taken into account. However, the Ch04 approach is to distinguish clearly between where uncertainty is substantial but an assessment is possible, and where no assessment can be performed. Only the latter is covered in Section 4.9.
34889	99	13	99	26	It is good that the SOD acknowledges the limitations of its analysis and recognises that there are the "unknown unknowns". Please see general comment #15. [Jim O'Brien, Ireland]	Noted.
106317	99	13	99	26	It would be more useful to formulate the limitations of the assessment in terms of how they impact or are reflected in the statements or confidence levels of the ES. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. However, the Ch04 approach is to distinguish clearly between where uncertainty is substantial but an assessment is possible, and where no assessment can be performed. Only the latter is covered in Section 4.9.
51051	99	13	99	26	Might be useful to include this whole section in the SPM. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Has been negotiated with other chapters but without success.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107013	99	13			Please find some suggestions for the limitation of the assessment: 1. Model biaises and missing physics similarly to Chap3 knowledge gaps, biases et drift in decadal forecast which has implication for near-term predictions, representations on the internal variability and its interaction with the forced response as estimated by the models. [Christophe CASSOU, France]	Taken into account. However, the Ch04 approach is to distinguish clearly between where uncertainty is substantial but an assessment is possible, and where no assessment can be performed. Only the latter is covered in Section 4.9.
53119	99	13			Could be further expanded. At least one paragraph coud be also devoted to "model response uncertainty" (e.g., summary of Box 4.1 and lack of methods to combine multiple, sometimes contradicting or redundant, observational constraints on projected climate change) and another to internal variability (e.g., limitations of decadal predictions and lack of accurate estimates of the upper limit of predictability at decadal to multi-decadal timescales?) [Hervé Douville, France]	Taken into account. However, the Ch04 approach is to distinguish clearly between where uncertainty is substantial but an assessment is possible, and where no assessment can be performed. Only the latter is covered in Section 4.9.
41435	99	13			Please expand this section and remove bullet points. There is a lot more to cover here in addition to the scenario limitations and unknown unknowns/ deep uncertainty. An informative pointer to limitations of CMIP models and implications, to feedbacks that are not sufficiently understood like clouds etc. [Alexander Nauels, Germany]	Taken into account. However, the Ch04 approach is to distinguish clearly between where uncertainty is substantial but an assessment is possible, and where no assessment can be performed. Only the latter is covered in Section 4.9.
12289	99	15	99	26	This appears rather cryptic and hurried. [Bryan Weare, United States of America]	Taken into account. The scenario paragraph has been expanded.
9837	99	25	99	26	This is not entirely true -- it is possible to make informed judgements about the presence of deep uncertainty, based on altenrative lines of reasoning (for example, see the US National Climate Assessment volume 1's final chapter, where hints of 'unknown unknowns' from paleoclimate/GCM mismatches are discussed) [Robert Kopp, United States of America]	Noted. The text is about unknown unknowns, not deep uncertainty, which is about known unknowns.
41011	100	0	102	0	the summaries of FAQ4.1 and 4.2 are a bit too long and introduce elements not mentioned afterwards. [TSU WGI, France]	Taken into account. Summaries shortened substantially. .
40141	100	0	104	0	FAQ4.1, FAQ4.2 and FAQ4.3 are nice and interesting but can sound very technical in places (e.g. radiative forcing, chaotic processes, transient etc are not terms the general public are familiar with) [TSU WGI, France]	Taken into account. Text has been revised for clarity and simplicity.
41001	100	0			the reason why we're sure the climate trends will continue in the near term should be clearer in the main text [TSU WGI, France]	Taken into account. Text has been revised for clarity and simplicity.
41005	100	0			the structure could be improved/ flow more logical. What do you think of adopting the following structure? 0) summary 1) introduction: delay because inertia of the system + natural variability [cf L3-9]] 2) link between CO2 emission -- CO2 concentration - Temperature change and other climate variables [cf L12-20] 3) context of CO2 emission reduction: Historical CO2 emissions + projections for the 2 scenarios presented [new] 4) effect on CO2 concentration [cf § L48-53] 5) effects on Temperature and other variables (+how to detect them) [new] [TSU WGI, France]	Taken into account. Suggestion for re-structuring has been incorporated. Note that comment applies to FAQ 4.2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41009	100	0			the structure the text is not the clearest and the flow could be more logical [TSU WGI, France]	Taken into account. Text has been revised for clarity and simplicity.
40323	100	0			from the text it's not clear that the results presented are one single model nor how different the results would be with another model [TSU WGI, France]	Taken into account. Text has been revised for clarity and simplicity. Note that comment applies to FAQ 4.2 (?)
40607	100	0			NOTE: FAQ4.2 could be expanded to use covid as an example of time delay. (in addition to a cross-chapter box) [TSU WGI, France]	Taken into account. COVID now mentioned.
40419	100	0			in FAQ4.2, the current status of GHG emissions is somehow lacking. I don't think we can assume the readers know exactly how they have evolved [TSU WGI, France]	Taken into account. Figure now shows emissions.
215	100	1	100	34	I agree that near-term (20 years) climate evolution depends on the interplay between external forcing and natural variability as mentioned in the text. However, the effect of near-term climate forcers as a fundamental part of the forcing is omitted in the FAQ1 text and they may play a predominant role in certain regions (Scanell et al., 2019), even globally (Acosta Navarro, et al., 2017), if their emissions change dramatically as has been the case for some of them in the recet past (e.g. anthropogenic aerosol and precursors changes in Europe/Norther America between 1970s and 2000s due to clean air legislation and economic activity changes). The text only acknowledges the role of well mixed GHGs in shaping near-term climate, but omits near-term climate forcers. Furthermore, additional to the important uncertainty stemming from natural variability, uncertainty in a 10-20 year horizon from emission changes of near-term climate forcers is possibly larger than that of CO2. Literature: 1. Acosta Navarro, J. C., et al. (2017). Future Response of Temperature and Precipitation to Reduced Aerosol Emissions as Compared with Increased Greenhouse Gas Concentrations. J. Clim. 30, 939–954. doi:10.1175/JCLI-D-16-0466.1. 2. Scannell C., et al. (2019) The Influence of Remote Aerosol Forcing from Industrialized Economies on the Future Evolution of East and West African Rainfall. Journal of Climate 32:23, 8335-8354. [Juan Camilo Acosta Navarro, Spain]	Noted. The requirement of simplicity limits possibilities of treating regional effects.
7985	100	1	100	55	One topic not commonly addressed is the short term variability and predictability of weather in a future warmer world. The references in Comment 4 address this issue. [Anthony Lupo, United States of America]	Noted.
127625	100	1	100	55	One topic not commonly addressed is the short-term variability and predictability of weather in a future warmer world. It is something meteorologists are interested in. [Trigg Talley, United States of America]	Noted.
21705	100	3			Unless I missed it this FAQ makes no reference to the potential joker in the pack that is a large scale volcanic eruption. Given that its what we can say about a 20 year time horizon there is a non-negligible chance that this will occur and I would suggest making an appropriate allusion to this in any redraft. [Peter Thorne, Ireland]	Accepted. Volcanoes now mentioned.
38721	100	5	100	6	To make sure all your readers are able to differentiate between aspects that increase and those that decrease, I would address them separately. [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38727	100	7	100	10	This very long sentences justifies confidence in a conclusion with confidence in model simulations. Building confidence on confidence might not sound too convincing, and building it on model calculations might evoke the questions why you trust your models. Even if FAQ 3.2 explains why models have improved, I would phrase this differently, for example not address the aspect of confidence at all, but only insert "model simulations show...". [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.
12287	100	7	100	10	There should be some reference to paleo and recent observations as well as theory and modeling. [Bryan Weare, United States of America]	Rejected. Influence of observations on predictions/projections is too indirect for an FAQ.
38723	100	8	100	8	"Radiative forcing" might not be understood by all your readers. Adding something like "from solar radiation" here would help in case a simpler term cannot be used. [Maike Nicolai, Germany]	Accepted. The term is no longer used.
38725	100	9	100	9	Please clarify what kind of emissions you are referring to. [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.
38729	100	12	100	12	The word "overwhelm" might be connotated differently outside the climate scinece community. Would "mask" or "override" work instead? [Maike Nicolai, Germany]	Accepted. The word is no longer used.
38731	100	15	100	17	The perspective the text seems to take here might appear rather IPCC-internal or science-internal. The target audience of the IPCC FAQs might not feel adressed. I would pick one or two aspects of this "societal need" and use them to catch interest - or simply say something like "in order to limit and adapt to climate change and minimise risks..." I think this FAQ offers a great opportunity to highlight why the next 20 years are crucial, and the text might become more powerful, if the "societal need" could be made more tangible (either here or in the introduction). [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.
38733	100	17	100	17	Who is "we"? Does the "we" really include your target audience, as it pretends to do? [Maike Nicolai, Germany]	Accepted. "We" avoided.
38735	100	19	100	19	Please help your readers understand what you mean by "scenarios" in this context. [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.
38737	100	19	100	23	If the FAQs should be suitable for non-expert audiences and educational purposes, I would argue these sentences expect too much prior knowledge. The word "chaotic" might have a different connotation among lay people, and they might not be used to read it in the context of processes that, as far as they might have learned, follow the "laws of nature". "larger-scale patterns", "oscillations", "radiative forcing", "time averages or trends" also sound very technical and difficult to decipher. [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.
51053	100	19	100	23	What about changes in aerosols (e.g. from fuel switching and AQ improvements?) I think the role of aerosol reductions in exacerbating near-term warming needs to be given some space here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The human influence is taken up more explicitly in FAQ 4.2; FAQ 4.1 has been revised to stress more clearly the influence of internal variability. Note that recent research attributes small influences to the effects mentioned here, in the near term and for global temperature.
39863	100	22		27	"there has been … other than temperature" I am not convinced this paragraph adds much to the story of this FAQ. Maybe it could be merged with the paragraph on "diagnosing the time of detection" (L40-46) [TSU WGI, France]	Taken into account. Suggestion for re-structuring has been incorporated. Note that comment applies to FAQ 4.2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6685	100	23	100	23	It is rather "over the top" write that "time averages or trends calculated over twenty years contain a substantial chaotic element" in the case of at least global temperature. What is meant by the word "substantial" in this context? What about averages over the 30-year periods chosen by WMO as climatological reference periods? How does the statement sit when compared with the words "20 years is considered necessary to be representative of the current state and to average over natural variations of multiple climate variables" written on page 48 of Chapter 1? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been revised for clarity and simplicity.
38739	100	26	100	30	"near-term" was defined as "the next twenty years" in line 15/16. In line 26, it is used in contrast to "long time scales such as years to decades". Even if the terms are used in different contexts, this might cause some confusion. Is it possible to use "long term" and "near term" for similar periods of time, independant of the context? Otherwise, the different definitions might have to be clarified in order to avoid confusion. [Maike Nicolai, Germany]	Accepted. Terms are now used consistently.
38741	100	32	100	34	Is the influence of natural variability too big, so that it is actually really difficult to say how the climate will change over the next 20 years? How does this match with the confidence that was reflected in the introduction? What is the overall message of this FAQ? The current version makes me wonder about so many new questions that I am afraid I might suggest to drop it because it does not appear useful enough to keep it. [Maike Nicolai, Germany]	Accepted. Overall message now clarified.
39641	100	40		46	"diagnosing the time of detection…displayed here" this paragraph might actually bring more confusion to a lay audience: the general public is not very familiar with uncertainty. [TSU WGI, France]	Taken into account. Text has been revised for clarity and simplicity.
38743	100	45	100	47	I am not sure if this would be a new insight for non-specialist readers, but the fact that the various greenhouse gas emissions scenarios do not show many differences in the next twenty years might be worth highlighting - as well as the fact that choices made within these twenty years are crucial for the development in the more distant future (beyond 20 years). Perhaps information from this paragraph can be integrated in FAQ 4.2. and the previous paragraphs of FAQ 4.1 be omitted? [Maike Nicolai, Germany]	Taken into account. The human influence is taken up more explicitly in FAQ 4.2; FAQ 4.1 has been revised to stress more clearly the influence of internal variability.
79787	100	49	100	49	I suggest to carefully check full consistency with the revised estimates from Chapter 2 [Laurent Terray, France]	Taken into account. Sentence has been has been dropped.
6687	100	49	100	51	This sentence is hard to reconcile with FAQ 4.1 Figure 1, as the pre-industrial temperature level (however defined) is not marked on the figure. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence has been dropped.
39819	100	49		51	"since …next twenty years" does this sentence add much to rest of the text? I know it's an important point but 1) I fear it will be confusing with the warming figure of 1.1C in FAQ1.4 and 2) It's maybe best to end on something more relevant for the FAQ itself? [TSU WGI, France]	Accepted. Sentence dropped.
15497	100		104		Suggest adding one FAQ on the comparison of AR5 and AR6 projections. [SAI MING LEE, China]	Rejected. This is covered in the chapter but would be too technical for an FAQ.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83099	100				FAQ 4.1 has a similar thread to FAQ9.2, which deals with what we can expect for global/regional sea-level change in the coming decades. It would be good to cross-reference and perhaps think about consistency of concepts/presentation? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The requirement of self-containedness and simplicity limits possibilities of cross-referencing.
117257	102	1	102	10	Very important FAQ. I am bit confused on the "20-30yrs" lag to see a temperature response to GHG reductions, doesn't seem consistent with section 4.7.2.2 and fig 4.43 I see that ZEC is longer. But the demostration in the this FAQ uses a different approach to calculate the response. Please clarify. [Maisa Rojas, Chile]	Taken into account. Text has been revised for clarity and simplicity. 4.7.2.2 addresses different questions, not those of detectability addressed here.
96515	102	1	102	53	FAQ 4.2 „How Quickly Would We See the Effects of Reducing Greenhouse Gas Emissions?“ is very well chosen; it seems to be important to give politicians this information in hand. Thus, the description of scenarios should be easier to understand. However, we have some concern about the content, please see our comment on SPM D4. [Nicole Wilke, Germany]	Taken into account. Text has been revised for clarity and simplicity.
38745	102	3	100	53	This is a very good FAQ! I think this would be the level of complexity and the language the FAQs should aim for. Perhaps key details from FAQ 4.1 could be (slighliy simplified and) inserted here and FAQ 4.1 be dropped? [Maike Nicolai, Germany]	Noted, thank you! FAQ 4.1 has been re-designed somewhat.
19369	102	3	102	53	This may be a suggestion for an additional FAQ or perhaps it’s a suggestion for inclusion in response to FAQ 4.2. I think it would be helpful to discuss the difference between annual GHG emissions and cumulative GHG emissions, explain that cumulative emissions are ultimately what drives climate change, but also explain the relationship of annual emissions to cumulative emissions. I find this is helpful in describing why achieving net zero emissions is so important. Until we do, cumulative emissions continue to rise, even where we decrease annual emissions (albeit at a slower rate). These concepts are critical for policymakers and everyday people to understand, and they are related to the question here about the impact of emissions and the latency/timing of the impact. [Lia Cairone, United States of America]	Accepted. Text and figure now explicitly differentiate between emissions and concentrations.
51055	102	7	102	8	emissions reductions are expected to leave a discernible fingerprint on atmospheric CO2 concentrations after about ten years' - surely this depends on how radical the emissions reductions are? A decrease of 0.1 MtCO2 is an emissions reduction and I suspect you wouldn't be able to discern it at all, let along after about 10 years. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Formulation clarified.
12291	102	13	102	14	net emissions approach zero. [Bryan Weare, United States of America]	Accepted. Formulation adopted.
38747	102	15	100	15	"Radiative forcing" might not be understood by all your readers (espacially non-native lay people). [Maike Nicolai, Germany]	Accepted. The term is no longer used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38749	102	18	100	18	"Chaotic processes" might have a different connotation outside the scientific community. "Ever-changing weather" is a good first explanation, but people might still wonder what kind of (other) processes you refer to. Would it be possible to replace or further illustrate the term with something like "dynamic" or "difficult to predict"? Or simply omit the word "chaotic"? [Maike Nicolai, Germany]	Accepted. The term is no longer used.
38751	102	32	100	38	People with little prior knowledge might perceive this as quite a deep dive into climate simulations. It might be hard to understand that uncertainty from natural internal variability is represented by the different initial states and differences between simulations are only caused by natural internal variability. Can the difference between these two aspects be explained more clearly? [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.
6689	102	36	102	38	The climate history that will actually unfold will also depend on natural external variability. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Each realisation experiences the same natural external forcing.
38753	102	44	100	46	Does the "smaller differences in the responses to different scenarios" refer to a potential different model that is not shown here and that is less sensitive (compared to line 41)? I would either phrase this even more clearly (simply highlighting this is only one model and there might be different pathways) or omit this sentence. [Maike Nicolai, Germany]	Taken into account. Text has been revised for clarity and simplicity.
38755	102	52	100	53	This is truly a communication challenge - but I would not adress it as such in an FAQ. On the contrary: This FAQ can help tackle this challenge. [Maike Nicolai, Germany]	Accepted. Reference to challenge deleted.
51057	102	52	102	53	Suggest addition: In summary, the benefits of mitigation are clearly discernible in crucial climate variables such as global temperature only after a delay of a few decades – a delay that might cause a substantial communication challenge' - and/or underlines the merits of earlier action to being forward the tangible societal benefits compared with a higher emissions future. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Connection to longer-term perspective now included.
11095	103	1	141	18	There are quite a lot 9999, such as Maher et al., 9999a, 9999b; Deser et al., 9999; Lehner et al., 9999; SIMIP Community (9999), Smith et al., 9999, and so on. Do they mean submitted paper or paper in publication? [Wen Wang, China]	Noted. All 9999 references were placeholders for papers submitted but not yet published. To be included in the FGD, those publication should have been accepted by the cut-off deadline (in which case they have been updated). If not, the references have been removed.
40985	104	0			the main patterns of temperature changes are missing in main text and it's a pity we don't have any explanation about the reason why observe those patterns. [TSU WGI, France]	Taken into account. Text has been revised to explain the patterns shown, instead of the method.
40143	104	0			FAQ4.3: at the moment I feel there is a mismatch between the title of the FAQ, the figure and the content of the text. I would suggest to restructure t to tackle this issue [TSU WGI, France]	Taken into account. Text has been revised to explain the patterns shown, instead of the method.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38757	104	1	100	50	The word "pattern" is used in different ways in this FAQ.In order to avoid confusion among people who are not familiar with the technical terms, it would be helpful to distinguish them clearly and explain what kind of "pattern" is meant: If I am not mistaken, there is a "spatial pattern" that could be read as a reference to the spatial distribution of certain phenomena. But there also seems to be a "pattern of change" that could describe modes or characteristics of changes. There might even be a mix of both, and it would be helpful to phrase carefully and explain. [Maike Nicolai, Germany]	Taken into account. Text has been revised to explain the patterns shown, instead of the method.
38759	104	1	100	50	I wonder if the text really answers the FAQ question. The way I read it, it explains why it is useful to look at patterns - but what are they, for a given level of warming, and where? [Maike Nicolai, Germany]	Taken into account. Text has been revised to explain the patterns shown, instead of the method.
90831	104	37			All of the AR4 predictions for precipitation, snow cover, and sea and land ice are less certain and more variable across the suite of AOGCMs than they are for both the global average nad the more robust geopgrahical patterns of temperature [Vivien How, Malaysia]	Noted. No action item discernible.
55507	105		141		There are near 35 references included in the TEXT as "submitted". But, in the text, some of theses references are included considering the year of publication. It´s necesary to check and correct in both, text and references. [Maria del Pilar Bueno Rubial, Argentina]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
93413	114	54	114	54	The first author of this ref is Ferrer González, and not just González [Carles Pelejero, Spain]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
10043	118	12	118	15	Duplicate referance [Andrew MacDougall, Canada]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
93417	118	12	118	19	Jones et al 2019a and b should be the same published reference [Carles Pelejero, Spain]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
11257	118	48	118	48	The citation should be Chang, E.K.M. (2018) instead of Kar-Man Chang, E. (2018) [Edmund Kar-Man Chang, United States of America]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
11445	120	40	120	45	Note repeated reference. [Douglas MacMartin, United States of America]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
11097	124	55	124	55	What's the meaning of "Science (80-.)" [Wen Wang, China]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
70907	126	35	126	37	Mindlin et al. is now published (2020): doi: 10.1007/s00382-020-05234-1 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
11099	128	53	128	55	The journal name is missing [Wen Wang, China]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
11101	128	58	128	61	Olson et al. 2019a and Olson et al. 2019b are the same. There are quite a lot similar mistakes, such as, Smith et al. 2019b and Smith et al. 2019 ; Yeager et al. 2018a and Yeager et al. 2018b. [Wen Wang, China]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
104613	135	7	135	10	Smith et al. (2019d) and Smith et al. (9999) which are the same paper has been accepted by Nature with a modified title: Smith, D. M., Scaife, A. A., Eade, R., Athanasiadis, P., Bellucci, A., Bethke, I., et al. (2020). North Atlantic climate far more predictable than models imply. Nature, accepted. [William Merryfield, Canada]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93409	135	28	135	29	The reference Sonntag et al has a wrong DOI. It should be: Sonntag, S., Ferrer González, M., Ilyina, T., Kracher, D., Nabel, J. E. M. S., Niemeier, U., Pongratz, J., Reick, C. H., & Schmidt, H. (2018). Quantifying and Comparing Effects of Climate Engineering Methods on the Earth System. Earth's Future, 6(2), 149–168. https://doi.org/10.1002/2017EF000620 [Carles Pelejero, Spain]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
127627	135	46	135	48	Stjern et al. 2018a and 2018b are the same reference. [Trigg Talley, United States of America]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
10251	137	61	138	3	Duplicate van Vuuren et al (2011a/b) references. [Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
4159	138	40	138	43	The Wang et al. submitted BAMS reference has been listed twice. Note that it is now published in early view and available online at: https://doi.org/10.1175/BAMS-D-19-0335.1 [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This kind of issues will be fixed during the production phase (if not sooner).
115189	142	2	142	15	It should be made clear that the uncertainties shown here are in the temperature pathway associated with a specific CO2 concentration scenario, which does not reflect the full uncertainty in the reponse to a particular emissions scenario because the experimental design does not include emission-driven ESM projections. If carbon cycle uncertainties were taken into account, an emissions scenario could result in a wider range of outcomes than when a single concentration pathway is used. [Richard Betts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The revised Section 4.3.1 makes the comparison between concentration-driven and emissions-driven simulations, which is much smaller than the uncertainty reported here. All uncertainties are conditioned on scenario anyhow. No change here.
19235	142	4	142	15	Box 4.1 figure 1: is there another way to show the range of internal variability (blue)? As shown, is gives the reader the impression that the red lines (best estimates) are somehow "outside" a range of something. [Anne-Marie Treguier, France]	Noted. There is no unambiguous way to provide all information. No change.
51061	142	8	142	8	Suggested addition: 'following scenario SSP2-4.5, which is considered to be plausible this century, with no further mitigation commitments beyond those within the Paris Agreement, at the time of publication' or similar [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is not the place to enter this immensely difficult discussion.
55501	142	9			Morice et al Chek Submitted. (included in TEXT).Check publication date [Maria del Pilar Bueno Rubial, Argentina]	Taken into account. Reference updated.
105569	142	19	154	19	The ref Lisieki et al 2008 is not the same than the one in the figure liesieki and raymo 2004 [Maxime Debret, France]	Taken into account. All references have been updated where required.
51059	142	Box 4.1 Fig	142	Box 4.1 Fig	Box 4.1 Fig 1: It would be clearer here to disaggregate the beige lines into AOGCMs and ESMs, to show the difference in the values between them. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. That distinction is of no import here.
116363	142		142		Explain why all the chapter reports anomalies against 1995-2014. [Valerie Masson-Delmotte, France]	Noted. Comment unclear; right y-axis shows change relative to 1850--1900. No change.
96517	143	1	144	1	Figure 4.1: Seasonal (summer, winter) precipitation change would be a more relevant indicator than annual precipitation. This could also be added to Figure 4.2 [Nicole Wilke, Germany]	Rejected. Not enough space in the Chapter..

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51093	143	1	190	9	Please clarify in the figure legends that the future projections are from models driven by GHG concentrations not emissions - this is important information for understanding whether the projections capture the contribution of uncertainties in climate-carbon cycle feedbacks to the overall uncertainties in the response of the climate system to emissions scenarios. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Good point.
11283	143		145		Why don't you show the observed time series in Figs. 4.1-4.3? It'd be helpful for readers to see that models can reproduce the observations during the historical period. [Masahiro Watanabe, Japan]	Taken in account. 1) Earlier on, the decision was made by the chapter team to not include observations in this figure, or in any of the other figures of the chapter. 2) SSP1-1.9 is now shown. 3) After much discussion between the chapter, the decision was made to highlight with shading SSP1-2.6 (as a low emission scenario) and SSP3-7.0 (as a high emissions scenario). SSP5-8.5 has been deemed highly unlikely, and SSP1-1.9 has too few simulations to obtain robust uncertainties.
21709	144	1	144	1	Can NH and NA not be spelt out in full by making titles two lines each? This would make it easier to use these figures as standalone items. [Peter Thorne, Ireland]	Accepted. Good point.
37899	145	0	145	0	The color of the line corresponding to RCP8.5 (2.0) is not clear. Please indicate in different color. [Junhee Lee, Republic of Korea]	Accepted.
21711	145	1	145	1	Adding "ice-free Arctic" above the dashed line in the lower left would make clearer what the line signifies and thus increase figure accesability. Y-axis should have 'sea-ice extent' as well as the units? [Peter Thorne, Ireland]	Accepted. 1) suggested text added. 2) adding to the y-label is redundant given the title of the plot.
37897	145	5	145	6	The description of the figure caption is unclear. Please make it clear like this. "The black and red curves are the average over twenty simulations following historical forcing to 2015 and RCP8.5 extentions to 2100, respectively." [Junhee Lee, Republic of Korea]	Accepted.
51063	145	6	145	6	It is unclear here how the RCP8.5 scenario stabilises to 1.5, 2 and 3 degrees this century - do these simulations assume a very low climate sensitivity? Please explain. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
37901	146	0	146	0	I hope to check the result of Lye et al., (2020). In this study, the 37 CMIP5 and 39 CMIP6 annual mean AMOC change in historical and scenario simulations; A clear tendency to weaken AMO was confirmed from 2081-2100 (long-term). - Regional dynamic sea level simulated in CMIP5 and CMIP6 models: mean biases, future projections, and their linkages [Junhee Lee, Republic of Korea]	Taken into account. The Chapter 4 assessment follows the one by Chapter 9, including updates in the literature.
69913	146	1	146	2	Figure 4.4: All the SSP's scenario ensemble represents the similar variation in weakening in AMOC. A huge uncertainty is seen in predicting AMOC in CMIP6 simulations. However, if the number of models used for the AMOC calculation are increase that might moderate the uncertainty and show much more vibrant depiction for individual SSP scenario. [SAHIL SHARMA, India]	Taken into account. With more models the scenario independence remains apparent.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87883	147	3	147	3	Figure 4.5: Please note that this figure presents the same information as figure 5.25 in Chapter 5. Also, please note that the number of simulations used in this figure for each scenario should be the same as in Fig. 4.15, as they show the same type of information -currently, these two figures use different numbers of models/simulations. [Katarzyna Tokarska, Switzerland]	Rejected. It is our strongly held chapter view that since surface carbon flux is a key indicator of global climate change, that this figure should remain. The two figures show somewhat different information and are complementary. While somewhat different model sets are used, the qualitative conclusions are the same.
51065	147	Fig 4.5	147	Fig 4.5	The scenarios displayed here don't all show an increase in net carbon uptake by the land and ocean over this century, as stated in the Executive Summary. Is the ES statement not based on CMIP6 results? Please clarify this across the chapter. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
21715	148	1	148	1	y-axis could be pH (uniteless). Title could allude to global average as otherwise the location / extent is not explicit? [Peter Thorne, Ireland]	Noted. 1) The addition of "pH" would be redundant with the title. 2) the title has been changed as suggested.
21717	149	1	149	1	Could arrows to the RHS of each panel be added denoting strengthening / weakening because the use of hPa isn't immediately intuitive to a reader what the practical implication is. Adding labelled arrows would remove any potential ambiguity and increase accessability? [Peter Thorne, Ireland]	Rejected. Arrows would redundant with the axes.
21719	150	1	150	1	Should y-axis label be Nino 3.4 SST Standard deviation to avoid ambiguity? [Peter Thorne, Ireland]	Rejected. This detailed information appears in the caption,
11285	150		150		I think the plot of sigma calculate in each year is misleading because the apparent interannual variability in sigma will simply reflect the sampling error but not anything physically meaningful. I suggest showing the time series in which sigma is calculated using a moving window with a fixed segment (e.g., 10 years) and argue in the text the dependence of the assessment on the chosen length of the segment. [Masahiro Watanabe, Japan]	Rejected. It is false that the variance across realizations reflects only sampling variability and is therefore is meaningless. Because of random temporal phasing from one realization to the next this approach will indeed capture changes in the amplitude of year-to-year variability.
21721	151	1	151	1	Would there be any value in adding a right hane y-axis offset by 0.86 to denote change since 1850-1900 ? Should the (d) be assessed GSAT changes from 1995-2014? [Peter Thorne, Ireland]	Accepted and implemented, thank you.
51067	151	Fig 4.9	151	Fig 4.9	For each of these charts please could the relative temp c.f. 1995-2014 be shown on the RH y axis and c.f. pre-industrial on the LH y-axis, as the latter is of greater relevance to global climate policy. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. An additional y-axis labelling has been added to the rhs. This choice was made because the primary change is relative to 1995--2014; change relative to 1850--1900 involves substantial additional uncertainty considerations
21723	152	1	152	1	A title for the figure would likely be helpful? [Peter Thorne, Ireland]	Not applicable. Figure 4.10 in the SOD is removed in the FGD.
12211	152	1	152	15	This critical figure is very had to relate to the text. Why are there no plots showing outcomes from only "natural" forcing to be compared against as in AR5? The "internal variability" shading is no substitute. The ranges of the emulator are largely lost in the swam of individual models. Horizontal lines at the 1.5 and 2.0 thresholds need to be added, especially when the appropriate labeling is on the right. Where are the model means? Much more needs to be done to make this important figure easily interpretable. [Bryan Weare, United States of America]	Not applicable. Figure 4.10 in the SOD was removed in the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55503	152	8			Morice et al Chek Submitted. (included in TEXT).Check publication date [Maria del Pilar Bueno Rubial, Argentina]	Not applicable. Figure 4.10 in the SOD is removed in the FGD. However, Morice et al. (2021) is updated which is also referred in Box 4.1 Figure 1.
12217	153	1	153	9	This and ALL of the similar maps are EXTREMELY hard to interpret. The stippling/hatching draws the eye AWAY from the important feature by drawing it to the hatching, the least important regions. Except at large magnification it is very difficult to see which is which, especially at higher latititudes. The stippling/hatching often obscur the color range of a region, for instance the N. Atlantic in all four panels. I strongly recommend a clearer, simpler way to show signficance. Perhaps stipple or hatch those regions that do NOT meet the 2sd,90% test. This would leave the important regions with unblemished shading. This flaw detracts considerably from this whole chapter and must be corrected. [Bryan Weare, United States of America]	Taken into account. The new stippling/hatching method was applied all map figures for future change. Thus, all map figures were considerably improved. You can find the detail information on displaying robustness and uncertainty in maps across the WGI report from Cross-Chapter Box Atlas.1.
105567	154	2	154	2	On the figure it is written "Lisiecki and Raymon 2004" but it is mispeleted it should be "Lisiecki and Raymo 2004" [Maxime Debret, France]	Not applicable. Lisiecki and Raymo (2004) is not used in the figure.
3705	154	3	154	3	If you don't want to show all 4 seasons, please at least show MAMSON, the combined tropical rainy seasons. These seasons are biased towards the traditional focus of climate and weather sciences, i.e. the mid-latitudes. It undoubtedly reinforces the under-representation of tropical nations and information in the climate change narrative. I know you won't want to make such a signficiant change this time, but I encourage the authors to seriously consider this for future IPCC reports if they involved again. [Declan Finney, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Your point is well taken but it is already considered in Chapter 8 on water cycle change. Please refer Figure 8.14 in chapter 8 which shows changes in seasonal precipitation by the end of 21st century including MAM and SON.
21725	155	1	155	1	Spell out NHSM in full in the panel b title for accessability? [Peter Thorne, Ireland]	Rejected. It is too long to be spelled out in the panel title. Figure caption provides the full name.
69915	155	1	155	11	Figure 4.13: The grey shading for Global Monsoon precipitation Index and NHSM Circulation Index for the historical simulation has no description in the text as well in in figure caption. Additionally, the shading around the different scenario from 5 to 95 % ensemble (as seen in figure 7 and other plots as well) is absent. [SAHIL SHARMA, India]	Taken into account. The figure and figure caption are revised accordingly. The historical change is added in the figure to provide uncertainty range for past change.
37903	156	0	156	0	The legend of the figure color is missing. Please make it clear about five SSPs. [Junhee Lee, Republic of Korea]	Accepted. A legend has been added.
87633	156	1	156	1	Fig. 4.14: There is no name of the SSP models. [Valentina Roberta Barletta, Denmark]	Accepted. A legend has been added.
103049	156	1	156	1	Fig. 4.14: There is no name of the SSP models. [Philippe Tulkens, Belgium]	Accepted. A legend has been added.
21727	156	1	156	1	An earlier sea-ice figure had used extent. Its probably worth sticking with one of SIE or SIA consistently. Ideally it would be consistent across 2-3-4 and 9. [Peter Thorne, Ireland]	Noted. This chapter consistently assesses SIA with the one exception of a figure adapted from the literature over which we have no control
12213	156	1	156	10	I see little value to including the trends over the 10,20,30 year periods ending at the same time. The results are essentially the same, except for the expected lower variabilty for the longer period. Why not include just say the 20-year period for 20 years ending in 2040, 2060, 2080, 2100? [Bryan Weare, United States of America]	Rejected. This section concerns the detection of significant trends in the near term, specifically over 10-, 20-, and 30-year periods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104647	156	8	156	10	Does the term "SSP-average percentage" mean the percentage within each SSP, averaged over the five SSPs, or the % of all the 80 simulations having negative trends, regardless of SSP? (The two will differ when the SSPs include differing numbers of simulations.) If it is the latter, recommend modifying "SSP-average percentage of simulated trends that are negative" to "percentage of simulations among all the SSPs with negative trends". [William Merryfield, Canada]	Accepted. Good point.
51069	156	Fig 4.14	156	Fig 4.14	10, 20 and 30 year rates of melt - no rate increase over time? What about the effect of positive feedback of albedo change? Is this saying the rate is more likely to be negative over time, with the 10 year group bars straying further into positive territory than the 20 and 30 year groups. But there's very little difference between the 20 and 30 year group. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. To answer this question would require a more detailed analysis that would go be beyond the scope of this subsection.
21729	157	1	157	1	Is there no way that the meaning of the %age numbers can't be explained by some use of in panel lanbelling so that the figures can better stand alone without reference to the caption? These could be very useful in outreach but the fact that the numbers given have no context is a major impediment to doing so. This applies to several other similar subsequent figures. [Peter Thorne, Ireland]	Rejected. We feel that these numbers are adequately explained in the caption.
12215	157	1	157	8	As in Fig. 4.14 I see little value in the trends for different periods ending at the same date. There is very low information content here, especially compared to many previous figures. [Bryan Weare, United States of America]	Rejected. This section concerns the detection of significant trends in the near term, specifically over the 10-, 20-, and 30-years.
87885	157	4	157	4	It looks that it is the uptake rate/flux. Please clarify. It would be good to keep this figure consistent with Fig. 4.5 that shows the time-series of these fluxes. Also, it would be good to keep the notation consistent with Figure 5.25 in Chapter 5. [Katarzyna Tokarska, Switzerland]	Accepted. We have changed the wording from "uptake" to "flux".
87887	157	6	157	6	Please note that the number of model simulations differs from those on Fig.4.5. Since both show the same type of information, the same set of models should be used in both figures. [Katarzyna Tokarska, Switzerland]	Rejected. Because 10-, 20-, and 30-year trends ending in 2021-2040 may require an SSP *and* historical simulation to be computed, the number of models used in these two figure are not necessarily identical.
104641	157	7	157	8	Does the term "SSP-average percentage" mean the percentage within each SSP, averaged over the five SSPs, or the % of all the 44 simulations having positive trends, regardless of SSP? (The two will differ when the SSPs include differing numbers of simulations.) If it is the latter, recommend modifying "SSP-average percentage of simulated trends that are positive" to "percentage of simulations among all the SSPs with positive trends". [William Merryfield, Canada]	Accepted. Good point.
21731	158	1	158	1	Per earlier comment on figure 4.15 if what the %ages represent could be incorporated in the figure would aid accessability. Per earlier modes figure adding arrows on RHS of each panel to denote strengthening / weakening would also likely help. [Peter Thorne, Ireland]	Taken into account - percentage are incorporated into the figure. Will not be adding additional annotation.
65705	158	1	158	10	Suggest including a colour legend for each SSP ensemble (as in Figure 4.7) for Figure 4.16 and Figure 4.33 (page 175). [Kushla Munro, Australia]	Taken into account - legend has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104643	158	7	158	8	Does the term "SSP-averaged percentage" mean the percentage within each SSP, averaged over the five SSPs, or the % of all the 100 simulations having positive anomalies, regardless of SSP? (The two will differ when the SSPs include differing numbers of simulations.) If it is the latter, recommend modifying "SSP-averaged percentage of simulations with positive anomalies" to "percentage of simulations among all the SSPs with positive anomalies". [William Merryfield, Canada]	Taken into account - text has been modified.
87635	159	1	159	1	Fig.4.17: does it refers to Nino3, or Nino3.4? [Valentina Roberta Barletta, Denmark]	Taken into account. For consistency, Niño 3.4 region is now used in Fig. 4.17
103051	159	1	159	1	Fig.4.17: does it refers to Nino3, or Nino3.4? [Philippe Tulkens, Belgium]	Taken into account. For consistency, Niño 3.4 region is now used in Fig. 4.17
104649	159	4	159	4	Why is ENSO precipitation considered to Niño3-area averaged precipitation? ENSO-driven precipitation variability in the Niño4 region (and to a lesser extent the Niño3.4 region) is considerably larger and has a greater bearing on teleconnections. See for example https://psl.noaa.gov/cgi-bin/data/correlation/corr.test1.pl?iregr=2&var=GPCP+Precipitation&level=Surface&mon1=12&mon2=2&iy%5B1%5D=1970&iy%5B2%5D=2019&ilead=0&ilag=0&type=4&timefile=&customtitle=&labelc=Color&labels=Shaded&cint=&lowr=&highr=&scale=&switch=0&proj=ALL&xlat1=&xlat2=&xlon1=&xlon2=&custproj=Cylindrical+Equidistant&level1=1000mb&level2=10mb&Submit=Create+Plot [William Merryfield, Canada]	Taken into account. The use of Niño 3 is more focusing on the extreme ENSO events (e.g., Cai et al. 2014). For consistency, Niño 3.4 region is now used in Fig. 4.17
51073	159	6	159	6	typo - 'whining'? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issue will be fixed then.
51071	159	Fig 4.17	159	Fig 4.17	It is not clear what 'normalised standard deviation' means in the context of ENSO precipitation - SD of 2. Please explain how much precipitation change this corresponds to. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. SD of 2 indicates ~ 100% increase of ENSO precipitation SD noted in caption of Fig. 4.17.
87637	160	1	160	1	Fig 4.18 title; Why not use “Global Surface Air Temperature change” as title, instead of Global temperature change? [Valentina Roberta Barletta, Denmark]	Accepted, revised.
103053	160	1	160	1	Fig 4.18 title; Why not use “Global Surface Air Temperature change” as title, instead of Global temperature change? [Philippe Tulkens, Belgium]	Accepted, revised.
21733	160	1	160	1	Could some non-code labelling be used rather than / in addition to two SSP3-7.0 etc labels that are incomprehensible as standalone? [Peter Thorne, Ireland]	Rejected. To enable unambiguous identification, the experiment names must be used.
104629	160	7	160	12	Neither the legend not the caption to Fig. 4.18 indicate the meaning of the black curve and associated shading. [William Merryfield, Canada]	Taken into account. Legend and caption expanded.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12221	161	1	161	14	In panel a, what is the blue shade and white are the blue dots? I assume that the "most extreme member" has a large eruption arou9nd 2045 and again around 2070. One has no idea how these correspond to 19th or 20th century eruptions or what other eruptions might be simulated say before 2040. Given apparently the 60 members in VOLC have their eruptions randomly distributed in time, it is not surprizing the mean is very similar to VOLC-CONST. The red and magenta are very difficult to distinquish. One needs some measure of average recovery time after eruptions and maybe some measure of any effect on say Arctic sea ice or sea level. This and the corresponding text on page 50 are wholly inadequate. [Bryan Weare, United States of America]	Taken into account. We added the details of this figure. Sea ice and sea level responses are mentioned in Bethke et al. 2017, but there's not enough space for all the details.
104657	162	5	162	5	It's not exactly clear what is meant by " Niño-3 index with zonal mean removed". Is it that the 0-360 zonal mean of SST between 5S and 5N is subtracted from the Niño-3 index? [William Merryfield, Canada]	Note. We isolate the intrinsic ENSO signal from the volcanically-induced surface cooling by using relative SST. The SST response is calculated by removing zonal means (0-360).
12223	163	1	163	7	Again the stippling makes it very difficult to see the color value of any sjpecific region. Why not simply hatch the low confidence regions and say that everywhere else meets xxx criterioa? [Bryan Weare, United States of America]	Taken into account. The stippling now follows the mew
12225	164	1	164	5	Here, where we are looking at differences that are less likely to be significant, there are no measures of signficance. I suspect most regions are not. [Bryan Weare, United States of America]	Noted.
69917	164	1	166	2	Figure 4.22 & Figure 4.24: The model agreement in the sign of change (as seen in figure 4.11 and several others) is absent these figures. [SAHIL SHARMA, India]	Taken into account. Revised.
51075	164	3	164	4	JJA minus warming in DJF in 2081-2100 relative to 1995-2014' - is this a combination of the 2 differences? It would be helpful to separate these out. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Yes, this is the change in the seasonal differences. The seasonal warming cannot be shown here due to space constraints but is shown in the Atlas.
12229	165	1	165	9	The legend is inadequate. Exactly what standard deviations are calculated? What is the reference period? What regions are significant? No mention is made of the fact that over much of the globe the three models give very different results such that a 3-model mean would be near zero exept at high latitudes. [Bryan Weare, United States of America]	Taken into account. The figure is updated using multi-model large ensembles.
104659	165	4	165	9	The caption to Fig. 4.23 does not state clearly over which periods these changes occur. [William Merryfield, Canada]	Taken into account. The caption has been updated with the periods considered.
11289	165		165		I suggest adding other large ensembles (IPSL CM6, MIROC6 etc) by the final draft. Same for Fig. 4.39. [Masahiro Watanabe, Japan]	Taken into account. The figure is updated using multi-model large ensembles.
21735	166	1	166	1	Overall figure title would help. Also, denoting the climatological tropopause in each panel would likely help reader interpretation [Peter Thorne, Ireland]	Taken into account. Figures are improved.
12231	166	1	166	4	There needs to be some measure of signficance like in most other figures [Bryan Weare, United States of America]	Taken into account. Shading has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12233	167	1	167	8	For the SSP1-2.6 the eye is drawn again to the insignificant regions and the small number of significant regions are hardly visible. It should be made clear that the units are % change from the local mean, not rh difference from the reference. [Bryan Weare, United States of America]	Taken into account. The hatching method has been updated in accordance with other chapters.
104661	167	7	167	8	The caption of Fig. 4.25, which refers to annual mean 2041-2060 and 2081-2100 changes for SSP1-2.6 and SSP5-8.5, does not reflect the actual figure which shows JJA and DJF changes for 2081-2100 for the two scenarios. [William Merryfield, Canada]	Corrected
55517	167	7			Figure 4.25:If possible, clarify ent corresponding epigraph, the meaning of the "hatching area" . The same comment would be valid for Figures 4.27 and 4.28 [Maria del Pilar Bueno Rubial, Argentina]	Taken into account. The hatching method has been updated in accordance with other chapters.
51077	167	Fig 4.25	167	Fig 4.25	The cross-hatching makes it difficult to discern the colour shading beneath it, perhaps a bolded outline could be used intstead? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The hatching method has been updated in accordance with other chapters.
21737	168	1	168	1	Add heat stress in parentheses to title for accessability as otherwise what simplified WBGT is an indicator of may be lost to the reader? [Peter Thorne, Ireland]	The figure is removed
87639	169	1	169	1	Fig. 4.27: The figure, as for 4.25, shows results for 2081-2100 only. Therefore the caption is wrong. [Valentina Roberta Barletta, Denmark]	Caption corrected
103055	169	1	169	1	Fig. 4.27: The figure, as for 4.25, shows results for 2081-2100 only. Therefore the caption is wrong. [Philippe Tulkens, Belgium]	Caption corrected
12239	169	1	169	4	Tt is difficult to find the significant stippled regions.There are few for SSP1-2.6. For SSP5-8.5 only with strong magnification can I see that the tropical features are signficant. Again, ALL similar figures must be plotted to properly highlight the significant regions and especially not highlight those with little significance. [Bryan Weare, United States of America]	Taken into account. The hatching method has been updated in accordance with other chapters.
104663	169	3	169	4	The caption of Fig. 4.27, which refers to annual mean 2041-2060 and 2081-2100 changes for SSP1-2.6 and SSP5-8.5, does not reflect the actual figure which shows JJA and DJF changes for 2081-2100 for the two scenarios. [William Merryfield, Canada]	Caption corrected
21739	171	1	171	1	Add title to the figure for accessability? [Peter Thorne, Ireland]	Taken into account. Figures are revised accordingly.
96519	171	3	171	5	Figure 4.29: Please explain the meaning of the hatched areas. [Nicole Wilke, Germany]	Accepted. Caption has been improved.
12245	172	1	172	7	Why is the significance criterium so much weaker than other plots in this chapter? I question including this at all. [Bryan Weare, United States of America]	Taken into account. It has been improved.
7419	174	0	174	0	Fig 4.35 : please check the coherency in the region name between the figure and the caption : Central Europe vs Greenland. [Geremy PANTHOU, France]	Noted - Corrected
113705	174	3	174	4	"(a) the Greenland region (65°W–20°W, 62.5°N–72.5°N), (b) the Central European region (20°W–20°E, 45°N–65°N)" -- either it should be the other way round: "(a) the Central European region (20°W–20°E, 45°N–65°N), (b) the Greenland region (65°W–20°W, 62.5°N–72.5°N)" or the Figure labels are mixed, please double check [Agnieszka Kowalczyk, Poland]	Noted - Corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21741	175	1	175	1	See comments on p.158 figure [Peter Thorne, Ireland]	Noted. Fig. 4.33 no longer displays percentage of model showing one sign of change in the legend.
87641	176	1	176	1	Fig 4.34: the color scale is such that almost all the warming regions are beyond saturation. The scale does not need to be symmetric, no cooling is expected, while it is more important to see what temperature could be reached locally. [Valentina Roberta Barletta, Denmark]	Taken into account. We've changed the colour bar to assign more intervals to higher temperature increases, and fewer to lower increases. However, we are consistently across the chapter (not only on this section) including in the colour bars for temperature also negative anomalies.
103057	176	1	176	1	Fig 4.34: the color scale is such that almost all the warming regions are beyond saturation. The scale does not need to be symmetric, no cooling is expected, while it is more important to see what temperature could be reached locally. [Philippe Tulkens, Belgium]	Taken into account. We've changed the colour bar to assign more intervals to higher temperature increases, and fewer to lower increases. However, we are consistently across the chapter (not only on this section) including in the colour bars for temperature also negative anomalies.
12253	176	1	176	9	I see no value to frames e),f), and g). The information content in c) and d) is already pretty low. Hatching only obscures. [Bryan Weare, United States of America]	Taken into account. We have changed the hatching convection, and for the variable temperature the graphics now show minimal hatching. The comments on frame e to f is rejected. These frames in combination clearly show regional consequences (in terms of temperature change) should the 1.5 degrees C level of global warming be exceeded.
104673	176	1	176	12	In Fig. 4.34 the >5 degree shading becomes saturated in the 4 degree warming map, masking the magnitude of the most extreme changes. Suggest therefore adding another contour level at 7 or 7.5 degrees to the first color bar. [William Merryfield, Canada]	Taken into account. We've changed the colour bar to assign more intervals to higher temperature increases, and fewer to lower increases. However, we are consistently across the chapter (not only on this section) including in the colour bars for temperature also negative anomalies.
96521	176	4	176	9	Fig. 4.34 (and other such figures): please explain what the numbers next to the maps mean (# available simulations?). Are ensemble members of the same model included? [Nicole Wilke, Germany]	Accepted. Yes, the numbers refer to the number of models contributing to the ensemble average shown. Only one ensemble member is used per model. These aspects are now made clear in the text.
96523	176	4	176	9	Fig. 4.34: Please specify if this is near-surface/2m air temperature or surface temperature or something else. [Nicole Wilke, Germany]	Accepted. These are maps of changed in near-surface temperature, and the Figure caption has been updated.
96525	176	7	176	8	Cross-hatching for areas where the sign of change agrees between 2/3 of models seems a rather weak measure of robustness for this figure. Please consider to modify. [Nicole Wilke, Germany]	Accepted. The Figure has been modified with a new hatching convention; robustness is now defined as the multi-model mean change exceeding two standard deviations of pre-industrial internal variability and where at least 90% of the models agree on the sign of change
12255	177	1	177	6	The hatching obscures this even more in this plot than most of the others.The large region of insignificance over the oceans jumps out and the smaller ones of signficance are nearly lost. Percent change values for places like the Sahara are misleading. [Bryan Weare, United States of America]	Taken into account. Much effort has gone into proper graphical choices for displaying robustness or lack thereof. Percentage changes have been retained, however, since they are superior to the alternatives.
104675	177	1	177	9	Fig. 4.35b has some white and gray areas in the Arctic whose meaning isn't indicated. [William Merryfield, Canada]	Taken into account. Figure has been modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51079	177	Fig 4.35	177	Fig 4.35	The areas of the map that are very dark green: do the projected change for these areas look particularly extreme because it's a percentage change and they are currently arid and therefore even a small change in precipitation corresponds to a large % change? It would helpful to clarify this in the figure annotation. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Yes, that is correct for some of the regions (e.g. the Sahara in Africa), but not true for others (e.g. tropical Pacific Ocean). We had added an explanation to the text to this effect (rather than in the Figure caption).
87643	178	1	178	1	Fig. 4.36: The line width and the font size of the text are too small. [Valentina Roberta Barletta, Denmark]	Taken into account. The figure has been modified
103059	178	1	178	1	Fig. 4.36: The line width and the font size of the text are too small. [Philippe Tulkens, Belgium]	Accepted, figure has been modified.
21743	178	1	178	1	As plotted and described in the caption I do not understand what this figure is showing. Efforts are required to increase accessability of this plot and describe it better in the caption. [Peter Thorne, Ireland]	Taken into account. The caption has been improved.
12257	178	1	178	6	No mention is made in the caption of the upper frames, which like very much like the lower. How is "significant precipitation increase" assessed? [Bryan Weare, United States of America]	Taken into account. Figure caption has been modified.
104677	178	1	178	8	This caption should indicate what the upper and lower rows are too. (There seems some ambiguity because the legend in the lower row suggests total global area fraction, whereas lines 11-13 on p. 4-73 make reference to ocean surface area fraction.) [William Merryfield, Canada]	Taken into account. Figure caption has been modified.
21745	179	1	179	1	The panel titles have lost the hyphenation in the SSP scenarios. You have room to spell out difference in full for the two right hand maps. The two line plots panels also could be spelt out in full rather than using the CF parameter codes which are inside-baseball speak. [Peter Thorne, Ireland]	not applicable, figure no longer presented
69921	179	1	179	2	Figure 4.37: The label bars are too small to read in the top and middle row. Likewise, there is no mentioning of variable unit in the figure caption. [SAHIL SHARMA, India]	taken into account, figure no longer presented
12259	179	1	179	8	I see little value to this figure and accompanying text. The left and middle frames look nearly identical and there is no significance assessment given to the differences on the right. I suspect there is little. Unlike what is stated in the caption the bottom row is not differences, but hard to separate means with no measures of variability going into those means. Given the large variability of precip near the equator, I doubt any of these are significant. [Bryan Weare, United States of America]	taken into account, figure no longer presented
104681	179	1	179	9	There is not enough information to inform the reader how this Fig. 4.37 was constructed. Does it use the same methodology as Fig. 4.34? [William Merryfield, Canada]	not applicable, figure no longer presented
87645	180	1	180	1	Fig. 4.38: Fonts and legends, reorganize the panel for better readability. [Valentina Roberta Barletta, Denmark]	accepted, figure has been clarified
103061	180	1	180	1	Fig. 4.38: Fonts and legends, reorganize the panel for better readability. [Philippe Tulkens, Belgium]	accepted, figure has been clarified
21747	180	1	180	1	An overall title for this figure such as "Effects of active CDR on key indicators" would increase accessability [Peter Thorne, Ireland]	taken into account, although this is not just about CDR. Figure and caption have been clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12263	180	1	180	10	These plots would be much clearer if there dates included along the lines identifying when a CO2 value is achieved. The important thing is to follow in increasing time the red lines to the right and the blue lines to the left. Why is the temperature different at the turn around point of CO2 571? There is no shaded grey bars as stated in the legend. There seems an important increase in temperature variability during the CO2 decrease in frame a). This important figure needs to be totally redone. [Bryan Weare, United States of America]	taken into account, figure has been clarified. The grey bars are there in the pdf version for review and have been retained.
21749	181	1	181	1	I find this figure and the caption very difficult to follow. Could efforts be made to improve clarity of what is being shown and why it matters? [Peter Thorne, Ireland]	Taken into account. Both the figure and caption are updated.
55505	181	11			Mahler ..Fig. 4.39. Submitted. (included in TEXT). Check publication date [Maria del Pilar Bueno Rubial, Argentina]	Taken into account. Text is revised. All references are updated where required.
87647	182	1	182	1	(Fig. 4.40: The figures are not very readable. Improve the quality/readability. [Valentina Roberta Barletta, Denmark]	Taken into account. The figure and caption are revised for FGD.
103063	182	1	182	1	(Fig. 4.40: The figures are not very readable. Improve the quality/readability. [Philippe Tulkens, Belgium]	Taken into account. The figure and caption are revised for FGD.
21751	182	1	182	1	An overall title for the figure and addition of panel figures would help enormously here. Many of the fonts are also barely legible at this size let alone if will be reduced in size in final production. [Peter Thorne, Ireland]	Taken into account. Figure is revised for FGD.
106319	182	1	182	17	Figure 4.40: It would increase the usefulness of this figure tremendously if diagnosed emissions compatible with this CO2 trajectory would also be shown. If these are not available from the CDRMIP experiments, maybe an illustrative path can be provided based on an established simple carbon-cycle and climate model or EMIC. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Diagnosed emissions are not available from CDRMIP. The figure, caption and text are revised for FGD.
12273	182	1	182	20	These 4xCO2 experiments are too extreme and over a time frame of little value to policy makers. Why only thermostatic sea level differences. Nothing about glacial extent? [Bryan Weare, United States of America]	In SSP5-8.5, CO2 levels could be 4 times the pre-industrial levels. Hence 4xCO2 experiments are not unrealistic. Model outputs provide only thermosteric sea levels. Climate models do not simulate glacier melt.
21753	183	1	183	1	Would an overall title for figure help? Should CO2 be Carbon Dioxide Removal [Peter Thorne, Ireland]	Taken into account. Figure caption is revised. CO2 case simply means the difference between 1CO2 and 4CO2 simulations, which serves as a reference case for the comparison with SRM effect.
12279	183	1	183	15	There needs to be some measure of significance for T. The "stippling" in the precip is very irregular and confuses the reader. [Bryan Weare, United States of America]	Taken into account. Figure is updated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41983	183	1			Chapter 4 page 183 lowest row of images shows quite beneficial results for marine cloud brightening with more precipitation in drought-stricken regions and strong Arctic cooling to save the ice. However the spray patterns which gave these beneficial results were chosen for the convenience of existing climate models. They were unchanged through the year often between latitudes just between 45N and 45S. They used the wide spreads of natural aerosol drop sizes built in to climate models rather than our mono-disperse spray which should reduce coalescence losses. We need to use the spray size to just get the Kohler nucleation for each place and season and nearly one cloud drop for each spray drop. The best places to spray might be close to the Arctic for two summer months when there is more solar energy that at the equator, a high cloud fraction and a low marine boundary layer depth. With exception of Stjern 2018 all the modellers ignore the agility of spray vessels and the high frequency response of the troposphere giving the low phase-lag desirable in engineering control systems. It is foolish to spray when it is raining or snowing or where there are high wind speeds giving large amounts of natural spray. [Stephen Salter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, but this discussion is beyond the scope of this chapter.
51083	183	12	183	15	Please clarify what is depicted in the top row. Rows 12-15 of the caption say it's the low-CO2 change pattern - but rows 3-5 of the caption seem to be saying something different? The text (p.84 rows 15-19) says this figure shows patterns relative to both high- and low-CO2 worlds, but this is difficult to see. Perhaps the figure would benefit from being split into two? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure caption is revised
51081	183	Fig 4.41	183	Fig 4.41	Do these GeoMIP simulations assume globally even distribution of stratospheric sulphate aerosal injection/MCB/SRM? It would be helpful to clarify this in the figure annotation. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure caption is revised.
69923	184	1	184	9	Figure 4.42: The different shading colors used for historical and SSP’s scenario has no prior information in the figure caption [SAHIL SHARMA, India]	accepted. Figure has been updated
9839	184	1	184	9	Here and throughout the report, I suggest using the tphrase "global mean thermosteric sea-level rise" as recommended by Gregory et al 2019 [Robert Kopp, United States of America]	accepted. We have removed the section on sea-level rise as this is covered much more comprehensively in chapter 9. We now clearly signpost to that assessment instead of the short coverage here.
51085	184	2	184	2	CMIP5' - is there a reason why CMIP6 results are not presented here? This may cause some confusion with results in this chapter and across the report - for example the sea level rise projections here vs those in the SROCC and Chapter 9. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. CMIP6 data was not available at time of SOD. This section now makes use of available CMIP6 data and emulators
19237	184	2	184	7	Figure 4.42. the reference period should be added in the caption. Perhaps the units for precipitation could be the same as in Fig 4.1? The title of panel d) could be "steric contribution to global sea level change" for better clarity. [Anne-Marie Treguier, France]	accepted. Figure caption has been updated

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21755	185	1	185	1	Small effiorts such as adding figure and panel titles would increase accessability of the figure enormously [Peter Thorne, Ireland]	accepted. figure improved
87649	186	1	186	1	Fig. 4.44: the color scale in the figure is symmetric but the change in the signal is almost completely positive. A different color scale could improve readability a lot. [Valentina Roberta Barletta, Denmark]	Rejected. Colour bar is consistent across chapter.
103065	186	1	186	1	Fig. 4.44: the color scale in the figure is symmetric but the change in the signal is almost completely positive. A different color scale could improve readability a lot. [Philippe Tulkens, Belgium]	Rejected. Colour bar is consistent across chapter.
12283	186	1	186	13	This figure needs to be totally revised. I see no value to the ill-defined c) and d) which do not include any measure of signficance. Clearly these frames are very much like e) and f). How much value can be ascribed by the "beyond likely range" respresented by only two models. A new pair of frames needs to be added that describes the "lower bound of the very likely range". Clearly this is equally likely and of interest. Exactly what is the stippling? [Bryan Weare, United States of America]	More models are added. Lower bound is not added because it is not associated with high risks as discussed in the test.
19251	187	1	187	16	Figure 4.45, panel d: the model showing high precipitation over Australia is striking. Which model is it? Is there any explanation for this behavior? [Anne-Marie Treguier, France]	The regional precipitation response is discussed in chapter 8 and the Atlas
12285	187	1	187	16	As in reference to fig. 4.44 b) should be eliminated. I do not understand the value of difficult to interpret e) and f). They do not represent any climate state. [Bryan Weare, United States of America]	Rejected. The panels contrast the local and large-scale uncertainties. The fact that they do not represent a coherent spatial response pattern is made clear in the text
40193	188	0			Fig FAQ4.1: I would shorten the title of the figure to : "FAQ4.1: How will climate change in the next 20 years? " [TSU WGI, France]	Taken into account. Title shortened.
40811	188	0			specify in the caption that SSPs are low/high emission scenarios (and maybe also on the plot itself?) [TSU WGI, France]	Taken into account. Caption says that already.
41111	188	0			Try to reduce the length of the caption [TSU WGI, France]	Taken into account. Request clashes with TSU suggestion to add more information to caption. (Note that FAQ captions do not follow the same guidelines as the rest of the chapter).
12293	188	1	188	13	These curves need prominent mult-model means. The lower frame needs comparable bars show the 2100 values. [Bryan Weare, United States of America]	Taken into account. Figure has been revised for clarity.
26879	188	10	188	10	It is needed to highlight these models, because the comment is difficult to understand by simply looking at the spaghetti figure [Eric Brun, France]	Taken into account. Figure has been revised for clarity.
51087	188	FAQ 4.1, Fig	188	FAQ 4.1, Fig	Please label the top and bottom graphs (a) and (b) and add 'September' to the y-axis annotation for (b). It would also be useful to include a right-hand-side y axis, showing the absolute area of Arctic sea ice in September (currently it is quite difficult to discern when it reaches 'ice free'). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure has been revised for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11291	188		188		I think FAQ4.1 Fig. 1 should be further revised to improve clarity. For example, * ensemble size seems small for each SSP, and for demonstrating relative roles of internal variability and externally forced response is it better to show results from large ensembles as in FAQ4.1 Fig 2? * Please show the response in 2100 in (b) * In the text please refer to figures that show the time evolution of emissions (or radiative forcing) for SSPs 1-2.6 and -8.5 so that readers understand if the forcing is different between the two in the near term [Masahiro Watanabe, Japan]	Taken into account. Figure has been revised for clarity.
40195	189	0			Fig FAQ4.2: I think it would be clearer to plot 3 variables CO2 emissions, CO2 concentrations, T change/trend. Either as 3 different panels our on the same figure [TSU WGI, France]	Accepted. Figure re-designed along the suggestion.
39687	189	0			x label is misleading: it's not a trend but rather a period [TSU WGI, France]	Taken into account. Figure has been revised for clarity.
39977	189	0			At the moment, it is a relatively hard to quickly get the take-home message of the figure. One option could be to change the labelling to highlight more the last date of the period (e.g. grey (2021) and black for 2040) could help understanding that it's a time period but at the same time we know that in 2040 we might be able to detect something? Another option could be to add visual cues to indicate when the trend starts to be detectable. [TSU WGI, France]	Taken into account. Figure has been revised for clarity.
40901	189	0			the caption is very long and seems a bit outdated [TSU WGI, France]	Taken into account. Figure has been revised for clarity.
39665	189	0			it would be helpful to see on the figure when GHG emission decrease (though it might be challenging because of x axis is 20 year period) [TSU WGI, France]	Accepted. Figure re-designed along the suggestion.
96527	189	1	189	1	FAQ 4.2 Figure 1: It would be helpful to add a thin horizontal line at 0.0. Then it would be easier to see that even with the low emissions scenario, it will take quite some time until the temperature trend reaches 0 and thus that even here, it will increase for several decades. [Nicole Wilke, Germany]	Taken into account. Figure has been revised for clarity.
12295	189	1	189	16	This figure should include a plot of emssions or net emissions in these two cases. Another frame should be added for sea level. Has this experiment not been reproduced with another model? [Bryan Weare, United States of America]	Accepted. Figure re-designed along the suggestion.
11293	189		189		I think this FAQ should mention the climate change commitment, and in doiing this, I suggest * showin time series of a slow component of the climate system (e.g., AMOC) in panel (b) * referring to figures that show the time evolution of emissions (or radiative forcing) for SSPs 1-2.6 and 3-7.0 in the text at L.29-38 [Masahiro Watanabe, Japan]	Taken into account. Figure has been revised for clarity.
40197	190	0			Fig FAQ4.3 adding visual cues would help to quickly get the take-home message of the FAQ [TSU WGI, France]	Taken into account. Figure has been revised for clarity.
87651	190	1	190	1	FAQ 4.3, Fig 1: the scale for negative temperature is not relevant, and it flattens the details of the warming part. [Valentina Roberta Barletta, Denmark]	Taken into account. Figure has been revised for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103067	190	1	190	1	FAQ 4.3, Fig 1: the scale for negative temperature is not relevant, and it flattens the details of the warming part. [Philippe Tulkens, Belgium]	Taken into account. Figure has been revised for clarity.
96529	190	4	190	5	FAQ 4.3 Figure 1 description: The non-expert reader might wonder why one scenario would result in such different warming levels. We therefore encourage the authors to clarify that the pattern shown are characteristic for any emission scenario for a given warming level, and that the warming levels shown are transient. This latter could be clarified by providing the respective time periods. [Nicole Wilke, Germany]	Taken into account. Text has been revised for clarity.
16019	4-44	42	45	4	The increase of Nino 3 precipitation may be asscociated with the background temperature increase in the Figure4.17, which is also seen in the Figure 4.12. If it is really associated with the ENSO, give more explainations. In addition, This figure does not focus on the description of the near-term change and it is better to move them into the former/latter section. [Lijuan Li, China]	Rejected. The figure explains the near-term change and further future changes (it's addressed in latter section).
16021	4-45	7	47	44	One or two figures/tables based on the DCPP or SSP experiments would strengthen the description. [Lijuan Li, China]	Noted. Thank you for the suggestion. However, considering the limitation in the number of pages and figures in the chapter, general comments by other reviewers and the limited amount of literature to assess related to these sections, we decided of not including further tables/figures.
16011	4-15	21		27	The words in red are suggested to added. "There is also a wide range of techniques employed to assimilate observed information into models in order to generate suitable initial conditions (Polkova et al., 2019). These range in complexity from simple relaxation towards observed time series of sea surface temperature (SST) (Mignot et al., 2016) or wind stress anomalies (Thoma et al., 2015a, 2015b), to relaxation toward three-dimensional ocean and sometimes atmospheric state estimates from various sources (e.g., Pohlmann et al., 2013; Knight et al., 2014), to sophisticated data assimilation methods such as the ensemble Kalman filter (Msadek et al., 2014; Karspeck et al., 2015; Polkova et al., 2019), the four-dimensional ensemble-variational hybrid data assimilation (He et al., 2017, 2020) and the initialization of sea ice (Guemas et al., 2016; Kimmritz et al., 2018)"References:He, Y. J., Wang, B., Liu, M. M., Liu, L., Yu, Y. Q., Liu J. J., et al. (2017). Reduction of initial shock in decadal predictions using a new initialization strategy. Geophys. Res. Lett. 44, 8538-8547. doi:10.1002/2017GL074028. He, Y. J., Wang, B., Huang, W. Y., Xu, S. M., Wang, Y., Liu, L., et al. (2020). A new DRP-4DVar-based coupled data assimilation system for decadal predictions using a fast online localization technique. Clim. Dyn. 1-19. https://doi.org/10.1007/s00382-020-05190-w [Lijuan Li, China]	Accepted

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16013	4-15	36		45	The words in red are suggested to added. “A consequence of model imperfections and resulting model systematic errors or biases is that estimates of these errors must be removed from the prediction to isolate the predicted climate anomaly. Because of the tendency for systematic drifts to occur following initialization, bias corrections generally depend on time since the start of the forecast, often referred to as lead time. In practice, the lead-time-dependent biases are calculated using ensemble retrospective predictions, also known as hindcasts, and recommended basic procedures for such corrections are provided in previous studies (Goddard et al., 2013; Boer et al., 2016). The biases are also dynamically corrected during hindcasts and predictions by incorporating the multi-year monthly mean analysis increments from the initialization into the initial condition at each integration step (Wang et al, 2013). Besides mean climate as a function of lead time, further aspects of decadal predictions may be biased, and additional correction procedures have thus been proposed to remove biases in representing long-term trends (Kharin et al., 2012; Kruschke et al., 2016; Balaji et al., 2018), as well as more general dependences of drift on initial conditions (Fučkar et al., 2014; Nadiga et al., 2019).” Reference：Wang, B., Liu, M. M., Yu, Y. Q., Li, L. J., Lin, P. F., Dong, L., et al. (2013). Preliminary evaluations of FGOALS-g2 for decadal prediction. Adv. Atmos. Sci. 30, 674–683. doi: 10.1007/s00376-012-2084-x. [Lijuan Li, China]	Accepted
16007	4-12	9		9	“dome” shoud be "done" [Lijuan Li, China]	accepted. Done
16009	4-12	26		28	The DCPP is missed in the table 4.1 while the DCPP is used in this chapter. [Lijuan Li, China]	Accepted. DCPP is included in the Table 4.1 in the final publication.
16015	4-25	38			There are some mismatches of the tempearature values between the description and the tables. For example, in the description the land warming ranges for SSP5-8.5 are 3.2-7.4, while in the table 4.2, the ranges are 3.1-7.4 [Lijuan Li, China]	Accepted
16017	4-28	10			In table 4.4, there are a lot of negative values for sea ice area in the parentheses. In theory, the area should not be negative. [Lijuan Li, China]	Accepted. 5-95% ranges based on percentiles are now used thereby solving the issue of "negative" ice area.
79719	3'	22	34	24	I would suggest using a low-frequency loess filter to define the long-term Pacific warming and use the residual to characterize the variability. I also think it makes more sense to do this for each model separately rather than using the multimodel mean. I also wonder if the analysis could use winter seasonal means rather than annual means. [Laurent Terray, France]	Noted. These are all valid suggestions, but since this calculation is underpinned by the existing CMIP6 literature, this is the approach that needs to be used here.
18999	table 4.10				Shouldn't the second column be called "abrupt change?" rather than "tipping point" [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	taken into account. Table columns and content were updated
18989	table 4.5				I know meters is the SI unit, but I think using mm or cm would make the table much more useful to use oustide of science and to see the big differences quickly [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Must use units of metres for consistency the quoted AR5 values.

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18997	table 4.9				This table is really useeful, it would be great if it had confidence statements for all entries. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	accepted. Confidence statements added
19001		27		31	this section is extremely useful, but this last sentence would be much more so if it would have an assessment on how (e.g. linearly or nonlinearly) these other variable would change in this high-warming scenario. Ideally it could be picked up e.g. in chapter 11 and/or 12 to include extreme and hazard storylines. [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Unclear what comment refers to.
69925					In general, one schematic diagram which exemplifies the teleconnection and modes of variability in the near and far future in different CMIP6 SSP scenario is included in chapter 4, to make the teleconnections more plausible to common public. [SAHIL SHARMA, India]	Taken into account. Such a schematic is being considered in a Technical Annex.
116273					Congratulations for a concise, well developed chapter. The preamble of the Es, for temperature, could highlight methodological changes compared to AR5 (use of emulators and assessed sensitivity to report warming levels expressed in GSAT). [Valerie Masson-Delmotte, France]	Taken into account. Suggestion duly noted.
116275					The comparison between CMIP5 and CMIP6 model results need to include a comparison of the actual forcing corresponding to the range of RCP and the range of SSP. [Valerie Masson-Delmotte, France]	Accepted. This is discussed in a newly designed subsection 4.6.2 and is also covered in emulator cross-chapter box hosted in chapter 7 (box 7.1).
116279					Could heat stress be also considered in ch 2 and 3 (observations, attribution) so that there would be a better flow of information on this aspect? [Valerie Masson-Delmotte, France]	Rejected. The part of heat stress assessment in Section 4.5.1 was moved to Chapter 12. Decision was made since heat stress is extensively covered by Ch12. We also considered structural coherence with Ch2 and Ch3.
116283					In the chapter ES, it is hard to link the outcome of chapter 3 (model evaluation, attribution) for how confidence in projections is addressed. [Valerie Masson-Delmotte, France]	Taken into account. Evaluation has been stressed more clearly in FGD.
116285					Cross chapter coordination is needed for monsoons, to develop integrated conclusions for the TS-SPM. [Valerie Masson-Delmotte, France]	Accepted and implemented. Ch8 takes charge of coordinating and integrating monsoon assessment.
116287					Reasons for major changes in projections compared to AR5 need to be provided in the ES (if aspects differ from AR5 or use of CMIP5 results in AR6 SR). [Valerie Masson-Delmotte, France]	Taken into account. This comparison was already explicit in SOD for GSAT change and has been added for other quantities.
116547					Please consider the insights from chapter 6, section 6.3 for confidence related to ERF by aerosols in SRM. [Valerie Masson-Delmotte, France]	Taken into account; reference to Chapter 6 is now more explicit.
116297					Could an integrated table related to key large scale climate variables (global, continental / ocean basin scales) be developed building on ch 2 (observations), 3 (detection + attribution, confidence in models / evaluation), and 4 (projections, time of emergence or condition of emergence related to level of warming) be developed for the TS? [Valerie Masson-Delmotte, France]	Taken into account and partially implemented, such as for GSAT change, but it did not materialize across the board.
111965					A bit inconsistent structure of the chapter. It looks strange after quite consistent structure of the section 4.3.2 and 4.5.2, why not to discuss cryosphere, … [Tomas Halenka, Czech Republic]	Taken into account. A visual abstract has been added. Cryosphere is mainly treated in Chapter 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116323					Closer links to chapter 3 are needed on teleconnections (sections 4.5.3) [Valerie Masson-Delmotte, France]	Taken into account and implemented.
116329					There is a potential to develop a storyline related to the potential occurrence of one or several major volcanic eruptions in the near term building on ch 2-3 (paleo, modes, model evaluation), 7 (feedbacks), and this chapter, including implications for regional water cycle (ch 8) and CID (ch 12). [Valerie Masson-Delmotte, France]	Taken into account. However, the occurrence of a major eruption in the near term is speculative.
116585					Please note that Chapter 4 is too long by around 5%, so attention to length is needed when revising the text, figures etc. [Valerie Masson-Delmotte, France]	Taken into account. Attention has been paid to length.
116339					It is a pity that biosphere aspects are only weakly covered in ch 4, while they are addressed in ch 2 (greening trend, marine productivity). Could ch 4 build on SROCC on the emergence of new ocean states? [Valerie Masson-Delmotte, France]	Rejected. This should be covered by either Ch05 or Ch09. We have already been criticised for every bit of overlap with these and other chapters.
71287					Mention reginal difference of tendency in precipitation change in summary. [Kenji Taniguchi, Japan]	Noted. Since Ch8 assesses regional differences of precipitation change in detail, we just cross-referred to Ch8 for those information.
71289					Mention possibility of reducing extremes by mitigation in summary. [Kenji Taniguchi, Japan]	Rejected. Covered by Ch11.
71291					It may be important message to show arctic ice-free condition could not recover even if CO2 reducing scenario (put in summary) [Kenji Taniguchi, Japan]	Rejected. The statement is incorrect; see Ch09.
15483					Suggest including the projections under SSP4-6.0 to facilitate comparison with AR5. [SAI MING LEE, China]	Rejected. The simulations are not available in sufficient numbers.
71293					Irreversibility is important message, but only once mentioned in summary. Additional description is better. [Kenji Taniguchi, Japan]	Taken into account. However, there is insufficient space to accommodate every request in the ES.
71297					Compared to Chapter 11, English is poorer. [Kenji Taniguchi, Japan]	Taken into account. Attention has been paid to quality of language.
29315					very good work. But you must include the "surprise" of the Pandemic COVID-19 [Zangari del Balzo Gianluigi, Italy]	Taken into account. COVID-19 is now covered in a new cross-chapter box in Ch06.
116103					For the timing of reaching different temperature levels,here and in the TS/SPM, I suggest to provide a range of years (not a single year). There is a need for a concise description of reasons for changes compared to AR5 and SR (observed warming level + method GSAT + assessed transient response). Applying the exact same method on datasets from AR5, and each change since AR5 (observed warming; GSAT vs hybrid approach; CMIP6 compared to CMIP5; assessed transient response in AR6 compared to AR5) would be very helpful (also/ SR15). [Valerie Masson-Delmotte, France]	Taken into account. However, due to the much more complex and asymmetric range structure here, compared to the simplistic SR1.5 range, giving an explicit central estimate is essential. The reasons for change have been explain much more explicitly and robustly, in part through Cross-Chapter Box 2.3.

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79497					It is suggested that PMIP modeling be mentioned as well, and that its importance in future modeling and the increased accuracy of CMIP6 predictions be noted. (The Paleoclimate Modelling Intercomparison Project (PMIP) was established in the 1990s (Joussaume and Taylor, 1995) to understand the response of the climate system to different climate forcings and feedbacks. Through comparison with observations of the environmental impact of these climate changes, or with climate reconstructions based on physical, chemical or biological records, PMIP also addresses the issue of how well state-of-the-art numerical models simulate climate change. To achieve these goals, PMIP has actively fostered paleoclimatic data syntheses, model-data comparisons and multi-model analyses. PMIP also provides a forum for discussion of experimental design and appropriate techniques for comparing model results with paleoclimatic reconstructions. Five different periods have been designed to contribute to the objectives of the sixth phase of the Coupled Model Intercomparison Project (CMIP6) : the millennium prior to the industrial epoch (past1000), the mid-Holocene, 6,000 years ago (midHolocene); the Last Glacial Maximum, 21,000 years ago (lgm); the Last Interglacial, 127,000 years ago (lig127k) and mPWP, the mid-Pliocene Warm Period, 3.2 million years ago (midPliocene-eoi400). These climatic periods are well documented by paleoclimatic and paleoenvironmental records, with climate and environmental changes relevant for the study and projections of future climate changes. Analyses of the individual periods, across all the periods and comparisons with other CMIP6 simulations, will allow examination of relationships between forcings of different nature and amplitude and climate responses, and comparison of the processes involved in these responses. New foci will be put on the role of the ice-sheet and of its feedbacks with the atmospheric and	Taken into account. The evaluation part/fitness for purpose has been strengthened. Note, however, that PMIP is well known and that no comprehensive coverage of it would be appropriate in this chapters.
116365					The choice of contrasted scenarios for all figures of the chapter need careful discussion x WG (currently, SSP26 and SSP85, to be explained). [Valerie Masson-Delmotte, France]	Accepted. Cross-chapter coordination needed. Note that substantially fewer simulations exist for SSP1-1.9 than for SSP1-2.6, and that extensions beyond 2100 only exist for SSP1-2.6 and SSP5-8.5 (plus one overshoot scenarios).

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127629					[ENSEMBLES] The use of the CMIP6 ensemble is problematic as performed. Given the spread in ECS in the models is substantially wider (particularly at the high end) than the assessed range in Chapter 7, the implicit equating of the ensemble spread with the assessed spread in many of the conclusions cannot be supported. This was not the case in AR5, and so the problem there was less acute (though epistemically, the same problem was present). The authors acknowledge this problem in Box 4.1, but do not follow through on the task of *assessment* in much of what follows. It is not "assessment" to simply show the CMIP6 ensemble mean and spread. One does not 'conclude' that the mean of a series of numbers is the average: It just is. Thus the language throughout Section 4.3 that speaks of assessment, when it is merely reportage, is misplaced. Beyond the language issue is what the *assessment* should actually comprise. If some models are excluded on the basis of excessive warming in recent decades (i.e., based on Tokarska et al., Nijsse et al., or Brunner et al., etc.) for GSAT projections, it is incoherent to include them in time series projections of rainfall change or other metrics. The illustration of the relationships between GSAT and other metrics across the broader ensemble (even the models with highly unlikely ECS values) using the changes in those metrics plotted as a function of global mean temperature, regardless of when in time those temperatures are met in any specific model, is much to be preferred (as in Figures 4.34 and 4.35). [Trigg Talley, United States of America]	Taken into account. The author team acknowledges the fundamental difficulty highlighted -- not created, as noted here -- by the difference between assessed and simulation GSAT changes. The author team furthermore concurs that it is simpler to assess change as a function of GSAT change than as a function of scenario and time, because going through GSAT allows us to use the GSAT assessment explicitly. However, it must also be noted that not all quantities of interest scale with GSAT change, that regional some model performance is independent of performance in GSAT change (e.g., Beusch et al., GRL, 2020), that substantial assessment is requested as a function of time and scenario, and that some regional processes leading to higher climate sensitivity cannot categorically be deemed unrealistic (e.g., Zelinka et al., GRL, 2020). Ruling out a model entirely because of high ECS is hence not justified. That said, the difference between diagnosing and assessing the CMIP6 ensemble has been made clearer in the FGD, as has the challenge inherent in using the CMIP6 ensemble in the assessment.
116367					Remark : chapter 1 has a clear framing of emergence. While the chapter text refers to the notion of emergence, outcomes of assessments of emergence are not represented visually explicitely. Is this a deliberate choice? [Valerie Masson-Delmotte, France]	Taken into account. Concerning the emergence of benefits of emissions reductions, the deliberate emphasis has indeed been on the lack of unambiguous emergence in the near term. Some text has been added to point to this emergence (== difference between scenarios) in the time series.
127631					[ENSEMBLES] The CMIP6 ensemble is relatively diverse, but there are many common features to the models structure and common unresolved processes that might well impact projections -- i.e., insufficiently high model tops, insufficient vertical resolution, no QBO in most models, assumptions of static ice sheets, missing impacts of mountain glacier melt, missing forcings (irrigation, dam building), as well as the more standard concerns related to cloud processes, etc. As part of the assessment process, the impact of these issues should inform how much confidence one should have in model projections, regionally or by variable. This could be added to Section 4.9. [Trigg Talley, United States of America]	Taken into account. The evaluation part/fitness for purpose has been strengthened.

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127633					[ENSEMBLES] An additional FAQ should be produced to answer the most common question that is raised related to climate model predictions. Why should climate model projections be taken seriously given their known imperfections? The response would encompass hindcasts, out-of-sample successful predictions, paleo climate, the resolution of model-observation discrepancies, but also the larger spread of ECS in CMIP6 than the assessed range. [Trigg Talley, United States of America]	Taken into account. The evaluation part/fitness for purpose has been strengthened. However, an FAQ on this topic was already included in AR5 Ch09 on model evaluation.
116371					The chapter uses the term "sensitivity" for various aspects in addition to climate sensitivity (the only one defined in the glossary), in the sense of "the sensitivity of particular quantities to emissions (of CO2, aerosols etc) / to levels of warming". [Valerie Masson-Delmotte, France]	Taken into account. Care has been taken to make the meaning unambiguous.
127635					[PROGRESS] How does the assessment differ from the last cycle? It would be useful for the executive summary and individual sections to provide this kind of information concisely in an easy to find manner. The chapter is very uneven with this sort of information. In some sections it is easy to find. In other sections it is entirely absent. In other sections the information is unevenly mixed throughout the text. [Trigg Talley, United States of America]	Accepted. Opening parts and explicit references to previous reports' assessments have been strengthened.
116373					What is the implication of the statement in chapter 3 that CMIP6 models tend to simulate a too large response to volcanic aerosol for the assessment done in chapter 4? [Valerie Masson-Delmotte, France]	Taken into account; folded into near-term assessment.
115887					FAQ4.1 should be clearer on the global state of the climate system vs regional aspects. The statement about reaching 1.5°C is important but appears disconnected from the text. Surprises could also be discussed (what if major volcanic eruptions). It would be good to show GHG concentrations or RF from scenarios to help readers understand. Why not show initialized projections too in the figure? [Valerie Masson-Delmotte, France]	Taken into account. The revision has striven for a stronger support of the text by the graphics and for greater coherence of the text.
115889					FAQ4.2 check if terms such as "discernable" and "fingerprint" are used consistently x chapters and with the glossary. Check coherency of description of internal variability across FAQs (chaotic processes / modes of variability / ever changing weather). Why the choice of this specific model here (that has a high sensitivity)? I suggest to remove speculation ("might cause substantial communication challenge"). If I understand correctly, the figure shows GSAT from models, but how does the rate of change compare with observations (GMST in SR15 is reported to increase at 0.2°C per decade)? [Valerie Masson-Delmotte, France]	Taken into account. The underlying chapter shows that discernible, detection, and fingerprint are used in their technical and rigorous sense here. We have harmonized the definition of IV across our FAQs. Speculation has been dropped. CamESM5 was used in the SOD because it had the only CMIP6-based large ensemble applied to a comprehensive set of scenarios.
71347					Discussion of teleconections seems fractions. It may be btter to summarize in a Table. [Kenji Taniguchi, Japan]	Taken into account. FGD makes stronger links to Technical Annex.
115891					Please check the use of confidence language in FAQs (I thought that it was not needed, it is quite heterogeneous and some FAS as FAQ4.3 use it). [Valerie Masson-Delmotte, France]	Taken into account. Uncertainty language has been eliminated.

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71349					It may be helpful to include results or descriptions of comparison beteen GSAT and SST [Kenji Taniguchi, Japan]	Rejected. As in AR5, the assessment of projection exclusively uses GSAT. The conversion is handled comprehensively in the much-expanded cross-chapter box 2.3.
5095					The chapter makes use of CMIP6 model data without giving information on which data from which models underlies the findings of this chapter. I suggest to add a reference to Annex III, where information on the contributing models is provided. The selection of MIPs, models, ensembles etc. from CMIP6 should be coordinated with the Ch. 1, which is responsible for Annex III. According to the FAIR guide a data reference should be part of the figure captions, which could be replaced by a reference to a table with data references in case of multiple underlying data sources. [Martina Stockhause, Germany]	Taken into account. Reference to Annex II has been added, as have been figure data tables for all figures.
9703					I understand GSAT projections have been reconstructed from emulators and ECS/TCR ranges estimated in Chapter 7. Has it been verified that a GSAT projection from a climate model whose TCR and ECS are within the very likely ranges of Chapter 7 falls in the very likely range obtained from the emulators? I think it should. Given the complexity of the procedure involved in 4.3.4 this is an indispensible sanity check. [Olivier Boucher, France]	Taken into account. This has indeed been checked and is now mentioned in text.
6639					It needs to be decided whether to remake tables concerning the reaching of the 1.5ºC and 2ºC levels so that these levels use an increase in temperature from pre-industrial to recent past fixed at or near that assumed at the time the Paris Agreement was made, either from the Structured Expert Dialogue that informed the Paris Agreement or from SR1.5. Please see comments 2, 3 and 98. If it is decided not to do this, but use an updated estimate of the warming from 1850-2000 to the recent past, this should not be based on HadCRUT5 alone, but on a set of observational estimates. Please see comments 5 and 100. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The issue is dealt with comprehensively in cross-chapter box 2.3, the results of which have informed the updated assessment performed here.

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6641					A more comprehensive discussion is needed of whether the CMIP6 multi-model mean 0.7ºC temperature increase from 1995-2014 to 2021-2040 is credible. Scaled linearly to a 39-year warming the temperature increase becomes 1.05ºC which is larger than any of the temperature increases based on observations for 1980-2018 shown in Table 2.4, and more than 40% larger than given by all datasets other than HadCRUT5. Please also see comment 132. In addition, the comparison of AR5 and AR6 projections should be made clearer and brought more to the front. One has to wait until page 37 to be told that AR5 projected a warming between 0.3ºC and 0.7ºC from 1986-2005 to 2016-2035. This scales to a range of 0.4ºC-0.91ºC for temperature increase over a 39-year period. This range encompasses the 1980-2018 changes shown in Table 2.4 for all datasets other than HadCRUT5. The CMIP6 models appear to give a short-term warming that on average is some 60% or so higher than given by the CMIP5 models, and it is the CMIP5 models that appear to be closer to reproducing what is implied by the observations made over the past 40 or so years. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The GSAT-change assessment and its use of and juxtaposition to CMIP6 is arguably the centrepiece of the Ch04 assessment, thus dealing with all issues addressed here. The connection to AR5 has been made clearer.