Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54927	0		0		(1) https://www.the-cryosphere-discuss.net/tc-2019-320/tc-2019-320.pdf [Nancy Hamzawi, Canada]	Taken into account and cited.
17377	0		180		Human activities which cause influence on different sectors are highly related to their social and cultural conditions that could be used as an opportunity in adaptation measures. These issues generally rise according to the region and even sub-regions social structures, which should be highlighted in this chapter in one way or another. [Mostafa Jafari, Iran]	Rejected. Adaptation is assessed in WGIII.
14877	0				The paleo context is given for the temperature (3.3.1.1.), for Precipitation, Humidity and Streamflow (3.3.2.), and briefly for Global Monsoon (3.3.3.2.). There are other section where there is not yet such a paleo context. It should be added for Cryosphere (3.4) in particular for Sea Ice (3.4.1.) and Ice Sheets (3.4.3.2.), for Ocean (3.5.) in particular Ocean Temperature (3.5.1.) and Sea Level(3.5.3), for Biosphere (3.6), [Marie-France Loutre, Switzerland]	Taken into account. Paleo assessment has been added to the sections where literature supports an assessment.
21445	0				Almost ubiquitously every figure citation in the text has an erroneous ":" trailing figure numbers e.g. Figure3.2: [Peter Thorne, Ireland]	Taken into account. This has been corrected in the final version.
21447	0				There is a propensity in several places to put figure caption like text describing what the figures are within the main text. Consider trying to remove such occurences. [Peter Thorne, Ireland]	Taken into account. We have considered this in revising the chapter.
21453	0				There are several very long paragraphs extending over in excess of half a page and covering in many cases several topics. These passages would be clearer to the reader if efforts could be made to split them into smaller paragraphs. [Peter Thorne, Ireland]	Taken into account. We have considered this comment in revising the chapter and have tried to shorten paragraphs.
21467	0				Overall the chapter is well written. Occassionally it feels like it reverts to a review whereby each study is covered in turn. I would suggest trying to identify all such cases and really try to redraft so that they are more of a synthesis and assessment and less of a play-by-play review. [Peter Thorne, Ireland]	Taken into account. We have revised the chapter to be more an assessment, less a review.
21499	0				Many, but not all, segments include a brief introductory paragraph which is mainly textbook type material. I noted the first few individually but raise it now as a general point. I'm not convinced that IPCC is the place for such text book style material. Furthermore the heterogeneity across sections as to whether and if so how these introductions are applied somewhat reduces readability. Personally I would tend to remove them. Regardless, I would strive for greater consistency across sections as to whether and if so how they are applied. [Peter Thorne, Ireland]	Taken into account. Textbook style material has been removed.
21501	0				For sections 3.4 and 3.5 (partly) I would expect to see a little more reflection of what relevant findings were made in SROCC. It is SROCC as much as AR5 which should constitute the jump-off points for these sections and yet SROCC is barely mentioned if at all. I would suggest better integrating what was found in SROCC into these sections in the FGD. [Peter Thorne, Ireland]	Taken into account. SROCC now added as a starting point.
21509	0				There are several places, particularly with respect to model assessment aspects where the chapter arguably sails too close to performing new and novel research which is, obviously, outwith of the scope of the report. I have called out a couple of the more obvious examples explicitly. I would urge particular attention be paid to paragraphs of substantive assessment that contain either no references or 1-2 references as being potentially areas where an overeach is being undertaken which may lead to (potentially) unwarranted accusations being levelled. At a minimum if the assessment is using pre-existing methods these should be cited such that it is immediately obvious that this is solely an update on existing work using the new CMIP6 ensemble. I would urge caution to be explicit if this is indeed the case. [Peter Thorne, Ireland]	Taken into account. We have considered this comment as we have revised the chapter, and ensured our assessments are fully based on the literature.

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21535	0				Overall my feeling is that the chapter underutilises assessment of model performance against paleo indicators and where it does there is a lack of systematic effort to use that evidence to build trust in both models and the forcing response. A more systematic and thorough inclusion of the paleo assessment aspects would, on balance, likely strengthen the overall chapter findings. I would suggest systematically using the paleo period to answer questions around i) model performance; ii) forcing response knowledge for as many of the indicators as availability of paleo records permits. It would be useful also be consistent in when and how the paleo evidence is assessed in each case as presently per indicator it comes in at varying points in the narrative. A more common structuring per indicator / mode of variability would aid readability overall. Although Section 3.8.2.1 does assess the paleo periods MH and LGM this doesn't feel like its backed up by prior sections in the way, perhaps, that other parts of 3.8 are. [Peter Thorne, Ireland]	Taken into account. Paleo assessment has been added to the sections where literature supports an assessment.
21569	0				Figures need, in general, significant work to be substantially improved. These figures may well be used shorn of the text and their captions in public outreach events or lectures. Thus they need to be far more self describing. Unnecessary text should be removed. Titles to panels should be self-describing and standalone. Wanting reproducibility is admiral but it is critical that it does not come at the cost of the graphics being able to be used as standalone images. Considerable effort is required to make these graphics sufficiently standalone for use in outreach / instruction / education prior to the FGD. I commented specifically on the first few but ran out of time so only thereafter on egregrious examples where a little effort could pay huge dividends on understandability of the figures. The general comments on those I did comment upon should be applied to all figures. [Peter Thorne, Ireland]	Taken into account. A visual abstract has been added, and the figures have been revised for clarity.
21613	0				Despite my large number of comments, overall I found the chapter an accessible read. Significant efforts are required on the figures to aid readability and very many of my comments are in this direction. [Peter Thorne, Ireland]	Taken into account. Figures have been revised for clarity.
28767	0				Congratualtions on an excellent SOD! Maybe there could be some better clarification of what a "slowdown" is [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Meaning of 'slowdown' now clarified in Cross-Chapter Box 3.1 by better describing the periods over which temperature trends were compared.
32201	0				This Chapter is very well written and illustrated but could be a little more homogeneous regarding the articulation between evaluation and attribution in the different sections. The lack of evaluation of the extended AMIP simulations of GMMIP is regrettable as the capacity of the models to reproduce the trends observed in AMIP mode (SST observed) is an important step in the evaluation. 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. Finally, there is a general lack of discussion of the "scaling factor" in formal attribution studies to determine whether models over- or underestimate observed trends. A summary table of the observed trends underestimated by most models would be good for this chapter, which could also be included in the Technical Summary and/or be the subject of an Cross-Chapter Box. [Eric Brun, France]	Taken into account. We are now assessing extended AMIP simulations in the chapter. Assessment of other GMMIP simulations will be carried out in Chapter 8.
37313	0				Sixty-one papers cited by this chapter are flagged as 'submitted'. Didn't you learm anything from IPCC TAR(1995)? Those papers might have been modified after they were cited by this chapter and might not even be published prior to the IPCC 6AR. Further, how can reviewers comment on the use of those papers if they haven't been published? [John McLean, Australia]	Noted. Only accepted papers are cited in the final draft. Revisions to these papers were considered in our updates to the chapter.

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67835	0				In section 3.5. "Human Influence on the Ocean", it is mentioned that 'anthropogenic forcing' is the cause of changes in several parameters, for example ocean salinity, temperature, sea level rise and others (e.g. on p.35, p.49). There is a need for further clarification in this chapter whether anthropogenic forcing applies equally to all marine parameters studied (ocean salinity, temperature, etc), and the contribution of the anthropogenic forcing (human influence) on these parameters. [Ruandha Agung Sugardiman, Indonesia]	Taken into account. We have refined the text to further clarify the role of anthropogenic forcing in the observed changes.
71885	0				Overall this chapter is appropriate and well worded. However, some sections are in my view inadequate. This relates particularly to the ocean, glacier and ice sheet sections. There is a section at the end of the chapter on model evaluation. But a more important question than have the models improved (as asked in this section) is are the models fit for purpose. This section does not address that as such. I would have thought a more appropriate way to address this question would have been to have this correlation assessment at the start of the chapter, and then address the indivuual phenomena (as in existing sections) addressing both the model adequacy and the human influence question. Also, I was really surprised to find no discussion of climate sensitivity - it remains one of the most important parameters and it needs to be assessed with observations as well as models across the full range of observations. Part of the shortcomings of this paper are likely the result of a narrow authorship list and too large a focus on CMIP. [John Church, Australia]	Taken into account. This is a duplicate comment - see response to Comment 127393.
74345	0				It is clear that human influence has a very strong contribution on the changing in climate, compared to natural forces (e.g. volcanic) [Yulizar Yulizar, Indonesia]	Noted. The chapter addresses this question.
93513	0				The chapter is excellent! No revisions noted. [Rahab KINYANJUI, Kenya]	Noted. Thank you and appreciated.
96247	0				In Ch9, models with unlikely warming trends have been treated separately and were assessed as "cannot be ignored". In Ch3 they are not mentioned - are they part of the multi model mean? [Nicole Wilke, Germany]	Taken into account. Behaviour of individual models with strong historical warming is now assessed in more detail in Section 3.3.
102787	0				In this chapter (92 pages) only 4 pages deal with the biosphere. This is disproportionately low, considering the massive human impact on the biosphere, which is an integral part the climate system (see definition of UNFCCC) fulfilling a unique regulatory function. The SR1.5, SROCC and SRCCL have demonstrated well this impact. This should also be recognised in AR6 [Philippe Tulkens, Belgium]	Noted. This is recognised, with a whole chapter focussed on the carbon cycle and biosphere (Chapter 5), which we refer to in our section on the biosphere.
114735	0				The FAQs are useful with relevant figures for outreach. Figure FAQ 3.2,Figure 1 could perhaps be lifted to be more visible? [Jan Fuglestvedt, Norway]	Taken into account. A version is included in the TS.
12515	1	1	1	21	The chapter read well and thanks authors for their efforts. The overall comment is the inequality of CA list: all CAs are from developed countries(100%), especially UK (11 authors out of 30), 7 from US. This is not good for IPCC, which seeks balance and equaliy. [Lijing Cheng, China]	Taken into account. We have improved the geographical balance of the CA team.
79541	1	1	120	20	It seems that it would be better to bring the figures in the text as before for better understanding, or only indicate the figure`s number in the text and not to write the subtitle so just refer to the end of the text.When the figures in the text is not sshown, there is no need to write subtitles in the text (comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Duplicate comment. See response to 79477.
77209	1	1	180	1	This is a very important chapter but some development is needed [Emer Griffin, Ireland]	Taken into account. We have further developed the chapter in response to the final round of reviews.
102789	1	1	180	1	Throughout the text is very dense, which is not a short-coming. At times, however, the authors could improve the flow by streamlining the logical flow and using less complex syntax and clause structures. [Philippe Tulkens, Belgium]	Accepted. We have tried to simplify the text where possible and use simpler syntax.

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102791	1	1	180	1	Most sections have a clear structure, explaining first the issue at hand, sumamrizing relevant parts of AR5, summarizing recent developments, and finally summarizing and assessing the evidence. This should be done fully consistently in all subsections. [Philippe Tulkens, Belgium]	Taken into account. We have considered this in revising the chapter, and aimed to use the same structure for each section.
102793	1	1	180	1	Clouds are mentioned briefly in some subsections, but there is no summary assessment of how well models deal with cloud and cloud-aerosol interactions. [Philippe Tulkens, Belgium]	Noted. Clouds are not one of the large-scale indicators assessed in Chapter 2-4 (Cross-chapter box 2.1), and they are assessed in more detail in Chapter 7.
102795	1	1	180	1	Various technical terms related to modeling performance (skill, error, fidelity; but also capable, difficulty in reporducing, ability to represent (p.59) etc) are being used throughout this chapter. It may help the reader to briefly explain these, and to use these fully consistently. [Philippe Tulkens, Belgium]	Taken into account. We have revised to avoid using 'skill'. Other terms are standard English phrases, rather than technical terms.
83371	1	1	180	7	There is no discussion or coverage of change in the thickness of both Arctic and Antarctic sea ice, a key factor. [Robert Massom, Australia]	Noted. Sea ice thickness is assessed in Chapter 9, Section 9.3.
79543	1	1	180	20	In adddition of the text, the refrences should be indicate in their figures`s subtitles.(comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Duplicate comment. See response to 79479.
32665	1	1	180	55	It seems that it would be better to bring the figures in the text as before for better understanding, or only indicate the figure`s number in the text and not to write the subtitle so just refer to the end of the text.When the figures in the text is not sshown, there is no need to write subtitles in the text [sadegh zeyaeyan, Iran]	Duplicate comment. See response to 79477.
32667	1	1	180	55	In adddition of the text, the refrences should be indicate in their figures`s subtitles [sadegh zeyaeyan, Iran]	Duplicate comment. See response to 79479.
32685	1	1	180	55	human activities wich cause influence on different sectors are highly related to their social and cultural conditions that could be used as an opportunity in adaptation measures. These issues generally rise according to the region and even sub - regions social structures, wich should be highlighted in this chapter in one way or another. [sadegh zeyaeyan, Iran]	Duplicate comment. See response to 17377
32995	1	1	180	55	It seems that it would be better to bring the figures in the text as before for better understanding, or only indicate the figure`s number in the text and not to write the subtitle so just refer to the end of the text.When the figures in the text is not sshown, there is no need to write subtitles in the text [Sahar Tajbakhsh Mosalman, Iran]	Duplicate comment. See response to 79477.
32997	1	1	180	55	In adddition of the text, the refrences should be indicate in their figures`s subtitles [Sahar Tajbakhsh Mosalman, Iran]	Duplicate comment. See response to 79479.
33015	1	1	180	55	human activities wich cause influence on different sectors are highly related to their social and cultural conditions that could be used as an opportunity in adaptation measures. These issues generally rise according to the region and even sub - regions social structures, wich should be highlighted in this chapter in one way or another. [Sahar Tajbakhsh Mosalman, Iran]	Duplicate comment. See response to 17377

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10557	1	1			Whole chapter: I fear the authors have really tied themselves up in knots regarding the issue with "GMST" v "GSAT". Chapter 2 seem to suggest that an adjustment should be applied to observations to account for the estimated differences in trends between "GMST" and "GSAT", and indeed that seems to be headed in chapter 7 say. So why complicate the issue in this chapter? The nuances of how to apply an adjustment are also not considered, which given how few studies have explored the issue, is perhaps not surprising. I have recently looked at this issue and deduced that there is some over confident reasoning in the importance of the difference between "GMST" and "GSAT" in models (Jones, `Apples and oranges': on comparing near surface temperatures from climate models with observations, submitted Q.J.R.Meteorol. Soc., 2019). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Cross-chapter Box 2.3 has been revised, and the new assessment is that the best estimate of the scaling factor between GMST and GSAT is 1. Chapter 3 has been revised to reflect this updated assessment.
2077	1	18	1	18	Dan Lunt should be "Daniel Lunt" [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed as suggested.
40033	1		1		Consider defining 'human influence' in the glossary [TSU WGI, France]	Taken into account. 'Human influence on the climate system' is now defined in the glossary.
112945	1		1		It would be good to include some elemnts of model performance and evaluation from paleo data-model comparisons, and specifialy where we need to lean on paleodata for some critical detection work. This is very obvious in areas like ENSO (models have some structural challenges, but we have some great new data to lean on), but also in drought, which is written deeply into the richest and most abundant soruces of paleo-data archives, via PDSI reconstructiojs, etc. There is also an opportunity to assess model performance vis a vis the representation of low-frequency variability (dec-cen) variability - as that can only be assessed with a paleo-perspetive, and there is some existing literature in that category. In other words, there are some key heavy-lifting areas for paleodata and model research to plug in, and it would be good to work with the paleo BOG membership to identify areas where key messages can be strengthened, and where some additional key messages can be highlighted that woudln't otehrwise be possible without a paleo perspective. [Kim Cobb, United States of America]	Taken into account. Paleo assessment has been added to the sections where literature supports an assessment.
110851	1		150		The chapter is very impressive in depth and breadth and really solid, congratulations. The merge of model evaluation and attribution of changes to causes has worked very well. Congratulations to the authors [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thanks and appreciated.
108081	1		180		No Comments [Asylbek Aidaraliev, Kyrgyzstan]	Noted.

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68853	1				Paleoclimate information has been successfully distributed across the WG1 report, as envisaged by the scoping documents. The Paleo BOG has now developed key messages to consolidate and convey the most policy-relevant paleoclimate content, and to advance it to the summary documents (TS & SPM). The Paleo BOG looks to CH3 to include critical information needed to address three of the key messages from paleoclimate and to include the outcome of the assessment in its Executive Summary, namely: Paleo key message I. (model veracity) How well do Earth-system models with paleoclimate forcings simulate large-scale Earth system changes? Justification: Models are the basis for detailed climate projections of future climate and its impacts. Future climate conditions are beyond the range of human observations; therefore, past climate enables an evaluation of model performance under forcings that may be of similar magnitude to future climate change. (cont.) [Darrell Kaufman, United States of America]	Taken into account. Additional paleo assessment has been added to Chapter 3, based in part on input from the paleo BOG.
68855	1				In support of this key message, please provide a quantitative assessment of GMST simulated by paleoclimate models for each of the key paleo reference periods for which observational evidence for GMST has been assessed in CH2. This information is needed to complete Table TS.9, which compares simulated with observed GMST, and will be the basis for a new summary figure in the TS. The following are suggested rough-draft ES statements needed to reinforce the conclusions in the TS : (1) Global temperature changes simulated by climate models for multiple paleo reference periods do not overestimate the actual temperatures (xxx confidence). CMIP6 models that simulate the effects of a variety of climate forcings show that simulated GMAT agrees with the indirect (proxy) evidence for former GMST, with an average absolute difference of 1.1°C among four paleo periods that span about 9°C (xxx confidence). (2) Paleoclimate models correctly simulate the full spectrum of global mean temperature variability (eg., www.pnas.org/cgi/doi/10.1073/pnas.1809959116). (3) Climate models do not include all existing Earth-system feedbacks and might misrepresent key processes needed to correctly simulate the dynamics of climate change on long timescales (cf. SR1.5; Fischer et al. 2018). (4) Paleoclimate models with realistic volcanic and solar forcing for the last millennium show xx relative to proxy evidence - or anything signficant about transient climate simulations for tha past 1000 years. What is new since AR5? (cont.) [Darrell Kaufman, United States of America]	Taken into account. Additional paleo assessment has been added to Chapter 3, based in part on input from the paleo BOG. Assessment of paleo-temperature (temperatures of the warmest and coldest months) has been added to Section 3.8.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68857	1				Paleo key message II. (prominent patterns) What are the prominent large-scale, recurrent spatial patterns associated with past global changes? Justification: Global changes are associated with large-scale regional trends and patterns that are quantitatively robust and therefore provide predictive capacity independently of climate models. In support of this key message, please provide a quantitative assessment land-sea contrast, polar amplification, tropical Pacific gradients simulated by paleoclimate models for each of the key paleo reference periods. These are to be compared with observational evidence as assessed in several chapters to address the first paleo key question. Land vs sea temperatures were reported for multiple paleo reference periods in AR5 (Box 5.1). This metric is important to support the key message about prominent characteristic patterns, and because it is the basis for proxy-based reconstructions of GMST: (1) GMST estimates for pre-LGM reference periods are primarily based on marine proxies scaled to global MAT. For the LIG and MPWP, Fischer et al. (2018) applied a scaling factor of 1.6 to convert global SST to global GMST. The scalar was based on warm interglacials as calculated by Snyder (2016, 10.1038/nature19798). More recent analysis indicates that a factor of 1.6 is too high. (2) GMST estimates from boreholes, which are used to compare with 2k temperatures (Figure SPM.4) are based on land temperatures scaled to global. Cuestra-Valero et al. (in review, JGR) used land vs ocean scaler of 2.36, which is based on Harrison et al. (2015, 10.1038/NCLIMATE2649) ensemble of paleoclimate simulations (below). Is this the best value? Polar amplification is important to support the key message about prominent recurring patterns, and is a prime target for data-model comparison. CH7 SOD includes qualitative information on Arctic amplification for the Eocene and Pliocene. We need to coordinate the assessment and presentation of polar amplification across the WG1 report. (cont.) [Darrell Kaufman, United States of America]	Taken into account. Additional paleo assessment has been added to Chapter 3, based in part on input from the paleo BOG. Land-sea contrast in paleo simulations is assessed.
68859	1				Paleo key message III. (natural variability) What is the role of natural (forced and unforced) climate variability, including extreme events, abrupt changes, climate modes, and volcanic eruptions? Justification: Accurate climate projections must account for internal variability and thresholds in the climate system. Detecting and attributing the human impact on climate requires quantification of natural variability. The following are suggested rough-draft ES statements needed to reinforce the conclusions in the TS: Analysis of residuals to multidecadal-timescale paleoclimatic detection and attribution studies, estimation of the unforced variability in climate simulations, and estimation of reconstructed variability from low forcing intervals of the pre-anthropogenic past millennium are roughly consistent, and suggest the unforced variability is about 0.03-0.04oC. Formal and informal paleoclimatic D&A studies suggest that decadal-centennial scale surface temperature variability in the pre-anthropogenic past millennium is most consistent with forcing by stochastic clusters of explosive volcanic events (e.g., doi: 10.1038/s41561-019-0400-0) [Darrell Kaufman, United States of America]	Taken into account. Additional paleo assessment has been added to Chapter 3, based in part on input from the paleo BOG. Figure of past1000yr simulations added, which is relevant to assessment of variability.

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68861	1				Please move the regional precipitation information from paleoclimate models and data to CH8. The RE for CH8 (Pascale Braconnot) is a leader in the PMIP community and specializes in regional precipitation simulations in paleo models. Her expertise is needed to oversee the review of this topic. In addition, moving regional information to other chapters will make room for new text to address the essential comparison between paleoclimate models and “large scale” (continential or larger; CCB 2.2) climate indicators that were selected by CH2-CH3-CH4, including GMST, which is a high priority topic for policy. How well do models simulate GMST, polar amplification and land-sea contrasts under different forcing? Thank you. [Darrell Kaufman, United States of America]	Rejected. This was discussed with Chapter 8, and it was decided to keep paleo assessment of model evaluation of rainfall in Chapter 3, even though some records are limited to particular regions. As paleo records of precipitation are by necessity regional, we have rewritten the text so that the regions considered are all within the subtropics, so that we can say something on larger scales, not on individual regions. Moreover, the figure from the Mid-Holocene was re-drawn so that it now presents changes in latitudinal gradients of precipitation, and does not focus on individual regions.
52833	2	1	3	21	Nice and comprehensive table of contents. Could me more homogeneous about the distinction between evaluation and attribution (sometimes within the same subsection, sometimes in separate subsections, sections 3.8.1 and 3.8.2 could be reversed). No use of the extended-AMIP experiments from GMMIP which however represent a new and valuable archive for model evaluation. [Hervé Douville, France]	Taken into account. AMIP simulations are now assessed in the chapter. Evaluation assessment has been revised to focus on fitness for purpose for attribution, in order to better clarify links between attribution and evaluation.
109675	2	7	2	45	Using section titles of the form, "Human influences on...", is clunky and could stand to be improved. Obviously, human influences on many of these things (atmosphere, biosphere, etc) include - but also extend far beyond - global anthropogenic climate change. This is important for maintaining credibility of the report with scientists working primarily in those fields (air quality scientists, fisheries ecologists, etc). A more carefully considered word choice would be prudent. [Sean Fleming, United States of America]	Taken into account. Text clarifying that we focus on the effects of human-induced climate change has been added to the introduction.
110853	2	20			Methods discussed in that section are much broader than optimal fingerprinting - I would at least kick the word 'optimal'. Or merge all the methods as there is nothing fundamentally different about optimal fingerprinting - we want methods that follow the attribution guidance, but beyond that it depends on the problem. the text in that section even mentions that not all approaches optimize. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
130479	2	31	2	34	Too frequent using "evaluation" in sub-section titles. I suggest to delete them in the sub-section titles. [Panmao Zhai, China]	Accepted. Subsection titles of 3.5.2 and 3.5.3 using 'evaluation' deleted.
32857	3	3	3	4	Rephrase and shorten to: This Chapter assesses the extent of human influence on climate system evolution and the ability of existing climate models to simulate observed changes and variability. [Aaron Werikhe, Uganda]	Rejected. We focus on the climate response to human influence, not the human influences themselves.

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98027	3	5	5	15	I think this summary could be improved upon for regional precipitation Detection and Attribution, because it is focused too much on shorter term changes (e.g., since about 1980 on) where it is difficult to find detectable signals over land, while not mentioning several longer-term trends cases (since 1901 or since 1951) that have more robust detectable trends, at least in some regions. Thus, while high latitude NH moistening is appropriately recognized, it could also be emphasized here that there is medium confidence for a detectable anthropogenic decrease in precipitation over the Mediterranean region and for several counterpart regions in the southern hemisphere subtropics. Moistening in southeast South America could also be highlighted. These features are more robust from a regional detection/attribution standpoint than the tropical changes currently being highlighted in this summary. While Knutson and Zeng (2018 Fig. 5c) show a few regions of tropical Africa with detectable moistening trends over 1981-2010, most such shorter term trends over tropical land regions are still indistinguishable from natural variability, and also not robust from a longer-term trend perspective. For example, In contrast to the notion of increased rainfall over tropical wet regions due to GHG increases, longer-term trends (e.g. 1951-2010) in observed precipitation in “tropical wet regions” actually show mixture of significant increases, decreases, and nonsignificant changes (e.g., Knutson and Zeng 2018, Fig. 4). As mentioned in the main text of CH. 3, apparently, tropical precipitation trends are complicated by competing effects of aerosols and greenhouse gases), but the shorter the record being analyzed, the greater the potential for multidecadal variability to obscure any forced signal even more at the regional scale. On the other hand, if one wants to focus on tropical wetting trends, the clearest long-term signal of detectable anthropogenic increasing tropical wet-region precipitation is probably for northern Australia (Knutson and Zeng 2018, Fig. 3c, 4c, 5c). Also the summary on p. 3-32 is a better indicator of how uncertain the precipitation D&A picture is for the tropics and monsoonal regions. [Thomas Knutson, United States of America]	Rejected. Chapter 3 focuses on continental and larger scales.
130611	3	7	3	7	Please consider change "Earth System" to "Climate System". [Panmao Zhai, China]	Rejected. 'Earth system' better conveys atmosphere/ocean/cryosphere/biosphere, so we prefer to keep this.
127205	3	11	3	50	Surface temperatures over land have the greatest certainty. There are still disagreements about surface temperatures over oceans, especially at high latitudes. The leading paragraph should say that climates of the last XX million years span the entirety of observed and projected surface temperatures. "external forcings" is too vague and mentioned twice. For the model side, the implementation of the forcings was very simple. Considerable improvements have been made within each model group since PMIP3 that have improved the way forcings are put into the models. Volcanoes are treated better. Solar has been explored better with attendant ozone changes. This section is too much hand waving. Moreover, many colleagues who research ice cores will be dissatisfied with this section. No mention of confounding influence of ozone or earth system components. Authors need to get one of the GCM groups who did these experiments to actually write a few sentences here. [Trigg Talley, United States of America]	Taken into account. (Assuming that the comment actually refers to page 11.) Relevant content from the paleo context section has been merged with the model evaluation section, and a Figure 3.1 has been revised to compare reconstructed and simulated temperatures over the past 1000 years, with also a plot of the forcings driving the simulations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7509	3	46	3	49	Why are you using 5-95% as a validation of the model? It is easy to be plus or minus two standard deviations away from a mean. This is a low bar to state that you have "high confidence". To reiterate my above comment, this type of reporting is unorthodox for us experts, and is confusing to the general public. The "pause" was barely captured by the models but the public would miss that due the chosen wording. [Hugh Lefcort, United States of America]	Noted. 5-95% uncertainty ranges are standard across the WGI report.
37237	4	1	4	1	It did not identify anything. It purported to identify. The paper, written largely by IPCC authors desperate for some material to support their argument, probably encouraged by IPCC mandarins to avoid the organisation being repalced by the UNFCCC's TSB, concocted a mathematical approach to claim a human influence on climate. The paper was published more than 12 months after the IPCC report that cited it and it was swiftly decalred rubbish. AR5 tried to cling to a similar line that you have here and it was nonsense back then too. [John McLean, Australia]	Taken into account. Bullet has been rephrased and 'identified' is no longer used.
99093	4	1	4	1	How can it be that this Executive Summary does not seem to mention the very deterioating states of the Greenland and Antarctic ice sheets. Their accelerating loss of mass is assuredly due to human-induced climate change and there is a strongly increasing risk of commitment to very significant sea level rise, and yet this risk is not even considered in the summary. [Michael MacCracken, United States of America]	Taken into account. An assessment statement on causes of melting/mass loss from Greenland and Antarctic ice sheets has been added.
96249	4	1	5	55	A statement about the assessment of progress between CMIP5 and CMIP6 models is missing, stating which relevant quantities showed improvement and which not. Alternatively please refer to Ch4. Please see also our comment in the Entire Report on the lack of consistency across chapters. [Nicole Wilke, Germany]	Taken into account. A statement relating the mean performance of the CMIP5 and CMIP6 models for most large-scale indicators is included in the final bullet of our ES. And the performance of CMIP5 and CMIP6 for many individual indicators is described in other ES bullets.
21435	4	1			There was no mention as far as I could tell relating to paleo records either with regard to causes of deep past changes or the use of deep past changes to build confidence in the climate models. This seems like a substantial oversight and I would urge better inclusion of insights based upon paleo evidence, where appropriate, in the FGD ES text. This may well need to cascade to better consideration of paleo evidence in the main body text. [Peter Thorne, Ireland]	Taken into account. A statement on paleo model evaluation has been added to the ES.
11295	4	3	4	8	The first ES bulet gives a good summary of this Chapter bryond AR5. very nice! [Masahiro Watanabe, Japan]	Noted. Thanks.
24115	4	3	4	8	From going through the headline findings I can see that there is a strengthening of the attribution statements on precipitation, Arctic sea ice, ocean heat content, hot and cold extremes. So this first paragraph refers to the confidence with which we can attribute some human influence on climate. But it isn't clear to me of the value of the statement "This evidence is now even stronger" when it doesnt also refer to the crucial quantification aspects of attribution and when we already had "human influence on the climate system is clear" from last time. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This statement considers evidence for human influence across the climate system, and taken as a whole it is correct that the evidence has strengthened.
52945	4	3	4	8	I would suggest to rephrase this paragraph, which could start with the following statement in bold (followed by a brief history of related statments since the second AR): "Human's influence on recent climate change is well established and increasingly well documented (or supported?) from one IPCC Assessment Report to the next." [Hervé Douville, France]	Taken into account. The paragraph has been re-phrased, and now leads with a summary of how the evidence for human influence has changed through IPCC assessment cycles.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127207	4	3	4	15	[PROGRESS] Suggest placing the second executive summary paragraph above the first paragraph. On line 11, it is unclear what "increases" is referring to (since AR5?). This sentence could be more declarative and less passive, so it doesn't sound like this is just something that could happen, but the authors' definitive conclusion based on the work they did. In the first executive summary paragraph (lines 3-8), the conclusion is in the last sentence but should be placed up front (e.g., "The evidence of human influence on the climate system is even stronger than it was in AR5, which concluded..." [Trigg Talley, United States of America]	Taken into account. In the first paragraph, the statement that the evidence for human influence is now even stronger has been moved to the first sentence as suggested. 'increases the level of confidence' refers to the effect of combining evidence from across the climate system, as explained. We prefer to keep the paragraph on historical context first, as an introduction.
98815	4	5	4	5	[...] the evidence of human influence in the climate system has been progressively strengthened. The AR5 concluded that the human influence on the climate system is clear, evident by the increasing in concentrations of greenhouse gases in the atmosphere, positive radiative forcing, observed warming and physical understanding of [...] [Julio Cesar Barreto da Silva, Brazil]	Taken into account. This bullet has been re-written to improve clarity and lead off with the statement that the evidence for human influence on climate is now even stronger.
18691	4	7	4	7	"positive radiative forcing" cannot be measured. Should this be changed "positive energy imbalance"? [Govindasamy Bala, India]	Rejected. This is a direct quote from the SPM of the AR5 WGI report.
17591	4	8	4	8	Statement "evidence is now even stronger" is not justified because of increasing uncertainties indicated in relevant literature. Example Hegerl(Clim.Change March 2018) : "discussion on hiatus reveals that decadal variability in the large scale climate is stil poorly understood." Also the increased knowledge about natural variability as possible significant driver of decadal, centennial and millennial temperature changes is not supporting this statement [ferdinand meeus, Belgium]	Rejected. While we agree that there are remaining uncertainties, as discussed in the chapter, on balance the evidence is stronger.
39163	4	8			No uncertainty language? [Lourdes Tibig, Philippines]	Noted. This text is discussing how the evidence for human influence on climate has changed, and doesn't need an uncertainty qualifier.
24117	4	10	4	10	This "virtually certain" statement sounds if anything a bit of a winding back from "Human influence on the climate system is clear" statement. Even if interpreted as an attribution statement on temperature changes only this is a strong confidence statement on a low-bar attribution, ie that human influence has had a greather than non-zero contribution. Again I don't quite see the value of this statement. The key one it seems to me is the statement quantifying some substantial contribution of human influence. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have strengthened the statement so it is now factual.
77211	4	10	4	10	The AR5 statement that the Human influence is clear can be used as its stronger see line 6 of this page. [Emer Griffin, Ireland]	Accepted, We have strengthened the statement so it is now factual.
96251	4	10	4	10	This formulation is weaker than the AR5 headline: "Human influence on the climate system is clear. This is evident from ..., observed warming, and ..". AR5 thought that for this statement no uncertainty language is necessary because it is a fact. 3-4-8- states "This evidence is now even stronger". Then why does AR6 in 3-4-10 provide an uncertainty statement? [Nicole Wilke, Germany]	Accepted. We have strengthened the statement so it is now factual.
98817	4	10	4	10	Human influence is almost certain to have warmed the global climate system. The combination of evidence from the entire climate system increases the level of confidence in the attribution of observed [...] [Julio Cesar Barreto da Silva, Brazil]	Taken into account. We have strengthened the statement so it is now factual.
99829	4	10	4	10	The first sentence of this ES statement could be read as contradictory with the previous ES statement and AR5. Are we not certain that human influence has warmed the climate system to some degree? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have strengthened the statement so it is now factual.
132111	4	10	4	10	Is "virtually certain" needed? Isn't this just a statement of fact, i.e. 100% certainty? [Sonia Seneviratne, Switzerland]	Accepted. We have strengthened the statement so it is now factual.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132119	4	10	4	10	On a similar note: The SPM states that "... it is now an established fact in AR6 that human activity has altered the climate system since the mid-20th century". For traceability, this exact sentence should be in the chapter 3 ES. [Sonia Seneviratne, Switzerland]	Accepted. We have strengthened the statement so it is now factual. Perhaps a verbatim repetition of the SPM formulation is not necessary.
98919	4	10	4	13	I don't understand why "virtually certain" is needed here--it is really unequivocal. In the IPCC lexicon, "virtually certain" means greater than 99 out of 100 likelihood. Somehow, by using this, it would seem some gradation in confidence exists between getting to 99 out of 100 and it just being clear and unequivocal, and I just don't understand, nor do I think policy makers will understand, how this gradation is determined and what its basis might be. I don't know of any other plausible explanation supported by any evidence at all, so why include the phrase. Simply say the only plausible explanation for the warming over the last century is human activities, specifically the emissions of CO2 and other GHGs [and then perhaps say--very high confidence or something, but "virtually certain" just not seem logically to fit. And then somehow lines 11-13 say that there is additional evidence, and somehow one does not get to certainty? If indeed the statistical analysis is only giving three sigma or so, then I presume the problem is that the denominator in the signal to noise is simply not well defined due to the shortness of the record--but I think that is a poor excuse for not just stating that there is no doubt that the net effect of human activities has been to warm the climate. [Michael MacCracken, United States of America]	Accepted. We have strengthened the statement so it is now factual.
21429	4	10	4	15	This is a much improved formulation but I remain unconvinced that this cannot be made as a statement of fact. Is it conceivable that all evidence be so wrong as to call into question this finding? I personally do not see how so many studies looking at so many variables and using so many simulations could all be so wrong as to call a bottom line finding of humans causing the warming being called into question even at the 1% level implied. [Peter Thorne, Ireland]	Accepted. We have strengthened the statement so it is now factual.
77213	4	10	4	15	This can be clearer and shorter. [Emer Griffin, Ireland]	Accepted. We have strengthened the statement so it is now factual.
112643	4	10	4	15	Probably would be better if it goes straigtht to the point by saying "It is ceretain", without "virutally". [Melissa Jiménez Gómez Tagle, Germany]	Accepted. We have strengthened the statement so it is now factual.
10869	4	11	4	14	This attribution is given with too much confidence. There have been (too) many studies trying to attribute the causes of a 15 year trend in observed temperatures, with many different conclusions. This seems to just reflect the opinion of the lead authors of this chapter rather than of the literature as a whole. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The statement does not talk about 15-year trends but about (now explicitly) warming since preindustrial times of the climate system, for which the only viable and straightforward explanation is human influence. We have dropped the uncertainty language in response to other reviewer comments and have added "since preindustrial times" to make clear this is not a decadal feature for which indeed sometimes conflicting explanations can exist.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37243	4	12	4	16	You are being quite deceitful. IPCC reports have made those claims but have not had the evidence to support them. The evidence in every report changes, usually because the evidence in previous reports has been shown to be false (e.g. the discredited Hockey Stick temperature graph). IPCC AR5 mentione dthe increase in CO2 over the 15 years prior to the report's drafting and buried deep in that report we find "... the rate of warming over the past 15 years (1998–2012; 0.05 [–0.05 to 0.15] °C per decade) ... is smaller than the rate calculated since 1951 (1951–2012; 0.12 [0.08 to 0.14] °C per decade)." [WG I SPM, page 5, section B.1, bullet point 3, and in full Synthesis Report on page SYR-6]. Despite the increase in CO2 there was no warming and that report and again now you claim it is "extremelyt likely" that mankind has caused warming. I think you should be very embarrassed. [John McLean, Australia]	Rejected. This comment actually refers to lines 17-31 containing an "extremely likely" statement. The reviewer makes a sweeping allegation of deceit which is simply incorrect. Yes, AR5 found that a "hiatus" in GMST increase had happened. Despite this being seized upon by critics as evidence of "deceit", it is not. It is just evidence of the existence of climate variability; this is well explained elsewhere in the report. Regarding the "hiatus", AR6 shows that for ocean heat content (which is the leading reservoir of the excessive heat in the climate system trapped by anthropogenic greenhouse gases) the hiatus never happened. For GMST it has now reversed. So no deceit.
34587	4	13	4	13	Should this sentence include some mention of the biosphere as well? [Russell Vose, United States of America]	Noted. It certainly could. We prefer to only mention atmosphere, ocean, and cryosphere, where the anthropogenic influence manifests in simple physical terms, whereas for the biosphere it is more complex with species habitat migration, shifting of seasons etc which are out of scope for this chapter.
10567	4	14	4	15	One would not expect the multi model mean to reproduce the observations perfectly. Variations due to internal variability on different time-scales won't be captured by a multi-model mean, e.g., El-Ninos. The best an analysis could do is look at the consistency of the ensemble spread compared with the observations. It is common to consider the observations as one realisation of a hypothetical ensemble of observations from alternative worlds where the weather/climate noise are being sampled differently. So an assessment should be based on the ensemble spread (an ensemble of opportunity as well) not the multi-model mean alone. e.g, Hegerl et al IPCC, 2007; von Storch and Zwiers, Climatic Change, 2013; Bindoff, IPCC, 2013. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These comments are all correct. Is however unclear how they relate to the ES statement, or what changes the reviewer would like to see. For the large-scale indicators and long-term trends discussed here, variability does not seem to play a substantial role, e.g. the slow sea level rise, century-scale retreat of glaciers, loss of Arctic sea ice, continental- and century-scale temperature increases, etc. For example, even large multi-model ensembles do not contain model simulations that show net global cooling since 1850.
9651	4	17	4	18	The 2010-2019 period is different from the 1995-2014 reference period used in Chapter 4. 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]	Rejected. Chapters 2 and 3 use the same reference period, but Chapter 4 needs to use an earlier period because of CMIP6 simulations. This is clarified in Chapter 1. Differences in assessed periods are not ideal, but difficult to avoid.
26683	4	17	4	18	Reference period should be harmonised between chapters and between chapters and the SPM. It might be confusing. Here the 2010-2019 period is different from the 1995-2014 reference period used in Chapter 4. [Eric Brun, France]	Rejected. Chapters 2 and 3 use the same reference period, but Chapter 4 needs to use an earlier period because of CMIP6 simulations. This is clarified in Chapter 1. Differences in assessed periods are not ideal, but difficult to avoid.
10561	4	17	4	19	It is good to have an assessment of the warming since the 19th century in an IPCC report finally, but can we also have an equivalent statement for the shorter period starting in 1950s? Just to enable some sort of comparison with previous AR assessments. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Warming over periods other than 1850-1900 to 2010-2019 is now assessed in the main text, in support of Chapter 5 Box 5.1. However, we do not wish to complicate the ES further.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17593	4	17	4	19	Contradicts with the observed °t numbers. Total °t increase since pre-industrial is about 1°C. Since 1950 °t increase is about 0,6°C, of which about 50% is human-caused. According to AR5 causes of °t increase since pre-industrial till 1950 is not significantly human-caused. So the human-caused °t increase numbers do not add up to the observed 1°C. Also Hegerl (Clim.Change March 2018) "about 50% warming from 1901 to 1950 was forced by combination natural/human" , no exact % for natural/human given. [ferdinand meeus, Belgium]	Rejected. The reviewer misunderstood the "half is human-caused" statement. This refers to the fraction that can be attributed as extremely likely. Most, or all, of observed warming is human-caused, but at a lower confidence level.
17595	4	17	4	19	"extremely likely" is not justified. At best "likely" . See Hegerl (Clim.Change March 2018) statement "exact contribution of each factor to large scale warming remains uncertain, largely due to uncertainty in the role of aerosols in the cooling or stabilization of climate following the middle of the 20th century" [ferdinand meeus, Belgium]	Rejected. The reviewer confuses attribution to specific anthropogenic forcing (well-mixed greenhouse gases, aerosols) with attribution to anthropogenic forcing overall. However, the "extremely likely" statement has been removed in favour of a clearer assessment.
99831	4	17	4	19	Agree with the shift to assess the total change since 1850-1900 rather than 1950 as in previous ARs. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
77215	4	17	4	21	This can be clearer and shorter. Is the reference to the AR5 needed here? [Emer Griffin, Ireland]	Taken into account. The opening of the paragraph has been rewritten. Reference to AR5 is now made in the context of progress made since its publication.
109323	4	17	4	23	The second sentence mentions "the high level of confidence," but the first sentence only gives likelihood statements. Add "(high confidence)" at the end of the first sentence? Or replace "The high level of confidence comes from…" with "High confidence in this assesssment comes from..."? [Paul Edwards, United States of America]	Accepted. The paragraph does not refer to confidence any more, and uses likelihood throughout.
1817	4	17	4	28	I find it very confusing that you use both GSAT and GMST. You do not clearly explain what the difference is. Is the first 2 m air temperature and the second the longwave radiative temperature of the surface? In any case, they tell the same story. Cross-chapter box 2.3 does explain the difference, but then says that there is a decision to "use GSAT as the primary metric of surface temperature changes in this report " and presents conversion factors. (But I only found this later, and readers of this summary will not necessarily have seen it.) FAQ 1.4 just mentions "Global surface temperature." Having two different ones here will just confuse readers. Stick to GSAT, which is the metric that has been used in all past assessements, and is what is actually observed at weather stations, and has been chosed to be the metric. I understand that it is a derived value from climate models, as there is not actually a vertical layer at 2 m, but we know how to deal with that. [Alan Robock, United States of America]	Taken into account. Chapter 2 has now re-assessed the difference between GSAT and GMST-based trends, and the best estimate of their scaling is now 1. The statement now states that GMST numbers are equal to GSAT numbers, and only gives the latter.
19751	4	17	4	28	It is understood that the discussion concerning GSAT against GMST is quite important for climate scientists and hence the WG1 community. Probably the interest of policymakers for this issue is not so keen. [philippe waldteufel, France]	Taken into account. Chapter 2 has now re-assessed the difference between GSAT and GMST-based trends, and the best estimate of their scaling is now 1. The statement now states that GMST numbers are equal to GSAT numbers, and only gives the latter.
24121	4	17	4	31	I think it would be of interest to provide the natural contribution as well (like was done in ar5). [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The natural contribution is now given.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65659	4	17	4	31	Suggest including a greater acknowledgement of uncertainties in aerosol forcing, and model biases in representing aerosols role in radiative forcing. This discussion is covered on Pages 13, 16 and 17 and should be reflected in the Executive Summary. Page 17 in particular notes "The warming driven by greenhouse gas increases is offset in part by cooling due to other anthropogenic forcing agents, mostly aerosols, although confidence in that attribution is lower than attributing to all anthropogenic drivers". Suggest the Executive Summary cover uncertainties more thoroughly, particularly those regarding aerosols. [Kushla Munro, Australia]	Taken into account. The paragraph now states that attribution to specific anthropogenic forcings is more uncertain than to anthropogenic forcing in general.
102797	4	17	4	31	Readibility could be improved by splitting in two: one para on GSAT, one on GMST - instead of hopping between the two. It may also be useful to write a sentence why they could be different. [Philippe Tulkens, Belgium]	Taken into account. Chapter 2 (Cross-Chapter Box 2.3) has now re-assessed the difference between GSAT and GMST-based trends, and the best estimate of their scaling is now 1. The statement now states that GMST best estimates are equal to GSAT best estimates, and only gives the latter.
114731	4	17	4	31	While Ch2 first gives the temperature trend in GMST and then in GSAT, the oposite order is used here. I think some coordination in use and weight to the two metrcis would help the reader. (It may seem trivial, but same structure in presenting all this info will be helpful). And I suggest you split this para into two parts. [Jan Fuglestvedt, Norway]	Taken into account. Chapter 2 has now re-assessed the difference between GSAT and GMST-based trends, and the best estimate of their scaling is now 1. The statement now states that GMST numbers are equal to GSAT numbers, and only gives the latter.
37239	4	18	4	18	Using 1850-1900 is not appropriate because of the low coverage of the Earth's surface. It wasn't until 1904 that even by the rather generous HadCRUT4 method of calculation, that coverage exceeded 50%. [John McLean, Australia]	Noted. The impact of coverage is discussed in Chapter 2. Detection and attribution studies include varying coverage as a time-varying uncertainty in observations.
98921	4	18	4	19	Again, there is just no other plausible explanation for what is happening, and I think better to say that way would be better. Or perhaps say "it is extremely unlikely that that human activities are not the dominant influence on the climate". At least, I would urge changing the word "main" do "dominant'. [Michael MacCracken, United States of America]	Taken into account. The statement now avoids the use of "main driver" and gives the assessed natural contribution instead.
132115	4	18	4	19	I understand that the authors include here a sentence on the likelihood of human influence being the main (>50%) driver of the observed warming given the continuity with past reports. But it seems that within the AR6, the assessment could go beyond an assessment of likelihood of human influence being responsible for more than 50% of the observed warming, given that the best estimate of human influence is approximately identical to the observed warming, i.e. about 100%. I would strongly encourage the authors to provide a likelihood assessment for the following statement: "Human influence is the overwhelming (>90%) driver of the observed warming since pre-industrial time" (maybe this statement would be assessed as being at the "likely" level?). A statement such as "It is likely that human influence is the overwhelming (>90%) driver of observed warming since pre-industrial time" would mark a clear progression in our understanding compared to the AR5. [Sonia Seneviratne, Switzerland]	Taken into account. The statement now avoids the use of "main driver" and gives the assessed natural contribution instead.
10565	4	18			The [1] note: Rounding the small estimated differences between "GMST" and "GSAT" to one decimal place may not be appropriate given how small the "adjustment" is. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Chapter 2 has now re-assessed the difference between GSAT and GMST-based trends, and the best estimate of their scaling is now 1. The statement now states that GMST numbers are equal to GSAT numbers, and only gives the latter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24119	4	19	4	19	Here we get to the crucial statement on the confidence in the main driver of the observed warming (what was called more than half of in ar5 and most of in ar3,4). This is "extremely likely" like as it was in ar5 although this time the statement applies to all warming since pre-industrial (as defined by relative to 1851-1900). I can't help feeling this is the main advance since ar5, the ability now to make a confident attribution statement about the human and natural contributors to warming since pre-industrial times (subject to caveat about defining pre-industrial as late 19th century). I think this aspect should be stressed in the AR5. Given the context of policy relevant but not prescriptive advice this seems to me an advance for attribution even if the "extremely likely" hasnt been ramped up - the point being that now it applies to a more policy-relevant metric - and this depended on advances in attribution understanding. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This advance is now clearly stated in the paragraph.
34589	4	19	4	19	The message indicates that human influence is the main driver of the observed warming (more than 50% of the change according to the footnote). Is it possible to make a more precise estimate? As currently stated, the reader could easily conclude that almost half of the observed warming was NOT from human influence. [Russell Vose, United States of America]	Accepted. The statement now avoids the use of "main driver" and gives the assessed natural contribution instead.
54929	4	19	4	19	The footnote here (#2) and footnote #3 on the next page provide extremely helpful and welcome specificity to the confidence/likelihood statements and it is essential that this specificity be retained. [Nancy Hamzawi, Canada]	Noted. "main driver" (footnote 2) is however now avoided for the main statement to avoid misinterpretation of its meaning. "main driver" is still used for other aspects of the assessment, and still refers to Footnote 2.
89871	4	19	4	19	How on Earth are we still sticking with the 50% notion? All it translates to is that >50% of the warming is human-induced, when - clearly - our best estimate is 100%. We have been trying for years now, to convince the public that our actual best estimate is 100%, when all they took from AR5 is that it is >50%, by which they simply understand 50%. It has been an uphill battle, and in my view, this needs to change in AR6. [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement now avoids the use of "main driver" and gives the assessed natural contribution instead.
89873	4	19	4	19	Continued: What I don’t understand in this context is why the range of human-induced GSAT warming in 2019 (relative to 1850-1900) of 0.8-1.4°C is only “likely”? Doesn’t the GSAT range include all uncertainty contributions we know? Sure, the response to individual forcing factors has large uncertainties, but the combined total forcing uncertainty is considerably smaller. If we really believe that 0.8-1.4°C is the likely human-induced fraction, why not state the this means 100% of the observed warming is likely human-caused? I’m afraid as it is phrased now, it will come back to haunt us for almost another decade and I’m therefore in strong opposition of how it is currently framed. While it is probably meant to strengthen the overall statement, it does the opposite. Our best guess is 100%. That needs to be stated here in bold letters. Alternatively, based on Fig 3.7, the definition of ‘main driver’ could be changed to: it refers to at least 50% and at most 170% change (if I eyeball the upper bound correctly). [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement essentially describes the probability distribution function of anthropogenic attributable warming. Extremely likely ranges could be given -- they would simply be wider. But the paragraph now gives the natural attributable temperature change, which is minor compared to anthropogenic attributable warming, as stated by the reviewer.
108941	4	19	4	19	The term "main driver" is ambiguous without a footnote and in the general use could imply that it is more important than others but still only explains a relatively small fraction. Using the term "more than half" is more accurate [Erich Fischer, Switzerland]	Taken into account. The statement now avoids the use of "main driver", which carried the "extremely likely" qualifier, and gives the assessed natural contribution instead.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108943	4	19	4	19	Why do you still limit the statement to "more than half". The statement is so often misused to suggest that there is another driver for the other half. It would be much stronger to state that likely or very likely all of the observed warming is anthropogenic. [Erich Fischer, Switzerland]	Taken into account. The statement now avoids the use of "main driver", which carried the "extremely likely" qualifier, and gives the assessed natural contribution instead.
132113	4	19	4	19	"extremely likely" seems on the low side. Is there really a 5% chance that human IS NOT the main (>50%) driver of the observed warming? [Sonia Seneviratne, Switzerland]	Taken into account. The statement now avoids the use of "main driver", which carried the "extremely likely" qualifier, and gives the assessed natural contribution instead.
37681	4	19	4	22	"High level of condidence comes …. from the strong warming observed since the publication of the AR5": this might give an impression of reliance on short-term record. [Masahide Kimoto, Japan]	Taken into account. The assessment is for the period 2010-2019, so necessarily includes the recent warm years. But the statement has been removed to avoid misinterpretation.
132117	4	19	4	23	It is confusing that the text mentions here "level of confidence" or "more confident assessment" when the assessments are actually expressed in likelihood language. It is assumed that any likelihood statements, even at the "more likely than not" level, imply "high confidence". Change "the high level of confidence" to "the high level of likelihood" and "a more confident assessment" to "a statement with higher likelihood". [Sonia Seneviratne, Switzerland]	Accepted. The paragraph does not refer to confidence any more, and uses likelihood throughout.
589	4	20	6	10	P4 L20, P5 L6, P6L10 New attribution approaches, new attribution studies, new evidence… it seems the new is so important, do we need to explain the new in the following context? [ZHIYAN ZUO, China]	Noted. The report assesses recent progress, so it can be expected that "new" will appear often.
10563	4	21	4	22	How much did ENSO contribute to the "strong warming observed since the publication of the AR5". That is a very short period, and we all know the dangers of over interpretating short periods of climate change (See page 14:37-40). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The assessment is for the period 2010-2019, so necessarily includes the recent warm years. But the statement has been removed to avoid misinterpretation.
37245	4	21	4	23	Are you trying to imply AR5 says that models improved? In fact AR5 said "... an analysis of the full suite of CMIP5 historical simulations (...) reveals that 111 out of 114 realisations show a GMST trend over 1998–2012 that is higher than the entire HadCRUT4 trend ensemble ...." [WGI contribution, chapter 9, text box 9.2, page 769, and in full Synthesis Report on page SYR-8]. You should be honest and say that AR5 reported that models performed badly when compared to the data from temperature observations. Is honesty too much to ask of you? [John McLean, Australia]	Rejected. That statement does not state that models have improved. It says that detection and attribution techniques better account for model uncertainties.
26685	4	22	4	22	Is "remaining" useful here? This will always be the case, uncertainty due to internal variability will remain. [Eric Brun, France]	Accepted. The statement has been substantially rewritten and "remaining" has been dropped.
104409	4	22	4	23	Parsons et al. (2020, GRL)show that even if the magnitude of GMST piControl variability approaches 20th century warming, the spatial patterns of forced climate change vs internal variability are distinct. Not sure if this point need to be made here or somewhere else, but this seems important. I'm sure the 'fingerprinting' section also addresses this issue. [Luke Parsons, United States of America]	Noted. The corresponding statement has been deleted to avoid delving too much into detail.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98923	4	23	4	23	On this issue of internal variability, does the historical record provide any indications of such a strong warming in the absence of it being caused by an external factor that might have missed our attention? For virtually all of the major variations, it is being found that there were various influences on the energy influence, such as land cover change, volcanic eruptions,glacial meltpond releases, etc. With changes in external factors being the factor responsible for virtually all significant climatic fluctuations, the internal variability that is left would seem to be limited to a few tenths of a degree, so far less than the change that has been observed. [Michael MacCracken, United States of America]	Noted. The paragraph now assesses the natural contribution to observed warming, which is minor compared to the forced contribution.
110855	4	23			the caveat is very blanket on climate variability - I think this would be more informative if it explained the issue, e.g. that observations provide a limited constraint due to sampling variability, and climate models have different variances of variability, with not all models consistent with observations in all comparisons. it would also be good to narrow this down in terms of how large this uncertainty is - its not orders of magnitude, and the last millebium for example is reasonably consistent between reconstructions and model simulations as well as the instrumenbtal period in terms of observations behaving within the model range a reasonably amount of time. otherwise this caveat also contradicts the later statements [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The corresponding statement has been deleted to avoid delving too much into detail.
579	4	26	5	41	greenhouse gas or greenhouse gases. There are some greenhouse gas, some greenhouse gases, the reader may be confused. [ZHIYAN ZUO, China]	Rejected. "greenhouse gas" is the form when used as the qualifier of a noun.
109325	4	27	4	28	Are both numbers in the aerosol range statement negative? Not sure how to make this clear typographically, but if so, one could write: "anthropogenic forcings is between -0.7°C and -0.2•C." If 0.2 is positive, "between -0.7°C and 0.2°C" would still be better. [Paul Edwards, United States of America]	Accepted. The second number is positive and the statement has been rewritten as suggested.
98925	4	28	4	28	I'd suggest changing "was the main driver" to "has been and continues to be the dominant driver" in that the effect is continuing [Michael MacCracken, United States of America]	Rejected. Attribution is for warming observed to date, and future changes are outside the remit of Chapter 3.
99833	4	28	4	28	dominated by increases in atmospheric greenhouse gas concentrations', rather than 'dominated by GHGs'? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten as suggested.
19753	4	28	4	41	Since this paragraph deals mainly with the surface, the sentence on lines 28-31 should more logically be part of the next paragraph L33-39 [philippe waldteufel, France]	Rejected. The two paragraphs are not interchangeable: the first is about attribution, the second about model evaluation.
110857	4	30			I am slightly surprised that it is dominated by ozone depletion - is this true over all latitudes? If not maybe phrase clearer? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "at high latitudes" added.
98927	4	31	4	31	I'd suggest adding a phrase at the end saying at least "with the rising concentration also playing an important role." Given that the ozone layer is starting to be healed, it might even be useful to say that the increasing CO2 is reinforcing the trend and will come to dominate as the ozone chemistry perturbation subsides. Also, I'm wondering if the ozone effect mainly dominates in high latitudes--is it really dominating in low latitudes--might not some indication of the geographical patterns of influence be useful to include? [Michael MacCracken, United States of America]	Taken into account. "at high latitudes" added. However, Chapter 3 does not discuss possible future changes.
34591	4	33	4	33	The ensemble averages reproduce more than just the trend at the global scale. They also produce the more general pattern of warming up until about 1940, quasi-stability until about 1970, then rapid warming thereafter. It seems to me this message would be even stronger if this point was included. [Russell Vose, United States of America]	Taken into account. "changes" used instead of "trend".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77217	4	33	4	34	This is assumed: what is the finding ? [Emer Griffin, Ireland]	Accepted. The statement has been completely rewritten to assess the fitness for purpose of CMIP6 models for detection and attribution.
102799	4	33	4	34	Compared to the other statements in bold in the ES, this one seems to fall short of a real assessment statement (i.e. something the authors have a certain level of confidence in). [Philippe Tulkens, Belgium]	Accepted. The statement has been completely rewritten to assess the fitness for purpose of CMIP6 models for detection and attribution.
77219	4	33	4	39	For the exec summary a quantified statement would be useful. The meaning is not clear. [Emer Griffin, Ireland]	Accepted. The statement has been completely rewritten to assess the fitness for purpose of CMIP6 models for detection and attribution.
9653	4	34	4	34	Why compare the multi-model multi-member average with the observations? In the presence of internal natural variability, should you not compare individual members to the observations and assess how consistent observations are with model multi-member ensembles? [Olivier Boucher, France]	Taken into account. The statistical nature of the comparison is now emphasised in the main text, but that level of detail is too high for the ES.
10559	4	34	4	35	One would not expect the multi model mean to reproduce the observations perfectly. Variations due to internal variability on different time-scales won't be captured by a multi-model mean, e.g., El-Ninos. The best an analysis could do is look at the consistency of the ensemble spread compared with the observations. It is common to consider the observations as one realisation of a hypothetical ensemble of observations from alternative worlds where the weather/climate noise are being sampled differently. So an assessment should be based on the ensemble spread (an ensemble of opportunity as well) not the multi-model mean alone. e.g, Hegerl et al IPCC, 2007; von Storch and Zwiers, Climatic Change, 2013; Bindoff, IPCC, 2013. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statistical nature of the comparison is now emphasised in the main text, but that level of detail is too high for the ES.
127209	4	35	4	35	By "well", do the authors mean "with high accuracy" or "with limited bias"? [Trigg Talley, United States of America]	Accepted. The statement now clarifies that the biases are small enough to support attribution studies.
26687	4	35	4	37	It would be better if the statement was not written as if the reader knows previous stories. We suggest to rephrase it . [Eric Brun, France]	Accepted. The statement has been completely rewritten to assess the fitness for purpose of CMIP6 models for detection and attribution.
98929	4	35	4	37	In that this conclusion is made only for the satellite era (so with MSU), it does make one wonder if this finding is justified given the many problems that have been found over the years in translating radiance data to estimates of temperature change (that the problem is apparent mainly in the area of Hadley Cell uplift and lots of precipitation, etc. seems to be a bit strange). The conclusion would seem to me more viable if the longer radiosonde record (which does have its own biases that one has to be careful of) were also showing this apparent bias. Overall, I wonder if it would not be better to note that this is a difference that needs resolution, rather than, given both model uncertainties and the past problems with satellite data inversions and radiosond biases, suggesting that one interpretation is more likely than the other. At least in my view, that models have a problem with simulating sufficient sea ice retreat is much clearer and justified than regarding the upper tropical troposphere difference from observations. [Michael MacCracken, United States of America]	Accepted. The assessment is based on radiosonde measurements, so "satellite era" was misleading and has been replaced with "over the period 1979 to 2014".
102801	4	35	4	37	It may be useful to mention what the implications are. [Philippe Tulkens, Belgium]	Taken into account. Implications are better discussed in section 3.3.1.2.
28769	4	35		36	A more quantifiable statement would be beneficial on how well models capture the surface temperature response and how much they overestimate upper tropospheric warming [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The degree of misestimate is now quantified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99835	4	37	4	37	I would prefer that 'satellite observations of stratospheric temperature' is phrased as 'satellite-derived estimates of stratospheric temperature'. And this comment applies more broadly throughout the Chapter and Report. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten as suggested.
1819	4	39			Change "than based" to "than those based" [Alan Robock, United States of America]	Noted. That statement has been rewritten, so the comment is not relevant any more.
1821	4	39			Rather than "some differences remain" say what those differences are. As stated, it has little information content. [Alan Robock, United States of America]	Noted. That statement has been rewritten, so the comment is not relevant any more.
110859	4	39			I struggle with confidence statements in very vague sentences. Is this useful (Id either make a clear statement with medium confidence or drop the confidence) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement has been clarified -- it refers to stratospheric temperature change biases.
581	4	41	4	43	high confidence about relation between phase chang of PDV and hiatus in 1998-2012. actually, in P86 L19, the faster warming in 2012-2024 have no clear relation with the change of PDV. [ZHIYAN ZUO, China]	Taken into account. In SOD, the faster 2012-2026 warming was assessed in comparison to the 1998-2012 warming. While the 1998-2012 trend is influenced by PDV, the lack of 2012-2026 PDV trend on average in selected simulation members acts to yield a faster warming than 1998-2012, and therefore it is consistent with the ES assessment. However, considering other comments and to shorten the text, the corresponding sentence in CCB3.1 has been removed.
19755	4	41	4	44	Although the meaning is clear, this sentence is clumsy. [philippe waldteufel, France]	Noted. We have rewritten this ES bullet.
99837	4	41	4	46	The bold part of this ES statement should also include the revision to the observational datasets which have increased the observed trend over this period (see CCBox 2.3). [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Together with other comments, we have shortened the bold statement. However, we added a sentence on how the updated observational data sets change the trend estimate.
21431	4	41	4	49	I wonder whether the impact of adjustments to the surface records should be more explicitly accounted for in the opening bolded statement or at least in the finding as a whole. Presently nowhere is that adjustments have increased the trend over the period explicitly noted. It is potentially implicit in the unbolded statement but, equally, this improved agreement may have arisen due to changes in the CMIP6 models and not the observations. I think that somehow this needs to be redrafted to more explicitly recognise the impact of new estimates of the observed rate of change over the period. [Peter Thorne, Ireland]	Taken into account. Together with other comments, we have shortened the bold statement. However, we added a sentence on how the updated observational data sets change the trend estimate.
65661	4	41	4	49	The conclusions in the Executive Summary appear to have some discrepancies with the text and figures in Box 3.1. The text on Page 4 states "all observed estimates of the 1998-2012 trend in GMST lie within the 5-95% range of CMIP6 trends". And indeed Figure 1, For Cross-Chapter Box 3.1, shows that both CMIP5 and CMIP6 trends are in general greater than the observed surface temperature trends, which appears to be at odds with the statement in the Executive Summary. [Kushla Munro, Australia]	Taken into account. We have rephrased "5-95% range" to "percentile range" to clarify that it indicates the ensemble spread.
11483	4	41	5	3	The bold headline statement could be shortened (maybe by just writing the second half of the HS in normal type). [Gerhard Krinner, France]	Taken into account. We have rewritten the ES statement and shortened the bold statement.
96253	4	41	5	3	Ch9 mentions some CMIP6 model showing unlikely high warming - is this a concern for the here used multi - model mean? These issues should not be ignored in Ch3. Please see also our comment in the Entire Report on the lack of consistency across chapters. [Nicole Wilke, Germany]	Noted. The influence of overall higher climate sensitivity in CMIP6 compared to CMIP5 is considered in Cross-Chapter Box 3.1.

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116177	4	41	5	3	Is it possible to quantify the change in RF during the period 1998-2012 (building x chapters) and the estimated influence of changes in solar and volcanic forcing in that period, compared to other drivers? This remains quite qualitative here. [Valerie Masson-Delmotte, France]	Rejected. Due to length limitation and low agreement among literature on the magnitude of contributions from internal variability and forcing updates, we cannot assess forcing changes in the Cross-Chapter Box. Besides, the current assessment focuses on the RF trend updates since CMIP5, and introducing the RF trend itself could cause confusion.
28771	4	41			Slower warming than what? Slower than model simulations seems the most appropriatre comparison to me since slower than earlier decades is ill defined given the differing states of forcing whereas models present our best estimate of the rate of warming given the best forcing estimates and the range of internal variability captured by ensemble members. As written the sentence could imply a more important influence of forcing than internal variability whereas I would expect the reverse to be true and so reordering or better still producing an approximate % contribution if available from the literature would be the most valuable. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have clarified the meaning of "slower", and put the internal variability before the forcing. Due to length limitation and low agreement among literature, we decided not to assess their relative contributions.
130483	4	42	4	44	I would suggest to move "solar and volcanic forcing " after the internal variability. [Panmao Zhai, China]	Accepted. We have placed internal variability first and then forcing.
110861	4	42			by a combination of variations…. Would be clearer [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have rephrased this sentence.
77221	4	44	4	46	Not clear on why this is so prominent? What is the message? [Emer Griffin, Ireland]	Taken into account. We have rewritten the ES statement and highlighted in the bold statement that it was a temporary event.
77223	4	44	5	3	Not clear on why some much detail has been included here? This can be shortened or removed. [Emer Griffin, Ireland]	Taken into account. We have considerably shortened this bullet.
28773	4	45			imbalance, [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part of the sentence has been removed.
26067	4	46	4	46	Please explain what is like-for- like comparison. [Don Alfonso Pino Maeso, Spain]	Not applicable. The part of the sentence has been removed.
109737	4	46	4	49	The 5-95% range might confuse people when comparing 1998-2012 and CMIP6 trends. For example, 5% sounds very low for 1998-2012 trends to align with CMIP trends; in addition, I'm further confused in the very next sentence when reading that the 1998-2012 trend is “consistent" with the CMIP6 ensemble. Clarification possible? [Eric Nolan, United States of America]	Accepted. We have rephrased it to "percentile range", and rewritten the sentence.
10569	4	46			A "like-for-like" comparison is not possible ('All models are wrong but some are useful', Box, 1978). The phrase can give the false impression that models can perfectly emulate the way observational datasets are created and how measurements were taken. Also implies comparisons can't be improved in future. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part of the sentence has been removed.
39165	4	46			Please explain "like-for-like comparison of simulated and observed…" (in a footnote, perhaps). [Lourdes Tibig, Philippines]	Not applicable. The part of the sentence has been removed.
109721	4	48	4	49	The 5-95% range might confuse people when comparing 1998-2012 trends and CMIP6 trends. For example, 5% sounds very low comparing both models yet in the very next sentence it states “consistent". Clarification possible? [Eric Nolan, United States of America]	Accepted. We have rephrased it to "percentile range".

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28775	4	48		49	This would seem a headline statement to me: "the observed 1998-2012 trend is consistent with the CMIP6 multi-model ensemble of trends over the same period (high confidence)." and an advance over AR5 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. After discussions among the authors, we decided to highlight the "temporary event" assessment in the headline statement.
102803	4	50	4	50	In footnote 2 the language of "main driver" is introduced. It is used consistently in the executive summary and in the first half of the chapter, but not after page 57. The authors could also sift through the latter part of the chapter and the FAQs for this chapter and use this language consistently also there. In particular in the FAQ and the example of the bicycle ride, the analogy could be strengthened by this language. [Philippe Tulkens, Belgium]	Accepted. We now ensure consistent use of 'main driver'.
99827	4		7		Many of the ES statements include a specific year denoting the start of a trend or assessed attribution period, e.g. 'since 1950', but no indication of why that particular year is relevant. Sometimes these years appear to be the start of an observational dataset, sometimes other reasons. Please can it made clear in each case why the year is chosen. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The choice of years is justified in the chapter text. We do not have space to justify the choice of start years in the ES. The choice of start year is generally based on the availability of observations.
15479	5	1	5	2	It has been confirmed by WMO that 2015-2019 is the hottest five-year period on record (Ref.: https://public.wmo.int/en/media/press-release/wmo-confirms-2019-second-hottest-year-record). Please consider revision. [SAI MING LEE, China]	Accepted. This part has been updated.
18695	5	1	5	2	2019 could be also included in the final draft. [Govindasamy Bala, India]	Accepted. This part has been updated.
17597	5	1	5	3	Not a fair balanced Executive Summary sentence, because warming in this so called "hottest" period 2014-2018 is most strongly influenced by El Nino and not CO2. Not mentioning El Nino "short time weather" as the major cause for this hottest warming period is giving a wrong and false "AGW-alarm" message to policy makers. Chapter 3-P86 Line 12 reads "El Nino event in 2014-2016 led to 3 consecutive years of annual record GMST". Not mentioning this "natural variability" as major cause for the hottest 5 year period in the Executive Summary is a very good example of strong "AGW groupthink", tunnel vision and selection bias. [ferdinand meeus, Belgium]	Noted. The influence of internal variability on the rapid warming is assessed in Cross-Chapter Box 3.1. PDV encompasses decadal modulations of El Nino and La Nina occurrences, so the sentence is consistent.
98931	5	2	5	2	Given that the report will be coming out in 2021, so three years past 2018, and will be the dominant reference for perhaps 7 years beyond, I would suggest changing "in the instrumental record until 2018", wording that implies this chain has been broked, to "in the instrumental record to date", which allows for this trend to be continuing. [Michael MacCracken, United States of America]	Taken into account. The observational record has been updated to 2020.
102805	5	2	5	2	it may help the reader to state when the instrumental record begins. [Philippe Tulkens, Belgium]	Accepted. We have added that the record traces back to 1850.
109739	5	2	5	2	Possible to qualify what is meant by “instrumental record”? It might help to put some examples in parenthenses This term is used later in the chapter but I do not have a reference of the period at hand here in the beginning of the chapter. Thank you. [Eric Nolan, United States of America]	Taken into account. The term "instrumental records" has been used in Chapter 2 and we do not redefine it. However we have added here that the record traces back to 1850.
55433	5	3	5	5	likely seems a very conservative interpretation of the evidence in the literature [Friederike Otto, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. "Likely" is consistent with available evidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110863	5	4		15	This section is a bit weak and unclear. High latitude precipitation has studies for a while now (Min et al. eg?) so why is it important that its new? Is it strengthening evidence? Also, the salinity section talks about increasing contrast between low and high salinity which could work well together with several findings on strengthening contrast over the tropics and subtropics. Also, the model evaluation here seems an afterthought. i would organize this first with evaluation (difficulties with biases in location timing etc of precipitation) limiting confidence yet the physically expected changes such as strengthening contrast in tropics and subtropics and enhancement in high latitudes are well understood, and that is supported by data. ... the likely level is appropriate i think - definitely not too strong [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. ES statement has been modified to include some of the concerns. The structure of the statement follows similar lines as others.
34593	5	5	5	5	Maybe I have this wrong, but I thought likelyhood statements were limited to cases in which confidence was high or very high. All of the sentences in this message seem to have medium confidence. [Russell Vose, United States of America]	Noted. Left as it is because the sentence doesn't include confidence statement. The bold sentence has high confidence implicitly.
77225	5	5	5	6	The likely statement should be cleared and the material below supports this. What large scale precipitation changes are likely changed? [Emer Griffin, Ireland]	Taken into account. This is a summary sentence, details are provided in the following sentences.
98933	5	5	5	6	Saying "since 1950" makes this seem very precise. I'd suggest saying that "That there has been a human influence on large-scale precipitation changes started to become evident in the mid-20th century and has grown since (medium confidence)" or something similar. [Michael MacCracken, United States of America]	Taken into account. Sentence has been modified.
127211	5	5	5	12	These changes in precipitation are not nearly as clearly articulated in Chapter 2 as implied here. Some cross-chapter discussion is required. [Trigg Talley, United States of America]	Taken into account. The section now presents better cross-reference with Ch 2.
52843	5	5	5	15	It might be possible and useful to add a statement about changes in atmospheric humidity (including near surface humidity) before summarizing the key findings about changes in precipitation and runoff. [Hervé Douville, France]	Taken into account. An ES statement on this has been included.
77227	5	5	5	15	This can be shorter and clearer, what is evident, what is not? [Emer Griffin, Ireland]	Taken into account. The whole ES statement was revised and large parts rewritten.
88903	5	5	5	15	Need to explicitly mention extreme rain events & flooding here. Eg. tripling of frequency in Sahel (Taylor et al. (2017, Nature), higher frequency in East Afria, and doubtless elsewhere in the tropics. I think there are one or two formal attribution studies, but sorry don't have the references. Anyway very important from an impacts point-of-view. [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There is a separate bullet on attribution of changes in extremes, including precipitation extremes.
71887	5	5		12	These changes in precipitation are not nearly as clearly articulated in Chapter 2 as implied here. Some cross chapter discussion is required. [John Church, Australia]	Taken into account. The section now presents better cross-reference with Ch 2.
45237	5	5			The clarifying footnote phrase for "human influence has contributed to" is really thoughtful and important. [Krishnan Raghavan, India]	Noted.
4011	5	7	5	8	Given the time frame being discussed is 1950 onwards, doesn't this seem to contradict the observed findings of chapter 2? A reader may naturally assume that the monsoon regions are the majority of the "wet regions of the tropics", for which the signal has been a declining one from the 1950s to the 1980s (see your own text lines 38-40 on page 3-31 which states: In the instrumental records, global summer monsoon precipitation intensity (measured by summer precipitation averaged over the monsoon domain) decreased from the 1950s to 1980s, followed by an increase"). So perhaps this ES statement needs to be revisited or the "wet regions of the tropics" component defined more carefully. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been modified to reflect weakening of the global monsoon from 1950 to 1980.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4013	5	7	5	8	Secondly, on the wording of "...increased due to enhanced greenhouse warming", this could have two interpretations (to a reader familiar with the experiment design). Does it mean (a) that rainfall has increased, and it was found to be due to GHG forcing or (b) that when forced with GHG alone, rainfall increases? The two are not the same. A better wording might be: "Despite large decadal variability, there is medium confidence that rainfall increases over the wet regions of the tropics are attributable to enhanced greenhouse gas forcing." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been removed when rewriting the ES statement.
45239	5	7	5	8	The following ES statement is not well supported by the main assessment "Despite large decadal variability, there is medium confidence that rainfall over the wet regions of the tropics has increased due to enhanced greenhouse gas forcing". In fact, precipitation decreased over several tropical areas since 1950s (eg., East Africa, monsoon precipitation over South Asia, West Africa). There has been recovery of the West African monsoon precipitation since the mid-1990s. Northern hemispheric land monsoon precipitation also declined during the last 6-7 decades. The global monsoon domain includes areas outside the "tropics" and also oceanic regions, which makes it difficult to distinguish between increase of global monsoon precipitation and increase of rainfall over the wet regions of the tropics. In short, there is not enough evidence / supporting material in {3.3.2.1} to indicate that "rainfall over the wet regions of the tropics has increased due to enhanced greenhouse gas forcing" [Krishnan Raghavan, India]	Taken into account. The sentence has been modified to reflect weakening of the global monsoon from 1950 to 1980.
98935	5	7	5	8	I'd urge providing an explanatory note for this statement, for example by saying "has increased due to the strengthening of the Hadley Cell as a result of enhanced greenhouse gas forcing." [Michael MacCracken, United States of America]	Rejected. Mechanisms are detailed in chapter 8. This statements are about detection and attribution.
39167	5	7	5	11	Has the influence of the anthropogenic aerosols ceased its decreasing effect in the Northern Hemisphere summer monsoon region after the late 20th century so that this muting effect has not contributed to how the rainfall has changed now? [Lourdes Tibig, Philippines]	Noted. According to Wu et al (2013) reductions of air pollution and aerosol emissions since the 1980s have already caused an acceleration of the hydrological cycle. If this reduction continues further intensification of the hydrological cycle is expected (see also Wild 2012).
4015	5	8	5	11	Similar to the preceding comment, the wording here is confusing. The most interesting part (not that I am biased to the monsoon!) is the decline in NH summer monsoon rainfall from 1950-1980 (at least) and its attribution to anthopogenic aerosols. However at present the sentence mixes this up with the previous discussion on the increasing wet region rainfall. It is also not immediately clear what the "medium confidence" applies to - is it to the attribution to aerosol of the NH monsoon rainfall decline, or of the proportion of the wet tropics to which the monsoon contributes a decreasing signal? The latter seems to be an observational confidence exercise rather than anything to do with attribution and the subject of this chapter. A better wording might be, "Despite the general increase in tropical wet region rainfall, there is medium confidence that anthropogenic aerosols have led to declining Northern Hemisphere summer monsoon rainfall in the mid-to-late 20th century." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been modified to reflect weakening of the global monsoon from 1950 to 1980.
99839	5	8	5	11	Unclear sentence [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence was rewritten.
28777	5	8			remove "enhanced" as redundent [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The sentence has been removed when ES statement was rewritten.
583	5	11	5	12	south ocean and south midlatitude? Are there some regions overlap between them or total separated ? [ZHIYAN ZUO, China]	Taken into account. Changed "southern ocean" to "southern high latitudes".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19757	5	11	5	13	The impact of ozone depletion on precipitation is counterintuitive to say the least. Inasmuch as this impact seems to transit through a change of circulation features and specifically the SAM, indicate this relation might help to understand this mechanism. [philippe waldteufel, France]	Taken into account. Sentence was rewritten to reflect that precipitation changes are due to the trend in SAM toward its positive phase.
98819	5	13	5	13	[...]However, despite improvements, the models still have deficiencies in the simulation of some characteristics of precipitation patterns, particularly in tropical oceans, and also in simulated runoff. [Julio Cesar Barreto da Silva, Brazil]	Taken into account. Modified as suggested.
50699	5	13	5	15	This sentence is a little confusing. Could a rephrase be: "However, even though there have been improvements, models still have deficiencies in simulating some characteristics of the precipitation patterns, such as simulated runoff, and in particular patterns in the tropical oceans." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentences modified.
98937	5	14	5	14	I would urge changing "in the" to "over the" [Michael MacCracken, United States of America]	Taken into account. Sentence modified.
28779	5	14			specifying precipitation deficiencies for particular policy relevant regions if appropriate would be more valuable than tropical oceans (e.g. seasonailty over southern west Africa; the Indian monsoon, etc) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This statement is about large-scale precipitation. Biases in tropical rainfall affect the global circulation.
45241	5	15	5	15	Relevant Chapter 8 sections {8.3.1.3, 8.3.2.4} may be referenced. [Krishnan Raghavan, India]	Rejected. This statement is about detection and attribution of large-scale changes in precipitation, not regional changes.
17601	5	17	5	20	"Very likely" is not justified . According to recent measurements April 2020 Finnisch meteo, snow fall NH is at record high for this year. More likely natural internal&external factors play important role. [ferdinand meeus, Belgium]	Rejected. The observed reduction between 1950 and 2015 is robust . There is always a possibility of extreme events occurring that appear to go against an observed trend. According to the peer-reviewed literature and our own analysis of CMIP6 simulations, the reduction in NH spring snow cover is well simulated, and CMIP5 and CMIP6 models indicate that a trend similar to the observed magnitude is only simulated with very high probability if anthropogenic forcings are taking effect, in particular increasing GHGs.
587	5	17	5	23	in this paragraph, two sentences for hadley cell, other sentences for monsoon and blocking, do we need to separate them in different paragraph since there are no logical relation between them. [ZHIYAN ZUO, China]	Rejected. This statement is about the atmospheric circulation and are thus summarized into a single bullet.
66967	5	17	5	23	If no other feature of atmospheric circulation has been affected by human influence, it could deserve a sentence. [Aurélien Ribes, France]	Rejected. Chapter 3 does not assess all aspects of atmospheric circulation. Chapter 3 focuses on large-scale atmospheric circulation components for consistency with Chapters 2 and 4, and more details are assessed in the subsequent chapters.
77229	5	17	5	23	This is very technical, perhaps explained in terms of energy balance and heating and cooling otherwise why is GHG warming and stratospheric ozone depletion important for this statement? [Emer Griffin, Ireland]	Taken into account. Considering our updated figure, we decided not to emphasize roles of individual forcings.
77231	5	17	5	23	This is not clear and therefore not very helpful. [Emer Griffin, Ireland]	Taken into account. We have revised the statement.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98939	5	19	5	20	I'm a bit surprised that there are sufficient data and observations around the world to come to this conclusion, that is, to have confidence that the contribution of internal variability has been accurately separated from the roles of various external factors in causing variations. My work with models indicated that the ITCZ is quite sensitive to various mid- and high latitude forcings, adjusting so as to tend to equalize the distribution of excess tropical heating between the hemispheres, so all sorts of things other than natural variability could be contributing to the observed fluectuations, and this would need to be done explicitly to really identify the contribution of "internal variability" (which would also include the role of external factors that have not yet been extracted from the record). [Michael MacCracken, United States of America]	Taken into account. The Hadley cell extent assessed in this chapter does not consider the latitude of its upwelling branch (i.e. ITCZ). We have clarified this by rephrasing the expansion as "the poleward expansion of zonal mean Hadley cell".
11951	5	21	5	22	There are a lot of systematic erros in the models. Two specific examples are very arbitrarily chosen. Better to focus on large temperature bisase (such as over continents) and precipitation. [Masaki Satoh, Japan]	Rejected. As this bullet is based on Section 3.3.3, this is an atmospheric circulation bullet, and it is natural to focus on model errors in simulating components of atmospheric circulation assessed in Chapter 3.
127213	5	21	5	22	The term "systematic errors" can be easily misinterpreted here. Suggest more exact language to note inconsistencies with observations. [Trigg Talley, United States of America]	Taken into account. We have rephrased the statement considering this and another comments.
109741	5	21	5	23	Please describe or give an example of a “blocking event”. [Eric Nolan, United States of America]	Rejected. This cannot be explained in the ES. Instead, a brief explanation of blocking is provided in Section 3.3.3.3.
98941	5	22	5	22	I'd suggest changing "errors" to say that "Systematic differences meriting investigation are, however, still …" Regarding the North Atlantic, the historical record has likely been influenced by the time-changing amounts of SO2 emissions and sulfate formation and, at least to date, the data bases for the altitudes of emissions of the SO2 through much of the 20th century have been pretty uncertain. So, rather than the models being in error, it just might be that this is a response to discrepancies with observations in the amount and effect of sulfate forcing, so it is the input to the models that is the problem. Aerosol-related issues could also be an influence affecting monsoon rains. So, I'd suggest saying "differences meriting investigation" rather than "errors" [Michael MacCracken, United States of America]	Taken into account. We have rephrased the statement considering this and another comments.
89875	5	25	5	25	What does “substantial” mean? As before, while the best estimate might not be 100% in case of OHC (due to increased uptake at the beginning of the instrumental period in response to strong volcanic activity), it’s certainly not far off. As before, a best estimate should be provided, together of a definition of “substantial”. [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Substantial contribution is changed to “main driver”, i.e. responsible for more than 50% of the change
104943	5	25	5	25	to the ocean heat -> to the observed ocean heat [Peter Gleckler, United States of America]	Taken into account. “Observed ocean heat” is now used
98943	5	25	5	26	On line 26, I'd suggest changing "increase over the historical period that extends" to "over the historical period, especially in the upper 700 meters, but also extending into" or something similar--the present phrasing seems to me too abrupt. In association with this change, on line 25, I'd suggest changing "has made a substantial contribution to" to "caused a substantial increase in" [Michael MacCracken, United States of America]	Taken into account. Substantial contribution is changed to “main driver”, i.e. responsible for more than 50% of the change
77233	5	25	5	28	The AR5 shows that over 90% of traditional energy trapped by GHG is taken up by the oceans. Can a similar statement be used here? [Emer Griffin, Ireland]	Rejected. This ES focuses on the increase in ocean heat content rather than energy balance which is a focus of Chapter 7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99841	5	25	5	28	What does 'substantial contribution' mean? Can this be more precise? Also it is unclear what is meant by 'forcing discrepencies'. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Substantial contribution is changed to “main driver”, i.e. responsible for more than 50% of the change. The ES has been revised and clarified with the term “forcing discrepancies” removed.
132623	5	25	5	33	This statement 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. The confidence level is revised: “very high confidence” is currently used.
585	5	26	5	26	what is the definition aobut historical period? [ZHIYAN ZUO, China]	Taken into account. “Over the historical period” is replaced with “since the 1970”
96255	5	26	5	26	The phrase 'over the historical period' is too vague in comparison to time frames, that are provided in other contexts. It should be replaced by a more precise frame, e.g. 'from 1850 to 2018' or similar. [Nicole Wilke, Germany]	Taken into account. “Over the historical period” is replaced with “since the 1970”
12517	5	27	5	28	Also because of the improvements in observational OHC estimates, see Cheng et al. 2019, Science. [Lijing Cheng, China]	Noted.
6581	5	29	5	29	Change "observed estimates" to "estimates from observations". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We prefer to use 'observed estimates' since it is shorter, and is widely used in the literature.
34999	5	30	5	33	This statement is very similar to a Chp 9 ES assessment "The ocean has warmed at all levels and continue to do so (very high confidence), contributing to future sea-level rise even under low emissions scenarios.", and perhaps represents undue overlap or a potential for inconsistency. For example, are we sure that these numbers are consistent with Figure 9.6? Model-data comparisons need to be handled carefully, as they span many chapters. I support this assessment being present, but care is needed to avoid conflict or duplication. In contrast to the next ES statement, which focuses specifically on salinity attribution, this warming statement drifts away from the attribution and toward just statements about warming observations & modeling. [Baylor Fox-Kemper, United States of America]	Noted. The ES has been revised. Text on model-data comparisons is shortened in order to have more emphasis on the Detection-Attribution part.
96257	5	30	5	33	it should be stated whether these numbers differ from the assessment in the SROCC or confirms them or have been taken from the SROCC. [Nicole Wilke, Germany]	Taken into account. The numbers are updated. They are taken from CMIP6 assessed in the main text.
127215	5	31	5	33	Not sure that explaining how models partitioned the ocean -- without presenting results based on those partitions -- rises to the level of importance of being in the executive summary. [Trigg Talley, United States of America]	Taken into account. The partitioning is now presented within the text with updated figures.
15927	5	31	5	34	The paragraph: "A warming level of 1.5ºC 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)." would be less vague if probabilities were given rather than subjective comments. For example "more likely than not" is better stated as having a probability of greater than 50%, or even better if the probability is known, this should be stated. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This comment does not correspond to Chapter 3.
6583	5	32	5	32	Why "1865"? The pre-industrial level is taken mainly as 1850-1900 in Chapters 2 and 4. So it would be neater if 1865 could at least be replaced by "1875", i.e. close to the mid-point of the period 1850-1900. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This has been revised to start from 1850, consistent with other assessments.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102807	5	39	5	41	The syntax of this sentence could be changed for better readability [Philippe Tulkens, Belgium]	Taken into account. Text has been revised.
77235	5	45	5	45	Thermal expansion would be clearer and easier to use rather than academic terminology [Emer Griffin, Ireland]	Taken into account. Text revised.
98945	5	45	5	50	I think it will be unnecessarily confusing to be using "thermosteric" on lines 46 and 47 and then using thermal expansion" on line 50. I would urge changing "thermosteric" to "contribution due to thermal expansion" on lines 46 and 47 and then on line 47 putting "thermosteric" in parentheses after the phrase. [Michael MacCracken, United States of America]	Taken into account. Text revised.
17599	5	45	5	51	Not a fair balanced summary for sea level rise. The starting point 1970 can be seen as bias. Most data are available from tide gauges going back more than 100 years. These tide gauge measurement indicate a linear sea level rise of about 1,5-2 mm/year. With no acceleration over the 100+ year historical time period. So "very likely" is not justified based on tide gauge measurements. Especially because tide gauge measurements are most important for local coastal planning and future climate projections. [ferdinand meeus, Belgium]	Rejected. This bullet summarizes only the global mean sea level changes which is within the ambit of this chapter.
96259	5	45	5	51	Why is the headline statement limited to thermosteric sea level? The following text includes the ocean mass change. [Nicole Wilke, Germany]	Taken into account. Text has been revised.
107225	5	45		51	It says, "...it is very likely that anthropogenic forcings are the main driver of the observed global mean sea level rise since 1970." That's nonsense. Notwithstandign the junk-science sea-level attribution papers, the simple fact is that coastal sea-levels are rising no faster now, with CO2 at 410 ppmv and CH4 at 1.86 ppmv, than they were nine decades ago, with CO2 at 307 ppmv and CH4 at 1.03 ppmv. All those GHG emissions and all that concurrent warming have caused no significant, detectable, sustained acceleration in the rate of sea-level rise. Refs: https://sealevel.info/1612340_Honolulu_Wismar_Stockholm_vs_CO2_annot3.png http://link.springer.com/article/10.1007%2Fs00382-013-1771-3 https://www.academia.edu/30694598/Tide_gauge_location_and_the_measurement_of_global_sea_level_rise?auto=download http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-12-00319.1 https://www.sciencedirect.com/science/article/pii/S0378383913000082 ### [David Burton, United States of America]	Rejected. This statement does not mention anything about acceleration of sea level rise.
115035	5	45		51	It says, "...it is very likely that anthropogenic forcings are the main driver of the observed global mean sea level rise since 1970." That's nonsense. Notwithstanding the junk-science sea-level attribution papers, the simple fact is that coastal sea-levels are rising no faster now, with CO2 at 410 ppmv and CH4 at 1.86 ppmv, than they were nine decades ago, with CO2 at 307 ppmv and CH4 at 1.03 ppmv. All those GHG emissions and all that concurrent warming have caused no significant, detectable, sustained acceleration in the rate of sea-level rise. Refs: https://sealevel.info/1612340_Honolulu_Wismar_Stockholm_vs_CO2_annot3.png http://link.springer.com/article/10.1007%2Fs00382-013-1771-3 https://www.academia.edu/30694598/Tide_gauge_location_and_the_measurement_of_global_sea_level_rise?auto=download http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-12-00319.1 https://www.sciencedirect.com/science/article/pii/S0378383913000082 [David Burton, United States of America]	Rejected. This statement does not say anything about acceleration and summarizes only the global mean sea level changes which is within the ambit of this chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112645	5	46	5	49	Rewording. Since the AR5, studies have highlighted that simulations which exclude… are unable to capture the thermosteric sea level rise of the historial period. Moreover, states that simulations including all forcings (antrhopogenic and natural) most cloesly match observed estimates. [Melissa Jiménez Gómez Tagle, Germany]	Taken into account. Text revised.
83595	5	46			"It is very likely that anthropogenic forcings are the main driver of the observed global mean 46 thermosteric sea level increase since 1970. Since the AR5, studies have highlighted that simulations that 47 exclude anthropogenic greenhouse gases are unable to capture the thermosteric sea level rise of the historical 48 period and that model simulations that include all forcings (anthropogenic and natural)" The difficulty with this logic is that the changes (parabolic increase?) plus superposed 65yr cycle are very clear from 1750CE. Difficult to justify anthropogenic dominance from 1970. Trends are obvious, but I suggest more weight be given to consideration of natural mechanisms not yet documented to explain the observed data set. The entire discussion of GMSL does not mention existence of a natural ~60year cycle superimposed on the GMSL rising trend from 1700CE. It is an unfortunate omission as it does influence how we judge GMSL rise in different past decades and into the future. Some relevant refs are Chambers, D. P., M. A. Merrifield, and R. S. Nerem, 2012: Is there a 60-year oscillation in global mean sea level? Geophys. Res. Lett., 39, L18607. Jevrejeva, S., Moore, J.C., Grinsted, A., Woodworth, P.L., 2008, Recent global sea level acceleration started over 200 years ago? Geophys. Res. Lett.35, L08715. The following ref extends Jevrejeva's analysis and demonstrates a clear 65yr period cyclic component from present day back to 1750CE. Asten, M.W., 2017, Phase relations of natural 65 year SST variations, ocean sea level variations over 260 years, and Arctic sea-ice retreat of the satellite era – issues of cause and effect, Geophysical Research Abstracts, Vol. 19, EGU2017-9833, EGU General Assembly 2017. [michael asten, Australia]	Noted.
26689	5	48	5	48	We suggest to delete "model", a simulation being made using a model [Eric Brun, France]	Taken into account. Text has been revised.
127217	5	50	5	51	The last sentence repeats the first sentence. [Trigg Talley, United States of America]	Noted that the similarities in the two sentences can be confusing. Text has been modified.
98947	5	51	5	51	I'd urge saying "since at least 1970" as the dominant human contribution very likely goes back earlier, if not due to GHG influences, due to land cover change, etc. [Michael MacCracken, United States of America]	Taken into account. Text revised.
99843	6	1	6	3	What sign are the 'changes' observed? This statement could be more precise. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised.
77237	6	1	6	7	This is not at all clear. What is the message? [Emer Griffin, Ireland]	Taken into account. Text revised.
98949	6	2	6	3	On line 2, I'd suggest changing "if these changes are due to" to "the relative contributions of" and on line 3 change "anthropogenic forcing" to " anthropogenic contributions of GHGs and aerosols over time." Both GHGs and aerosols can have influences, and all of these influences could be changing over time. [Michael MacCracken, United States of America]	Taken into account. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109327	6	2	6	3	Parallelism error. "...to determine if these changes are due to internal variability, solar and volcanic forcing or a response due to anthropogenic forcing" should be: "to determine whether these changes are due to internal variability, solar and volcanic forcing, or due to anthropogenic forcing." (You could omit the second "due" in the proposed rewrite.). [Paul Edwards, United States of America]	Taken into account. Text revised.
130481	6	5	6	8	The statement on " human induced climate trends are relative large for water-cycle variables than temperature....." is not precise and clearly. Please consider to rephrase it. [Panmao Zhai, China]	Taken into account. This statement is not included in the FGD.
34595	6	9	6	9	This key message discusses sea ice, but can anything about loss of ice mass loss on Greenland and Antarctica? [Russell Vose, United States of America]	Taken into account. Assessment of mass loss from Greenland and Antarctica added.
89877	6	9	6	9	As before again, our best estimate that all the Arctic sea ice loss is human-induced (as far as the 20-year running mean is concerned). [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Main driver indicated more than half of the changes are due to human influences.
98951	6	9	6	9	It is not at all clear why the year 1979 was chosen--why be so specific? It that is when adequate observations became available, then say that, so say, "since a comprehensive observational record became available in 1979." This is also important since the next sentence indicates that there is an adequate record back to the 1950s and that is when sea ice loss started (and is that really the case given sea ice was supposedly less in the Atlantic basin in the 1930s and 1940s and it had then built back up. [Michael MacCracken, United States of America]	Taken into account. Comprehensive satellite data are available since 1979. Rephrased it as "late 1970s".
112647	6	9	6	10	There is new evidence that an increase in ussage of anthropogenic aerosols have offset part of the greenhouse gas induced Arctic [Melissa Jiménez Gómez Tagle, Germany]	Noted. The point was included already.
39169	6	9	6	15	Why is the uncertainty language very likely when the models do not agree on their findings and there is low confidence in the understanding of the causes? [Lourdes Tibig, Philippines]	Noted. The justification for the likelihood assessment is provided in Section 3.4.1..
99845	6	9	6	15	Why 1979 in this statement, when aerosols are implicated since the 1950s in the next sentence? There are pre-satellite estimates of Arctic sea ice extent (even back to 1850) so this statement could be clarified and strengthened. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Comprehensive satellite data are available since 1979. Rephrased it as "late 1970s".
77239	6	9	6	26	Are these not factual statements? [Emer Griffin, Ireland]	Rejected. Based on the literature, the confidence level for this statement is not as high as factual.
99847	6	12	6	12	captured by' is unclear. Is 'simulated in' (or similar) a better option? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. 'captured by' replaced by 'reproduced in'.
98953	6	12	6	15	First, thank you for pointing out the the trends in Antarctic sea ice went up and now are going down, as opposed to chapter 2 which says that no trend exists, presumably because they did only an undivided linear analysis (they need to change their statement). I had thought the increasing trend had been associated with an atmospheric circulation change due to the stratospheric ozone depletion in the area, and that as this influence has waned the global warming influence has taken over. Perhaps alter the wording to say "and there is low confidence in understanding the relative roles of stratospheric ozone depletion (tendency to increase sea ice cover), GHG-induced warming (tendency to reduce sea ice cover), internal variabiity, and other possible factors in affecting sea ice cover." [Michael MacCracken, United States of America]	Taken into account. Our assessment here is focused on attribution of large-scale long-term changes in Antarctic SIE. We have revised the sentence to address no significant observed trend in Antarctic SIE by saying "small observed increases". Detailed physical processes are covered by Ch9.
26691	6	15	6	15	We suggest to change "these changes" with " these 'bserved changes" [Eric Brun, France]	Taken into account. Sentence re-phrased to simplify. We still refer to 'this change' at the end of the sentence, but it is now clearer what it refers to.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96261	6	17	6	20	The statement on glaciers seems out of place under the headline statement on springtime snow cover. [Nicole Wilke, Germany]	Noted. We do not wish however to elevate the sentence on glaciers to become a free-standing bullet point in the Executive Summary.
69557	6	19	6	20	"Anthropogenic forcings very likely contributed to the observed retreat of glaciers." In think this is a much much weaker statement than it should be. 'contributed' could mean 5%. There is no quantification of the degree - it is certain that human activity has contributed something. If there is a statement in this report that it is "extremely likely" that humans are the primary driver of temperature changes since the 19th century, then it is my view that you can use Roe et al. (2017) to conclude that it is also "extremely likely" that human activity is the primary driver of the retreat of mountain glaciers over that period. We know the connection between temperature and mass balance well enough to link them. [gerard Roe, United States of America]	Accepted. In the chapter (3.4.3) we note that the "retreat of glaciers is very likely attributable to anthropogenic influence" which indeed warrants a stronger statement in the ES. We have modified the phrase.
98955	6	19	6	20	Phrasing it this was seems to me to leave an opening to deniers to say, well, this particular glacier is growing so IPCC is completely wrong. How about saying something like: "contributed to the retreat of virtually all mountain glacier, the exceptions being special locations where snowfall was increased by GHG induced changes in circulation and additional moisture availablity due to warmer ocean waters." So, basically acknowledge that there can be glaciers that are growing without this being a contradiction. [Michael MacCracken, United States of America]	Accepted. We have rephrased the sentence to "near-universal glacier retreat" to allow for some variation of glacier loss (including for growth in some glaciers).
99849	6	19	6	20	Can the glacier retreat sentence be expanded to give geographical or timing details, or other statistics? The current version is vague. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It would be outside the remit of this chapter to go into regional differences of glacier retreat.
127219	6	20	6	20	It was unclear whether this last sentence included glaciers outside the northern hemisphere. [Trigg Talley, United States of America]	Accepted. We now make clear this refers to glaciers globally.
98957	6	22	6	22	Might you want to say "primarily attributable to" to allow for other factors like changing boundary layer depth, a longer growing season, increased overall precipitation, etc. also playing a role. I'd be careful of sort of implying there is only one factor at play. [Michael MacCracken, United States of America]	Accepted. Sentence rephrased as suggested.
116179	6	22	6	22	Where to find an attribution for the greening / browning trend to update what was discussed in SRCCL (qualitatively)? This would deserve x chapter coordination, as SRCCL stressed that some of the greening trend outside cold regions is driven by effects of land management, nitrogen deposition, irrigation in Asia. [Valerie Masson-Delmotte, France]	Accepted. A statement on the greening trend has been added.
17099	6	22	6	23	I suggest these change: The observed increased amplitude of the seasonal cycle of atmospheric CO2 is likely attributable to the increased in CO2 fertilization effect to plant growth fertilisation of plant growth by increased CO2. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been rewritten based on the suggestion.
81595	6	22	6	23	In P3-57 L 50, the chapter rightly says that there is medium confidence, and lists a range of other factors contributing to this trend. This statement here does not adequately reflect the uncertainty and the fact that CO2 isn't the only cause for the increase [Sönke Zaehle, Germany]	Accepted. Sentence rephrased following comment number 98957.
34863	6	22	6	26	Detailed Comments by SOD Chapter – Chapter 3: It is good that the SOD notes the increased fertilisation effect of slightly elevated CO2 levels. Please see general comment #16 above. [Jim O'Brien, Ireland]	Noted. CO2 concentrations have increased from 280 ppm in 1750 to more than 400 ppm today, which is more than a slight increase.
50701	6	22	6	26	Found this sentence a little unclear, suggest rephrasing or adding more explantion. Does it mean: "The observed increase in amplitude of the seasonal cycle of atmospheric CO2 is likely attributable to increased CO2 causing increases in fertilisation of plant growth." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been rewritten based on the suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96263	6	23	4	26	The sentence is too long, thereby hindering the understandability. It should be split after 'limitation on plant growth'. Up to this point the statement is hard to understand and after that even harder. Is it correct to understand that models, taking into account for nutrient limitation on plant growth, do NOT simulate the interannual variability of the carbon sink very well? If so, it should most probably be rephrased to 'even though they take into account nutrient limitation on plant growth'. Otherwise it could be misunderstood such that they do not simulate well, BECAUSE they take into account nutrient limitation. The statement beginning with 'but a possible underestimate...' is really hard to grasp. What is underestimated? Magnitude and interannual variability of the carbon sink? Or is it 'the role of warming ... in affecting plant growth ... '. The statement should be rephrased completely. [Nicole Wilke, Germany]	Taken in account. The sentence has been shortened by reducing the amount of detail, focusing only on the main conclusions that support the low confidence level.
98821	6	23	6	23	fertilization [Julio Cesar Barreto da Silva, Brazil]	Rejected. The report uses British English spelling.
26693	6	23	6	26	this sentence is difficult to follow. It would be better to cut it in two sentences [Eric Brun, France]	Taken in account. The sentence has been shortened by reducing the amount of detail, focusing only on the main conclusions that support the low confidence level.
34597	6	23	6	26	This sentence is a long and dense read. [Russell Vose, United States of America]	Taken in account. The sentence has been shortened by reducing the amount of detail, focusing only on the main conclusions that support the low confidence level.
50703	6	23	6	26	This is a very long sentence. Suggest this is split into two separate sentences. "There is medium confidence that Earth system models simulate the magnitude and large interannual variability of the land carbon sink well if they account for nutrient limitation on plant growth. There is a possible underestimate by models, however, of the role of warming of surface temperature in affecting plant growth which prevents a more confident assessment." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken in account. The sentence has been shortened by reducing the amount of detail, focusing only on the main conclusions that support the low confidence level.
102809	6	23	6	26	The syntax of this sentence could be changed for better readability (split?) [Philippe Tulkens, Belgium]	Taken in account. The sentence has been shortened by reducing the amount of detail, focusing only on the main conclusions that support the low confidence level.
21433	6	28	6	28	Given the very simple geochemical process responsible for Ocean Acidification is this really requiring a likelihood qualifier? It is surely so unambiguous that the link to human elevation in CO2 can be reported as a fact? [Peter Thorne, Ireland]	Rejected. “Virtually certain” is consistent with the existing evidence. However “substantial” is replaced with “main driver”
98959	6	28	6	28	How can this be only "virtually certain"--this is pure chemistry. There is no doubt, so just eliminate "It is virtually certain that". Maybe instead at end of sentence say "extremely high confidence" or do what is done in Chapter 5, saying: "It is unequivocal that ..." [Michael MacCracken, United States of America]	Rejected. “Virtually certain” is consistent with the existing evidence.
7511	6	28	6	32	Higher temperature results in lower capacity for carbonic acid. Therefore this statement does not make sense. "The observed increase in CO2 concentration in the subtropical and equatorial North Atlantic since mid-2000 is likely in part associated with an increase in ocean temperature, a response that corresponds to the expected weakening of the ocean carbon sink with warming." I get that the ocean is absorbing CO2 and getting more acidic, but the use of the term "corresponds" does not follow. [Hugh Lefcort, United States of America]	Taken into account. The term “corresponds” replaced.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96265	6	28	6	32	Please explain that the higher CO2 uptake of the oceans is largely dominated by the higher concentration of CO2 in the atmosphere, and hence, its partial pressure. This would avoid the common misunderstanding that readers intuitively compare the ocean to a bottle with sparkling water, which looses CO2 when warmed. [Nicole Wilke, Germany]	Noted. This explanation is well detailed in Chapter 5, section 5.2.1.3.
99167	6	28	6	34	I would suggest to use "upper ocean" instead of "surface ocean" to be consistent with the paragraph on page 59, and the trends in ocean de-oxygenation. What is the definition of upper ocean in this chapter? There could be a cross check between chapters 3, 5, and 9. [Leticia Cotrim da Cunha, Brazil]	Accepted. Suggested change made.
17603	6	29	6	29	The term "acidification" is wrongly used here and creates a wrong message. pH of oceans is still well above pH=7. And pH values above 7 are not "acidic" [ferdinand meeus, Belgium]	Taken into account. Text revised and “acidification” is replaced with “acidity”
34599	6	29	6	29	I realize the term "ocean acidification" is used with some regularity, but it is technically inaccurate. The average pH of the ocean is about 8.1, wheres a pH less than 7 is acidic, so ocean water is slightly basic. A more precise choice of words would be, "...contributed to an increase in the relative acidity of the global ocean..." [Russell Vose, United States of America]	Taken into account. Text revised and “acidification” is replaced with “acidity”
18697	6	30	6	31	Is the increase in surface CO2 associated with surface layer warming or the increase in the vertical stability of the surface layer? This may be clarified. [Govindasamy Bala, India]	An explanation of this mechanism is provided in Chapter 5, section 5.2.1
18037	6	32	6	33	Why is deoxygenation placed within a section on acidification? It is directly linked to ocean warming and stratification. Please consider reference to SROCC discussion of deoxygenation rather than only citing AR5. [Lisa Levin, United States of America]	Text revised and SROCC referenced.
18699	6	33	6	33	"anthropogic forcing" => "anthropogenic CO2"? [Govindasamy Bala, India]	Rejected. Deoxygenation is not due to anthropogenic CO2 alone.
99851	6	33	6	34	Simulate over which period? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We chose not to add detail on the period to the ES, but the time periods are discussed in the underlying assessment.
98961	6	36	6	41	Would it not be appropriate to have this finding right along with the finding on Antarctic sea ice since the two findings are very likely related? [Michael MacCracken, United States of America]	Rejected. Many aspects assessed in this chapter are related, and it is impossible to align all the statements with their relationship.
77241	6	36	7	30	Many of these statements could be expressed more clearly . [Emer Griffin, Ireland]	Noted. We have tried to be clearer in the revised version.
84175	6	39	6	40	"last several decades" and "recent decades" should be better specified with numbers of the years/decade [Annalisa Cherchi, Italy]	Accepted. Period specified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98963	6	43	6	43	Using statistical jargon such as "robust" is a primary cause of the denier-created furor over the detection-attribution chapter authored by Ben Santer in the IPCC Second Assessment. Basically, this means, I presume, is that there is not two-sigma confirmation (so not yet 20 to 1 odds in favor of this finding) of the anthropogenic influence on the principal modes, etc. Well, this may well be the case, but two-sigma hypothesis testing is not the decision-making framework used by policy-makers or the public or the business community, and what this phrasing does is to essentially hold back information that there are some indications of changes (if that is indeed how "robust" is being used here. To avoid this, one has to avoid using statistical jargon. So, perhaps say "There are some, but limited, indications that anthropogenic forcing is beginning to have a larger influence than natural variability in affecting the principal modes of climate variability and associated regional teleconnections, with the exception of the SAM." In Santer's case, the SPM statement was that there was a "discernible human influence" and then lots of questions about what this meant and how it compared to the statement in the chapter that talked about not having robust or not having convincing confirmation, etc. In any case, the lesson to be learned is do not use statistical jargon, so language that means something specific to statisticians but that when quoted to the public ends up meaning something else. [Michael MacCracken, United States of America]	Taken into account. We have rephrased the statement and paid attention to the terms used in all Executive summary. Robust has been replaced.
110865	6	45		46	This sentence is in slight contradiction to the statement about variability in the attribution section further up. probably less so if the statement above gets a bit clearer [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The part of the sentence has been removed.
52835	6	45			Does climate change project onto the SAM or does the SAM respond to climate change? What about the PNA/PSA that could also deserve a specific assessment in both CH3 and 4? [Hervé Douville, France]	Taken into account. First comment taken into account. The link between the mean changes in the westerly jet (position and strength) and SAM is now mentioned in the ES. Second comment: Rejected. PNA and PSA are not assessed per se in AR6. They are addressed through the teleconnection paradigm in link to ENSO.
99853	6	49	6	49	poorly represented' rather than 'misrepresented'? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Misrepresented replaced by "poorly represented".
99855	6	49	6	50	Biases in what? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part of the sentence has been removed.
96267	6	50	6	50	It should be considered whether or not the word 'errors' should be replaced by 'uncertainties', since in line 51 it is explained that internal variability overwhelms the influences of anthropogenic forcings. [Nicole Wilke, Germany]	Taken into account. "Errors" has been rephrased by "biases", and "modelled" has been added to make it clearer.
127221	6	50	6	50	The word "errors" can be easily misinterpreted. Suggest using language to demonstrate the model outputs are inconsistent with observations or include bias, etc. [Trigg Talley, United States of America]	Taken into account. "Errors" has been rephrased by "biases", and "modelled" has been added to make it clearer.
99857	6	52	6	52	Is 'historical era' formally defined anywhere? If not, can this be changed? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The ES has been rephrased
84177	6	54	6	54	not clear if antrhopogenic aerosols have a positive/negative influence, on what phase of the AMV and over what period [Annalisa Cherchi, Italy]	Taken into account. The period has been added in the ES. On the other hand, we don't mention the sign of the phase because both the negative AMV phase in the 1960s and the positive phase since the 1990s have been shown to be affected by aerosols (strong emission in the 1960s and its reduction from the 1980s). We therefore used "Since the 1960s" formulation in the final ES

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10885	6	54	7	7	Much more care is needed to distinguish between the indices and the underlying modes of climate variability of AMV and PDV. Analysis and assessment of the indices of AMO and PDO seem to be used interchangably with the oceanic modes of variability. For instance there is discussion in sections 3.7.6 and 3.7.7 of the impact of anthropogenic aerosols on SST patterns which are used in the indices. But these are not necessarily influencing the modes of variability themselves. Conversely claiming modes of variability contribute to short term surface temperature trend variations risk circular reasoning as surface temperatures are often used to indicate what those modes are doing in the first place. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. When applicable, the distinction between the phenomenon and the metric used to describe the phenomenon (index) is now clearly made in section 3.7.6 and 3.7.7. In the ES, the term "index" has been added to the AMV to gain in precision.
110867	7	6			crude representation' that shouldn't really matter is it wrong or are there discrepancies? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part of the sentence has been removed.
127223	7	9	7	12	Suggest moving this paragraph higher up in the executive summary. [Trigg Talley, United States of America]	Accepted. This paragraph on extremes has been moved up to the section of the ES titled 'Human influence on the atmosphere and surface'.
110869	7	9		12	good that this is represented [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
98965	7	10	7	12	How does this finding relate to the finding about changes in monsoon rains- (not sure if it was in this summary or that of Chapter 2---are the two statements consistent. [Michael MacCracken, United States of America]	Noted. Unclear link with monsoon rain changes, which are assessed in a separate statement.
102811	7	11	7	11	Main cause or main driver (cf earlier comment on consistent use of terminology) [Philippe Tulkens, Belgium]	Taken into account. We used main driver.
19759	7	14	7	14	Certainly you do not want to state that the simulated climate is improved! What is improved would rather be the quality of the simulation. [philippe waldteufel, France]	Noted. We think it's clear what is meant here.
102813	7	14	7	16	At first reading this sentence it is not clear how the "mean climate" has improved. Please make this statement about model performance more accurate. [Philippe Tulkens, Belgium]	Accepted. We can't go into detail here as to exactly which fields were assessed, but we make explicit that this is for "recent" climate and versus "observational references". Hopefully this addresses the reviewer's concern.
35001	7	14	7	20	As written, I think this ES statement belongs largely in chapter 4, not here. Greater specificity on the time window of the comparison, as well as the implications for detecting the human imprint, would make this a stronger and more Chp 3 statement. [Baylor Fox-Kemper, United States of America]	Accepted. We now make explicit that the improvement is for "recent" climate but not so much for the paleo-periods covered here. It is not meant to refer to projections, so not a Ch4 statement. Regarding the implications for detecting the human response, this is not a focus of the section the ES statement refers to, so we prefer not to expand the statement in this direction.
52837	7	14	7	20	Could be expanded as: "Yet, observational and modelling uncertainties still place upper limits on the level of confidence in our assessment and improved model performance at simulating present-day climate does not warrant improved simulations of past and future climates."? [Hervé Douville, France]	Rejected. While we agree with the reviewer that his suggested addition is correct, the chapter does not explore any implications of model problems in reproducing past climates for future projections. The sentence would therefore be out of place here.
99859	7	14	7	20	Should this statement come earlier to provide confidence in the attribution assessments presented? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It could certainly come earlier. We prefer to keep the current order which follows the organization of the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99861	7	14	7	20	Given the wide range, I think a more detailed assessment of the historical simulations of GSAT/GMST is necessary. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. GSAT (not mentioned by name in the bullet) is amongst the variables for which the simulated annual-mean climatology has generally improved versus CMIP5 and CMIP3 (FAQ 3.2, figure 1). We agree with the reviewer that the "broad range of warming" is a concern, and that understanding this better would be a worthwhile scientific undertaking. However the fact remains that the simulation of historical GSAT has improved between model generations. This bullet point is the wrong location to explore in more detail the reasons for this behaviour.
9655	7	16	7	16	It is not just the high resolution models that exhibit reduced biases. I think you are trying to say two things in one sentence. Model resolution has increased. Biases have reduced. However combining the two results in an incorrect message in that not all the models are high resolution, some models with low resolution have reduced biases, and resolution is probably not the main driver for the bias reduction. [Olivier Boucher, France]	Accepted. This sentence is meant to refer only to the HighResMIP models. We now make clear that this statement is about the impact of only changing resolution.
26695	7	16	7	16	Resolution is not the only factor that has changed in models and other factors have also contributed to reduce model biases. The amphesises should not be only put on resolution and the statement should be completed. Resolution is only one aspect. Model improvement and resolution should appear as too different statements, it should be explicilty mentioned that models with low resolution have also reduced biases, suggesting that resolution is not the only factor of model bias reduction [Eric Brun, France]	Accepted. This sentence is meant to refer only to the HighResMIP models. We now make clear that this statement is about the impact of only changing resolution.
98823	7	17	7	17	Although a broad range of warming rates between models and a prolonged observational record mean that significant differences between the climate response in models and individual observations can often be identified, the average of several models captures most aspects of observed climate change well (high confidence). [Julio Cesar Barreto da Silva, Brazil]	Noted. This formulation is essentially the same as in the SOD. We think the "average of several models" could be misinterpreted as meaning we cherry-pick the models. This would be counterproductive.
17101	7	17	7	20	I suggest these changes: The significant differences between the climate response in individual models and observations can often be identified from While a broad range of warming rates across models and a lengthening observational record. The multi-model average mean captures most aspects of observed climate change well (high confidence).{3.8.2} [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It is not clear what changes the reviewer would like to see.
96269	7	17	7	20	Due to limited understandability we propose to rephrase the sentence beginning with 'while' to: "While there still are significant differences in climate responses between individual models and observations, which show up as a broad range of warming rates across models, the lengthening of the observational record enables the demonstration that the multi-model mean captures most aspects of observed climate change well." [Nicole Wilke, Germany]	Noted. Actually this is not what we mean. The lengthening record means there is more opportunity for models not to capture the most recent accelerated warming and other forms of climate change, i.e. the lengthening record poses a greater challenge for models to reproduce.
102815	7	19	7	21	The statement 'the multi-model mean captures most aspects of observed changes well (high confidence)'. Can this statement be justified given that terrestrial carbon cycle models omit interactions such as permafrost thaw, nitrogen cycle etc. as described in section 3.6 (p 56 lines 20-25)? [Philippe Tulkens, Belgium]	Noted. Yes it can be justified. It is clear that models are not perfect. By observed climate change we mean "of essential climate variables" (temperature, pressure, precip, etc.). It does not refer to shortcomings in the formulation of models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110871	7	19		20	most aspects' high confidence - somewhat vague statement is the confidence statement really useful here? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We agree it's a bit vague, but confidence statements are generally required for most sentences in the ES. We can't really go into detail in the ES about what exactly "most aspects" mean -- this refers to a basket of climate variables that is detailed in the main text.
116181	7		7		Based on model evaluation, what are particular points of attention regarding recent trends or lack of trends that differ between historical simulations and observtions, and implications for fitness for purpose and confidence in projections? Antarctic sea ice is a clear aspect not reflected in the ES. [Valerie Masson-Delmotte, France]	Noted. In the revised version, we now emphasize more explicitly and in a more consistent way: the sign of the trend, the model performance in simulating the observed trends and the attribution of the trends to natural (internal +solar/volcanoes): see for instance for Hadley/Walker circulation, NAM and SAM.
116183	7		7		A summary table of key findings from ch 3 would be very helpful to have an overview of observed trends, role of modes of variability, role of natural forcing (incl. volcanic forcing, direct and indirect effects on eg modes), and attributed trends (what has emerged, driven by which aspect). The issue of the validity of extrapolating past trends within climate information (see ch 10) needs careful consideration, and insights from ch 3-4 are needed on this (including changes in forcing trends and thus lack of "analogy" between past and projected responses). This includes aspects related to ozone recovery. The table could highlight whether the assessment supports / confirms/ Strengthens AR5 findings, what has changed, and what is novel in an IPCC context. [Valerie Masson-Delmotte, France]	Taken into account. A summary table for the Modes of variability, which summarizes the assessments from Chapters 2-3-4, has been added in the Technical Summary. A summary table about the teleconnection associated with the MoVs has been added in the Atlas.
19761	8	1	9	18	: In this short section 3.1, the word assessment or corresponding verb expressions is found 22 times (excluding document titles). The accurate meaning of this word is far from clear. On lines 7-8 one reads for example "in each case assessing human influence and evaluating climate models' simulations"; then on line 9: "This chapter assesses the evaluation and attribution of (…) indicators". What is meant by assessing human influence? Assessing an evaluation? Ideally one would be happy to understand an IPCC assessment as the fact of assigning a likelihood; but this simplistic option does not work when the assessment bears on literature. Methodological indications in section 1.4 do not provide all the answers. [philippe waldteufel, France]	Accepted. We have added reference to Section 1.3.4 which introduces the historical context on the assessment on the human influence in IPCC reports, and Cross-Chapter Box 1.4, which introduces attribution in IPCC assessments.
35423	8	1	9	18	It deals with important aspects of human influence on the climate system and to what extent climate models can simulate the changes and variability observed, guiding the deepening of the topic of simulations as a key resource for this evaluation. [Gladys Linares-Fleites, Mexico]	Noted.
71351	8	1	9	18	I find this scoping section a bit unbalanced towards attribution. While there are two major paragraphs (2nd and 3rd) dedicated to attribution, with reference to AR5, no comparable motivation of why evaluation is so crucial is given. I would suggest to add one paragraph between lines 45 and 47. This paragraph should also refer back to the discussion of fitness for purpose in Chapter 1. [Douglas Maraun, Austria]	Accepted. The scoping section has been expanded and additional motivation for evaluation is now expanded, specifically noting that we assess fitness-for-purpose for evaluation. We also refer to relevant discussion in Section 1.5.4.
37311	8	3	8	3	This paragraph should start with a statement of the IPCC's role so that readers understand why it focusses on human influences on climate. [John McLean, Australia]	Rejected. A discussion of IPCC role would be more general than the scope of our chapter, and is covered to some extent in Chapter 1. Also we already refer to UNFCCC and the Paris Agreement, which makes the relevance of attribution clear.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104957	8	3	8	10	The fundamental role of models in most formal D&A studies needs to be highlighted more prominently. It is mentioned in lines p8 line 34-37 but this intro into the use of models feels spread out. It needs to be more clear why they are an integral part of D&A. [Peter Gleckler, United States of America]	Accepted. We did already explain that an estimate of the forced response and variability is needed, which is usually taken from CMIP models. We have now generalised this to indicate that it is always taken from some form of model.
2543	8	3	9	37	this needs to be tightened; it is unclear what the scope really is [Bryan Weare, United States of America]	Taken into account. We have improved the introduction and added more cross-references to other chapters. This said, we believe that the final paragraph of the introduction gives a clear account of the scope and contents of the chapter.
52839	8	3			"This chapter assesses the extent to which the whole climate system has been affected by a human influence..." [Hervé Douville, France]	Accepted and changed as suggested.
52841	8	5			Temper or tell why since it is not necessarily intuitive that recent climate can be used to constrain climate projections? [Hervé Douville, France]	Rejected. We simply note here that understanding the causes of climate change 'informs our confidence' in projections.
65663	8	6	8	10	Please maintain consistency with the Paris Agreement goals as referenced in the Agreement. Suggest changing to: "Moreover, an understanding of the amount of human induced global warming to date is key to assessing how close we are to holding the increase in global average temperature to well below 2°C above pre-industrial levels and pursuing efforts to limit the temperature increase to 1.5°, as defined in the Paris Agreement of the United Nations Framework Convention on Climate Change (UNFCCC) 21st session of the Conference of the Parties (COP21, UNFCCC (2015))..." [Kushla Munro, Australia]	Accepted and Paris agreement now directly quoted.
40477	8	7	8	7	It would be more accurate to use the term goals rather than "targets" when referring to the Paris Agreement. It would also help to ensure consistency across chapters. [TSU WGI, France]	Taken into account. We now directly quote the Paris Agreement, and the term 'targets' is no longer used.
77243	8	7	8	10	The Paris Agreement temperature goal should be stated correctly and not interpreted, eg on 1.5C [Emer Griffin, Ireland]	Accepted and Paris agreement now directly quoted.
127225	8	7	8	10	There needs to be a qualifier in front of targets. There are no temperature targets per se in the Paris Agreement, but rather a long-term temperature goal. Please clarify. [Trigg Talley, United States of America]	Accepted and Paris agreement now directly quoted.
37241	8	8	8	8	The global average pre-industrial temperature, which I take to mean prior to 1750 (but where's the start year for that vague time, or is it only 1750?) is both unknown and unknowable. Claiming that 1850-1900 temperature data as an indicator is completely false because the so-called global average was heavily biased in the northern hemisphere towards European data because that's where most of the data was collected (and Europe was emerging from the Little Ice Age at the time) and in the Southern Hemisphere (SH) was biased towards the shipping routes used by European vessels. Further, as the CRUTEM4 station data tell us, a single weather station in the SH supplied the only data from January 1850 to July 1852 and the increase in the number of reporting stations was very slow, only averaging 78 in 1900. I refer you to McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" because it seems that for 30 years now the IPCC has failed to audit the primary temperature data that it uses. [John McLean, Australia]	Taken into account. A reference to Cross-Chapter Box 1.2, which discusses this issue, has been added where we introduce the 1850-1900 reference period.
98825	8	12	8	12	The evidence of the human influence [...] [Julio Cesar Barreto da Silva, Brazil]	Taken into account. Changed to 'The evidence of human influence'.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21437	8	16	8	16	In addition seems the wrong phraseology here as this sentence is comparative / a caveat so something like 'However' instead would seem more appropriate and readable. [Peter Thorne, Ireland]	Taken into account. This text has now been deleted.
127227	8	16	8	23	[PROGRESS] Strongly suggest dropping all this text. It is unclear why so much text is taken up here (in the scope and overview) with findings of AR5 (which are repeated later in the chapter as well). It is also unclear why the findings that seem the least certain from AR5 are those being highlighted in the second paragraph of this chapter. The reader comes away from this paragraph with the phrases "significant uncertainties" (line 16), "low confidence" (line 19), and "systemic biases" (line 22), which may imply that we knew very little in AR5. These sentences do not provide value in this section and should be dropped, or at the very least framed as important research gaps that this chapter addresses. [Trigg Talley, United States of America]	Accepted. Text deleted as suggested.
104929	8	22	8	23	Nonetheless, several systematic biases were detected (Flato et al., 2013). -> Nonetheless, persistent systematic biases were identified (Flato et al., 2013). [Peter Gleckler, United States of America]	Taken into account. Text has now been deleted.
39937	8	23	8	23	Any reason the SRCCL is not mentioned? No findings from this report were used in this Chapter? [TSU WGI, France]	Accepted and SRCCL now cited.
13317	8	24	8	24	Add the acronym used for the report 1.5 used further down SR1.5 [Maria Amparo Martinez Arroyo, Mexico]	Accepted and changed as suggested.
98827	8	26	8	26	(SROCC; IPCC, 2019). [Julio Cesar Barreto da Silva, Brazil]	Accepted and SROCC reference added.
11953	8	28	8	29	It should be corrected as "Cross-Chapter Box 2.2". [Masaki Satoh, Japan]	Accepted and changed as suggested.
127229	8	28	8	30	Cut first two sentences and mention the cross cutting box on line 35. [Trigg Talley, United States of America]	Rejected. The first sentence is important for setting the context on which variables are covered. The second describes progress relative to AR5.
64647	8	28	8	45	Clarify the reasons for adopting the simulation of the model from other models(CMIP5 and CMIP6) despite the presence of others with higher horizontal and spatial accuracy. The simulation model in this chapter is considered regional with average accuracy in determining human activity and the extent to which this is well defined. Therefore, please add explanatory reasons that we will see in the analysis and results of the chapter to its importance in Evaluating the effect of human activity and the importance of using simulation [Eman Abdelazem, Egypt]	Rejected. Our focus is on large-scale indicators of climate change, and hence we rely on global climate/earth system models. High-resolution models are assessed in this chapter in addition to the "standard" CMIP6 models. We also refer to Chapter 10 where regional models are assessed.
102817	8	28	8	45	Attribution and model assessment come together in this chapter and this is welcome. Around here a clarifying framing statement for the non-expert reader may be useful along the following lines: the human influence on the climate system cannot be measured directly. Rather, need models that can be run with and without anthrogenic drivers, and from this the human influence can be estimated. For this we need high quality models. We have confidence in models that can reproduce features of the real world. Better models can explain more features consistently. Hence in this chapter we assess in detail which of the features of the real world are described and explained by model. etc. (cf. p.12, l33) [Philippe Tulkens, Belgium]	Taken into account. A modified version of this statement has been added.
21439	8	29	8	29	It is cross-chapter box 2.2 not 2.1. Sorry! [Peter Thorne, Ireland]	Accepted and changed as suggested.
104931	8	30	8	30	most dataobservational datasets -> many observational datasets [Peter Gleckler, United States of America]	Accepted and changed as suggested.
81481	8	31			What does it mean internal? Time (inter-seasonal) or spatial (scale) Please describe clearly the meaning of internal. [Kyaw Moe Oo, Myanmar]	Accepted and clarified.
98829	8	34	8	34	(CMIP6; Eyring et al., 2016a) [Julio Cesar Barreto da Silva, Brazil]	Accepted and changed as suggested.
88945	8	36	8	37	Past1000 simulations can be used to assess internal variability as well. [Schurer Andrew, United Arab Emirates]	Taken into account. We have added assessment of past1000 years to the chapter.

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10571	8	39			Is "Jones et al., 2016b" the correct reference? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. It was incorrect, and has now been deleted.
104933	8	41	8	43	primarily based on studies using the CMIP6 -> emphasizing studies using the CMIP6 [Peter Gleckler, United States of America]	Accepted and changed as suggested.
127231	8	41	8	44	Cut this sentence. [Trigg Talley, United States of America]	Accepted and sentence deleted.
10573	8	47	9	4	This is really important, and I am very glad to see it here. Has there been any attempt to use available information on whether models have been tuned to observed trends in this chapter's assessment? For instance 1.5.3.2 is aware of 6 out of 29 models that were tuned to observations. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We present the information in the chapter and cite Bock et al. (2020).
11297	8	47	9	4	Advantage of assessing CMIP6 models and D&A together in a Chapter has been clearly stated at the beggenning -- It's good. However, Chapter could discuss little the extent to which model systematic errors affect results of D&A in a quantitative sense. For doing such assessment, I wonder you need to use emergent constraints (ECs) on each of specific aspects of climate changes, but there was no mention to ECs in the remaining text. An example is an EC for the sea ice albedo change by Thackeray and Hall (2019), which may be applied to attribute the past sea ice reduction in 3.4.1. Thackeray and Hall, 2019: An emergent constraint on future Arctic sea-ice albedo feedback . Nat. Clim. Chg., 9, 972–978 (https://www.nature.com/articles/s41558-019-0619-1) [Masahiro Watanabe, Japan]	Taken into account. Emergent constraints are not assessed in this chapter, but we explicitly mention them now and refer to other chapters.
83597	8	52			"Where simulated and observed 53 changes are consistent, this can be interpreted both as supporting attribution statements, and as giving 54 confidence in simulated future change in the variable concerned. However, if a model's simulation of 55 historical climate change has been tuned to agree with observations, or if the models used in an attribution study have been select ed or weighted on the basis of the realism of their simulated climate response, this 2 information would need to be considered in the assessment and any attribution results correspondingly 3 tempered: an integrated discussion of evaluation and attribution supports such a robust and transparent 4 assessment." Excellent statement – it deserves to be highlighted in gold lettering in all discussions of the significance of natural cycles in global climate change. [michael asten, Australia]	Noted.
19505	8	55	8	55	after historical cliamate change add "data" [Hamideh Dalaei, Iran]	Accepted and changed as suggested.
71353	9	6	6	18	Here I would include a link to Chapter 10 stating that (1) Chapter 3 provides a basis for some aspects of Chapter 10 (in the sense that a credible representation of the large-scale aspects of climate change is a prerequisite for having credible regional projections. And (2) that evaluation of regional aspects can be found in Chapter 10.3.3. And (3) that some regional attribution case studies will be handled in Chapter 10.4 and 10.6. [Douglas Maraun, Austria]	Accepted and link to Chapter 10 included.
11955	9	13	9	13	Remove ":" from "Box 3.1:", "Box 3.2:", etc. Unnecessary ":" frequently apears everywhere in this chapter. [Masaki Satoh, Japan]	Accepted and changed as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44221	9	13	9	15	It should also be mentioned here, where the interactions and feedbacks between the different spatial and temporal scales are discussed in this report. [Nektarios Chrysoulakis, Greece]	Accepted. The implications of our assessment of human-influence of large-scale indicators of climate change for changes on regional scales is assessed in Chapter 10.
68055	9	21	11	3	Section 3.2: could a schematic figure be added to illustrate both the similarities and differences bewteen the D&A techniques used? Make the link to what was done in AR5 and what's new here? [MIchael Evans, United States of America]	Rejected. FAR and AR5 provided schematic figures and we cited them.
35425	9	21	11	5	Introduces new techniques for process-based assessment of Earth system models against observations. It also develops assessment tools for faster and more comprehensive assessment of models with observations [Gladys Linares-Fleites, Mexico]	Noted. This is described in Section 1.5 as we mentioned at the beginning.
71355	9	21	11	5	Here I would add a new paragraph on methods for climate model evaluation. Otherwise, again, evaluation is devalued. A focus should be on process-oriented evaluation. This paragraph should also include a discussion of sources of model errors such as limited resolution, missing processes, parameterisations, missing forcings, scale interactions etc. [Douglas Maraun, Austria]	Noted. However these are already assessed in Chapter 1, Section 1.5 and not repeated here.
52845	9	21			Beyond D&A methods and given the lack of methodological introduction in Section 3.7, a brief reminder of the method used for distinguishing changes in variability from changes in mean state could be useful. This framework could be also/rather clarified in the foreword of Annex VI given the shared definition of modes of variability within CH2, 3 and 4. The large ICE could be used more extensively to assess changes in variability (i.e., after removing the forced mean state response). [Hervé Douville, France]	Rejected. Methods assessed in this Chapter are limited to D&A only. Annex or Chapter 1 could be appropriate for that.
18333	9	23	9	23	Section 1.4 did not discuss model evaluation method! Section 1.5.4 appears to have discussed some of the methods used to evaluate models in the literature. [Aiguo Dai, United States of America]	Editorial. Section 1.4 changed to Section 1.5
37247	9	23	9	31	Stop trying to pretend that "evaluation" is the same is "validation". [John McLean, Australia]	Noted. This is relevant for definition of model "evaluation" and is covered by section 1.5.
64649	9	23	9	35	Must clarify the accuracy of the simulation model in general here and the possibility of determining the scope of human influence through the model, this is especially useful academics and professionals in general [Eman Abdelazem, Egypt]	Rejected. This section only focuses on methods whereas model evaluation and detection and attribution studies are assessed from section 3.3 on.
11957	9	24	9	24	Section 1.4 shoule be 1.5, for referring to model evaluation tools. [Masaki Satoh, Japan]	Editorial. Section 1.4 changed to Section 1.5

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104987	9	26	9	31	Using ESMValTool for the figures in this chapter represents an important step to improve traceability and quality control of the IPCC process. However, it is not representative of the progress that has been made in the field toward the systematic evaluation of CMIP as advocated in Eyring et al. (2019b). Specifically, as described in that paper, the PCMDI Metrics Package (PMP) has been advanced to serve a similar but more focused goal - providing traceable summary statistics across generations of CMIP. PCMDI has contributed performance metrics to the WGI model evaluation chapters since the SAR and is prepared to continue doing so. Results from ENSO (Planton et al, 2020) and extra-tropical modes (Lee et a., 2019) used in the draft CH03 were integrated into the PMP by the authors of these papers. In both cases, the calculations are complex with many processing choices. To ensure not just transparency but also accurate representation of these published works, the CH3 LA's should work with PCMDI to get the results needed related to these papers. With the results PCMDI provides as data - thoroughly documented with provenance on an established public repository - figures can be produced with ESMValTool software to ensure visual consistency with others in the chapter. To not recognize or leverage this complementary effort misrepresents progress in the field. I suggest the following addition to the text: "Results from several figures in this chapter have been produced with the PCMDI Metrics Package (PMP; Gleckler et al., 2016) and visualized via ESMValTool." [Peter Gleckler, United States of America]	Noted. Please refer to Chapter 1, Section 1.5 where PMP is assessed. It is to be noted, as described in the text, that ESMValTool is used for plotting purposes in this Chapter (Page 9, line 28-29).
127233	9	33	9	35	Move this information after the first sentence on line 23. [Trigg Talley, United States of America]	Rejected. We put model evaluation method first and then focus on detection and attribution methods.
19763	9	34	9	34	Probably 1.4 rather than 1.5 [philippe waldteufel, France]	Editorial. Cross-Chapter Box 1.5 changed to 1.4.
66969	9	36	9	36	"Optimal fingerprinting" has been the dominant terminology in published literature (and I unfortunately contributed to this), but to avoid confusion, could you just speak about "linear regression" methods instead? [Aurélien Ribes, France]	Taken into account. We revised it appropriately.
781	9	38	10	11	Many of the publications are outdating (1-2 decades old). Suggest to cite more recent works [Baruch Rinkevich, Israel]	Noted. We cited old papers only when needed to explain recent studies in view of them.
127235	9	38	11	5	[PRECISION] It is not clear who the audience for 3.2 is. This is almost unintelligible if you are not already fairly well read in the relevant literature, and it also contains no assessment statements. Might a box that is better suited to bringing a less well-informed reader up to speed be more appropriate? [Trigg Talley, United States of America]	Taken into account. Text revised to be accessible to readers.
21441	9	38			Section 3.2.1 felt like a very good synopsis but the lack of a concluding paragraph tying it all together and telling me what the chapter team feels it means in terms of the advances since AR5 - what do all these innovations mean at the bottom line - hurt the section as a whole. I would suggest due consideration of addition of a brief summary paragraph that ties everything together and tells the reader what that means practically for the in-depth per domain assessment that follows would be useful. Either that or sum up what both 3.2.1 and 3.2.2 mean for your assessment just before 3.3? [Peter Thorne, Ireland]	Accepted. We added a summary as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87929	9	40	10	37	The summary of optimal signal detection methods makes no mention of the common practise of filtering climate data by "projection onto spherical harmonics". For a revision of a paper submitted to J Clim I have been asked to take account of this step, since in its absence the conventional attribution results weaken substantially and the reviewers insist that filtering using spatial harmonics is an integral part of the methodology. The trouble is that the method has (as far as I can see) never been explained in print, and recent papers simply refer the readers in a chain back to papers in the late 1990s which in turn refer to an unpublished 1972 Danish technical report. AR5 only briefly and tangentially alluded to it. There are no papers that provide sensitivity analyses on a with/without basis (I corresponded with Gareth Jones and while he thought he had once seen one he couldn't recall what it was). My impression is that filtering via spherical harmonics is a "secret sauce" that boosts the significance of results but also introduces bias in the process, that its usage has not adequately been debated and for some reason authors have not been asked to explain their methods or to report on sensitivity of results to its usage. My suggestion is that this section should highlight this matter and caution the reader that a lot of the results to be discussed appear to depend on an arbitrary filtering step and may not be robust to its removal. [Ross McKitrick, Canada]	Taken into account. We added dimension reduction procedure in this respect.
127239	9	40	10	37	[PRECISION] Much of this section could be cut or summarized. This section includes a lot of background and older citations (not the 'new methods and improvements" the previous paragraph said it would include). It also has a lot of statistical jargon that feels unneccesary. Much of this section is just a listing of papers, not a synthesis or assessment. Even the findings reported are not stated boldly (the authors of the papers are said to have "proposed" or "suggested" rather than "found" or "concluded"). It is neither clear what is new or improved, nor how these papers/findings relate to climate. The text on lines 47-50 is a good example of how such findings could be shown to be applicable to to climate change. [Trigg Talley, United States of America]	Taken into account. We added a summary with relevant assessment statements.
127237	9	40	11	5	[PRECISION] Sections 3.2.1 and 3.2.2 will be very difficult to follow for anyone who does not hold an advanced degree in statistics. Suggest boiling this language down if possible so that a larger fraction of readers will understand what it is saying. Can the main point of these two sections at least be more clearly laid out up front, so that people wo do not understand the rest will know what they're missing? [Trigg Talley, United States of America]	Taken into account. We added a summary in the end for better understanding.
110873	9	42		43	the citations in line 43 are all for the first part of this sentence - extention to other variables are different papers and you could cite some classics there - eg Zhang et al 2007 for precipitation . Hasselmann 97 definitely needs to go to the first bracket but i recommend all these quotes going up there with examples for 'classic' papers on other variables. could also be Schnur and Hasselmann; Min et al;. 2008; 2011; maybe a gillett paper?) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We changed as suggested.
37249	9	43	9	49	The key word is "assume" (line 44) but you fail to mention what happens if the assumption is wrong. [John McLean, Australia]	Noted. We added that this assumption usually holds.
37251	9	43	9	49	I put it to you that there are no grounds for assuming a linear relationship. Further, no relationship, linear or otherwise, can be projected with any confidence unless the relationship and all of the factors are understood, including interactions between components, so your statements are nonsense. Also, even previous IPCC reports have argued that the warming theoretically caused by CO2 is logarithmic, not linear, so assuming a linear relationship is not appropriate when considering CO2 changes. [John McLean, Australia]	Noted. Linear relationship here is between observed change with model-simulated fingerprint pattern for the same variable.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11959	9	48	9	48	"inconsistent with zero": It is better to state "robustly different from zero". [Masaki Satoh, Japan]	Accepted. Changed to "significantly different from zero".
102819	9	49	9	49	insert "the" between "of" and "regression" [Philippe Tulkens, Belgium]	Accepted. Corrected.
110875	9	55			this 'optimization is usually applied' is already less strong than the heading and below are cases without. Optimization is a technical detail that doesn't need to be in header [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We rephrased it appropriately.
116185	9		9		Links to the notion of emergence from ch 1 are missing in the introduction. [Valerie Masson-Delmotte, France]	Accepted and links to the notion of emergence added by referring to Chapter 1.
37253	10	1	10	7	You are using model-simulated by (not of?) internal variability and then testing the results against the output of models? This isn't science; it's computer games. [John McLean, Australia]	Rejected. Observed residual variance is compared with simulated internal variability as explained.
37255	10	13	10	25	Is this deliberate obfuscation? State what all this means in a single clear sentence. [John McLean, Australia]	Noted. We added a summary at the end of section 3.2.
90231	10	13	10	25	Two additional references on methods for detection and attribution. [Richard Smith, United States of America]	Rejected. This comment doesn't specify which two additional references on methods we should include.
90233	10	13	10	25	Noting the recent proliferation of methods for detection and attribution, Lenssen et al. (2018) developed a simulation testbed for the comparison of different techniques. Their testbed has the ability to generate a wide class of isotropic and non-isotropic correlation matrices to simulate the climate variability. The forcing response fields are tunable, spatially correlated fields with adjustable signal-to-noise ratios. The flexibility of the simulation method allows for replicating a variety of climate model-like output in a controlled setting. In addition to the methods used in the testbed, synthetic data for simulated climate scenarios and a user manual were also included. [Richard Smith, United States of America]	Taken into account. Suggested literature assessed.
90235	10	13	10	25	N.J.L. Lenssen, A. Hannart and D.M. Hammerling (2018), Simulation Testbed for Trend Detection and Attribution Methods. NCAR Technical Note NCAR/TN-555+STR, National Center for Atmospheric Research, P. O. Box 3000, Boulder, Colorado 80307-3000 [Richard Smith, United States of America]	Taken into account. Suggested literature assessed.
90237	10	13	10	25	Hammerling et al. (2019) gave a comprehensive review of methods for detection and attribution, including detection and attribution of extreme events, aimed at a statistical readership. [Richard Smith, United States of America]	Taken into account. Suggested literature assessed.
90239	10	13	10	25	Hammerling, D., Katzfuss, M. and Smith, R.L. (2019), Climate Change Detection and Attribution. Chapter 34 of Handbook of Environmental and Ecological Statistics, edited by A. Gelfand, M. Fuentes, J. Hoeting and R.L. Smith. Chapman and Hall/CRC Handbooks of Modern Statistical Methods, pp. 789-817. [Richard Smith, United States of America]	Taken into account. Suggested literature assessed.
110877	10	15			there is also mention of other uncertainties explicitlty in hasselmann papers. So cite eg Hasselmann 1997 here (ir could be in the 1979 but I think its in 97) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Suggested literature assessed.
37257	10	27	10	37	This paragraph is as clear as mud. Is that deliberate? If not then explain things in simple terms. [John McLean, Australia]	Noted. We added a summary at the end of section 3.2.
35571	10	29	10	29	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. This paper is now published.
35573	10	29	10	29	Bibliographic citations in chronological order [Carlos Antonio Poot Delgado, Mexico]	Accepted. Changed.
110879	10	43			explain philosophical difference in Ribes et al 2017 - assumes pattern uncertain as well, at the cost of the observations being required to be within the range of models not outside. Also, here Schurer et al., 2018 (sorry self serving) is also an innovation introducing a Bayesian framework to use the pattern response uncertainty beyond the multimodel mean; and find that results based on the multimodel mean can be overconfident [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Suggested literature assessed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102821	10	45	10	45	"who linked it to Kriging" - unclear [Philippe Tulkens, Belgium]	Taken into account. We have removed the reference to Kriging, since it was not needed.
37259	10	48	10	50	This bald statement is merely an assertion because you fail to clear explain the techniques, which appears to have been created specifically to torture data into giving the output you want. [John McLean, Australia]	Noted. We added a summary at the end of section 3.2.
98831	11	1	11	1	[...] indicated that the combined use of temperature-precipitation spatial structure can be more accurate. [Julio Cesar Barreto da Silva, Brazil]	Accepted. Changed.
52847	11	3	11	5	Did you use this approach in this chapter? If yes, could you summarize here this approach? If no, why? [Hervé Douville, France]	Taken into account. We added this for completeness.
98205	11	5	12	2	Please be comprehensive and quantitative. Use numbers to describe the data-model agreement. The fact that they have improved says little if they are still miles apart. How much do they differ? Include other well-studied paleo reference periods for a more complete assessment of the ability of models to simulate GMST under different forcings, not just Pliocene and Eocene. Include LGM, LIG and HM. The comparison between CMIP5 and CMIP6 is less important than addressing the fundamental policy-driven question: How well do models simulate GMST under conditions different than the last century? [Darrell Kaufman, United States of America]	Taken into account. More quantitative information on comparisons now included.
110881	11	5			it would be useful to end this section with a sentence on the effect of these new methods. My reading is that the new methods and better incorporation of uncertainties improve on shortcomings and increase confidence, but do not lead to a substantial revision of results. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We added a summary in the end as suggested.
99863	11	6	11	6	A summary of which methods have been adopted in AR6 and why would be helpful here. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Clearer information on which methods are used now included.
85023	11	8	12	30	No comments [Katrine Husum, Norway]	Noted.
64651	11	9	11	9	Is it possible here for the importance of the point of the impact of urban human activity on the global climate change through the elements of the atmosphere in a point to boot or a line within each element below [Eman Abdelazem, Egypt]	Rejected. The chapter focuses on the large scale, where urbanisation effects are small. See also section 2.3.1.1.3 where urbanisation impacts, or lack thereof, on instrumental temperature are mentioned.
10059	11	10			You are discussing human influence on temperature in this chapter. But where do you actually discuss systematically natural influence on temperature? It is well known that “modes of variability” such as AMO, NAO, SAM and ENSO influence regional (and together global) temperature on decadal to multidecadala timesscales. This is significant. You can only detect and quantify the anthropogenic contribution to temperature trends when you have described and understood the natural component. For example, Lüdecke et al. 2020 (doi 10.1016/j.jastp.2020.105294) have systematically studied natural variability of European temperature and likely natural drivers. It would be important that you provide the same room in your report to natural factors as to human factors, otherwise the IPCC could be critizised to pay too little attention to natural factors. This is even more important as chapter 3 describes that climate models still very much struggle to replicate this natural climate variability. [Sebastian Luening, Switzerland]	Rejected. The chapter is about human influence, so natural drivers of temperature change are not the focus. However, the chapter does discuss the importance of properly accounting for natural variability in a detection and attribution context.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28301	11	12	11	27	If it not yet has been done, suggest to carefully consider whether "surface temperature" is the best name for this parameter. It seems that the paragraph is about air temperature 2 m above ground level. Calling this surface temperature always causes some misunderstandings between sub-disciplines and my impression was that for good reasons we are moving away from it. Especially non-meteorological readers might be confused. "Surface air temperature" as used later in the caption of Fig. 3.5, while still the word "surface" in it is misleading, might at least be a compromise. [Alexander Graf, Germany]	Rejected. Although most of the discussion uses GSAT, there are aspects based on GMST as well. In addition, the chapter uses the same indicators as Ch2, which uses the term "surface temperature".
37293	11	12	15	51	(Most of this section) IPCC AR5 compared model projections for the previous 15 years to data from temperature observations. Surely if AR5 had outputs from models to do that then there should be no problem with AR6 doing that. Please include those projections. [John McLean, Australia]	Rejected. That discussion is in Cross Chapter Box 2.1.
19765	11	12	19	30	This 3.3.1.2 section, while very developed and interesting, is not easy to follow. Following a part (P11 L29) devoted to the paleoclimate context, comes a subtitle concerning model evaluation (P12 L32). On P13 L5 does the sentence beginning with "in summary" announce the conclusion? Not yet: it turns out that that part was devoted to patterns, whereas a second act of model evaluation is concerned with historical performance until a "in summary" sentence P14 L4 closes it. Next there is a part with no subtitle which mostly deals with internal variability. Next comes (P15L54) the "detection and attribution" subtitle; P16 L15 a mention of expert judgement will encourage to look a bit outside the numerical simulation universe. Then comes a particularly interesting discussion (P16 L14-44) about the challenge of separating contributions even within the anthropic influences. The "in summary" sentence on P18 L47 announces a rather long conclusion and finally the end of the subsection, confirming plainly the statements of the report1.5. It is perhaps possible to make this subsection less heavy by removing some elements which address continental scales rather than the global one. In any case, some work on the intermediate title might help the reader to find his way. [philippe waldteufel, France]	Accepted. The paleo context subsection has now been subsumed into the Model Evaluation and Detection and Attribution subsections. The Model Evaluation section now has a single summary paragraph. The lengthy summary paragraph of the Detection and Attribution section is needed as important assessments, which populate the Executive Summary, are made there.
104601	11	14	11	14	A novel result would be better to cite here by updating '(Collins et al., 2010; Shepherd, 2014).' as '(Collins et al., 2010; Shepherd, 2014; Zhou et al., 2020).' Zhou et al. (2020) found anthropogenic contributions in the increased probability of summer extreme heat in Northeast China from dynamic and thermodynamic point of view, which provides a physical way to better interpret formation and evolution of temperature extremes. Reference: Zhou, C., D. Chen, K. Wang, A. Dai, and D. Qi, 2020: Conditional attribution of the 2018 summer extreme heat over Northeast China: Roles of urbanization, global Warming, and warming-Induced circulation changes. Bull. Am. Meteorol. Soc., 101, 71-76. [Chunlüe Zhou, United States of America]	Rejected. The discussion here is about thermodynamics at the global scale.
37261	11	14	11	18	Despite relying heavily on the HadCRUT4 temperature data, the IPCC has never audited it (see 5AR chapter 2 review comment 1106). I refer you to McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" which identifies more than 70 areas of uncertainty about the HadCRUT4 temperature record, some of those areas applying to just a few data values but others applying to most of the data. Any argument about agreement with other near-surface datasets is pointless when they all obtain data from the same sources. (Chapter 2 states that routines are shared between different climate models and that the assessment of models has to take this into account. The situation with shared temperature data is obviously similar.) [John McLean, Australia]	Rejected. Those considerations are the remit of Chapter 2 -- see their discussion in section 2.3.1.1.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127241	11	14	11	18	This sentence is long and confusing. Suggest: "Surface temperature change is the aspect of climate in which the climate research community has had most confidence over past IPCC Assessment Reports, largely because of relatively good long-term observations. The response to anthropogenic forcing is large compared to variability in the global mean, allowing detection from just a day of observations (Sippel et al., 2020), and a strong theoretical understanding of the key thermodynamics driving its changes (Collins et al., 2010; Shepherd, 2014)." [Trigg Talley, United States of America]	Accepted. Sentence split into two to clarify that we are listing the three reasons for the better confidence.
34865	11	14	11	27	It beggars belief that an unusual 1-day temperature could be attributed to climate change. [Jim O'Brien, Ireland]	Accepted. The sentence mentions detection, not attribution, but it has been clarified that that detection is global. Regional detection needs much longer time series.
21443	11	15	11	15	relatively good long-term observations risks being compared and contrasted with chapter 2 which has concluded that the AR5 assessed datasets on a like-for-like comparison have been shifted c.12% in the estimate of 1880-2012 trends reported. I would maybe dispense with such a qualitative descriptor which may be used in combination with the chapter 2 assessment to cast aspertions. Is such a statement truly necessary? [Peter Thorne, Ireland]	Accepted. The sentence has been clarified by adding "compared to other indicators"
52849	11	16	11	17	Make a more explicit statement about what is meant here by detection from just one day of observations (could be misleading since it does not necessarily applies at the regional scale) [Hervé Douville, France]	Taken into account. That statement was an unnecessary side remark and has been deleted.
6585	11	17	11	17	Sippel et al.'s study did not use direct observations, but rather reanalyses. As reanalysis uses data assimilation, the analysis for any particular day is influenced by observations not only from the latest day, but also from earlier days, from which observational information is carried forward by background forecasts. One could get round this by changing "just a day of observations" to "weather data for just a day". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. That statement was an unnecessary side remark and has been deleted.
102823	11	17	11	17	replace "a day" with "one day" [Philippe Tulkens, Belgium]	Accepted. Done as suggested.
2545	11	17			what is meant by "detection from just a day of observations"? [Bryan Weare, United States of America]	Taken into account. That statement was an unnecessary side remark and has been deleted.
110883	11	17			just a day of observations' is not really clear what this means can you expand slightly to explain it? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. That statement was an unnecessary side remark and has been deleted.
37263	11	18	11	21	It doesn't matter a damn what AR5 found if its conclusions were based on hogwash, which they were. That AR5 showed that climate models were inaccurate - "... an analysis of the full suite of CMIP5 historical simulations (...) reveals that 111 out of 114 realisations show a GMST trend over 1998–2012 that is higher than the entire HadCRUT4 trend ensemble ...." [WGI contribution, chapter 9, text box 9.2, page 769, and in full Synthesis Report on page SYR-8] - and yet AR5 used those same flawed models to assert that mankind had caused half of the warming shows the lack of integrity of these reports. [John McLean, Australia]	Rejected. The AR5 discussion quoted in the comment relates to the "hiatus" period. CMIP5 models did indeed struggle to simulate that period but the reasons for that are now better understood and do not put the physical accuracy of models in question. See Cross Chapter Box 2.1.
112649	11	19	11	20	Should update the years and extend timing until 2017 or further. This information is from 2017, stating the same. https://www.carbonbrief.org/analysis-why-scientists-think-100-of-global-warming-is-due-to-humans [Melissa Jiménez Gómez Tagle, Germany]	Rejected. That statement summarises the AR5 assessment, which covered the period to 2010. We are now able to make an assessment to 2019.
37265	11	21	11	24	Why do you keep ignoring the fact that global coverage did not exceed 50% until 1904 and coverage of the southern hemisphere did not regularly exceed 50% until 1949? These factos make a mockery of data prior to 1950, which I suspect is why IPCC reports make claims about man-made warming only from 1951 onwards (see previous sentence of the SOD). [John McLean, Australia]	Models are sampled according to the availability mask of observed temperature datasets, so changes in coverage with time are accounted for in the comparisons.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37267	11	21	11	24	High confidence (established how?) about models reproducing general features? That's incredibly subjective regards so-called confidence and what " reproducing general features" might mean. [John McLean, Australia]	Rejected. That statement summarises the AR5 assessment, which provide evidence behind their high confidence. They clarified their use of the word "features" by giving the example of the more rapid warming in the second half of the 20th century and the cooling immediately following large volcanic eruptions, but repeating those examples would extend the summary unnecessarily. The focus on the section should be on the new assessment.
104935	11	22	11	22	something needs to be said about lower confidence at smaller scales otheriwise this can be percieved as misleading [Peter Gleckler, United States of America]	Rejected. That statement summarises the AR5 assessment, and they did not make an executive summary statement on modelled surface temperature trends at smaller scales than global.
37269	11	24	11	27	If CMIP6 models can't simulate all aspects then you should explain where they fail. [John McLean, Australia]	Accepted. The chapter is about human influence, so the discussion focuses on model fitness for the purpose of detecting and attributing changes. The sentence now clarifies that the aspect discussed are patterns, trends, and the statistics of variability.
52851	11	25			suppress "the current generation of"? [Hervé Douville, France]	Accepted.
68057	11	30	11	32	This is incomplete: paleoclimatic D&A requires (1) observations or reconstructions (2) independent radiative forcing estimates, and (3) realistically forced climate simulations based thereon (e.g. from PMIP4). The logic has been: where there are consistencies between (1) and (3), diagnose either (2) or (3) to identify the mechanisms most likely to explain features in (1), and then test those hypotheses using simplified models, single forcing experiments, and against the null of no forcing. [MIchael Evans, United States of America]	Rejected. The current statement covers the uses listed in the comment, but in a more general way.
88943	11	34	11	36	Do the comments made on page 12 lines 8-14 about the trends for the mid-holocene to present agree or contradict this statement from AR5? More of a link should be made between these two points. [Schurer Andrew, United Arab Emirates]	Taken into account. The statements are now better linked thanks to moving the paleo context section into the model evaluation and attribution subsections. Model problems in the mid-Holocene are not large enough to affect the attribution of the past 5000 year cooling trend to anthropogenic forcing.
127245	11	34	11	36	This sentence is confusing. Which has high confidence? That the cooling trend reversed or that this is attributable to anthropogenic forcing or both? Not clear as the sentence is written. [Trigg Talley, United States of America]	Accepted. The sentence has been clarified by saying "and attributed the reversal to anthropogenic forcing with high confidence."
11485	11	34	11	49	Candid question: Is Ruddiman's Early Anthropocene hypothesis still alive? Would it be worth mentioning/discussing it here? [Gerhard Krinner, France]	Noted. The hypothesis is still alive, and may contribute to the role of greenhouse gas forcing over the period discussed, which is now mentioned following comment number 10871.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68059	11	34	11	49	Many of the studies cited in this paragraph rely on larger compilations of paleoclimatic data with more complete metadata collected for their interpretation (e.g. Emile-Geay et al 2017; PAGES2K Consortium 2013, 2019). I would add that Neukom et al (2019) produced estimates using D&A, of the multidecadal GMST variability. This is important because this speaks to the question: how unusual are the recent changes, given the natural forcings on similar timescales? They showed that the atrribution for the preanthropogenic past millennium, using PMIP3/CMIP5 simulations, was to clusters of volcanic events; for the last 1-2 centuries, primarily the well-mixed GHGs. The residual of the D&A was speculatively shown to be not inconsistent with the unforced variability on these timescales from unforced PMIP3/CMIP5 simulations, and reconstructed from a forcing-'quiet' period, 850-1100CE. All three estimates have medians between 0.03-0.04 degC. I think this is useful additional context to add to this paragraph, and it adds to D&A results ands conclusions on more recent timescales and from direct observations. This also supports the results which are described at pg 14, l. 27-42. [MIchael Evans, United States of America]	Accepted. A context sentence has been added, noting that the attribution to greenhouse gases is unusual in the longer-term context.
127243	11	34	11	49	This information seems ripe for a simple diagram. [Trigg Talley, United States of America]	Taken into account. Figure 1 has been revised to show modelled and reconstructed temperatures over the last millennium, and also shows volcanic forcings, so now acts as a good visual guide to forcing attribution over that period.
37271	11	36	11	39	You are pretty foolish if you think McGregor et al (2015) is accurate. Even in 1850 SST data was only available for about 350 grid cells (c.f. the maximum that reported to December 2017 of 1579). In 1850 the average number of observations per grid cell was 6 with many grid cells reporting fewer observations than that, but even in 1970, which is well before Argo buoys were in common use, the average number of observations per grid cell was 138. [John McLean, Australia]	Rejected. That sentence has been deleted, since it was in the remit of Chapter 2 and those aspects are discussed in section 2.3.1.1.2.
37273	11	36	11	39	What "trend reversal"? The only trend you mention is for the high latitudes of the Northern Hemisphere but this sentence talks about ocean temperatures (presumably global), tropical ocean temperatures, NH land temperatures and SH temperatures. [John McLean, Australia]	Rejected. That sentence has been deleted, since it was in the remit of Chapter 2 and those aspects are discussed in section 2.3.1.1.2.
37275	11	36	11	39	What is your source for your claim about 5000 years of cooling, which you claim was in the high latitudes of the NH? (1) Chapter 2 (pg 94) talks about 5000 years of WARMING. (2) Data from GISP2, which is certainly in the high latitudes of the NH, is readily available from Alley (2004) and it plots as shown on web page http://mclean.ch/climate/Ice_cores.html. It does show a small cooling trend but that's because the last 750 years has been the longest cold period of the last 10,000 years and for more than 80% of the time temperatures have been about 1 degree warmer. [John McLean, Australia]	Rejected. The statement is from the Executive Summary of AR5 Chapter 5, with evidence presented in their Section 5.5.1. The reviewer is incorrect that Chapter 2 FAQ 2.1 talks of 5000 years of the warming. It reads "Prior to the 20th century, global average temperature was slowly decreasing for as long as 6000 years." (2-94:19-20). The Central Greenland ice core dataset by Alley (2004) ends in 1855 so does not capture anthropogenically forced warming.
2081	11	36	11	49	This paragraph needs some calibrated language, i.e. confidence and/or likelihood statements. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paleo context subsection has now been subsumed as a line of evidence into the Model Evaluation and Detection and Attribution subsections. This means that paleo evidence contributes to the calibrated language statements in those sections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102825	11	37	11	37	insert "the year" between "around" and "1800" [Philippe Tulkens, Belgium]	Rejected. That sentence has been deleted, since it was in the remit of Chapter 2 and those aspects are discussed in section 2.3.1.1.2.
109017	11	37	11	37	I would change 'around 1800' to 'after 1800' [Belen Martrat, Spain]	Rejected. That sentence has been deleted, since it was in the remit of Chapter 2 and those aspects are discussed in section 2.3.1.1.2.
2079	11	40	11	40	"common era" is not defined here, and it does not appear in the box in Chapter 2 Cross-chapter Box 2.1. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected to "past millennium"
110885	11	40			it would be good if this section would, in addition to attribution results, also were to discuss to what extent the model simulations manage to reproduce the global and hemispheric temperature evolution from reconstructions (sometimes it helps to mention that forcing and temperature reconstruction uncertainties are independent from each other). a [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure 3.1 has been revised to compare reconstructed and modelled temperatures over the past millennium, also showing the forcing datasets that drove the simulations.
110887	11	40			A figure comparing the 2k or lastmill model runs with the reconstructions would be very useful for this section. I recognize the reconstructions are in ch2, but showing that we know enough about forcings and models are decent enough in hemispheric and global variabiltiy to broadly reproduce the reconstructions is quite powerful particularly in the presence of the concerns about differences in decadal variability in models [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure 3.1 has been revised to compare reconstructed (same data set as shown by Chapter 2) and modelled temperatures over the past millennium, also showing the forcing datasets that drove the simulations.
102827	11	41	11	42	Not clear what the implications are of this strong disagreement. Does this have an effect on confidence levels further downstream? [Philippe Tulkens, Belgium]	Accepted. The paleo context subsection has now been subsumed as a line of evidence into the Model Evaluation and Detection and Attribution subsections. This means that paleo evidence contributes to the calibrated language statements in those sections.
10871	11	42	11	49	This needs to be more carefully expressed. The solar influence is being overstated, even by the use of "small". GHGs not only contributed warming but, due to their concentration reduction after the 15th to 17th century, also a cooling contribution to temperature variations. This needs to be made clearer. i.e.: Schurer et al (2014) found that solar forcing played a minor role, on NH temperatures over last 1000 years and both volcanic and GHG forcings had most important influence on variability. Neukom et al. (2019) found solar had no detectable influence to climate changes over 1300-1800, but volcanic and GHG forcing were detectable. Also, while not a formal attribution study, Owen et al, The Maunder minimum and the Little Ice Age: an update from recent reconstructions and climate simulations, J. Space Weather Space Clim. 2017 found model simulations also suggested a substantial role for GHGs contributing cooling/warming, as well as volcanic cooling over 15th to 19th centuries. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Solar forcing role now qualified as "minor", and the statement mentions the role of greenhouse gases. Added Owens et al. as relevant study.
102829	11	51	11	52	The English of this sentence could be improved. Starting this sentence with "In terms of…" is not appropriate. [Philippe Tulkens, Belgium]	Accepted. "In terms of" replaced with "regarding".
2093	11	51	12	17	The ordering of this paragraph is unusual. It goes EECO, MPWP, LGM, MH, LIG. If it were chronological, the LIG should be moved up to before the LGM. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. LIG statement moved up to before the LGM.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102831	11	51	12	17	This is very dense; the flow of this paragraph is repeatedly interrupted and syntactical turns make it hard to read. Please keep the language simple and clear. [Cluttering words include: "thanks", "indeed", "better appreciation", etc. Line 4 p12 contains three "-ed": noted, overestimated, indicated. [Philippe Tulkens, Belgium]	Accepted. The paragraph has been rewritten for clarity.
127247	11	54	11	54	Is "deficiencies" the right word here? Should it be "discrepancies"? [Trigg Talley, United States of America]	Accepted. Reworded as suggested.
100647	11	55	11	55	Note: Can add material re:MCO [Matthew Kohn, United States of America]	Rejected. Although the Miocene Climate Optimum is of interest, multi-model studies are not sufficiently developed for the Miocene compared to other paleo time periods assessed here.
100649	12	1	12	1	Note: Is that all that we want to say about the Pliocene and Eocene (We're doing better now than before)? I recommend adding detail here [Matthew Kohn, United States of America]	Rejected. The reason for the improvement are listed, and that is the main message to get across.
2083	12	2	12	2	the first "has improved" in this line should be "have improved" [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded as suggested.
26697	12	2	12	2	When possible it would be nice to avoid using the word proxy, and be more specific on what it is. Uncertainties depend on the type of paleoclimate archives and geochemical species analysed and calibration. [Eric Brun, France]	Taken into account. Accurate terms for temperature proxies are too technical for this report. However, the word "proxy" has been replaced with "temperature proxies" in most instances to clarify which indicator is discussed.
13319	12	3	12	3	LGM must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Rejected. The acronym was defined on line 3-11:54.
100651	12	3	12	3	Add: "For the MCO, climate models (Krapp and Jungclaus, 2011; Herold et al., 2011; Goldner et al., 2014; Burls et al., in review) cannot reproduce either GMST unless pCO2 is set higher than proxy estimates (c. 800 ppm vs. 500 ppm), and no model has reproduced the observed "flat" meridonal temperature gradient, regardless of assumed pCO2. [Matthew Kohn, United States of America]	Rejected. Although the Miocene Climate Optimum is of interest, multi-model studies are not sufficiently developed for the Miocene compared to other paleo time periods assessed here.
2095	12	5	12	5	Figure 3.1: In the legend for the proxies there are some numbers in brackets which I guess refer to the underlying paper, and should be removed. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The figure was a placeholder and has been remade.
2097	12	5	12	5	Figure 3.1: The axes should be modified so that there is less white-space around the data, e..g -5 to 0 on the x and y axes. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The figure was a placeholder and has been remade.
2085	12	6	12	6	remove "much" becasue this has no meaning unless it is quantified. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded as suggested.
35575	12	7	12	7	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
26699	12	7	12	8	Is this sentence valid for the absolute value or for the mid-Holocene changes? This is unclear [Eric Brun, France]	Accepted. The sentence has been reworded to clarify that it refers to the LGM.
2087	12	10	12	11	"There is no evidence of improved agreement for CMIP6 models for these time periods" needs a reference. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence was intended as a bridge between the previous and next sentences, but these have been rewritten and the bridge is not needed any more.
2089	12	11	12	11	It is not clear to me why "Indeed" is needed here. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been deleted, see response to comment number 2087.
26701	12	13	12	13	Brierley et al includes comparisons with several dataset, which should be reflected here. [Eric Brun, France]	Accepted. The reference to Brierley has been moved later in the sentence to clarify that is only includes comparisons.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2091	12	15	12	16	"brought improved understanding for the reasons behind apparent model-data inconsistencies" could perhaps better be "has provided improved understanding of the reasons behind previous model-data inconsistencies". [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence has been shortened following comment number 102831.
52853	12	16	12	17	Is there a statistical link between the individual model performance under paleo versus present-day climates? If yes, which paleoclimate shows the strongest link with present-day model performance? What are the implications for our confidence in the projections of future climate? [Hervé Douville, France]	Noted. There is no literature on quantitative links between paleo performance of models and their present-day performance. The paradigm is rather to test models under a wide range of different climates, some of which being of possible relevance to the future.
98809	12	18	12	18	It is important to note that there was a variation in these anomalies in mean annual temperature, from 2013 to 2019, going from approximately -3 to -3.7 over the land surface; and from -1.7 to -2.7, over the oceans, observed from the reconstructions of the paleo proxy, according to black crosses, in Figure 3.1. [Julio Cesar Barreto da Silva, Brazil]	Rejected. The variations are already explained by the statement at lines 3-12:5 to 7.
23469	12	22	12	27	It may be worth noting that global cooling for the LGM (assed at -6°C overall in Ch 2) is consistent with the smaller cooling that is shown here (presumably systematic biases in location of data) if that is the case. [Jean Lynch-Stieglitz, United States of America]	Accepted. The caption now clarifies the difference between the average of all temperature proxies at their locations and global mean temperature.
98207	12	22			Fig 3.1 Please be more comprehensive. Restricting this analysis to the LGM makes little sense. Data-model comparisons for land and sea are available for the mid-Holocene and should be available for MCA, LIA and possibly LIG. [Darrell Kaufman, United States of America]	Accepted. Figure 3.1 has been revised to include more time periods in addition to LGM: mid-Holocene, LIG, and EECO.
37287	12	24	12	25	No, there was no "global scale annual mean surface temperature" of any credibility in 1850 or any year since because there is insufficient global coverage. [John McLean, Australia]	Noted. Models are sampled according to the availability mask of observed temperature datasets, so changes in coverage with time are accounted for in the comparisons. Constantly referring to coverage changes in the model evaluation section would be distracting for little gain. And of course GSAT is perfectly well defined in a model context.
37277	12	33	12	37	You need to state explicitly that "evaluation" is different to "validation". [John McLean, Australia]	Rejected. The choice of the word "evaluation" already clarifies what is undertaken in the chapter.
15229	12	34	12	34	omit ' from physically-based understanding,' as its redundant [Sergio Aquino, Canada]	Rejected. It is important to note that the models are built on such understanding. They are not statistical models.
26703	12	35	12	36	It should be slightly reformulated because previous section on paleoclimate also provided an evaluation of the latest generation of climate models [Eric Brun, France]	Noted. The paleo context section has now been merged into the Model Evaluation and Detection and Attribution sections.
37279	12	39	12	40	This is blatant cherry-picking. AR5 also said … "... an analysis of the full suite of CMIP5 historical simulations (...) reveals that 111 out of 114 realisations show a GMST trend over 1998–2012 that is higher than the entire HadCRUT4 trend ensemble ...." [WGI contribution, chapter 9, text box 9.2, page 769, and in full Synthesis Report on page SYR-8] [John McLean, Australia]	Rejected. The AR5 assessment repeated here refers to the simulation of present-day temperature patterns. The statement quoted in the comment refers to simulations of temperature trends over the "hiatus" period, which is a different aspect of model evaluation.
15231	12	42	12	42	Ocean, but understimation [Sergio Aquino, Canada]	Accepted. Comma added as suggested.
37283	12	43	12	45	Please provide a source and a justification for the use of the "reanalysis". It seems to me that any reanalysis that supports your argument is used without any critical review of whether it is accurate. I suppose that if the IPCC doesn't audit the key temperature data that it uses then it's never going to audit a reanalysis. [John McLean, Australia]	Accepted. The reader is now pointed to Chapter 1 section 1.5.2, where the rationale for using reanalyses in climate research is made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99865	12	45	12	45	Which reanalysis? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ERA5 now stated.
104945	12	45	12	45	Here the term "reanalysis" is used for the first time here and in figure caption. A breif discription and rationale for using atm reanlysis as a benchmark of models is needed, either here or earlier in the chapter [Peter Gleckler, United States of America]	Accepted. The reader is now pointed to Chapter 1 section 1.5.2, where the rationale for using reanalyses in climate research is made.
35577	12	48	12	48	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
102833	12	48	13	3	Could be streamlined. Details essential for the assessment? [Philippe Tulkens, Belgium]	Accepted. The level of detail has been reduced.
2551	12	49	12	50	there is slim evidence in Fig. 3.2 of a significant bias along the equator of the Pacific; Lauer et al. refers to specifically to CMIP5. [Bryan Weare, United States of America]	Accepted. That statement has been deleted. The discussion has been revised to give the key message of Figure 3.2, that CMIP5 and CMIP6 are essentially indistinguishable in terms of temperature biases.
37285	12	51	12	54	Given that you've just mentioned AR5 then you should mention that AR5 said, when trying to explain why models exaggerated warming, ... (1) "There may also be a contribution from forcing inadequacies and, in some models, an overestimate of the response to increasing greenhouse gas and other anthropogenic forcing (dominated by the effects of aerosols)." [WG I SPM, section D.1, page 15, bullet point 2, and in full Synthesis Report on page SYR-8], and (2) "This difference between simulated [i.e. model output] and observed trends could be caused by some combination of (a) internal climate variability, (b) missing or incorrect radiative forcing and (c) model response error". [WGI contribution, chapter 9, text box 9.2, page 769] and explicitly admit that AR5 was wrong. [John McLean, Australia]	Rejected. The discussion here is about present-day patterns of temperature. The quotes given in the comment relate to simulations compared to observations during the "hiatus" period. Systematic biases over a 20-year period are dominated by limitations in the model representation of relevant physical processes, with internal variability and radiative forcing being of lesser importance. Trends are dominated by different uncertainties, as discussed by the quotes given in the comment.
26705	12	52	12	52	errors in atmospheric moisture and surface latent heat should be mentioned here Hourdin, F., A. Gainusa-Bogdan, P. Braconnot, J. L. Dufresne, A. K. Traore and C. Rio (2015). "Air moisture control on ocean surface temperature, hidden key to the warm bias enigma." Geophysical Research Letters 42(24) or Găinuşă-Bogdan, A., F. Hourdin, A. K. Traore and P. Braconnot (2018). "Omens of coupled model biases in the CMIP5 AMIP simulations." Climate Dynamics. [Eric Brun, France]	Taken into account. References given are relevant and have been used, but at a decreased level of detail.
104947	12	52	12	52	There is ample literature on clear-sky biases that should be overlooked. [Peter Gleckler, United States of America]	Noted. There is ample literature on model biases, but the section cannot go into much detail (see also comment number 102833).
52855	12	54			This is not the right reference for the CMIP6 ESM of CNRM (CNRM-ESM2-1, Séférian et al., 2019) [Hervé Douville, France]	Rejected. The citation is not to document CNRM-ESM2-1, but to mention the possibility of links between temperature biases and vegetation schemes. Seferian et al. 2016 discusses such links, so is the relevant paper to cite.
112651	12	55	12	55	Increasing horizontal resolution, however, shows promise of decreasing long-standing biases in surface [Melissa Jiménez Gómez Tagle, Germany]	Rejected. "however" has been deleted.
42673	12	55			'Long-standing biases…' – indeed these bias patterns have broadly remined unchanged since the first un-flux corrected coupled models of 20 years ago. Can anything be said about why the basic bias pattern has been so robust? (i.e. Southern Ocean warming, tropical stratocumulus regions, north Pacific cooling, equatorial cold bias etc.) [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been revised to point out that causes remain elusive and to list the main suspects.
2553	12	58	13	3	There is little evidence in Fig. 3.2 d) and e) for most of these conclusions. No statistical tests are made for this relatively small sample. The only obvious reductions in bias are in the upwelling regions. [Bryan Weare, United States of America]	Accepted. Statistical test results have been added to panels d and e of Fig 3.2, and the text rewritten accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11487	13	1	13	3	Just to note that in Antarctica, which is one of the regions where model deficiencies are large, HighResMIP (prescribed SST) models do not seem to be better, in terms of surface air temperature, than the lower resolution AMIP runs: Roussel et al., submitted (https://www.the-cryosphere-discuss.net/tc-2019-327/). Disclosure: I'm a coauthor of that paper, so I'm not upset if you immediately discard this comment. [Gerhard Krinner, France]	Noted. The regional level of detail of the discussion has been reduced, so the reference, although relevant, does not fit the discussion.
104949	13	1	13	3	Be careful with this. Any apparent large scale improvements via HiResMIP at this stage could simply be from sampling (too few models included). There are areas where higher resolution leads to demonstrated benefit, but not large scale climatolgies. It would be a disservice to the progress being made with higher resolution to innapropriately attribute this [Peter Gleckler, United States of America]	Accepted. The text now points out that HighResMIP models are not representative of CMIP6 models taken as a whole.
7233	13	5	13	7	When will be the CMIP6 database completed and published? Is this new research/database will affect and changes the previous results.(similar to P.14, L.5-7) [Asaad Irawan, Indonesia]	Noted. The AR6 has a cut-off data deadlines of 31 January 2021, so any change made to the CMIP6 database before then can be included.
67837	13	5	13	7	When will be the CMIP6 database completed and published? Will this new evidence/database affect and change previous results? (similar to P.14, L.5-7) [Ruandha Agung Sugardiman, Indonesia]	Noted. The AR6 has a cut-off data deadlines of 31 January 2021, so any change made to the CMIP6 database before then can be included.
69173	13	5	13	7	This summary is not as critical as the P18L47-P19L21 summary in the same section. Omitting "In summary" from the top of the paragraph would make the main message of this section clearer. [Kaoru Magosaki, Japan]	Accepted. The summary section has been moved into a single summary section at the end of the Model Evaluation section.
98811	13	8	13	8	In the Multi model Mean, Figure 3.2A, the surface temperature shows means around 25-35°C, over the Equatorial region; while in the Tropics, 20-25°C; reaching -35°C at the South Pole. In Figure 3.2B, the Multi Model Mean Bias presents differences between the CMIP6 and the ERA5, assuming positive values of 6°C in places on the west coast of Chile and between India and China; while 5-6°C positive on the west coast of the African continent. On the West Coast of the USA and in the central portion on the South Pole, these differences reach positive values of 2-3°C. In Figure 3.2C, the Multi Model Mean of Root Square Error shows the positive differences over temperature values, making clear those areas identified with positive differences of 6°C or more, as observed in Figure 3.2B. Both the low resolution model, Figure 3.2D, and the high resolution model, Figure 3.2E, present an attenuated form of the temperature differences observed in Figure 3.2B; above all, while the first reveals areas with greater positive differences in temperature between the tropics, the second reveals those established on the poles, specifically on the South Pole. [Julio Cesar Barreto da Silva, Brazil]	Noted. Figure 3.2 now includes more statistical information.
37683	13	12	13	19	How many models are used for each panel? [Masahide Kimoto, Japan]	Taken into account. Figures come with FAIR data tables that identify the number of models included in multi-model means.
35579	13	18	13	18	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
37281	13	24	13	27	Repeating a nonsense claim from AR5 doesn't make it correct. Are IPCC authors completely unaware of the rather significant matter of global coverage? The CRU's very simple web page https://crudata.uea.ac.uk/cru/data/temperature/HadCRUT4-gl.dat shows that coverage of more than 50% was very rare prior to 1904 global coverage didn't reach 50% in any month until after year 1900. Based on this, the temperature data prior to 1850 is not useful for anything other than regional studies (and maybe not even those). [John McLean, Australia]	Noted. See answer to comment number 37287.

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2557	13	24	14	7	Partly because of the poor Fig. 3.3, very few of these conclusions are verifiable. In addition in many cases the stateements are not true, based on the figure. Some of the more serious, obvious problems are listed separately. However, the entire section needs to be rethought and rewritten. [Bryan Weare, United States of America]	Accepted. The section has been updated to reflect updates to Figure 3.3.
21451	13	24	14	7	This is a huge paragraph covering several quasi-distinct issues. It would be more readable if it could be split into several smaller paragraphs. [Peter Thorne, Ireland]	Accepted. The paragraph has been split into 3.
29209	13	24	14	7	Is the cold bias in the trends acceptable (Fig. 3.3)? There has been clearly a protocoll problem in CMIP6 compared to CMIP5, as all models seem to suffer an exaggerated aerosol cooling. Why are we confident that this does not effect the projections for the future? [Fred Kucharski, Italy]	Noted. The section, and indeed the chapter, does not address fitness for purpose for projections. This is done in Chapter 4. In addition, not all models suffer an exaggerated aerosol cooling. The role of forcing and physics error in explaining trends biases in CMIP6 models remains unclear.
104959	13	24	14	7	This very important paragraph is well written but dense. It may need to change substantially given the reliance on numerous manuscripts in review and additional new models meaning substantial change is likely after the review process. Just a recommendation here - be careful how you balance the comments you recieve on this draft with the changes you feel inclinded to make as new information unfolds. [Peter Gleckler, United States of America]	Noted. The section did indeed require substantial revisions.
10575	13	25	13	27	I think a specific reference to where in AR5 this statement is pulled from. I think this might be an over-enthusiastic interpretation of what AR5 said. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is an accurate quotation of AR5 Chapter 9 Executive Summary that reads "There is very high confidence that models reproduce the general features of the global-scale annual mean surface temperature increase over the historical period, including the more rapid warming in the second half of the 20th century, and the cooling immediately following large volcanic eruptions. Most simulations of the historical period do not reproduce the observed reduction in global mean surface warming trend over the last 10 to 15 years." The source (Flato et al. 2013) has been clarified.
10577	13	25	13	27	First 15 years of the 20th century? Box3.1 refers to 1998-2012. Which period does AR5 use? I think it is also 1998-2012, which is also not the first 15 years of the current century (or previous). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected to 1998-2012.
102835	13	26	13	26	21st century? [Philippe Tulkens, Belgium]	Noted. It should indeed have been 21th century, but sentence changed in response to comment number 10577.
15233	13	27	13	27	Figure 3.3 shows [Sergio Aquino, Canada]	Accepted. The extra semicolon was added by a technical error.
37289	13	27	13	29	Yet another repeat of the foolishness of of using supposedly global averages when data was available from less than 50% of the Earth's surface. The period 1850-1900 was at times heavily biased towards European and North Atlantic conditions (NH) and the shipping routes from those countries to south-east Asia (SH) because these regions provided a greater prercentage of the hemispheric average of temperature data than the percentage of the hemisphere than they occupy. [John McLean, Australia]	Noted. See answer to comment number 37287.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52857	13	27			Panel c in Fig. 3.3 is hardly discussed and could be removed given the difficulty to assess the effect of increased resolution based on a single realization (cf. the large difference between the two configurations of the CNRM model which may be mostly due to internal variability rather than to horizontal resolution). It could be replaced by the results of one or two large ICEs in order to assess the possible influence of internal variability on the GSAT evolution. [Hervé Douville, France]	Accepted. The panel has been removed and replaced with a comparison of the two most recent CMIP ensembles.
21449	13	29	13	30	It is not that anomalies are less uncertain. It is rather than anomalies have far greater spatial scales than absolute temperatures which can show strong gradients due to elevation, surface type, proximity to coastline or water features etc. etc. The reason to look at anomalies is that they vary much more smoothly spatially than absolute values. This will also (increasingly) be true in climate models (particularly as they become better at discerning critical aspects such as topography). The sentence should be rewritten accordingly. [Peter Thorne, Ireland]	Accepted. The sentence has been rewritten to reflect and point to section 1.4.1, where those matters are discussed in greater detail.
99867	13	29	13	30	Reference to Chapter 1, section 1.4.1 which discusses this issue, may be appropriate here [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Absolutely. The sentence has been rewritten to reflect and point to section 1.4.1.
6587	13	30	13	30	For any individual observation, the uncertainty of the anomaly is larger than the uncertainty of the observation, as there is uncertainty in the reference value used to compute the anomaly as well as uncertainty in the observation, unless bias dominates and that bias does not change between the present observation and those earlier observations that determine the reference value. Anomalies are used in producing datasets such as HadCRUT4 partly because they provide a better basis for blending values from multiple observing stations over land within a computational grid-square, as they filter out much of the effects of variations in height of the observing stations and variations in their environment (e.g. rural/urban). Thus datasets such as HadCRUT4 are available only as anomalies. Reanalyses provides absolute values, but anomalies help filter out biases originating from the background model. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been rewritten to reflect and point to section 1.4.1, where those matters are discussed in greater detail.
15235	13	30	13	30	Figure 3.3 suggest [Sergio Aquino, Canada]	Accepted. The extra semicolon was added by a technical error.
42675	13	30			Suggest enhancing the visibility of the multi-model mean in figure 3.3, it is hard to distinguish in the plot. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The revised version of Figure 3.3 is more legible.
79237	13	32	13	34	Does the sentence "Virtually all…" refer to the ability of simulating the climatology or the GMST evolution? I was expecting GMST evolution as this is the topic of the paragraph, but "to simulate current climate" suggests the authors refer to the climatology. Golaz et al. (2019) and Swart et al. (2019) mention issues of their models in reproducing the GMST evolution. If it refers to the climatology, move the sentence to the previous section of model biases in the mean state. If it refers to the GMST evolution, rewrite as it's odd if all models report an imporvement but overall CMIP6 performs less well than CMIP5. [Martin Stolpe, Switzerland]	Taken into account. Sentence rewritten to highlight the important contrast between reported improvements in climatologies but continued difficulties in simulating GSAT trends.
35581	13	34	13	34	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
2559	13	35	13	36	Rather than what is stated, it may be true that the models miss the observed MID century warming. Contrary to the statement about 1940-90, it is impossible to see that the models are cooler during 1970-90, which is what is stated in line 42. [Bryan Weare, United States of America]	Accepted. A panel has been added comparing the CMIP5 and CMIP6 ensemble, and that panel makes clear that CMIP6 is indeed cooler over the period in question.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10579	13	35	13	36	Does the model ensemble encompass the observed trend? One would not expect the multi model mean to reproduce the observations perfectly. Variations due to internal variability on different time-scales won't be captured by a multi-model mean (von Storch and Zwiers, Climatic Change, 2013). The statistical characteristics of the model datasets and observations need to be accounted for in an assessment. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Yes, and a paper comparing the statistics properly has appeared since the SOD (Papalexiou et al.), and is now assessed in the section.
99869	13	36	13	36	I don't think 'cooler' is the right word here, given the dependence of this statement on the choice of reference period. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The "it" in the statement refers to the CMIP6 multi-model mean, which is compared to CMIP5 and observations, all using the same reference periods. The sentence has been clarified.
10581	13	36	13	37	The observations seem to generally lie within the model spread for most of the period in Figure 3.3. One would not expect the multi-model mean to be identical to the observations in all periods, even in a perfect model world. The statistical characteristics of the model datasets and observations need to be accounted for. That the CMIP6 ensemble is different than CMIP5 should be statistically assessed in some way. e.g., as was done for historical trends for CMIP3 and CMIP5 (Jones et al, JGR, 2013). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A paper comparing the statistics properly has appeared since the SOD (Papalexiou et al.), and is now assessed in the section.
110889	13	36			when I look at the figure I do not see at all 'very little early 20th century warming'. I see a distinct warming trend that then is interupted by slight cooling in the MM mean and many models leading to no net gain but there is ETCW! [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. That statement has been deleted because revised Figure 3.3 does not support it any longer.
35583	13	37	13	37	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
79239	13	37	13	41	Smith et al. (2020) (see Chapter 7), however, found for a limited set of CMIP6 models that "The spread of aerosol forcing ranges from −0.63 to −1.37 W m−2, exhibiting a less negative mean and narrower range compared to 10 CMIP5 models." (https://www.atmos-chem-phys-discuss.net/acp-2019-1212/). [Martin Stolpe, Switzerland]	Accepted. The link to aerosol forcing is indeed not as clear-cut as thought at the time of the SOD. The text has been rewritten to be more nuanced.
21455	13	37	13	42	It would seem to me worth remarking whether this relates to a potential model process bias, a potential bias in the prescribed forcing series, or both. At the moment the phenomenon is remarked but there is no clear effort made to diagnose why. If this isn't possible it needs I think to be stated explicitly. If it is possible then the assessment should be extended to do so. [Peter Thorne, Ireland]	Taken into account. The text now discusses the two factors, but does not delve into detail.
11489	13	40	13	40	" Indeed, several models had to reduce the strength…" - it would probably be more precise to write that the model developers had to adapt the parameterizations. Climate models don't think (at least mine definetly doesn't). [Gerhard Krinner, France]	Accepted. Sentence rewritten to clarify it is a decision of the modelling groups.
26707	13	40	13	40	We suggest to replace "several models" with "several modeling groups" [Eric Brun, France]	Accepted. Reworded as suggested.
10583	13	40	13	42	And HadGEM3-GC31 (Williams et al, The Met Office Global Coupled Model 3.0 and 3.1 (GC3.0 and GC3.1) Configurations, JAMES, 2018). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Relevant reference added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79241	13	42	13	42	I don't think that the statement that "CMIP6 multi-model mean warms at the same rate as observations after 1970" is correct. See for example Stolpe et al. (2020, submitted "Pacific Variability Reconciles Observed and Modelled Global Mean Temperature Increase since 1950") for the warming trend from 1970 - 2018 for CMIP6 multi-model mean compared to the observed warming. The CMIP6 mean warming is higher than the observed trend. Tokarska et al. (2020) also found the observed trend to be at the lower end of the CMIP6 ensemble since the 1980s (https://advances.sciencemag.org/content/6/12/eaaz9549). Similarly, Clarke and Richardson (https://www.essoar.org/pdfjs/10.1002/essoar.10502294.1) report larger than observed warming since 1979 (their Table 2; the central estimates compared). [Martin Stolpe, Switzerland]	Taken into account. The assessment has been updated to account for periods more carefully, based in part on Papalexiou et al. (2020), but also the three references given in the comment. Different studies consider slightly different periods, and compare to different observational datasets, and come to different conclusions.
6589	13	42	13	44	The sentence spanning these lines cannot be correct if Table 2.4 is correct. It is vital that it is expanded to a paragraph or more containing a quantitative comparison of the temperature increase since 1980 for the CMIP6 models and the main observationally-based datasets. Table 2.4 shows a substantial difference in the temperature increase between 1980 and 2018 from HadCRUT5 and the other observationally-based datasets, and the projections discussed in Chapter 4 show that the CMIP6 multi-model mean projects a near-term temperature increase that is larger than given for 1980-2018 by any of the observationally-based datasets, and larger also than given by the CMIP5 models. With which of the observationally-based datasets does the CMIP6 multi-model mean agree well for 2000-2018? It can't be all of them. What is required for the CMIP6 models is a calculation of the 1980-2018 (or 1980-2019 if Table 2.4 is amended) temperature increase calculated using OLS fitting in the same way as done for the observationally-based datasets in Table 2.4. See also comments 4 and 5 on the whole report, and comments 99 to 101 on Chapter 2. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Observational disagreements are now noted, and they explain some disagreements between studies. A link to Table 2.4 has been added.
10585	13	42			One would not expect the multi-model mean to be identical to the observations in all periods, even in a perfect model world. The statistical characteristics of the model datasets and observations need to be accounted for in an assessment. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, identifying systematic biases that manifest themselves over long periods, or biases that appear in groups of models, is of interest.
40101	13	43	13	43	exaggerated -> larger. To have a less value-laden term. [TSU WGI, France]	Accepted. Reworded as suggested.
35585	13	44	13	44	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
79243	13	44	13	44	Also cite: Nijsse et al. (2020) and Liang et al. (2020) https://www.earth-syst-dynam-discuss.net/esd-2019-86/ & https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL086757 [Martin Stolpe, Switzerland]	Accepted. References added.
26711	13	44	13	46	We suggest to add references to strengthen this finding. [Eric Brun, France]	Taken into account. The statement could indeed have been phrased better, but it is not only indirectly relevant to the revised discussion, so has been deleted.
42677	13	44			Sentence 'Note however….' – Is this expressed correctly? When looking at an individual ensemble member of a particular model there will be a large signal associated with natural variability (ENSO etc) and the difference with observations (especially spatially) will in part be due to the incorrect phasing of the variability and not model bias. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement could indeed have been phrased better, but it is not only indirectly relevant to the revised discussion, so has been deleted.
15237	13	46	13	46	Figure 3.3, the CMIP6 [Sergio Aquino, Canada]	Accepted. The extra semicolon was added by a technical error.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2561	13	46	13	47	Frame b) contradicts this statement. Even frame a) is barely suggestive without say year mean departure after volcanoes. [Bryan Weare, United States of America]	Taken into account. Revised Figure 3.1 is now a clearer way to assess the response to volcanic eruptions, and does not suggest a systematic issue with models. The forcing dataset is however crucial. Text has been revised accordingly.
26709	13	46	13	47	It is not that obvious based on Figure 3.3, especially Figure 3.3b. Even in (a), it may be true for the Pinatubo, but not for other eruptions. In any case, it is difficult to compare meaningfully a multi-model mean to observations, strongly impacted by internal variability, especially at such short time scales [Eric Brun, France]	Taken into account. Revised Figure 3.1 is now a clearer way to assess the response to volcanic eruptions, and does not suggest a systematic issue with models. The forcing dataset is however crucial. Text has been revised accordingly.
13321	13	48	13	48	ENSO must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Acronym now defined.
102837	13	48	13	48	"ENSO" needs to be spelled out here (1st time in chapter, unless you also use the acronym in the ES) [Philippe Tulkens, Belgium]	Accepted. Acronym now defined.
79245	13	49	13	49	The authors might want to cite Stolpe et al. (2020, "Pacific Variability Reconciles Observed and Modelled Global Mean Temperature Increase since 1950") who show this is also the case for CMIP6 (Figure 12 in the manuscript) [Martin Stolpe, Switzerland]	Accepted. Relevant reference added in support of Lehner et al. (2016).
79251	13	49	13	49	Cite Chylek et al. (2020) who argue that the CMIP5 models indeed overestimate the response to volcanic forcing. https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020GL087047 How strongly models appear to overestimate volcanic cooling, however, seems to depend quite strongly on the inclusion of the AMO in the regression model, e.g., Rypdal (2018): https://www.mdpi.com/2225-1154/6/3/64 [Martin Stolpe, Switzerland]	Accepted. Relevant references added to nuance the discussion.
2563	13	51	13	52	Are the asterisks for CMIP6 models or the CMIP5 of Houdin et al.? [Bryan Weare, United States of America]	Accepted. The sentence has been rewritten to clarify that we are talking of CMIP6 models here.
10587	13	52	13	53	How comprehensive is the list of "asterisk" models? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The list is maintained by Chapter 1 and they tried to make is as comprehensive as possible. Tuning is however not always clearly acknowledged in documentation papers. This is now clarified in the text.
79247	13	52	13	55	I'm wondering whether the tuning strategy is known for every model as I find it suprising that tuning the model to match the observed warming doesn't improve the simulation (which seems to suggest that the tuning is not very successful, or?). Or does this statement refer to the ensemble means of the tuned and un-tuned models (suggesting that model biases cancel?) [Martin Stolpe, Switzerland]	Accepted. The sentence now clarifies that we are comparing the ensemble means, and adds the caveat about bias cancellation.
35587	13	53	13	54	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
42679	13	53			Sentence: 'However, Bock et al…' – this general statement seems inconsistent with figure 3.3. There are clearly some models that do poor job in reproducing the observed warming. It would be useful if a more detailed statement of the Bock et al findings could be included. As it stands, it's not clear how the sentence 'Model spread around the mean ….' fits with this first statement. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence now clarifies that we are comparing the ensemble means.
26713	13	55	13	56	This sentence is unclear. Is spread the right word here? [Eric Brun, France]	Accepted. The sentence now clarifies that we are looking at spread across an ensemble of models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26715	14	1	14	1	A reference is needed or, better, a figure showing the historical forcings in CMIP6 models and observations. Maybe this figure is shown in another chapter? Before inter-comparing and evaluating climate models, a first logical step would be to show and discuss the realism of their forcings. By the way, it is not sure that "land-use changes" can be qualified of "radiative forcing" [Eric Brun, France]	Taken into account. The text now mentions both causes of biases, but does not delve into detail. And Chapter 7 refers to land use forcing, so we follow its practice.
34867	14	1	14	7	It is an interesting admission that the CMIP6 models less well represent historic temperatures than CMIP5 models. Please see general comment #2 above. [Jim O'Brien, Ireland]	Noted.
26717	14	3	14	3	Is the expression " physical climate models" really appropriate? Is it standard? [Eric Brun, France]	Accepted. It is used within the community, but the sentence has been rewritten to avoid using the term.
102839	14	3	14	3	"less fidelity" - does this have any implications? [Philippe Tulkens, Belgium]	Accepted. The implications depends on the application, as fitness for purpose differs between detection and attribution and projections, for example. This is now discussed in the section.
127249	14	3	14	7	Will this be finalized by publication? [Trigg Talley, United States of America]	Accepted. No, the assessment cannot conclude at this stage whether Earth System models have a more difficult task of simulating past surface temperature change than physical models. This is now clearly indicated.
42681	14	3			This sentence is a bit confused. If I interpret it correctly, suggest changing to something like '… with less fidelity than when these forcings are prescribed in the climate models'. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence reworded as suggested.
42683	14	3			Related to the previous comment, the statement that 'CMIP6 models … less well than their CMIP5 counterparts', is likely to be interpreted by some as 'climate models get worse not better'. It might be worth emphasising the general point that as more processes are made interactive in the model, it becomes less constrained by the observations but it is more realistic in its physical mechanisms. Aerosols and land-use changes are one example of this. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence reworded to clarify that point.
2565	14	4	14	7	There is nothing in Fig. 3.3 or the previous text that supports this statement. This is in contraction to the statement in lines 32-34. A careful analysis is needed to support either statement. [Bryan Weare, United States of America]	Taken into account. The text now clarifies the apparent contradiction that models may represent present climate better, but past surface temperature trends worse. The assessment has also been toned down, noting instead that the CMIP6 ensemble behaves similarly to the CMIP5 ensemble. Figure 3.3 has been revised to more clearly compare the two.
21457	14	4	14	7	It seems like it is necessary to extend this assessment to try to say, to the extent you can, why this apparently retrograde step has occurred and what the implications may or may not be both for chapter 3 but also, potentially, for remaining chapters and key metrics (I'm thinking thresholds, ch. 4, carbon biudgets, ch. 5 and ECS ch. 7). This also perhaps needs elevating to the Exec Summary and from there to the TS and even SPM? [Peter Thorne, Ireland]	Taken into account. The implications differ greatly depending on purpose. For the purpose of Ch3 of attributing human influence, the implications seem small. Other chapters, especially Ch4 and 7, reached different conclusions on the fitness of CMIP6 models for their purposes. The conclusion of the model evaluation are now more clearly given in the Ch3 Executive Summary.
10589	14	6	14	7	It would help to show the CMIP5 compared with CMIP6 in a plot somewhere, even if it is in the supporting information of the chapter. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure 3.3 has been revised and a panel summarising the two ensembles has been produced.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98813	14	8	14	8	In Figure 3.3A, the Multi Model Mean (thick red line) shows a positive variation of 0.9°C, in temperature anomalies, between 1850-2010; while the average temperature increases from 13.0°C to 14.4°C, between 1850-1900. In Figure 3.3B, the same Model (thick red line) shows a reduction in both values to 0.7°C and 14.1°C, respectively. [Julio Cesar Barreto da Silva, Brazil]	Noted.
6591	14	12	14	12	The time series are not observed. They are calculated from observations using various assumptions. The first line of the figure caption could be replaced by "Time series of the anomalies in annual and global mean surface temperature derived from observations and simulations. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Caption reworded as suggested.
37301	14	22	14	25	Figure 3.4 misses the fundamental point that standard deviation is inversely related to temperature. It's not the latitude that matters so much as the mean temperature. See section 2.8 of McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" (and for what it's worth, section 2.8 is based on the CRUTEM4 data for individual weather stations). [John McLean, Australia]	Rejected. Variability is not only related to absolute temperature. Equatorial temperatures are more variable than mid-latitudes, for example. The increase in temperature variability in globally incomplete datasets like HadCRUT4 is linked to incomplete coverage.
104955	14	27	14	27	Important background is needed here - models are a fundamental tool for most formanl D&A studies. See comment on first para of 3.1 (p 8). [Peter Gleckler, United States of America]	Accepted. Sentence rewritten to clarify the need for models in D&A.
104951	14	27	14	28	Include appropriate references [Peter Gleckler, United States of America]	Taken into account. The reader is now pointed to Section 3.2 for more detail.
87925	14	27	14	42	The assumption necessary for using climate models in D&A is stronger than what is stated here. D&A regressions use the piControl run to generate a pre-whitening operator which, to be valid, requires that the climate model generates not only accurate location-specific time series variances but all cross-sectional covariances as well, and that the expectation of the D&A regression residuals is independent of the product of the piControl covariance and the signal vectors. The way the opening sentence is phrased makes it sound like the model need only generate a reasonable univariate time series variance. [Ross McKitrick, Canada]	Taken into account. It is true that climate models need to simulate realistic covariance of internal variability, not just realistic variance for valid D&A results. However we cannot take the risk of making the text less understandable to non specialists with that level of detail. So the text now refers to 'realistic statistics of internal variability on multi-decadal timescales' to cover covariance and variance.
2567	14	28			"large mis-estimate" should replace "underestimate" An overestimate could be equally problematic. [Bryan Weare, United States of America]	Taken into account. Sentence rewritten to clarify the link between variability estimate and confidence of attribution.
37291	14	29	14	32	In fact AR5 found "... an analysis of the full suite of CMIP5 historical simulations (...) reveals that 111 out of 114 realisations show a GMST trend over 1998–2012 that is higher than the entire HadCRUT4 trend ensemble ...." [WGI contribution, chapter 9, text box 9.2, page 769, and in full Synthesis Report on page SYR-8]. This was the key finding regards CMIP5 climate models. Ignore the petty details. [John McLean, Australia]	Rejected. See answer to comment number 37263.
2569	14	34			"variance maxima" [Bryan Weare, United States of America]	Accepted. Sentence reworded as suggested.
42685	14	34			'Variance move poleward..' – not sure what this is saying. In the Pacific the decadal mode still has high variance near the equator but the overall pattern of variability has a greater meridional extent moves and perhaps this is what this is saying. Suggest rewording. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence reworded to "variance maxima" following comment number 2569.
112653	14	35	14	35	There may, however, be sizeable interdependencies between ENSO and sea surface [Melissa Jiménez Gómez Tagle, Germany]	Accepted. Commas added as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104961	14	36	14	37	See Wittenberg, A. T. (2009) Are historical records sufficient to constrain ENSO simulations? 694 Geophysical Research Letters, 36:L12702. doi: 10.1029/2009GL038710 [Peter Gleckler, United States of America]	Rejected. The focus is on studies published since the AR5.
14455	14	36			The following recent review paper is a good reference here to include on ENSO's interaction with other ocean basins: Cai et al. 2019 (DOI: 10.1126/science.aav4236) [Malte Stuecker, United States of America]	Accepted. Relevant reference included, and text modified to highlight that interactions are two ways.
37529	14	37	14	38	What is a "large ensemble of 20th and 21st century climate change"? If it is an ensemble of models then say so; and if it's not then say what it is. [John McLean, Australia]	Accepted. Clarified that we are talking of model simulations here.
37295	14	38	14	38	"internal variability" means no external inputs or outputs. The ENSO cannot be regarded as internal variability because it gets its energy from the sun. [John McLean, Australia]	Rejected. The AR6 glossary defines internal variability as arising from fluctuations of processes internal to the climate system, of which ENSO is an example. Note that the whole Earth system gets virtually all of its energy from the Sun.
104411	14	40	14	41	Please cite a paper that shows tropical Pacific is main driver of/associated with global mean, decadal variability in most models or observations. [Luke Parsons, United States of America]	Taken into account. That statement was too detailed and has been deleted.
37297	14	44	14	44	You are assuming that internal variability plays a significant part but that hasn't been proven. [John McLean, Australia]	Rejected. This is consistent with the assessment of Cross Chapter Box 3.1: "there is high confidence that the observed slower GMST and GSAT increase in the 1998-2012 period was a temporary event induced by internal and naturally-forced variability"
127251	14	44	14	44	Not sure what "renewed interest" is referring to. [Trigg Talley, United States of America]	Accepted. Sentence rewritten to focus on studies published since AR5.
71357	14	44	15	17	One could consider adding a link here to Chapter 10.3.4 and 10.4, where the role of internal variability for regional projections is discussed (it does not necessarily have to be here, but it should be somewhere in the Chapter) [Douglas Maraun, Austria]	Accepted. See response to comment number 71365.
88949	14	44	15	17	This section should also refer to Haustein et al (2019) https://doi.org/10.1175/JCLI-D-18-0555.1 who found that observational and forcing uncertainty could explain much of the discrepency between models and observations during the 20th century. [Schurer Andrew, United Arab Emirates]	Taken into account. That paper is part of the discussion on the assessment of the contribution of internal variability to observed warming.
127253	14	44	15	17	This is a long list of papers and authors, but no synthesis or assessment beyond the buried sentence on page 15, lines 7-9. [Trigg Talley, United States of America]	Accepted. The paragraph has been rewritten, merging in relevant contents of the former paleo context section. Doing so gives the opportunity to make an assessment on model skill at simulating variability.
10591	14	44			Try not to use terms like "slowdown". There are plenty of studies out there that argue there was no "slowdown" (I know there are more that say there was!). Maybe at least add "apparent" to it. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded to mirror wording of Cross Chapter Box 3.1.
10593	14	45	14	49	Jones et al, JGR, 2013 and Knutson et al, Journal of Climate, 2013 would dispute this, both studies show observed surface temperature variability over a range of timescales are consistent with the CMIP5 model ensemble. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The section focuses on literature published since the AR5. In addition, the section is clear that evidence on how well models reproduce variability is mixed, with different studies reaching different conclusions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37299	14	46	14	46	Models don't provide evidence unless those models have been validated. Not only have climate models not been validated but in various places already in this chapter you've said ... (a) "In summary, CMIP6 models reproduce observed large-scale mean surface temperature patterns as well as their CMIP5 predecessors, but with little evidence for reduced systematic biases" and (b) "In summary, the CMIP6 results currently available suggest that CMIP6 models reproduce global-scale annual mean surface temperature change over the historical period less well than their CMIP5 counterparts, but medium confidence is placed on that assessment until CMIP6 historical simulations have been submitted in larger numbers" These statements show that climate models are inaccurate and only good for studies of how sensitive they are to certain inputs, not for providing "evidence". [John McLean, Australia]	Taken into account. The evidence discussed in this sentence is observational/proxy reconstructions, not model-based. This has been clarified.
79265	14	47	14	49	You could cite Kajtar et al. (2019) here (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2018GL081462). They offer an explanation: "An apparent paradox arises in this study: models underrepresent IPO and AMV, but multimodel mean internal GMST variability is close to observations across all time scales. At longer time scales, it was found that the underrepresentation of AMV in models is offset by stronger sensitivity of GMST to AMV, as compared to observations, thus providing a possible explanation for the discrepancy." [Martin Stolpe, Switzerland]	Accepted. Relevant reference included in the discussion.
211	14	49	14	49	I think that the role of external forcing on the Atlantic modes of variability is still highly debated and should be briefly acknowledged here (as is done in 3.7.7). It is possible that variability attributed to internal climate fluctuations partially stems from externally forcing and that models may fail to reproduce the amplitude of certain indices because they fail also at capturing externally forced signal over those regions. For example Booth et al. (2012), Haustein et al. (2019) and Watanabe & Tatebe (2019) to mention a few, suggest anthropogenic aerosols as prime drivers of Atlantic Multidecadal variability in the past 100 years or so. Literature: 1. Watanabe, Masahiro, and Hiroaki Tatebe. "Reconciling roles of sulphate aerosol forcing and internal variability in Atlantic multidecadal climate changes." Climate Dynamics 53.7-8 (2019): 4651-4665. 2. Booth, Ben BB, et al. "Aerosols implicated as a prime driver of twentieth-century North Atlantic climate variability." Nature 484.7393 (2012): 228-232. 3. Haustein, K., et al. "A limited role for unforced internal variability in twentieth-century warming." Journal of Climate 32.16 (2019): 4893-4917. [Juan Camilo Acosta Navarro, Spain]	Accepted. Caveat added in the text when pointing to section 3.7.7
102841	14	49	14	49	remove comma after "evidence" [Philippe Tulkens, Belgium]	Accepted.
99343	14	51	14	52	The statement that past changes tend to be not as rapid as the changes in the anthropogenic era is not strictly true. While orbitally driven changes in the past and millennial variability are on slower timescales than anthropogenic warming there are many very abrupt changes in the last glacial cycle and the Holocene. These include the transitions between D/O events, the duration of some D/O events and several abrupt events in the Holocene. The challenge going forward is to successfully integrate enough records, with sufficient resolution to understand climatic events, and different feedbacks. This is possible in some cases, such as the dynamics of the Last Glacial to Interglacial transition in the North Atlantic realm and much of Europe, where record synchronisation is possible at sub-centennial to centennial scale. There have also been attempts to model some of these abrupt events in GCM simulations (e.g. Peltier et aln2014 and follow up papers). [Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, the statement is true in the context of the studies cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112655	14	53	14	53	models they used, may have been associated with larger variability than the full CMIP5 ensemble [Melissa Jiménez Gómez Tagle, Germany]	Rejected. The comma is not needed here.
2099	14	54	14	54	Zhu et al (2019) should be Zhu et al (2019a) [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
88963	14	56	14	57	Given the importance correctly placed on models reliably simulating internal variability a plot comparing models with the reconstructions of the last millennium (figure 2.11a) could be very useful here. As it could show that the models match the reconstructions remarkably well - thus providing evidence that the internal variability is unlikely to be extremely large. [Schurer Andrew, United Arab Emirates]	Accepted. That plot is now part of the revised Figure 3.1.
112657	15	1	15	1	why since 850? Why not earlier/later? [Melissa Jiménez Gómez Tagle, Germany]	Noted. The simulations start 1000 years before historical simulations, which start in 1850. Hence 850.
71359	15	3	15	3	Is “underestimates” a proper English noun? [Douglas Maraun, Austria]	Noted. It is. See https://dictionary.cambridge.org/dictionary/English/underestimate
6593	15	4	15	4	"In the SH" could be changed to "For the SH". The present text reads as if Hegerl et al. made their report in the SH. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded as suggested.
110891	15	5			Hegerl et al 2018 found an instance of variability far outside the model control range, but questions remain about the reliability of observed data so close to the data edge. Friedman et al 2020 found model biases in the SST interhemispheric contrast in some of the analyzed models (not all actually more than half are within obs range; only correlation between NH and SH appears too low in models but this may well be due to residual forcing in the observed residual so i wouldnt interpre that figure as a clear model issue. The paper is out; https://doi.org/10.1175/JCLI-D-19-0102.1 [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been modified to nuance the interpretation of those studies.
104413	15	7	15	8	Brown et al. 2017, Nature Climate Change (DOI: 10.1038/NCLIMATE3381), show that decadal variability also impacted by warming (abrupt 4xCO2 in GFDL CM3) [Luke Parsons, United States of America]	Accepted. Relevant reference added, and discussion of implications of forced changes in variability added.
102843	15	10	15	10	"Autumn" is "autumn"? (consistency of capital letter use throughout the chapter) [Philippe Tulkens, Belgium]	Accepted. Capital letter removed.
37875	15	12	15	13	Park et al. (2018) indicated that anthropogenic forcing has been the main factor driving the temperature seasonal cycle. Add to the references. Park, B.-J., Kim, Y.-H., Min, S.-K. and Lim, E.-P. (2018) Anthropogenic and natural contribution to the lengthening of the summer season in the Northern Hemisphere. J. Clim. 31, 6803-6819. doi:10.1175/JCLI-D-17-0643.1 [Junhee Lee, Republic of Korea]	Accepted. Relevant study added. A companion study for the South Hemisphere is also added (Weller et al. accepted 2020)
26719	15	13	15	15	It is difficult to show Figure 3.4 without discussing the huge differences between the different datasets [Eric Brun, France]	Accepted. Differences have been reduced by the switch to more globally complete datasets, but the impact of coverage on variability is now discussed.
29211	15	13	15	17	If I understand correctly Fig. 3.4, then HadCRUT4 seems to be a severe outlier in the data (thick black line). Is this possible? Are you sure the figure is correct? It seems not reasonable compared to other datasets. [Fred Kucharski, Italy]	Noted. HadCRUT4 has sparse coverage at high latitudes. Figure 3.4 now uses HadCRUT5, which is more globally complete, reducing variability in those regions.
96271	15	14			Please explain 'zonal-mean surface temperature' for non-climate scientists please, and why this property is shown in order to increase the usefulness of the report to the target audiences. [Nicole Wilke, Germany]	Rejected. Zonal mean is not such a very technical term and it is used many times in the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10595	15	15	15	17	The variability of the models at high latitudes seems pretty consistent with the observations to me (Fig 3.3). What might be causing the authors to consider this statement is the low variability of the "Cowtan-Way" dataset. But it is well known that infilling techniques will cause smoothing to happen over data sparse regions, so those regions will end up having artificially low variability (Jones, Advances in Atmospheric Sciences, 2016). It is worth being aware of that and mentioning it. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Relevant study has been added and discussion updated accordingly.
102845	15	15	15	17	Unclear what the implications are. CMIP6 is consistent with CMIP5. OK. CMIP5 do not agree well in areas with large variability, e.g.tropics and mid- to high latitudes. [what is NOT included here?]. CMIP6 tend to overestimate variability at high latitudes. Does this latter statement imply that CMIP6 models agree even less in the high latitudes? Or what is the point? [Philippe Tulkens, Belgium]	Accepted. Revisions to Figure 3.4 have prompted a revision of the discussion, which now notes that modelled variability is consistent with observations, once limitations of the datasets are taken into account.
42687	15	16			But CMIP6 ……. High latitudes' – from figure 3.4 this seems to depend on which observational dataset is used. Is the 'over-estimate' statement robust considering the uncertainty in the observations at these high latitudes? [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revisions to Figure 3.4 now show better agreement between datasets, as more of them are now more globally complete. The discussion has been revised accordingly.
26721	15	17	15	17	It depends on the observation dataset. If some datasets should not be trusted in the high latitudes, it should be said, otherwise it should not be shown. [Eric Brun, France]	Accepted. Revisions to Figure 3.4 now show better agreement between datasets, as more of them are now more globally complete. The discussion has been revised accordingly.
2573	15	17			There is no evidence of this in Fig. 3.4 given the enormous range of the observations. [Bryan Weare, United States of America]	Accepted. Revisions to Figure 3.4 now show better agreement between datasets, as more of them are now more globally complete. The discussion has been revised accordingly.
6595	15	22	15	25	If HadCRUT5 replaces HadCRUT4 in the FGD, the Cowtan and Way dataset should be dropped in Figure 3.4. See comment 96 on Chapter 2. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The choice of datasets included has been harmonised with Chapter 2, and is now based on the datasets HadCRUT5, Berkeley Earth, NOAA Global Temp, and Kadow.
26723	15	30	15	30	Figure 3.5: It is nice to see the time series, but a deeper characterization of the variability would be very interesting (e.g. spectra etc.) [Eric Brun, France]	Taken into account. The figure has been completely remade and shows the histograms of temperature trends in piControl and historical simulations.
37303	15	30	15	31	Yet again, there is no such thing as GSAT because of the poor global coverage. In 1750, which I think is the date that glossary if trying but failing to say marks pre-industrial, just four weather stations, all in Europe, which was in the Little Ice Age at the time, reported data. [John McLean, Australia]	Noted. See answer to comment number 37287.
11299	15	30	15	41	Could you briefly discuss a possible influence of the diverging characteristics of GSAT variability in piControl runs on the assessment of the warming hiatus in Box 3.1? [Masahiro Watanabe, Japan]	Rejected. Cross-chapter box 3.1 covers a wider range of evidence that the few variable models discussed here.
21459	15	30	15	41	Much of the text here felt like it should be in the figure caption. The assessment aspects get somewhat lost as a result currently. [Peter Thorne, Ireland]	Accepted. Discussion of Parsons et al. (2020) has been refocused, and its findings better put in context of paleo evidence. An assessment has been added, essentially that there is no evidence for a large error in the estimated statistics of temperature variability that would challenge the conclusions of attribution studies.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29213	15	30	15	41	Fig. 3.5: The first row of models is just amazing! I have never seen anything like this before. Particularly EC-Earth3 (supposedly one of the best European CGMs). I don't want to play devils advocate, but in the right phase these simulations could explain a large part of the temperature increase from 1950 to present. You surely want to discuss why this cannot be the case. It would be interesting to discuss where these come from, likely AMO or PDO, or combined. You may have a look at the pattern (horizontal and vertical) of extreme warm minus cold phases and argue they look very different from the global warming we have seen in the last 60 years. Otherwise, I don't see how we (I include myself here), can be virtually certain that the observed temperature increase in the recent past may not have a substantial (I mean more than half) contribution from internal variability? [Fred Kucharski, Italy]	Accepted. Spatial patterns are now discussed, as they differ between unforced variability (occurs in the high latitudes and the east Pacific) and forced variability (more global, but mostly in Tropical deep convective regions), as shown by Parsons et al. (2020) in their Figure 4. That discussion moderates the implications of the Parsons et al. results.
42689	15	30			In this paragraph no mention is made of comparison with the observations shown in the lower right panel. It would be useful to include comments on this comparison. The forced signal is clear in the observations. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The text already compares with the observations in the lower right panel of Figure 3.5. The point is that unforced variability in some models is a sizeable fraction of the observed forced signal.
52859	15	30			Rather than showing all timeseries in Fig. 3.5, what about showing a scatterplot of internal variability of GSAT (as indicated by red numbers) versus an index of AMV or AMOC variability, and only show the GSAT timeseries for the two most extreme models (EC-Earth3 and CAMS-CSM1-0) together with the timeseries of the North Atlantic index? [Hervé Douville, France]	Taken into account. The figure has been completely remade and shows the histograms of temperature trends in piControl and historical simulations. Focusing on ocean variability would have been speculative because the source of variability has not been identified conclusively in the 10 highlighted models.
127255	15	31	15	35	It may help to define "unforced control simulations" and the CNRM acronym. The sentence on line 33 is vague (Their variability in a small number of occurrences approaches that..."). Perhaps it would be stronger to say that none are equal to or above that observed under anthropogenically forced conditions, but X number of them are within one standard deviations (or something similar). [Trigg Talley, United States of America]	Accepted. The sentence has been clarified as suggested.
35589	15	34	15	34	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Rejected. At the draft stage it is fine to cite papers that have been submitted before the AR6 publication cut-off date.
13323	15	35	15	35	CNRM must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Acronym now defined.
2575	15	39	15	41	Don't many of the other models also share these oceans. [Bryan Weare, United States of America]	Accepted. That statement is not supported by the other model families, and has been deleted.
10597	15	39	15	41	That is interesting. How many models with low variability share common components? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. That statement is not supported by the other model families, and has been deleted.
79253	15	39	15	41	How relevant is this observation? The model with the smallest decadal variability - CAMS-CSM1-0 - for examples also uses MOM. [Martin Stolpe, Switzerland]	Accepted. That statement is not supported by the other model families, and has been deleted.
104953	15	41	15	41	Include one or more references for both NEMO and MOM [Peter Gleckler, United States of America]	Noted. The statement in question has been deleted.
6597	15	47	15	47	The reference to "GISTEMP GMST observations". GMST is not an observable, and GISTEMP is not a dataset of observations. GISTEMP is a dataset derived using observed temperatures (and assumptions that, for example, produce values for grid squares containing no usable observations.) GMST is a single value derived from that dataset. The figure caption needs rewriting. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded to "GMST estimated from the GISTEMP dataset"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35591	15	49	15	49	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
127257	15	54	16	30	This section predictably started with AR5, again. While lines 2-8 on page 16 are a much better synthesis/assessment than the previous section, lines 7-13 feels like a buried lede. A new paragraph could begin on line 14. Lines 14-17 are written in a weak and passive way. For example, the phrase "could be strengthened" is baffling: Do the authors of this chapter conclude that it is strengthened, or not? Use stronger, bolder language than "could be strengthened" (line 14), "probably" (line 16), and "may" (line 17). The sentence between lines 17-24 is a runon. When the reader finally reaches line 30, there is no conclusion, no assessment by the authors what this all means, or why it is important. [Trigg Talley, United States of America]	Taken into account. The structure of starting with the AR5 conclusions has been agreed at chapter level, and this paragraph opens a new section so starts with AR5 conclusions. The paragraph has been cut in two as suggested. The second part has been rewritten to clarify the conclusion, which is to moderate the (strong) conclusions of previous studies by pointing out their limitations.
10599	15	54	19	40	I fear the authors have really tied themselves up in knots regarding the issue of applying an adjustment to "GSAT" to get "GMST" describe in this section. Chapter 2 seem to suggest that an adjustment should be applied to observations to account for the estimated differences in trends between "GMST" and "GSAT", and indeed that seems to be headed in chapter 7 say. So why complicate the issue in this chapter? I think anyone (most people!) unfamiliar with the issue will be very confused. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Chapter 2 has now re-assessed the difference between GSAT and GMST-based trends, and the best estimate of their scaling is now 1. So from the point of view of Ch3 it is not necessary to carry both sets of numbers, and the text now clarifies that.
37305	15	55	16	1	The findings of AR5 lacked integrity. It showed that climate models were inaccurate - "... an analysis of the full suite of CMIP5 historical simulations (...) reveals that 111 out of 114 realisations show a GMST trend over 1998–2012 that is higher than the entire HadCRUT4 trend ensemble ...." [WGI contribution, chapter 9, text box 9.2, page 769, and in full Synthesis Report on page SYR-8] - and yet used the output from those same flawed models to assert that mankind had been the dominant cause of warming since 1950. The natural causes of most, if not all, of the post-1950 warming can be found in the peer-reviewed paper McLean (2014) "Late Twentieth-Century Warming and Variations in Cloud Cover". [John McLean, Australia]	Rejected. See answer to comment number 37285. The suggested reference is not relevant to industrial-era warming.
37307	15	55	16	1	This sentence only refers to two possible reasons for temperature change. Natural influences should also be mentioned. I am aware that this chapter is about the alleged human influence on climate but you should not deny the possibility that natural influences played a major part. [John McLean, Australia]	Noted. "Internal variability" is essentially a natural influence, unless the reviewer is thinking of changes forced by external forcings of natural origin. Those are discussed in the section too.
10601	15	55	16	30	The confidence in the assessment that anthropogenic warming is consistent with observed warming must be tempered with the practical limitations of attribution techniques (e.g., Jones et al, JGR, 2016a). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. See answers to comment numbers 10603 and 10605.
10603	15	55	16	30	Detection analyses that attempt to attribute anthropogenic and natural influences in historical temperatures, using climate models are effectively just comparing anthropogenic influences with the observations, as the natural contribution to trends is near zero (Jones et al, JGR, 2016a). Thus, it is unsurprising that the attributed anthropogenic trend is consistent with the observed trend (Section 6.1.2 in Allen et al, Surv Geophys, 2006). Thus it is also unsurprising that this is consistent across different model analyses (e.g., Bottom left panel of Figure 3.6 on page 138). Thus more weight should be put on results from alternative techniques which don't rely on one factor having near zero influence. This should be noted where it is appropriate. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The discussion covers a much wider range of attribution techniques than in the past and although not all are used in the assessment, it is now noted that their qualitative agreement increases confidence in the attribution conclusions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10605	15	55	16	30	It is important to note that consistent anthropogenically attributed temperature trends from different results and analyses, may actually lead to inconsistent WMGHG and other anthropogenic influences. For instance the GHG and aerosol contributions to the "ANT" models used in Figure 3.6 on page 138, will be inconsistent across the models. This is what was found in a similar analysis of CMIP5 models (Jones et al, JGR, 2016a). This will raise concerns about whether such close agreement of anthropogenic attributed temperature trends are an artifact or not. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The contrast between single-forcing attribution and attribution to anthropogenic influences more generally is now noted. But, confidence in overall attribution comes from an alternative line of discussion (see reply to comment number 10603) so should be unaffected by challenges in single-forcing attribution.
116189	15		15		Insights from Antarctic2k / PAGES2K on Antarctic variability could be used here. [Valerie Masson-Delmotte, France]	Rejected. The discussion focuses on insights at the hemispheric scale, avoiding regional considerations.
71361	16	1	16	14	This section reads a bit like name dropping or showing off. For an expert in the field, it does not provide new information, for a non-expert the information is not accessible (e.g. reference to an impulse-response model). I would suggest to either rewrite this paragraph in an accessible style (this should be possible), or to shorten it. [Douglas Maraun, Austria]	Accepted. The paragraph has been shortened to avoid jargon and to highlight the key message that attribution studies are going beyond “standard” regression-based techniques yet still make strong attribution statements.
87927	16	1	16	30	I appreciate the note of caution in this paragraph about the list of positive detection findings perhaps being overstated. One of the strong assumptions in D&A methods is that climate models omit nothing relevant for explaining patterns of climate change. In McKitrick, Ross R. and Lise Tole (2012) “Evaluating Explanatory Models of the Spatial Pattern of Surface Climate Trends using Model Selection and Bayesian Averaging Methods” Climate Dynamics, 2012, DOI: 10.1007/s00382-012-1418-9 we showed that the spatial pattern of warming trends over land cannot be explained without including measures of urbanization-related land use change which are not included in models. Once they are included most climate models had no explanatory power over land. I have also done an extensive re-examination of the Allen&Tett optimal detection procedure which is under revision for resubmission at JClim (I don't know the procedure for sharing submission-stage materials with LA's but contact me if you'd like to see it). For 20 years the field has relied on the Allen&Tett Residual Consistency Test, TLS estimation and the claim that P-weighting of the regression model satisfies the Gauss-Markov conditions. I show that these are all invalid claims. The RCT is uninformative as a test of regression misspecification, TLS imparts an upward bias rather than correcting downward bias (the case where it corrects attenuation bias doesn't apply in signal detection regression) and Allen&Tett were mistaken in their presentation of the Gauss-Markov conditions. Standard econometrics tests show that several types of specification error commonly exist in D&A regressions and their remediation substantially weakens attribution results. [Ross McKitrick, Canada]	Taken into account. As noted in AR5 Chapter 10, McKitrick and Tole (2012) looks at trends over a period (1979–2002) that is dominated by internal variability, especially at the local scales discussed in the comment. Regarding limitations of regression-based studies, the assessment is now based on results from studies applying a wider range of approaches than before, not just the standard regression based approaches. All approaches lead to the same attribution results. This is how clarified in the section.
104969	16	3	16	7	Poorly written - please revise [Peter Gleckler, United States of America]	Taken into account. That sentence has been rewritten following comment number 71361.
66971	16	4	16	4	Remove "in optimal fingerprinting" (e.g., Ribes et al., 2017, does not use optimal fingerprinting) [Aurélien Ribes, France]	Accepted. Reworded as suggested.
110893	16	4			add schurer et al 2018 to innovations; also uses ensemble of observations and pattern uncertainty [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Relevant reference added.
37309	16	6	16	7	It is unacceptable to replace observed temperatures with modelled temperatures. [John McLean, Australia]	Noted. The reviewer might have misunderstood the statement. Observed temperatures are not replaced by modelled temperatures. Instead, a model is fitted to them for detection and attribution purposes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89879	16	6	16	7	Actually, Haustein et al. (2019) does not fit an impulse-response model to observed temperatures. Instead, a best estimate of the forced temperature evolution is provided, based solely on published physical constraints. I suggest some light edits along the following lines: […], reducing the dependence of the attribution on uncertainties in climate sensitivity and forcing by fitting an impulse-response model to observed temperatures (Otto et al., 2015; Haustein et al., 2017), or estimate the forced temperature evolution with an impulse-response model constrained by robust physical forcing parameters (Haustein et al., 2019). [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The level of detail of that sentence has been lowered following comment number 71361.
7553	16	7	16	7	1/1: As methodological advance, could you please add: "impulse-response model to observed temperatures (Otto et al., 2015; Haustein et al., 2017, 2019); or accounting for complex temporal signal (e.g., associated with temporal changes in positive greenhouse gas and negative aerosol forcing) in pattern-based fingerprinting method (Bonfils et al. (submitted))." Note: this represents a major step forward in pattern-based fingerprinting techniques. Instead of simply comparing the trends in signal and noise time-series, we now compared regression coefficient obtained between the fingerprint and the signal time-series, with the regression coefficients obtained between the fingerprint and the noise-time series. Bonfils, C. J., Santer, B. D., Fyfe, J. C., Marvel, K., Phillips, T. J., and Zimmerman, S. R. H. Human influence on joint changes in temperature, rainfall and continental aridity. (submitted). [Celine Bonfils, United States of America]	Taken into account. The study is cited when discussing attribution at the hemispheric level.
104415	16	10	16	14	Also that the spatial patterns of forced change are distinct from the spatial patterns of internal variability (Parsons et al, 2020, GRL) [Luke Parsons, United States of America]	Taken into account. Differences in pattern are important, and that point has been made elsewhere following comment number 29213.
102847	16	12	16	12	"anthropogically" - is this terminology that is agreed upon with all chapters and all WGs? Never heard of it. Really necessary to use? [Philippe Tulkens, Belgium]	Accepted. That was a typo, the correct word is anthropogenically.
52861	16	14	16	17	Cut the too long paragraph here and rephrase the first sentence which seems to challenge the previous findings while the remaining paragraph does not? [Hervé Douville, France]	Accepted. The paragraph has been cut in two here. The second part of the paragraph has been rewritten to emphasise the point that this assessment does not find we can be as confident as the studies suggest, so we moderate their conclusions.
66973	16	15	16	17	This sentence suggests that accounting for internal varaibility appropriately is the main challenge in D&A, but I tend to disagree with this. Several papers have highlighted that modeling uncertainty (potentially discussed as forcings + feedbacks) is a really challenging issue, particularly given limits of CMIP ensembles (small size and dependence among models). This could be discussed further. [Aurélien Ribes, France]	Accepted. The sentence has been rewritten to make that point in addition to the point on variability, which remains an important consideration.
35593	16	17	16	17	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
35595	16	22	16	22	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
66975	16	22	16	22	Remove Ribes et al. (2017, submitted) here. Ribes (submitted) is appropriate at the end of the same sentence. [Aurélien Ribes, France]	Accepted. This has been corrected as suggested.
10607	16	25	16	27	This is not quite right, Jones and Kennedy (2017) found that it was the variance of the scaling factor uncertainty that increased by about 20%. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected as suggested.
2577	16	27	16	30	greenhouse scaling factor is defined much later in Fig. 3.6 [Bryan Weare, United States of America]	Accepted. The discussion now clarifies the meaning of the values of the scaling factor.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
591	16	28	16	30	do we need to use GMST and GSAT over here? [ZHIYAN ZUO, China]	Taken into account. The paragraph now clarifies why GSAT and GMST matter little for attribution, based on a revised assessment of their difference in Chapter 2.
21461	16	29	16	30	This should refer to cross-chapter box 2.3 rather than all of chapter 2. The similarity between the inflation factor reported here and the scaling factor in moving from GMST to GSAT is interesting and I wonder whether this should at a minimum be explicitly noted? [Peter Thorne, Ireland]	Taken into account. The statement now refers to Cross-chapter Box 2.3. But revision to the Chapter 2 assessment of the scaling factor from GMST to GSAT makes the second point moot.
37685	16	30	16	30	Need a brief explanation for "greenhouse scaling factor"? [Masahide Kimoto, Japan]	Accepted. See response to comment number 2577.
21463	16	32	16	34	Without a reference supporting the assertion this risks being seen as a strawman statement. Also, the reader would be interested in why this is the case, surely? You then pretty much repeat the same statement anyway, this time with references later in the paragraph (ln 37-39 and then supported by the balance of the paragraph). I would suggest rejigging the paragraph to remove the redundancy. [Peter Thorne, Ireland]	Accepted. The second statement is now used to open the paragraph. The first statement has been deleted.
87931	16	32	16	47	Given that the general tone of the summary sections is that confidence in attribution has increased since AR5 I think this paragraph should outline more clearly that attempts to make attribution to GHG's distinctly from aerosols has gone in the opposite direction, namely the detection experiments often fail. Jones et al (2016a) didn't simply note the range of results, they found that when the anthropogenic forcing signal is separated into greenhouse gas and other effects including aerosols, the greenhouse gas signal was detected in only 8 of 15 cases (each case being based on using one of 15 climate models over the 1910 to 2005 interval) and varied widely in magnitude across models, that the influence of other anthropogenic effects was detected in only 5 of 15 cases, and that aerosol forcing effects were detected in only 7 cases. They noted that their results add to some other recent studies showing “little consistency in the magnitude of the scaled greenhouse gas warming across a sample of CMIP5 models” (Jones et al. p. 6980) and specificaly called into question the credibiltiy of claiming GHG detection when the model failed to etect another signal that should be just as clear. I know that practitioners have been quick to blame this on "signal degeneracy" and assume that a bit more data or some fancy ad hoc statistical methods will resolve it, but that doesn't square with the discussion in Ch 7 on ECS estimation, which takes the position that the spatial pattern of aerosols much more precisely known now. I think it is more likely that D&A has been relying on the circa 1999 Allen-Tett-Stott ad hoc regression methods that have serious robustness problems and that are not used ANYWHERE outside of climatology for well-known reasons. Given the size of the data sets involved, there should be no difficulty distinguishing GHG and aerosol signals if the influence were as significant as has been claimed, and the effect magnitudes should not jump around so much from study to study. [Ross McKitrick, Canada]	Accepted. The discussion now gives more attention to model-by-model differences in attributable warming estimates, since their non-overlap reflects underestimated uncertainties in single model results. However, the discussion also notes that an innovation since AR5 is that several new studies validate their multi-model approaches using imperfect model tests (Gillett et al., 2020; Ribes et al., 2020; Schurer et al. 2018). Doing so increase our confidence in the results. On regression-based attribution approaches, we now include results from studies applying a wider range of approaches, not just the standard regression-based approaches. Other approaches lead to similar attribution results.
10611	16	32			I am a bit confused by the first part of the sentence. The previous paragraph also tried to attribute individual forcing factors, "ANT" and "NAT". Clarify. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Removing the redundancy in the paragraph noted by comment number 21463 has the effect of clarifying that we are talking in that paragraph of the decomposition of the ANT term into GHG and AER.
37315	16	34	16	34	You cite yet another junk study that ignores coverage (because if it took notice of coverage it wouldn't have been published). According even to the generous HadCRUT4 system of determining coverage, data was not consistently available from more than 50% of the Southern Hemisphere until 1949. [John McLean, Australia]	Rejected. The study in question (Hegerl et al. 2018) does account for data uncertainty, which decreases with improving coverage.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
110895	16	34			The contributions to the early 20th century warming are estimated from the entire period attribution; and include data uncetainty. Rest is good thank yoU! [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been rewritten to clarify those points.
7555	16	37	16	37	1/1: If this helps, add (as a suggestion): "substantial. Bonfils et al. (submitted) show that reliable historical simulations of the observed temperature changes between 1950 and 2014 (both in term of global temperature, or interhemispheric temperature contrast) requires combined forcing by greenhouse gases, large volcanic eruptions, and a full representation of aerosol direct and indirect effects. A growing body of". We show that including both direct and indirect aersols is required to best match the observations, but we did not give a percentage of contribution from GH, AA, and volcanic forcings. That is why I am not sure that this paper is good fit here. [Celine Bonfils, United States of America]	Noted. At this stage, the need to account for GHG and AER forcing is not in question. It is the quantitative assessment that is discussed. So the reviewer is correct that the suggested reference is not relevant to this paragraph.
74343	16	37	16	37	I would like to suggest to add this sentence 'In the tropical areas, changing in land-use has a dominant contribution of increasing the greenhouse gas concentrations' [Yulizar Yulizar, Indonesia]	Rejected. There is no attribution literature that makes that particular argument.
89881	16	37	16	37	May I suggest the following addition before “Indeed”? Using a subsampling approach for both hemispheres and land+ocean, and accounting for observational uncertainty during WWII, Haustein et al. (2019) find that almost all warming during 1901-1950 was externally forced. Yet, a large body of literature […] [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. A similar statement is made earlier in the section, where it is more relevant.
104971	16	37	16	37	A "growing body of literature"? Is this only the 3 studies below? [Peter Gleckler, United States of America]	Accepted. Reworded to "Studies published since the AR5"
10609	16	37	16	40	Schurer et al (2018) also used SAT over sea ice. This has implications for an attribution analysis which uses model experiments with very different sea ice coverages. The changing use of SST and air temperatures over sea ice across models/experiments will not be more comparable to observations (Jones, `Apples and oranges': on comparing near surface temperatures from climate models with observations, submitted Q.J.R.Meteorol. Soc., 2019). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This high level of detail is out of the scope of the discussion.
21465	16	39	16	47	This feels more review, less assessment. Each study is covered off in turn and the reader is left to pull together the synthesis for themselves. Suggest redraft to be more a synthesis and assessment. [Peter Thorne, Ireland]	Accepted. The paragraph has been rewritten following comment number 87931.
7235	16	46	16	47	It is stated in text that "A more recent third study also finds.....". Thus this text refers to what study? What are those studies? [Asaad Irawan, Indonesia]	Noted. That statement has since been deleted.
67839	16	46	16	47	It is stated in text that "A more recent third study also finds.....". There is a need to clarify what study is referred in this statement. [Ruandha Agung Sugardiman, Indonesia]	Noted. That statement has since been deleted.
13325	16	47	16	47	GISS must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Noted. The text has been rewritten and the acronym is not needed any more.
37319	16	49	16	50	Repeating a doubtful claim doesn't make it correct. Bindoff et all (2013) bases its claims on the output of climate models that chapter 9 of IPCC 5AR showed exaggerated warming, probably because they "over-estimated" the influence of greenhouse gases. [John McLean, Australia]	Rejected. The statement is on additivity of temperature response, which is a property that seems to hold over a range of response sizes.
33271	16	50			Change “.. by (Bindoff et al., 2013)…” by “….by Bindoff et al. (2013)……”. [Guiomar Rotllant, Spain]	Noted. That statement has since been deleted.
71363	16	51	16	51	This sentence is not clear. Any attribution framework aims at separating signal and noise. So what does it mean that an attribution method accounts for internal variability? [Douglas Maraun, Austria]	Accepted. "and internal variability" has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10613	16	51			More caution is needed when interpretating simplistic attribution techniques (e.g., as investigated by Benestad and Schmidt, JGR, 2009) e.g., ones that don't test for statistical under/over-fitting (Hegerl et al, Good Practice Guidance Paper on Detection and Attribution Related to Anthropogenic Climate Change, IPCC, 2009) [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. That statement repeats the conclusions of the SR1.5.
37321	16	54	16	54	As I have said previously in my comment for this chapter, the data from 1850 to 1900 is worthless due to poor global coverage. It wasn't until 1904 that global coverage exceeded 50%. [John McLean, Australia]	Noted. See answer to comment number 37287.
2579	17	1	17	10	The fingerprinint regression coefficients need to be defined in the text. The corresponding Fig. 3.6 is difficult to interpret. In particular what does a value of 0 mean? How does one interpret the three-way factors? Do GHG and OTH add to ANT in the top left frame? I find the bottom frames much more easily interpreted. Are the top necessary? [Bryan Weare, United States of America]	Taken into account. The meaning of the regression coefficient is now defined in the text, and the discussion makes a clearer use of them, following comment numbers 87931 and 87939.
87939	17	1	17	11	The discussion does not adequately convey what Figure 3.6 shows. The Figure shows that there is almost no consistency in D&A results from one model to the next. In the 2-way diagram regression coefficients (top left) there are no 2 model outputs that yield the same pair of inferences about ANT and NAT. The lower panels don't seem to connect to the upper panels since all the variabiltiy vanishes. In the top right diagram, switching to a 3-way attribution, namely all the authors do is separate GHG from non-GHG, mainly aerosols, which should have a distinct pattern, yet the results scatter all over the place and become completely incoherent. Again no pair of models gives the same results and MIROC6 explodes (my guess is they are using TLS regression and the coefficients are going to zero but TLS crashes near zero). The top-right panel cannot be described by saying "all models are consistent in attributing most of simulated warming to anthropogenic influences" because there is no consistency among the models. The cautions in Jones et al. 2016a should be noted here: "it is then legitimate to question the confidence of the magnitude of the attributed greenhouse gas warming when another important forcing factor with known strong radiative effects is not detected at the same time. As other anthropogenic influences are not robustly detected, is the factor not important for twentieth century temperature changes? Are there errors or biases in the other anthropogenic response patterns? Are other important factors not being included? Or is the detection analysis methodology flawed?" (p.6980) My opinion based on very extensive reading is that the latter is at the root of the problem. [Ross McKitrick, Canada]	Accepted. See response to comment number 87931.
35597	17	2	17	2	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
52863	17	2	17	4	Usually they over or underpredict both so that the net response is realistic. This could be emphasized, as well as the need to move to a multivariate analysis (e.g., interhemispheric and/or interseasonal contrasts?) in order to better constrain the response to the individual forcings? [Hervé Douville, France]	Accepted. The discussion now reflects those comments.
35599	17	10	17	10	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
127259	17	10	17	10	What is the conclusion? [Trigg Talley, United States of America]	Accepted. The paragraph now concludes by highlighting the need for new attribution methods, either multivariate or based on asymmetries between hemispheres etc.

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37687	17	15	17	26	How many ensemble members are used for each model? [Masahide Kimoto, Japan]	Accepted. The number of ensemble members is now given in the caption of Figure 3.6.
37689	17	15	17	26	How to interpret "unconstrained" MIROC6 results on the right-hand panels is not very clear. [Masahide Kimoto, Japan]	Noted. Data update has made this statement obsolete.
99345	17	17	17	18	The Holocene sees multiple abrupt climate oscillations driven by variability in TCH through ocean ice interaction until ~8ka BP, followed by several events linked to solar forcing including the 2.8 ka BP event and the LIA. In the highest resolution records (e.g. Martin Peurtas et al., 2012) show changes in atmospheric circulation, such as NOA, reduced temperatures and changes in precipitation over Europe that impact on wetness in bog records. Moreover changes in the humification of peat bogs in response to abrupt Lateglacial and Holocene events have been reported from afar afield as Ireland and China. [Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Noted. (Assuming that the comment refers in fact to page 18, lines 17 to 18.) The discussion focuses here on progress since the AR5.
13327	17	18	17	18	change acronym of GHG to WMGHG as in chapter 2 [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Changed as suggested.
13329	17	19	17	19	OTH must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Accepted. "Other" is used instead of "OTH".
35601	17	26	17	26	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
15239	17	31	17	31	Figure 3.7 shows [Sergio Aquino, Canada]	Accepted. The extra semicolon was added by a technical error.
21469	17	31	17	37	Most of this feels like material that should be in the figure caption as it is explaining what the figure shows. The text should interpret the figure and the figure caption should be there to explain the figure. [Peter Thorne, Ireland]	Accepted. The opening of the paragraph has been shortened.
37323	17	31	17	37	Wrong. Figure 3.7 doesn't show GSAT because no such thing exists. As I stated above, the 1850-1900 is useless because of the shortfall in coverage. On top of these problems the only references that you cite are three papers that haven't even been published. [John McLean, Australia]	Noted. See answer to comment number 37287. In addition, at the draft stage it is fine to cite papers that have been submitted before the AR6 publication cut-off date.
127261	17	31	17	37	This information is in the caption, so could be dropped here. [Trigg Talley, United States of America]	Accepted. The opening of the paragraph has been shortened.
52865	17	31			In Fig.3.6 and Fig. 3.7, the GHG contribution could be in red rather than grey (also used for the observations)? [Hervé Douville, France]	Taken into account. The figure has been revised to improve its legibility.
110897	17	31			double global [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The two "global" refer to distinct properties: global completeness of the analysis, and the global averaging of the temperatures.
2581	17	32			The fime periods listed here and those in corresponding Fig. 3.7 differ. [Bryan Weare, United States of America]	Accepted. This has been corrected.
35603	17	33	17	33	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
10615	17	35	17	36	More formal names are needed for these datasets, as well as version numbers etc. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. That statement was duplicating the caption so has been deleted. Figures come with FAIR data tables that indicate dataset versioning.
10617	17	35			It should be mentioned that the use of observational datasets that use infilling techniques may have disproportionate influence on results when they don't actually contain any more information than in non infilled datasets (e.g., Jones and Kennedy, Journal of Climate, 2017). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Attribution results do not change substantially when switching from HadCRUT4 to HadCRUT5, based on Gillett et al. (2021).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10619	17	38	17	39	It is always an issue for detection studies, but the use of just 6 CMIP models and how there may be sampling issues when drawing from an "ensemble of opportunity" needs to be mentioned. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The models shown were not used in a detection framework -- instead, the figure shows their raw output, which generally covers the ranges assessed by Chapter 7.
37325	17	39	17	47	All this is mere speculation if the models haven't been validated. [John McLean, Australia]	Noted.
89883	17	44	17	47	In light of this statement and Fig 3.7, what I said above about the magnitude of our best estimate (100%) is corroborated in the strongest possible sense. The figure also provides lower AND upper bound for the 'main driver' statement. [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The Chapter 7 estimates are however not fully independent on the D&A conclusions, so the section remains more cautious than the reviewer. See comment number 10621.
110899	17	44			very good figure linked here, but you need to explain what the physicall based estimates are (they are without errorbars?) response to natural forcing leads to longterm trends that are close to zero (it is episodically important!) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Physically-based" is now defined. Note that those estimates come with an uncertainty range. The reviewer may have confused them with the raw climate model output, also shown in Figure 3.7
10621	17	45	17	47	The estimates of trends are not independent from observed warming, or from the physics based climate models. 7.3.3.3 (page 42:19-20) says the historical temperature record is used to constrain the assessed ECS, which is used in the very simple climate model being used to create the trends. 7.5.6 (page 105:50-51) says that global climate models are also used in the assessment of ECS and ERF, so saying that these trends are from "a totally different approach" is an exaggeration. I strongly recommend not including the trends from chapter 7. Their inclusion will, I fear, lead to circular reasoning and too confident an assessment. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. It is true that Chapter 7 estimates use models as lines of evidence, but among other lines of evidence. And we consider that it is useful to be able to compare the assessments from different chapters. Nonetheless we have moderated the text here concerning independence.
127263	17	47	17	47	What is the conclusion? [Trigg Talley, United States of America]	Noted. The conclusion of this paragraph comes a couple of paragraphs later, in the overall summary of the section.
13331	17	53	17	53	change acronym of GHG to WMGHG as in chapter 2 [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Changed as suggested.
35605	17	56	17	57	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
35607	18	1	18	4	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
37327	18	2	18	2	An estimate based on subtracting a number (to 2 decimal places no less) from another estimate is just another estimate. [John McLean, Australia]	Noted.
37329	18	15	18	17	AR5 made these findings by using CMIP5 models that chapter 9 of that report showed were seriously flawed. These findings therefore have no credibility whatsoever. [John McLean, Australia]	Rejected. The present assessment puts substantial effort to evaluate the fitness for purpose of climate models for detection and attribution, finding that although the models are flawed in some respects, their errors are not large enough to challenge the conclusion of detection and attribution studies.
127265	18	15	18	24	Instead of beginning with AR5, start this paragraph at line 24 with the finding. [Trigg Talley, United States of America]	Rejected. The structure of starting from the relevant AR5 conclusions has been agreed chapter-wide.
37331	18	15	18	31	This paragraph cites "models", "modelling", "simulations", "simulation", "simulations" and "models" but there's no evidence that ANY of the models had been validated. [John McLean, Australia]	Rejected. See response to comment number 37329.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35609	18	17	18	17	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
593	18	17	18	19	interesting. Why is cooling before 1980? [ZHIYAN ZUO, China]	Accepted. Causes of cooling clarified from answer to comment number 7547.
7545	18	17	18	20	1/4: I strongly recommend to cite Bonfils et al. (submitted) who conducted a formal pattern-based detection and attribution analysis on this specific topic (see Fig. 8.11, chapter 8, extracted from this study). You could simply add: "Friedman et al. (submitted) and Bonfils et al. (submitted) detect an anthropogenically forced response of inter-hemispheric contrast in surface temperature change, with the Northern Hemisphere cooling more than the southern hemisphere until 1980 but then warming more from 1980 to 2012 (Figure 8.11, right column)". [Celine Bonfils, United States of America]	Taken into account. A similar suggestion made by the same reviewer in comment number 7547 has been taken into account.
7547	18	17	18	20	2/4: Then I also recommend to add somethong like this: Friedman et al. (submitted) show that CMIP5 models simulate the correct sign of the inter-hemispheric contrast when forced with all forcings but underestimate its magnitude. Bonfils et al. (submitted) however show that the remarkably complex temporal behavior in interhemispheric temperature contract, with a abrupt reversal around 1975 (Figure 8.11, bottom right panel), is detectable in reanalyses at a stipulated 5% significance threshold. Formal single-forcing attribution highlights that while the Northern Hemisphere was cooler than the Southern Hemisphere before 1975 in response to European and American sulfate aerosol emissions, it became warmer than the Southern Hemisphere after 1975, due a combination of reduced sulfur dioxide emissions and of greenhouse gases-induced warming of Northern Hemisphere landmasses". [Celine Bonfils, United States of America]	Taken into account. The discussion follows the suggested wording, but at a lower level of detail.
7549	18	17	18	20	3/4: FYI, Bonfils et al. (submitted) results are based a multivariate pattern-based detection and attribution analysis, using temperature, precipitation and an aridity index. They show a first fingerprint that focuses on the changes in mean-states in response to GHG-induced warming (Figure 8.11, left column). The second fingerprint focuses on the inter-hemisperic contrast signature (Figure 8.11, right column). Using a single-variate pattern-based detection and attribution focusing on the temperature variable alone leads to very similar detection and attribution conclusions (Supplemental Material). [Celine Bonfils, United States of America]	Noted. See response to comment number 7547 by the same reviewer.
7551	18	17	18	20	4/4: Bonfils, C. J., Santer, B. D., Fyfe, J. C., Marvel, K., Phillips, T. J., and Zimmerman, S. R. H. (submitted). Human influence on joint changes in temperature, rainfall and continental aridity. (submitted). [Celine Bonfils, United States of America]	Noted. See response to comment number 7547 by the same reviewer.
15241	18	24	18	24	Figure 3.8 shows [Sergio Aquino, Canada]	Accepted. The extra semicolon was added by a technical error.
29215	18	24	18	31	Fig. 3.8: why does the cold bias in the temperature trends essentially disappear here? [Fred Kucharski, Italy]	Rejected. The bias does not disappear. Figures 3.4 and 3.9 now use the same set of CMIP6 models.
71365	18	31	18	31	Here it could be referred to the case studies in Chapter 10.4 and 10.6 (currently undergoing restructuring. The relevant example about Europe from 10.4 will likely be included into in 10.6). [Douglas Maraun, Austria]	Accepted. A sentence linking to section 10.4 has been added, although regional attribution has a slightly different definition of attribution.
7237	18	31			How about other parts of the world? It is suggested to conduct a comprehensive study literature to have a better understanding to global conditions, otherwise it will be geographical biases [Asaad Irawan, Indonesia]	Taken into account. The statement was made to reflect the high variability in North American and European temperature timeseries shown in Figure 3.8, compared to other continents. But there is no need to highlight specific regions and the statement has been deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37877	18	36	18	36	I recommend to the wider spread of Y-axis in Figure 3.8. It is difficult to recognize the time series in North America and Europe. [Junhee Lee, Republic of Korea]	Accepted. The legibility of Figure 3.8 has been improved.
37333	18	36	18	42	Do you have any evidence at all that the observed temperature plotted in these graphs is correct or are you merely making assumptions? I put it to you that if you haven't audited the data then you have no idea if it is correct or not. I refer you to McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset". [John McLean, Australia]	Rejected. See relevant discussion in Chapter 2.
127267	18	47	18	57	This lede is completely buried. [Trigg Talley, United States of America]	Rejected. This paragraph is the summary of the whole section, so is rather prominent.
83577	18	47	19	21	This comment refers to concerns about the framing of the summary statements on attribution of global temperature change in chapter 3. In its current form, the text lacks clarity and exposes itself to misinterpretation, particularly by non-expert audiences. 1. The SOD states that "new literature has emerged which better accounts for methodological and climate model uncertainties in attribution studies (Ribes et al., 2017; Hannart and Naveau, 2018), reporting results consistent with probabilities above 99% for human activities causing more than half of the observed warming over the 1951-2010 period' (empahsis mine). While not strictly wrong, I am concerned that this is a misleading representation of the findings of Ribes et al. (2017) which states (referring to the period 1951-2010) that 'most of the observed warming over this period (+0.65 K) is attributable to anthropogenic forcings (+0.67 ± 0.12 K, 90% confidence range), with a very limited contribution from natural forcings (-0.01 ± 0.02 K)'. Ribes et al. (2017) does not provide an estimate for the lower bound of attributable warming at the 99% confidence level. Rather, the main conclusions of this paper are that there is high confidence that, for this period, anthropogenic warming is approximately equal to observed warming and it is at least as likely that anthropogenic warming exceeds observed warming than observed warming exceeds anthropogenic warming. 2. Providing lower bounds for the portion of warming attributable to human activities at the 99% confidence level, but not providing equivalent statements for the upper bounds (insofar as this is available in the published literature) risks misleading readers. As noted on p19 lines 6-16 (for lower confidence levels) our best estimate of the magnitude of anthropogenically-forced warming is that it is approximately equal to the observed warming, with some uncertainty either way. However, the headline assessment is that it is 'extremely likely that human influence is the main driver of the observed increase in global-mean surface air temperature, causing more than half of the observed warming in 2010-2019 relative to 1850-1900'. I am aware of high-profile legal procedings (and other circumstances) where the	Accepted. The opening of the paragraph has been rewritten to avoid the "half of observed warming" statement and prefer the comparison to full observed warming. This also better feeds into the revised ES statement.
93579	18	47	19	30	Not very clear how the assessment has changed compared to AR5. Not clear why only 10 years (2010-2019) is being compared to 50 years (1850-1900). [Nnamchi Hyacinth, Germany]	Taken into account. The text now notes that "Progress in attribution techniques allows the important advance of attributing observed GSAT warming since 1850-1900, instead of since 1951 as was done in the AR5." Regarding averaging periods, they are harmonised at the report level, see Chapter 1.
37317	18	49	14	50	"consistent with" isn't proof. McLean (2014) "Late Twentieth-Century Warming and Variations in Cloud Cover" Atmospheric and Climate Sciences, showed that the temperature pattern was consistent with a shift in the ENSO and then changes in cloud cover. [John McLean, Australia]	Rejected. See response to comment number 37305.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116191	18	50	18	50	remove one of the "caculated" words. [Valerie Masson-Delmotte, France]	Accepted. The sentence has been rewritten and calculated is now only used once.
89885	18	52	18	53	As before: … caused more than half, but less than 170% the observed warming trend … [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The "more than half" statement has been rewritten following comment number 83577.
71367	18	53	18	53	Is the assumption of normality really sensible? To which uncertainties does it apply? We have an ensemble of opportunity which is most likely not normally distributed. So we should avoid carrying over naive assumptions into the assessment report. At least a comment would be useful, i.e., an assessment of the sensibility of this assumption and the resulting likelihood statement. [Douglas Maraun, Austria]	Noted. The "more than half" statement has been rewritten following comment number 83577, so the assumption of a normal distribution of uncertainties is not needed any more.
10623	19	1	19	5	I am confused. Earlier it is claimed that CMIP5 underestimates observed variability (page 14:45-49), but here now there is concern that CMIP6 is over estimating variability? I have had a look at piControl variability in CMIP3, CMIP5, and CMIP6 and didn't notice much in the way of major differences. Are the authors cherry picking certain models as outliers? One needs to be careful how to interpret an "ensemble of opportunity". [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The assessment is now that there is no evidence in the CMIP6 ensemble for a large error in modelled surface temperature variability.
50705	19	1	19	21	It would be useful to policymakers if authors are able to make a best estimate of human induced GSAT warming here too to remain consistent with SR1.5 - if it is scientifically appropriate to do so. Without it, it seems like there is less certainty than a 18 months ago about the extent of human-induced warming. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A best estimate is now assessed.
110901	19	1			See above - Friedman et al raises questions about some but not all models [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Statement toned down as suggested.
104417	19	2	19	3	Please remove first citation to Parsons et al. as this work characterizes the range of variability and associated spatial patterns. Parsons et al., 2017, J Clim (DOI: 10.1175/JCLI-D-16-0863.1) does question models' ability to simulate temperature variability across a range of timescales, as do others such as Laepple and Huybers, previously cited in this chapter. [Luke Parsons, United States of America]	Accepted. References revised as suggested.
35611	19	2	19	13	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
66977	19	5	19	6	SR1.5 was dealing with GMST -- that could be said more clearly. [Aurélien Ribes, France]	Accepted. This is now clarified in the statement.
80277	19	6	19	18	The chapter lacks a summary of what distinguishes CMIP5 and CMIP6 simulations (or reference to where to find such information in the whole report). Since stratospheric ozone depletion has been a major driver of surface climate change in the Southern Hemisphere in recent decades and its recovery will also have an impact, it would be interesting to know how processes linked to stratospheric ozone are taken into account in the new CMIP6 or CCMI simulation. I acknowledge however the fact that stratospheric ozone depletion is better accountedfor in this chapter than in chapter 2 and I like the broad term "anthropogenic forcing" used in the executive summary. However in the chapter this broad term is often linked in the chapter to aerosols, ozone and greenhouse gases. In the case of ozone, it is not clear whether it relates to tropopsheric ozone increase due to emission of procursors or stratospheric ozone depletion, which are 2 different issues. [Sophie Godin-Beekmann, France]	Rejected. The chapter cannot go to that level of detail, although the role of stratospheric ozone changes in driving upper air temperature changes is discussed in section 3.3.1.2. CMIP6 simulations of stratospheric ozone trends are discussed in Chapter 6, section 6.2.2.5.2, although that section does not discuss progress since CMIP5.
127269	19	6	19	21	This could be put in a diagram. [Trigg Talley, United States of America]	Taken into account. The process is best described in the text but Table 3.1 has been added to carry values from individual studies and the outcome of the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11961	19	11	19	11	Is "scaling factor" corrected to "adjusment factor"? In Cross Chapter Box 2.3, "4.3%" is not explicitly referred to. Please refer to a corresponding value. [Masaki Satoh, Japan]	Noted. Scaling from GMST and GSAT has been revised in Cross Chapter Box 2.3 to a best estimate of 1, so there is no need for the statement.
66979	19	11	19	11	This range for ANT-induced warming seems a bit wide to me. Given the reported ranges are all 90% or 95% confidence ranges, which are here considered as likely ranges only (and this treatment seems appropriate), could you take min max of published studies and then round to .1°C rather than "span all"? Given revision of Gillett et al., I suspect that this would lead to .9-1.3°C. For instance, the lowest lower bound comes from Ribes et al. (submitted) and is .88°C, which is still subtantially higher than the currently proposed .8°C lower bound. The same treatment could be applied to GHG-induced and OA-induced changes. [Aurélien Ribes, France]	Rejected. This is conservative but justified in the context of an assessment.
10627	19	11	19	21	I would strongly recommend not including the 4% "adjustment" here to account for the model estimated difference between "GMST" and "GSAT". The use of an adjustment to "GSAT" to get "GMST" over simplified in this report, but more so here. The process of rounding attributed trends to 1 decimal place introduces bigger uncertainties/biases than the "adjustment". The nuances of how to apply an adjustment are also not considered, which given how few studies have explored the issue, is perhaps not surprising. The adjustment is almost entirely based on the analysis, methods, code and data of one study (Cowtan et al 2015). There needs to be more critical assessments before such adjustments can be applied with this much confidence. I have recently looked at this issue and deduced that there is some over-confident reasoning in studies reporting the importance of the difference between "GMST" and "GSAT" in models (Jones, `Apples and oranges': on comparing near surface temperatures from climate models with observations, submitted Q.J.R.Meteorol. Soc., 2019). I recommend not putting so much emphasis on this minor issue, compared to all the other observational, model, methodological uncertainties we do know about. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Cross-chapter Box 2.3 has been revised, and the new assessment is that the best estimate of the scaling factor between GMST and GSAT is 1. Chapter 3 has been revised to reflect this updated assessment.
102849	19	12	19	13	It is unclear why this single study by the CLA has to be mentioned here, when all other studies are summarized in the half-sentence before. Its prominent place looks forced and self-promoting. [Philippe Tulkens, Belgium]	Noted. The study is the only one that attempts the scaling from GSAT to GMST. The issue is however moot, since Chapter 2 revised their best estimate of that scaling to 1. The statement has been deleted.
89887	19	15	19	16	Why suddenly GSAT rather than GMST? What is the natural attributable GMST warming which I would expect to be stated here before moving on the GSAT. Very confusing as is. [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The GSAT assessments have been grouped together.
34869	19	17	19	22	The key (but rather weak) SOD statement is that “human influence caused more than half of global warming 2010-2019 relative to 1850-1900”; this causes into question the usefulness of CMIP6 models and the inappropriate degree of confidence expressed in all SOD conclusions. Please see general comments #2 and #13 above. [Jim O'Brien, Ireland]	Noted. The "more than half" statement has been rewritten following comment number 83577.
10629	19	18	19	21	An additional assessment of trends after 1950 will help in comparison with previous ARs. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Table 3.1 has been added to give assessed attributable anthropogenic warming for four periods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21471	19	18	19	21	This risks perpetuating the communications issue around the more than half. The best estimate is all of the warming is caused by anthropogenic influences and this formulation risks being abused in the same way the AR5 statement was. Consider reformulating to lead with the best estimate of the change attributable and not the lower bound. Even better give the best estimate and the bounds and then use that to drive a finding that stresses both the upper and the lower bounds, not just the lower bound. [Peter Thorne, Ireland]	Taken into account. The "more than half" statement has been rewritten following comment number 83577. A best estimate is now given.
110903	19	19			I agree with that assessment [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
89889	19	20	19	21	And one last time: …causing more than half of observed warming in 2010-2019 relative to 1850-1900, but less than 170%, with a best estimate of ~100%. After all, for the trend in the very next sentence (line 23/24), a best estimate is immediately provided. So absolutely no reason to refrain from stating it for human-induced warming as well. [Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The "more than half" statement has been rewritten following comment number 83577. A best estimate is now given.
10625	19	21			Cross chapter box 2.3 claims the adjustment is "4%", not "4.3%". [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The study is the only one that attempts the scaling from GSAT to GMST. The issue is however moot, since Chapter 2 revised their best estimate of that scaling to 1. The statement has been deleted.
34871	19	23	19	30	The SR1.5 estimated warming of 0.2°C/decade was rebutted in the Bates paper “Deficiencies in the IPCC’s Special Report on 1.5 Degrees – Revised and Updated Version”, a paper by Prof J Ray Bates, published by the GWPF, January 2019, see www.thegwpf.org/content/uploads/2019/01/Bates-2018b.pdf. Observations based on satellite data indicate a warming of ~0.1°C/decade since 1979. Please see general comments #2 and #3 above. [Jim O'Brien, Ireland]	Rejected. The suggested reference looks at lower troposphere temperature trends, which are different from surface air temperature trends in non-trivial ways, so cannot be directly compared.
66981	19	23	19	30	Widening published uncertainty ranges seems appropriate, but there is a suspicion that the 0.1--0.3°C/decade is slightly biased low. In Ribes et al. (submitted), taking all 3 scenarios considered, the estimated ANT-induced warming range varies from +.14 to +.30°C/decade. So, rounding to .1°C/decade might remove about 10% of the current rate (.2°C/decade instead of .22°C/decade). [Aurélien Ribes, France]	Noted. The bias is slight compared to the width of the uncertainty range.
35613	19	24	19	25	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
93405	19	26	19	27	Please double check that these numbers are correct, because they don't make much sense, attribution greenhouse gas forcing being equal or larger than the total attributed estimation. The referenced paper has not been published yet, so I couldn't check this. [Carles Pelejero, Spain]	Rejected. Greenhouse gas warming is expected to be larger than observed warming, since other anthropogenic forcings (especially aerosols) contribute a cooling.
2585	19	27			define and specify "SSP" [Bryan Weare, United States of America]	Accepted. The acronym has been spelled out.
21473	19	29	19	30	for attributable surface temperature warming rates surely? The observed rate would be the domain of chapter 2 and not chapter 3. [Peter Thorne, Ireland]	Accepted. The text now clarifies that this is the anthropogenic attributable warming rate.
35615	19	30	19	30	° C repeats [Carlos Antonio Poot Delgado, Mexico]	Rejected. The repetition avoids misinterpretations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35253	19	33	27	11	This comment pertains to the sections on tropospheric temperatures (beginning on page 3-19) and precipitation (ending on page 3-27), with special reference to Figure 3.9, the 1998-2014 vertical simulations. The text needs to be modified because of the extremely large errors that the non-prescribed-SST are making, centered around 300mb. These are the red lines, where the errors average around 0.4^{o}K/decade. That is 1.2^{o}K in three decades—but the error appears to get even larger as lower troposphere temperatures increasingly warm. The text should note that this forces large systematic errors in both convection and the transport of moisture aloft. This in turn will affect midlatitude precipitation simulations in agriculturally critical regions; a substantial majority of the moisture that falls in the corn/soybean/wheat production regions in North America east of the western cordillera is of tropical origin. More likely than not the CMIP6 models have a systematically reduced tropical moisture flux as a result of the overestimation of warming in the upper troposphere. In turn this has to affect temperature forecasts because the model will simulate systematically drier tropical and temperate latitudes so that sensible heating is overestimated on a systematic basis. Figure 3.9, 1998-2014 also reveals a profound disconnect in the stratosphere. While the radiosonde and ERA data show a very slight warming trend (appearing to average around 0.1^{o}K/decade), the nonprescribed (red) models maximize their cooling around -0.3^{o}K/decade. They are still simulating an ozone-depletion cooling where there should be little (and none was observed in the upper air records). The stratosphere is extremely dry so there isn't any convection or moisture advection to complicate things. Operationally, this error is probably of little sensible consequence, but physically it means that the models are incapable of simulating physically uncomplicated place. It is also inadvisable to remove Figure 3.9. That wouldn't be consistent with the tenor of the WG1 AR-6, which is the most candid and inclusive of all of the IPCC assessments. But it is very important information that will surely be cited in subsequent policy discussions. [patrick Michaels, United States of America]	Taken into account. Text has been revised and Fig. 3.9 has been updated.
67555	19	42	19	42	In comparison to AIRS, CMIP5 models can generally reproduce the climatological features of tropospheric air temperature well, but the models have a tropospheric cold bias (around 2 K), especially in the extratropical upper troposphere (Tian et al., 2013) Tian, B., Fetzer, E. J., Kahn, B. H., Teixeira, J., Manning, E., & Hearty, T. (2013), Evaluating CMIP5 models using AIRS tropospheric air temperature and specific humidity climatology, J. Geophys. Res., 118(1), 114-134, https://doi.org/10.1029/2012jd018607 [Baijun Tian, United States of America]	Taken into account. Note that our focus in this section is evaluation of temperature trends in the troposphere, rather than the absolute biases in models. However, we do now cite Tian et al. (2013) in the section on atmospheric water vapour.
595	19	42	20	28	Could we add a sentence about the observed change message (like the surface temperature?) in the tropospheric temperature in this part, not only the comparison between CMIP5 and CMIP6. [ZHIYAN ZUO, China]	Accepted. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21477	19	42			This section gets itself very confused in comparing constrained studies - looking at the ratio of surface to troposphere trends - and unconstrained studies looking at absolute trends. The studies looking at the constrained aspect of the ratio of trends aloft compared to the surface consistently find no evidence of a discrepancy. The studies which look in the unconstrained component of absolute trends find an overestimate. That tells me that the issue is primarily one of the models overestimating the surface warming which gets propogated and becomes more marked with height as would be expected given that the amplification is an emergent property. I think you need to pick apart and treat very distinctly studies (or components thereof) which looked in absolute trend space from studies which (components of) looked at the vertical amplification aspect. I would start by discussing the former then proceed to discuss the latter. The assessment finding that comes out would likely be clearer and I would suggest would be stronger in judging that the absolute trend divergence primarily results from the CMIP5 and CMIP6 ensembles over-estimating the surface warming and not a fundamental issue with the model physics per se. [Peter Thorne, Ireland]	Accepted. Text revised.
7327	19	43	20	16	Understood that CMP CMIP3 and CMIP5 models overestimated the observed warming trend in the tropical troposphere during the satellite period 1979-2012 in AR5. AR6 uses CMIP5 and CMIP6. But, AR5 (2nd order draft) explains that CMIP6 modelsoverestimate temperature trends compared to radiosonde data in the tropical troposphere significantly between 300 and 100 hPa over the 1979-2014 period forced by anthropogenic and natural forcings again. It is confusing. More simply clarification is requested. [SAN WIN, Myanmar]	Accepted. This part has been rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52829	19	43	20	28	The authors have chosen to repeat contestable language that observational tropospheric trends show "low confience" when a statistical analysis shows the various datasets are quite close, especially when accounting for known (i.e. published) errors. (See comments in chapter 2 pg 41, 42). Regarding errors, briefly, it has been demonstrated that satellite datasets such as RSS and NOAA contain spurious warming in the NOAA-14 period (Christy et al. 2018, IJRS). Then, radiosonde networks have a spurious warming shift in 2010 due to a change in the Vaisala RS92 software for whcih corrections have not yet been applied in RICH and RAOBCORE. This warm shift is +0.14 C as determined indepedently by 3 satellite datasets and the ECMWF Reanalyses (also Christy et al. 2018). Yet even without accounting for these, the observational tropical trends are fairly close, and certinaly not of "low confidence" relative to the wide range of model results. Indeed the Reanalyses show extremely high consistency. Think of it this way, if +/- +0.04 C/decade is low confidence, what is the confidence one would place in the ECS of the AR6 models which ranges over a FACTOR of THREE? See BAMS State of the Climate reports for expressions of trends, confidences and model comparisons - there is little doubt to the objetive observer that the models are signficantly too warm (and this has been published). The comparison between observations and AR6 models will be shown in the upcoming State of the Climate 2019 report, indicating with extrememly high confidence that AR6 models overstate the tropical atmospheric warming. As McKitrick and Christy 2018 demonstrated for CMIP5 models, the same is true for CMIP6 models (McKitrick and Christy, 2020 submitted). The "smaller inconsistencey with fixed SSTs" doesnt state the real conclusion of this result which is that models must have positive feedbacks that are not realistic (this should be the conclusion stated in the text.) The section should state categorically that models with the latest forcing are running too hot. Trying to explain some of the discrepancy with forrcing errors or natural variability errors (but see next comment) are not convincing because the evidence is too weak and with 41 years, the period-length gives high confidence the models are simply deficient. That said, I do think there is a problem in Fig. 3.9 discussed next. [John christy, United States of America]	Accepted. Text revised.
21475	19	50	19	53	This choice is hugely problematic for reasons you have just succinctly outlined in the prior sentence. The constrained model aspect is the ratio of the trends between surface and aloft - first shown by Santer et al., 2005 and then shown repeatedly, most recently in Mitchell et al., 2013 and for a large model ensemble by Suarez-Guitterez et al.. If the CMIP6 models warm the tropical surface too quickly - which most do - then we would expect to see a growing divergence aloft. In fact if we didn't see this it would be a far bigger problem because it would cast substantial doubt upon our fundamental understanding of the physical processes underlying vertical mixing in the tropical troposphere. I would urge these data be replotted using the ratio of triopospheric change to surface change which is the emergent constraint behaviour across all models as has been known now for 15 years. [Peter Thorne, Ireland]	Accepted. This part has been rewritten.
26725	19	53	19	53	The differences over 1998-2014 are also very large, with no observed warming but a strong warming in most models. It may not be judicious to show the trends on 1998-2014, as the 1998 El Nino event has a huge impact, unless a solid discussion about that is provided. [Eric Brun, France]	Taken into account. This figure is for the analyses of tropospheric and stratospheric temperature. For the stratospheric temperature, 1998 was chosen because the first period is the ozone depletion period 1979-1997 and the second one is the ozone recovery period 1998-2014. We have clarified this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127271	19	53	19	53	Should this time period be "1998-2014"? [Trigg Talley, United States of America]	Taken into account. This text has been re-phrased, and it is now clear that the meaning is that when looking at the whole period shown (1979-2014) the differences are largest in the upper troposphere.
102851	20	16	20	16	delete ", but a discrepancy remained." This is implied. [Philippe Tulkens, Belgium]	Editorial. Revised.
104973	20	18	20	18	"models indicated more warming than observations" -> "model simulations warm more quickly than observations" [Peter Gleckler, United States of America]	Editorial. Revised.
11301	20	18	20	28	The disrepacy between models and observations in terms of the tropospheric warming rate appears interesting. I tend to agree with the argument 'in part because of an everestimate of the SST trend pattern', and then should you assess the SST trend pattern somewhere in the Chapter (probably in 3.5.1)? [Masahiro Watanabe, Japan]	Taken into account. Text revised.
116197	20	18	20	28	This paragraph is hard to follow. It refers to an overestimation of surface warming (where is it assessed? I could not find this in the section on ocean surface temperature, in section 3.4). [Valerie Masson-Delmotte, France]	Taken into account. This part has been rewritten.
35617	20	19	20	20	Bibliographic citations in chronological order [Carlos Antonio Poot Delgado, Mexico]	Editorial. Revised.
35619	20	38	20	38	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Editorial. Revised.
87933	20	42	21	17	Why does this section only refer to post-1979? McKitrick and Vogelang 2014 ("HAC-Robust Trend Comparisons Among Climate Series with Possible Level Shifts" Environmetrics DOI: 10.1002/env.2294) looked at the nature of the warming of the tropical troposphere over 1958 to 2012 and found the observations were best represented by a step-function with a break in the late 1970s and no significant trend on either side, whereas the models show a smooth trend with no break. Thus not only did the models overstate warming in the tropical troposphere (as pointed out in 3.3.1.2) but they characterize its time profile differently, which goes against the idea that anthropogenic forcings were dominant over the whole period. The Santer references in this section look at relatively short time intervals and don't address the 1970s step change. [Ross McKitrick, Canada]	Noted. The focus on post-1979 data is based on the availability of multi-observational data.
127273	20	43	20	43	Drop first sentence. [Trigg Talley, United States of America]	Accepted. Revised
21479	20	43	21	14	This remains potentially problematic in that all studies are led by one author. I certainly don't think you can say "has received much attention" when that attention then appears to be from one investigator. I also suspect that nuancing the confidence / likelihood from AR5 based upon works by one author may prove problematic. Equally, I am not sure I have come across any formal D&A studies by others so this situation may be unavoidable. But, I would flag that it is potentially problematic to base the whole assessment on the work of a single lead author presumably using (variants upon) a single underlying technique. The comparison to the surface temperature assessment is stark. [Peter Thorne, Ireland]	Accepted. We add more references.
127275	20	47	21	17	These two paragraph include a lot of methods discussion, which could potentially be cut or summarized more concisely. The lede on page 21, lines 16-17 is again buried. [Trigg Talley, United States of America]	Accepted. Text revised.
9983	20	52	20	52	A suggestion to include more clarity on what "multiple observationally-based datasets" (reference datasets) and the "multiple models" used by Santer et al. (2018) to conclude that there is clear human influence on the changes of seasonal cycle. [Renard Siew, Malaysia]	Taken into account. Because of limited space, we chose not to include more details here.
11963	20	52	20	52	"cyle" should be "cycle". [Masaki Satoh, Japan]	Editorial. Revised.
19767	20	52	21	24	4 typos: looks like a problem with the keyboard. [philippe waldteufel, France]	Editorial. Revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9985	20	53	20	53	Define "Human influence" used by Santer et al. (2018) in their study. I think this is actually important to ensure there is no confusion in the report. [Renard Siew, Malaysia]	Taken into account. This text has now been re-phrased, and 'human influence' in quotes no longer appears in the sentence, and has been replaced by 'anthropogenic forcing'.
26727	21	1	21	2	All this section is very detailed for a limited number of studies. It could be reduced and highlight only the key findings. [Eric Brun, France]	Accepted. Text revised.
87935	21	4	21	8	What is "stochastic uncertainty"? It sounds like a redundancy--like saying stochastic randomness. When you say they "quantified the stochastic uncertainty" does that mean they estimated a variance? [Ross McKitrick, Canada]	Taken into account. Text revised.
102853	21	9	21	9	affected [Philippe Tulkens, Belgium]	Editorial. Revised.
2589	21	9			spelling "affected" [Bryan Weare, United States of America]	Editorial. Revised.
11965	21	10	21	17	It is not clear the logic of this part: "If CMIP5 overestimate the observed natural variability, the previous detection resuts may be conservative". Need more specific rather than stating "conservative". [Masaki Satoh, Japan]	Taken into account. Text revised.
102855	21	11	21	11	timescales [Philippe Tulkens, Belgium]	Editorial. Revised.
102857	21	13	21	13	delete "thus" [Philippe Tulkens, Belgium]	Editorial. Revised.
112263	21	19	22	29	Regarding stratospheric temperature trends, some literature is reviewed regarding the attribution of trends to different factors, for example that "about two-thirds of the global long-term cooling is attributed to GHGs and one third to ozone depletion". However, I would find it helpful if the physical mechanisms by which the cooling is brught about would also be explained briefly. While the mechanism for ozone is very straight forward (but should still be mentioned), it is not as trivial for the GHG-induced effect on stratospheric temperatures. This is something that has likewise not been addressed properly in previous Assessment Reports. Please excuse this self-promotion, but I would recommend to refer to Goessling and Bathiany 2016 (https://www.earth-syst-dynam.net/7/697/2016/), where we have not only disentangled the different mechanisms by which different GHGs affect stratospheric temperatures, but also ran idealised AGCM experiment to roughly quantify the contributions through different mechanisms. We concluded that "[the indirect solar effect and the blocking effect] contribute roughly equally to the CO2-induced cooling, with the indirect solar effect dominating around the stratopause and the blocking effect dominating otherwise." [Helge F. Goessling, Germany]	Accepted. Text revised.
79991	21	24	21	25	Chemistry-Climate Model Validation Activity (CCMVal) – typo [Gabriel Chiodo, Switzerland]	Editorial. Revised.
18693	21	25	21	25	"Avtivity" => "Activity" [Govindasamy Bala, India]	Editorial. Revised.
102859	21	25	21	25	Activity [Philippe Tulkens, Belgium]	Editorial. Revised.
6599	21	26	21	26	"ozone depletion by ozone depleting substances" reads strangely, and the acronym ODS for ozone-depleting substances is introduced only later on the page, in line 42. The acronym should be introduced on line 26, which could use text such as "ozone depletion by chemical interactions involving other trace species, known as ozone depleting substances (ODS)" [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised.
79993	21	30	21	48	I find this paragraph too qualitative – I would recommend adding numbers to each of the statements concerning cooling of the lower stratosphere [Gabriel Chiodo, Switzerland]	Accepted. Revised.
13333	21	37	21	37	GCM acronym normally used for global climate model not radiosonde observations and climate. Change acronym [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79995	21	44	21	44	It would be good to add another reference concerning the flattening of the temperature trends since cessation of the ozone depletion (via ODS); Polvani et al., 2019; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JD029516 [Gabriel Chiodo, Switzerland]	Accepted. Revised.
19769	21	45	21	48	Actually Figure 3.9 suggests that, both below and above the tropopause, trends of the temperature according to models are considerably less sensitive to ozone concentrations than the real world. In case this interpretation is meaningless, it would be wise to warn the reader that he/she should not believe his/her eyes too rapidly. [philippe waldteufel, France]	Taken into account. Also please see Mitchell et al. (2020).
116199	21	47	21	47	What are the possible deficiencies in prescribed ozone changes. [Valerie Masson-Delmotte, France]	Taken into account. Reworded to "poor representations of stratospheric ozone forcing", because issues are not limited to models with prescribed ozone.
112659	21	47	21	48	"underestimate it over 1998-2014, which they speculate may be due to possible deficiencies in prescribed stratospheric ozone changes." Could we also attribute it to the natural phenomenon, such as El Niño or La niña? Then, it woudln't be directly because of human acitivity [Melissa Jiménez Gómez Tagle, Germany]	Rejected. The disagreement between models and observations in stratospheric temperature trends over these periods is largely outside the range of simulated internal variability (see Figure 3.10b,c), suggesting it cannot be explained simply by internal variability.
104975	21	52	21	52	"which are in better agreement than previous versions". Better than what? This point is also confusing on p22 line 24. Are you trying to say "the consistency between observed and simulated changes in global mean temperature through the depth of the strasophere more consistent than comparisons made with older versions of the observational data? [Peter Gleckler, United States of America]	Accepted. Text revised.
32669	22	1	22	55	In some of the pages the title under the subtitle don`t have any number. It would be better to be numbered Or be marked with a sign [sadegh zeyaeyan, Iran]	Editorial. We use non-numbered subtitles in some sections.
32999	22	1	22	55	In some of the pages the title under the subtitle don`t have any number. It would be better to be numbered Or be marked with a sign [Sahar Tajbakhsh Mosalman, Iran]	Editorial. We use non-numbered subtitles in some sections.
105049	22	3	22	3	"AMIP-type" has not been defined or explained anywhere. [Peter Gleckler, United States of America]	Taken into account. Text revised.
13335	22	4	22	4	AMIP must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Text revised.
13337	22	5	22	5	AMSU must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Text revised.
100793	22	5	22	5	I would suggest to replace North Atlantic Blocking with Euro-Atlantic blocking, since the underestimation is particularly evident in the central Europe region (see Figure 2 from Davini and D'Andrea 2020 –in revision). [Corti Susanna, Italy]	Taken into account. Text revised.
79997	22	21	22	21	Again, I find this statement very qualitative – it would be good to know also what the relative importance of volcanic eruptions and the solar cycle are [Gabriel Chiodo, Switzerland]	Accepted. Text revised.
6601	22	28	22	28	See preceding comment. The acronym ODS can be used here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised.
28277	22	32			See also attribution of changes in dry season water availability. Padrón, R. S. et al. (2020). Observed changes in dry season water availability attributed to human-induced climate change. Accepted in Nature Geoscience. This reference is also relevant for subsecion in Chapter 2, page 48, line 30 about changes in P minus ET and for Chapter 8, section 3. [Ryan Padrón, Switzerland]	Rejected. Chapter 3 does not consider (P-E). This reference is very relevant to section 8.3.1.1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52867	22	32			What about "Humidity and precipitation" and merging the assessment of observed changes in streamflow with Section 8.3.1.5 in Chapter 8? [Hervé Douville, France]	Rejected. Chapter 3 also considers changes in global streamflow. So, we kept the title.
88951	22	36	22	37	This statement could be made a bit clearer. Does this mean that the sign of change depends on location , period , or process? [Schurer Andrew, United Arab Emirates]	Taken into account. Added "Moreover, the sign of the change depends on location and time of the year."
52869	22	37	22	38	This is true and nicely illustrated by the remaining paragraph. Yet, a more balanced assessment could also highlight that paleo constraints on attributable or projected changes should be considered cautiously since different dominant mechanisms can mediate the climate response to current versus past radiative forcings (e.g. D'Agostino et al. 2019). [Hervé Douville, France]	Not considered. In what follows, we discuss last millennium paleoclimate features which do indeed involve similar forcings as present. Moreover, even if the forcings are different that does not preclude use of past climates to understand physical mechanisms.".
110905	22	37			explain that the mixed signs of precipitation changes can lead to attenuated signals and with tha low signal to noise ratio (otherwise this sentence is hard to understand) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Added "This low signal-to-noise ratio hinders the emergence of the anthropogenic signal from natural variability."
26729	22	40	22	40	What does "drying" mean here: reduction of precipitation, of soil moisture, or of runoff?. If precipitation is concerned, as suggested by the context, the link with the evaporative demand is not that obvious. [Eric Brun, France]	Taken into account. Removed the sentence on "evaporative demand".
100653	22	42	22	42	See Williams et al. (2020; Science) re:recent megadrought [Matthew Kohn, United States of America]	Taken into account. Included: "Furthermore, Williams et al (2020) used a combination of hydrological modelling and tree-ring reconstructions to show that the period from 2000 to 2018 was the driest 19-year span since the late 1500s."
11967	22	45	22	52	Three examples of recent drought are mentioned. Please check consistency with other chapters such as Chap 8 and 11. [Masaki Satoh, Japan]	Noted. Text has been written in coordination with chapter 8.
127277	22	46	22	49	With respect to California, is it worth also mentioning the wildfires? This may be addressed in other chapters, but it's broadly related to the hydrological cycle. [Trigg Talley, United States of America]	Rejected. Chapter 3 does not include wildfires as there are no continental scale data base and these are not among the large-scale climate indicators agreed in Table Cross-Chapter Box 2.2.
87937	22	48	22	50	Griffin and Anchukaitis (2014) found the 2012-2014 California drought was exceptional in the paleo record, but the change in precipitation was not--it was an ambiguous finding. Christy and McKitrick (Assessing Changes in US Regional Precipitation on Multiple Time Scales Journal of Hydrology vol. 578 Nov 2019, https://doi.org/10.1016/j.jhydrol.2019.124074) show evidence that recent precipitation and drought in western and eastern US records is within natural variability measured either in daily records over 150 years or annual paleclimate records back 2000 years. [Ross McKitrick, Canada]	Taken into account. Added reference. The sentence that refers to Griffin and Anchukaitis 2014 does not mention rainfall as the reason.
88907	22	50	22	50	Rowell et al. (2015, JCL, p9768) also provides observational evidence of the decline of East African rains (as do others therein), its historical context over the last ~130 years (data not used in other studies), and uniquely an overview of the plausible drivers of this decline. Also note this decline is for Long Rains season only (not the Short Rains). [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Included reference.
39705	22	53	22	53	"...prolonged megadroughts (droughts lasting two decades or more)" -> Note that tthe glossary definition for megadrought is "A very lengthy and pervasive drought, lasting much longer than normal, usually a decade or more." [TSU WGI, France]	Taken into account. Deleted the definition; the reader can consult the glossary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19771	22	55	23	1	So megadroughts, observed through the last millennium before the 20th century, have been associated to internal climate variability with medium confidence. But can anyone imagine another explanation? Both solar activity and volcanic events can certainly be ruled out by climate scientists, as well as orbital forcing. Hence: why MEDIUM confidence? [philippe waldteufel, France]	Taken into account. Deleted "medium confidence".
111033	22	55			This page would have been a good one to cite Kate Marvel's recent nature paper on drought detectible by the mid 20th century [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Included reference in subsection on precipitation.
116203	22		22		In some occurrences, there is duplication between the assessment of paleoclimate evidence in this chapter with the one done in chapter 2. I expect chapter 2 to make the assessment of reconstructions, and chapter 3 to focus on model data comparisons, and understanding past changes (attribution). But there are places where a full assessment of observational evidence, including at regional scales, is developed here (eg in 3.3.2 for drought). This needs careful consideration so as to place thisimportant and relevant aspect at the right place in the right chapter, so as to have a logical flow of information within and across chapters. [Valerie Masson-Delmotte, France]	Accepted. Text revised.
79481	22				In some of the pages the title under the subtitle don`t have any number. It would be better to be numbered Or be marked with a sign (comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Editorial.
71369	23	4	23	4	I would add “tropical” between large-scale and atmospheric. [Douglas Maraun, Austria]	Not applicable. Content of this paragraph has been moved to Sections 3.3.3.1 and 3.3.3.2 and the particular sentence has been removed.
52871	23	4	23	21	Shift part of this paragraph to Section 3.3.3? You may also consider reversing sections 3.3.2 and 3.3.3 since precipitation changes are the results of changes in atmospheric moisture but also of large-scale circulation. [Hervé Douville, France]	Taken into account. We moved paragraph of lines 4-21 to section 3.3.3. We kept order 3.3.2 and 3.3.3 to maintain same order as in chapter 2.
4017	23	7	23	10	Given that this discussion is setting up the interpretation and attribution of modern-day tropical changes against a background of mid-Holocene change (certainly for the Harrison reference), a caveat should be stated that palaeo-time periods are not necessarily appropriate constraints for CO2-induced changes in the monsoon, since the nature of the mechanism is different. See the work of D'Agostini et al. (2019) on the subject: https://doi.org/10.1029/2018GL081589 "Northern Hemisphere Monsoon Response to Mid-Holocene Orbital Forcing and Greenhouse Gas-Induced Global warming" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Not considered as this paragraph was deleted.
127281	23	12	23	14	Needs to include Wara et al. (2005) in citations. [Trigg Talley, United States of America]	Rejected. The suggested paper focuses on proxy evidence which is assessed in Chapter 2. We have revised to cite the Cross-Chapter Box 2.4.
127279	23	12	23	16	Confusing text: "Paleoclimate data from the Pliocene epoch suggest that there was a reduction in the zonal and meridional gradients of SST in the tropical Pacific ... with similar CO2 as today. Some studies suggest that this higher concentration of CO2 at that time (410 ppm) weakened the Walker circulation ... " The first sentence suggests that Pliocene CO2 was similar to today but the second suggests it was higher than today. What is meant is that CO2 in the Pliocene was high relative to other geologic epochs, just as today is high relative to recent history, correct? Clarification needed. [Trigg Talley, United States of America]	Accepted. This sentence has been moved to Section 3.3.3.1. The higher CO2 concentration here is relative to the preindustrial level. We have revised to clarify it.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50707	23	12			Not sure the reference to Haywood et al. 2013 is robust here. This paper merely states that "The East Asian Summer Monsoon, as well as other monsoon systems, may have been enhanced" and then references the Wan et al., 2010 paper. There is no specific monsoon analysis in this paper other than pointing out increases in precipitation in 'regions influenced by monsoons'. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence has been moved to Section 3.3.3.2. Haywood et al (2013) show that precipitation overall increases over land including monsoon domains. The sentence has been revised accordingly.
83523	23	14	23	14	I recommend to cite here also the new compliation of McClymont et al. (submitted), which is cited in Chapter 2: McClymont, E. L., Ford, H. L., Ho, S. L., Tindall, J. C., Haywood, A. M., Alonso-Garcia, M., et al. (submitted). Lessons from a high CO2 world: an ocean view from ~ 3 million years ago. Clim. Past Discuss. (submitted). 41 doi:10.5194/cp-2019-161. [Antje H. L. Voelker, Portugal]	Not applicable. This part has been moved to the Walker circulation section where the meridional SST gradient is not assessed.
100655	23	14	23	14	Add: "…as today, while even weaker meridional temperature gradients were present with higher pCO2 during the MCO (c. 500 ppm; Herold et al., 2008; Goldner et al., 2014; Burls et al., in review) [Matthew Kohn, United States of America]	Not applicable. This part has been moved to the Walker circulation section which focuses on the zonal SST gradient.
127283	23	14	23	16	In addition to Tierney et al., 2019, Ravelo 2006 (Oceanography) needs to be added here for changes in Walker Circulation. [Trigg Talley, United States of America]	Rejected. Our focus here is model evaluation and detection and attribution. The suggested paper discusses proxy evidence and its implications, but whether models can capture the weaker SST gradient under the Pliocene condition is not examined.
2101	23	15	23	15	410 ppmv should probably be "about 410 ppmv" or similar. Ensure the value given is consistent with chapter 2 and the pliocene cross-chapter box. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have cited Cross-Chapter Box 2.4 instead of providing the CO2 level here.
100657	23	15	23	15	Note: Be sure that reported pCO2 is consistent with other chapters and sections. [Matthew Kohn, United States of America]	Taken into account. We have cited Cross-Chapter Box 2.4 instead of providing the CO2 level here.
71371	23	16	23	16	I would replace “theory” with a more specific word. If I am not mistaken, you are referring to radiative-convective equilibrium here, so this should be mentioned (but check!). [Douglas Maraun, Austria]	Taken into account. This is more relevant to the Walker circulation section and the suggested revision has been made there.
26731	23	20	23	20	It should be clarified which feedbacks this sentence is refering to. [Eric Brun, France]	Not applicable. This sentence has been removed.
127285	23	20	23	20	Possibly specify here cloud feedbacks. Unresolved climate feedbacks is vague. [Trigg Talley, United States of America]	Not applicable. This sentence has been removed.
100659	23	21	23	21	Add: "Similar concerns apply to the MCO, where GMST and meridional temperature gradients cannot be simultaneously explained (Goldner et al., 2014; Burls et al., in review)" [Matthew Kohn, United States of America]	Not applicable. This part has been moved to the Walker circulation section where the meridional SST gradient is mentioned only in relation to the zonal SST gradient.
26733	23	24	23	24	Section 3.3.2.1 should be better connected to previous section [Eric Brun, France]	Taken into account. Subsections are being rewritten.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18335	23	24	27	10	I'm really surprised to see that this section would focus on the direct comparison between the observed and model-simulated precipitation changes during the last a several decades, such as that shown in Figs. 3.10-3.12. The observed precipitation is from one realization, whose short-term (<60yr) trends may be dominated by internal variability at regional, continental or event global scales (e.g., Deser et al. 2012; Dai and Bloecker et al. 2019). Such incorrect comparison has previously led to incorrect and misleading conclusins about models' performacne (e.g., Wentz et al. 2007, Science, DOI: 10.1126/science.1140746). Thus, its trend is not comparable with the multi-model ensmeble mean, which contains mostly forced changes. The proper comparison is to the put the observed trend within the trend PDF from the model ensemble simulations, as done in many recent studies (e.g., Dai and Bloecker et al. 2019). The IPCC AR6 appears to lack this very basic concept in model comparison and evaluation that has been emphasized in many studies of the last 10 years using large ensemble simualtions. Furthermore, besides the observational uncerternties, the large realization-dependent variaiblity makes the comparison of preciptiation from two different realizations (e.g., from two model runs or one from obervations and one from a model run) difficult. The issue applies to temeprature and other variables, although may to a lesser degree. These very basic issues appear to be not recognized and stressed in IPCC AR6. This is really disappointing given the recent signficant advances in studying the impact of interna climate varaibility on global-mean temeprature and on regional precipitation changes using large ensemble simualtions. Revelant refs.: 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. Deser C, Knutti R, Solomon S, Phillips AS (2012) Communication of the role of natural variability in future North American climate. Nat Clim Change 2:775–779. https://doi.org/10.1038/nclimate1562. [Aiguo Dai, United States of America]	Taken into account. Figure 3.10 (Figure 3.12 in the new version) has been modified to include the range of internal climate variability. In the figure regions with stippling are those where more than 90% of the models agree on the sign of the bias and the bias is greater than two standard deviations of the internal variability. This is an indication of the robustness of the biases. At the same time crosshatching in the panels that compare the biases in HighResMip simulations indicates where in at least 4 of the 5 HighResMIP models the bias in its high resolution version is smaller than in its low resolution version. Figure 3.11 (Figure 3.10 in the new version) has been reframed to represent the latitudinal gradient in terrestrial precipitation change for the NH, and more models have been included. Figure 3.12 (Figure 3.13 in the new version) has been also modified to include histograms of model trends versus observed trend.
127287	23	24	27	10	This section on "precipitation" is only about precipitation amount and should be labeled as such. It does not deal with precipitation intensity, frequency, or duration. Moreover, there are a number of evaluations that should be included. Gehne, M., T. M. Hamill, G. N. Kiladis and K. E. Trenberth, 2016: Comparison of global precipitation estimates across a range of temporal and spatial scales. J. Climate, 29, 7773-7795, doi:10.1175/JCLI-D-15-0618. Trenberth, K. E., Y. Zhang and M. Gehne, 2017: Intermittency in precipitation: duration, frequency, intensity, and amounts using hourly data. J. Hydrometeor. 18, 1393-1412, Doi: 10.1175/JHM-D-16-0263. Covey, C., C. Doutriaux, P. J. Gleckler, K. E. Taylor, K. E. Trenberth, Y. Zhang, 2018: High frequency intermittency in observed and model-simulated precipitation. Geophys. Res. Lett., 45, 12,514--12,522, doi:10.1002/2018GL078926. In addition, the material in Trenberth, K. E., 2011: Changes in precipitation with climate change. Climate Research, 47, 123-138. doi:10.3354/cr00953. All would help to frame a lot of this discussion, especially how SSTs relate to water vapor and precipitation patterns. Question whether Figure 3.12 is correct and recommend removing it. [Trigg Talley, United States of America]	Taken into account. New sentences included based on the references provided. Section title is not changed because the structure of the section follows the one of chapter 2. In addition "precipitation amount" is not commonly used. Figure 3.12 was redrawn using a low-pass filter.
88905	23	24			As above, need to include extreme rain events & flooding here. Eg. tripling of frequency in Sahel (Taylor et al. (2017, Nature), higher frequency in East Afria, and doubtless elsewhere in the tropics. Obviously very important from an impacts point-of-view. [Dave Rowell, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Extreme precipitation events are considered in chapter 11.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127289	23	24			In this section, the following reference should be relevant: Zhang, W., and G. Villarini, Greenhouse gasses drove the increasing trends in spring precipitation across the Central United States, Philosophical Transactions A, 2020 (in press). [Trigg Talley, United States of America]	Rejected. This chapter is concerned with continental and larger scales.
13405	23	29	23	31	It's recommended to address the causes that limit the characterization of precipitation in the tropics and the associated systematic errors. [Maria Amparo Martinez Arroyo, Mexico]	Rejected. This sentence is a statement of AR5. More information on model biases are given in the following paragraphs.
104963	23	29	23	31	For the end of this para Flato et al. (2013) is the more appropriate IPCC reference [Peter Gleckler, United States of America]	Accepted. A reference to Flato et al. (2013) has been inserted.
104965	23	33	23	34	Since the AR5 there have been a number of global studies providing more in-depth evaluation of simulated precipitation , e.g., via comparison of observed and simulated distribtutions of intensity and amount (e.g., Pendergrass and Deser, 2017; DOI: 10.1175/JCLI-D-16-0684.1). While not directly connected to D&A research, they do advance our understanding of simulated precip and should therefore at least be identiied. [Peter Gleckler, United States of America]	Taken into account. Reference included in the context of model evaluation on daily time scales.
98021	23	33	23	39	Should clarify here that Li et al. looked at precipitation trends *for the period 1948-2005*. This is year range is needed, otherwise their results seem to conflict with those of Knutson and Zeng discussed on p. 3-25 (lines 24-29). The two studies are not in conflict on this, as Knutson and Zeng find that the large (significantly larger than CMIP5 simulated) increasing trends in extratropical precipitation are for trends 1901-2010, and not for 1951-2010. [Thomas Knutson, United States of America]	Taken into account. Included the time period.
67557	23	33	23	55	Based on NASA satellite humidity data from Atmospheric Infrared Sounder (AIRS) and NASA satellite rainfall data from GPCP available in Obs4MIPs and 44 GCM outputs from Coupled Model Intercomparison Project Phases 3/5 (CMIP3/5), Tian (2015) show that the double-ITCZ bias persist in CMIP3 and CMIP5 models and the double-ITCZ bias and ECS in climate models are negatively correlated. Based on the long-term annual mean tropical precipitation from two observations (GPCP and TRMM) and 75 CMIP3/5/6 models, Tian and Dong (2020) find that all three generations of CMIP models share similar systematic annual mean precipitation errors in the tropics. The double-ITCZ bias with a big inter-model spread evident in CMIP3 and CMIP5 models persists in CMIP6 models but it is slightly reduced in CMIP6 models in comparison to CMIP3 and CMIP5 models. Tian, B. (2015), Spread of model climate sensitivity linked to double-Intertropical Convergence Zone bias, Geophys. Res. Lett., 42(10), 4133-4141, https://doi.org/10.1002/2015gl064119 Tian, B., & Dong, X. (2020), The Double-ITCZ Bias in CMIP3, CMIP5, and CMIP6 Models Based on Annual Mean Precipitation, Geophys. Res. Lett., 47(8), e2020GL087232, https://doi.org/10.1029/2020gl087232 [Baijun Tian, United States of America]	Taken into account. References and a sentence are included.
71373	23	36	23	39	I find this sentence unsatisfactory. The first half sounds like you are talking of a model error, the second half says it could be internal variability (the typical yes and no or yes, but). Maybe it could be rephrased a bit. [Douglas Maraun, Austria]	Taken into account. Sentence modified.
7557	23	39	23	39	1/3: I strongly recommend to cite Marvel and Bonfils (2013) and Bonfils et al. (submitted) who both detected with high confidence a human fingerprint in the global change in precipitation: " [...] [Celine Bonfils, United States of America]	Taken into account. References included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7559	23	39	23	39	2/3: You could add: (Li et al. 2016b). An intensification of the wet-dry zonal patterns and to a redistribution of global precipitation are expected in response to greenhouse gas and ozone changes, but detecting these changes is complicated by the model errors in locating the main features of rainfall patterns. To deal with this issue, Marvel and Bonfils (2013) identified in each CMIP5 historical simulation the latitudinal peaks and throughs of the rainfall latitudinal patterns, measured the amplification and shift of these patterns in a pattern-based fingerprinting study, and found that the the simultaneous amplification and shift in zonal precipitation are detectable in Global Precipitation Climatology Project (GPCP) observations over the 1979-2012 period (extremely likely, with a signal-to-noise ratio>1.96.). Similarly, Bonfils et al. (submitted) found that the intensification of wet-dry zonal patterns identified in CMIP5 historical simulations is detectable In reanalyses over the 1950-2014 period (Figure 8.11, right column). The CMIP5 models have been shown [...]". [Celine Bonfils, United States of America]	Taken into account. Sentence included.
7561	23	39	23	39	3/3: Note for the authors: The intensification of the wet-dry zonal patterns results in the wet tropical and mid-latitude latitudes becoming wetter, and the dry subtopics becoming drier. In Marvel and Bonfils (2013), the combined changes are inconsistent with model estimates of internal variability, incompatible with simulations forced by natural forcings only, but fully compatible with climate predictions including both natural and human forcings. Bonfils et al. (submitted), also found that the changes cannot be explained by internally generated variability alone (at stipulated 5% significance threshold), and that GHG forcing drives the changes, partially masked by the response to aerosol forcing. Marvel, K., and C. Bonfils (2013), Identifying external influences on global precipitation, Proceedings of the National Academy of Sciences of the United States of America, 110(48), 19301-19306, doi:10.1073/pnas.1314382110; Bonfils, C. J., Santer, B. D., Fyfe, J. C., Marvel, K., Phillips, T. J., and Zimmerman, S. R. H. Human influence on joint changes in temperature, rainfall and continental aridity. (submitted). [Celine Bonfils, United States of America]	Taken into account. Marvel and Bonfils (2013) is now cited in 3.3.2.2 and its findings described.
127291	23	40	23	41	Please explain the meaning of "the double ITCZ bias in the equatorial Pacific" unless authors are confident that most people will be familiar with this. [Trigg Talley, United States of America]	Rejected. Did not clarify, as it is expected the readers are familiar with the concept.
7563	23	41	23	41	1/1: I suggest to add, if useful: A forced southward shift in ITCZ prior 1975, followed by a northward shift in ITCZ after 1975 has been detected in reanalyses, in response to the forced inter-hemispheric temperature contrast found by Friedman et al. (submitted) and Bonfils et al. (submitted) (section 3.3.1.1 and 8.3.2.1, Figure 8.11). Bonfils, C. J., Santer, B. D., Fyfe, J. C., Marvel, K., Phillips, T. J., and Zimmerman, S. R. H. Human influence on joint changes in temperature, rainfall and continental aridity. (submitted). [Celine Bonfils, United States of America]	Taken into account. Text and reference included.
4019	23	41	24	2	The subsequent paragraph (lines 47-2) will need to be modified to take into account the performance of the global monsoon in CMIP6, which is currently not mentioned, unlike for other phenomena such as the storm tracks. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence included and link to 3.3.3.2.
28781	23	41			The seasonailty of southern West Africa monsoon is not represented by CMIP5 models: Dunning et al. (2017) ERL http://iopscience.iop.org/article/10.1088/1748-9326/aa869e [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is on subcontinental scales and thus out of scope of the chapter.
26735	23	43	23	44	The expression "compensation between precipitation extremes and the rest …" is unclear. It should be made more explicit [Eric Brun, France]	Taken into account. Sentence has been modified.
2591	23	47	23	48	Fig. 3.10 says nothing about CMIP5 [Bryan Weare, United States of America]	Taken into account. The new Figure includes the biases of the CMIP5 multimodel mean.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26737	23	47	23	48	CMIP5 models are not shown in the figure [Eric Brun, France]	Taken into account. The new Figure includes the biases of the CMIP5 multimodel mean.
26739	23	48	23	48	"The persistent biases include the double ITCZ" is already noticed in previous paragraph. It would be better to gather the information in one place [Eric Brun, France]	Rejected. The previous paragraph focuses on CMIP5 models, the current paragraph on CMIP6 models.
52873	23	48			Same comment as for Fig. 3.2, replace panel c by two panels for DJF and JJA biases respectively? [Hervé Douville, France]	Rejected. We kept the RMSE and included a new panel with CMIP5 bias, as we believe it is more informative.
35621	23	55	23	55	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Only papers accepted by Jan 31st 2021 are cited in the final government draft.
597	23	55	24	2	It's hard to find the message from figure 3.10 [ZHIYAN ZUO, China]	Taken into account. Figure 3.10 (Figure 3.12 in the new version) has been thoroughly modified. It now includes CMIP5 biases to compare with CMIP6 biases. Also, it considers the range of internal climate variability in panels b) and d): In the figure regions with stippling are those where more than 90% of the models agree on the sign of the bias and the bias is greater than two standard deviations of the internal variability. This is an indication of the robustness of the biases. At the same time crosshatching in the panels that compare the biases in HighResMip simulations indicates where in at least 4 of the 5 HighResMIP models the bias in its high resolution version is smaller than in its low resolution version.
2593	24	1	24	2	The key feature is the improvement of the split ITCZ. [Bryan Weare, United States of America]	Taken into account. Sentence modified.
11971	24	1	24	2	In the text, Figure 3.10 (d) and (e) are not explicitly referred to. They should be referred to in this sentence. [Masaki Satoh, Japan]	Taken into account. These panels are now referred to in the text.
21481	24	4	24	6	This is purely observational so should be in chapter 2 and not here? [Peter Thorne, Ireland]	Talen into account. Paragraph has been removed.
37879	24	4	24	7	This paragraph, which is observation data set improvement, is unnecessary in the content. [Junhee Lee, Republic of Korea]	Taken into account. Paragraph has been removed.
28783	24	4			The satellite data record has not been mentioned yet has been used to attribute precipitation changes since the 1980s and a line conveying this would be useful [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Paragraph has been removed as suggested by other reviewers.
35623	24	18	24	19	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Noted. Bock et al is now accepted.
4021	24	24	24	41	You need to hammer home what the purpose of this paragraph is. Given that the subject of the chapter is the role of human influence on the climate system, then presumably the implication here is that the palaeo information tells us about the 20th century - in which case this should be stated (although bearing in mind the caveat posed in my comment above on the role of past climate analogues). Otherwise this is 18 lines spent on something that is not salient to the topic of the chapter. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph and the next one were moved to "paleoclimate context". The point here is that paleo information can also help validating the models used for detection and attribution.
21483	24	24	24	54	It feels really weird to have had a section entitled paleoclimate and then suddenly to get two paragraphs of paleo here that are arguably longer than was given to the topic in the segment entitled paleoclimate. Shouldn't these be combined for narrative continuity and also to avoid folks playing the proverbial games of spot the difference? [Peter Thorne, Ireland]	Taken into account. Moved the two paragraphs to "paleoclimate context".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52875	24	24	24	54	Shift this paragraph and the next one to former "paleoclimate context" subsection? [Hervé Douville, France]	Taken into account. Moved the two paragraphs to "paleoclimate context".
11969	24	31	24	31	"As result" should be "As a result". [Masaki Satoh, Japan]	Taken into account. "As result" was deleted.
102861	24	31	24	31	Delete "As result" [Philippe Tulkens, Belgium]	Accepted. Suggested change made.
68063	24	38	24	41	It seems like Fig 3.11 suggests that the reconstructed vegetation precipitation change is qualitatively in agreement with simulations for the Sahel, but there's very little simulated 6k-0k response anywhere else. This is not inconsistent with reconstructions, but perhaps only because of the large uncertainties in the latter. [MIchael Evans, United States of America]	Taken into account. Figure was redrawn to focus on changes in the latitudinal precipitation gradient in the European-African sector. This allows separating changes in Africa, where reconstructions show robust signals, from those in Europe where reconstructions show no consistent change.
13339	24	39	24	39	remove : of Figure 3.11: [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Removed.
26741	24	41	24	41	Brierley et al also discuss that there is an improvement in the representation of monsoon that is mainly due to improved representation of the modern monsoon and monsoon patterns [Eric Brun, France]	Taken into account. Modified the sentence "Results suggest that CMIP6 models show a more robust and stronger rainfall increase in Western Africa than CMIP5, which was ascribed to a better representation of the present-day monsoon (Brierley et al., 2020). Overall, however, large discrepancies remain between model simulations and reconstructions."
35625	25	8	25	9	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Noted. Paper already accepted.
28787	25	9			Liu & Allan (2013) ERL http://dx.doi.org/10.1088/1748-9326/8/3/034002 is a more appropriate reference [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This figure is based on the D&A analysis performed by Brierley et al 2020.
6603	25	12	25	12	Figure 3.12 needs replotting, as lines go off-scale in the bottom two panels. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure has been redrawn.
127293	25	14	25	14	Clarify that the "observed trend for precipitation" refers to mean annual mm/day. [Trigg Talley, United States of America]	Taken into account. Added "annual".
11973	25	14	25	15	This statement is something confusing. Please clarify the relation or the difference between the trend throught the whole 20C and that of the latter half of 20C. The latter trend is stated in "The AR5 concluded that there was medium confidence that human influence had contributed to large-scale precipitation changes over land since 1950, including an increase in the NH mid to high latitudes" (p.23 L26-27) and p.25 L24-26. [Masaki Satoh, Japan]	Taken into account. Modified sentence as "Different from the trend observed since 1950, the observed…"
28785	25	16			The lack of present day global precipitation trend is consistent with physical understanding of the energy budget (Section 8.2.1). In addition to suppression of warming, fast atmospheric adjustments to greenhouse gas and absorbing aerosol are currently offseting the increases relating to the warming. This is alluded to but could be made clear. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Modified sentence.
104967	25	19	25	21	Awkward - reword this [Peter Gleckler, United States of America]	Taken into account. Modified sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98023	25	24	25	29	Knutson and Zeng (2018) provide more regional detail of detectable (unusual compared to simulated natural variability) trends, likely attributable in part to anthropogenic forcing. For land-based precipitation 1901-2010 the following detectable decreases were identified: over the Mediterranean region (especially from Egypt though Syria) and northern tropical Africa. The detectable anthropogenic reduction in precipitation in the region surrounding the Mediterranean was also shown (for winter season) by Hoerling et al. 2012. Ref: Hoerling, M., J. Eischeid, J. Perlwitz, X. Quan, T. Zhang, and P. Pegion, 2012: On the Increased Frequency of Mediterranean Drought. J. Climate, 25, 2146–2161, https://doi.org/10.1175/JCLI-D-11-00296.1. Small, but important, regions of detectable anthropogenic decrease in precipitation include parts of southwest Africa, far southwest Australia, Tasmania, parts of the Caribbean and Maritime Continent, parts of Chile, Japan, and Sri Lanka, plus a few other smaller regions. There may be lower confidence in these regional results due to the small size of affected region, but they are still quite important for impacts (e.g., regional increases in drought risk). Regions with detectable anthropogenic increases (which are far more common than detectable decreases in the Knutson and Zeng analysis) include large regions of the extratropics in northern Eurasia (in regions with sufficient data for trend analysis), the north-central to northeastern United States, southern to southeastern Canada, southeast South America, and northern Australia. [Thomas Knutson, United States of America]	Rejected. This chapter addresses changes on continental and larger scales. Regional climate changes are addressed in other chapters.
13341	25	34	25	34	remove : [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Corrected.
11975	25	39	25	39	"day-1": "-1" should be superscript. [Masaki Satoh, Japan]	Taken into account. Corrected.
37889	25	39			I recommend to the wider spread of Y-axis in Figure 3.12. It is difficult to recognize the time series in Southern Hemisphere [Junhee Lee, Republic of Korea]	Taken into account. Figure 3.12 has been smoothed with a low-pass filter and now the y-axis is correct.
71375	25	50	25	50	Please check for consistency with showcases on South America and Australia in Chapter 10.4.1 [Douglas Maraun, Austria]	Taken into account. A reference to 10.4.2.2 on rainfall increases in southeastern South America has been added, and the assessments are consistent. There no longer appears to be a section on Australian rainfall changes in this part of Chapter 10.
11977	25	52	25	54	The text states that the trends in these regions are arrtibutable to ozone depletion and greanhouse gases. Are these trends clearly separted from the natural variabilities? [Masaki Satoh, Japan]	Taken into account. Yes, Knutson and Zeng (2018) show that the trends in south-eastern South America and northern Australia are attributable at least in part to anthropogenic forcing.
40623	25	52	25	54	perhaps to avoid having this paragraph being taken out of context specify, "stratospheric" ozone. [TSU WGI, France]	Taken into account. Included "stratospheric" in sentence.
50709	25	55	26	1	This is a slightly confusing sentence structure. Suggest rephrasing to "During austral winter, wetting and drying conditons at high and middle latitudes are not zonally homogenous, due to both changes in eddy activity and increased lower troposphere humidity." [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence modified as suggested.
127295	26	7	26	8	Confusing phrase "...the effect of external forcing on precipitation following the wet gets wetter, dry gets drier paradigm..." What is meant here? Could this be simplified to say "the effect of external forcing on the wet gets wetter, dry gets drier paradigm"? Also, further definition of the "wet gets wetter..." paradigm would be useful since this is referred to multiple times and some may not be familiar with it. [Trigg Talley, United States of America]	Taken into account. Sentence was modified as: In the tropics and subtropics, results from Polson and Hegerl (2017) give support to an intensification of the water cycle according to the wet- gets- wetter, dry- gets- drier paradigm if one takes into account the seasonal and interannual movement of the regions (Allan, 2014)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71377	26	7	26	20	There is a nice conceptual figure in the book “Climate System Dynamics and Modelling” from Hugues Goosse on the processes contributing to changes in precipitation patterns (increasing moisture, slowing circulation, spatial Hadley cell expansion), a bit similar to the FAQ14.1 figure in AR5 on the monsoon. Maybe this figure can help to frame this paragraph. Currently it looks a bit muddled. [Douglas Maraun, Austria]	Taken into account. We thank the reviewer for pointing us to the figure. We have rewritten the paragraph.
26743	26	8	26	12	It needs to make sure that the overall statement on "the wet gets wetter, dry gets drier" paradigm, is consistent with chapter 8 [Eric Brun, France]	Taken into account. Section 8.3.1.1 assesses that "In the tropics, there is evidence of increasing P-E in the wet regions and decreasing P-E in the dry regions over land (medium confidence)", which is consistent with the wet-gets-wetter paradigm.
88953	26	9	26	11	The analysis in this paper only looked at the combined wet/dry fingerprint - and not the wet and dry regions separately. But the analysis did attribute the observed change to natural and antropogenic factors so it would be good to mention this. E.g. "A follow-up study using CMIP6 models also found that the observed contrast over wet and dry regions was detectable, although was significantly larger than in the multi-model mean. The change was attributed to a combination of antropogenic and natural forcings, with anthropogenic forcings detectible in multi-signal analyses." [Schurer Andrew, United Arab Emirates]	Taken into account. Sentence modified as suggested.
111035	26	11			the discussion of the rainfall paper Schurer et al isnt 100% spot on; it misses attribution to anthropogenic forcing and also is done for dry and wet combined. I think andrew sent you a rephrase suggestion [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Modified sentence as Schurer suggested.
17103	26	13	26	14	The authors stated that they identified significant increases in precipitation in the tropics and decreases in the subtropics. This statement is contradictive to the previous statement in Chap.3, Page 23, Line 35 to 36, that states: "models capture the drying trends in the tropics and along 45°S and the wetting trend in the NH mid-to-high latitudes, but the amplitude of the changes are much smaller.". Please explain and syncronize these statements. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There is no contradiction as one statement is concerned with land regions, and the other with oceanic regions.
37881	26	20	26	20	It is need to describe to response of volcanic eruptions in the precipitation change. Paik and Min (2017) found the precipitation change and monsoon circulation weakends after volcanic eruptions. Add to the references. Paik, S., and Min, S.-K. (2017) Climate responses to volcanic eruptions assessed from observations and CMIP5 multi-models. Clim. Dyn. 48, 1017-1030. doi:10.1007/s00382-016-3125-4. [Junhee Lee, Republic of Korea]	Taken into account. We have included sentences and several references on the impact of volcanic forcing on precipitation, in particular over wet tropics including global monsoon regions.
71379	26	20	26	20	Check for consistency with Chapter 10.4.1 and 10.6.3 (Monsoons) [Douglas Maraun, Austria]	Taken into account. Sentences removed to focus on large scales and global monsoon.
35627	26	25	26	26	Bibliographic citations in chronological order [Carlos Antonio Poot Delgado, Mexico]	Taken into account. Order changed.
28789	26	25			Could also refer to 8.2.2.1 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Included link to section 8.2.2.1
127297	26	26	26	26	This should refer to Cheng et al 2020 submitted (see Chapter 2) which details salinity changes for the top 2000m. [Trigg Talley, United States of America]	Taken into account. Reference added.
52877	26	47	26	54	Link to or even merge with Box 8.2? [Hervé Douville, France]	Taken into account. Linked to Box 8.2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28791	26	53			There is some CMIP5 vs observational analysis showing later more intense wet seasons over northern Africa that can be physically related to a strengthening Sahara heat low e.g. Dunning et al. (2018) J. Clim https://journals.ametsoc.org/doi/10.1175/JCLI-D-18-0102.1; Dong & Sutton (2015) Nature Clim. http://www.nature.com/nclimate/journal/v5/n8/full/nclimate2664.html [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Chapter 3 focuses on continental and larger scales.
65665	26	56	27	10	Suggest listing some examples of the "several new studies" mentioned. This is a useful paragraph (partly used in the SPM) but has no references. [Kushla Munro, Australia]	Rejected. This last paragraph is a summary of the subsection. "New studies" refer to what was written above; it has been changed to "studies published since AR5".
81957	26	56	27	10	This useful paragraph (partly used in the SPM) has no references. It would be helpful to list some examples of the "several new studies" mentioned. [Dan Zwartz, New Zealand]	Rejected. This last paragraph is a summary of the subsection. "New studies" refer to what was written above; it has been changed to "studies published since AR5".
98025	26	56	27	10	This summary seems inadequate, and difficult to interpret. The most confident findings for human influence on precipitation would be the increasing precipitation in middle to high latitudes, noted in Zhang et al. (2007), Wan et al. (2015) for the period 1966-2005 and Knutson and Zeng (2018) for the period 1901-2010. The most confident large-scale regional detectable anthropogenic decreasing precipitation trend would be over the Mediterranean region (e.g., Hoerling et al. 2012 and Knutson and Zeng 2018). These studies are based on land-based records extending back to 1901 and comparison of observed trends with model historical runs and control runs. Ref: Hoerling, M., J. Eischeid, J. Perlwitz, X. Quan, T. Zhang, and P. Pegion, 2012: On the Increased Frequency of Mediterranean Drought. J. Climate, 25, 2146–2161, https://doi.org/10.1175/JCLI-D-11-00296.1 [Thomas Knutson, United States of America]	Taken into account. Paragraph has been rewritten to clarify and focus on main results of this section.
111037	27	1		2	this could be nicely brought together with the wet wetter work as its essentially the same finding with different datasources and they strengthen each other [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section text now includes a more integrated vision of the wet-gets-wetter paradigm and a related sentence has been included in this paragraph.
4023	27	2	27	6	See the comments I have made elsewhere pertaining to Chapter 3 ES statements containing the same wording. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence was deleted as Chapter 8 is dealing with regional monsoons. In the ES statement we included a sentence about the global monsoon.
127299	27	13	27	52	There are major outstanding issues about the homogeneity of water vapor records and the paper by Dai et al 2011, J Climate p 965-991 is an essential reference. [Trigg Talley, United States of America]	Taken into account. We have included a newer reference on inhomogeneity of water vapor records (Schroeder et al 2019).
52879	27	13			Shift this subsection before precipitation? [Hervé Douville, France]	Taken into account. Moved before precipitation to mirror structure of chapter 2.
11979	27	15	27	16	It is not clear what kind of contribution is concluded in AR5 in this sentence: "The AR5 concluded that an anthropogenic contribution to specific humidity is found with medium confidence at and near the surface." [Masaki Satoh, Japan]	Taken into account. Changed as suggested in comment #52881.
67559	27	15	27	55	In comparison to the AIRS specific humidity, CMIP5 models have the well-known double-ITCZ bias in the troposphere from 1000 hPa to 300 hPa, especially in the tropical Pacific (Tian et al., 2013) Tian, B., Fetzer, E. J., Kahn, B. H., Teixeira, J., Manning, E., & Hearty, T. (2013), Evaluating CMIP5 models using AIRS tropospheric air temperature and specific humidity climatology, J. Geophys. Res., 118(1), 114-134, https://doi.org/10.1029/2012jd018607 [Baijun Tian, United States of America]	Taken into account. The reference was already cited. A sentence was added to reflect also the double ITCZ bias.
52881	27	15			"increase in" rather than "contribution to"? [Hervé Douville, France]	Taken into account. Changed as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6605	27	21	27	21	Typo - "humidity" [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Spelling of 'humidity' has been corrected.
127301	27	21	27	21	"humidity" is misspelled (last word of paragraph). [Trigg Talley, United States of America]	Taken into account. Spelling of 'humidity' has been corrected.
11981	27	33	27	34	This sentence is a copy of Chung et al. (2014) and misleading: " the moistening observed in the upper troposphere over the period 1979–2005 cannot becannot be explained by natural causes and results principally from an anthropogenic warming of the climate." Better to add "trend" such that "mostening trend". Otherwise, it seems that only the period 1979-2005 is the moist period, and it may returned back to a normal value after that. [Masaki Satoh, Japan]	Taken into account. Sentence was modified as suggested.
7533	27	33	27	38	1/6: I suggest to add for the record the follwing (this could be simplified of course): In 2007, Santer et al. (2007) used estimates of the atmospheric water vapor from satellite-based Special Sensor Microwave Imager (SSM/I) and from CMIP3 historical climate simulations to provide evidences of the profound influence of human-induced warming on the atmosphere's total moisture content. The human-induced warming yields to a moistening of the atmosphere, which thus traps more heat, and increases further the ability of the atmosphere to hold more moisture. This simulated human fingerprint pattern is detectable at the 5% level by 2002 in water vapor satellite data (from 1988 to 2006). The observed changes match the historical simulations forced by GHG changes, and cannot be due to climate noise alone. By 2006, this detection is extremely likely, with a signal-to-noise ratio>1.96. [Celine Bonfils, United States of America]	Taken into account. Included a summary of proposed text.
7535	27	33	27	38	2/6: Then, Santer et al. (2009) repeated this study with CMIP5 models, and found that the D&A conclusions are not sensitive to model quality (defined by mean state, annual cycle and variability metrics). These results demonstrate that the human fingerprint is governed by robust and basic physical processes, such as the water vapor feedback. [Celine Bonfils, United States of America]	Taken into account. Included a summary of proposed text.
7537	27	33	27	38	3/6: Finally, Chung et al. (2014) extended this line of research by focusing on the global-mean water vapor content in the upper troposphere. Using satellite-based observations and sets of CMIP5 climate simulations run under various climate-forcing options (historical, GHG and NAT simulations), they show that the observed moistening of upper troposphere over the 1979-2005 period cannot be explained by climate noise alone, but is attributable to a combination of anthropogenic and natural forcings [Celine Bonfils, United States of America]	Taken into account. Included a summary of proposed text.
7539	27	33	27	38	4/6: Santer, B., et al. (2007), Identification of human-induced changes in atmospheric moisture content, PNAS, 104(39), 15248-15253, doi:10.1073/pnas.0702872104. [Celine Bonfils, United States of America]	Noted.
7541	27	33	27	38	5/6: Santer, B. D., et al. (2009), Incorporating model quality information in climate change detection and attribution studies, PNAS 106(35), 14778-14783, doi:10.1073/pnas.0901736106. [Celine Bonfils, United States of America]	Noted.
7543	27	33	27	38	6/6: Chung, E., B. Soden, B. Sohn, and L. Shi (2014), Upper-tropospheric moistening in response to anthropogenic warming, PNAS, 111(32), 11636-11641, doi:10.1073/pnas.1409659111 [Celine Bonfils, United States of America]	Noted.
2595	27	33	27	52	This discussion begs a figure analyzing upper tropospheric moisture in CMIP6. This is important. [Bryan Weare, United States of America]	Taken into account. A new figure comparing observed (RSS and ERA5) and simulated trends (CMIP5 and CMIP6) of column water vapor path has been now included in the subsection.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52883	27	47	27	48	Yet, the widespread drying trends found in the atmospheric reanalyses should be considered cautiously at the regional scale given the possible artifacts due to changes in the measurement method and the need to homogeneize the relative humidity observations which are assimilated in such reanalyses (Freychet et al., 2020). [Hervé Douville, France]	Rejected. Not included because it refers to observations and regional scales, and thus it is out of scope of the chapter.
127303	27	47	27	48	"This drying was underestimated ... with potential implications for the projected 21st century changes in these regions." Expand: What are the potential implicaitons? If drying was underestimated 1951-2005, does that imply drying would continue to be underestimated in 21st century? [Trigg Talley, United States of America]	Taken into account. The text no longer includes "with potential implications...". We removed it because the widespread drying trends found in the atmospheric reanalyses should be considered cautiously at the regional scale given the possible artefacts due to changes in the measurement method and the need to homogenise the relative humidity observations. We included a sentence cautioning on this.
68065	27	50	27	51	This evidence appears to be limited to 1979-2005, but is there additional evidence from the same approaches (e.g. Chung et al 2014) for 2006-present? The upper tropospheric moistening signature should be even more evident in such estimates, if RH is conserved, and considering the tropospheric warming since then? [MIchael Evans, United States of America]	Taken into account. To our knowledge there are no new studies on detection and attribution of global atmospheric moisture trends. However, we have included previous papers to provide more background on the topic, and a paper analysing a regional trend in precipitable water.
71381	27	50	27	52	This confidence statement builds upon the literature cited above. But the way it is written ("Based on new evidence") this does not become clear. Please link explicitly to the literature above. [Douglas Maraun, Austria]	Taken into account. Modified to "In summary…"
45243	27	52	27	52	"Owing to the limited number of studies and model biases we conclude that there is low confidence in the attribution of changes in the surface humidity". On the other hand, one of the ES statements in Chapter 8 (page 5, lines 25-26) says that "There is high confidence that mechanisms driving declining continental near-surface relative humidity suppress precipitation response to warming over land relative to the ocean". The two assessments need to be harmonized. [Krishnan Raghavan, India]	Taken into account. New summary seems to be in line with chapter 8.
111039	27	52			what about vertically integrated water vapour doesn't this follow the warming very well? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Yes, integrated water vapor content follows tropospheric warming, increasing at a rate of about 7%, and this is described in chapter 2. We have not found global D&A studies on that to include here. We include a reference on D&A for regional precipitable water.
127305	28	1	28	39	Isn't the homogeneity of streamflow records a major issue? Many rivers have dams. See also Dai, A., T. Qian, K. E. Trenberth and J. D. Milliman, 2009: Changes in continental freshwater discharge from 1949-2004. J. Climate, 22, 2773-2791. [Trigg Talley, United States of America]	Noted. Yes, engineering and water management may affect river flow records and effects of human water withdrawals on large-scale trends were already assessed. Further notes include: (1) Effects of water management are typically heterogeneous in space. (2) Water management contributes to the "net human impact" on water resources, and can hence be interpreted as part of the signal (and not an artefact). (3) Water management is typically heterogeneous in space and time, contrasting climate change signals. (4) Day et al (2009) focus on continental discharge to the oceans and don't account for variability within the continents.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127307	28	1	28	39	The following reference may be relevant: Giuntoli, I., G. Villarini, C. Prudhomme, and D.M. Hannah, Uncertainties in projected runoff over the contiguous United States, Climatic Change, 150(3), 149-162, 2018. [Trigg Talley, United States of America]	Rejected. This is about future projections.
52885	28	1			Merge with Section 8.3.1.5 to avoid overlaps? [Hervé Douville, France]	Rejected. Ch3 focuses on large-scale change attribution.
6607	28	3	28	3	"Stream flow" should be "Streamflow" to be consistent with the title of the section and later usage. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected.
52887	28	4			river basin to global scales? [Hervé Douville, France]	Rejected. We have now studies covering global-scale changes.
11983	28	6	28	6	It is not clear what is the kind of the anthropogenic influence stated in AR5: "AR5 concluded that there is medium confidence that anthropogenic influence on climate has affected streamflow in some middle and high latitude regions." [Masaki Satoh, Japan]	Rejected. This is following AR5 statement. Also specifying that anthropogenic climate change is the focus of this chapter (in contrast to water/land management).
33273	28	30			Add comma between words: “Recently Gudmundsson....” [Guiomar Rotllant, Spain]	Accepted. Corrected.
33203	28	33	28	34	Zuo et al. (2019) also revealed reduced streamflow following tropical volcanic eruptions. I suggest adding this reference here. References:Zuo, M., T. Zhou, and W. Man, 2019: Hydroclimate Responses over Global Monsoon Regions Following Volcanic Eruptions at Different Latitudes.	Accepted. We now cite it.
111041	28	33			thank you for citing this paper; it found that … driving reduced streamflow in rivers in the wet tropics' [and increased streamflow in some dry regions] [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We revised that part as suggested.
17105	28	36	28	37	It is a great to know that there are several studies that support the claim on climate change impacts to stream flow (medium confidence). It would be better explained if authors present some stream flows graphs from those studies. It must be strongly stated in the text that it is imposible if some places are getting drier or wetter but there is no change in stream flow. There must be a change in stream flow correspondingly, although the studies for the evidences are still limited. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This regional assessment is relevant for Ch. 8.
100511	28	36	28	39	Note an apparent inconsistency with Chapter 11 (page 70, lines 50-52), where it is mentioned that 'As there is only one study and multiple caveats, including relatively poor observational data coverage, there is low confidence about human influence on the global scale.'. While both statements are referring to different spatial scales (regional/local vs. global), it would be useful to cross-check and harmonise the two paragraphs. [Wim Thiery, Belgium]	Taken into account. Ch11 part focuses on 'extreme' river flow, different from our point (mean changes in river flow).
11985	28	37	28	38	No reference is given in the text on "that the associated global-scale trend pattern is inconsistent with pre-industrial control simulations". [Masaki Satoh, Japan]	Taken into account. We have revised it by replacing "pre-industrial control simulations" by "internal variability". This is a summary sentence so we do not cite references. References were cited in the text above.
111043	28	38			this is a nice strong statement, it would be nice to know if the discrepancy with human intervention is discernible based on spatial or temporal inhomogeneity? Or some idea how this conclusion comes about:? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have improved that part in the text. (1) The study investigates the spatial pattern of regional mean trends (SREX region-scale). (2) This study relies on global impact models that are driven by GCM output and can therefore account for effects of water/land management.

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21487	28	42			I found this section overall a tough read. There are several sentences that could be much better constructed and several words out of place. A more careful proofing should pick most of these so I do not pick them out individually here. [Peter Thorne, Ireland]	Taken into account. We have carefully proofread this section.
130487	28	44	28	44	sub-section title "tropical overturn circulation" is new too most of readers. Why not using "Walker Circulation and Hadley Circulation？ [Panmao Zhai, China]	Accepted. We changed the title as suggested.
21485	28	46	28	51	This is all text book material and there is no assessment. Is it essential to retain? It feels out of place. [Peter Thorne, Ireland]	Noted. We retained this material for context and to help the reader understand the rest of the section.
71383	28	49	28	49	If I am not mistaken it is Walker ciculation, not cell (in case of the Hadley cell both work I think). [Douglas Maraun, Austria]	Accepted. Replaced with 'Walker circulation'.
3353	28		3	30	It is very valuable in the contribution that researchers make to the global perspective and context, please, it is important to expand in conceptualization, specific details and if possible in expanding ideas in order to outline with more argument in the ideas expressed here that are very important [Eduardo Erazo Acosta, Colombia]	Noted. The literature suggests that impacts of anthropogenic climate change on streamflow are regionally diverse. Nonetheless, the combined evidence suggests that the emerging large-scale patterns of change can only be explained by anthropogenic climate change. Locally, effects of e.g. land-use-change/water-management can also play a large role.
116207	28		28		What about the possible effects of land and water management on streamflow trends? This may need to be coordinated with WGII water chapter. [Valerie Masson-Delmotte, France]	Noted. (1) General note: Effects of water and land management are extensively treated in WGII. Assessing these effects typically requires impact models, since GCMs typically do not account for water/land management. (2) Effects of land and water management can be locally significant. However, these effects are often not uniform in space and may therefore not be visible in large-scale assessments (i.e. averaged out). In some cases, there may be instances with feedbacks between climate change and water management (e.g. more water withdrawal for irrigation in response to drying conditions). This impact-relevant issues at local scales are dealt in Ch8.
107019	29	3	29	30	New detection/attribution study on the Hadley cell in the Southern Hemisphere to be added: Jebri et al. (2020, https://doi.org/10.1175/JCLI-D-19-0304.1). [Christophe CASSOU, France]	Taken into account. This study is now cited.
37335	29	4	29	30	Please report if the models used in papers cited by this paragraph have been validated. [John McLean, Australia]	Taken into account. A comparison of the simulated and observed Hadley Cell is included in this section.
52889	29	4			First assess the model ability to capture the present-day mean state, then assess recent trends? [Hervé Douville, France]	Taken into account. In the revised section the simulated mean state is first briefly discussed before assessing changes.
127309	29	21	29	21	Not all reanalyses are created equal. An assessment of reanalysis products should be included. In fact, ERA-interim is much better understood in this regard. [Trigg Talley, United States of America]	Rejected. Reanalyses are assessed in Chapter 2.
599	29	27	29	27	delete the second "in" [ZHIYAN ZUO, China]	Accepted. This text has now been revised.
104979	29	27	29	27	change to "is not detected in other established metrics (e.g., Grise et al., 2019)." [Peter Gleckler, United States of America]	Not applicable. This text has been revised and no longer refers to particular metrics.
2597	29	28			define and explain "PDV" [Bryan Weare, United States of America]	Accepted. Now defined on first use with a reference to Annex IV.2.6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37337	29	33	30	16	Please report if the models used in papers cited by these paragraphs have been validated. [John McLean, Australia]	Taken into account. A comparison of the simulated and observed Hadley Cell is included in this section.
35629	29	44	29	45	Bibliographic citations in chronological order [Carlos Antonio Poot Delgado, Mexico]	No longer applicable. This text was removed in revision.
132627	29	46	30	5	We should make sure that this text and that in Chapter 7 (Section 7.4.4.2.1 "Critical processes determining changes in tropical sea-surface temperature gradients") and Chapter 9 (page 16) is consistent. It appears to be qualitatively consistent, but there are some differences in mechanisms of Walker Circulation changes discussed, with Chapters 7 and 9 discussing the potential role of Atlantic SST trends, for instance. [Kyle Armour, United States of America]	Taken into account. We worked to coordinate this with Chapter 7 when revising.
13343	29	50	29	50	AGCM must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. This acronym is no longer used.
79269	29	50	29	51	Please provide more details: Why does that suggest dominance of internal variability? If the Pacific trends are partly forced by tropical Atlantic SST trends, also the origin of the recent Atlantic SST trends matter for the interpretation as argued by McGregor et al. (2018), and I'm not sure it has already been established that the warming there is from internal variability during the 'hiatus' [Martin Stolpe, Switzerland]	Taken into account. The possible remote influence of Atlantic SSTs is now discussed in the revised version.
79255	30	5	30	5	Also cite Kuntz & Schrag (2016), https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016JD025430 [Martin Stolpe, Switzerland]	Accepted. The reference has been added.
11987	30	8	30	9	It is not useful to mention the two periods since 1900 and 1950s. It can be combined. If not, please distinguish the two periods: "the equatorial zonal SST gradient from the eastern Indian Ocean through the Pacific has strengthened since 1900 (Coats and Karnauskas, 2017) and since the 1950s (Seager et al., 2019)". [Masaki Satoh, Japan]	Taken into account. While some papers examine trends since the 1900s, many other studies focus on trends since the 1950s for which observations are more reliable. The sentence has been split into two as suggested.
35631	30	13	30	13	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Noted. The cited paper has been published.
21489	30	17	30	36	I may be misremembering but I thought the earlier paleoclimate section covered hydrological cycle and circulation aspects? If so then similar to an earlier comment having paleo in twice on the same thing is a little inviting the reader playing spot the difference and it would be worthwhile co-locating and integrating these segments to avoid that. [Peter Thorne, Ireland]	Accepted. The earlier text on the paleoclimate hydrological cycle has been merged to Sections 3.3.3.1 and 3.3.3.2.
37339	30	18	30	20	Your conclusion is laughable. The Great Pacific Climate Shift occurred at the end of the first half of 1976. This is obvious from the ENSO shift that occurred at that time. Previous IPCC reports often referred to changes, especially in the Pacific, that can be dated to about that time. [John McLean, Australia]	Noted. The late-1970s climate shift is associated with the phase transition of PDV, which is also manifested as the frequency modulation of ENSO. The PDV influence has been reflected in our assessment.
52891	30	18	30	29	This summary is about both Hadley and Walker circulations. Add a "Summary" paragraph title in italics? [Hervé Douville, France]	Accepted. We have added an italic subheading for this paragraph.
13345	30	19	30	20	either use since or around not both [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. We have rephrased it to "since the 1980s".
104981	30	25	30	29	Break this into two sentences to make more clear. [Peter Gleckler, United States of America]	Not applicable. This part has been rewritten.
6609	30	34	30	34	It is difficult to distinguish the line styles used for the reanalyses shown in Figure 3.14, especially in the legend. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72069	30	46	32	19	Based on recent study there is also clear understanding that Indian summer monsoon are declining during La Nina years (which is historically the wetter years) after 1980 relative to pre-1980 due to weaker La Nina events and warming of tropical Indian ocean. The relevant reference is also should be mentioned. --- Samanta, D., Rajagopalan, B., Karnauskas, K. B., Zhang, L., & Goodkin, N. F. (2020). La Niña's Diminishing Fingerprint on the Central Indian Summer Monsoon. Geophysical Research Letters, 47(2), e2019GL086237. [Samanta Dhrubajyoti, Singapore]	Rejected. Regional monsoon variability and changes are out of scope of this section.
4057	30	46	32	34	The subsection is a good attempt to sumarise relevant controls on the global monsoon with respect to human influence, although I would expect that a revised FGD version of this section will be able to make much better use of literature examining DAMIP etc. in CMIP6. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A study based on CMIP6 DAMIP (Zhou et al. 2020) has been cited.
11989	30	48	30	49	Redistribution of moist static energy is just one aspect of the monsoons, and should not be specifically referred to at the beginng of this section. [Masaki Satoh, Japan]	Taken into account. We have revised the paragraph to describe the global monsoon in terms of the annual cycle of solar insolation, and the idea of global monsoon.
40915	30	48	30	49	The current glossary definition is "A monsoon is a tropical and subtropical seasonal reversal in both the surface winds and associated precipitation, caused by differential heating between a continental-scale land mass and the adjacent ocean. Monsoon rains occur mainly over land in summer." Would you like to update it? [TSU WGI, France]	Noted. This part has been revised in response to other comments.
4025	30	49	30	50	The role of this sentence ("The global monsoon encompasses all the monsoon systems with specific metrics" is unclear. Firstly, what are the specific metrics and why are they (if at all) of interest to the reader? Secondly, while the global monsoon does encompass all the regional monsoons, it is also something that undergoes coherent variations in its own right in response to forcing. (See e.g. Wang et al., 2013, PNAS.) [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised to note the precipitation-based index, and the connections of the regional monsoons through mass, momentum and energy budgets.
4027	30	50	30	51	It is good to see that links are being made to regional monsoon activities in other chapters. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We have updated the section numbers.
52893	30	51			Section 8.3.2.4 [Hervé Douville, France]	Editorial. Revised as suggested.
17927	30	53	30	56	While AR5 may not have concluded about the attribution of decreasing 20th century trends in monsoons, research since then, still largely exploiting CMIP5 simulations, has conclusively shown that, in a remarkable improvement over CMIP3, the multi-model ensemble can reproduce the post-World War II decrease in Sahel rainfall, and its recovery since the driest mid-1980s [Giannini and Kaplan, 2019, in Climatic Change <https://link.springer.com/article/10.1007/s10584-018-2341-9>]. Supporting the argument for a role of aerosols in the SST changes that drive Sahel rainfall, the CMIP5 MMM also reproduces drought in the Sahel in 1982 in response to the El Chichon volcanic eruption. [Alessandra Giannini, France]	Noted. Regional monsoon changes are out of scope of this section. However, changes from the mid-20th century to the 1980s is reflected in global monsoon as assessed later in this section.
116209	30		30		There is a need for x chapter coordination on monsoons. Several statements are not very clear (for instance, what is the reason for a link between change in monsoon precipitation and temperature during the last millennium, is it linked to volcanic forcing?). Insights from paleoclimate information are not reflected in summary statements. [Valerie Masson-Delmotte, France]	Taken into account. The dominance of volcanic forcing in the last millennium has been added. Findings based on paleoclimate modelling studies has been reflected in the summary statements.
71385	31	3	31	15	I am wondering why the reference to Sperber et al., 2013, DOI 10.1007/s00382-012-1607-6) is missing here. Or has this been cited in AR5? [Douglas Maraun, Austria]	Noted. It has been cited in AR5 (Flato et al).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37341	31	3	32	6	Please report if the models used in papers cited by these paragraphs have been validated. [John McLean, Australia]	Noted. As described in Section 3.1, whether models capture observed or proxy-based variations and trends is one of model validation processes. Therefore the model performance is assessed in this and other sections in this paper based on literature.
29217	31	4	31	36	Fig. 3.15: Why is the spread in models reducing with time? Please provide an explanation. [Fred Kucharski, Italy]	Not applicable. The time series have been changed to 5-year running means where the spread is stationary.
35633	31	7	31	7	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Noted. The cited paper has been published.
26745	31	10	31	10	Please mention what are the common biases: lack of precipitation:? [Eric Brun, France]	Accepted. The biases have been clarified.
4029	31	10	31	11	The sentence here ("Common biases are identified across CMIP5 models in the Northern Hemisphere monsoon") does not convey any information to the reader. What are the common biases? Otherwise remove the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The biases have been clarified.
2601	31	10			define MME [Bryan Weare, United States of America]	Editorial. Revised as suggested.
104983	31	13	31	13	"of up to ~ ~20 km" -> "approaching 20 km" [Peter Gleckler, United States of America]	Editorial. Revised as suggested.
105105	31	17	31	17	the word "proxy" should not be used without specifying what the proxy is for [Masa KAGEYAMA, France]	Editorial. Revised to "paleoclimate proxy".
4037	31	17	31	36	It needs to be made much more obvious to the reader what the purpose of this paragraph is. It is very interesting to read about the palaeomonsoon, but why should a policymaker interested in the human influence on the climate system be concerned with this? The authors need to make clear why this paragraph is relevant to the study of human influence on the monsoons - it is not self-evident. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The purpose of assessing paleoclimate simulations is already described in Section 3.1, and we do not repeat it here.
105107	31	17	31	36	The evidence of monsoon changes related to abrupt AMOC changes should be added here as the time scales are relevant for current climate change [Masa KAGEYAMA, France]	Accepted. We have added the relationship with the AMOC changes.
127311	31	17			Other paragraphs have topic sentences that include "paleoclimate". Paleoclimate proxies instead of just proxy? [Trigg Talley, United States of America]	Editorial. Revised to "paleoclimate proxy".
4031	31	20			"monsoon" change to "monsoons" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Revised as suggested.
4033	31	20			"under the pre-industrial conditions" change to "under pre-industrial conditions" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Revised as suggested.
50711	31	21			There are some more recent mid-Pliocene modelling monsoon papers that could be referenced here. They go into more detail about effect of different boundary conditions compared with proxy data. Notably the larger influence orbital forcing has on monsoon systems rather than higher CO2. Zhang et al. 2019. "Modeling the late Pliocene global monsoon response to individual boundary conditions". Prescott et al. 2019. "Indian monsoon variability in response to orbital forcing during the Late Pliocene. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been revised following the comment, with citation to the suggested papers.
21091	31	25	31	31	A recent paper by D'Agostino et al. has also reported the simulated strengthening and wetting of Northern Hemispheric monsoon during the mid-Holocene, and revealed the dominant contribution of dynamics which is further constrained by the integrated energy balance. It would be good to cite this paper here to provide a better understanding. [D'Agostino R, Bader J, Bordoni S, et al. Northern Hemisphere Monsoon Response to Mid-Holocene Orbital Forcing and Greenhouse Gas-Induced Global Warming[J]. Geophysical Research Letters, 2019, 46(3): 1591-1601.] [Wenxia Zhang, China]	Accepted. The text has been revised following the comment with citation to the suggested paper.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26747	31	28	31	28	May be it could be mentioned here that improvent in the simulation of mid Holocene monsoon in some of the models has been related to improved representation of the modern monsoon pattern (Brierley et al. CP, Submitted) [Eric Brun, France]	Taken into account. Brierley et al. (2020) find that the bias in monsoon expansion remains in CMIP6, and relate it to mean monsoon domain bias. We have pointed it out by citing Brierley et al.
105109	31	28	31	28	another reference here would be Perez-Sanz, A., Li, G., González-Sampériz, P., and Harrison, S. P.: Evaluation of modern and mid-Holocene seasonal precipitation of the Mediterranean and northern Africa in the CMIP5 simulations, Clim. Past, 10, 551–568, https://doi.org/10.5194/cp-10-551-2014, 2014 [Masa KAGEYAMA, France]	Accepted. The suggested paper has been added.
4035	31	30			insert comma after "northern Australia" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed.
4039	31	38	31	40	We are already three paragraphs (and more than halfway) into section 3.3.3.2 on human influence on the global monsoon and this is the first time it has been mentioned what the observed 20th century trend in the global monsoon has been. This should be mentioned in the first paragraph (page 30, lines 48-51) along with the definition of global monsoon and maintaining the cross-reference to Chapter 2 in which the observed trend has been introduced. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The section is organized for consistency with other subsections.
45245	31	38	31	40	"In the instrumental records, global summer monsoon precipitation intensity decreased from the 1950s to 1980s, followed by an increase mainly due to Northern Hemispheric land contributions". This statement is inconsistent with the subsequent summary statement (Chapter 3, page 32, lines 8-9) "In summary there is medium confidence that anthropogenic aerosols contributed to weakening of global land summer monsoon precipitation intensity from the mid-to-late 20th century". [Krishnan Raghavan, India]	Accepted. The summary sentence has been revised to avoid unnecessary confusion.
17925	31	38	32	7	The first sentence in this section attributes the decrease in "global summer monsoon intensity" between 1950 and 1980s to Northern hemisphere land contributions. Or maybe it is the increase that is attributed to land. In any case, I beg to differ! Sahel drying is an important component of the discussed decrease [and increase], and has been attributed to oceanic change, in part driven by emissions of aerosols and greenhouse gases [most recently by Giannini and Kaplan, 2019, in Climatic Change <https://link.springer.com/article/10.1007/s10584-018-2341-9>] [Alessandra Giannini, France]	Taken into account. We intended that the global monsoon variations arose mainly from Northern Hemisphere monsoon variations. We have revised the text to clarify this point.
21093	31	43	31	44	The mechanisms of aerosol-induced drying trend of Northern hemispheric monsoon during the latter half of the 20th century are revealed in this study, which demonstrates both the thermodynamic and dynamic contributions. The cooling effects of aerosol forcing are two-fold: a thermodynamic effect due to the reduction in atmospheric humidity and a dynamic effect due to weakening of the land-sea thermal contrast and thus monsoon circulation. It might be good to provide some physical understanding here. [Zhou T, Zhang W, Zhang L, et al. The dynamic and thermodynamic processes dominating the reduction of global land monsoon precipitation driven by anthropogenic aerosols emission[J]. Science China Earth Sciences, 2020, 63.] [Wenxia Zhang, China]	Accepted. This point has been added together with the suggested reference.
21095	31	43	31	44	Would it be good to briefly note the detection and attribution results on the drying effect of aerosols on regional monsoons, e.g., for South Asia (Undorf et al. 2018) and East Asia (Zhang et al. 2020), particularly considering that these are major aerosol source regions currently. [Undorf S, Polson D, Bollasina M A, et al. Detectable impact of local and remote anthropogenic aerosols on the 20th century changes of West African and South Asian monsoon precipitation[J]. Journal of Geophysical Research: Atmospheres, 2018, 123(10): 4871-4889. /// Zhang W, Li W, Zhu L, et al. Anthropogenic Influence on 2018 Summer Persistent Heavy Rainfall in Central Western China[J]. Bulletin of the American Meteorological Society, 2020, 101(1): S65-S70.] [Wenxia Zhang, China]	Rejected. The suggested papers focus on regional monsoon changes, which are out of scope of Chapter 3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33205	31	44	31	48	Zuo et al. (2019) also revealed significant global monsoon response (including precipitation and circulation) to volcanic eruptions on interannual time scales. I suggest adding this reference here. References:Zuo, M., T. Zhou, and W. Man, 2019: Hydroclimate Responses over Global Monsoon Regions Following Volcanic Eruptions at Different Latitudes. Journal of	Accepted. The suggested paper has been added.
4041	31	50			"are well captured" --> "is well captured" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. "are" is correct here.
40913	31	51	31	53	The current glossary definition for the NAM is "A winter fluctuation in the amplitude of a pattern characterized by low surface pressure in the Arctic and strong mid-latitude westerlies. The NAM has links with the northern polar vortex into the stratosphere. Its pattern has a bias to the North Atlantic and its index has a large correlation with the North Atlantic Oscillation index." Please check if it needs updating. [TSU WGI, France]	Noted. The comment is misplaced. The NAM is assessed in Section 3.7.1 with citation to the Technical Annex on modes of variability, where the details of NAM is provided.
2603	31	51			nothing can be said from Fig. 3.15c [Bryan Weare, United States of America]	Noted. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
4043	31	54	31	55	The purpose of this sentence needs to be stated so that the reader does not have to infer your meaning. I suggest appending: "..., suggesting that the role of internal variability cannot properly be assessed." [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised as suggested.
4045	32	1	32	3	I don't understand this sentence. How can an intensification of circulation found in CMIP6 ("…captured by the CMIP6 models…") lie outside the range of increase ("simulated by the CMIP6 ensemble")? Unless you are meaning an observational intensification, in which case the sentence should be improved to something like: "This tendency is accompanied by observed intensification of the Northern Hemisphere summer monsoon circulation, which appears to be outside the range of increase simulated by the CMIP6 ensemble" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence has been revised to distinguish the observed trends and model results.
45247	32	8	32	9	The weakening of global land summer monsoon precipitation intensity from the mid-to-late 20th century is inconsistent with the increase of global monsoon precipitation since 1980s mentioned in page 31, lines 38-40. This needs to be addressed. [Krishnan Raghavan, India]	Accepted. The summary sentence has been revised to avoid unnecessary confusion.
2753	32	9	32	19	Although it states that evidence is limited, there should be an idea of what eviddence exists. If there is limited evidence and low confidence in this section there should still have citations on the existing evidence [Carianne Johnson, Belize]	Not applicable. Considering new evidence and the updated figure, this assessment has been revised.
4047	32	9			The "late 20th century" is rather ambiguous. Do you mean the 1980s as mentioned on page 30 line 55? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The summary sentence has been revised to avoid unnecessary confusion.
4049	32	10			Is the word "rainfall" missing after "global land summer monsoon" or some other word? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. "precipitation" is added.
4051	32	18	32	19	It is not clear where this summary statement on CMIP6 model performance comes from, especially the confidence aspect. Limited evidence of what? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. With new literature and more models that are included in the figure, we have revised the assessment.
102863	32	19	32	19	"due to limited evidence": evidence for what? And is this half-sentence important? [Philippe Tulkens, Belgium]	Accepted. With new literature and more models that are included in the figure, we have revised the assessment.
4055	32	22	32	34	In Figure 3.15, I cannot at all distinguish the MME line from the individual members. The quality of the bold and thin lines needs to be improved for the FGD version. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6611	32	24	32	24	The period used to construct the climatogical fields shown in Figure 3.15 should be specified. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Gray shading added to (c, d) to indicate the period for climatology.
37891	32	24			In Figure 3.15 (c) and (d), it is difficult to recognize the "MME mean" and "individual members". Change to color or line style [Junhee Lee, Republic of Korea]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
4053	32	28	32	30	Clarify here or somewhere else within the section whether this is the monsoon circulation index introduced in Chapter 2. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The citation for this index is identical between Chapters 2 and 3, so it is clear that the index is also the same. Chapter 2 doesn't give details of the index, but providing this information in Ch 3 is strange and would disrupt the flow of argument.
11991	32	28	32	30	The definition of the NH summer monsoon circulation index for the longitude: "120W-120E" is confused as either "120W-180-120E" or "120W-0-120E". This is on the Greenwitz side. Please clarify. [Masaki Satoh, Japan]	Editorial. Revised to "from 120ºW eastward to 120ºE".
111045	32	37	33	10	Very interesting section. It would be useful to also hear how well the models do with summer blocking (although I guess that depends on how you define it) - as this is so important for extremes? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We thank the reviewer for this positive comment. The general improvement in CMIP6 versus CMIP5 is seen both in summer and winter (Schiemann et al., 2020). We explicitly now state "all seasons" to highlight this fact.
7999	32	39	32	44	In line 39 the jets denote "baroclinic zones" or 'zones of baroclinicity' instability should be dropped. Baroclinicity and baroclinic instability are not synonymous. The in line 41 Extratropical storms result from baroclinic instability ("such") should be dropped. [Anthony Lupo, United States of America]	Accepted. In shortening this section we follow the reviewer's recommendation.
21491	32	39	32	44	I'm not sure that this text book like material is necessary here as per earlier similar comments on similar segments within this chapter. [Peter Thorne, Ireland]	Accepted. In response to this comment, we have shortened this introductory paragraph and have more links to other parts of the report.
127313	32	39	32	44	In line 39, the jets denote "baroclinic zones" or 'zones of baroclinicity' instability should be dropped. Baroclinicity and baroclinic instability are not synonymous. The in line 41 extratropical storms result from baroclinic instability ("such") should be dropped. [Trigg Talley, United States of America]	Accepted. Ditto comment 1027.
52895	32	42			heat and moisture [Hervé Douville, France]	Noted. The reviewer is correct but in response to other comments we have shortened this paragraph and removed this sentence altogether.
52897	32	43	32	44	Refer to Sections 8.3.2, 10.3.3 and 11.7.2 [Hervé Douville, France]	Accepted. We add these links.
37343	32	53	34	17	Please report if the models used in papers cited by these paragraphs have been validated. [John McLean, Australia]	Noted. Indeed the papers cited here compare model simulations to observations and discuss model shortcomings. So yes, the models have been validated. The validations show that the models are not perfect, as is discussed in the text. No change to the text is required.
71387	32	54	32	54	What does “cyclones and blocking and seasons” mean? In particular “seasons”? Seasonality? Of what? Do you mean “cyclones and blocking and their seasonality”? [Douglas Maraun, Austria]	Accepted. "Seasons" must have been a typing error that slipped through. Now corrected.
35635	32	54	32	55	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Both "submitted" papers (Davini & D'Andrea, Priestley et al.) are now accepted and published.
80597	32	55	32	55	An additonal blocking reference: Schiemann (2020), accepted. https://www.weather-clim-dynam-discuss.net/wcd-2019-19/, [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now discuss Schiemann et al. (2020).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100795	33	7	33	7	The reference (Schiemann et al 2017) should be moved to line 6 (after “underestimated”), while Davini et al. 2017 should replace Schiemann et al 2017 at line 7 (Davini et al. 2017 pointed to a possible compensation of errors as resolution increases). [Corti Susanna, Italy]	Accepted. We follow the reviewer's recommendation.
71389	33	10			Here I would state that Chapter 10.3.3.4 discusses the relevance of misrepresentations in blocking for regional aspects of climate. [Douglas Maraun, Austria]	Accepted. Reference to 10.3.3.4 (now renumbered to 10.3.3.3) added.
19773	33	12	33	12	Too many "simulations" [philippe waldteufel, France]	Accepted. We have rephrased the sentence.
80599	33	12	33	12	Repeat of simulations [Malcolm J. Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased the sentence.
52899	33	15	33	16	also quote Oudar et al. (Clim. Dyn., 2020, https://doi.org/10.1029/2019GL086695) based on both CMIP5 and CMIP6 models [Hervé Douville, France]	Noted. Oudar et al. discuss zonal wind not cyclones or blocking (the themes of this subsection). Discussing shifts in zonal winds would constitute a digression from the main rationale of this section.
235	33	33	12	26	The entire two parapgrahs rely on a single submitted paper of Priestly et al. This is not good practice. The fact that storm tracks are placed to far equatorward in CMIP5 should be polstered by more literature. For example Chang et al. 2012 (doi: 10.1029/2012JD018578) and Zappa et al. (2013; doi: 10.1175/JCLI-D-12-00501.1) [Sebastian Schemm, Switzerland]	Noted. We agree that relying on only one reference is not ideal, but this one reference quite comprehensively investigates storm tracks in CMIP6 and CMIP5 models. Chang et al. (2012) appeared before the cut-off date for literature applied to AR5 and thus should be treated with lower priority here (as mainly updates to AR5 understanding need to be considered). Zappa et al. 2013 is referred to earlier in the paragraph. Essentially, their results are superseded by Priestley et al. 2020 who confirm and update their CMIP5 findings with CMIP6 results.
10965	33	41	33	46	This is also supported by Coumou et al (2015; DOI: 10.1126/science.1261768), who focused on eddy kinetic energy rather than cyclones per se. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Coumou et al. (2015) take an "Eulerian" point of view whereas here the emphasis is on "Lagrangian" features (i.e. cyclone tracking and track densities). The two views are complementary, but considering the late stage of the review process we do not wish to considerably expand this section by elaborating on this alternative perspective. Note also the response to comment 1040.
104989	33	49	33	49	"while some other studies" -> "while other studies" [Peter Gleckler, United States of America]	Accepted. We have removed "some".
13347	34	5	34	5	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted. We have fixed this typo.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80279	34	5	34	9	The statement seems to contradict the last WMO Assessment on the state of the ozone layer (2018). Its executive summary states that: New research supports the findings of the 2014 Ozone Assessment that Antarctic ozone depletion was the dominant driver of the changes in Southern Hemisphere tropospheric circulation in austral summer during the late 20th century, with associated weather impacts. - Over the period 1970 to 2000, tropospheric jets in the Southern Hemisphere shifted poleward and strengthened, the Southern Annular Mode (SAM) index increased, and the southern edge of the Hadley Cell expanded poleward. Since 2000, the SAM has remained in a positive phase. - For austral summer, most model simulations show a larger contribution to these trends from Antarctic ozone depletion compared to increases in well-mixed greenhouse gases during the last decades of the 20th century. During other seasons, the contribution of ozone depletion to circulation changes is comparable to that from well-mixed greenhouse gases. It is also in contradiction with summary of section 3.7.2 of the chapter. [Sophie Godin-Beekmann, France]	Noted. There is no contradiction with WMO (2018). We have rephrased the sentence to now explicitly only refer to blocking in the South Atlantic, not any wider impacts of ozone depletion which the reviewer is referring to.
10967	34	13	34	14	This is also supported by Patterson et al (2019; https://doi.org/10.1029/2019GL083264) in a larger sample of models. The only systematic bias found is in the Australia - New Zealand sector in DJF. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now discuss Patterson et al.
52901	34	19			Shoudn't you here specify "summertime decrease"? [Hervé Douville, France]	Accepted. We have inserted "summertime".
11253	34	20	34	21	This statement should be clarified by perhaps stating that by anthropogenic the authors mean not only greenhouse gas increase but also ozone decrease. [Edmund Kar-Man Chang, United States of America]	Accepted. We now make this explicit.
37345	34	20	34	22	There is nothing in the paragraphs earlier in this section that justify "high confidence". [John McLean, Australia]	Noted. While the reviewer is correct that the preceding paragraphs do not themselves justify the "high-confidence" statement, we make it explicit that the statement is based on a discussion of the SAM conducted in section 3.7.2. The literature evidence for this is unanimous and convincing.
11993	34	25	34	25	"medium performance" is ambuiguous and is not defined in terminogy. Please refrase this by other words. [Masaki Satoh, Japan]	Accepted. We have rephrased this sentence and have dropped "high confidence" in response to comment 1050.
21493	34	25	34	25	medium performance is a strange phraseology and risks being conflated with official confidence / likelihood language so I would suggest rewording. [Peter Thorne, Ireland]	Accepted. We have rephrased this sentence (and have replaced "high" with medium confidence here).
104991	34	25	34	25	Is this to be interpreted as there is high confidence that models have medium performance in …. ? If not rewording is needed. [Peter Gleckler, United States of America]	Accepted. There is indeed a misfit here between the "medium performance" and the "high confidence". We have rephrased this now to express "medium confidence" in models.
37347	34	25	34	28	With unproven models used for the studies you cite how can you possibly claim "high confidence"? [John McLean, Australia]	Noted. We have replaced "high" with "medium" confidence, in response also to review comment 1050.
37691	34	33	34	38	What is the difference in bottom two panels? Different hi-res models? Then, is it sugested that high resolution does not gurantee improvement I blocking statistics? [Masahide Kimoto, Japan]	Accepted. The figure is now more concise and easier to read, hopefully addressing the reviewer's comment.
35637	34	35	34	35	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. The paper has now appeared. Also we no longer use any figure directly taken from their paper.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11995	34	37	34	38	Clarify the difference between the lower two panels of Fig. 3.16. These should be appropriately described in the text. [Masaki Satoh, Japan]	Accepted. We now explain this better and have updated the figure.
41175	34	52	35	20	What is the time horizon for this "sudden" event? [TSU WGI, France]	Accepted. We now add how long these events typically last.
100767	34	52	35	20	I understand that this is a second draft; however, I do not think that 'Stratospheric Sudden Warming Activity' should be discussed in the IPCC AR6. Therefore, I would request a discussion on the removal of this subsection from the report. My view is that it must be removed. My reasoning for it is as follows: 1. There is little evidence on the relationship of climate change with such phenomena. It is acknowledged in the text. It is not only a problem of 'low confidence' on the impact of climate change but that it is 'low confidence' on the models. Indeed, this is true and well-know. There are only a few models with the ability to reproduce SSWs, and they do it with enormous limitations (and someway very idealized conditions such as dependency on specific parametrizations). They present problems even trying it with nudged data, continuous failures in the dates of final warmings, etc. 2. To include a specific subsection on sudden warmings makes the discussion unbalanced. The same level of attention is not paid to other phenomena (and maybe more relevant for climate change and atmospheric dynamics) such as the stratospheric polar vortex or the Quasi Biennial Oscillation (both barely discussed later in the chapter), or the age-of-air. Indeed, phenomena such as changes in the Brewer-Dobson circulation are presented someway obscured in its tropospheric fingerprints in the subsection 3.3.3.1 (Hadley Cell and Walker Circulation). Probably the impacts (and feedbacks) of climate change on the stratosphere get less attention than they deserve in the IPCC reports (something that perhaps should be conveniently addressed in the future, maybe with a specific chapter). However, we must acknowledge that observational records (data series) of SSWs are short and that the capability of the climate models to reproduce SSWs is in its infancy. Therefore, this subsection is included with a weaker account than is desirable because of the lack of scientific evidence. It is done in this way because, at present, we are still trying to have enough data and reliable models to study this phenomenon. Besides, it is not adequate to include the discussion on SSWs with the prominence of a specific subsection, that other perhaps more relevant phenomena - from the point of view of the evidence of impacts of climate change - do not have. We should be aware that including excessively speculative information can weaken the AR6 message as a whole, and our understanding of the	Rejected. The discussion of SSWs is part of the remit of Ch3 as agreed early on in the process. While the reviewer is right about the model limitations regarding SSWs, topics discussed in this chapter are not chosen based on how well they are simulated, but rather how consequential they are. While our understanding of the relationship between SSWs and climate change certainly has room to grow, stating that this is a limitation does not weaken the report as a whole, but rather strengthens its position as an authoritative source of information.
19775	34	55	34	55	Rather section 2.3.1.4.5 [philippe waldteufel, France]	Accepted. We have corrected this reference.
104993	35	3	34	3	"There is a corellation" what kind? High? Low? [Peter Gleckler, United States of America]	Accepted. We have now rephrased this sentence, avoiding "correlation" which the reviewer correctly objects to as having a numerical meaning which is not intended here.
11997	35	15	35	15	"does have" could be replaced by "has". [Masaki Satoh, Japan]	Accepted. We have rephrased the sentence.
111047	35	17		20	in variables that show unclear changes,it is very informative to the reader to get a summary of the performance in variability - my reading was the variability is ok which would be good to say (or not if its not) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now cite Taguchi (2017) who finds an underestimation of dynamical variability in CMIP5 models and relates that to aspects of wave-mean flow interactions that however would be too technical to convey to the typical AR6 reader.
104995	35	18	35	18	"any such trends" -> trends in the observed characterics of SSWs [Peter Gleckler, United States of America]	Accepted. We have rephrased the sentence.
24137	35	23	35	23	Why is permafrost not treated? [Wilfried Haeberli, Switzerland]	Noted. That was an executive decision, also reflected in this quantity not being covered in Ch2. Permafrost is assessed in Chapter 5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105111	35	25	35	25	I am missing a paleocontext here. At minimum warm periods should be addressed, such as the LIG (e.g. Kageyama et al., in revision for Climate of the Past, Otto-Bliesner et al., in revision for Climate of the Past) [Masa KAGEYAMA, France]	Taken into account. Paleoclimate perspective based on these papers has been provided in other subsections.
12193	35	25	38	28	The section on Sea Ice is interesting and well documented. However it is a weak point (for the WGI report as a whole) that the observation data sources used in Chapter 3 (HadISST, Bootstrap, NasaTeam) differ from those adopted in Chapter 9 and Chapter 2. In Chapters 2 and 9, it is argumented that Sea Ice Area is a better metric than Sea Ice Extent. Yet Sea Ice Extent is used in Chapter 3. Chapter 9 (and Chapter 2) carefully selected 3 sea-ice concentration observation datasets, but these are not the same shown in Figure 3.18 and 3.19. Except for the colour scheme and SIE, Fig 3.19 (top) is the same as Fig 9.15 and 9.17. I strongly recommend that Chapter 3 adopts the same observation records and methodology (e.g. SIA) than in Chapter 9 (and 2). Ideally, Chapter 9 authors can provide Chapter 3 with the data to avoid re-computation. Thank you. [Thomas Lavergne, Norway]	Accepted. We now used same SIA observations as Ch 2 and 9 with showing SIA figures.
93053	35	27			A paleoclimate perspective should be added to this section. The results from the CMIP6 lig127k experiments are available from 17 CMIP6 models and discussed in the Climate of the Past Discussion paper: Otto-Bliesner et al., [https://www.clim-past-discuss.net/cp-2019-174/] which is being updated during the paper revision stage. It would be informative to add a figure that includes the minimum Arctic sea ice in the lig127k ensemble vs simulated Arctic temperatures in that ensemble, with an assessment from the data reconstructions. [Bette Otto-Bliesner, United States of America]	Taken into account. Paleoclimate perspective based on these papers has been provided in other subsections.
52015	35	29	38	12	There's something odd going on with the figure citations in the sea ice section (3.4.1). Despite the fact that both sea ice figures (3.18 & 3.19) contain Artic and Antarctic panels, Fig 3.18 is exclusively cited in the Arctic subsection (3.4.1.1) and Fig 3.19 is exclusively cited in the Antarctic section (3.4.1.2). [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure citations corrected.
13407	35	32	35	34	It's suggested to address which processes (biophysical, climatic, etc). lead to a decline in the e xtent of the ice in September. [Maria Amparo Martinez Arroyo, Mexico]	Noted. This is relevant for Ch 9.
37887	35	33	35	33	Figure 3.19, which is seasonal evolution of Arctic and Antarcic SIE, do not mention in "3.4.1.1". Add to "Figure 3.19" after "see Figure 3.18" [Junhee Lee, Republic of Korea]	Accepted. Figures cited correctly.
52013	35	35	35	39	The sentence "The envelope of simulated ice loss across model simulations encompasses the observed change, although observations fall at the low end of the CMIP5 distribution." is not fully representative of the current SIMIP position on the modelled rate of Arctic sea ice loss. Rosenblum & Eisenman (2016; https://doi.org/10.1175/JCLI-D-16-0391.1) clearly showed that the CMIP5 models that do produce Arctic se aice decline as high as observed do so for the wrong reasons (high global warming in those models).This finding is underlined by the SIMIP "Arctic sea ice in CMIP6" paper (https://doi.org/10.1029/2019GL086749) where we conclude that, although things are improved for CMIP6, the models still fail to simulate a plausible evolution of sea-ice area without an unplausible representtaion of GMST. Additionally, as written, I find this sentence difficult to follow anyway because "fall at the low end of the CMIP6 distribution" is not clear enough. What you're really saying here is that the observations show a higher rate of decline than most of the CMIP5 models? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We revised the sentence accordingly.
104997	35	39	35	39	"better" than what? CMIP5? [Peter Gleckler, United States of America]	Accepted. Rephrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104985	35	40	35	40	While what is written about Ivanova et al. (2016) is correct, perhaps the more relevant point is that they also proposed well-defined "sector scale" metrics which can be more robustly computed than the objective calculation of the longitudinal distribution of the sea-ice edge and still provide information about compensating errors. [Peter Gleckler, United States of America]	Taken into account. We removed the sentence, citing section 9.3.1.1 for the related assessment of physical processes.
65177	35	46	36	4	It has been suggested by Polvani et al 2020 (doi: 10.1038/s41558-019-0677-4) that ozone depleting substances played a substantial role in the loss of Arctic sea ice loss (and Arctic warming) in the second half of the twentieth century. [Mark England, United States of America]	Accepted. We added a sentence on this citing the paper.
37883	35	48	35	48	CanEAM2 change to CanESM2 [Junhee Lee, Republic of Korea]	Accepted. Corrected.
54931	35	48			That should be CanESM2. Also, minor point, but the ensemble from IPSL was not large; it was brought in to demonstrate a consistent signal in a model with a different bias. I would recommend leaving the note about large ensembles and just removing IPSL from the parenthetical list. [Nancy Hamzawi, Canada]	Accepted. Revised the text accordingly.
104999	35	50	35	50	differences in simulated mean state [Peter Gleckler, United States of America]	Accepted. Corrected.
71391	35	51			Several of these papers are pre-AR5, but the paragraph is about post-AR5 literature. [Douglas Maraun, Austria]	Accepted. Added CMIP6-based studies available.
52903	36	13	36	16	Recent observed trends have been also used to constrain projections (Knutti et al., 2017) but should be considered cautiously given the possible contribution of internal variability. [Hervé Douville, France]	Taken into account. Revised the text accordingly.
98029	36	18	36	52	I don't find the discussion very convincing to support the conclusion that it is "very likely that anthropogenic forcings mainly due to greenhouse gas increases…explaining at least half of the observed decreasing trend in summer sea-ice extent". The several studies cited are close to 50% in their estimate of the internal variability contribution. The statement on line 26 "internal variability explains no more than 42.3% of the observed September sea ice melting trend, confirming previous studies" is problematic on two accounts: over-precision (open to obvious criticism if it were actually published as is) and the new study can only support the previous one—it cannot confirm it. [Thomas Knutson, United States of America]	Taken into account. Rephrased related sentences accordingly.
11999	36	25	36	25	"thre" should be "the". [Masaki Satoh, Japan]	Accepted. Corrected.
54465	36	25	36	25	Typo in "thre". [Maria del Pilar Bueno Rubial, Argentina]	Accepted. Corrected.
33275	36	25			Change "thre" by "the". [Guiomar Rotllant, Spain]	Accepted. Corrected.
21495	36	26	36	26	Is a statement to 3 sf (42.3%) really justifiable? This may be what the authors indeed claimed but is it the case that IPCC should persist such an undue precision in this manner? Approximately 40% would be a better characterisation I suspect? [Peter Thorne, Ireland]	Accepted. Corrected as suggested.
102865	36	26	36	26	42.3% - is the precision needed here? Is this robust? Remember this is an assessment report. [Philippe Tulkens, Belgium]	Accepted. Corrected as "about 40%".
37885	36	27	36	28	CESM change to CESM1 and CanESM change to CanESM2 [Junhee Lee, Republic of Korea]	Accepted. Corrected.
52017	36	31	36	33	I am a bit confused about the inclusion of "with enhanced ridging over the Arctic Ocean" here. Do you mean more "barometric ridges" in the atmosphere? Or are you talking about enhanced ridging in the sea ice (i.e., mechanical thickening through convergence). I would take this as the latter but that doesn't fit with Ding et al. (2017), who only dicuss thermodynamic impacts of the atmospheric circulation - namely anomalies in temperature, water vapour, and downward longwave radiation. [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. It represents atmospheric ridges. Rephrased for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102867	36	31	36	37	The Arctic is considered consistently ice free when the 5 yr mean extent for a given month falls below the 1 million km2 threshold. Thackeray, C.W., Hall, A. An emergent constraint on future Arctic sea-ice albedo feedback. Nat. Clim. Chang. 9, 972–978 (2019) [Philippe Tulkens, Belgium]	Noted. This comment is about future projections, and such details are not relevant in our chapter.
13349	36	33	36	33	CESM must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Noted. We used acronyms at other places.
65173	36	34	36	34	In agreement with England et al 2019 (doi: 10.1175/JCLI-D-18-0864.1) which examined CESM large ensemble and CMIP5 models. [Mark England, United States of America]	Taken into account. Suggested literature assessed.
21497	36	35	36	35	likely should either be italicised or if not intended as a likelihood statement a different word used here. Similarly extremely unlikely used across lines 42-43 in the next paragraph [Peter Thorne, Ireland]	Accepted. Corrected.
112661	36	39	36	44	Could be deleted, since most of the evidence suggests and concludes that anthropogenic forces have contributed to ice loss [Melissa Jiménez Gómez Tagle, Germany]	Rejected. Most of the assessment is focussed on attribution of trends in sea ice, but this paragraph focusses on attribution of sea ice loss in individual years. These are distinct.
52019	36	48	36	48	I would avoid using ambiguous wording like "observed decreasing trend". Do you mean that the trend is itself decreasing (i.e., becoming more negative) or just that it is a negative trend (i.e., causing sea ice decrease)? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rephrased it appropriately.
19777	37	1	37	9	Figure 3.18 does show that CMIP6 simulate Arctic sea ice better than CMIP5 and that the performance concerning the Antarctic remains poor. A puzzling feature is provided by the regression slopes. They belong to a pure exercise of comparison among models, from the viewpoint of a relationship to explore between sea ice extent mean value and trend. What is the idea? [philippe waldteufel, France]	Taken into account. We revised this part accordingly.
54463	37	3	37	7	Please, consider adding a short sentence in the figure caption of Figure 3.18 pointing out the different scales used in x- and y-axis for North and South hemispheres (left and right panels respectively). [Maria del Pilar Bueno Rubial, Argentina]	Accepted. Added the info in figure caption.
12001	37	4	37	4	The panels on February in Fig. 3.18 are not referred to in the text. Needs a message based on this figure, since all the models do not capture the observed increasing trend of SIE. This observed increasing trend in the February SIE is inconsistent with Fig. 3.19. [Masaki Satoh, Japan]	Taken into account. We revised the text accordingly.
12003	37	4	37	4	The observed increasing trend in the February SIE in Fig. 3.18 seems inconsistent with Fig. 3.19. All the observed show decreasing trend for all the seasons. [Masaki Satoh, Japan]	Taken into account. We considered same periods for trends.
35027	37	12	38	12	Please add a link to Section 9.3.2 [Baylor Fox-Kemper, United States of America]	Accepted. This section did already contain one reference to 9.3.2 on observed regional changes in Antarctic sea ice, but two more have been added - one more for observed Antarctic sea ice trends more generally, and a third on the causes of trends.
13351	37	23	37	23	use precesses [Maria Amparo Martinez Arroyo, Mexico]	Rejected. Processes are correct.
33277	37	38			Section: Antarctic Sea Ice. Why thickness of the ice is not take in account? [Guiomar Rotllant, Spain]	Noted. We focused on SIA assessment due to limited D&A studies for ice thickness.
12005	37	45	37	47	Does this sentence implies inadequate representation of the fresh water fluxes by mass loss of the Antarctic ice sheet in CMIP models? If so, it should be clearly stated, such as a need for more realistic physical processes of ice sheet melting. [Masaki Satoh, Japan]	Taken into account. We revised the text accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107717	37	47	37	47	"Increased fresh water fluxes caused by mass loss of the Antarctic ice sheet (either by melting at the front of ice shelves or via iceberg calving) have been suggested as a possible mechanism driving the multidecadal Antarctic sea ice expansion (Bintanja et al., 2015; Pauling et al., 2016)" - although Bintanja et al. (2015) and Pauling et al. (2016) both deal with increased freswater fluxes, they do not reach the same conclusion, as shown from this quote by the later Pauling et al. (2017) paper: "Satellite observations of Antarctic sea ice extent have shown an overall slight increase over time in recent decades (Parkinson & Cavalieri, 2012), in stark contrast to the rapid decline seen in the Arctic (Cavalieri & Parkinson, 2012). This increase has not been reproduced by models in the Coupled Model Intercomparison Project phase 5 (CMIP5) (Zunz et al., 2013). Proposed reasons for the discrepancy between models and observations include ... freshwater input from ice shelf melt (Bintanja et al., 2013, 2015) (although the studies of Swart and Fyfe (2013) and Pauling et al. (2016) found that this had no significant effect on the rate of change of sea ice area with respect to time). A consensus on the cause of sea ice expansion in recent decades is therefore lacking." Pauling, A.G., Smith, I.J., Langhorne, P.J., Bitz, C.M. (2017). Time-Dependent Freshwater Input From Ice Shelves: Impacts on Antarctic Sea Ice and the Southern Ocean in an Earth System Model. Geophysical Research Letters, 44(20):10454–10461, doi: 10.1002/2017GL075017. I suggest rewording this as follows: "Increased fresh water fluxes caused by mass loss of the Antarctic ice sheet (either by melting at the front of ice shelves or via iceberg calving) have been examined as a possible mechanism driving the multidecadal Antarctic sea ice expansion (Bintanja et al., 2015; Pauling et al., 2016) but there is a lack of consensus on this mechanism's impacts (Pauling et al., 2017)" [Inga Jane Smith, New Zealand]	Taken into account. We revised the text accordingly.
116217	37		37		Please improve consistency with ch 2and 9 for the description of changes in Antarctic sea ice changes, and avoid duplication. Also, check the use of the term "abrupt" (see glossary). [Valerie Masson-Delmotte, France]	Accepted. We showed SIA using the same observations as Ch 2 and 9 and updated text based on updated results with using more CMIP6 models.
52021	38	10	38	12	The wording here (i.e., "subsequent abrupt decrease...2016-2019...not generally captured by global climate models") is a bit misleading because you wouldn't expect the CMIP HIST simulations to sepcifically represent the timing of this sort of thing. I guess the issue is that the models collectively do not include that sort of feature (reversal of negative trend)? [Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We revised the text accordingly.
11491	38	12	38	12	"...and there is low confidence in the attribution of these changes in Antarctic sea-ice extent." Is there any attribution at all? As there is no significant Antarctic trend, there isn't anything to attribute anyway, is there? In some sense, there could be high confidence that there are no changes to attribute yet... [Gerhard Krinner, France]	Taken into account. Revised the statement to better represent attribution results.
13353	38	19	38	19	use explanation from figure 3.20 more complete [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. Revised the text accordingly.
4231	38	31	39	50	In this section the emphasis is laid on snow cover extent. However, changes in snow cover thickness are highly important for the thermal state of permafrost and river flooding and discharge in Arctic areas. There is evidence of increasing snow cover thickness in areas under the influence of the Arctic ocean (Bintanja and Selten, 2014, in Nature). Could a paragraph be added on the outcomes of the CMIP models in this respect? [Jacobus (Ko) van Huissteden, Netherlands]	Accepted. We agree that snow thickness (SWE) is an important climate variable. SWE is covered for CMIP5 simulations (p39L30-35 of the SOD). We additionally now discuss Mudryk et al. (2020) who have evaluated SWE in CMIP6 simulations.
88165	38	31			section 3.4..2 - Chapter 2 could also be referenced particularly for observations of snow cover change [Sharon Smith, Canada]	Accepted. We now refer to Ch2 for observational background.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11493	38	35	38	37	These two sentences are not absolutely necessary and can be cut if the chapter is too long. [Gerhard Krinner, France]	Accepted. We have shortened this paragraph (also in response to a general comment to avoid those textbook-style intros).
12007	38	37	38	37	"Section 9.5.4" should be "9.5.3". [Masaki Satoh, Japan]	Accepted. We have fixed this issue.
11495	38	39	38	53	This paragraph could be shortened or updated with more recent CMIP6 literature. [Gerhard Krinner, France]	Accepted. We now discuss Mudryk et al. (2020) representing CMIP6 literature.
33279	38	39			Section: Snow Cover. Why thickness of the Snow cover is not take in account? [Guiomar Rotllant, Spain]	Noted. We do cover SWE. See comment 4231.
116219	38		38		Please check coherency with chapter 7 on the snow albedo feedback. [Valerie Masson-Delmotte, France]	Taken into account. We checked for consistency with Chapter 7 and there were no inconsistencies. Chapter 7 focusses mainly on the combined surface albedo feedback due to both snow and sea ice changes, whereas our focus here is on snow changes only.
54941	39	3	39	16	It's unclear why the right hand side of Figure 3.20 is required and what it contributes to the discussion. Just showing the upper left and lower left panels (side by side) may be simpler and sufficient. [Nancy Hamzawi, Canada]	Accepted. We merged the left and right hand panels into one each for CMIP5 and CMIP6.
54943	39	11	39	12	Why are different definitions for the spread used for CMIP5 compared to CMIP6? If possible it would be better to use a consistent definition of the spread. [Nancy Hamzawi, Canada]	Accepted. With more CMIP6 models available for the FGD, both groups are now compared using the same metrics.
54469	39	21	39	21	Typo in "CMP6", please replace with "CMIP6". [Maria del Pilar Bueno Rubial, Argentina]	Accepted. We have corrected this typo.
54933	39	22			The text in the paragraph says February-March while the figure caption reads March-April. [Nancy Hamzawi, Canada]	Accepted. We have made the text and figure consistent, by referring to March-April in the text.
11497	39	24	39	26	See The Cryosphere, https://doi.org/10.5194/tc-2019-320, submitted (disclosure: I'm a coauthor of that paper). [Gerhard Krinner, France]	Accepted. We now discuss Mudryk et al. (2020), resulting in a slightly modified assessment.
54935	39	24	39	26	It's unclear what the "earlier studies" are. The Brutel-Vuilmet study examined CMIP5 and Chapter 9 states there were no clear differences in SCE trends between CMIP5 and CMIP6. What then is this improved agreement with observations? The statements regarding SCE trends made here and in Chapter 9 should be consistent with one another. [Nancy Hamzawi, Canada]	Noted. The statement does not imply that CMIP6 models are better than CMIP5. Rather, "both CMIP5 and CMIP6 models show improved agreement with observations by simulating stronger declines during recent years" (i.e. the addition of several years with strong simulated and observed SCE declines improves the agreement). We have rephrased the sentence to make this clearer.
64703	39	29	39	29	I suggest referring here also to Najafi et al. (2017) (already quoted), to illustrate the low number of attribution studies for the mountain snow cover, as noted in Hock et al. SROCC Chapter 2. This chapter also referred to Pierce et al., 2008, as one of the few examples of Detectation and Attribution for mountain snow cover trends (Pierce, D.W. et al., 2008: Attribution of declining Western U.S. snowpack to human effects. J. Clim., 21(23), 6425–6444, doi:10.1175/2008JCLI2405.1.) [Samuel Morin, France]	Noted. We agree with the reviewer on the small number of attribution studies on this topic. However, we prefer not to specifically discuss mountain snow cover here as our focus in Ch3 is on the continental scale (i.e. individual mountain ranges are below this scale). Mountain snow cover is also not covered in Ch2, making it out of scope for Ch3 to assess the realism of the simulation of mountain snow in CMIP models.
54937	39	38	39	39	This statement is not attributed. Mudryk et al., 2020 (submitted) perhaps? Mudryk, L., Santolaria-Otín, M., Krinner, G., Ménégoz, M., Derksen, C., Brutel-Vuilmet, C., Brady, M., and Essery, R.: Historical Northern Hemisphere snow cover trends and projected changes in the CMIP-6 multi-model ensemble, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-320, in review, 2020. [Nancy Hamzawi, Canada]	Accepted. We have now added two references (Brutet-Vuilmet et al., 2013, and Mudryk et al., 2020) to support this statement (which is really based on the preceding paragraph).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52905	39	44	39	46	Processes are well understood but still not well represented in many models that do not compute separate energy budget for the canopy and the underlying snow pack) => remove the sentence or at least temper this statement? [Hervé Douville, France]	Accepted. We have made the statement more precise by replacing "lack of process understanding" with "Deficiencies in representing this process in climate models".
12009	39	46	39	46	Figure 9.24 shows NH permafrost extent and volume. Fig. 9.26 is an appropriate link. [Masaki Satoh, Japan]	Accepted. See comment 1125.
130607	39	46	39	47	Surface air warming is a strange expression. [Panmao Zhai, China]	Accepted. We have rephrased this sentence.
54939	39	46			This should refer to Figure 9.26. (Figure 9.24 is for permafrost) [Nancy Hamzawi, Canada]	Accepted. We have corrected this error.
96273	39	51	40	45	A lot of space is devoted here to the discussion of how glaciers a represented in CMIP type models. However, for D&A, the topic of this chapter, this hardly plays a role, as is stated on page 3-40, line 47. We suggest to shorten this section considerably. (E.g., GlacierMIP so far has only addressed projections, the paragraph around this can be deleted.) [Nicole Wilke, Germany]	Accepted. We have shortened the text in response to this comment.
96275	39	55	40	1	Please provide example references for CMIP models including land surface ice in the form of glaciers for many years. [Nicole Wilke, Germany]	Not applicable. We have cut out this sentence as we now focus on specialist glacier models rather than CMIP-type models that don't represent glaciers well enough to matter here.
12011	40	8	40	9	This sentence is too specific to an example of one model, and can be removed in this chapter. [Masaki Satoh, Japan]	Rejected. The sentence is here to illustrate progress with modelling, not merely to single out one model.
26749	40	11	40	12	It should tell about the result, not the fact that there is a MIP [Eric Brun, France]	Rejected. The research environment relevant to this topic is relevant here.
112663	40	14	40	21	Could be a good suggestion to write additional information on the methane bubbles and the unknown diseases frozen in the ice caps, emphasizing that it won't just be loss of water, but release of these elements [Melissa Jiménez Gómez Tagle, Germany]	Rejected. While these are undoubtedly interesting topics, going into the aspects would be a digression at this point.
130605	40	14	40	21	This part is not on attribution assessment. [Panmao Zhai, China]	Noted. That is correct; the attribution of SMB changes in ice caps and glaciers is following in the subsequent paragraphs. It is necessary to set the scene though before getting into this.
32123	40	17			reference is named (Shepherd et al., 2019a) in Chapter 3 and (The IMBIE Team 2019) in Chapter 9, as it is proposed in the publication itself [Anja Wendt, Germany]	Taken into account. We cite all papers by author(s) and year, for consistency. We have liaised with Ch9 to change their style of citation.
89375	40	21	40	21	Is this meant to be “22% from glaciers and 18% from both ice sheets” (i.e., total land ice contribution = 40%), or 18% from each ice sheet (i.e., total land ice contribution = 58%)? cf. 24% for glaciers and 16% from Greenland, 11% from Antarctica given on 9-113, lines 19-21. [Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have inserted "both" (i.e. 40% contribution from land ice loss, 60% from thermosteric expansion). The discrepancy w.r.t. FAQ9.2 is there because of different periods assessed (i.e. no discrepancy).
71889	40	24	41	5	This section is inadeuqte - very incomplete. Glaciers are one of the two main contributors to sea level rise during the 20th century and will make major contributions during the 21st century. I strongly recommend that the authors get an additional contributing author (such as Ben Marzeion and Tad Pfeffer) to write a more complete assessment. [John Church, Australia]	Taken into account. We now have an additional CA to cover glaciers, and have substantially updated this section.
127315	40	24	41	5	This section is inadequate and very incomplete. Glaciers are one of the two main contributors to sea-level rise during the 20th century and will make major contributions during the 21st century. Strongly recommend that the authors get additional contributing authors (such as Ben Marzeion and Tad Pfeffer) to write a more complete assessment. [Trigg Talley, United States of America]	Taken into account. We now have an additional CA to cover glaciers, and have substantially updated this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
88055	40	24			This section is, to some extent, repetitive in respect to the Ch9 text on the glacier change drivers and I find the Ch9 assessment more comprehensive. You may try to harmonize with Ch9. [Georg Kaser, Austria]	Taken into account. Indeed Ch9 by design has a more comprehensive treatment of glaciers. There are however no factual disagreements between the two chapters. We have removed remaining overlaps with Chapter 9.
6613	40	26	40	26	The definition of "glaciers" that appears here includes ice caps, yet on line 4 of page 51 of this chapter one sees reference to "glaciers and ice caps". There are also references to "ice caps" in Chapter 2. I did not check other chapters. Terminolgy needs to be made consistent. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now make clear that for the purpose of assessing mass balance, we do not draw a distinction between glaciers and ice caps. Of course they are separate concepts in glaciology.
71393	40	26	40	33	I like this introductory paragraph giving a brief overview of relevant processes (although I am not sure it is a definition, maybe use a different term). I would only suggest that you use such type of paragraph consistently throughout the Chapter. E.g., for Sea Ice, precipitation patterns etc.). [Douglas Maraun, Austria]	Noted. Other reviewers actually don't like such "textbook-style" introductions. This is being shortened in response to other comments. Also the textbook-style intros elsewhere have been shortened.
2609	40	26	41	5	Is there no relevant figure that shows the progress of glacier modelsing? [Bryan Weare, United States of America]	Noted. For space reasons we have decided not to include a figure here for glacier mass loss but now refer to section 9.5 which has a figure illustrating glacier mass loss.
111049	40	26		33	this is nice but a bit a textbook text. Could be condensed some [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have removed some textbook material.
2849	40	27	40	27	Replace "occur most often" by "can be found" and add "and high altitude" after "high latitude". The sentence would become: "Glaciers can be found in high latitude and high latitude cold regions where …". [Antoine RABATEL, France]	Not applicable. The whole paragraph has been shortened in response to other review comments.
11499	40	28	40	30	Is that sentence really useful? Also the preceding one ("Glaciers occur most often…") probably doesn't contain much new information for most readers. [Gerhard Krinner, France]	Accepted. The sentence has been removed.
26751	40	36	40	39	The text should go straight to the result [Eric Brun, France]	Accepted. We have shortened the introductory paragraphs in response also to other review comments.
35639	40	38	40	38	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. The paper (Marzeion et al., 2020) has now been accepted.
11501	40	38	40	39	Doesn't GlacierMIP simply compare existing global glacier models using coordinated experiments? Not sure it's an intercomparison of previous modeling efforts. [Gerhard Krinner, France]	Accepted. We have rephrased this sentence.
339	40	44	40	44	Spelling is “Randolph” instead of “Randolf” [Etienne Berthier, France]	Accepted. This has been corrected.
13409	40	47	40	48	It's suggested to mention the associated uncertainty of offline simulations obtained from a subset of CMIP5 historical simulations [Maria Amparo Martinez Arroyo, Mexico]	Accepted. The uncertainty ranges from Marzeion et al. (2014) include both model selection and natural variability components.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83579	40	48	41	5	This comment refers to the use of the findings of Marzeion et al. (2014) with respect to attributing global glacier mass loss to anthropogenic influence. Chapter 3 rightly notes that: "The conclusion of this work was that 25 ± 35% of the global glacier mass loss was attributable to anthropogenic influence, with this number increasing to 69 ± 24% over the 1991 to 2010 period (Marzeion et al., 2014)" By contrast, the summary comment on attribution of glaical retreat to human influence appears to directly contradict the findings of Marzeion et al. (2014), in stating: "we conclude that the recent observed retreat of global glaciers is very likely attributable to anthropogenic influences". Readers may be understandably confused as to how to reconcile this finding with the evidence from Marzeion et al. (2014) provided above. Regarding the summary statement on attribution of global glacier retreat, and how it relates to the findings of Marzeion et al. (2014): 1. "Recent observed retreat": I am unsure if "recent" is defined elsewhere in the report as referring to a specific period of time, but, if not, the time period to which this comment refers is ambiguous. 2. "is very likely attributable to anthropogenic influences" - does this mean all retreat is very likely attributable, or that at least some portion of observed retreat is attributable? I find this statement to be unclear. 3. The findings of Marzeion et al. (2014) would imply that 20th Century glacier mass loss is primarily natural, which would appear to contradict the summary statement made here (unless 'recent' refers only to 1991-2010 and 'attributable' refers to most but not all observed retreat). 4. My personal view is that this difference is explained by the fact that the findings of Marzeion et al. (2014) are not correct. These results significantly underestimate the human contribution to observed glacial retreat (which is, rightly, used as a key symbol of climate	Taken into account. Assessment of Marzeion et al. (2014) has now been supplemented with assessment of Roe et al. (2021) who have found a substantially larger proportion of glacier mass loss is attributable to human influence. As Roe et al. include Marzeion as co-author, we feel the more recent study supersedes Marzeion et al. (2014). This resolves the problems noted by the reviewer.
69553	40	49	40	50	The Marzeion et al study also excluded the main Greenland ice sheet, but did include peripheral glaciers around the Greenland margin. [gerard Roe, United States of America]	Accepted. We now state explicitly that only the two ice sheets are excluded (i.e. all small glaciers including in Antarctica and Greenland are included).
102869	40	50	40	51	For a scientifically trained non-expert it is at least surprising to see a range from -10% to +60% of attributable glacier mass loss. What does it mean that -10% of the mass loss is attributable to anthropogenic influence? Does it mean that some glaciers have grown, and this is also attributable to anthropogenic influence? Or does it mean that 10% are actually attributabel to non-anthropogenic influence. What does 100% of the mass loss then represent. The statement is at least ambiguous. [Philippe Tulkens, Belgium]	Accepted. These numbers are quoted from Marzeion et al. (2014). It means that human influence would have been consistent with no change (or even some growth) in glacier mass at the 1-sigma confidence level. This paper is controversial; we therefore do not base our conclusion only on this paper, and have updated our assessment on this topic with new literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16039	40	50	40	52	in line 50, it is written that: "The conclusion of this work was that 25 ± 35% of the global glacier mass loss was attributable to anthropogenic influence, with this number increasing to 69 ± 24% over the 1991 to 2010 period (Marzeion et al., 2014)" This sentences is extracted from the abstract of this paper. However, I found this sentence a little bit misleading. From this sentence, we could believe that the glacier mass loss could reveal the anthropogenic influence on climate, i.e the origin of the temperature increase. Unfortunately, I do not believe it is the case. These modelling studies enable to quantify the part of glacier mass loss due to the temperature increase related to the greenhouse gas increasing using climate modelling. From the glaciological model, the authors calculated the glacier mass loss from the increase of temperature due to GHG increasing. But these calculations do not tell anything about the origin of the temperature increase. In other words, the glacier mass loss reveals the increase temperature, but not the cause of the temperature increase. It is very different. I suggest to reformulate the sentence to make it clear. For instance "The conclusion of this work was that 25 ± 35% of the global glacier mass loss was attributable to the temperature increase due to the anthropogenic influence calculated from climate modelling..." [Christian Vincent, France]	Rejected. Elsewhere in the report, global warming is "unequivocally" linked to human activities. This allows us to not just link glacier mass loss to warming but also to anthropogenic forcing.
69551	40	50	40	52	"The conclusion of this work was that 25 ± 35% of the global glacier mass loss was attributable to anthropogenic influence, with this number increasing to 69 ± 24% over the 1991 to 2010 period (Marzeion et al., 2014)". The Marzeion et al. 2014 study was also given a lot of weight in the SROCC report. I think that the AR6 should directly confront the impllications of the Marzeion et al (2014) conclusions. If it was true that only 25% of the glacier mass loss since 1850 was anthorpogenic, then the observed glacier reteat over the 20th century should absolutely not be used as an icon of anthropogenic climate change. Further, if it was true that it was not until 1990 that the anthropogenic causes of mass loss became larger than the natural causes, then the anthropogenic contribution to actual glacier *retreat* would be minuscule. The reason is that glacier lengths have multidecadal response times, and so have just barely begun to respond to the mass-balance of the last 20 years. Most of the glacier's reponse is to earlier climate changes. If the conclusions of Marzeion et al. (2014) were true for all glaciers it would, for instance, be deeply misleading to contrast images of early-20th-century and modern glacier positions, and state that as evidence of anthropogenic climate change. The Maerzeion et al. (2014) conclusions also stand in contradiction to other iPCC statements about detection and attribution of climate change: In the SR15 report, for instance, the central estimate is that the long-term temperature change since 1850 is essentialy all anthropogenic. It light of that, it is not possible for there to be a 150-yr period of naturally negative mass balance that exceeds the anthopogenic component for all but the last 20 years. In a constant climate, glacier mass-balance naturally reverts towards zero as the glacier adjusts. The only way to sustain negative mass balance is to have a steady warming trend, so the glacier is always out of adjustment. The only other possibility is that glaciers in 1850 were so far out of equilbbrium because of a dramatically intense little ice age (one that ended prior to 1850), and that glaciers have such long timescales that they remember those initial conditions for the next 150 years. However, that is inconsistent with our proxy reconstructions of temperature, and our knowledge of glacier response times. It is my view that there are methodological flaws in the Marzeion et al. (2014) paper that give rise to their results. But the point of my comment here is that the study stands in	Accepted. Assessment of Marzeion et al. (2014) has now been supplemented with assessment of Roe et al. (2021) who have found a substantially larger proportion of glacier mass loss is attributable to human influence. As Roe et al. include Marzeion as co-author, we feel the more recent study supersedes Marzeion et al. (2014). This resolves the problems noted by the reviewer.
89377	40	55	40	55	I would prefer ‘glacier increase’ rather than ‘glacier accretion’ here – accretion sounds odd to my ear in this context. [Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased this sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32661	41	1	41	30	Although climate change does not directly affect groundwater, it is best to note the indirect effects of global warming on groundwater withdrawal. [sadegh zeyaeyan, Iran]	Not applicable. This comment is misplaced, we cannot identify what it refers to.
32991	41	1	41	30	Although climate change does not directly affect groundwater, it is best to note the indirect effects of global warming on groundwater withdrawal. [Sahar Tajbakhsh Mosalman, Iran]	Not applicable. This comment is misplaced, we cannot identify what it refers to.
16047	41	2	41	2	the results of Thibert et al. 2018 coul be added here, given that , from melting and energy balance analysis, they found the influence of long-wave irradiance increase in glacier mass loss during the last decades. They wrote in abstract of this paper that " Long-term trends are driven by the lengthening of melt duration due to earlier and longer-lasting melting of ice along with melt intensification caused by trends in long-wave irradiance and latent heat due to higher air moisture." I believe that it is a rare paper to show the influence of long-wave irradiance increase in glacier mass loss over several decades. Thibert, E., Dkengne Sielenou, P., Vionnet, V., Eckert, N. and Vincent, C. (2018). Causes of glacier melt extremes in the Alps since 1949. Geophysical Research Letters, 45. https://doi.org/10.1002/2017GL076333 [Christian Vincent, France]	Rejected. This paper is out of scope for Ch3 as it is focussed on too small a region. Their conclusions are however consistent with our analysis.
16049	41	4	41	5	I wonder if the sentence "In summary, based on new evidence since the AR5, we conclude that the recent observed retreat of global glaciers is very likely attributable to anthropogenic influences." could be misleading. The cited papers estimated the part of ice mass loss due to anthropogenic greenhouse gas, using climate models. In this way, the temperature increase due to the anthropogenic influences is driven by the climate modelling. The anthropogenic influences is estimated from GCM experiments. However, the glaciers decrease cannot tell us anything about the anthropogenic influences, unfortunately. The glacier mass loss reveals the temperature increase, but not the cause of the temperature increase. It is very different. My feeling is that this sentence is an extrapolation of the cited papers. In this way the "very likely" seems to me too strong. [Christian Vincent, France]	Rejected. While the reviewer is right that the glacier mass loss is mainly (not only) reflecting temperature increases, elsewhere in the report this warming is "unequivocally" linked to human influence. Thus we can therefore link glacier mass loss to human influence. Our new summary statement now has that "human influence is very likely the main driver of recent near-universal retreat of glaciers globally".
69555	41	4	41	5	"In summary, based on new evidence since the AR5, we conclude that the recent observed retreat of global glaciers is very likely attributable to anthropogenic influences." I think the langauge is a bit slippery here. What does 'recent' mean? Two studies are cited about mass balance (Marzeion et al., 2014; Hirabayashi et al., 2016), but the attribution statement is about glacier retreat. Mass-balance is connected to glacier length via glacier dynamics. And, as noted in a comment above, the Marzeion et al. (2014) study in particular would actually imply the opposite conclusion: for glaciers with decadal-and-longer response times, the interval over which anthropogenic forcing has dominated is too short and too recent for it to be the cause of glacier retreat. I find Hirabayashi et al. (2016) hard to understand, but it is also an attribution to mass-balance changes since 1980. An alternative approach is to make the argument that Roe et al. (2017) showed that temperature trends over the last 140 years (i.e., since 1880) have caused glaciers to retreat far beyond their envelope of natural variability; and that those 140-yr temperature trends have been indepedently attributed to anthropogenic causes (e.g., SR15). When taken together, that implies a much stronger attribution statement. I am the 1st author of Roe et al. (2017), but please don't take these concerns as self serving. [gerard Roe, United States of America]	Noted. The statement actually does imply that humans are the leading influence for glacier mass loss. We have rephrased the sentence to deal with the length versus mass issue, and now clarify what we mean by "recent".
91097	41	4	41	5	This sentence is misleading and/or poorly worded. "Recent" is undefined and could mean anything. In addition, the evidence indates that part (perhaps the dominant part) of the glacier retreat is due to AGW, not all [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased the sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98031	41	4	41	5	I believe this should say either "…very likely attributable in part to anthropogenic" or "…most of the recent observed retreat is very likely attributable to anthropogenic…" [Thomas Knutson, United States of America]	Noted. We have rephrased the sentence.
105113	41	8	41	8	I am missing a paleocontext here. Are current models able to simulate past ice sheets, especially smaller ice sheets/fast changing ice sheets. This could also be a reference to another chapter [Masa KAGEYAMA, France]	Noted. Ch3 is focused on ocean basin and continental scales, and Ch9 provides a comprehensive assessment of model evaluation for glaciers and ice sheets, which are smaller-scale than the focus of this chapter.
71891	41	8	41	50	Again, this section is inadequate - very incomplete. The focus is too strongly on just coupled models and ice sheet MIPS. A lot has been written since the AR5 on ice sheet contributions. I strongly recommend that the authors get an additional contributing author (such as Anthony Payne and Anders Levermann) to write a more complete assessment. [John Church, Australia]	Noted. We have substantially revised this section, also in response to other reviewers.
127317	41	8	41	50	This section is inadequate and very incomplete. The focus is too strongly on just coupled models and ice sheet MIPS. A lot has been written since the AR5 on ice sheet contributions. Strongly recommend that the authors get additional contributing authors (such as Anthony Payne and Anders Levermann) to write a more complete assessment. [Trigg Talley, United States of America]	Noted. We have substantially revised this section, also in response to other reviewers.
111051	41	8		51	quite long section for a not very strong conclusion and a bit a hard read - could condense slightly? (with less model detail?) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have condensed the material a bit.
32125	41	11			ice sheets of Antarctica: The ice sheet can be devided in East and West Antarctic Ice Sheet, but Here I would use singular. [Anja Wendt, Germany]	Accepted. We have rephrased this.
32127	41	11			ice sheets of Greenland? There is only one, afaik [Anja Wendt, Germany]	Accepted. We have rephrased this.
2851	41	17	41	17	"the observed mass of loss" shouldn't be "the observed mass loss"? [Antoine RABATEL, France]	Accepted. We have rephrased this.
69683	41	17	41	17	"observed mass of loss" should be "observed mass loss" [Matthew Hoffman, United States of America]	Accepted. We have rephrased this.
26753	41	20	41	29	For an IPCC assessment all the details on intercomparison are too long. This should be gathered in a specific section with CMIP6 and all other MIPS, so that only the developement that are relevant for the assessement should be discussed here. Also Chapter 9 is on ice-sheet, so may be part of this would be better in chapter 9. [Eric Brun, France]	Noted. We have improved linkages with Ch9 and have shortened the text.
99095	41	20	41	50	Fine to be explaining the situation with models, but the modeling of the ice sheets is difficult, and it would seem must be done then is to be relying on observations of the accelerating flow of ice streams and the thinning of ice shelves. Just because models can reproduce what is happening is not an excuse for not providing an assessment of the likelihood based on the coincidence of the ice sheet increaseing deterioration with human-induced warming. And just because it cannot be confirmed to two-sigma significance does not mean that it is not human induced (at least much more likely than that). [Michael MacCracken, United States of America]	Taken into account. The ice sheet attribution assessment has been substantially updated in the FGD.
69685	41	23	41	23	"project" should be "projects" [Matthew Hoffman, United States of America]	Accepted. We adopt the reviewer's suggestion.
12013	41	25	41	29	The specific names of the models are not necessary here, and can be removed. [Masaki Satoh, Japan]	Accepted. We adopt the reviewer's suggestion.
11503	41	31	41	36	An important paper for ice sheet attribution could be Holland et al., 2019: West Antarctic ice loss influenced by internal climate variability and anthropogenic forcing https://doi.org/10.1038/s41561-019-0420-9. [Gerhard Krinner, France]	Accepted. We now cite this work.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46549	41	31	41	46	With respect to detection and attribution studies, the study of Fyke et al. (2014) may be relevant to mention here. The authors explore when a detectible pattern of human anthropogenic influence on Greenland ice sheet surface mass balance will emerge from internal variability (Fyke, J. G., Vizcaíno, M., & Lipscomb, W. H. (2014). The pattern of anthropogenic signal emergence in Greenland Ice Sheet surfacemass balance. Geophysical Research Letters, 41(16), 6002–6008. https://doi.org/10.1002/2014GL060735) [Stephen Price, United States of America]	Accepted. We now cite this work.
39101	41	31	41	50	About the anthropogenic attribution of mass loss from AIS, the findings from Holland et al deserve a mention. Holland, Paul R., et al. "West Antarctic ice loss influenced by internal climate variability and anthropogenic forcing." Nature Geoscience 12.9 (2019): 718-724. [Ola Kalen, Sweden]	Accepted. We now cite this work.
99097	41	32	41	32	How can it be written that there is only an obervational record since 1992--this may be the time for good satellite data, but the overall record goes back much further. This is an absurd statement and the idea that this is what limits indicating that the changes going on are very likely due to a human influence. And also that models have difficulty is not an excuse either. [Michael MacCracken, United States of America]	Noted. The lower quality of any data in existence prior to 1992 does limit what can be said though. Moreover AR6 can only assess literature on attribution and does not engage in plausibility arguments.
32129	41	32	41	33	Reference (Shepherd et al., 2019b) is the same as (Shepherd et al., 2019a), delete one. Furthermore. It is cited as (The IMBIE Team 2019) in Chapter 9, as it is proposed in the publication itself. Please harmonize across chapters. [Anja Wendt, Germany]	Accepted. This is fixed in the final draft.
14719	41	35	41	36	"but also appropriate atmospheric and oceanic conditions to use as a boundary forcing to drive the models" -> "but also appropriate atmospheric and oceanic conditions to use as a boundary forcing to drive the models, and appropriate levels of coupling between ice sheets and surrounding Earth system components necessary to represent ice-sheet/Earth system feedbacks" [Jeremy Fyke, Canada]	Noted. The suggested formulation is too wordy in this context.
32447	41	35	41	36	Forcing of models is only one part. We still have only incomplete coverage of sub-ice shelf cavities and also ice thicknesses, especially in Antarctica. Should be listed here. Improving observational BCs are also required to have models produce results with lower uncertainties. [Olaf Eisen, Germany]	Noted. It is unclear how the reviewer's comment fits here.
12015	41	42	41	45	ISMIP6 is already introduced before (L24), so if no attribution literature since AR5, this sentence is redundant. In particular, the example of a model "such as SeaRISE (Bindschadler et al., 2013; Nowicki et al., 2013)" is too specific and not necessary. [Masaki Satoh, Japan]	Accepted. We have shortened the text, removing this part.
26755	41	42	41	46	Too much on organisation and not enough on the key results and what we learn. [Eric Brun, France]	Accepted. We have revised this text for the FGD.
91099	41	48	41	50	see comment 9 above and Trusel, L. D., S. B. Das, M. B. Osman, M. J. Evans, B. Smith, X. Fettweis, J. R. McConnell, B. P. Y. Noel and M. R. van den Broeke (2018). "Nonlinear rise in Greenland runoff in response to post-industrial Arctic warming." Nature 564(7734): 104-+. [Jonathan Bamber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now cite this reference.
99099	41	48	41	50	This conclusion is just far too cautious given what observations are showing and analyses such as by Rignot, etc. Paleo evidence suggests that the equilibrium sea level sensitivity to global warming is 15-20 METERS per degree C, and these changes are clearly responding with faster ice flow, ice shelf thinning, and hiding behind the challenge of modeling and the need for two sigma significance is just not an excuse for drawing some conclusions from observations. [Michael MacCracken, United States of America]	Noted. This is an assessment of the available literature. The remit of this chapter is to assess attribution studies for the human influence; this is what they support. If other lines of evidence support stronger statements, these would have to make their way into the literature (and would likely be assessed in Ch9 not here). We have rephrased the sentence to say that ""modelling studies indicate that" to make clear this is evidence discerned from models, not directly from observations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14721	41	49	41	49	While not official attribution, the model-based signal-to-noise emergence study of GrIS SMB Fyke et al. 2014 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014GL060735), and/or similar studies if they exist, support the 'likely' ranking that anthropogenic forcing is playing a role in recent GrIS surface melting trends. [Jeremy Fyke, Canada]	Accepted. We now cite this reference.
11505	41	49	41	50	"low confidence in attributing the causes of the observed mass of loss from the Antarctic ice sheet since 1993": I didn't see any attribution of Antarctic ice loss in the paragraphs above, so how can there be any (even low) confidence in this attribution? [Gerhard Krinner, France]	Noted. We have revised the assessment and now do make an attribution statement for Antarctic ice sheet mass loss (albeit with low confidence).
2853	41	50	41	50	"the observed mass of loss" shouldn't be "the observed mass loss"? [Antoine RABATEL, France]	Accepted. We rephrase this sentence.
116223	41		41		A special discussion across various sections of chapter 3 on changes in the southern ocean, Antarctic sea ice, Antarctic mass balance would make sense. [Valerie Masson-Delmotte, France]	Noted. We agree with the reviewer that that might be desirable. Given the late stage of review we are in, this section would not see any peer review. In response to the comment, we strengthen the linkages between different parts of the report, e.g. sea ice and ocean circulation in the Southern Ocean.
71395	42	1			I am surprised that in many of the following subsections, and in particular in the introductory section to 3.5, there is no reference made to the SROCC. Many of the issues have been dealt with there in detail, so you need to refer to that (e.g., the Ocean Heat content attribution, 3.5.1.3). [Douglas Maraun, Austria]	Accepted. SROCC is now cited more often in these sections.
35003	42	4	42	4	Please add links to relevant Chp 9 and Chp 5 sections here as well. [Baylor Fox-Kemper, United States of America]	Accepted, links added as requested.
35641	42	19	42	19	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account. This paper has now been accepted for publication.
19779	42	24	42	31	Over continental surfaces, improvements are expected from higher resolution due to both a more accurate description of the surface boundary condition (land cover, topography) and more refined description of subgrid phenomena. In the ocean case, only the latter cause operates, yet it appears to show efficiency. [philippe waldteufel, France]	Rejected. Increases in horizontal resolution should also lead to the improved representation of ocean bathymetry which is import for bottom currents and friction.
35005	42	24	42	31	I think these remarks on resolution, and especially the assessment of whether these models' resolution improvments represent a process improvement, should be left to Chp 9, section 9.2.2 and 9.2.4, with just a reference to that section here. [Baylor Fox-Kemper, United States of America]	Rejected. This chapter reports some improvements due to higher-resolution, so this text is required. However, we do remove some specific details and add pointers to relevant sections.
96277	42	25	28	42	We suggest to add Juricke et al., 2019 in Ocean Modelling. They showed that a new kinetic energy backscatter parameterization could achieve the same results without having to go to higher resolution. [Nicole Wilke, Germany]	Rejected. While this is a very interesting study, we are assessing existing literature, and we can't base an assessment on one study.
6615	42	25	42	31	The terms "eddy permitting" and "eddy resolving" are used here, and the horizontal scale assigned to each is essential the same (0.1º and 10km). I had thought eddy-permitting resolution was coarser than eddy-resolving resolution. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Consistent terminology is now used.
35643	42	31	42	31	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Rejected. This paper has now been published.
112665	42	31	42	31	simulations. However, inter-model differences remain [Melissa Jiménez Gómez Tagle, Germany]	Taken into account. Suggested text edit has been considered.
12019	42	33	43	4	This paragraph does not fit to this chapter. More appropriate to the observation chapter (Chap 3), or the ocean chapter (Chap 9). [Masaki Satoh, Japan]	Taken into account. This comment has been considered when revising the text of this paragraph, which has now been shortened.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102871	42	33	43	4	Is the discussion on the question of how representative point measurements are really needed ? And that certain phenomena are present at higher spatial and temporal scales? True for the atmosphere too, and understood. [Philippe Tulkens, Belgium]	This comment has been considered when revising the text of this paragraph, which has now been shortened.
6617	42	37	42	43	SST analyses are also produced using in situ data from buoys and ships, as well as (or instead of) satellite data. Analyses are also provided at higher temporal and spatial resolution than indicated here, and are used in reanalysis as well as numerical weather prediction. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This comment has been considered when revising the text of this paragraph.
96279	42	38	42	40	We suggest to also add the remote sensing SSH (sea surface height) measurements here, they are relevant for the sea level topic. [Nicole Wilke, Germany]	Accepted. Text changed as suggested.
12017	42	39	42	39	What this means is unclear. Please clarify "achieving global measurement coverage over a 7-day or similar time period." [Masaki Satoh, Japan]	Accepted. This has now been reworded.
2611	42	42			"0" should be replaced by whatever the minimum model upper layer is. [Bryan Weare, United States of America]	Rejected. However we have reworded this bracketed text to show that the measurements displayed are the upper and lower levels of the models upper layer.
19781	42	45	42	53	Same as the resolution issue, this passage suggests to look around for similar situations in the climate system. Prior to the satellite era, this was probably the general case, including radiosondes, rain gauge networks, soil moisture). So this is again an issue of general interest, which might conceivably deserve a couple of paragraphs in a technical annex [philippe waldteufel, France]	Taken into account. This comment has been considered when revising the text of this paragraph.
96281	42	47	42	48	We suggest to add a statement that by combining remote sensing SSH measurements with Argo data, climatologies with a higher temporal (daily) and spatial (0.25degs) are available for certain ocean regions (for instance Stendardo et al., 2016, "A high resolution salinity time series 1993-2012 in the North Atlantic from Argo and altimeter data" J.Geophys. Res Ocean, doi: 10.1002/2015JC011439). [Nicole Wilke, Germany]	Taken into account. This comment has been considered when revising the text of this paragraph.
127319	42	50	42	55	The Argo sampling density is arguably much higher than atmospheric soundings. And, while it is true that direct measurements of ocean circulation are sparse, that is even more true for the atmosphere. These sections should address the real issues of mapping the ocean over different time scales and decades, the important circulation components to observed, etc. [Trigg Talley, United States of America]	Taken into account. This comment has been considered when revising the text of this paragraph.
71893	42	50		55	This is an interesting (and naïve) criticism. The Argo sampling density is arguably much higher than atmospheric soundings. And while it is true that direct measurements of ocean circulation are sparse, that is even more true for the atmosphere. These sections should address the real issues of mapping the ocean over different time scales and decades, the important circulation components to observed, etc. [John Church, Australia]	Taken into account. This comment has been considered when revising the text of this paragraph.
96283	42	54	42	54	Ocean circulation is also inferred from transient tracer measurements. Rhein et al. 2015 showed the importance of the deep water boundary currents for the propagation of climate signals in the deep ocean - this was challenged by publications - and provided time scales and main spreading pathways. We suggest to add this information here. Rhein et al., 2015 J. Geophys. Res.Ocean "Advection of North Atlantic Deep water ..." [Nicole Wilke, Germany]	Taken into account. This comment has been considered when revising the text of this paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96285	42	55	42	55	Please here add some recent literature to direct observations of circulation, suggestions: Smeed et al., 2018 Gephys. Res.Lett. 45, doi: 10.1002/2017 GL076350; Rhein, M., et al., (2019), Observed transport decline at 47°N, western Atlantic. J. Geophys. Res. Oceans, doi:10.1029/2019JC014993; Fraijka-Williams et al., (2019). Atlantic Meridional Overturning Circulation: Observed transports and variability. Frontiers in Marine Science, 6:260, doi:10.3389/fmars.2019.00260.; McCarthy et al., (2020), Sustainable observations of the AMOC: Methodology and Technology. Rev. Geophys. 58, e2019RG000654, doi:10.1029/2019RG000654. The former two papers summarize recent findings from measurements at 26N and at 47N. The latter two papers are reviews encompassing many time series of ocean circulation. [Nicole Wilke, Germany]	Accepted.
96287	42	55	42	55	We suggest to add direct LONG-TERM measurements of ocean circulation. [Nicole Wilke, Germany]	Accepted. Wording added as suggested.
98341	43	18	46	6	This evaluation does not account for the role of internal climate variability when discussing climate model biases in SSTs. The following recent reference shows large-scale consistency between observed and simulated trends in SST patterns, which should be discussed here: Olonscheck, D., M. Rugenstein, and J. Marotzke (2020), "Broad consistency between observed and simulated trends in sea surface temperature patterns", Geophysical Research Letters 47, 1-10, doi:10.1029/2019GL086773 [Dirk Olonscheck, Germany]	Taken into account. The assessment of the internal variability is discussed in Section 3.3.1.1. The recommended reference has been added.
19783	43	20	43	26	Maybe this paragraph is not essential [philippe waldteufel, France]	Rejected. The paragraph serves as a guideline on what to expect in the subsection. It is therefore kept for a smooth flow of the text.
19787	43	31	43	41	Any unprejudiced reader will be stricken by the similarity between figure 3.21a and figures 3.10a,b. It is suggested that this similarity is mentioned and whenever possible commented. [philippe waldteufel, France]	Rejected. Figures 10a,b are precipitation maps and do not show any similarities with Figure 3.21a
19785	43	34	43	34	Quote Figure 3.21a rather than 3.21 [philippe waldteufel, France]	Accepted. We now referer to panel a of this figure.
2615	43	34	43	41	Few of these conclusions are evident from the current Fig. 3.24 [Bryan Weare, United States of America]	Noted. The text and figures updated.
21503	43	43	44	5	It feels very odd to have an entire assessment paragraph performing a substantive analysis without a single reference. This is potentially opening the assessment up to unnecessary attack. The assessment should be a synthesis of available literature not substantively new data analysis. I would suggest revisiting this paragraph and making sure that this analysis is not explicitly over-stepping the mark and undertaking a new substantive piece of original research which would be contrary to the given role and remit of IPCC. [Peter Thorne, Ireland]	Taken into account. New references are assessed and added.
35031	43	49	43	55	Please add a link to water mass discussion in section 9.2.2.3 alongside Fig. 3.22 references [Baylor Fox-Kemper, United States of America]	Taken into account. Text revised as suggested.
105001	43	52	43	53	Drift can be a first order issue here. Make sure you convince yourselves simulation by simulation you are treating it properly. [Peter Gleckler, United States of America]	Noted.
105003	43	54	43	54	"more effective analysis" is confusing. How about something like "Differences in distinct ocean basins are shown in …." [Peter Gleckler, United States of America]	Taken into account. Text revised
116225	43		43		It would be good to better explain the implications of biases identified for ocean temperature aspects, and link them to other sections of this chapter. Is there a specific assessment for model data comparisons for global SST trends, LSAT trends? [Valerie Masson-Delmotte, France]	Taken into account. Attribution assessment for LSAT now added. SST trends now linked to tropospheric temperature biases.
116535	43		43		It is striking that dimming / brigthening is addressed in ch 2, 7, 6, but not in ch 3 (attribution). Please check. [Valerie Masson-Delmotte, France]	Noted. Surface SW is not one of the large-scale indicators listed in Cross-Chapter Box 2.2, Table 1 as the focus of Chapters 2,3,4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105005	44	7	44	9	Poorly written - please revise [Peter Gleckler, United States of America]	Taken into account. Text revised
105007	44	11	44	13	This line is very confusing [Peter Gleckler, United States of America]	Noted. Text revised with more clarification.
112667	44	13	44	13	However, the overall spread is larger than that of the CMIP6 models [Melissa Jiménez Gómez Tagle, Germany]	Noted. Text revised.
35007	44	13	44	15	This comment on resolution alone misses the important counterpoint of parameterizations, and I would suggest that there is not a sufficient room for process-level treatment with sufficient detail. [Baylor Fox-Kemper, United States of America]	Agreed. Text revised in order to briefly mention the importance of parameterizations along with increased resolution.
42691	44	13			'however the overall spread is larger than that of the CMIP6 models' – can some explanation be offered as to why this is the case? We might have anticipated that the higher resolution (more realistic representation of processes) models would show a greater convergence in the results. This could depend on whether the std is calculated using the individual model ensemble mean or it includes all ensemble members (the spread of ensemble members for any given model may well be larger with high resolution). [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Discussion on larger spread in CMIP6 has been removed as it is not valid any longer by including the same numbers of ensemble and models in the assessment. Please refer to Figure 3.24.
98669	44	14	44	14	Delete "in" after "increases" [Sonya Legg, United States of America]	Taken into account. This text has been re-phrased.
111053	44	15		17	it would be good to know what aspect of variabiilty and change is affected by improved physical processes in high res. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
13355	44	24	44	36	verify date of the world ocean atlas used in the figures and cite corectly in figure 3.21 [Maria Amparo Martinez Arroyo, Mexico]	Noted.
19789	44	54	45	4	Why is the standard ZJ unit not used in figure 3.23, same as otherwise in the report? This seems technically easy. But maybe there is a reason. [philippe waldteufel, France]	Noted. ZJ is used in the revised figures
37895	44	54			Need to add or change to CMIP6 results in Figure 3.23 [Junhee Lee, Republic of Korea]	Taken into account. Figure 3.23 and the corresponding text are updated to CMIP6
112669	44	68	44	68	Typo on "characteristics" (says "charactOristics") [Melissa Jiménez Gómez Tagle, Germany]	Editorial. Revised as indicated.
11303	45	7	46	6	In 3.5.2.2 you have attributed the past change in surface salinity pattern. Then why don't you make a similar assessment for SST in this subsection? Assessing zonal mean and equatorial profiles would not be sufficient given a large impact of the SST pattern change on changes in the atmospheric state (e.g., tropospheric temperature in 3.3.1.2). [Masahiro Watanabe, Japan]	Rejected. An assessment for SST is included in the surface temperature section (3.3.1.1). Equatorial profiles are particularly assessed here due to the importance of air-sea coupling in these areas.
42693	45	16			'not statistically significant …. entire multi-model ensemble' – I'm not sure how to interpret this statement. It only tells us something about the multi-model mean, which is a very broad-brush measure of model development. Some models may well have an improved cold-tongue simulation and, that this can be achieved is important. So I have some caution is giving too much emphasis to the multi-model mean when looking at regionally specific phenomenon. As written, there is a danger of downplaying the progress that has been made with some models. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The assessment is mainly focused on ensemble mean. Significant improvement in individual models is assessed if available in the literature.
35645	45	17	45	17	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
12021	45	18	45	18	"coincident" is correct? Is it not "consistent"? [Masaki Satoh, Japan]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35647	45	18	45	19	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
35649	45	25	45	25	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
35651	45	37	45	37	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
35653	45	40	45	40	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
105009	45	44	45	47	Poorly written - please revise [Peter Gleckler, United States of America]	Noted. Text revised.
35655	45	51	45	51	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
42695	45	53			The last sentence in this para. But it has just been stated that the high-res bias is not significantly different to low-res? The previous comment may also apply here. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The last sentence particularly addresses the zonal mean SST. Text revised for more clarification.
105011	46	1	45	2	Suggest "In summary, for the purposes 1 of evaluating basin-scale properties, the CMIP5 and CMIP6 models"' -> "In summary, the consistency between the observed and simulated basin-scale ocean properties as simulated by CMIP5 and CMIP6 suggest that the CMIP5 and CMIP6 models are suffcient tools for…." [Peter Gleckler, United States of America]	Taken into account. Text revised accordingly.
7239	46	1	46	2	It is suggested to clearly explain the advantage and limitations of each model (the CMIP5 and CMIP6) to investigate ocean temperature and OHC responses. [Asaad Irawan, Indonesia]	Rejected. Improvement from CMIP5 to CMIP6 are already assessed in this section.
12023	46	1	46	2	It is not clear that this coclusion was derived from in the text: "the CMIP5 and CMIP6 models are appropriate tools for investigating ocean temperature and OHC responses to forcing". What is the evidence to say "appropriate tools"? [Masaki Satoh, Japan]	Noted. This is based on the evaluation subsections in 3.5.2.1 and 3.5.2.2. The models reasonably represent the observed signals.
67841	46	1	46	2	It is suggested to explain the advantage and limitations of each model (the CMIP5 and CMIP6) in the investigation of ocean temperature and OHC responses. [Ruandha Agung Sugardiman, Indonesia]	Rejected. Improvement from CMIP5 to CMIP6 are already assessed in this section.
42697	46	1			As noted in last two comments, there is an unstated assumption in some the text in this section that particular regional features cannot be said to have improved unless there is a statistically significant change in the multi-model mean. This is a highly restrictive criteria and has a rather dubious physical and statistical basis (e.g. what assumption is made regarding the statistical distribution in conducting the significance test?). [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Agreed, improvement from increasing horizontal resolution in a particular area is model dependant (Section 3.8.2.2). Here the assessment mainly focuses on the ensemble mean and spread rather than individual models. Text revised and clarified.
13357	46	2	46	2	OHC must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Rejected. Ocean Heat Content (OHC) is already used in Page 5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12027	46	23	48	3	There is no figure in this subsection 3.5.1.3 to show attributions on OHP, such as comparison with the anthropogenic forcing and the natural variability, and separation of individual effects of greenhouse gases and others effects. Only Fig. 3.25B shows the total OHP trend is somehow reproduced by the models for the period since 2005. [Masaki Satoh, Japan]	Taken into account. The suggested figure is shown in 3.40 in addition to an equivalent figure on thermosteric sea level change (Figure 3.28, 3.29). The text is therefore updated by pointing to these two figures, rather than doing a separate figure for OHC in order to avoid redundancy.
83083	46	25	46	32	I have suggested that Chapter 2 summarises the breakdown of OHC change by layer, since Chapter 7 has moved to reporting these % in terms of the overall heat inventory. We will need to cross check the numbers across Ch2, Ch3, Ch7 and Ch9. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text updated.
12531	46	25	47	56	At the start of chapter, it is stated that this chapter is to (1) "assess the extent to which human influence on clumate system has affected its evolution" and (2) "to what extend climate models are able to simulate observed change". This section including Fig.3.23 and 3.25 are all about the second question (to some extend, overlap with chapter-2). The first question is currently not well developed for ocean temperature/OHC change. My recommendation is (1) merge Fig.3.23 and 3.25, generate a clear/simple figure for salinity change. (2) provide a new figure contrasting CMIP6 simulations of anthropogenic forcing vs natural forcing, which addresses the first question. [Lijing Cheng, China]	Taken into account. Recommendation (1) accepted; Figures 3.23 and 3.25 are now merged (please refer to Figure 3.26). Recommendation (2): The suggested figure is shown in 3.40 in addition to an equivalent figure on thermosteric sea level change (Figure 3.29). The text is therefore updated by pointing to these two figures, rather than doing a separate figure for OHC in order to avoid redundancy.
37485	46	27	46	29	Make clear what you mean by 64%, 27% and 9% (although given such precise values it looks like they came from a single paper). [John McLean, Australia]	Noted.
83085	46	34	46	37	I think a stronger statement could be made here that links to the Ch7 assessment of Earth's energy budget (noting that the ocean is the majority shareholder in Total Earth System Warming). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised accordingly.
31527	46	39	46	51	Line 39-51: this paragraph seems to be chapter 2 matters rather than chapter 3. I think it could be removed from here as it is redundant with chap 2 and chap 9 text (ie. there would be no gap by removing it), and is not validation/attribution. The summary of chap 2 above is enough to go on with attribution I find. [Jean-Baptiste SALLEE, France]	Taken into account. Paragraph removed to avoid overlap.
104937	46	45	46	47	"recent independent study"? What is this? Is it referring to the Zika et al. (submitted) referenced in the next sentence? Why "independent"? [Peter Gleckler, United States of America]	Paragraph removed to avoid overlap with other chapters
35657	46	49	46	49	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
127321	46	50	46	50	Add Roemmich et al. (2015) and Wijffels et al. (2016). [Trigg Talley, United States of America]	Noted. However, paragraph has been removed to avoid overlap with other chapters
19791	46	50	46	51	The report appears a bit overreactive on this issue. Ending the sentence after the (Liu et al) reference is recommended. CCB3.1 tellscalmly all there is to say. [philippe waldteufel, France]	Noted. However, paragraph has been removed to avoid overlap with other chapters
71895	46	50			Add Roemmich et al. 2015 and Wijffels et al. 2016. [John Church, Australia]	Noted. However, paragraph has been removed to avoid overlap with other chapters
93475	46	51	46	51	Change "pause" to "slowdown" which is widely recognized as a more accurate term and is used in the cross-chapter Box 3.2. [David Clarke, Canada]	Noted. However, paragraph has been removed to avoid overlap with other chapters
35659	46	55	46	56	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83081	46				Section 3.5.1.3. Please include in your assessment the work of Weller et al (2016) who performed an multi-model detection and attribution study of ocean warming in an isothermal framework. https://www.nature.com/articles/nclimate1461 [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Weller et al., (2016) is now included in the assessment.
83087	46				Section 3.5.1.3. Please include the recent work of Rathore et al (2020) on the recent observed hemispheric asymmetry in ocean warming in your assessment of the attribution literature. https://www.nature.com/articles/s41467-020-15754-3 [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rathore et al., (2020) is now included in the assessment.
105013	47	9	47	9	It would be useful to indicate why th SO uptake is strongest or provide cross-chapter reference. [Peter Gleckler, United States of America]	Accepted. Text revised accordingly.
41883	47	9	47	10	I think it would be worth adding a reference here to the published work Garry et al. 2019 which also confirms the sentence (specifically see Figure 3 at https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JC014225). The full reference is: Garry FK, McDonagh EL, Blaker AT, Roberts CD, Desbruyeres DG, Frajka-Williams E, King BA (2019) Model derived uncertainties in deep ocean temperature trends between 1990-2010. Journal of Geophysical Research: Oceans. 124, 1155–1169. https://doi.org/10.1029/2018JC014225. The submitted work referenced (Garry et al. submitted) is still under review. [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Garry et al. 2019 is now assessed in the text.
31513	47	10	47	12	I would remove this last sentence. The next paragraph is clearer and redundant. [Jean-Baptiste SALLEE, France]	Taken into account. The last sentence has been removed with the text being revised
21505	47	10	47	20	Not quite clear what an apparently purely observational set of statements is doing in chapter 3 but it either needs redrafting to make clear it is not an observational finding or this text needs to be removed. This is significant overeach and overlap with assessments in chapters 2 and 9 otherwise. [Peter Thorne, Ireland]	Taken into account. Text revised
105015	47	15	47	15	"model-based historical period" -> "industrial era" [Peter Gleckler, United States of America]	Taken into account. The text has been revised accordingly.
35009	47	16	47	16	I'm skeptical that these approaches, based on a combination of a lot of model and limited observations, are sufficiently model-free to evaluate model performance at this level. A more measured description is warranted. [Baylor Fox-Kemper, United States of America]	Accepted. Text revised for more clarification. Although some of the estimates are not model-free, other reconstructions that do not depend on models and model output do show an agreement.
12025	47	19	47	19	"the deep ocean OHC (below 2000 m) has increased since 1992": This statement is not consistent with Fig. 3.25, which shows persistent increase of the deep ocean OHP since (say) 1960. [Masaki Satoh, Japan]	Taken into account. Text revised based on updated figures.
104939	47	25	47	26	Pierce et al (2012) should probably be cited here [Peter Gleckler, United States of America]	Taken into account. Suggested reference added
105017	47	29	47	29	Don't erosols tend to reduce OHU? [Peter Gleckler, United States of America]	Taken into account. Agreed, however the sentence does not refer to “aerosols warm the ocean” but to “the importance of different forcings to ocean warming which can be negative or positive. Text has been clarified
111055	47	30			to greenhouse gases alone' no its been attributed to a combination of forcings; with greenhouse gas influences detectible… or some other rephrase ;) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised accordingly.
104941	47	37	47	38	In summary, there is strong evidence and understanding on the increase in global OHC. ->. In summary, there is strong evidence and improved understanding (since AR5) in the global OHC increase. [Peter Gleckler, United States of America]	Taken into account. Text revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19793	47	45	47	52	For the time being, every symbol for volcanic eruptions seems to have the same size. [philippe waldteufel, France]	Taken into account. The figure has been merged with Figure 3.26 and the volcanic eruptions are no longer shown.
13359	47	47	47	47	Reference is different style [Maria Amparo Martinez Arroyo, Mexico]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
26069	47	52	47	52	It could be helpful to define volcanic explosivity index (or at list some reference) [Don Alfonso Pino Maeso, Spain]	Noted.
102873	48	8	48	9	delete "due to poor measurement coverage of other oceanic variables" - not the focus here. Replace the word "improving" with "improved" & "has" by "have. [Philippe Tulkens, Belgium]	Accepted. Text changed as suggested.
98671	48	13	48	18	The increase in density contrast at the base of the mixed layer (from Sallee et al, submitted) is being compared with the earlier estimates of increased stratification (from Bindoff). Firstly, it is not explained whether a X% increase in this density contrast is equivalent to an X% increase in upper ocean stratification. Secondly these values for the density contrast increase (7.4-13.9% per decade) seem very large. Over the 50 year period for which the Bindoff estimate was made, that would correspond to about a 100% increase. This is not one, but 2 orders of magnitude larger than the Bindoff estimate. Something must be incorrectly calculated here.... [Sonya Legg, United States of America]	Taken into account. We have updated text to reflect the published manuscript. Their new analysis undertakes physically-focused assessment, following the mixed layer in-situ, rather than averaging 0-200 m across the global ocean. For this reason, a far larger change has been recorded. Numbers reproduced in the revised text reflect the results discussed in the final paper
31517	48	13	48	20	This is all obs&processes: should be moved to Chap 2/9? [Jean-Baptiste SALLEE, France]	Accepted. We have liaised with CH2 and 9 and revised the text accordingly.
2619	48	13			define SROCC [Bryan Weare, United States of America]	Accepted. Text changed as suggested.
12535	48	14	48	19	How well do CMIP6 models simulate stratification? Can a D&A be applied to stratification? [Lijing Cheng, China]	Noted. Detailed assessment of stratification is included in Chapter 9.
12537	48	14	48	19	The SROCC stratification assessment is not comparable with Sallee et al. They are different things: SROCC/AR5 calculated temperature difference between surface and 200m; Sallee et al/chapter-9 used pynocline changes. The later can not represent "ocean stratification change", instead, it represents "local stratification change" or "stratification change at the base of mixed layer" [Lijing Cheng, China]	Noted. Agreed that the analyses are focused on different aspects of ocean change, however, they both represent "stratification" change. The previous SROCC/AR5 focused on the binned 0-200 m changes, which is oblivious to the changing structure and depth of MLD across ocean realms. The Sallee et al analysis targets this using a feature-following analysis, which reports greater signal-to-noise, and larger absolute % changes
12029	48	16	48	17	What exactly indicates "summertime density contrast"? What are the values 7.4% to 13.9%? [Masaki Satoh, Japan]	Noted. These numbers and text have been revised to reflect the published manuscript. The numbers represent percent changes with reference to the climatological mean
13411	48	23	48	24	It's recommended to mention how evaporation-precipitation patterns have been modified indifferent oceanic regions. [Maria Amparo Martinez Arroyo, Mexico]	Noted. Descriptive text discussing basin-scale changes are available in section 3.5.2.2
35011	48	24	48	24	Add section 9.2.2 please [Baylor Fox-Kemper, United States of America]	Accepted.
28793	48	24			Also Section 8.2.2.1 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
15481	48	29	48	29	Please replace "tropical cyclones and hurricanes" by "tropical cyclones". [SAI MING LEE, China]	Rejected. We believe that both words are required.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19795	48	30	48	33	This interesting remark brings to mind the fact that a reader of this report learns only in a very patchy way about the functional relationships which organize the climate system and base ESM. It is not clear that this can be corrected. In the specific case here, the striking fact is that, according to the next subsections, simulations are able to match major features of the salinity distribution and changes, and getting better. Certainly, significant other constraints are at work. [philippe waldteufel, France]	Noted. The text was revised with a number of new citations
12539	48	35	48	42	This paragraph seems irrelevent, suggest remove [Lijing Cheng, China]	Taken into account. We have liaised with CH9 and revised the text accordingly.
31525	48	35	48	43	All processes: should be moved to Chap 9? Or maybe take Chap 9 assessment and explain how that increased process understanding help validation/attribution assessment? [Jean-Baptiste SALLEE, France]	Taken into account. We have liaised with CH9 and revised the text accordingly.
21507	49	3	49	28	This is another paragraph where almost all of the assessment is of the analysis performed in the chapter with one single reference to the literature being two works by a single lead author. Given the need to perform a synthesis and assessment and not perform substantive new and novel analysis this paragraph arguably risks crossing the bound and being a new substantive piece of research. Due consideration needs to be given to this. [Peter Thorne, Ireland]	Noted. No such analysis was available for this assessment
102875	49	7	49	28	Here could be an opportunity to cut text, if space needs to be saved. This text does not seem to add much value to the summary conclusions. [Philippe Tulkens, Belgium]	Accepted. Text revised as suggested
35013	49	23	49	26	This is too vague, a list of biases that are reproduced would be preferable, as surely they are not all the same--i.e., "structure" can be made much stronger. [Baylor Fox-Kemper, United States of America]	Accepted. Revised figures and supporting text are now included
12031	49	31	49	32	It is not clear whether the salinity biases of CMIP6 models shown in this section (Fig. 3.21, 3.22) are sufficiently small or not relevant to argue the confidence level of the utilty of the models for detection and attribution of ocean salinity studies. [Masaki Satoh, Japan]	Noted. CMIP5 models have successfully been used in salinity detection and attribution studies. For CMIP6 the structure (and magnitude) of biases has not changed substantially, and at basin-scales have utility just like their CMIP5 predecessors
12523	49	37	50	24	Please consider to use Salinity-contract metric invented in AR5 and further developed in Cheng et al. 2020 paper for D&A here. This metric provide a simple, identifiable and effective metric for salinity change and attribution. [Lijing Cheng, China]	Noted. The salinity-contrast methodology is dependent on a time history of salinity change/anomalies. The observational analysis presented in Ch2 did not reproduce this metric, and we have reused their observational analysis to contrast to model results here
12533	49	37	50	56	At the start of chapter, it is stated that this chapter is to (1) "assess the extent to which human influence on clumate system has affected its evolution" and (2) "to what extend climate models are able to simulate observed change". This section includes both Fig.3.26 and 3.27 are about the second question.The first question is currently not well developed for salinity change. My recommendation: (1) Merge Fig.3.26 and 3.27, to address the second question. (2) provide a new figure contrasting CMIP6 simulations of anthropogenic forcing vs natural forcing, which address the first question. [Lijing Cheng, China]	Noted. We have considerably revised figures and text to more effectively assess the available literature in the context of the new CMIP6 simulations.
31531	49	48	49	50	I am unsure why this paragraph is useful for Attribution. If it is the link to E-P, then it could probably be a shorter summary linking to Chap 9, and leaving those « mechanistic » aspect to Chap 9. But I see that the link with E-P is not used later for attribution. Don’t want to move all I see to Chap 9 :) (I will be told off by my CLA, we are already at our page limit) but I thought it was not helping the flow here [Jean-Baptiste SALLEE, France]	Noted. We have considerably revised figures and text to more effectively assess the available literature in context of the CMIP6 simulations

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52907	49	48			Fig. 3.26: Also show the simulated ensemble mean climatology and update with both CMIP5 and CMIP6 models? [Hervé Douville, France]	Noted. We have considerably revised figures and text.
12519	50	6	50	9	This pucture has been updated since AR5, besides of Pacific freshening and Atlantic salinification, it is also about North Atlantic freshening (>40N), Indian Ocean dipole structure (freshening in the east and salting in the northeast). [Lijing Cheng, China]	Rejected. The chapter is focused on the basin-scale changes, with Ch9 addressing processes and features that a sub-basin-scale.
40307	50	9	50	9	For some reason, I found 36 examples of ":)." throughout this chapter. [TSU WGI, France]	Editorial. This has been corrected in the final draft.
31515	50	11	50	14	This is all obs: should be moved to Chap 2? [Jean-Baptiste SALLEE, France]	Noted. This result is relevant to detection and attribution. Due to data sparsity, even the longest available multi-decadal time history shows agreement between observations and models
11305	50	16	50	24	I'm curious how much the human influence is disernible in the P-E trend. Please discuss it or at least cite an appripriate section in Chapter 8. [Masahiro Watanabe, Japan]	Noted. In the section opening text (Section 3.5.2) we reference Sections 8.2.2.1 and 9.2.2 where linkages are discussed
52909	50	18			What about the scaling factor? Is the forced response simulated with the correct magnitude? Note that this comment applies to all successful formal attribution studies quoted in this chapter: showing that the scaling factor cannot be zero does not tell the whole story and, in many circumstances, it is also relevant to specify whether the models tend to overestimate or underestimate the attributable observed changes. A Table highlighting all successful formal attribution studies with columns for variable, domain, season, period, attributed effect (ANT, GHG, AER, NAT) that however underestimate the observed response could be useful. [Hervé Douville, France]	Noted. The chapter is focused on basin-scales, with this section focused on ocean salinity. While a table documenting multi-variable attribution would be useful, it is out of scope for this section (and chapter basin-scale focus)
31519	50	19	50	21	Inconsistent with Chap 2 : Chap 2 says « there is a strong link (high confidence) ». But they continue: « It is unclear, however, if the reported increasing rates of salinity at the ocean surface change since the 1980s or 1990s are realistic in reflecting an enhancement of the global hydrological cycle or whether they potentially result from a change in sampling methodology (Skliris et al., 2014; Grist et al., 2016; Aretxabaleta et al., 2017), especially after the 2000s (Durack, 2015). » I m unsure if it is Chap 2 or Chap 3 mandate for assessment, but surely there should be consistency here. I would recommend to talk to Chap 2 says it is part of the attribution assessment and actually uses numerical simulations. So they take your assessment and refer to your chapter. [Jean-Baptiste SALLEE, France]	Noted. Further coordination with Ch2 has led to considerable revision of text across chapters. These are now in agreement.
21511	50	24	50	24	Through to future projections part: i) is not supported by the preceding text; ii) is over-reach into chapter 4 and/or 9 territory. Suggest removal [Peter Thorne, Ireland]	Noted. The consistency between the observed patterns and the patterns that continue to magnify under future climate change is indeed relevant information
12521	50	41	50	54	1950-2000 is out of date, should update ideally to 2018. [Lijing Cheng, China]	Accepted. The analysis presented in Ch2 is now reproduced alongside CMIP6 analysis in the chapter. This revised observational analysis extends from 1950 to 2019
21513	51	3	51	3	Is this cross-chapter box 9.2 in this report (which would make sense) or are you referring to a box in AR5? This is unclear as presently drafted. [Peter Thorne, Ireland]	Accepted. The reference to box 9.2 has been removed from the sentence to avoid confusion.
7241	51	3	51	7	To have a comprehensive background, it is suggested to explore literature not only to the global factor that caused a rising sea level (e.g., thermal expansion and melting of land-based ice) but also to the local factors (i.e., a slowing Gulf Stream and sinking land) [Asaad Irawan, Indonesia]	Rejected. Out of scope for this chapter. Text has been added to guide readers looking for this to find the relevant section in chapter 9. (section 9.6)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67843	51	3	51	7	To give a comprehensive picture, it is suggested to explore literature not only on the global factors that cause rising sea level (e.g., thermal expansion and melting of land-based ice), but also local factors (i.e. slowing Gulf Stream and land subsidence) [Ruandha Agung Sugardiman, Indonesia]	Rejected. Out of scope for this chapter. Text has been added to guide readers looking for this to find the relevant section in chapter 9. (section 9.6)
37407	51	10	51	37	This is not an evaluation of sea level. It is just purports to be an evaluation of the ability of models to estimate sea level. [John McLean, Australia]	Noted. The text has been added.
127323	51	10	51	37	Crosslink to evaluation of sea-level models in Chapter 9. [Trigg Talley, United States of America]	Accepted. Cross references have been inserted.
127325	51	12	51	14	This is an incomplete and misleading statement. There were model evaluations on glaciers although they were not part of CMIP (there is more to the world than CMIP). Historical ice sheet and land water (particularly antropogenic components: groundwater depletion, reservoir construction) were inadequate. [Trigg Talley, United States of America]	Taken into account. Text revised.
37401	51	12	51	21	Please state whether any of these models were validated. [John McLean, Australia]	Rejected. The underlying studies cited describe the model evaluation and validation steps.
71897	51	12		14	This is an incomplete and misleading statement. There were model evaluations on glaciers although they were not part of CMIP s (there is more to the world than CMIP). Historical ice sheet and land water (particularly antropogenic components - ground water depletion, resevoir construction) were inadequate. [John Church, Australia]	Taken into account. Text revised.
105021	51	14	51	14	The same is true for CMIP6 although as noted below although advanced have been made as noted below. [Peter Gleckler, United States of America]	Noted.
10873	51	17	51	18	Some modeling institutions "introduced" background volcanic forcing years ago! e.g., Gleckler et al, Krakatoa lives: The effect of volcanic eruptions on ocean heat content and thermal expansion, GRL, 2006. A number of CMIP5 models had background volcanic forcings in their piControl. How many CMIP6 models implemented the CMIP6 volcanic forcing recommendation? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised.
35015	51	17	51	20	This statement needs more support from the literature--I don't believe that is the only reason why the improvements have occurred. [Baylor Fox-Kemper, United States of America]	Taken into account. Text revised.
105019	51	18	51	18	include Gregory et al. reference that first proposed the background forcing. [Peter Gleckler, United States of America]	Taken into account. Text revised.
37403	51	23	51	23	What? The previous sentence says that SROCC has confidence in models and now you are saying that the models are incomplete. This doesn't make any sense. [John McLean, Australia]	Rejected. The previous sentence is referring to the confidence in simulated ocean thermal expansion, while this one is describing studies which also coinsider glacier and ice sheet melt contriubtions to sea level rise.
37405	51	27	51	27	State why a reanalysis should be preferred over an original analysis. [John McLean, Australia]	Rejected. Reanalyses are well accepted datasets for climate studies.
35017	51	31	51	31	Please add link to section 9.4 [Baylor Fox-Kemper, United States of America]	Accepted.
79077	51	31	51	31	additional cross reference for ice sheets is 9.4 [Aimee Slangen, Netherlands]	Accepted.
2621	51	31			define ISMIP6 [Bryan Weare, United States of America]	Rejected. This acronym has been defined in section 3.4.3
79079	51	35	51	37	need to mention here that this sentence is about glaciers: this could be done by replacing 'of the sea-level budget' by 'of the projected contribution from glaciers to sea-level change' - or something alike ; the additional cross reference for glaciers is 9.5. [Aimee Slangen, Netherlands]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107227	51	42	52	24	[pt 1 of 3] It says, "The SROCC concluded with high confidence that the dominant cause of global mean sea level rise since 1970 is anthropogenic forcing..." Since sea-level trends have not significantly changed since the 1920s, this is 100% nonsense. Trying to attribute the cause of something which doesn't exist is a fool's errand. The simple fact is that coastal sea-levels are rising no faster now, with CO2 at 410 ppmv and CH4 at 1.86 ppmv, than they were nine decades ago, with CO2 at 307 ppmv and CH4 at 1.03 ppmv. All those GHG emissions and all the consequent warming have caused no significant, detectable, sustained acceleration in the rate of sea-level rise. Refs: https://sealevel.info/1612340_Honolulu_Wismar_Stockholm_vs_CO2_annot3.png http://link.springer.com/article/10.1007%2Fs00382-013-1771-3 https://www.academia.edu/30694598/Tide_gauge_location_and_the_measurement_of_global_sea_level_rise?auto=download http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-12-00319.1 https://www.sciencedirect.com/science/article/pii/S0378383913000082 [cont'd] [David Burton, United States of America]	Rejected. Observed rates of sea level rise are assessed in Chapter 2, not in Chapter 3. Chapter 2 makes the following assessment 'Global mean sea level (GMSL) is rising, and the rate of GMSL rise since the 20th century is faster than over any preceding century in at least the last three millennia (high confidence). Since 1901, GMSL has risen by 0.20 [0.15–0.25] m, and the rate of rise is accelerating.'
107229	51	42	52	24	[pt 2 of 3] It continues, "... Marcos and Amores (2014) found the human influence on thermosteric sea level rise in the 0-700m global ocean to be 87%. Both thermosteric and regional dynamic patterns of sea level change in individual forcing experiments from CMIP5 were considered by Slangen et al. (2015)..." One of my pet peeves is people who call computer model runs or other calculations "experiments." THEY ARE NOT EXPERIMENTS! In the case of GCMs, they're not even decent quality calculations. The output of an unverified computer model is not even scientific evidence, it is, at best, speculation. A computer model, until it has been proven by verification against reality, is simply a complicated hypothesis. Since the CMIP models make projections which cannot be verified or disproven until decades after their code will be retired, they are effectively untestable hypothesis, which is the worst kind. When hypotheses are not testable, and thus are not subject to the risk of falsification, they are not a scientific hypothesis at all. Please extirpate this infection from the report! [cont'd] [David Burton, United States of America]	Rejected. The word 'experiments' is used in this context in the underlying literature assessed here.
107231	51	42	52	24	[pt 3 of 3] It continues, "Marzeion et al. (2014) found that between 1991 to 2010, the anthropogenic fraction of global glacier mass loss was 69 ±24%. Slangen et al. (2016) considered all quantifiable components of the global mean sea level budget and showed that anthropogenic forced changes account for 69 ± 31% during 1970 to 2005..." etc. etc. It disappoints me that anyone takes seriously these "studies" done solely with unverifiable computer models. Since Slangen is from CSIRO, I annotated a NOAA graph of sea-level at Australia's longest tide gauge, to illustrate the findings of her paper: https://sealevel.info/680-140_Sydney_2016-04_anthro_vs_natural.png Now, why do you suppose she didn't didn't include a graph like that in her paper? :-) Seriously, though, the models are NOT good enough for this sort of analysis, not even close. These sea-level attribution studies are junk science, which don't belong in the report. At the very least, if you include such drek you should note that the conclusions are unverifiable from actual evidence. ### [David Burton, United States of America]	Repeat of comment No. 115037 addressed previously.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115037	51	42	52	24	[pt 3 of 3] It continues, "Marzeion et al. (2014) found that between 1991 to 2010, the anthropogenic fraction of global glacier mass loss was 69 ±24%. Slangen et al. (2016) considered all quantifiable components of the global mean sea level budget and showed that anthropogenic forced changes account for 69 ± 31% during 1970 to 2005..." etc. etc. It disappoints me that anyone takes seriously these "studies" done solely with unverifiable computer models. Since Slangen is from CSIRO, I annotated a NOAA graph of sea-level at Australia's longest tide gauge, to illustrate the findings of her paper: https://sealevel.info/680-140_Sydney_2016-04_anthro_vs_natural.png Now, why do you suppose she didn't include a graph like that in her paper? :-) Seriously, though, the models are NOT good enough for this sort of analysis, not even close. These sea-level attribution studies are junk science, which don't belong in the report. At the very least, if you include such dreck you should note that the conclusions are unverifiable from actual evidence. ### [David Burton, United States of America]	Taken into account. In response to this and other comments we now include a more detailed discussion of the findings of Marzeion et al. (2014) in Section 3.4.3.1, including this statement "While Marzeion et al. (2014) found that anthropogenic influence contributed only 25 ± 35% of glacier mass loss for the period 1851-2010, their naturally-forced simulations exhibited a substantial negative mass balance, which Roe et al. (2020) argued is unrealistic." We have now inserted a reference to 3.4.3.1 here. Hence we do now introduce some caveats on the interpretation of Marzeion et al. (2014), in line with this review comment.
79081	51	46	51	46	the human contribution to SLR was also detected in glaciers (marzeion et al 2014), not just in thermosteric sea level [Aimee Slangen, Netherlands]	Accepted.
37409	51	48	52	19	The IPCC's so-called evidence for manmade climate change differs in every report. Previous "evidence" is not mentioned because it has been proven incorrect, sometimes even by IPCC reports. In the absence of any solid evidence for manmade climate change it is disingenuous to claim that anthorpogenic influences on sea level are substantial. [John McLean, Australia]	Rejected. Previous assessments have been cited appropriately and literature since then assessed.
105023	51	49	51	49	athropogentic forccings (combined greenhouse gas and aerosols) [Peter Gleckler, United States of America]	Taken into account. Editorial.
105025	51	52	51	52	humance influence to explain more than 85% of the 0-700m thermosteric global sea level rise [Peter Gleckler, United States of America]	Taken into account. Editorial.
116227	51		51		Duplications between sections on ice sheets and on sea level need to be fixed. [Valerie Masson-Delmotte, France]	Accepted. The overlaps have now been accounted for. The text has been fixed.
21515	52	5	52	19	This paragraph left me feeling very proverbially machine gunned by percentages. Is there a way to say the same thing without quite such a set of percentages being given in rapid fire? In particular the studies seem to be coming to broadly similar conclusions so extra synthesis may help here? [Peter Thorne, Ireland]	Accepted. This part has been reworded.
2631	52	12			define GMSL [Bryan Weare, United States of America]	Rejected. The GMSL acronym has been introduced at the beginning of 3.5.3
6619	52	15	52	15	The IPCC terminology "virtually certain" and "extremely likely" are used elsewhere in the report without specifying the probability levels they correspond to. Is there a particular reason to include "(P=0.99)" and "(P=0.95)" here? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This part has been reworded.
127327	52	16	52	19	[CONFIDENCE] The statement regarding Kopp et al. (2016) should be "extremely likely " that at least 41% of 20th century GMSL rise would not have occurred. (K16 provides 90% central ranges of either 49%-132% anthropogneic or 41% to 113% anthropogenic, depending on assumptions.) More broadly, there is a question of whether assessment language should be used to describe the results of individual studies, or only for the overall assessment. In which case, we would say that K16 said there is at least a 95% probaility that... [Trigg Talley, United States of America]	Accepted. Text revised.
127329	52	18	52	18	Give the magnitude of the observed AMOC for comparison. [Trigg Talley, United States of America]	Accepted
71901	52	18			Give the magnitude of the observed AMOC for comparison. [John Church, Australia]	Accepted
127331	52	21	52	24	Slangen et al. (2016) did include estimates of changes in groundwater and terrestrial storage. [Trigg Talley, United States of America]	Noted. Text has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71899	52	21		24	As I recall, Slangen et al. (2016) did include estimates of changes in groundwater and terrestrial storage. [John Church, Australia]	Accepted. Now taken into account.
127333	52	22	52	22	Local subsidence is not relevant to the attribution of GMSL change. Regional relative sea level change is addressed in Chapter 9. Chapter 3 is intended to address global metrics. [Trigg Talley, United States of America]	Taken into account. The reference to local subsidence has now been deleted.
21517	52	22	52	23	Direct groundwater abstraction is as unambiguously a human intervention as it is possible to conceive. Why is it being treated somehow as not an anthropogenically produced perturbation here? This seems very strange. It is the one part of the sea level change that is directly and unambiguously human in origin without any ambiguity beyond our ability to measure / estimate the quantities involved. [Peter Thorne, Ireland]	Accepted. Now taken into account.
35019	52	22	52	23	It would be better to link to section 9.6 rather than this partial list. [Baylor Fox-Kemper, United States of America]	Accepted. Text revised.
17605	52	23	52	24	A sea level rise analysis with starting point 1970 is not a fair balanced summary of the issue. Should also include the available tide gauge measurments of the last 100+ years. Then the conclusion is different because there is no accelleration in the sea level rise during the last 100 years. So the conclusion "since 1970 very likely human-caused" is misleading and not justified. Another example of groupthink and tunnel vision. [ferdinand meeus, Belgium]	Rejected. Out of scope for this chapter. Text has been added to guide readers looking for this to find the relevant section in chapter 9.
13361	52	32	52	32	Reference is different style [Maria Amparo Martinez Arroyo, Mexico]	Editorial, this kind of issue will be fixed prior to publication.
105115	52	49	52	49	I am missing a paleocontext here: a few sentences asserting abrupt changes in AMOC have been reconstructed and simulated. This is key to understand past abrupt climate changes [Masa KAGEYAMA, France]	An introductory sentence that provides past context of AMOC changes has now been added in the first paragraph of this section.
71907	52	49	54	41	There is no mention of the Recent Bryden et al (2019--20) paper on the impacts of the observed slowing of the AMOC. [John Church, Australia]	Accepted. This paper is now cited.
127335	52	49	54	41	There is no mention of the recent Bryden et al. (2019?) paper on the impacts of the observed slowing of the AMOC. [Trigg Talley, United States of America]	Accepted. This paper is now cited.
7797	53	3	53	4	This seems to contradict Chapter 2 which says a weakening during the 20th C is low confidence. Please discuss with them [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now directly reference the CH2 assessment.
2633	53	9	53	45	The tone of this is unnecessarily negative given the data available in Fig. 3.29, especially a) [Bryan Weare, United States of America]	Accepted. We have tried to improve the tone of text to be more positive.
13363	53	11	53	11	Missing or extra () [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
68067	53	24	53	45	As context, Lund et al (2006) reconstructed a weakening of Florida Straits flow in the Little Ice Age, (1400-1850), relative to modern, of about 2-3 Sv (+/-1 Sv), over the last 1100y BP. [MIchael Evans, United States of America]	Noted. Thanks for the paper reference. However, as discussed above, we have chosen to largely focus on the historical period. We rely on CH2 to present evidence of AMOC strength over the last millennium.
32899	53	26	53	28	should say "RAPID observations show that the overturning at 26˚N is 2.9 and 2.5 Sv weaker in the multi-year averages 2008-2012 and 2012-2017 relative to the 2004-2008 period, respectively" [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text changed as suggested.
32901	53	29	53	29	should say "weakening pf the AMOC over the 2012-2017 period realtive to 2004-2008" [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.
127337	53	30	53	45	It is argued that, because models do not simulate the observed change, they must be underestimating natural variability and that the observed change must be natural variability. While this may be true, evidence is not presented to support the conclusion. The argument has become circular. [Trigg Talley, United States of America]	Accepted. We have altered the text to better highlight the evidence available to support the argument presented.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71903	53	30		45	I do not understand the argument here. It is argued that because the models do not simulate the observed change they must be underestimating natural variability and that the observed chagne must be natural variability. While personally I might suspect this is true, evidence is not presented to support the conclusion. The argument is becoming circular. [John Church, Australia]	Accepted. We have altered the text to better highlight the evidence available to support the argument presented.
32903	53	32	53	32	should say "the observed weakening over 2004-2017" [Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.
7801	53	33	53	35	In the original paper the GFDL CMIP5 models did not underestimate the variability. I assume those are the dots outside the bars in the figure. Hence say most/majority/nearly all of CMIP5 models. [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.
2637	53	34	53	36	I see no way that Fig. 29:b,c shoud the models undestimate variability [Bryan Weare, United States of America]	Rejected. The observed AMOC 8yr trend is outside of the 99% confidence range for 84% (21/24) of the CMIP6 models presented, which strongly supports our assessment that the majority of models are underestimating AMOC variability.
81549	53	35	53	36	I do not agree with this statement that the interannual to decadal variability is underestimated in CMIP6 models. Figure 3.29 caption is saying that the results are based on using ensemble mean of AMOC for each model, and is not surprising that ensemble mean of the members would underestimate the variability. The range of variability for any model should be obtained by taking all the the individual members into account instead of using the ensemble mean. In fact, for EC-Earth model which is part of CMIP6, I find a reasonable range of variability. [Pasha Karami, Sweden]	Taken into account. There appears to have been a misunderstanding of what is presented in Figure 3.29, as the ensemble means are not presented. So we have now updated the figure to be more stand alone, and updated the caption to bettern explain what is presented.
35021	53	36	53	45	There is speculation here about causal processes that should not be assessed in chapter 3 and is inconsistent with process assessment in Section 9.2. [Baylor Fox-Kemper, United States of America]	Accepted. This section of text has been removed.
96289	53	47	53	47	Improve formulation please: what are instrument based reconstructions? Probably an error by transferring the sentence from Ch2. (reconstructions based on instrumental observations...)? [Nicole Wilke, Germany]	Accepted. The sentence has been modified to more clearly present the required information.
96291	53	47	53	47	It should please be mentioned that the assessment of this fact in 2.3.3.4 leaves doubt on the robustness of this finding. [Nicole Wilke, Germany]	Accepted. The sentence has been modified to more clearly present the findings of CH2.
127339	53	49	53	50	This needs rewording: "… multi model mean opposes the observations …"? [Trigg Talley, United States of America]	Accepted, this sentence has been reworded as suggested.
7799	53	49	54	8	There seems to be a lot of repetition - this could be said more concisely [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, this section of text has been revised to be more concise.
71905	53	49		50	This needs rewording - "multi model mean opposes the observations? [John Church, Australia]	Accepted, this section of text has been revised to be more concise.
35661	53	50	53	51	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted, this paper has now been published after peer review.
116231	53	52	54	20	Please be explicit on which external forcing is identified to affect AMOC. The assessment in this part of the chapter can be sharper and more concise. [Valerie Masson-Delmotte, France]	Accepted, the text has now been clarified so explicit external forcing have been details.
35663	53	53	53	53	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted, this paper has now been published after peer review.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116229	53		53		Please update the assessment of modelled AMOC during past periods from recent literature, including inconsistencies between estimates of freshwater fluxes linked to last deglaciation sea level rise, and the amount required to inject to coupled models to mimic reconstructions of abrupt changes (Liu et al PNAS 2012). Full simulations of the last deglaciation were not available at the time of AR5. To coordinate with ch 2 and ch 9. [Valerie Masson-Delmotte, France]	Noted. Here, we have focused the recent period, due to chapter constraints. But we note that a strong paleo perspective of past changes was offered in CH2, while modelling these past changes was covered in CH9.
35023	54	4	54	5	Please add link to section 9.2.3.1 [Baylor Fox-Kemper, United States of America]	Accepted. Added, as suggested.
35665	54	5	54	6	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted, this paper has now been published after peer review.
37359	54	16	54	18	Considering that the models haven't been validated it is dishonest to claim that there is "little evidence for a significant role of external forcing" in the NAO. Unvalidated models can't provide evidence for anything other than how sensitive the model is to certain inputs. [John McLean, Australia]	Rejected. The models have been validated here and in the numerous publications cited in this section.
7803	54	22	54	24	In the original paper the GFDL CMIP5 models did not underestimate the variability. I assume those are the dots outside the bars in the figure. Hence not outside the range [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, we have now updated the wording of this sentence to be more consistent with existing literature.
52911	54	22			replace "understand" by "assess" or "quantify"? [Hervé Douville, France]	Accepted, text changed as suggested.
6621	54	29	54	29	Why are reanlyses classed as "model simulations"? They default to model simulations if no observations are assimilated, but for fields from reanalyses to be taken seriously into consideration those fields should have been influenced by the assimilated observations. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the text has now been adjusted so it is clear
31523	54	30	54	31	It is maybe only my english, but I find the wording is confusing here. Should we say : In summary, there are hints from differing lines of evidence that AMOC decline has been caused by anthro forcing, but we still have low confidence in this attribution ». [Jean-Baptiste SALLEE, France]	Taken into account. We have reworded this concluding sentence so it should now be easier to read.
37693	54	36	54	51	It is hard to get convinced of the appropriateness of the results shown in panels b-f without time series. [Masahide Kimoto, Japan]	Noted. AMOC time series are presented in CH9.
35667	54	50	54	51	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted, this paper has now been published after peer review.
31511	55	8	55	13	This is all observation, ie chap 2 matter, so I would suggest to remove the assessment here and just report Chap 2/9 assessment : There is no evidence in ACC transport change, and it is unlikely that the mean meridional position of the ACC has moved southward in recent decades (Section 2.3.3.4.2 ; Section 9.2.3.2). The modelled strength…. [Jean-Baptiste SALLEE, France]	Accepted, this assessment, which was based off AR5 and SROCC, is now based off observations presented in CH2 and CH9.
12033	55	12	55	12	"that that" should be "that". [Masaki Satoh, Japan]	Accepted, however, this sentence has now been removed due comments of another reviewer.
35669	55	24	55	25	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted, this manuscript has now been published.
31521	55	27	55	29	This can be replaced by Chap 2/9 assessment (rather than SROCC assessment): Confidence in an intensification of the southern upper ocean overturning is low, and there is medium confidence for a slowdown of AABW circulation and commensurate AABW volume decrease since the 1990s (Section 2.3.3.4.1; Section 9.2.3.2). While the two-cell… [Jean-Baptiste SALLEE, France]	Accepted, this assessment has now been updated as suggested.
35025	55	35	55	35	Please add link to section 9.2.3.2 where CMIP6 updates are present [Baylor Fox-Kemper, United States of America]	Rejected, this sentence has now been replaced with a more up to date statement.
98673	55	39	55	43	Some of the CMIP6 ocean models are 1/4 degree (e.g. NOAA-GFDL CM4), without eddy parameterizations, so it is not true that Southern Ocean mesoscale eddies are wholely parameterized. [Sonya Legg, United States of America]	Accepted, we have adjusted the wording to take this comment into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31529	55	45	55	55	This is already said above. This para can be removed entirely. Anyway, I would not put ACC and upper cell in the same basket. SROCC/AR6 (Chap 2/9) says that ACC has not increased (low confidence), and that upper cell has increased (low confidence). [Jean-Baptiste SALLEE, France]	Accepted, this paragraph has been deleted as suggested.
127341	56	1	56	7	This is an inadequate summary. The focus of the chapter is human influence on the climate system, not on CMIP models. They are just one of the tools to explore the human influence. [Trigg Talley, United States of America]	Accepted, we have adjusted this sentence in response to this comment.
71909	56	1		7	This is an inadequate summary. The focus of the chapter is human influence on the climate system, not on CMIP models. They are just one of the tools to explore the human influence. I think this goes back to an incomplete spread of author expertise across the relevant disciplines. [John Church, Australia]	Accepted, we have adjusted this sentence in response to this comment.
31505	56	2	56	2	"substantial observational uncertainty": substantial observational uncertainty and climate model challenges preclude …. [Jean-Baptiste SALLEE, France]	Accepted, wording altered as suggested.
31507	56	4	56	4	It is confusing to come back here to a summary on AMOC which was done above [Jean-Baptiste SALLEE, France]	Accepted, the reference to AMOC has now been removed.
31509	56	5	56	7	It is unclear what is the paper supporting that statement. I don't see where you describe that above. [Jean-Baptiste SALLEE, France]	Accepted, this sentence has now been removed.
67635	56	10	56	10	As currently structured, "Section 3.6 Human influence on the Biosphere" only discusses the biogeochemical processes associated with the carbon cycle. However, it is also well-known and recognized by the recent Land Use IPCC report that there is an equally significant but much more uncertain biophysical impact – change of energy flows (transpiration and albedo) resulting from changes in vegetation cover. In other Chapters, e.g. in Chapter 7 on Climate sensitivity, this is discussed at least briefly (see p. 45 in Chapter 7) and a reference is given to Jia et al. 2019 for the full report. Some mention seems appropriate in Section 3.6 too, especially as it relates to some key knowledge gaps (e.g. Winckler et al. 2018 https://doi.org/10.1029/2018GL080211). The biophysical impact is highly uncertain and depends strongly on atmospheric moisture convergence, which for major river basins of the world is poorly known (e.g., Marengo 2006, Hagemann et al. 2011). [Antonio Nobre, Brazil]	Taken into account. The section only discusses indicators for which there have been attribution studies, but the reader is now redirected for Chapter 7 for forcing discussions.
102877	56	10	56	10	The title of the section should refer to "ecosystems", instead of "biosphere". Oceans and the atmosphere are part of the biosphere. [Philippe Tulkens, Belgium]	Rejected. Agreed in principle, but need to be consistent with the structure of Chapters 2 to 4.
10953	56	10	58	45	In section 3.6.1 there are multiple references to leaf area index and two seperate defintions of the terms acronym (LAI). These repetitions occur on page 56 line 55 to page 57 line 1 and page 57 line 53. I suggest defining the acronym at the first mention of leaf area index (page 56 line 28). [Joseph Thomas, United States of America]	Accepted. Changed as suggested.
39639	56	10	58	49	Regarding to this section, nowadays solar-induced chlorophyll fluorescence has been recently applied. There are several publications about SIF studies related with CO2 flux or gross primary production (GPP) as well. I think it may reflect the sub-section 1.5. [Nyein Chan, Myanmar]	Noted. Observations are of the remit of Chapter 2.
102879	56	12	56	25	Lade et al (2020) identify nine key processes as climate change, biogeochemical (nitrogen and phosphorus) flows, land-system change, freshwater use, aerosol loading, ozone depletion, ocean acidification, loss of biosphere integrity, including functional and genetic biodiversity, and introduction of novel entities, such as toxic chemicals and plastics (Lade, S.J., Steffen, W., de Vries, W. et al. Human impacts on planetary boundaries amplified by Earth system interactions. Nat Sustain 3, 119–128 (2020). https://doi.org/10.1038/s41893-019-0454-4) [Philippe Tulkens, Belgium]	Noted. Not sure how the comment relates to this paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28303	56	12	58	47	While everything mentioned in section 3.6.1 is important, it mostly ignores other pathways of human influence by land use and management (apart from a very brief mentioning of agricultural productivity), that might be expected from the title. Suggest to either include a cross-reference to a part of the report where this is discussed, do it here, or change the title accordingly. [Alexander Graf, Germany]	Taken into account. Cross-references are now made to Chapter 5, but the section only deals with indicators for which there are attribution studies available.
106507	56	12			Useful recent paper for 3.6.1: Beyond Static Benchmarking: Using Experimental Manipulations to Evaluate Land Model Assumptions By: Wieder, William R.; Lawrence, David M.; Fisher, Rosie A.; et al. GLOBAL BIOGEOCHEMICAL CYCLES Volume: 33 Issue: 10 Pages: 1289-1309 Published: OCT 2019 [camille parmesan, France]	Noted. The reference has been considered.
111017	56	12			maybe parts of this section could be omitted by referring to 5.2.1.4 and 2.3.4 were already detailed explanations are presented [Julia Nabel, Germany]	Taken into account. Cross-references have been added, and level of detail has been reduced in places.
102881	56	14	56	14	"SRCCL" needs to be spelled out (first use in chapter) [Philippe Tulkens, Belgium]	Accepted. Now defined.
70821	56	14	56	25	Another important omission we found in our research is the omission of key land-management impacts beyond large-scale land-cover changes. For the case study of Austria and France we could show that these omissions could potentially change the overall picture (Erb et al., 2013 10.1038/nclimate2004, Le Noe et al., 2020 10.1111/gcb.15004) and Erb et al., for a global assessment (10.1038/nature25138). [Karlheinz Erb, Austria]	Accepted. Relevant reference added.
81601	56	14	56	25	suggest to add a link to Section 5.4, where this is discussed in more detail [Sönke Zaehle, Germany]	Accepted. Link now made.
8953	56	14	58	50	It is surprising that no mention to wildfires is done in this chapter, in spite of being a clear human factor with relevant influence on the carbon cycle. There are many publications on this issue: van der Werf et al., 2017 for instance. [Chuvieco Emilio, Spain]	Taken into account. Cross-references are now made to Chapter 5, but the section only deals with indicators for which there are attribution studies available.
2639	56	14			define SRCCL [Bryan Weare, United States of America]	Accepted. Now defined.
13365	56	17	56	17	ESM must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Now defined.
81599	56	18	56	20	Amongst all the uncertainties in land models (treatment of allocation, soil organic matter dynamics, land-use, nutrients), it seems unduly to single out this factor [Sönke Zaehle, Germany]	Accepted. The list has been made more exhaustive.
14879	56	19			a discussion about land-use land-cover changes would be welcome [Marie-France Loutre, Switzerland]	Taken into account. Cross-references are now made to Chapter 5, but the section only deals with indicators for which there are attribution studies available.
81051	56	20	56	25	ch5 dives into detail on both on what drives the CO2 sinks and model evaluation and what models are missing. It might be apprpriate to cite sections 5.2.1.4.1 and 5.4. [canadell pep, Australia]	Accepted. Link now made.
96293	56	20			It could be helpful to hint on the thermal responses of respiration and photosynthesis which is not clear enough in the current text. [Nicole Wilke, Germany]	Noted. Both are cited in the sentence.
81597	56	22	56	22	Half of the C4MIP models now include a N cycle, hence this cannot be called a "routine omission" anymore. [Sönke Zaehle, Germany]	Accepted. Reworded to "frequent"
64653	56	27	56	33	Besides the simulation models, if you mention the carbon footprint here, and explain how much it has changed for the world, it will be an important point [Eman Abdelazem, Egypt]	Rejected. These considerations are for Chapter 5 or possibly other working groups.
81603	56	35	56	35	Use of "Since AR5" is inappropriate here, because this was done at least since Heimann et al. 1998, Global Biogeochemical Cycles [Sönke Zaehle, Germany]	Accepted. Reworded to "further tested"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64461	56	38	56	38	if you consider to update to a more recent global carbon budget (GCB) (e.g. le Quere et al 2018 or Friedlingstein 2019) than the land carbon sink is not an observational estimate but assessed from DGVM simulations [Julia Nabel, Germany]	Accepted. Reworded accordingly.
64463	56	39	56	39	why le Quere et al 2016 and not a more recent GCB, e.g. Friedlingstein et al. 2019? [Julia Nabel, Germany]	Accepted. Updated to the most recent GCB.
10955	56	39	56	41	The sentence "The CMIP6 models simulate a range of current CO2 values centered around the observed value of 380 ppmv in 2010, with a range of approximately 360 to 400 ppmv." This sentence is confusing for two reasons. The word current is not appropriate here, as the sentence refers to CO2 concentrations that occurred in 2010. Additionally, it is important to note that while the observed value for CO2 is ~380 ppmv on the graph, and this is within the larger range of modeled values (360 to 400 ppmv), there is only a single model that constrains the lower most value in this range. There are multiple models (4) that projected higher CO2 values during this time. It would be more appropriate to say that “CMIP6 models simulate a range of CO2 values that bound the observed value of 380 ppmv in 2010…”. [Joseph Thomas, United States of America]	Taken into account. "current" has been deleted. Figure 3.30 has been updated and models are now more evenly distributed around the observed CO2 concentration.
6623	56	40	56	40	I would not regard a 2010 value of CO2 as "current", given the rate at which CO2 is changing. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "current" has been deleted.
64467	56	42	56	46	this sentence needs a rephrasing because currently it might be understood as if only models without nutrient limitations suffer from the other listed uncertainties. Maybe consider splitting the information onto two sentences but integrating the next sentence which again deals with nutrient limitations [Julia Nabel, Germany]	Accepted. The sentence has been rewritten.
106503	56	42	56	46	A good list, but 2 more could be added. N-limitation is important, but so can be water limitation (as I can see is mentioned on the next page 57, line4). And forest systems change dramatically over time, being large C-sinks during regrowth (e.g. reforestation of abandoned hay meadows and agricultural fields in the eastern USA and parts of Europe), but are often fairly C-neutral once becoming mature forests. [camille parmesan, France]	Taken into account. Water limitation has been added, and a mention to maturity-related changes has been made.
81605	56	42	56	48	This section uses outdated CMIP5 results in which N limitation was overestimated by one, and not considered by all other models. Arora et al. 2019, Biogeosciences Discussions, Meyerholt et al. (2020) Global Change Biology both show that C-cycle only models are not generally inconsistent with current C cycle obserations for the historical period, while they still show a strong effect for future projections. Similarily, N cycle models in the TRENDY ensemble (Friedlingstein et al. 2020, ESSD) do not generally outperform C-cycle only models wrt to their capacity to reproduce the current sink [Sönke Zaehle, Germany]	Accepted. The discussion has been updated as suggested and the assessment has been fine-tuned accordingly.
64465	56	43	56	43	Figure 3.30c does not really support this statement [Julia Nabel, Germany]	Accepted. Statement deleted as not true any more in recent studies.
375	56	45	56	45	replace „CO2“ with „atmospheric CO2 concentration“ [Wolfgang Obermeier, Germany]	Accepted. Reworded as suggested.
96295	56	45	56	46	It could also help to hint at carbon allocation and turnover, and land-use change. [Nicole Wilke, Germany]	Noted. Both are cited in this sentence.
81607	56	48	56	48	There seems to be an error in Figure 33, because of the few lines shown. Nevertheless, the statement made appears not to be supported. Also, I don't see the point of including the residual land sink in the figure, if the model results are the net flux (no need to confuse the readers). If the replotted figure still makes the case, I still recommend to outline clearly that there are more differences between these sets of models than only N cycling, and therefore simply blaming the lack of performance of N might be an oversimplification [Sönke Zaehle, Germany]	Taken into account. Discussion on nutrient limitation has been revised to account for recent advances.
81609	56	51	56	54	I don't see the need to bring up emergent constraints here, but rather recommend to link to Section 5.2.1.4, where the IAV aspect is covered [Sönke Zaehle, Germany]	Accepted. Change in emphasis made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81615	56	55	57	3	This contradicts Chapter 5, in particular Figure 5.24. because it ignores the effect of land-use related C losses. Please harmonise the text with Chapter 5 content [Sönke Zaehle, Germany]	Accepted. Sentence rewritten to agree with Chapter 5.
112869	56	56	57	2	There is consolidating evidence that the terrestrial carbon uptake is mainly driven by the extratropical rather than the tropical ecosystems, as opposed to what the study by Sitch et al., 2015 suggests: “the majority of current terrestrial carbon accumulation is in the tropics”. A more recent study suggested that the tropical forests have already switched to being a net source of carbon, also considering land-use emissions (Baccini et al.,2017, Science). Also Ciais et al. (2019, Nature) demonstrate the dominance of the Northern Hemispheric land as a carbon sink. Further, the study by Winkler et al. (2020, in review AGU Advances, https://doi.org/10.1002/essoar.10503202.1) report an extension of browning regions (LAI decreases) in tropical forests, which suggests that the photosynthetic activity is decreasing, too. [Alexander Winkler, Germany]	Accepted. Sentence corrected and relevant studies are cited.
81613	57	3	57	5	This is not universally the case. Perhaps clarify by stating that in many regions, small scale IAV is controlled by water availability, but at large scales, these either cancel or correlate well with large-scale temperature anomalies? [Sönke Zaehle, Germany]	Accepted. Reworded as suggested.
81611	57	8	57	8	This sentence is hard to read. Please condider consolidating this with Chapter 5, which has and FAQ on the air-borne fraction, and will likely make a stronger case on the sink fractions in the revised version [Sönke Zaehle, Germany]	Accepted. The sentence has been clarified and link with Chapter 5 has been made.
64471	57	9	57	9	please clarify "to be an on-going substantial fraction of emissions" [Julia Nabel, Germany]	Accepted. The sentence has been clarified that it is about the airborne fraction.
64473	57	9	57	11	repetition of p57 46-48, consider deletion [Julia Nabel, Germany]	Rejected. The second statement is made in another context.
67719	57	9	57	11	"'but only when the land models include representation of nutrient limitation (Figure 3.30:)": How can we see which model considered nutrient limitation in Figure 3.30? [Hiroaki Kondo, Japan]	Noted. Discussion on nutrient limitation has been revised to account for recent advances, and that downplays the importance of including nitrogen limitation.
81053	57	9	57	11	Chapter 5 high confidence statement is in fact to highlight that we are not confident yet we are getting the distribution of the sinks right. "There is high confidence that land carbon cycle models continue to underestimate the Northern Hemisphere land carbon sink, when compared to estimates from atmospheric inversion". The statement in Ch3 is almost the oposite. Best if we talk about it so the messge is consistent. p64, l 19 in ch5 [canadell pep, Australia]	Accepted. The assessment has been made consistent with Chapter 5.
10949	57	27	57	51	The connection between the text of this section and the title "Human Influence on the Biosphere - Terrestrial Carbon Cycle" is a bit unclear. In this section the seasonal variations in CO2 are explained, but focus is placed on how models cannot accurately describe the current state of seasonal CO2 without a focus on the human influence. It would benefit this section to clearly state which parts of the seasonal carbon cycle are directly attributable to human activity. Some recent work has been conducted to attempt to tease out which human influence contributes more strongly to seasonal variations in CO2 (Bastos et al., 2019 - doi: http://dx.doi.org/10.5194/acp-19-12361-2019), which may benefit directly relating this section to human influences. Additionally, some in text citations of other chapters, namely 5.2.1.4.2 (Interannual variability in land-atmosphere CO2 exchange) and 5.2.2.3 (Land biospheric emissions and sinks), may benefit readers who are interested in learning more about the subject. [Joseph Thomas, United States of America]	Accepted. Relevant reference now discussed, and focus of the section has been clarified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64475	57	27	58	24	Potentially relevant additonal recent literature: Bastos et al. (2019): contrasting effects of CO2 fertilization (positive) and warming (negative) on NH seasonal amplitude (increase); Wang et al. (accepted in GCB): noted a stall in the increase of the Mauna Loa SCA and studied the potential driving factors of the observed change in Mauna Loa SCA with DGVMs. The study found that intensified drought stress is offsetting the CO2 fertilization. Winkler et al. (submitted): LAI satelite product reveals a slowing down of greening and strengthening of browning trends and "a collection of model simulations in conjunction with causal theory points at climatic changes as principal driver of vegetation changes". Kondo et al. (2018) suggest past land use change (abandonment and afforestation in the NH) and the subsequent plant regrowth as an important factor of increased NH carbon uptake. Bastos et al. 2019 https://doi.org/10.5194/acp-19-12361-2019; Wang et al. accepted in GCB, "Causes of slowing-down seasonal CO2 amplitude at Mauna Loa"; Winkler et al. submitted - https://www.essoar.org/doi/abs/10.1002/essoar.10503202.1; Kondo et al. (2018) https://doi.org/10.1029/2018GL077633 [Julia Nabel, Germany]	Accepted. Relevant references now discussed.
6625	57	29	57	29	"its" should be "in". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reworded as suggested.
64469	57	29	57	29	Figure 3.31 deals with global land carbon uptake and is thus not suitable to support this statement about the NH [Julia Nabel, Germany]	Accepted. Reference to Figure 3.31 deleted.
64477	57	30	57	31	Wang et al. accepted in GCB found a recent slowing-down of the seasonal CO2 amplitude at Mauna Loa [Julia Nabel, Germany]	Accepted. Relevant reference now discussed.
106505	57	33	57	34	Longer term controlled CO2 lab experiments consistently show a burst of growth under early phase of increased CO2 that typically flattens as other factors become limiting. This suggests that CO2 "fertilization" may be a temporary phenomenon in natural systems, and so less likely to continue to influence global atmospheric CO2 levels in the long term (decades). Useful Refs: (1) A meta-analysis of 1,119 manipulative experiments on terrestrial carbon-cycling responses to global change By: Song, Jian; Wan, Shiqiang; Piao, Shilong; et al., NATURE ECOLOGY & EVOLUTION Volume: 3 Issue: 9 Pages: 1309-1320 Published: SEP 2019 (2) Effects of elevated CO2 on plant C-N-P stoichiometry in terrestrial ecosystems: A meta-analysis By: Du, Chenjun; Wang, Xiaodan; Zhang, Mengyao; et al. SCIENCE OF THE TOTAL ENVIRONMENT Volume: 650 Pages: 697-708 Part: 1 Published: FEB 10 2019. (3) Rising atmospheric carbon dioxide: Plants face the future By: Long, SP; Ainsworth, EA; Rogers, A; et al. ANNUAL REVIEW OF PLANT BIOLOGY Volume: 55 Pages: 591-628 Published: But for a counter-point, see: FACE facts hold for multiple generations; Evidence from natural CO2 springs By: Saban, Jasmine M.; Chapman, Mark A.; Taylor, Gail GLOBAL CHANGE BIOLOGY Volume: 25 Issue: 1 Pages: 1-11 Published: JAN 2019 [camille parmesan, France]	Noted. The section discussed attributable changes, so does not speculate on future evolution.
64479	57	36	57	38	This statement is also supported by findings in Wang et al. (accepted in GCB); Burmann et al. (2018) and Winkler et al. (submitted). Wang et al. accepted in GCB, "Causes of slowing-down seasonal CO2 amplitude at Mauna Loa"; Buermann, et al. (2018), https://doi.org/10.1038/s41586-018-0555-7; Winkler et al. submitted - https://www.essoar.org/doi/abs/10.1002/essoar.10503202.1 [Julia Nabel, Germany]	Accepted. Relevant references now discussed.
64481	57	39	57	39	Wang et al. (accepted in GCB) found a recent slowing-down of the seasonal CO2 amplitude at Mauna Loa [Julia Nabel, Germany]	Accepted. Relevant reference now discussed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64499	57	40	57	40	recent studies tend to identify a negative effect due to warming (Bastos et al. (2019), Wang et al. (accepted)) [Julia Nabel, Germany]	Accepted. Relevant references now discussed.
64483	57	41	57	43	Note that listed factors mainly influence the simulation of managed land and less the simulation of natural vegetation [Julia Nabel, Germany]	Noted.
102883	57	43	57	43	Delete 'may'. The sentence should read: All these missing factors influence the response, although their combined effect is not clear. [Philippe Tulkens, Belgium]	Accepted. Reworded as suggested.
64485	57	45	57	45	Bastos et al. (2019) found contrasting effects of CO2 fertilization (positive) and warming (negative) [Julia Nabel, Germany]	Accepted. Relevant reference now discussed.
102885	57	46	57	48	This is somewhat cryptic: "Autumn"? "Barrow"? [Philippe Tulkens, Belgium]	Accepted. "Autumn" reworded to "autumn". Clarified that Barrow is in Alaska.
96297	57	48			Reference to Li et al 2018 is ambiguous (two different authors with last name Li for 2018 in bibliography). Please amend. [Nicole Wilke, Germany]	Accepted. Ambiguity has been lifted.
377	57	50	57	50	replace „CO2“ with „atmospheric CO2 concentration“ [Wolfgang Obermeier, Germany]	Accepted. Reworded as suggested.
64487	57	50	57	50	there is no 5.2.2.4.3 -> 5.2.1.4? [Julia Nabel, Germany]	Accepted. Corrected.
81617	57	53	58	14	Why does this text not mention climate change related greening in high-latitude ecosystems (Forkel et al. 2016, Sciene, and many others?). [Sönke Zaehle, Germany]	Accepted. The discussion is not aimed at being regional, but reference is relevant in context.
112867	57	53	58	14	The results by Zhu et al. 2016 claiming that CO2 fertilization mainly drives the global greening trend has been challenged by new studies, for example, by Chen et al. (Nature Sustainability, 2019) as already mentioned in the text. Whereas Chen et al. focused on regions dominated by land management, another new study by Winkler et al. (2020, https://doi.org/10.1002/essoar.10503202.1 under review in AGU Advances) investigated the underlying drivers of LAI changes for Earth’s individual major natural biomes. In this study, the authors analyze the longest available satellite record of global leaf area index (LAI, 1981-2017) and several clusters of significant long-term changes are identified. Using process-based model simulations (Earth system and land surface models), the authors disentangle the effects of anthropogenic carbon emissions on LAI in a probabilistic setting applying Causal Counterfactual Theory. The analysis prominently indicates the effects of climate change on many biomes - warming in northern ecosystems (greening) and rainfall anomalies in tropical biomes (browning). The results of this study do not support previously published accounts of dominant global-scale effects of CO2 fertilization. Altogether, our analysis reveals a slowing down of greening and strengthening of browning trends, particularly in the last two decades. Most models substantially underestimate the emerging vegetation browning, especially in the tropical rainforests. [Alexander Winkler, Germany]	Accepted. Discussion has been amended to better document the debate, and relevant reference has been added.
64489	58	1	58	7	Winkler et al. (submitted) analysed LAI of a satellite product, as well as results of factorial simulations with an earth system model and a set of land surface models. Results of the study point to climate change effects on vegetation changes (warming or rainfall anomalies depending on the different biomes) and do not support the dominant influence of CO2 found in earlier studies. [Julia Nabel, Germany]	Accepted. Discussion has been amended to better document the debate, and relevant reference has been added.
3667	58	2	58	4	thank you for citing my two important “greening” papers (Mao et al., 2013 and 2016). However, I would suggest relevant two sentences be changed into “This observed greening supports findings of Mao et al. (2013 and 2016). Also, because of the south-to-north asymmetric land surface warming, this fertilization effect was much more strengthened in the Northern Hemisphere than that in the Southern Hemisphere causing the latitudinal asymmetry of land greening trend (Mao et al., 2013).” [Jiafu Mao, United States of America]	Taken into account. Sentence rewritten to make hemispheric difference clearer, based on suggested wording.
52913	58	3			This is not intuitive and may need further explanation [Hervé Douville, France]	Accepted. See answer to comment number 3667.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81055	58	4	58	6	Zhu paper is very important in so far it shows that agricultural intensification globally is responsible for a large part of the observed greening. That is beyond China and India where the greening trend is dominated by agriculture. It is important to show that it is not an addity of China and India. [canadell pep, Australia]	Taken into account. The discussion has been extensively rewritten to have a more global outlook. However, recent studies focus on India and China, and that focus needs to be stated clearly.
81619	58	16	58	17	This statement seems off topic for Chapter 3 and should be coordinated with Chapter 5 [Sönke Zaehle, Germany]	Taken into account. Model performance and its link to attribution is on topic. Inconsistency with Chapter 5 has been fixed.
81057	58	16	58	21	The section spends more time on attribution to long term trends than interannual variaiblity. I would suggest tha the summary paragraph this is reflected. [canadell pep, Australia]	Accepted. The summary has been rebalanced as suggested.
52915	58	17			probably not the main driver of interannual variability => temper this statement about the key role of nutrient limitation? [Hervé Douville, France]	Accepted. The statement has been revised to be consistent with Chapter 5.
81621	58	18	58	18	Use of "mostly" conflicts with the correctly more balanced text in p57 l27-51 and needs to be revised. [Sönke Zaehle, Germany]	Accepted. "Mostly" replaced by "albeit counteracted by other factors".
81059	58	21	58	24	I think based on the literature available we can state with high confidence that boht the CO2 fertilization effect and the warming of the high latitudes has contributed to the greening trend observed, but that land management activities such intensification of agricutlure has contributed too. [canadell pep, Australia]	Accepted. The revised discussion supports this comment, and the assessment has been rewritten along these lines.
81623	58	21	58	24	I don't understand the focus on CO2 fertilisation effects on LAI here, when other human factors (climate change, land-use, N enrichment) also play a role. [Sönke Zaehle, Germany]	Accepted. The revised discussion supports a wider focus.
52917	58	24			Also add a conclusion about the possible role of surface warming on the LAI phenology, at least in the mid and high latitudes? [Hervé Douville, France]	Accepted. The revised discussion supports a wider focus.
19797	58	30	58	30	Please print "nbp" in capitals, so as to make easier the connection with figure 3.31b [philippe waldteufel, France]	Noted. Figure 3.31 has been revised and its caption has changed.
102887	58	50	60	27	The section 3.6.2 should include a subsection on the marine ecosystems (from phytoplankton to whales, marine organisms play a key role for carbon sequestration and storage. This aspect is missong int the current section. [Philippe Tulkens, Belgium]	Rejected. The chapter focuses on large scale - biogeochemistry climate change indicators rather than marine ecosystem. This is done in consistency with Chapter 2 and 4
35671	58	53	58	54	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
116233	58		58		Please check the assessment of SRCCL and the attribution of vegetation greening, changes in land carbon uptake, to the CO2 fertilization effect, considering the potential importance of land management and land use (see Chen et al 2019 also included in SRCCL assessment). [Valerie Masson-Delmotte, France]	Accepted. The discussion now refers to SRCCL assessment.
116235	58		58		Could the section conclusion be more explicit about emergence and differences or confirmation with SROCC findings? [Valerie Masson-Delmotte, France]	Accepted. The conclusion has been clarified.
99165	59	1	60	27	In chapter 5 we were advised to use "ocean de-oxygenation" instead of deoxygenation [Leticia Cotrim da Cunha, Brazil]	Noted.
35673	59	4	59	4	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102889	59	10	59	53	See one of the general comments to this chapter: here we have "capabie of representing", "difficulty reproducing" (l19) and "ability to represent these signals" - is this harmonized language on model performance? [Philippe Tulkens, Belgium]	There is no particular harmonized language on model performance. The terms used here best describe how the models are representing the corresponding observed signals.
35675	59	11	59	11	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
19799	59	13	59	13	A comma following "AR5" would be useful [philippe waldteufel, France]	Editorial - Taken into account.
99161	59	22	59	41	I would suggest a hand-shake to chapter 5 WGI as de-oxygenation is assessed in chapter 5, secion 5.3, for both ocean interior and coastal areas. What do you define as upper ocean? [Leticia Cotrim da Cunha, Brazil]	Accepted. Text revised to include a handshake with Chap 5. The upper ocean assessed in the text is basically from 0-1000m
18039	59	28	59	30	To clarify this point, climate change (warming, precipitation or even wetland loss from sea level rise) may account for rapid rise in number of dead zones - also earlier seasonal onset, intensity, duration etc. Altieri and Gedan 2015 [Lisa Levin, United States of America]	Noted.
19801	59	40	59	40	A comma following "effects" would be useful [philippe waldteufel, France]	Editorial - Taken into account.
30591	60	5	60	10	This statement is inconsistent with the values reported in Chapter 5. The ranges reprted her are different from those in Chapter 5. [nina bednarsek, United States of America]	Rejected. Chapter 5, Section 5.3.2.1 is using the same numbers with more details for different regions
36405	60	5	60	10	This statement is inconsistent with the values reported in Chapter 5. The ranges reprted here are different from those in Chapter 5. [Adrienne Sutton, United States of America]	Rejected. Chapter 5, Section 5.3.2.1 is using the same numbers with more details for different regions
40619	60	10	60	10	Perhaps give SROCC subsection reference, rather than the entire report. [TSU WGI, France]	Taken into account. SROCC – 5.2 is now used
21519	60	21	60	21	I assume you mean the mid-2000s rather than implying that the date of the shift is known to a monthly precision? [Peter Thorne, Ireland]	Accepted, text updated.
99163	60	25	60	27	I would suggest to use "upper ocean" instead of "surface ocean" to be consistent with the paragraph on page 59, and the trends in ocean de-oxygenation. [Leticia Cotrim da Cunha, Brazil]	Taken into account. Upper ocean is now used.
84181	60	26	60	26	it is AVI.2 [Annalisa Cherchi, Italy]	Noted. Comment not clear
11307	60	30	60	30	The section 3.7 has been overall well written, but it'd be better to give a short synthesis subsection at the end because many different 'modes' of variability were discussed in parallel and readers may find a bit difficulty in understanding the entire picture of possible human influences to the internal modes of climate variability. [Masahiro Watanabe, Japan]	Taken into account. Due to length limitation, we decided not to add another subsection. However, a table that summarizes the Chapter 2-3-4 assessments on modes of variability has been added in the Technical Summary. A summary table about teleconnection has been also added in the Atlas chapter for all the modes assessed in AR6.
71397	60	30			Somewhere you might add a link to Chapter 10. We are discussing the role of modes of variability as drivers for regional variability, and partly discuss the performance in simulating teleconnections (for ENSO). We should at least check for consistency. [Douglas Maraun, Austria]	Taken into account. We added 4 lines of introduction at the beginning of Section 3.7 devoted to the MoVs to describe how MoVs are addressed throughout the report and we explicitly cite the Chapters where MoVs are assessed.
21523	60	32			In terms of overall chapter balance the section on NAO/NAM feels disproportionately long compared to many other segments. It may be worth reducing this segment length accordingly if possible. [Peter Thorne, Ireland]	Noted. We reduced it in the final version.
52919	60	34	60	42	There is an on-going debate about the reality of the NAM (at least in the lower troposphere) which may be partly a statistical artifact of two regional modes (NAO and PNA) which are however not strongly inter-connected. This may be need a more cautious statement about the NAO and NAM interpretation. [Hervé Douville, France]	Taken into account. Because of length constraint on the text, this remarks has been included in the technical annex. Two references have been also added to address this issue.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84179	60	42	60	42	it is AVI.2 [Annalisa Cherchi, Italy]	Editorial.
111057	60	46			did not reproduce… daily ..NAO' what does that mean? On daily timescales the model was too sluggish? Or is the issue persistence? Would be good to clarify [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This confusing sentence has been actually removed because the issue of persistence is addressed later on in a more complete way.
12035	60	47	60	48	"the observed positive trend of the NAO/NAM": What of trend? Some index or amplitudes or else? Need to be clarified. [Masaki Satoh, Japan]	Taken into account. The word "indices" has been added in the sentence.
90815	60	48			Refer study from "Testing the robustness of a precipitation proxy-based North Atlantic Oscillation reconstruction" by Lehner et al., on the failure of the reconstruction to verify against instrumental records of the NAO [Vivien How, Malaysia]	Taken into account. A full paragraph has been added regarding the reconstruction of the NAO over the last millennium together with the assessment of the external natural forcing influence.
35907	61	5	61	5	Lee et al., 2018 --> Lee et al., 2019. [Jiwoo Lee, United States of America]	Accepted. Ref corrected.
35677	61	8	61	8	Use published sources [Carlos Antonio Poot Delgado, Mexico]	All the papers included in the report have been accepted prior to the literature cutoff deadline of 31 January 2021 and are therefore published by the time the report is published.
37695	61	21	61	21	The meaning of "temporality" is not very clear. [Masahide Kimoto, Japan]	Taken into account. Temporality has been replaced by timing.
12037	61	25	61	25	"2.4.5.1" should be "2.4.1.1". Trend of what? It may better to add index or else. Check also several places in L4-12 of p.62. [Masaki Satoh, Japan]	Editorial + taken into account. The word index has been added when applicable.
10969	62	4	62	12	The discussion of a suggested negative summer NAO trend could acknowledge the presence of strong multidecadal variability (eg Sutton and Dong 2012). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken in to account. The presence of strong multidecadal variability has been added in the text.
90817	62	4			Refer sutdy from "On the low frequency variability of wintertime Euro-Atlantic planetary wave-breaking" by Messori et al (2018) which justify the discrepancies of the large scale atmospheric circulation. [Vivien How, Malaysia]	Not applicable. This part has been largely shortened and we instead cite the Cross-Chapter Box 10.1.
35679	62	12	62	12	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Not applicable. The citation has been removed.
213	62	15	62	17	Being an author of Navarro et al. (2016) I fail to see the direct link between changes in NAM/NAO and sulfate aerosol reductions in Europe during 1980-2005, mostly because that question was not addressed in the article. I think the sentence could be better explained to clarify more what is really meant. [Juan Camilo Acosta Navarro, Spain]	Taken into account. This citation was not relevant and misplaced. The entire sentence has been accordingly removed.
102891	62	22	62	24	"These conclusions…". The transition between these paragraphs is a bit awkward, especially since in the last sentence there were no conclusions, rather the opposite if the direction of causality is unclear. This can easily be mended by starting the paragraph differently. [Philippe Tulkens, Belgium]	Taken into account. Text has been changed accordingly.
2649	62	24			It is not at all clear what "These conclusions" refers to. Certainly not the previous paragraph [Bryan Weare, United States of America]	Taken into account. Text has been changed to make the transition between paragraphs clearer.
52181	62	25	62	25	Typing error in the word soils in the sentence "… the region of Valparaíso with 57% of eroded souls" . [Maritza Jadrijevic Girardi, Chile]	Not applicable. This sentence does not belong to chapter 3.
21521	62	28	62	28	section is 2.2.1 not 2.1.1 [Peter Thorne, Ireland]	Accepted. Number of section changed.
52947	62	29	62	31	although such a solar cycle influence has been recently denied by Chiodo et al. (2019) https://www.nature.com/articles/s41561-018-0293-3 [Hervé Douville, France]	Taken into account. Reference suggested by reviewer added + another one to illustrate the controversy on the solar influence on the NAO/NAM now addressed in the revised version.
79999	62	30	62	30	Actually, there are also papers that refute a solar connection to the NAO, i.e. Chiodo et al., NatGeo 2019 – at the very least, it would be good to acknowledge that the solar connection is subject to major uncertaintites [Gabriel Chiodo, Switzerland]	Taken into account. Reference suggested by reviewer added + another one to illustrate the controversy on the solar influence on the NAO/NAM now addressed in the revised version.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35681	62	36	62	36	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Not applicable. The citation has been removed.
10055	62	38	63	1	You need to mention that the NAO is significantly influenced by solar activity changes. This has been widely published. See e.g. summary in Lüdecke et al. 2020 (Decadal and multidecadal natural variability in European temperature, https://doi.org/10.1016/j.jastp.2020.105294). You also need to discuss in this chapter if climate models consider this solar influence in their modeling runs. If not, this could be part of the explanation why modeling performance remains pretty poor with regards to the NAO. The NAO also has a major impact on some regional temperatures, e.g. winter temperatures in central Europe (e.g. Lüdecke et al. 2020). Hence, solar activity changes are apparently influencing regional temperatures and precipitation through oceanic “modes of variability” (e.g. NAO, AMO, ENSO…) that amplify the solar signal and link it to climatic changes. [Sebastian Luening, Switzerland]	Taken into account. Reference added and put in context with other studies.
84183	62	43	63	1	not clear why "detection and attribution" is assessed in the summary [Annalisa Cherchi, Italy]	Taken into account. The summary paragraph has been shortened and the signal-to-noise paradox discussion moved before the final paragraph as it is part of the overall assessment and should not be included in the summary.
40647	62	44	62	45	Please refer the glossary definitions for 'Storyline' and 'Physical climate storyline'. [TSU WGI, France]	Not applicable. This words are not mentioned in the text. Likely wrong numbering of the comment.
102893	62	45	62	48	Somewhat lopsided emphasis. Suggest to delete "However, although" and start with "CMIP5 and CMIP6" , and in line 47 after "(high confidence)" add ". However, there is an apparent…." [Philippe Tulkens, Belgium]	Accepted. Corrected according to reviewer's suggestion
100797	62	51	62	52	It might be worth adding here that the persistence of the NAO regime is strongly underestimated in climate models and this bias (on the contrary of that of Scandinavian Blocking for example) is very stubborn: no improvements with enhanced horizontal resolution were found (see figure 8 from recent the multi-model study by Fabiano et al. 2020 (Fabiano, F., Christensen, H.M., Strommen, K. et al. Euro-Atlantic weather Regimes in the PRIMAVERA coupled climate simulations: impact of resolution and mean state biases on model performance. Clim Dyn (2020). https://doi.org/10.1007/s00382-020-05271-w [Corti Susanna, Italy]	Accepted. Reference added In the section + a discussion about the chronic biases in the persistence.
52921	62	51			Is there a specific timescale for the biases in persistence? [Hervé Douville, France]	Taken into account. "Daily" has been added to clarify the timescale to which persistence refers.
100837	62	52	62	52	Zhang and Kirtman, 2019 can be added to Strommen and Palmer here. Zhang, W. and B. Kirtman (2019). Understanding the Signal-to-Noise paradox with a simple Markov model. Geophys. Res. Lett., 46, 13308-13317, doi.org/10.1029/2019GL085159 [Corti Susanna, Italy]	Accepted. Reference added in the section.
100835	62	52	62	53	However, the signal-to-noise problem itself undergoes multi-decadal variability: while is has been detected in several models for the recent decades, due to non-stationarities in the climate system and sampling uncertainty it is not robust if assessed over the full 20th Century (Weisheimer et al., 2019; Weisheimer et al., 2020). Weisheimer, A.D. Decremer, D. MacLeod, C. O'Reilly, T. Stockdale, S. Johnson and T.N. Palmer (2019). How confident are predictability estimates of the winter North Atlantic Oscillation? Q. J. R. Meteorol. Soc., 145, 140-159, doi:10.1002/qj.3446 - Weisheimer, A., D. Befort, D. MacLeod, T.N. Palmer, C. O'Reilly and K. Strommen (2020). Seasonal forecasts of the 20th Century Bull. Amer. Meteor. Soc.doi:10.1175/BAMS-D-19-0019.1 [Corti Susanna, Italy]	Accepted. 2nd reference added + discussion

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42699	62	53			What can be deduced from the results referred to in this last sentence? Does the 'better quantify' support the summary statement of 'little evidence' for an anthropogenic component? Presuming this is the case, it would be useful to say this. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This sentence was misplaced at the end of the summary paragraph. It has been removed in the revision of the entire subsection.
111059	62	55			in this section, it would be good to briefly mention how reconstructions of the NAO look compared to mdoels. There are new efforts to reconstruct and data incoming from the logbook rescues. This would also raise questions to what extent NAO longterm changes as hypothesized for key periods of the last millennium are reliable and if these are reproduced by mdoel simulations of the last millennium [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A full paragraph has been added regarding the reconstruction of the NAO over the last millennium together with the assessment of the external natural forcing influence.
116237	62		62		What about NAO reconstructions spanning the past centuries and links with volcanic forcing? (eg studies by P. Ortega, D. Swingedouw and colleagues)? [Valerie Masson-Delmotte, France]	Taken into account. A full paragraph has been added regarding the reconstruction of the NAO over the last millennium together with the assessment of the external natural forcing influence.
13367	63	10	63	10	Missing or extra () [Maria Amparo Martinez Arroyo, Mexico]	Editorial.
52941	63	46	63	49	As for the NAM, there is a recent alternative to the usual interpretation of the SAM which may not be best interpreted in terms of midlatitude variability and could be a regional (i.e., NAO-like) rather than hemispheric-scale feature related to a Pacific-South America pattern (Spensberger et al., 2020) [Hervé Douville, France]	Noted. The discussion of how to define the SAM is now relegated to the Technical Annex which is a better place for discussing this. Here the focus is on model validation and attribution; for this we prefer to use common definitions of the SAM.
12039	63	47	63	47	"Figure 3.32e" should be "Figure 3.32c,d". [Masaki Satoh, Japan]	Editorial. Corrected.
84185	63	49	63	49	it is AVI.3 [Annalisa Cherchi, Italy]	Accepted. We have corrected the error.
116239	63	50	63	55	Please check formulation ("model failure"), implied role of solar forcing ("imposed insolation is too weak") [Valerie Masson-Delmotte, France]	Not applicable. We don't know which paragraph the reviewer is referring to. "Imposed insolation is too weak" does not appear anywhere in Chapter 3. I can't find the phrase anywhere else either.
102895	63	54	63	54	and throughout chapter decide whether to use greenhouse gas(es) or GHGs [Philippe Tulkens, Belgium]	Accepted. We now introduce and use consistently "GHGs".
84187	64	4	64	5	sentence unclear, better to rephrase [Annalisa Cherchi, Italy]	Accepted. We have rephrased the sentence.
65667	64	5	64	7	Suggest adding a reference for SAM trends in CMIP6: Grose MR and co-authors, Insights from CMIP6 for Australia's future climate, under review in Earth's Future. https://doi.org/10.1002/essoar.10501525.1. [Kushla Munro, Australia]	Accepted. We now cite Grose et al. (2020).
35909	64	6	64	6	Lee et al. (2018) --> Lee et al. (2019). [Jiwoo Lee, United States of America]	Accepted. We have corrected the year.
65669	64	20	64	23	Suggest adding a reference for influence of ozone and GHG on SAM trends: Arblaster, J.M. and G.A. Meehl (2006) Contributions of External Forcings to Southern Annular Mode Trends. J. Climate, 19, 2896–2905, https://doi.org/10.1175/JCLI3774.1 [Kushla Munro, Australia]	Noted. This is a classic bit of literature already assessed in AR4 and therefore not cited again here.
70891	64	20	64	48	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 [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This paper discusses a correlation between the breakdown date of the polar vortex and the latitude of the tropospheric jet. We feel it would be a digression to discuss the paper in this context as it does not address the question of what forces trends in the state of the polar vortex.
111061	64	25			this sentence answers questions I had about the role of the two forcings nicely and only slightly longer than the vague version in the ES! [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We thank the reviewer for this positive comment. We have also rephrased the ES accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70889	64	30	64	31	This point has been demonstrated using a purely observational causal analysis by Saggioro and Shepherd (2019 doi: 10.1029/2019GL084763) [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now cite this paper.
65175	64	31	64	31	Banaerjee et al 2020 is now published (doi: 10.1038/s41586-020-2120-4). [Mark England, United States of America]	Accepted. We have updated the citation and text.
90819	64	45			Also cite "The impact of stratospheric volcanic aerosol on decadal-scale climate predictions" by Timmreck et al. (2015) [Vivien How, Malaysia]	Rejected. Impacts of volcanic eruptions on short-term climate prediction are out of scope for this chapter.
65671	64	46	64	48	Suggest adding a comment here on the relationship between SAM and ENSO and how CMIP5 models fail to capture this relationship during austral summer. see Lim, E.-P., H. H. Hendon, J. M. Arblaster, F. Delage, H. Nguyen, S.-K. Min, and M. C. Wheeler (2016), The impact of the Southern Annular Mode on future changes in Southern Hemisphere rainfall, Geophys. Res. Lett., 43,7160–7167, doi:10.1002/2016GL069453. [Kushla Munro, Australia]	Accepted. We now cite this paper.
68069	64	50	65	4	Since there are large uncertainties in the reconstructions and also in the simulations, it would be more accurate to conclude that they disagree and the disagreement arises from uncertainty in both. Are the reconstructions all independent of one another? [MIchael Evans, United States of America]	Accepted. We have rephrased this to make clear that the disagreement could be due to either models or reconstructions. On the question of the independence of the SAM reconstructions, we refer the reviewer to Ch2 which has a more in-depth discussion of these reconstructions.
21525	65	15	65	17	As given these could be interpreted as factual statements so I would suggest making recourse to the uncertainty language and redrafting these accordingly as I believe you intend these either as low confidence or evidence/agreement constructs. [Peter Thorne, Ireland]	Accepted. We have made this a "low confidence" statement.
13369	65	26	65	26	AD must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Accepted. "Anno Domini" is not needed in the figure; it has been removed.
26757	65	32	65	32	Section 3.7.3 is mostly accurate and up to date [Eric Brun, France]	Thanks
105119	65	32	65	32	Some paleocontext, in relation with the response of ENSO to future climate change, might be introduced here. For instance based on Brown et al, https://www.clim-past-discuss.net/cp-2019-155/cp-2019-155.pdf [Masa KAGEYAMA, France]	Accepted. Some further paleo-climate information has now been introduced.
44047	65	34	67	13	It would be better to give more in depth anaylsis on the difference/improvment between CMIP6 and CMIP5 models in ENSO simulations. [Lijuan Li, China]	Accepted. We have added detail to the figures and text to better highlight the CMIP6 model improvements.
12041	65	38	65	38	"Annex A VI.3" should be "Annex A VI.4". [Masaki Satoh, Japan]	Accepted. Updated as suggested.
84189	65	38	65	38	it is AVI.4 [Annalisa Cherchi, Italy]	Accepted. Updated as suggested.
90821	65	40			Also cite "Evaluation of ENSO simulations in CMIP5 models: A new perspective based on percolation phase transition in complex networks" by Lu et al. (2018) [Vivien How, Malaysia]	Rejected. While this appears to be an interesting paper, it would be out of context to cite in this location.
90823	65	46			..(from text) there was low confidence in the role of a human-induced influence in these changes, "especially when the implications of El Niño for human health are usually more intense in less developed countries where populations are often more exposed to climate events, have limited coping capacity and are more vulnerable to climate impacts" (Refer WG III: CC mitigation??) [Vivien How, Malaysia]	Rejected. The implications for ENSO on human health has no influence on our confidence on the role of anthropogenic greenhouse gasses past changes of ENSO. However, the wording of this sentence has now been tweaked to ensure no further misunderstanding.
105765	65	49	65	55	You want to consider including a sentence about the findings of Brown et al (https://www.clim-past-discuss.net/cp-2019-155/), which draws comparisons of ENSO in CMIP6 & CMIP5 models from past and future simulations. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Now cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13371	65	51	65	51	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted, removed as suggested
35683	65	51	65	51	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Noted. This paper has now been published
13373	65	53	65	53	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Removed as suggested
84191	65	53	65	55	this sentence is quite vague and not clear how it relates with the sentence in the lines before [Annalisa Cherchi, Italy]	Accepted. This sentence has now been revised to aid interpretation.
35685	65	54	65	55	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Noted. Both of these cited papers have now been published.
13375	66	23	66	23	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted, removed as suggested
13379	66	26	66	26	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted, removed as suggested
35687	66	26	66	26	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted, this paper has now been published
13377	66	35	66	35	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted, removed as suggested
68071	66	43	66	45	Needs citations for historical period variability; e.g. Trenberth and Hurrell (1994); Trenberth and Hoar (1997), references therein; Chen et al (2004), Cole et al (1993). [MIchael Evans, United States of America]	Accepted. A historical period citation has been added as requested.
37349	66	43	66	54	You are playing semantics because the drivers of the ENSO are not yet understood. The "symptoms" of the shifts are clear but not the causes and while the causes are unknown if it impossible to know whether any anthropogenic actions play any part whatsoever. [John McLean, Australia]	Rejected. We have a good understanding of the dynamics of ENSO. However, the wording of this paragraph has been tweaked as it appears the reviewer had a misunderstanding of what was being presented.
37351	66	43	68	53	Please report if the models used in papers cited by these paragraphs have been validated. [John McLean, Australia]	Noted. The majority of text in this section has the aim of validating the models.
21527	66	47	66	49	I think it very dangerous for chapters to disagree in this manner. A further flag is that no supporting reference is given to back up the assertion being made. It is critical that this issue be resolved and that there is no inter-chapter disagreement of this kind in the FGD. [Peter Thorne, Ireland]	Accepted. This sentence has been reworded as suggested and it is now consistent with CH2.
26759	66	56	66	56	Please specify what "reastically" means in that context [Eric Brun, France]	Accepted. This sentence has now been reworded and the word "realistically" is no longer used.
35689	66	57	66	57	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. This paper has been published.
33281	66				Niño 3.4 SST anomalies might be described in the text not only in figures 3.35 and 3.36. Why 3.4? As a non specialist this number do not mean anything to me. 3.4 ºC temperature difference? Why? [Guiomar Rotllant, Spain]	Accepted. Nino 3.4 region SST anomalies are now described in the text as requested, and rather than simply refer to the region we mostly refer to its location, central Pacific.
26761	67	4	67	4	Please specify what "significant improvement" means in that context [Eric Brun, France]	Accepted. We mean statistical significance, the text has now been corrected.
199	67	6	67	9	As for the asymmetry of ENSO: Underrepresentation of the ENSO phase asymmetry in CMIP5 is well studied in Ohba et al. (2010, JC). Please consider my proposal to add the following reference. Ohba, M., D. Nohara, and H. Ueda, 2010: Simulation of Asymmetric ENSO Transition in WCRP CMIP3 Multi-model Experiments. J. Climate, 23, 6051-6067, doi:10.1175/2010JCLI3608.1. https://journals.ametsoc.org/doi/full/10.1175/2010JCLI3608.1 [Masamichi Ohba, Japan]	Rejected. The paper referred too analyses CMIP3 models which are directly reported here.
26763	67	9	67	12	This should be quantified [Eric Brun, France]	Accepted. The values are now quantified in the revised Figure 3.35.
105043	67	10	67	10	suggest "both CMIP model phases" -> "both CMIP5 and CMIP6 models" … "phases" may confuse some [Peter Gleckler, United States of America]	Accepted, updated as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
84193	67	11	67	11	"asymmetry" is misspelled [Annalisa Cherchi, Italy]	Accepted, spelling has been corrected.
10971	67	15	67	55	There is some evidence (observations + model) that the boreal summer teleconection from ENSO has changed over the past few decades, due to rising SST trends (O'Reilly et al, doi 10.1029/2019GL084079). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, while this is a very interesting paper, at this stage it is stand alone and can not feature in our assessment.
12043	67	17	67	17	"Annex A VI.3" should be "Annex A VI.4". [Masaki Satoh, Japan]	Accepted, wording changed as suggested.
12047	67	17	67	17	"Annex A VI.4" should be "Annex A VI.5". [Masaki Satoh, Japan]	Accepted, wording changed as suggested.
84195	67	17	67	17	it is AVI.4 [Annalisa Cherchi, Italy]	Accepted, wording changed as suggested.
37893	67	17	67	18	(more than 4,000 are now in the ocean; (Roemmich et al., 2019): missiong parenthesis [Junhee Lee, Republic of Korea]	Not applicable. The sentence does not exist in the present subsection. Wrong page and line numbers for comment.
105045	67	18	67	18	suggest changing 2nd use of "different" to "distinct" [Peter Gleckler, United States of America]	Accepted, wording changed as suggested.
14457	67	18			Page 67, line 18: The following comprehensive review paper on ENSO teleconnections is a good reference here to include: Taschetto et al. (2020): Atmospheric teleconnections of ENSO. In “El Niño Southern Oscillation in a Changing Climate”, American Geophysical Union, ISBN: 978-1-119-54812-6 [Malte Stuecker, United States of America]	Accepted, reference has been added.
132053	67	21	67	21	Section 3.7.3 A Central Pacific El Nino and an El Nino Modoki are not the same thing and there are two kinds of Modokis but that is post Ashok. [Sigmund Silber, United States of America]	Rejected. We are assessing research found in the literature and reporting as it is typically viewed. The reference of Capotondi et al. 2014 titled "Understanding ENSO diversity" provides a good idea of the community's current understanding on this matter.
45315	67	22	67	22	Liu et al. (2017) also showed paleo evidence that CP El Nino variabilityl increase with higher temperature. Doi: https://doi.org/10.1038/ncomms15386 [Anson Cheung, United States of America]	Accepted, this manuscript is now also cited here.
105047	67	23	67	23	suggest "observational record, observational" -> "observational record combinded wiith observational" [Peter Gleckler, United States of America]	Accepted, wording changed as suggested.
132051	67	25	67	25	Section 3.7.3 The geographical areas created for the various Nino Indices may no longer correlate well with the impacts of the phases of El Nino and this can be tested by the current correlation with weather patterns. [Sigmund Silber, United States of America]	Rejected. We are assessing research found in the literature and reporting on areas typically used. It is also worth noting that SSTs in the Nino3.4 region are highly correlated with equatorial SSTs east of approximately 160oE (Figure 3.37)
116241	67	34	67	34	The term 'impact" in the IPCC context has a precise meaning (see glossary). Here it can be replaced by "consequences" or "effects" if the meaning is a description of teleconnections? [Valerie Masson-Delmotte, France]	Accepted, changed from “impacts” to “climatic effects”
19803	67	34	67	39	Since it is understood that major impacts of ENSO occur through teleconnections, the question arises whether some ENSO indices represent teleconnection phenomena better than others. Reading through Annex VI page 9, this does not seem to have been considered. [philippe waldteufel, France]	Rejected. Focusing on individual teleconnected region will provide information about that location, rather than ENSO itself. It is common practise to start with a good index of ENSO, and in this case we chose the most commonly used index. It is worth noting that this index is used for operational forecasts in many countries around the globe.
29219	67	34	68	3	Fig. 3.37: Be careful. I have doubts that the ENSO rainfall teleconnection to the Mediterranean is statistically significant and robust, because of the short observed rainfall data (1979 to 2014). There are some studies that only find a statistically significant and robust response in the very western parts, like Portugal. [Fred Kucharski, Italy]	Noted, but we cannot change the data and there are manuscripts published to support this teleconnection.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71399	67	34	68	3	This section partly overlaps with a discussion in Chapter 10.3.3.4. We should decide whether you keep everything here and we refer to you, or whether we duplicate part of the material, but with a focus on regional aspects. At least we need to check for consistency. [Douglas Maraun, Austria]	Accepted, we now coordinate with CH10.
12045	67	41	67	41	"short periods": It is not clear what short periods range. One might think it as periods of weather range (week~month) if nothing is noted. [Masaki Satoh, Japan]	Accepted, we now define the period of time.
13381	67	53	67	53	MMM must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Accepted, wording changed as suggested.
102897	67	53	67	53	SREX needs to be spelled out (first use in chapter). Are there 25 or 26 regions? [Philippe Tulkens, Belgium]	Accepted, the acronym SREX has now been removed.
13413	68	23	68	24	Mention for which period the CMIP5 and CMIP6 models show behavior similar to that of the observed ENSO. [Maria Amparo Martinez Arroyo, Mexico]	Accepted, we now directly refer to the historical period.
35691	68	26	68	26	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. This paper has now been published
26765	68	30	68	30	We suggest to replace "fluxes of heat" with "heat flux feedback" [Eric Brun, France]	Accepted, wording changed as suggested.
116243	68	34	68	34	For ENSO variability in the past centuries, please check carefully the coherency with the assessment in ch 2, 'In summary, there is medium confidence that both ENSO amplitude and the frequency of high-magnitude events since 1950 is higher than over the pre-industrial period from 1850 as far back as 1400, but low confidence that it is outside the range of variability over periods prior to 1400." [Valerie Masson-Delmotte, France]	Accepted, the wording has now been tweaked for consistency with CH2
14461	68	42			“biennial periodicity” is the correct terminology here instead of “biennial tendency” as the latter refers to a time derivative [Malte Stuecker, United States of America]	Accepted, wording changed as suggested.
35693	68	44	68	44	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted, this paper has now been published.
84197	68	44	68	44	"characteristics" is misspelled [Annalisa Cherchi, Italy]	Accepted, spelling has been corrected.
26767	68	46	68	47	There is also low confidence that a change of ENSO has been observed (cf. above). This should be reflected here as well. [Eric Brun, France]	Rejected. Medium confidence was reported for past changes. However, the ordering of the text has been revised to more clearly explain the section's assessment.
52923	68	46	68	47	or high confidence that anthropogenic forcing was not the primary driver of observed changes? [Hervé Douville, France]	Rejected. However, the wording of this paragraph has been re-ordered and revised to more clearly explain this sections assessment.
111063	68	46			the way its phrased it sounds like we expect an ENSO change in the 20th century. Given the huge enso variability variability I don’t think so (also classic wittenberg paper) - also I get the impression we arent even clear if and what sign change we would expect. would it be clearer to say Thus we have little evidence to suggest taht anthropogenic forcing... (i know low confidence is uncertainty language but it sounds like you really do think it could be forced but have low confidence - i wouldnt think so but might be wrong?) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The wording of this paragraph has been re-ordered and revised to more clearly explain this section's assessment.
105051	68	49	48	49	suggest "repored" -> "reporst" [Peter Gleckler, United States of America]	Accepted. Wording changed as suggested.
105053	68	51	68	51	"may impact the El Nino event type" [Peter Gleckler, United States of America]	Accepted. Wording changed as suggested.
26769	68	53	68	53	There is also low confidence that a change of ENSO has been observed (cf. above). This should be reflected here as well. [Eric Brun, France]	Rejected. This is present just two sentences prior, so I do not believe that it makes sense to repeat this here.
84199	69	5	69	5	it is AVI.5 [Annalisa Cherchi, Italy]	Editorial. Revised as indicated.
14459	69	6			The following reference is fitting for “often in concert with ENSO”: Stuecker et al. (2017), Revisiting ENSO/Indian Ocean Dipole phase relationships, Geophys. Res. Lett., 44, doi:10.1002/2016GL072308 [Malte Stuecker, United States of America]	Rejected. The subject of the paper is out of the scope of Ch 3. Instead this paper is now cited in Technical Annex IV, and here we cite the Annex.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4059	69	24	69	26	The work of Hirons and Turner (2018) may also be of relevance here (https://doi.org/10.1175/JCLI-D-17-0804.1, "The impact of Indian Ocean mean-state biases on the representation of the East African short rains"), in which the persistent coupled biases of the Indian Ocean easterly winds and thermocline are discussed. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The suggested paper is cited.
19805	69	55	69	55	The "low" number, for sure? [philippe waldteufel, France]	Taken into account. Rephrased to "low statistical degrees of freedom"
116245	69		69		Please check the use of the term "accelerated anthropogenic Indian Ocean warming" (exact meaning?). [Valerie Masson-Delmotte, France]	Taken into account. Rephrased to "acceleration in anthropogenic forcing"
102899	70	15	70	15	add "the" between "and" and "Atlantic Meridional" [Philippe Tulkens, Belgium]	Accepted. The word "the" has been added.
12049	70	17	70	17	"Annex A VI.5" should be "Annex A VI.6". [Masaki Satoh, Japan]	Accepted. Wrong link to section corrected.
84201	70	17	70	17	it is AVI.6 [Annalisa Cherchi, Italy]	Accepted. Wrong link to section corrected.
4061	70	19	70	22	References could be added for the monsoon teleconnections mentioned here. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Because of length constraint, references have not been added, the teleconnection being described in details in the Technical Annex. That said, a clearer mention of the technical annex is now provided for all modes in the revised version. Note also that a summary table for teleconnections has been added in the Atlas and in the Technical Summary.
2655	70	39			"tridimensional temperature gradient" needs to be defined. [Bryan Weare, United States of America]	Taken into account. This term has been replaced by "along the thermocline"
19807	70	40	69	70	Suggested additions: a comma after "5.1.1)" and a "s" at the end of "affect" [philippe waldteufel, France]	Accepted.
35695	70	53	70	53	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. This paper is now published.
3355	70		15	53	This section is fundamental, I consider, respectfully, that they carry out analysis and contributions, highlighting examples, with other disciplines, sciences, in order to give more progress to their studies, which in themselves are already very valuable and of great contribution [Eduardo Erazo Acosta, Colombia]	Noted. Thanks for the compliment.
23791	71	3			The work of Wainwright et al. (2019) may also be relevant here: https://doi.org/10.1007/s00382-019-04973-0 "The impact of air-sea coupling and ocean biases on the seasonal cycle of southern West African precipitation." The implication is that ocean-atmosphere coupling worsens existing errors in the seasonal cycle due to the role of systematic biases in SST. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added.
100865	71	4	71	4	The more recent paper Roberts et al 2020 can be included here Roberts, M. J., Camp, J., Seddon, J., Vidale, P. L., Hodges, K., Vanniere, B., Mecking, J., Haarsma, R., Bellucci, A., Scoccimarro, E., Caron, L.-P., Chauvin, F., Terray, L., Valcke, S., Moine, M.-P., Putrasahan, D., Roberts, C., Senan, R., Zarzycki, C. and Ullrich, P. (2020) Impact of model resolution on tropical cyclone simulation using the HighResMIP-PRIMAVERA multi-model ensemble. Journal of Climate, 33 (7). pp. 2557-2583. ISSN 1520-0442 doi: https://doi.org/10.1175/jcli-d-19-0639.1 [Corti Susanna, Italy]	Accepted. Reference added.
100867	71	5	71	6	for instance, Stochastic Physics has a nearly equivalent effect on the mean number and distribution of TCs (Vidale et al. 2020, submitted to J. Clim). [Corti Susanna, Italy]	Accepted. Reference added
35697	71	6	71	6	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Editorial. Reference updated.
19809	71	8	71	10	Is not a situation in which changes are not detectable although they are observed a bit paradoxical? [philippe waldteufel, France]	Taken into account. Thanks for having pointed out the awkwardness of this sentence. The sentence has been revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78851	71	12	71	12	Alaska is not to be included in the category of mid-latitude glacier ranges. Conversely, the EU Alps are missing in this category. [MONICA TOLOTTI, Italy]	Not applicable: this comment is misplaced and does not correspond to this section.
45319	71	28	72	55	I feel like there needs to be a discussion about PDV recibstryctuib. There has been a lot of work on reconstructing PDV using proxies such as tree rings (e.g. Buckley et al. 2019 doi: https://doi.org/10.1007/s00382-019-04694-4; MacDonald and Case 2005 doi: doi:10.1029/2005GL022478), lake sediment (Lapointe et al. 2017 doi: 10.5194/cp-13-411-2017), marine sediments (O'Mara et al. 2019 doi: https://doi.org/10.1029/2019GL084828). Even though reconstructions do not necessarily agree with each other (Newman et al. 2016 doi: https://doi.org/10.1175/JCLI-D-15-0508.1), I believe the disucssion isn't complete without PDV reconstruction over the Common Era [Anson Cheung, United States of America]	Rejected. Proxy-based evidence without comparison with models is out of the scope of Chapter 3. Most of the suggested papers are cited in Chapter 2.
10887	71	28			Much more care is needed to distinguish between the indices and the underlying modes of climate variability. There is discussion here of the impact of anthropogenic aerosols on SST patterns which are used in the indices. But these are not necessarily influencing the modes of variability themselves. Conversely claiming modes of variability contribute to short term surface temperature trend variations risk circular reasoning as surface temperatures are often used to indicate what those modes are doing in the first place. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised the text to reflect whether essential feedback mechanisms are operating in the forced changes.
39103	71	30	71	36	Regarding PDO and AMO, Mann et al 2020 call into question if the can really be classified oscillations. The use of PDO/PDV and AMO/AMV is not consistent througout chapter 2,3,and 4. Mann, Michael E., Byron A. Steinman, and Sonya K. Miller. "Absence of internal multidecadal and interdecadal oscillations in climate model simulations." Nature Communications 11.1 (2020): 1-9. [Ola Kalen, Sweden]	Noted. This issue is discussed in Technical Annex IV.
12051	71	36	71	36	"Annex A VI.6" should be "Annex A VI.7". [Masaki Satoh, Japan]	Editorial. The error has been corrected.
84203	71	36	71	36	it is AVI.7 [Annalisa Cherchi, Italy]	Editorial. The error has been corrected.
45317	71	43	71	46	Additional reference on climate variability and predictability: Steinman et al. (2015) DOI: 10.1126/science.1257856; Mann et al. (2016) doi:10.1002/2016GL068159 [Anson Cheung, United States of America]	Agreed. Steinmann et al (2015) is added.
78853	72	1	72	7	Split this sentence in two as too long and complex, i.e. avoid the long text in brakets. [MONICA TOLOTTI, Italy]	Not applicable. This comment is misplaced, we cannot identify what it refers to.
84207	72	22	72	22	better to specify what decades are of interest in this statement [Annalisa Cherchi, Italy]	Accepted. We have added that this period overlaps with the "hiatus" period.
105039	72	22	72	22	suggest "might project well onto…" [Peter Gleckler, United States of America]	Rejected. The projection is not large.
84205	72	25	72	25	"tropical" is misspelled [Annalisa Cherchi, Italy]	Not applicable. The sentence has been rewritten.
79273	72	26	72	27	Also cite Kuntz & Schrag (2016), https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016JD025430 [Martin Stolpe, Switzerland]	Accepted. The suggested papers is cited.
37697	72	30	72	30	There is another study investigating Indian Ocean impact on PDV: Mochizuki, T., M. Kimoto, M. Watanabe, Y. Chikamoto, and M. Ishii, 2016: Interbasin influence of the Indian Ocean on the Pacific decadal climate change. Geophys. Res. Lett., 43, 7168–7175, doi:10.1002/2016GL069940. [Masahide Kimoto, Japan]	Accepted. The suggested papers is cited.
84209	72	34	72	36	reference apparently missing. Why CMIP6 vs CMIP5 differences would suggest a greater influence from anthropogenic and natural forcing? And which of the two? Do they have the same effect? [Annalisa Cherchi, Italy]	Taken into account. Rephrased to clarify the meaning. The relative contributions of anthropogenic and natural forcings are not assessed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42701	72	36			Sentence 'However,…' Suggest adding something like the following after (Figure 3.38) – 'supporting the conclusion that the PDV is driven by internal variability.' [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence added to state the dominance of internally-driven variability, with more evidence from new literature.
84211	72	43	72	44	"of PDV" seems a repetition [Annalisa Cherchi, Italy]	Editorial. Revised as suggested.
105041	72	46	72	46	The role of sampling limitations at longer timescales could be more clear [Peter Gleckler, United States of America]	Accepted. The limitation in the spatial coverage of observations is added as an origin of difficulty in model evaluation.
13383	73	14	73	14	MME must be expanded acronym has not been used [Maria Amparo Martinez Arroyo, Mexico]	Editorial. Revised as suggested.
10889	73	22			Much more care is needed to distinguish between the indices and the underlying modes of climate variability. There is discussion here of the impact of anthropogenic aerosols on SST patterns which are used in the indices. But these are not necessarily influencing the modes of variability themselves. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thanks for this remark. In the revised version, we have added expressions such as "SST fingerprint" assessed from "SST anomalies" to make a clear distinction between the AMV phenomenon and the AMV SST index defined at the beginning
40909	73	24	73	26	The current glossary definition for AMO/AMV is "A multi-decadal (65- to 75-year) fluctuation in the North Atlantic, in which sea surface temperatures showed warm phases during roughly 1860 to 1880 and 1930 to 1960 and cool phases during 1905 to 1925 and 1970 to 1990 with a range of approximately 0.4°C." Please check if it needs updating. [TSU WGI, France]	Taken into account. The definition of the AMV has been updated in the final version
7805	73	26	73	28	I'm not sure that this sentence makes sense - it's certainly very vague. How about saying something like "The AMV is associated with many physical processes including …" [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence changed accordingly.
12053	73	32	73	32	It is not clear what the "medium performace" means, despite "low confidence". [Masaki Satoh, Japan]	Taken into account. The confidence statement has been removed
111065	73	41			there are also nice papers by Wolfgang Mueller on thismight be this one: Müller W A, Matei D, Bersch M, Jungclaus J H, Haak H, Lohmann K, Compo G P, Sardeshmukh P D and Marotzke J 2015 A twentieth-century reanalysis forced ocean model to reconstruct the North Atlantic climate variation during the 1920s Clim. Dyn. 44 1935–55 [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added
130609	73	44	73	44	The use of "piControl" is too tecknical. [Panmao Zhai, China]	Taken into account. Picontrol has been replaced by preindustrial control
84213	73	48	73	48	"composition" of what? [Annalisa Cherchi, Italy]	Taken into account. "composition" did not mean anything here. Replaced by "sea ice"
13385	73	52	73	52	remove : [Maria Amparo Martinez Arroyo, Mexico]	Noted. Typo corrected
13387	74	7	74	7	remove : [Maria Amparo Martinez Arroyo, Mexico]	Noted. Typo corrected
35699	74	10	74	11	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Taken into account: references have been updated.
10057	74	38	74	41	You are briefly mentioning solar activity as a possible driver of AMV variability. You should make it clearer here that AMV/AMO influence climate in a systematic way (e.g. summer temperatures in and around Europe, doughts in Sahel, hurricanes in Atlantic basin). See e.g. summary in Lüdecke et al. 2020: Decadal and multidecadal natural variability in European temperature, https://doi.org/10.1016/j.jastp.2020.105294. You also need to discuss in this chapter if climate models consider this solar influence in their modeling runs. If not, this could be part of the explanation why modeling performance remains pretty poor with regards to the AMO. [Sebastian Luening, Switzerland]	Taken into account. Reference suggested by reviewer added. The solar influence is also better put into perspective with another reference dealing with such an issue added in our assessment. "historical" is the CMIP6 standard name for simulations including all forcing. This is re-specified in the revised version.
116247	74		75		The statement on AMV (incl response to volcanic forcing) could be reported as a key finding in the chapter ES. [Valerie Masson-Delmotte, France]	Taken into account. The ES statement related to AMV has been revised accordingly to include all the forcings including the volcanoes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39887	75	31	75	32	"While there is some overlap between mitigation and CDR…" -> The glossary definition for mitigation is "A human intervention to reduce emissions or enhance the sinks of greenhouse gases.", so I would argue that CDR is included within the term 'Mitigation'. [TSU WGI, France]	Not applicable. This comment is not a Chap3 comment.
52925	75	38			"Synthesis across different classes of models and multiple Earth system components" could be a more representative title? [Hervé Douville, France]	Noted. This title is as agreed in the scoping document.
601	75	41	76	17	could we add some possible connection between the overestimated/underestimate metrics. For example, the possible consequence of the underestimated AMV/PDV, the overestimated impact of aerosols… the saltier Atlantic and AMOC？ [ZHIYAN ZUO, China]	Accepted. We have revised this section, pointing out some of these linkages.
52927	75	41			Start with the multivariate model evaluation (reverse 3.8.1 and 3.8.2)? [Hervé Douville, France]	Rejected. We prefer the current order.
19811	75	45	75	47	This does not seem completely exact. Many of the discussions in previous subsections (particularly 5, 6 and 7) address possible anthropogenic influences on climate change which have little to do with warming [philippe waldteufel, France]	Accepted. We replace "warming" with "climate change".
37355	75	45	75	47	The claims and assertions based on unvalidated models certainly do not strengthen any such conclusions. McLean (2014) "Late Twentieth-Century Warming and Variations in Cloud Cover" (Atmospheric and Climate Sciences) showed conclusions that are more plausible. Mind you it's difficult to be certain of anything because my audit of the temperature data - action that the IPCC hasn't taken after 30 years of using that data - shows it to be virtually certain that the historical temperature record is not correct. [John McLean, Australia]	Rejected. Evidence across variables and climate domains in many cases point towards the human influence. The IPCC temperature record is not plainly "incorrect" -- rather, the latest version shows slightly larger trends than the previous version for GMST.
2659	75	48			The summary Fig. 3.40 is never directly discussed. In fact moisture and snow cover, which are discussed, are not in the figure. The text and figure need to be much better integrated. [Bryan Weare, United States of America]	Accepted. We have revised the discussion of the figure.
21533	75	50	76	4	Missing here are any of the biospheric statements. Given that you assessed these they are surely worth mentioning here and help build confidence? [Peter Thorne, Ireland]	Accepted. We now include a sentence on biospheric changes.
127343	75	52	75	54	Mention global mean sea level change? [Trigg Talley, United States of America]	Accepted. We now mention SLR.
127345	75	52	77	45	"The observed warming trends in the atmosphere, ocean and at the surface over the past 65 years are only explained when contributions from both anthropogenic and natural forcings are included." This sentence is somewhat inaccurate. Natural forcings have sharp short-term spikes associated with volcanic events and some TSI-related variability, but by-and-large the inclusion of natural forcings has relatively little impact on the resulting long-term trends (vs. anthropogenic-only forcing runs), at least at a global scale (and in most regions per Figure 3.40, where CMIP6 natural-only simulations are largely trendless). Suggest rewording this a bit, perhaps removing the word "trends" and just saying "the observed warming in the atmosphere..." [Trigg Talley, United States of America]	Accepted. We now state that long-term trends in these variables are only explained when anthropogenic influences are taken into account.
6627	75	53	75	53	Is it really as long as 65 years? There is not a lot of separation from 1955 to 1975 or thereabouts in the global plot in Figure 3.8, and it is clearly not 65 years for some regions. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased this to remove the specific reference to 65 years. The specific single-variable statements of earlier sections all refer to specific time periods, but here this reference is not needed as it is based on these single-variate statements.
4721	75	56	76	1	References required [Ibikunle Olaleru, Nigeria]	Noted. This paragraph is a summary of statements arrived at earlier in the chapter. This is now made explicit. Supporting references are generally in these earlier sections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
108085	75		76		Category 3.8.1. Multivariate Attribution of Climate Change [Asylbek Aidaraliev, Kyrgyzstan]	Noted.
83367	76	3	76	4	Please change text to: "There continues to be low confidence in the scientific understanding of observed regional and seasonal changes in Antarctic sea-ice coverage". [Robert Massom, Australia]	Accepted. We adopt the reviewer's suggestion.
52929	76	6	76	9	Would it be also feasible to highlight potentially consistent underestimation of observed climate changes across multiple variables (or components) which may deserve more attention and a multivariate analysis framework? [Hervé Douville, France]	Noted. This might be feasible but would require more original research. This would need to be grounded in literature that we think does not exist.
37353	76	6	76	17	Such chutzpah! You have shown NO evidence whatsoever. Models can't provide evidence if they haven't been validated (and don't you agree that no climate models have been validated?) Correlations also aren't evidence of cause. [John McLean, Australia]	Rejected. In the preceding sections there is plenty of evidence to back up this statement. Models have generally been validated. We agree that models are not perfect. This is reflected in our general usage of confidence and uncertainty language. So no chutzpah.
19507	76	8	76	8	emphasis on "virtually certain" is so important in this part [Hamideh Dalaei, Iran]	Noted. In response to comment 21531 this is now strengthened to "unequivocal", repeating a phrase used in AR5. We agree it's important.
21531	76	8	76	9	I'm not sure that this isn't overly conservative. So many very likely / likely / extremely likely all being wrong seems impossibly implausible. Is this really a likelihood rather than a fact based statement? [Peter Thorne, Ireland]	Accepted. We now state that it is unequivocal that human influence has warmed the global climate system.
127347	76	8	76	9	Is this statement intended to be only about GMST increase, as implied by current phrasing, or about multivariate climate changes? [Trigg Talley, United States of America]	Noted. GMST is not mentioned in the statement. We mean that due to human causes, more heat is retained in the climate system, most of it actually in the ocean.
50713	76	11	76	17	This assessment would benefit from a sentence summarising the consequences of these developments e.g. is human influence on the climate system now an established fact? This would ensure it is consistent with other chapters, for example Chapter 7 Exec Summary p7 L31 which states that "It is unequivocal that human activity has had a warming effect on the Earth since 1750." This assessment should then be elevated to the Executive Summary of this chapter. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now also use the term "unequivocal" for global warming, here and in the ES.
19813	76	20	76	31	While Figure 3.40 is beautiful, WG1 will have to be careful with the setting and resolution, since in the document for expert review some features and characters are not readable. [philippe waldteufel, France]	Accepted. We have worked on the presentation of figure 3.40, making it more intelligible. The SOD had a low-resolution version of the figure; that is changing in the FGD.
105123	76	34	76	34	In this section, we are missing multi-period multi-variate assessements: assessments on present + several past periods, and maybe a summary figure showing the progress from PMIP2-3-4 in parallel to CMIP3-CMIP5-CMIP6, which could also serve for an executive statement (no statement on ability of models to simulate past climates!) and the technical summary, which doesn't include much information on paleo... [Masa KAGEYAMA, France]	Accepted. We now have a revised figure 3.43 which details large-scale model results from PMIP3 and PMIP4. Most results for periods other than the MH and LGM are not mature enough for a multivariate assessment, but we now have panels for the performance of GSAT in five periods. There also now is a statement in the Executive Summary on the simulation of GSAT during selected paleo-periods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102901	76	43	77	50	Ecosystem feedbacks are only insufficiently addressed in most of the models. [Philippe Tulkens, Belgium]	Noted. The reviewer is correct that climate models don't include a full description of ecosystem responses. Many models include a simplified treatment though, in the context of simulating the carbon cycle. Some of these variables are covered in panel (b) of figure 3.41.
71911	76	43	80	13	A more important question than have the models improved is are they fit for purpose. This section does not address that as such. I would have thought a more appropriate way to address this question would have been to have this correlation assessment at the start of the chapter, and then address the indivuual phenomena (as in earlier sections) addressing both the model adequacy and the human influence question. [John Church, Australia]	Noted. Duplicate comment. See comment 127349.
127349	76	43	80	13	A more important question than have the models improved is: Are they fit for purpose? This section does not address that as such. A more appropriate way to address this would have been to have this correlation assessment at the start of the chapter, then address the indivuual phenomena (as in earlier sections) addressing both the model adequacy and the human influence question. [Trigg Talley, United States of America]	Noted. The reviewer is right that fitness-for-purpose is an important concept. AR6 is elevating this concept prominently, with an introduction in Chapter 1 and assessments of fitness for specific purposes in every chapter that used climate models. We are not in a position, at this late stage of the review process, to follow the reviewer's suggestion to re-order Ch3. As is laid out in section 3.1, the concept of fitness-for-purpose is specific to every chapter of the report (particularly those that deal with projections). Here we lay the ground work for these assessments of fitness-for-purpose, through a multi-variate model evaluation. Ch3 is a bit different in that it is backwards-looking. We therefore refer the reviewer to other chapters for more in-depth discussions of this concept.
127351	76	54	75	54	Just two reference datasets? One would expect in many case (e.g., surface temperature) that there are more than two used for model/observation comparisons. [Trigg Talley, United States of America]	Noted. Indeed the reviewer is correct that in some cases there are more than two reference datasets. Using all of these would however yield an unmanageably complex figure. We refer the reviewer to earlier sections of this chapter where for individual fields sometimes more than two reference datasets are used. In almost all cases, the inter-model differences, highlighted in this plot, are larger than the differences between these datasets, anyway.
108087	76		76		Category 3.8.2. Multivaruate Model Evaluation [Asylbek Aidaraliev, Kyrgyzstan]	Not applicable. It is unclear what change the reviewer wants to see.
6629	77	6	77	6	Should a word such as "modest" be added before "incremental progress". If the increments are big enough, incremental progress is fine. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We follow the reviewer's recommendation.
127353	77	9	77	9	Should be "is expended in the AR6 to the..." [Trigg Talley, United States of America]	Accepted. We follow the reviewer's recommendation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68073	77	20	77	50	I'm not sure the conclusion can be made: progress from CMIP5 to CMIP 6. For one thing, there are too few CMIP6 model families relative to CMIP5. And since the models in CMIP5 are related to those in CMIP6, a paired analysis of differences, pairing between model evolutions, would be needed. Hopefull this becomes clearer as the rest if the CMIP6 results are available? [MIchael Evans, United States of America]	Noted. For the FGD there are indeed more models available (i.e. fewer white patches in fig. 3.41) which prove quite clearly that as a group, the CMIP6 models compare better to the observational references than the CMIP5 models.
102903	77	26	77	26	"are not critical" - unclear, please rephrase [Philippe Tulkens, Belgium]	Accepted. We have rephrased the sentence.
19815	77	41	78	42	Is the multi-model mean included in figure 3.41? [philippe waldteufel, France]	Accepted. The new version of the figure includes the multi-model means for the three CMIP generations.
35701	77	42	77	43	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. The paper has been accepted in JGR.
127355	77	45	77	45	Should be "Regarding performance for the ocean an the cryosphere" as "regarding... with regards" is a bit redundant. [Trigg Talley, United States of America]	Accepted. We have rephrased the sentence.
19817	77	53	78	13	Figure 3.41 appears to offer a relative comparison; in other words the issue is how the models compare among themselves through time and generations, rather than assessing their absolute performance. Information concerning both the spelling out of the variables and what the alternative datasets are should be given. [philippe waldteufel, France]	Accepted. The reviewer is right that figure 3.41 offers a relative assessment of model performance across generations of multi-model ensembles. We think this is clear from the description. We now provide a link to a table expanding the names of the variables and defining the observational datasets used here. Putting all this information into the caption would make it unreadable, we feel, and would run against the gist of this section (which is to provide a multivariate, high-level overview of model performance where the precise details of the variables are secondary in importance).
116249	77		77		For the performance / Antarctic sea ice, please check coherency with ch 9 (also on missing processes, eg freshwater flux from ice shelves etc). [Valerie Masson-Delmotte, France]	Accepted. We have compared Ch9 and the paragraph here on sea ice. There is no conflict. We have added ice shelves as a possible contributing factor. However, the brief analysis conducted here cannot conclusively determine the causes of the differences in performance.
78855	78	1	78	4	This part may be reinforced (more detail) in relation to its crucial relevance for future development, and in contrast to possible future development of surface ice. And, what about zones with discontinuous permafrost? [MONICA TOLOTTI, Italy]	Not applicable. This comment is misplaced, we cannot identify what it refers to.
35703	78	10	78	10	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. The paper has been accepted in JGR.
78857	78	11	82	14	The section 9.5.3. is focussed on changes in the Northern Hemisphere, without much detail on the polar region. The Southern Hemisphere, including the Antarctica, is strongly underrepresented. Despite the scarcity of data and models, I suggest to try to balance the section by including more details on the mentioned underrepresented regions. For the rest, the whole section 9,5 on terrestrial cryosphere is very well written and exhaustive. [MONICA TOLOTTI, Italy]	Not applicable. This omment wrongly assigned to chapter 3 so we cannot address this point.
19819	78	27	78	39	Figure 3.42: table 5 cannot be found. Again the reference to alternate (concerning figure 3.41 it was "alternative") datasets is obscure. [philippe waldteufel, France]	Accepted. We replace "table 5" with "table 1 of the Technical Annex on Observations" as in figure 3.41, and replace in figure 3.41. "alternative" with "alternate".
35705	78	36	78	37	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. The paper has been accepted in JGR.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23793	78	42	79	19	With reference to the whole discussing on benchmarking starting p78 line 42 and specifically the sentences here (page 79, lines 15-19): see earlier comment regarding Roberta D'Agostino's work. Can we be confident that palaeoclimate periods are useful analogues for the type of forcing that is changing in the 20th century? https://doi.org/10.1029/2018GL081589 "Northern Hemisphere Monsoon Response to Mid-Holocene Orbital Forcing and Greenhouse Gas-Induced Global warming" D'Agostino et al. (2019) would argue that since the mechanisms of monsoon change are different when considering orbital configuration versus GHG, then the mid-Holocene is not necessarily a suitable analogue. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We do not claim that the mid-Holocene is an "analogue" for the present-day situation. Rather, due to differences in orbital and GHG forcing, the MH does provide (as claimed) an out-of-sample test of climate models. In lines 15-19 we compare the MH to the preindustrial climate. This is a valid comparison, with differences due to orbital forcing.
2105	78	43	78	43	"more" than what? Maybe just remove "more". [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We follow the reviewer's recommendation.
52943	78	44	78	45	Yet, differences in model performance at simulating past climates are only weakly related to present-day model biases or to simulated climate sensitivity under enhanced CO2 concentration (Harisson et al. 2015). [Hervé Douville, France]	Accepted. Despite Harrison et al. (2015), model validation remains a major tool to build confidence in model projections. Reservations like the one expressed by the reviewer (which we appreciate now much better than at AR5) are behind the decision, elsewhere in AR6, to use multiple lines of evidence rather than pure model output in determining most likely warming trajectories and the ECS/TCR. We have added a sentence to this effect to the following paragraph.
2103	78	47	78	48	State why they aren't included. You could add the Last Millenium to this list too. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Analysis of these periods is less advanced than for the MH and the LGM, but we now include panels for GSAT covering all PMIP periods.
102905	78	47	78	48	why is it important to mention the other periods here if tf they are not discussed? "but are not discussed here." could be deleted. [Philippe Tulkens, Belgium]	Noted. Analysis of these periods is less advanced than for the MH and the LGM, but we now include panels for GSAT covering all PMIP periods.
127357	78	47	78	48	To the extent the literature permits, it'd be valuable to assess the LIG and mPWP, since they represent periods of elevated GMST. [Trigg Talley, United States of America]	Noted. Analysis of these periods is less advanced than for the MH and the LGM, but we now include panels for GSAT covering all PMIP periods.
2107	78	56	79	2	The LIG and mPWP are mentioned here, but above it says that these two time periods are not discussed. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Analysis of these periods is less advanced than for the MH and the LGM, but we now include panels for GSAT covering all PMIP periods.
105117	79	6	79	6	Kageyama et al, 2018, should be replaced by Kageyama et al 2017: Kageyama, M., Albani, S., Braconnot, P., Harrison, S. P., Hopcroft, P. O., Ivanovic, R. F., Lambert, F., Marti, O., Peltier, W. R., Peterschmitt, J.-Y., Roche, D. M., Tarasov, L., Zhang, X., Brady, E. C., Haywood, A. M., LeGrande, A. N., Lunt, D. J., Mahowald, N. M., Mikolajewicz, U., Nisancioglu, K. H., Otto-Bliesner, B. L., Renssen, H., Tomas, R. A., Zhang, Q., Abe-Ouchi, A., Bartlein, P. J., Cao, J., Li, Q., Lohmann, G., Ohgaito, R., Shi, X., Volodin, E., Yoshida, K., Zhang, X., and Zheng, W.: The PMIP4 contribution to CMIP6 – Part 4: Scientific objectives and experimental design of the PMIP4-CMIP6 Last Glacial Maximum experiments and PMIP4 sensitivity experiments, Geosci. Model Dev., 10, 4035–4055, https://doi.org/10.5194/gmd-10-4035-2017, 2017. [Masa KAGEYAMA, France]	Accepted. We have replaced the reference.
2109	79	7	79	8	This is a little unclear to me. How about: "Both the mid-Holocene and the LGM climates have been a part of either AMIP or CMIP through several assessment cycles, and as such serve as references to quantify model-data agreement from one IPCC assessment to another". [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We adopt the reviewer's suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68075	79	7	79	8	"Both the mid-Holocene and the LGM climates have been continuously modeled with AMIP to CMIP models 8 and serve as references to quantify model-data agreement from one IPCC assessment to another.": this needs a brief rationale: why is the mid-Holocene and important hindcasting target, for detection and attribution? [MIchael Evans, United States of America]	Accepted. Details about these two periods are given in Cross-Chapter Box 2.1. Essentially, these are the two periods with the most consistent simulations available spanning CMIP3, 5, and 6.
105757	79	10	79	10	The number of models in Brierley et al (submitted) has been increased to 17 during revisions. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have corrected the statement accordingly.
2111	79	10	79	19	This paragraph needs to make it a bit clearer what the observations show, so it is clearer whether the changes described for the models are a "good" thing or not. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased this paragraph, making this more transparent.
68863	79	10	80	13	The conclusion of “low confidence” in the ability of models to simulate MH and LGM climate at regional scale contrasts with AR5, which concluded that models reproduce large-scale patterns climate change during the LGM and MH. Understandability, simulations at the “regional scale” are less robust than for “large-scale”, but the AR5 and the AR6 messages about paleoclimate model performance are shockingly different. CH3 should focus on “large scale” (continental and larger) climate because it is the remit of CH2-CH3-CH4 as scoped. [Darrell Kaufman, United States of America]	Accepted. Taking into account more analysis of PMIP results, and especially considering the simulations of GSAT for these paleo-periods, the assessment is now more consistent with AR5.
105759	79	12	79	12	The subclause of "albeit with weaker cooling for Northern Hemisphere winters" is strange. This is not something our paper highlights. I recommend replacing with "albeit with a slight cooling related to the use of observed, lower CO2 concentrations". [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased the sentence.
13389	79	15	79	15	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted. This is fixed in the FGD.
105761	79	15	79	15	This sentence about the monsoon expansion is missing the clarification that these are for the Northern Hemisphere only - the southern monsoons are more equivalent. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have added this information.
105763	79	17	79	18	This sentence would benefit from a citation of both Fig 3.11 (before the comma) and then Fig 3.43 at the end. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have inserted these references.
13391	79	22	79	22	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted. This is fixed in the FGD.
2113	79	22	79	26	This sentence is a bit long and confusing (and I would use "models" at the end instead of "ones". [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have broken up and rephrased this sentence.
2115	79	26	79	29	It is not clear if these changes are in agreement with data or not….I feel that only variables for which we have observations should be discussed here. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The changes in oceanic circulation are large enough to warrant mentioning here, although we agree that ocean circulation is generally poorly covered by paleo-data. Restricting this part to observations only would limit considerably what can be said here about oceanic changes in those paleo-periods. Note also comment 73855 requesting a more in-depth discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73855	79	27	79	29	Changes in the depth of LGM AMOC gives important information on understanding the variability of carbon cycle as well as other materials in the past, which is also important for the future. In Chapter 2 P70L46-48, changes in the depth of the LGM AMOC is described from the proxy side, however, I couldn't find a similar description from the modeling side. In the context of PMIP, many efforts have been made in simulating and understanding the changes in the depth of LGM AMOC with comprehensive climate models. I think, it is important to describe the results from PMIP LGM experiments on the depth change. Below shows one example of modifying a sentence in Chapter 3 page 79 line 28-29."Most models in PMIP3-CMIP5 and PMIP4-CMIP6 simulate a stronger and deeper LGM AMOC compared to PI (Muglia and Schmittner 2015, Sherriff-Tadano et al. 2018, Kageyama and PMIP4, submitted), though some models show a shoaling of LGM AMOC (Marzocchi and Jansen 2017, Sherriff-Tadano and Abe-Ouchi 2020), which is consistent with reconstruction data." Marzocchi, A. & Jansen, M. F. Connecting Antarctic sea ice to deep-ocean circulation in modern and glacial climate simulations. Geophysical Research Letters 44, 6286-6295, doi:10.1002/2017gl073936 (2017).; Muglia, J. & Schmittner, A. Glacial Atlantic overturning increased by wind stress in climate models. Geophysical Research Letters 42, 9862-9869, doi:10.1002/2015gl064583 (2015).; Sherriff-Tadano, S., Abe-Ouchi, A., Yoshimori, M., Oka, A. & Chan, W. L. Influence of glacial ice sheets on the Atlantic meridional overturning circulation through surface wind change. Climate Dynamics 50, 2881-2903, doi:10.1007/s00382-017-3780-0 (2018).; Sherriff-Tadano, S., and Abe-Ouchi, A. Roles of sea ice-surface wind feedback in maintaining the glacial Atlantic meridional overturning circulation and climate. Journal of Climate, https://doi.org/10.1175/JCLI-D-19-0431.1 <https://doi.org/10.1175/JCLI-D-19-0431.1> (2020). [Takashi Obase, Japan]	Accepted. We thank the reviewer for these suggestions and changed the sentence accordingly.
105121	79	29	79	29	There might be some reason for this unsatisfactory response of the NADW for the LGM. We will try to include it in the revised version of the paper [Masa KAGEYAMA, France]	Noted. Thanks a lot Masa for your help as a contributing author.
2117	79	36	79	37	This sounds like a bit of an anticlimax to this section - either elaborate or remove this short paragraph. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have removed this short paragraph.
2121	79	42	79	42	Figure 3.43: (and equivalent text), ther should be more clairty around the relationship between CMIP5/CMIP6 and PMIP3/PMIP4. If my understadning is correct, then a PMP4 model is not necessarily a CMIP6-class model, and may be a CMIP5-class, or even CMIP4-class model. Maybe there needs to be some differentiation between a PMIP4 *simulation* (could be carried out by any model) and a CMIP6 *model* (which has to be CMIP6 class). [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. PMIP4 is indeed using CMIP6 as well as other models. In figure 3.43 we now distinguish between these two classes of models. Ignoring the other models would substantially reduce the amount of available information, in some cases.
2123	79	42	79	42	Figure 3.43: Could add the PMIP3 and PMIP4 ensemble mean, so that ensemble improvement (or otherwise) can be assessed. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now additionally display the multi-model mean.
35707	79	54	79	55	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Both references are now published.
2119	80	11	80	12	I find that a little hard to believe…there must be SOME regions where the model-data agreement is good for both LGM and MH, even if those regions are quite small….and see MAP in N America in Figure 3.43. [Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now make clearer that this statement refers to the validation of PMIP4 simulations against reconstructions, and the sentence refers to the lack of simultaneous progress across all variables and both regions for which reconstructions are available for all three variables.
2665	80	13			Spell out MH and LGM [Bryan Weare, United States of America]	Accepted. We now spell out these acronyms.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68077	80	16	82	4	Is there somewhere else in the report in which the differences between CMIP5 and CMIP (structural, resolution) will be described? For instance, I learned that ice sheets are modeled within CMIP6 simulations; this seems like a big advance; how does this affect results? Comparison with sea level chgange estimates? [MIchael Evans, United States of America]	Noted. This is discussed in Section 1.5.3. We note that with the exception of model variants participating in ISMIP, CMIP6 models generally do not feature interactive ice sheets, and sea level projections discussed in this chapter are not primarily based on CMIP6 simulations, for the component due to ice sheet melt. They are for the component due to thermal expansion of the ocean.
72071	80	16	83	9	One relevant reference could of study that showed low-frequency variability in the tropical pacific oceans can emerges from the westward extended ENSO variability in the tropical Pacific. This spurious centennial scale variability can alter historical trends and futrue projections, therefore, caution intepretations are required. -- Samanta, D., Karnauskas, K. B., Goodkin, N. F., Coats, S., Smerdon, J. E., & Zhang, L. (2018). Coupled model biases breed spurious low frequency variability in the tropical Pacific Ocean. Geophysical Research Letters, 45(19), 10-609. [Samanta Dhrubajyoti, Singapore]	Noted. We agree with the reviewer that this is an interesting study. We don't quite see how discussing it here would fit. We now cite this study elsewhere in the report, in the context of ENSO.
52931	80	16			A more representative "hierarchy" of model uncertainties could start with structural and parametric uncertainties in the physical components, then model resolution and new Earth system model components? [Hervé Douville, France]	Noted. We agree with the reviewer that this would be a possible sequence for a discussion. Unfortunately structural and parametric uncertainties are a very big topic that we can't even begin to discuss in any detail in this chapter. It would also require a completely new, non peer-reviewed section. This is beyond what we can deliver at this late stage of the review process. Also the focus here is on model performance, not on an in-depth analysis of causes of performance issues. (Some such analysis is the subject of subsequent chapters of AR6.)
71401	80	16			I find this section too important to be buried as a 2nd level subsection. Shouldn't this be moved to a higher level, and maybe even to 3.2? In any case, I would also link to the adequacy for purpose section in Chapter 1. You could also highlight the relevance of getting these processes right for the simulation of regional climate, and link to Chapter 10.3. [Douglas Maraun, Austria]	Accepted. While changing the structure of the chapter is beyond what we can do at this late stage of the review, we now address the fitness-for-purpose aspect, linking to Ch2, 1 and 10. Position in the chapter and depth of indexing are also no indications of the importance of various bits of text in this chapter.
13393	80	39	80	39	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted. This is fixed in the FGD.
13395	80	44	80	44	remove : [Maria Amparo Martinez Arroyo, Mexico]	Accepted. This is fixed in the FGD.
35709	80	53	80	53	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Bock et al. (2020) has been accepted.
116251	80		82		I would expect to find a specific assessment in section 3.8.2.2 about HighresMIP performance; about insights from the evaluation aspects related to models that have high sensitivity in CMIP6 (to link to ch 7 also related to state dependent feedbacks). Based on the assessment related to model data comparisons, attribution, does chapter 3 has messages to share on issues of emergent constraints? On fit for purpose and confidence (or lack of confidence) for specific aspects, on which ch 4, ch 10 and others can build? This handshake between the global picture and regional information can be improved. [Valerie Masson-Delmotte, France]	Part 1: Accepted. We now provide an assessment of the results of HighResMIP (updated to account for post-SOD findings). We have added a sentence on how high-sensitivity models compare versus lower-sensitivity models. Part 2 on emergent constraints: Rejected. The emergent-constraints topic is in Chapters 4 and 7. This topic is out-of-scope for Ch3. Part 3: Accepted. We have worked with Ch10 to improve the handshake between Ch10 and Ch3 on large-scale versus regional aspects of climate change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35711	81	2	81	3	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Roberts et al 2020 has appeared in JAMES.
35713	81	5	81	5	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Bock et al. (2020) has been accepted.
35715	81	10	81	10	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. The paper has appeared in GRL.
100839	81	10	81	10	The role played by increasing resolution in the simulation of tropical cyclones is highlighted also in a recent 2-model study by Vidale et al. 2020 (Vidale PL, and co-authors in submission to the J. of Climate) [Corti Susanna, Italy]	Accepted. We now cite Vidale et al. (2020).
35717	81	15	81	15	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Schiemann et al. (2020) has now appeared.
100799	81	15	81	15	About blocking frequency improvement in HighresMip models: results consistent with Schiemann et al. 2020 are shown in Fabiano et al. 2020 (figure 10) for Scandinavian blocking using a different diagnostics (Fabiano, F., Christensen, H.M., Strommen, K. et al. Euro-Atlantic weather Regimes in the PRIMAVERA coupled climate simulations: impact of resolution and mean state biases on model performance. Clim Dyn (2020). https://doi.org/10.1007/s00382-020-05271-w [Corti Susanna, Italy]	Accepted. We now cite Fabiano et al. (2020).
52933	81	19	81	23	There are many cases where "equally" could be replaced by "more", at least in the range of horizontal resolution explored by CMIP6 models. This brief paragraph could be shifted at the beginning of the section and link to other chapters (including Section 8.5.1), before focusing on model resolution and biogeochemical aspects. [Hervé Douville, France]	Accepted. We have moved the paragraph to near the top of the section, and now only list resolution as one of several model aspects that affect performance. We also now refer to section 8.5.1.
100869	81	19	81	23	The inclusion of Stochastic Physics parameterization to represent unresolved scales has an effect similar to that of the increased resolution for the simulation for example of tropical cyclones (Vidale et al. 2020 J CLim Submitted) [Corti Susanna, Italy]	Accepted. We now cite Vidale et al. (2020).
13415	81	25	81	34	It's recommended to mention which biogeophysical components have been considered or wh ich is the advance of their incorporation in the climate models, since the combination of com ponents of the biogeochemical and biogeophysical cycles determine the characteristics of the climate at a global and regional scale. [Maria Amparo Martinez Arroyo, Mexico]	Accepted. We now mention three of these without going into detail (which is beyond the scope of the chapter). For such details, we refer the reviewer to the cited literature.
35719	81	26	81	26	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Danabasoglu et al. (2020) is now published.
35721	81	29	81	29	Use published sources [Carlos Antonio Poot Delgado, Mexico]	Accepted. Mulcahy et al. (2020) has been accepted in GMD.
108009	81	29	81	34	Coupled model with feedbacks are exceedingly useful for climate intervention (SRM) research, and should be mentioned amongside the mitigation and management approaches listed. [Kelly Wanser, United States of America]	Accepted. We now mention (without going into any details) "geoengineering" as a research dimension for ESMs.
102907	81	30	81	30	The paragraph speaks of 'potentail future feedbacks'. Feedback loops are already engaged, including related to factors mentioned (land use and terrestrial carbon). Please clarify whether the developments mentioned relate to the representation of feed-backs that already exist, just not represented in the models, or feedback phenomena that are expected to emerge only in the future (and what those may be). To the extent it is the former, a discussion on the implications would be important. [Philippe Tulkens, Belgium]	Accepted. The reviewer is right that those feedbacks exist also in the past and influence the present. The paragraph mentions that some developments target performance issues in simulations of the past. We have removed "future" to address the comment.
70819	81	31	81	31	An overview can be found here: Pongratz et al., 2018 10.1111/gcb.13988 [Karlheinz Erb, Austria]	Accepted. We now cite this paper.
102909	81	31	81	32	Replace "managed land-use change" with "changes in land management". Management changes are unlikely to be limited to "land-use change". Other types of change (e.g., an eventual intensification of forest harvest without involving land-use change) are likely to happen and their impacts on GHG fluxes and other factors can be comparable. [Philippe Tulkens, Belgium]	Accepted. We follow the reviewer's recommendation.
13397	81	33	81	34	Missing or extra () [Maria Amparo Martinez Arroyo, Mexico]	Accepted. We have inserted ")".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45793	81	47	81	47	The acronym "ECS" should be reserved for "Equilibrium Climate Sensitivity", in line with the use in other chapters. Note that in Andrews et al. "EffCS" is used for "Effective Climate Sensivitiy". It should be explained why it is the effective sensitivity that is mentioned here, and why "Effective Climate Sensitivity via changes in regional surface albedo" is an example of "future interactions across the full Earth system". [Twan van Noije, Netherlands]	Accepted. See comment 21537. We adopt "Equilibrium Climate Sensitivity" for consistency with the rest of the report. The surface albedo effect comes about because CO2 fertilization changes reflected SW radiation, i.e. vegetated land gets darker, the albedo feedback. We have rephrased the sentence to make this clearer.
21537	81	47	81	48	Do you mean Equilibrium climate sensitivity? If not you should avoid using an acronym - ECS - associated throughout the remainder of the report with such a metric. [Peter Thorne, Ireland]	Accepted. We mean "equilibrium climate sensitivity". "Effective climate sensitivity" is used by Andrews et al. 2019 to mean Equilibrium Climate Sensitivity as estimated from a non-equilibrium system.
88167	81	50	81	50	Revise "permafrost" to "permafrost thaw" which better reflects the process which needs to be incorporated into models ("permafrost changes" could also be used which is more inclusive and refers to aggradation and degradation of permafrost). [Sharon Smith, Canada]	Accepted. We follow the reviewer's recommendation.
21539	81	54	81	54	Stating categorically that higher resolution alone improves aspects seems at odds with a number of more nuanced findings that were made in preceding sections of the chapter. Is this strong a statement without equivocation really supported by all the preceding sections of the chapter? If so, do you need to revisit those preceding sections? [Peter Thorne, Ireland]	Rejected. The full sentence states (we think in balance with earlier sections of this chapter) that indeed "aspects improve.. but discrepancies remain and there are some regions where currently attainable resolution produces inferior performance (high confidence). Such model behaviour can indicate deficiencies in model physics that are not simply associated with resolution". This is clearly not the categorical improvement that the reviewer seems to have understood here.
111067	81	55	82	1	I don’t understand this sentence - inferior performance in what sense? How do you know that higher resolution would be better if its not doable? Confusing sorry [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We don't know what unattainable resolution would do to model performance. We only state that feasible resolution increases do not yield unequivocal improvement, and think the sentence as stated (adding a clarification that this is about a comparison between high- and regular-resolution models) is clear.
52935	82	4			but still does not warrant an improved simulation of the physical components of the climate system? [Hervé Douville, France]	Accepted. Improvements in the simulations of physical components are always warranted, and in some cases such physical-component limitations also limit ESM performance. We have added a half-sentence that these physical limitations are also present in ESMs.
34873	82	7	83	1	This section describing factors limiting the SOD assessment is very welcome, and seriously questions the degree of confidence used in many of the SOD conclusions. Please see general comment #15 above. [Jim O'Brien, Ireland]	Not applicable any more. The entire section has been removed.
102911	82	7	83	9	When describing the limits to the assessment, the limitations of the representation of ecosystem processes, including response to management (both historic and current, not limited to "land-use change), should be elaborated in more detail (beyond a single mention of "land-use changes"). [Philippe Tulkens, Belgium]	Noted. We added few lines about the limitations associated with the representation of ecosystem processes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102913	82	7	83	9	When describing the limits to the assessment, the limitations of the representation of ecosystem processes, including response to management, should be elaborated in more detail (beyond a single mention of "land-use changes"). [Philippe Tulkens, Belgium]	Not applicable any more. The entire section has been removed.
7243	82	7			It is suggested to include review of all of the “Low Confidence” statements within this Section. [Asaad Irawan, Indonesia]	Not applicable any more. The entire section has been removed.
21541	82	7			I was surprised to see that the multi-model ensemble represents an ensemble of opportunity was not raised here along with inter-related families making an assessment across the ensemble non-facile. The spread may also be under-dispersive with implications for aspects of teh assessment. This is alluded to directly or indirectly in several preceding sections and I would expect to see a paragraph addressing this aspect here. Maybe the final paragraph in the section is trying to get at aspects of this, but if so my feeling is that it could be substantively redrafted to do so much more directly. Similarly early in the chapter a number of challenges around D&A were outlined but I don't think I see all of these covered here. I fear that overall this section is misbalanced in overly bemoaning issues around the observational evidence basis availability and maybe needs rebalancing to also note deficiencies in the available model ensemble? [Peter Thorne, Ireland]	Not applicable any more. The entire section has been removed.
6631	82	12	82	12	Typos: "assessment" and "limited". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable any more. The entire section has been removed.
52635	82	12	82	12	"the limied length" should be replaced with "the limited length" [Nazan AN, Turkey]	Not applicable any more. The entire section has been removed.
67721	82	12	82	12	limied --> limited? [Hiroaki Kondo, Japan]	Not applicable any more. The entire section has been removed.
112671	82	12	82	12	"Firstly" into "In First place" or "First",. [Melissa Jiménez Gómez Tagle, Germany]	Not applicable any more. The entire section has been removed.
2667	82	12			limited [Bryan Weare, United States of America]	Not applicable any more. The entire section has been removed.
28795	82	12			limied --> limited [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable any more. The entire section has been removed.
50715	82	12			typo: "limited length" instead of "limied length" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable any more. The entire section has been removed.
83369	82	17	82	18	Why is it stated that the observational record of Antarctic sea ice extent (that dates back to 1978) is short? [Robert Massom, Australia]	Not applicable any more. The entire section has been removed.
19821	82	27	82	30	Very true. Available data will not, for decades, give access to decadal/multi-decadal internal variability. It remains to recognize that nevertheless the numerical simulation tools make, more and more, a splendid job in most major aspects of climate change, and to demonstrate/understand how these performances can coexist with the practical inability to progress concerning internal variability at multi-decadal scales. Possibly, the answer will depend whether one looks, beyond global results, for regional climate projections. [philippe waldteufel, France]	Not applicable any more. The entire section has been removed.
7807	82	30	82	33	Do you mean stronger AMOC variability? AMOC strength changes have not been mentioned [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable any more. The entire section has been removed.
52937	82	30	82	33	Cf. former suggestion about Fig. 3.5 which could include a scatterplot of GSAT variability versus AMOC or AMV amplitude? [Hervé Douville, France]	Not applicable any more. The entire section has been removed.
2669	82	33			Spell out AMV and AMOC [Bryan Weare, United States of America]	Not applicable any more. The entire section has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111069	82	46			discussion of uncertainty in aerosols: this could more clearly say that the very diverse response of climate models to aerosol forcing particularly spatially makes it hard to constrain aerosols - this is I think in the Gareth Jones papers but also in Schurer et al 2018 where the aerosol response is so diverse even in hemispheric gradient [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable any more. The entire section has been removed.
116253	82		82		For the first paragraph, there are paleoclimate data available that show that the contrasted regional trends for the Antarctic sea ice were probably already at play for the whole 20th century (check with SROCC and ch 2). The aspect related to the realism of forcings in CMIP5 and CMIP6 models needs to be addressed, at the start of the chapter, in coordination with Ch 2 and Ch 7. [Valerie Masson-Delmotte, France]	Noted. We do not wish to extend the validation of sea ice trends in CMIP model simulations further back than 1979 because of large uncertainties in these reconstructions noted in Ch2, and also because of a lack of literature extending the analysis of SIE back further than 1979.
108089	82		83		Category 3.9 Limits to the Assessment: [Asylbek Aidaraliev, Kyrgyzstan]	Noted. It is unclear whether the reviewer is suggesting any change.
32671	83	1	180	55	From page 83 the titles don`t have any number. (Cross chapter- Bo 3/1) [sadegh zeyaeyan, Iran]	Noted. This is the format of Cross-Chapter Boxes.
33001	83	1	180	55	From page 83 the titles don`t have any number. (Cross chapter- Bo 3/1) [Sahar Tajbakhsh Mosalman, Iran]	Noted. This is the format of Cross-Chapter Boxes.
52939	83	1			of present-day climate (but only limited features of Mid-Holocene and LGM climates)? [Hervé Douville, France]	Rejected. The phrase "... systematic biases remain in many aspects of climate" covers both present-day and past climates.
19823	83	3	83	9	This paragraph hardly adds any information and might be spared. Ending the section with the previous paragraph seems quite satisfactory. [philippe waldteufel, France]	Not applicable any more. The entire section has been removed
15181	83	3			Cross Chapter Box 3.1: This box (and the executive summary statement summarizing the findings) is highly valuable but the title and the initial framing is problematic. The introduction needs to recognize that the widespread perception of a slowdown in the GMST trend comes from mistake of trying to draw conclusions out of a short time series with a particular starting point (e.g., Lewandowsky et al. 2015 Scientific Reports). The title "Slower Surface Global Warming over the Early 21st Century" buys in to an incorrect narrative -- the box essentially argues that the perceived hiatus was perceived but not statistically meaningful in evaluating climate change. A more accurate title, would be something like "Global surface warming in the early 21st century" - no slower. This is a really important issue because the assessment here is quite well done, but the title and the framing falls into the same communications trap that AR5 fell into. [Simon Donner, Canada]	Accepted. We have changed the title of the Box following the suggestion, and revised the introductory part of the Box.
28797	83	6			Physical undertstanding requires detection of the correct magnitude and spatiotemporal characteristics rather than just the sign: has there been an advance in this aspect of D&A? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This issue has been addressed in the methodological section where advances have been addressed in more detail.
111071	83	12	86	54	excellent box but very long compared to other important material in the body of paper that is very compressed. I think it's a bit excessive given the 'hiatus' is over. I also don't understand the sentence at the end of 'updated forcing' confidence is medium that natural forcing was missing yet confidence is low in forcing contribution? i think i understand but its confusing and you dont really need to estimate that x% of that anomaly was missing forcings isnt that almost impossible anyway? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Considering this comment, we have shortened the text and omitted one panel from the figure. The natural forcing contribution has been updated with additional literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10877	83	12	86	56	I am pleased that the mistake of an over confident assessment of a specific cause of the 'hiatus' made in AR5 is not being repeated here. But, there still is too much confidence being put on an 'attribution' of a 15 year temperature trend. This box does not really reflect the wide range of views across the climate science community on this subject. For instance I have come across at least 50 'hiatus'/'pause' papers published since AR5 (there are many more). There are a lot of explanations, and some refutations, of possible causes of the perceived changes in the short period temperature trends. I am sure the authors of this box are aware of these (e.g. see Lewandowsky et al, The 'pause' in global warming in historical context: (II). Comparing models to observations, ERL, 2018), and should make some changes to show the variety of views. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We cannot reflect all related research due to length limitations, but we have confirmed that the revised text reflects most views on the mechanisms raised by post-AR5 papers listed in Table 1 of Lewandowsky et al (2018), with additional citations where relevant.
10879	83	12	86	56	A discussion is needed about the limitations of attributing the causes of temperature change for such a short period. When the likely range of the 1C anthropogenic warming over last 150 odd years is 0.6C, how can we be at all confident about causes of differences in trends of tenth of a degree C/decade over 15 years? There is little in the way of a discussion of attribution frameworks, or the pitfalls and caveats of various approaches (Hegerl et al, Good Practice Guidance Paper on Detection and Attribution Related to Anthropogenic Climate Change, IPCC, 2009). Contrast the confidence in this Box to Section 4.6.3.1, where they say the temperature response difference between the different RCPs wouldn't be detectable for 25-30 years! This inconsistency in assessments must be addressed. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the text to highlight that internally-driven decadal variation in GSAT trends is not unique to the period assessed here, with citation to Chapter 1 Section 1.4.2.1. Uncertainty in quantitative contributions from forcing and internal variability has been described.
10883	83	12	86	56	The role of anthropogenic influences, such as aerosols, is rather understated in this Box. Indeed there is evidence that they may be as significant or even more so than natural factors, For instance Smith et al.(2016), found that anthropogenic aerosol changes - in particular the spatial distribution - may influence short term global temperature trends, such as over the turn of the 21st century. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Smith et al. (2016) suggested the role of aerosol forcing through driving the PDV. The aerosol influence on PDV is assessed extensively in Section 3.7.6, and this Box cites that section.
39583	83	12	86	56	In Fig. 1(a) of Box 3.1, it is seen that the agreement of CMIP6 with observations is even worse than CMIP5. Hence the projections of CMIP6 till 2026 in Fig. 1(b) are even less convincing. [François Gervais, France]	Rejected. The CMIP6 ensemble better captures the observed trend within its spread than CMIP5, as assessed in the text based on the figure.
21553	83	12			Overall I like this box and fear that efforts to fit to 2-sides of IPCC layout may yield unacceptable loss of information content. I would advocate for it being able to retain close to its present length. [Peter Thorne, Ireland]	Noted. Thank you.
52949	83	12			XC Box 3.1: Among the lessons that may be learnt from the global warming hiatus controversy, what about recognizing the limitations of state-of-the-art decadal forecast systems which need accurate initial conditions and radiative forcings, still show limited skills at predicting GSAT, and even stronger deficiencies at capturing the geographical and seasonal patterns of the decadal GSAT anomalies? [Hervé Douville, France]	Rejected. Decadal predictions are assessed in Chapter 4. To shorten the box, we do not assess it in detail here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18337	83	14	86	52	Glad to see some discussions on the effect of internal variability on global-mean surface temperature (GMST). I wish this box can emphasize that even for GMST, each model realization will generate a different temporal time series, as shown by Dai and Bloecker 2019, https://doi.org/10.1007/s00382-018-4132-4), let alone for regional time series and for precipitation and other more variable fileds. This thinking needs to be incoporated into many other sections of IPCC AR6, when the comparisons of historical climatology or changes are compared between observations and model simulations: For example, in Figs. 3.10-12, it is inappropirate to compare the observed historical precipitation climatology (from one random realization) to that of the multi-model ensemble mean without considering the inter-model spread in the multi-model ensemble. This is because the internal variability can cause large differences at the locall to continental scales in 20-60year mean precipitation, as shown by Deser et al . (2012, Nature Commn.) and Dai and Bloecker (2019, CD). [Aiguo Dai, United States of America]	Taken into account. We have revised the text to explicitly state that the decadal GMST/GSAT trend is subject to internal variability. Regarding the comparison of surface temperature and precipitation climatology, we have added hatching to Figs. 3.3 and 3.13 to show where the climatology difference is inconsistent with internal variability.
127359	83	14	86	52	In the spirit of reducing length of the WGI AR6 and Chapter 3 in particular, Cross-Chapter Box 3.1 and its embedded figure could be cut without detracting from overall messaging. [Trigg Talley, United States of America]	Taken into account. We have removed panel (e) to save space.
10631	83	14			Please change this title. "Slower" compared to what? The whole of the 20th century, since 1850? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The title has been changed. The meaning of "slower warming" is clarified in the introductory part of the Box.
10633	83	14			More context is needed in this chapter. How often would you expect 15 year (or similar length periods) periods to have "unusual" trends sometime in a 160 odd long temperature record? More link up with section 1.4.2.1 is needed in this cross chapter box. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been revised to highlight that the internally-driven variations of decadal warming rate are not unique to the 1998-2012 period. However, to shorten the text, we decided not to describe the frequency of similar slow warming events.
10635	83	19	83	20	So? That you would not expect the multi-model mean of an ensemble to capture the observed trend over a few years needs to be noted. It should not be implied that the multi-model mean is expected to capture variations in an observed temperature record as it does in the current text. Elsewhere in this chapter (3.3.1.1, page 14:37-41) and in chapter 1 (1.4.2.1) the important role of internal variability on short term trends is highlighted, it must not be forgotten here. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised to explicitly state that decadal GSAT trends are subject to internal variability, with a stronger link to Section 1.4.2.1.
28799	83	22			slowdown or slower than expected: slowdown applies to the comparison with earlier decades (which is somewhat ill defined) while slower than expected refers to comparison with model simulations that provide the best physically-based estimate of warming rate given the forcing and feedbacks plus the range which covers internal variability (which seems a better comparison in terms of physical understanding) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the beginning of the box to say that the 1998-2012 surface warming was slower than the observed warming for 1950-2012 and than the multimodel ensemble mean of CMIP simulations.
11309	83	23	83	25	It would be good if you could quantify relative controbution of the forced trend and internally generated variability. [Masahiro Watanabe, Japan]	Rejected. Due to length limitation and low agreement among studies, we decided not to assess the relative contributions.
10881	83	23			You need to debunk the use of "hiatus" as a term. global warming did not 'pause', and as you show in Box 3.1, Figure 1 the temperature trend for that 15 year period was positive. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Due to length limitations, we cannot add this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10637	83	31			The significance of a 15 year trend being low (or lack of), after being drawn from the 1981-2012 period, needs to be mentioned. There are only 2 degrees of freedom! [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added stippling to Cross-Chapter Box 3.1, Figure 1c to show statistical significance of the trends.
6633	83	35	83	38	The ERA5 dataset, which replaces ERA-Interim, gives the same 1998-2012 trend (to two decimal points, in ºC/decade). But of the datsets examined by Simmons et al.(2017), it was ERA-Interim that had the highest trend for this period - 0.14ºC/decade, so still a little higher than HadCRUT5, though this does not apply for the longer 1980-2018 period, to judge from Table 2.4. The statement in the SOD is nevertheless formally correct in that AR5 did not include ERA-Interim in its assessment of the 1998-2012 trend, so I don't think a change is necessary. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you.
6635	83	41	83	41	Typo "shows". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Corrected.
21543	83	41	83	43	New products better accounted for the bias offset between ship and buoys. They did not actually change the amount of buoy data included. This text should be rewritten accordingly to avoid giving a mis-impression, Reference could be made to the HadSSTv4 and ERSSTv4 papers where a reader could understand these new bias corrections and their impacts. [Peter Thorne, Ireland]	Taken into account. This part has been largely removed and instead cites Cross-Chapter Box 2.3.
21545	83	44	83	45	There are more apposite references than Karl et al here - again I would suggest the ERSST and HadSST papers would be more appropriate. [Peter Thorne, Ireland]	Taken into account. This part has been largely removed and instead cites Cross-Chapter Box 2.3.
21547	83	45	83	47	This should in addition cite the HadCRUT5 paper which explicitly quantifies the impacts of infilling on the HadCRUT product. [Peter Thorne, Ireland]	Taken into account. This part has been largely removed and instead cites Cross-Chapter Box 2.3.
21549	83	47	83	49	I wonder whether this finding is sufficiently germane to the global mean scale as to be worth inclusion or whether, instead, it risks serving to distract? [Peter Thorne, Ireland]	Accepted. Removed this sentence to shorten the Box.
65673	83	50	83	55	Please check for consistency. This Box states "global mean near-surface air temperature (GSAT), a field widely used for model outputs including by Flato et al. (2013), tends to show stronger warming trends than GMST..." However the following page states the trend difference between GSAT and GMST is small. [Kushla Munro, Australia]	Not applicable. This paragraph has been removed for consistency throughout the report.
10639	83	50	84	2	Jones (submitted Q.J.R.Meteorol. Soc., 2019) found that using a measure of model "GMST" could reduce an apparent 'discrepancy' between observations and the multi-model mean in last couple of decades, but increase the 'discrepancy' in other periods of the 20th century. Just because the apparent "discrepancy" is reduced in one's favourite period does not mean the 'fix' is correct or appropriate in first place. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The suggested paper is cited in Cross-Chapter Box 2.3, and we cite it.
10641	83	50	84	2	I am disappointed to see so much focus on just the multi-model ensemble mean. We know internal variability is important (1.4.2.1), let alone model sampling/uncertainty. The ignoring of the impact of sampling of an ensemble of opportunity' is surprising (e.g., Benestad et al, New vigour involving statisticians to overcome ensemble fatigue, Nat. Clim.Chan., 2017). Are we that confident of the forcing changes (and that they lack any meaningful uncertainty)? Please reconsider how this is being communicated. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The result that the CMIP6 ensemble well captures the observed trend within its spread has been highlighted in the text. The comparison with the estimated forced trend is one focus in AR5 Box 9.2 (Flato et al 2013). Since this Cross-Chapter Box is its update, and identification of the key modes of variability is the major progress since AR5, the assessment on the role of internal variability is important.
108091	83		87		Cross-Chapter Box 3.1 [Asylbek Aidaraliev, Kyrgyzstan]	Not applicable. The comment is missing.
79483	83				From page 83 the titles don`t have any number. (Cross chapter- Bo 3/1) (comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Noted. This is the format of Cross-Chapter Boxes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10643	84	9			I strongly suspect wider range of climate sensitivities, and forcing uncertainties can't be ruled out as playing a role. Some formal assessment of the statistical differences between CMIP3 and CMIP5 are needed to defend such a statement. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed to shorten the text.
10645	84	16	84	17	This assertion that blending and masking "explains" 14-20% of the trend difference is logically flawed. "Explains" implies it has been attributed in some way. But that is not the case. Other factors have not been ruled out, e.g. what impact was there of the inclusion of models that did not simulate indirect aerosols? The focus on the 'discrepancy' between the multi-model mean and the observations is confusing many. One would actually be surprised if the multi-model mean matched observed trends over short periods exactly. A closer agreement when an adjustment has been applied, does not in itself mean the adjustment is correct. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sentence has been removed.
10647	84	16	84	17	It needs to be mentioned that masking by observational coverage has been pretty much standard practice in reputable studies for some time, e.g. Fig 12.7 in Mitchell et al, Detection of Climate Change and Attribution of Causes, IPCC, WG1, 2001 and subsequent ARs. So saying masking "explains" some of the trend difference implies it wasn't being applied in previous studies, which is incorrect! [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This part has been largely shortened and the suggested revision is unnecessary.
90825	84	24			Some study claimed that that is little interannual variability and a regular (but weak) seasonal cycle (Checking out the book: Global Change and The Earth System: A Planet Under Pressure) [Vivien How, Malaysia]	Rejected. In the instrumental era, ENSO is the mode of variability that affects the GMST/GSAT the most strongly on interannual time scales.
104407	84	31	84	33	Parsons et al. find that the majority of models indicate the Atlantic/Arctic is assocaited with GMST interdecadal variability. PDV is important in several CMIP6 piControl simulations, but not in the majority of CMIP6 models. [Luke Parsons, United States of America]	Not applicable. This sentence has been removed and instead Technical Annex IV is cited to shorten the text.
21551	84	35	84	35	I assume you mean substantial amelioration or underlying trends rather than substantial absolute decreases? This terminology, regardless requires revisiting as it could be interpreted in several ways. [Peter Thorne, Ireland]	Taken into account. Revised to distinguish decadal slower warming under transient forcing and decadal cooling under fixed radiative forcing
79267	85	27	85	29	I think it would help to provide some context to the results of Thorne et al. (2015). Why do they come to differing conclusions? It seems to related to volcanic aerosols and tropospheric aerosols (Outten et al., 2015; Figure 7): Pinatubo forcing is more negative in their sensitivity experiment with updated forcings, but for CMIP6 the Pinatubo forcing is less negative than for CMIP5 (Figure 2.2 of Chapter 2, AR6 WG I). Further, tropospheric aerosol forcing is less negative in their sensitivity experiment. (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JD023859) [Martin Stolpe, Switzerland]	Accepted. The potential underestimation of volcanic forcing, which is mentioned in Outten et al (2015) and is confirmed from comparison with Chapter 2 Fig. 2.2, is added.
28801	85	28			a reduction in GMST trend of around 0.03–0.05 K decade−1 was linked to a rapid change in the growth rates of ozone-depleting gases and weakening in growth rates of methane and tropospheric ozone radiative forcing by Checa-Garcia (2016) ERL http://iopscience.iop.org/article/10.1088/1748-9326/11/9/094018 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The suggested paper is certainly valuable, but the focus of this part is on the forcing updates from CMIP5, instead of forcing change from preceding decades.
79257	85	30	85	32	Since the simulated TCR and ECS increased from CMIP5 to CMIP6 and the recent warming depends on these properties (Tokarska et al., 2020; Nijsse et al., 2020), I'm not sure how much the difference in warming really tells about differences in forcing. [Martin Stolpe, Switzerland]	Taken into account. We have included the higher climate sensitivity in the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10651	85	31	85	34	It would be really helpful to call back to previous ARs that have made similar assessments, e.g., IPCC 1st AR "Because of long-period couplings between different components of the climate system, for example between ocean and atmosphere, the Earth's climate would still vary without being perturbed by any external influences. This natural variability could act to add to, or subtract from, any human-made warming, on a century time-scale this would be less than changes expected from greenhouse gas increases.", [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been added to highlight that internal variability can modulate decadal warming rates, citing Section 1.4.2.1.
10649	85	41	85	55	This is quite important. This should be nearer the top of the box to make sure readers don't misunderstand the significance of variability in surface temperatures over short periods. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. An important and policy-relevant message is that the slower warming for 1998-2012 was a temporary event. We make this assessment first.
28803	85	41			Check for consistency with 7.2 which states that "reconstructions indicate that the Earth's energy imbalance was larger in the 2000s than in the 1990s (high confidence)." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This part has been revised to reflect the assessment of Chapter 7.
28805	85	45			Ocean data and independent satellite estimates (since the change over time is independent of Argo) indicate an increasing rate of heat uptake from the 1980s to the 2000s [Cheng et al. 2017 Sci. Adv https://advances.sciencemag.org/content/3/3/e1601545.full; Allan et al. 2014 GRL http://onlinelibrary.wiley.com/doi/10.1002/2014GL060962/full] [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This part has been revised and cites relevant sections that cite the suggested papers.
21555	86	1			To me the inclusion of this box in this manner feels a bit forced. I'm not convinced from the perspective of the reader that its inclusion here makes much sense. Given that most of the apparent content arises from chapter 11 if retained it might be better to do so there. The comparison to the preceding box, which very clearly integrates substantial contributions from several underlying chapters is marked. [Peter Thorne, Ireland]	Taken into account. The justification for the inclusion of this box in Chapter 3 is now more clearly stated - in particular attribution of changes in extremes feeds into our overall assessment for attribution of human influence on the climate system in Section 3.8, and this is now stated in the box. Moreover, the box now includes an author from Chapter 8 and includes references to Chapter 8, as well as 3 and 11.
6637	86	14	86	15	This statement can now be amended by replacing 2014 by 2015 and 2018 by 2019. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised as suggested.
105037	86	14	86	15	Can this be updated to 2016-2020? [Peter Gleckler, United States of America]	Accepted. Now updated to this period.
79259	86	25	86	28	Mention that the ECS - decadal variability relationship doesn't seem to exist in CMIP3 (Colman & Power, 2018) and is much weaker in CMIP6 (unpublished). From Colman & Power (2018): "Despite the relationships found in CMIP5, no such relationships (e.g. between ECS and SDT_10y) are found for the earlier set of CMIP3 models (Fig. 9b)." https://link.springer.com/article/10.1007/s00382-018-4113-7 [Martin Stolpe, Switzerland]	Taken into account. Considering the uncertainty and to shorten the text, this part has been removed.
79261	86	33	86	33	also cite Medhaug and Drange (2016), https://link.springer.com/article/10.1007/s00382-015-2811-y [Martin Stolpe, Switzerland]	Accepted. The paper is cited.
65675	86	37	86	54	Please check for consistency. The figure seems to show that both CMIP5 and CMIP6 trends are in general greater than the observed surface temperature trends, which appears to be at odds with the statement in the Executive Summary that CMIP trends agree with observations over the 1998-2012 period. [Kushla Munro, Australia]	Taken into account. The difference of the observed trends with the CMIP5 and CMIP6 ensemble mean trends is one of the main theme of this Box. The introductory paragraph has been revised to clearly describe this point.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65677	86	37	86	54	Suggest removing the trend histograms and maps for 2012-2026 from this figure, since they distract from the main point of Box 3.1 and the figure: namely an analysis of the apparent slowing of warming in the 1998 through 20102 time interval. It is difficult to see what light this future period sheds on the 1998-2012 warming trend. Suggest far more relevant would be the same analysis, but for the 1974 to 1988 period referred to in the text, during which the observed trend was greater than the ensemble of historical simulations. This would reinforce the point that models capture the long-term drivers of climate change even if they do not currently capture the timing and amplitude of all modes of natural variability. [Kushla Munro, Australia]	Taken into account. We have removed the panel (e) to save space.
116255	86		86		"CMIP5 models that have a higher sensitivity tend to have stronger variability" : is this also valid in CMIP6? Why? [Valerie Masson-Delmotte, France]	Not applicable. This part has been removed considering another comment and to shorten the text.
603	87	1	88	2	no references in the CCB. We also need to add the period in the confience sentences. [ZHIYAN ZUO, China]	Accepted. Revised accordingly, citing recent papers and indicating the period.
105031	87	10	87	10	suggest: "…for evaluating model performance and future projections" [Peter Gleckler, United States of America]	Taken into account. Rephrased accordingly.
105033	87	16	87	16	suggest: "One important aspect of temerature extreme indicators is …" [Peter Gleckler, United States of America]	Taken into account. Rephrased accordingly.
105035	87	21	87	23	Given that human influence on the warming of the global climate is virtually certain…". Clause in confused by 3 uses of "that" [Peter Gleckler, United States of America]	Taken into account. Rephrased accordingly.
17107	87	45	87	46	Please define "extreme precipitation" term. In my opinion as a reader, the term leads to higher precipitation rate condition. However, is it possible for the author to describe the precipitation frequency? For example the projected days with rain or without rain in a year in respect to "extreme precipitation". Thanks. [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We assessed both intensity and frequency of extreme precipitation.
42703	87	51			A striking feature of the top right panel in Cross-Chapter Box 3.2 Figure 1 is that the MME mean results are consistently below the observational curve after 1970. Is this suggesting that the models systematically underestimate the response in RX1-day? It maybe that the record is too short to make any definitive conclusion but a comment on this would be useful. [Christopher Gordon, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Unclear basis for making a robust assessment of possible underestimation by the models.
116257	87		87		This box needs to be further developed and referred to in ch 3 and ES. I understand that the focus is the link between warming trends and trends in extremes that are directly affected by mean temperature levels but it is currently somehow implicit here. There is also some overlap between this box and the FAQ, and I note that the box is not (yet?) developed as an assessment. Is it really needed? [Valerie Masson-Delmotte, France]	Accepted. Revised accordingly, emphasizing post-AR5 findings with relevant citations and discussing the associated physical processes with new inputs from Ch8.
111073	87				cross chapter box: excellent that it exists. It reads not very mature - no citations or very few etc. if this entirely covered and clearly covered in section 11 then its fine (in last review I couldn't find it) - maybe could use a decision to what extent its covered in this cross chapter box with all D+A and model evaluation moving here; or if the box could be much shorter and just crossrefer. maybe with some key findings from ch11 directly referenced. overall i think extremes are a vital part particularly extreme precip is a clearer indication of forced response than means... [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised accordingly, citing recent papers and emphasizing new findings.
40311	88	7	88	7	for the FAQ, I would suggest including the acronym (ENSO) after the word Oscillation, as some people may only know this. [TSU WGI, France]	Taken into account. Reference to ENSO and other modes of variability have been removed from this FAQ in response to this and other comments.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39675	89	0			the summary is a bit longer than it should and introduces element not mentioned afterwards. [TSU WGI, France]	Accepted, the text has been substantially revised and this comment has been taken into account.
39681	89	0			the text focuses on the temporal influence of the natural variability but it would be good to also talk about the spatial effect (as it is currently done in the summary) [TSU WGI, France]	Accepted, the text has been substantially revised and this comment has been taken into account.
40137	89	0			FAQ3.1 is nice! [TSU WGI, France]	Thank you.
40401	89	0			-I would try to simplify the language in some parts of the text, to make it more accessible to a lay audience (e.g. radiative forcing sounds very jargony) [TSU WGI, France]	Accepted, the text has been substantially revised and this comment has been taken into account.
11313	89	1	89	1	Please cite FAQ Fig 3.1 somewhere in the text. [Masahiro Watanabe, Japan]	Accepted, the FAQ figure is now clearly cited in the text.
39171	89	1	92	50	FAQ is meant to encapsulate what are significant in the specific chapter. FAQ 3.3 captures best (to non-science readers) what this chapter is all about [Lourdes Tibig, Philippines]	Rejected, this FAQ also addresses a question that is very prominent in this chapter. We do note, however, that the ordering of FAQs has now been changed.
38681	89	3	89	42	All in all, I think this is a very important FAQ, but perhaps there is a way to ensure that even people who are just starting to learn about natural variability understand the most important points (natural internal versus natural external versus human-induced, long versus short timescales, regional versus global scale, variability versus long-term trends). [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
85025	89	3	89	54	No comments [Katrine Husum, Norway]	Noted.
81483	89	3			Yes, climate models have improved and continue to do so but climate model outputs still have not reliable for some regions [Kyaw Moe Oo, Myanmar]	Accepted, the text has been substantially revised and this comment has been taken into account.
13417	89	5	89	17	In order to make clear to the reader the importance of natural external forcing and to clearly distinguish anthropogenic external forcing, it is suggested to briefly mention the difference. [Maria Amparo Martinez Arroyo, Mexico]	Accepted, the text has been substantially revised and this comment has been taken into account.
37357	89	6	89	7	"internal variability" means no external inputs or outputs. The ENSO cannot be regarded as internal variability because it gets its energy from the sun. Further, pg 66 lines 43 to 54 show that external forcings di indeed influence the ENSO. [John McLean, Australia]	Rejected, this is incorrect, and the report text referred to does not support your argument.
38661	89	6	89	7	Is there a way to explain more clearly for non-specialists what "modes of variability" and the "El Niño-Southern Oscillation" are? For people who do not know much abut the background, the sentence basically says that variability is caused by modes of variability. It would also avoid confusion to clarify what "internal" and "external" means (what is considered as part of the climate system and what is not), because all causes mentioned here as examples may simply be perceived "natural" causes (in contrast to human-induced ones). Your readers might be more familiar with a differentiation between natural and human-induced, but not between internal and external causes. [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
28807	89	6			"via modes of variability" is quite technical - you could just say "from ocean fluctuations"; maybe replace 2nd "driven by" in the next line with "including"… throughout the TSU can advise on if the language is good or needs simplifying (e.g. climate model simulation --> complex computer simulation?) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the text has been substantially revised and this comment has been taken into account.
7273	89	7	89	7	as well as external climatic variations driven by changes in solar brightness and by aerosols released through volcanic eruptions. [Julio Cesar Barreto da Silva, Brazil]	Accepted, the text has been substantially revised and this comment has been taken into account.
13419	89	8	89	8	It's suggested to add "(natural external forcings)" after the text "changes in solar brightness and by aerosols released from volcanic eruptions". [Maria Amparo Martinez Arroyo, Mexico]	Accepted, the text has been substantially revised and this comment has been taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7275	89	10	89	10	as fluctuations in climate on a regional scale. This variability is motivated by changes in the Earth's energy balance, and its impacts are more evident in large-scale climatic indices, such as the average global surface temperature. [Julio Cesar Barreto da Silva, Brazil]	Accepted, the text has been substantially revised and this comment has been taken into account.
38663	89	10	89	10	What is meant by "regional-scale fluctuations in the climate", what is regional and, more importantly, what kind of fluctuations do you refer to? [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and what is referred to as regional has now been better described.
37361	89	10	89	11	Rubbish. The sun is clearly external and it influences Earth's climate. [John McLean, Australia]	Taken into account. The reviewer is correct and our text is not intended to imply otherwise. Now clarified.
38665	89	11	89	13	The repetition of "large scale" might make it difficult for non-specialists to differentiate between the "large scale climate indices" and "large-scale climate changes". Can this be rephrased and probably made more specific? [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
38667	89	12	89	12	Do you mean "large-scale" on a spacial level? This sounds confusing to me because the previous sentences adressed regional and global differences. The question might be (again, see comment above): What is large-scale? [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
10049	89	12	89	14	Natural influence on multidecadal regional and global temperature trends is NOT small as claimed. The long-term warming trend of the past 150 years is modulated by multidecadal cycles such as the PDO and AMO. In Europe for example, summer temperatures are very clearly linked to the AMO which has a cyclicity of 60-80 years, i.e. “multidecadal”. See e.g. Lüdecke et al. 2020: Decadal and multidecadal natural variability in European temperature, https://doi.org/10.1016/j.jastp.2020.105294. Influence is therefore not restricted to “one or two decades” but corresponds to the entire 60-80 years. A half-cycle is 30-40 years. [Sebastian Luening, Switzerland]	Accepted, the text has been substantially revised and this comment has been taken into account.
17607	89	13	89	13	the claim that influence of natural climate variability on multidecadal trends is small, is not justified ; Example Hegerl (Clim.Change March 2018) "natural variability also made a large contribution,.." related to 1901-1950 period. [ferdinand meeus, Belgium]	Accepted, the text has been substantially revised and this comment has been taken into account.
37363	89	13	89	17	False claim. It can be shown that global average temperatures in the late 1970s rose sharply as a consequence of the Great Pacific Climate Shift (virtually certain). Over the previous 25 years there had been few El Nino events but several La Nina events, but the Great Pacific Climate Shift reversed the situation and sent temperatures rising. The uptick in temperatures meant that the temperature trend from 1950 to anywhere from 1980 onwards would show an upward trend. [John McLean, Australia]	Accepted, the text has been substantially revised and this comment has been taken into account.
26771	89	14	89	14	It coult also affect longuer time scale I.E millennium variability seen in the paleo records. The point is that anthropogenic forcing is of larger magnitude. The explanation is a little bit confusing and misleading. It let people think that there is no internal or natural forced varvariability at scale lareger that 2 decades, which is not correct [Eric Brun, France]	Taken into account. It is not that there no natural millennial variability, it is just that the rate of change is small in magnitude when compared to anthropogenic forcing. The text has been substantially revised and this comment has been taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10051	89	14	89	17	This statement is speculation and does not represent a consensus in the climate sciences. The quantitative attribution of 20th century warming far from settled. You cannot claim or speculate that the entire 20th century warming is due to anthropogenic forcing when until recently the interpretation was “more than half of the warming is anthropogenic”. For example, the recent Swiss climate report sticks to the former statement of “more than half” vs. “up to half” for anthropogenic vs. natural. Authors should clearly stay on the scientific side even if political pressure may exist to declare the case settled in order to justify political action. The truth is that the anthropogenic contribution is probably somewhere between 50-100% and this should be stated here. None of the typical climate parameters is attributed 100% to anthopogenic action. 40-50% of the warming of the last few decades is due to “modes of variability” (PDO, AMO) (Tung, K.-K., Zhou, J. (2013): Using data to attribute episodes of warming and cooling in instrumental records: Proceedings of the National Academy of Sciences 110 (6), 2058-2063). Only about one quarter of the melting of global mountain glaciers of the past 100 years is due to anthopogenic forcing (paper by Marzeion et al.). Up to 55% of global sea level rise of the past 10 years has natural causes (paper by Dangendorf et al.). The IPCC loses it credibility if it insists on 100% anthropogenic when many other climate scientists strongly disagree with this alarmistic statement. Protect the IPCC and its credibility by refraining from unsupportable black-and-white statements. [Sebastian Luening, Switzerland]	Accepted, the text has been substantially revised and this comment has been taken into account.
21557	89	14	89	17	Wasn't the assessment in SR1.5 and also in the main body of the text that the best estimates was all of the warming since 1900 was due to human influences? If so why the equivocation and wiggle room being given here and why not say that? [Peter Thorne, Ireland]	Accepted, the text has been substantially revised and this comment has been taken into account.
38671	89	14	89	17	I would suggest to rephrase the "become the dominant contributor" part because it might not be entirely clear what you mean by "become" here. It becomes obvious that human-induced drivers dominate - but they will always do so, whether or not "the observational period becomes longer" or not, don't they? The current version could also be interpreted like a suggestion to tweak calculations: Expand the observational period, and human-induced drivers become dominant. [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
38669	89	15	89	15	The target audience of the FAQs might not know immediately what "human-induced forcing changes" are. Can this be phrased mire simply and more specifically? [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
37365	89	16	89	16	You claim that "large scale warming" has occurred but McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" shows more than 70 areas of uncertainty in that data (and because other datasets share the same source data many of those problems will also be present in those other datasets. For example, anyone using the WMO-recommended methods of data homogenisation or techniques derived from those methods, has virtually certainly made incorrect temperature data adjustments. In particular, any trends caused or influenced by gradually increasing non-meteorological influences (e.g. UHI, growth of vegetation, deteriorating condition of screens) will not have been removed (see section 9.9 of McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset") [John McLean, Australia]	Rejected. Observed temperature changes are assessed in Chapter 2, not Chapter 3. But we note that Chapter 2 bases its assessment that global mean temperature has warmed on four main surface temperature observations datasets (Cross-Chapter Box 2.3), which all show similar warming.
37367	89	16	89	16	This sentence is unsustainable. The assertion is based on the output of models that haven't been validated and is contrary to the UAH LTT data that closely matches the ENSO pattern, meaning that if there is any human influence at all then it is very minor. [John McLean, Australia]	Rejected, these models are validated throughout CH3 and in the various papers cited within.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102915	89	16	89	16	"almost entirely" - do you want to give a level of confidence? [Philippe Tulkens, Belgium]	Accepted, the text has been substantially revised and this comment has been taken into account.
96299	89	16			The issue of "detection and attribution" is covered in principle within different FAQs, but the terms are not mentioned explicitly. In order to include the term "attribution" in FAQ 3.1, please change to "…since 1900 is almost entirely driven by attributed to human influence." [Nicole Wilke, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
7277	89	19	89	19	Paleoclimatic records (indirect measurements that go back thousands of years) and computer models show that global temperatures change and are always changing - and that these changes can occur for several reasons, among them, the natural variability related to climate. These are either generated internally in the climate system or driven externally by natural forcing changes. Thus, as well as variations in solar brightness and volcanoes, changes in the Earth's orbital characteristics can also create natural radiative forcing changes and have been linked to major climatic changes in the past. However, orbital changes operate over very long timescales, which means that they have had very little influence on the changes observed in the past century. [Julio Cesar Barreto da Silva, Brazil]	Accepted, the text has been substantially revised and this comment has been taken into account.
37369	89	19	89	19	If the models that you refer to have not been validated then explicitly state this. [John McLean, Australia]	Rejected, these models are evaluated throughout CH3 and in the various papers cited within.
96301	89	19	89	20	… computer models all show that global temperatures have, and are always changing - …": add the verb 'changed': "… global temperatures have changed, and are always changing…" [Nicole Wilke, Germany]	Accepted, text changed as suggested.
38673	89	21	89	23	Again, the difference between "internally generated within the climate system or externally driven by natural forcing changes" might be difficult to grasp for people who are not familar with the concept. I would repeat that "variations in solar brightness and volcanoes" (or rather particles from eruptions?) count as external an add an example for internal (El Nino is only mentioned in the summary/introduction). [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
28809	89	22			This seems to repeat information in the previous paragraph: "which refers to variations in climate that are either internally generated within the climate system or externally driven by natural forcing changes..." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the text has been substantially revised and this comment has been taken into account.
14875	89	23	89	26	It is indeed true that orbital changes operate on very long time scale although 'long means almost nothing! It depends so much of the audience. If you ask a three-year old child to wait for one hour … that will be 'very long'. My point here is to say that there are also short time scale, such as the 18 year cycle related to the moon. The amplitude of this short period changes is much smaller than the amplitude of the long period changes. Therefore the conclusion of this sentence remains valid that these changes 'have had very little influence on the changes 26 observed over the past century'. [Marie-France Loutre, Switzerland]	Accepted, the text has been substantially revised and this comment has been taken into account.
19509	89	24	89	24	after larg climate add " scale" [Hamideh Dalaei, Iran]	Accepted, the text has been substantially revised and this comment has been taken into account.
83967	89	25	89	25	Add "(thousands of years)" after "...on very long time scales", so that the reader has a notion of how long orbital forcing acts. [Marco Tulio Cabral, Brazil]	Accepted, text changed as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7279	89	28	89	28	In order, to understand which aspects of the observed climate changes were caused by natural variability, scientists simulate the weather and climate conditions using numerical deterministic models. When only natural climatic factors are used to force climate models, which implicitly generate their own natural internal variability, the explanatory variables used in the model are generally called natural climate forcings. These simulations show small variations in climate in response, for example, to volcanic eruptions, variations in solar brightness and internal models of climatic variability, but do not show long-term warming trends comparable to observational variations ocorred in real time. However, when human influences are included, particularly greenhouse gases, the models simulate a warming comparable to that observed, that is, variations very close to those occurring in the atmosphere. [Julio Cesar Barreto da Silva, Brazil]	Accepted, the text has been substantially revised and this comment has been taken into account.
19511	89	28	89	28	after observed climate change add "data" [Hamideh Dalaei, Iran]	Rejected, while we appreciate the comment the suggested text change slightly changes the meaning of the sentence away from its intended purpose.
11315	89	28	89	34	The rationale sounds, but you probably should mention an assumption that the models' internal variability has no bias in magnitude, spectral property etc (the assumption is sometimes not right, cf. Fig 3.5). [Masahiro Watanabe, Japan]	Accepted, the text has been substantially revised and this comment has been taken into account.
37371	89	28	89	34	Whoever wrote this paragraph should be very embarrassed. The climate models (a) have not been validated and (b) were shown earlier in this chapter to be no better than the CMIP5 models used in AR5. How well did these models perform in studies for AR5? We were told "... an analysis of the full suite of CMIP5 historical simulations (...) reveals that 111 out of 114 realisations show a GMST trend over 1998–2012 that is higher than the entire HadCRUT4 trend ensemble ...." [WGI contribution, chapter 9, text box 9.2, page 769, and in full Synthesis Report on page SYR-8] [John McLean, Australia]	Rejected, these models are validated throughout CH3 and in the various papers cited within.
96303	89	28			FAQ 3.1: The issue of "detection and attribution" is covered in principle within different FAQs, but the terms are not mentioned explicitly. In order to include the term "attribution" in FAQ 3.1, please insert "…have been caused by natural variability (a process referred to as "attribution"), scientists...". [Nicole Wilke, Germany]	Accepted, text changed as suggested.
38675	89	29	89	30	"force climate models" and "implicitly generate their own natural internal variability" will sound very technical to some of the readers of IPCC FAQs and could confuse them. For example, they might conclde that models calculate internal natural variabilities that do not really represent what is happening in the real world (so how reliable are they?). [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
26773	89	31	89	31	We suggest to add "on a wide range of time scales" after "climate in response" [Eric Brun, France]	Accepted, the text has been substantially revised and this comment has been taken into account.
77685	89	31			"similations" replace by "simulations" [Emer Griffin, Ireland]	Accepted
11311	89	32	89	32	models should be modes [Masahiro Watanabe, Japan]	Accepted.
38677	89	32	89	32	What is meant by "internal models of climate variability" within a simulation? Is this a typo (models/modes)? [Maike Nicolai, Germany]	Accepted, this was a typo that has now been fixed.
38679	89	32	89	34	I think the message of this sentence could be carved out more clearly. I would also add it to the introduction/summary which is very complex at the moment. [Maike Nicolai, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
7281	89	36	89	36	In reality, what these combined informations mean is that, in short time scales of a decade or less, natural climate variability can dominate the human-induced warming trend, leading to periods with little warming. [Julio Cesar Barreto da Silva, Brazil]	Accepted, the text has been substantially revised and this comment has been taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102917	89	36	89	38	Unclear sentence. Delete "in reality". If natural variability can dominate, then it can surely include not only "little warming", but also cooling. But that is trivial, as otherwise every single year should set a temperature record (a "short time scale of a decade or less" includes annual). [Philippe Tulkens, Belgium]	Accepted, the text has been substantially revised and this comment has been taken into account.
17609	89	36	89	42	Claim that influence natural climate variability is restricted to about 20 years is not justified. There are examples of natural variability on century to millennium scale. Also Hegerl (Clim. Change March 2018) mentions significant natural variability on 50 yr scale. And there is a lot that we simply do not know or sufficiently understand. [ferdinand meeus, Belgium]	Accepted, yes but the millennial changes have a small rate of change (oC per year) compared to anthropogenically induced changes. The text has been substantially revised and this comment has been taken into account.
37373	89	36	89	42	A laughable paragraph based on premises for which you have no evidence because no climate models have been validated. [John McLean, Australia]	Rejected. Model evaluation and assessment of fitness-for-purpose is a key focus of the chapter.
102919	89	36	89	42	Some of the language could be clearer. For example, could one say, "Choose *any* time period after the year 1900 that is 20 years or longer, and you will see a warming."? The image of the bicycle ride is appreciated, it fits especially because in this chapter (see above) you have chosen to use the language of the "main driver". But this could be expressed more clearly, along the following lines: maybe you can start the para like this: "Human activity is driving the warming, although over short time scales the natural variability can dominate and mask this effect.... Temporarily the speed is reduced, ..." etc. "Over longer periods the natural variability evens out and the human induced warming can be detected as an independent trend..." or similar, whatever you can defend, but this paragraph is worth revisiting and getting absolutely clear and accessible. [Philippe Tulkens, Belgium]	Accepted, the text has been substantially revised and this comment has been taken into account.
26775	89	38	89	39	This is not entirely correct. If the period before 1990 is considered what is said is wrong, because the anthropogenic signal was still in the internal noise... so this is not only a question of decades. We suggest to insist more on the fact that the anthropogenic forcing modifies the Earth's energetic and induces a long term trend [Eric Brun, France]	Accepted, the text has been substantially revised and this comment has been taken into account.
7283	89	39	89	39	Insert paragraphy: Another way to think of this is, although [...] [Julio Cesar Barreto da Silva, Brazil]	Accepted, the text has been substantially revised and this comment has been taken into account.
96305	89	40			The issue of "detection and attribution" is covered in principle within different FAQs, but the terms are not mentioned explicitly. In order to include the term "attribution" in FAQ 3.1, please insert "...observed global warming over recent decades. This process of evaluating the relative contributions of different drivers of climate change is referred to as "attribution"." [Nicole Wilke, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
7285	89	41	89	41	a role in how fast or slow temperatures rise. Roughly speaking, as a way of exemplifying the effect of this natural variability, the act of cycling in mountainous terrain is mentioned: the bicycle is always advancing, but the presence of the hills reduces or increases the speed. [Julio Cesar Barreto da Silva, Brazil]	Rejected, it is unclear whether anything needs to be changed in response to this comment.
96307	89	41	89	41	Please replace the example of riding a bike over a hilly terrain by riding a bike through gusty winds, because in my option gusty winds are a better representation for natural variability than a hilly terrain. In uneven terrain every the ordinary biker would try to avoid slopes as far as possible, which is impossible with gusts of wind. [Nicole Wilke, Germany]	Accepted, the text has been substantially revised and this comment has been taken into account.
111075	89	41			the hilly bikeride isnt a great example as the bike pedals forward and we know - I much prefer the tide coming in where individual waves get longer or shorter but over time a trend is discernible [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the text has been substantially revised and this comment has been taken into account.
2673	89	45			The interesting FAQ3.1, Fig.1 is not directly discussed at all. [Bryan Weare, United States of America]	Accepted, the text has been substantially revised and this comment has been taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37379	89	47	89	51	This Figure should be filed under "Fiction" because (a) you haven't audited the temperature data and corrected the errors and (b) it is based on output from unvalidated climate models, which incidentally have been tuned to match near surface temperature data that is flawed. [John McLean, Australia]	Rejected, the models are validated throughout CH3 and the numerous studies cited within.
7287	89	52	89	52	Also note in FAQ 3.1, Figure 1 that, in the simulation by human-induced change, the maximum temperature found in 2020 is similar to that shown in the observed climate change, around 0.7 °C; while in the simulation by natural forcings (warming and cooling), there was practically no change in temperature in 2020, compared to the year 1930, with a variation of approximately 0.1 °C. [Julio Cesar Barreto da Silva, Brazil]	Accepted, the text has been substantially revised and this comment has been taken into account.
39653	90	0			- I find the part about Earth system models unclear: are they systematically used now? in the intercomparison is it partly ESM partly climate models? [TSU WGI, France]	Accepted. We now state that the present generation of models includes both ESMs and other "climate-only" models.
39673	90	0			the logic of the structure of FAQ3.2 is not entirely clear to me (e.g. the 4th paragraph on earth system models is a bit odd there I find) [TSU WGI, France]	Accepted. We have reordered the structure of the FAQ to make it easier to follow.
40377	90	0			I think it is important emphasize more the two aspects of the figure/model validation: 1) the correlation with observations= how close model simulations are from observations and 2) how well model agree between each others (the spread between the model estimates). I think the second component is currently missing. [TSU WGI, France]	Accepted. We now make explicit that the agreement also improves w.r.t. inter-model comparisons.
40979	90	0			the link between the text and the figure is a bit weak at the moment [TSU WGI, France]	Accepted. We have rephrased the text to hopefully address this comment.
38709	90	1	91	1	This FAQ starts off very confidently and convincingly but then addresses so many "marginal" and "only gradual" improvements that I start doubting the models really have improved. Is this your intention? [Maike Nicolai, Germany]	Accepted. We now reference, in the opening paragraph, the difficulties with narrowing model uncertainties with climate projections as an example that not everything about models has improved. The opening paragraph did not reflect the balance of evidence about climate models characterizing the report; we think the new introduction better captures this.
111077	90	1			the FAQ body is great but nobody would ask that question. How about what are climate models or reliable good are climate models in simulating the weather and climate or … could also mention the seamless concept between weather and climate prediction [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The question is the result of a comprehensive "user" survey and cannot be changed. We now note the relationship between weather and climate models.
37381	90	3	90	5	If CMIP6 models are so good then where's the figure showing their retrospective predictions of warming over the last 15 or 20 years? Failure to show such a figure will cast suspicion on your claims. [John McLean, Australia]	Noted. This figure is in the main text (figure 3.3) and in the previous FAQ 3.1. We don't repeat it here, mostly for space reasons.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10053	90	3	90	7	I am sorry, are you serious? It has been widely reported in the press and in publications that many climate models that have been updated for the AR6 suggest warming rates which exceed the measured temperature record sigificantly. The new models are worse than the old ones. Why are you hiding this finding from the readers here? It is embarrassing, yes. But there is no need to be silent about it and claim the opposite. Stay ethical and report the true findings as inconvenient as they might be! [Sebastian Luening, Switzerland]	Accepted. We agree that the previous formulation did not reflect the more nuanced findings of Ch4 which indeed grapples with the implications of the large-sensitivity models. Dismissing them as "worse" than the AR5 models is however too simplistic. The purpose of Ch3 is to use historical simulations to evaluate models and attribute any trends. It is objectively true that CMIP6 models, as a group, outperform CMIP5 models. This includes the high-sensitivity models. However we have also learnt that such improved performance does not imply a reduction of uncertainty for projections where the magnitude of projected change is affected by deficiencies in the representation of processes characterized by cancellation of errors that are hard to identify and correct.
38687	90	3	90	7	What does "now" exaclty mean in your introduction (line 3 and 6)? Does this refer to the CMIP6 models etc.? Or are you highlighting differences between AR5 and AR6? [Maike Nicolai, Germany]	Accepted. We have rephrased the opening paragraph, avoiding "now".
28811	90	3			Do "climate models" need to be described/explained here if it is a public-facing section (briefly as I see they are described very well in the body) e.g. "Yes, complex computer simulations of our climate system are continuing to become more realistic"? Also "man-made": really?? "human caused"? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Indeed climate models are very briefly introduced in the subsequent paragraphs. We have rephrased the paragraph so "man-made" is now gone.
38683	90	4	90	5	"compare better with observations" might reflect a perspective that the target audience of IPCC FAQs might not be familiar with. I would point out more clearly that this shows that models have improved. But improving them is the aim and the comparison with observations is a way to measure this - but it is not the aim in itself, isn't it? [Maike Nicolai, Germany]	Accepted. We have rephrased this paragraph, now avoiding "validation" (which is jargon). We now make explicit that comparing against observations is indeed a way to measure progress.
26777	90	5	90	5	We suggest to replace "models show" with "the use of climate models has allowed to show" [Eric Brun, France]	Accepted. We now phrase this more correctly.
38685	90	5	90	5	"man-made" - you might like to reflect that women also cause emissions (suggesting "human-induced")… [Maike Nicolai, Germany]	Accepted. "Man-made climate change" is a commonly used phrase in the English language. We agree a gender-neutral way of describing this would be better. We avoid the phrase completely now.
21559	90	5	90	7	This sentence as written is potentially ripe for mis-interpretation and, anyway, the question is not about human influence so why does the answer lede need to state this? It would make more sense for the answer lede to focus explicitly on answering the question and to not shoe-horn in an attribution issue already covered in FAQ 3.1 and then again covered in FAQ 3.3 anyway. [Peter Thorne, Ireland]	Accepted. We have rephrased the opening paragraph to no longer refer to climate projections, addressing the comment.
37393	90	9	90	9	Scientists have not used computer models. They have used climate models. [John McLean, Australia]	Noted. While technically correct, we want to convey the information that climate models are a piece of scientific software.
2675	90	9	90	14	Some reference needs to be given to developments and evaluation of forecast models and the role they play in climate model development. [Bryan Weare, United States of America]	Noted. The reviewer is correct but due to space reasons the reference to “weather models” and the role NWP plays in climate modelling are now gone.

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39791	90	10			"models have improved due to advances in technology" I don't agree. For me it's not only technological advances but also more observations (better constraints), theoretical advances/better understanding [TSU WGI, France]	Accepted. This was not meant to be the only reason models have improved, but it certainly is an important contributing factor. We now make this explicit, by adding the word "partially".
38689	90	11	90	13	The idea of "comparing" model results with observations comes up here again. I would spell out more clearly what you conclude from the comparison (and the fact that new models compare better) to observations. The last sentence of ths paragraph offers kind of an explanation, but I would help readers to understand this more easily. [Maike Nicolai, Germany]	Accepted. We conclude from such comparisons that the models are producing more and more realistic simulations of climate, even though problems remain (as becomes clear in the later parts of this FAQ).
37383	90	12	90	14	"generally validated"? That's a sweeping generalisation without merit. AR5 Figure 3.3 showed how primitive those early models are. [John McLean, Australia]	Rejected. AR5 figure 3.3 does not show how primitive early models were; we are not sure which figure the reviewer is referring to. Indeed the models used by FAR were comparatively primitive. Key messages from FAR included e.g. that warming rates would range from 0.1 to 0.5 degrees/decade, depending on scenario. Indeed such rates of warming have ensued. FAR predicted greater warming over land than over the ocean (correct) and the Arctic to warm more than the rest of the globe (correct). To this level of granularity FAR has generally been validated. Of course models available then were low-resolution, low complexity, but some of the gaps of understanding recognized back then still remain relevant.
40813	90	12			specify that it's the IPCC 5th assessment report [TSU WGI, France]	Rejected. The text correctly refer to the First Assessment Report (FAR) of 1990 which set an early benchmark for climate understanding.
105027	90	16	90	16	This section FAQ is well written but suggest here -> "Climate models solve equations…" [Peter Gleckler, United States of America]	Noted. It is unclear which changes the reviewer wants to see.
17611	90	16	90	33	This gives a serious false impression and summary, because some of the key basic laws of physics for climate (ocean-atmosphere turbulent circulations) have coupled non-linear equations which cannot be solved as such. It neglects the crucial importance of tuning and parameterizations as clearly explained by Hourdin (BAMS March 2017) and Voosen (Science October 2016). For key processes of the climate modelling, parameters are used to describe the effects of clouds, ocean heat distribution, albedo,...Sometimes opposing parameters in sign and magnitude are used by different groups for the same physical proces. This is a seriously misleading summary of the climate modelling reality. Another good xample of groupthink and tunnel vision, neglecting the recent climate modelling literature (Voosen, Hourdin, Knutti,...) [ferdinand meeus, Belgium]	Noted. The summary is not "seriously false" but we agree that parameterizations are important (which replace some physical processes that cannot be solved explicitly). We have rebalanced the text to mention and give greater weight to remaining model inadequacies (which may well be linked to these parameterizations), and also mention the parameterizations explicitly here (without naming them as such).
39859	90	18		20	"the quantity and spacing… Earth's climate system" I would try to explain a bit better (or with an analogy) what a better resolution implies. [TSU WGI, France]	Not applicable. We have deleted the sentence here because the aspect of resolution is expanded in the fourth paragraph here, where also an example is given of what improved resolution means in terms of representing processes.
38691	90	25	90	25	"internal make-up" might sound very technical to the target audience of IPCC FAQs. I would say more specifically what this includes or use a more common term. [Maike Nicolai, Germany]	Accepted. We now simply note that "models continue to evolve" (i.e. the phrase is gone).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105029	90	25	90	25	Models continue to be improved with advancements in observations and theory, making them increasing suitable for simulating a variety… [Peter Gleckler, United States of America]	Accepted. We adopt the reviewer's suggestion.
28813	90	30			"eddies" --> "circulations" may be less technical [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The final text avoids both concepts.
26779	90	33	90	33	Would not "better represented" be more appropriate than "more realistic" [Eric Brun, France]	Not applicable. We have replaced this sentence with one that hopefully makes more sense to the lay reader.
38693	90	33	90	33	"coupling processes between the upper and the lower atmosphere" also sounds rather technical. Could this be simplified and highlighted what these processes are and why they are important to look at? For example "interactions between the upper and the lower layers of the atmosphere that influence the Earth's climate...." [Maike Nicolai, Germany]	Accepted. We have replaced the phrase with a new one which is hopefully less technical.
40353	90	35		40	how this paragraph fits with the rest of the structure is not that clear to me [TSU WGI, France]	Accepted. We have restructured this FAQ, hopefully discussing ESMs in a suitable context.
38695	90	36	90	36	What does the prescription of changes mean and imply, for non-specialists? [Maike Nicolai, Germany]	Accepted. We now state that such feedbacks affect atmospheric CO2 (and by inference global warming).
38697	90	38	90	38	There are also plants in the ocean that are able to absorb carbon dioxide. Please adjust. [Maike Nicolai, Germany]	Accepted. We have rephrased the whole paragraph, avoiding the phrase "plants on land".
102921	90	38	90	40	Interactive representations of the absorption of carbon dioxide and land is an improvement, but it does not do justice to the range of terrestrial ecosystems and the climate regulating service they provide. Also, these systems do not respond to environmental change in isolation, but in the presence of management impacts (current and legacy), which often dominate. The representation of these (and their interaction with the mentioned environmental change) should be mentioned. [Philippe Tulkens, Belgium]	Noted. We agree that this is a complex, multifaceted topic which however we cannot even begin to unpick in this context, and especially within the space constraints given here.
7289	90	39	90	39	including, for example, the impacts of warming and acidifying the oceans either on marine biota or on the biosphere as a large system. [Julio Cesar Barreto da Silva, Brazil]	Noted. In view of comment 38699, we prefer to avoid jargon terms such as "biota" or "biosphere" here. We have rephrased the whole paragraph, hopefully conveying the linkages better now.
38699	90	39	90	40	Suggestion to replace "ocean biology" by "marine life" or "plants and animals in the ocean" to avoid what might sound like jargon. [Maike Nicolai, Germany]	Accepted. We now use the term "marine life".
64655	90	42	90	55	Introduction Simulation models are evolving in Chapter 3 (CMIP5 and CMIP6) [Eman Abdelazem, Egypt]	Noted. It is not clear what changes the reviewer would like to see.
38701	90	43	90	48	This might become a useful explanation about how comparisons with observations helps to check th quality of models. But what are "observational estimates"? Would simply "observations" also work? Or analyses from observations? For me, as a non-native speaker and non-natural scientist, the word "estimate" makes this sound very vague again, and I wonder how reliable the basis for your "quality check" is. [Maike Nicolai, Germany]	Accepted. We have replaced "observational estimates" with "observations". Complications arise because of problems with these observations (to do with coverage, data discontinuities, spatial representativeness etc) hence we used the "estimates" previously, but for the target audience this is a moot point.
96309	90	45	90	45	Please extend the phrase "...often using multiple climate variables" to incorporate examples of what a 'climate variable' is, since this text addresses laypeople and 'climate variable' seems to be too abstract. Concrete instances of climate variables are mentioned in the following sentence, but it is not obvious for anyone, that these are the quantities meant by 'climate variables'. [Nicole Wilke, Germany]	Accepted. We now make clear that the three fields are examples of such climate variables.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38703	90	45	90	48	Are "multiple climate variables" the ones listed? Is sea level pressure a climate variable? Or are the elements listed examples of what has improved in the models? The connection of the two sentences might not be entirely clear. [Maike Nicolai, Germany]	Noted. Indeed SLP is a climate variable. The three quantities are chosen because they are widely understood by non-specialists. We have slightly rephrased these sentences, hopefully addressing the reviewer's concern.
11317	90	47	90	47	FAQ3.1 should be FAQ3.2 [Masahiro Watanabe, Japan]	Accepted. We have corrected this error.
38705	90	49	90	49	The target audience of IPCC FAQs might not know what you refer to when speaking of "intercomparisons". [Maike Nicolai, Germany]	Accepted. We have replaced "intercomparisons" with "evaluations".
40987	90	49			the mention of intercomparisons is a bit out of context for people not familiar with modelling [TSU WGI, France]	Accepted. We have replaced "intercomparisons" with "evaluations".
38707	90	52	90	55	The various models might have to be introduced and explained more simply for non-specialists. [Maike Nicolai, Germany]	Noted. We have made an effort to describe models in simple terms earlier in the FAQ and hence do not wish to repeat this here. We have removed the word "climate" to just talk about "models", in line with previous usage and to avoid a possible confusion that "climate models" might exclude ESMs.
28815	90	52			suggest "1 km needed to begin realistically representing clouds." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We adopt the reviewer's suggestion.
37385	90	53	90	55	If you are saying that earlier generations of models performed poorly then please be explicit about this. [John McLean, Australia]	Rejected. We are not saying that previous generations performed poorly. We only say that there is progress in the field of climate modelling.
12055	90	54	90	54	"Figure 1" should be "FAQ 3.2 Figure 1". [Masaki Satoh, Japan]	Accepted. We follow the reviewer's recommendation.
37387	91	6	91	13	Why are you basing this on only 20 years of data? Please explain. [John McLean, Australia]	Accepted. We now explain that this period is chosen because 1980 marks roughly the start of the satellite era, heralding a quantum leap in the quality and quantity of meteorological information available for validation, and 1999 marks the end of the "historical" simulations conducted for CMIP3. Twenty years is also a typical length of climate simulations needed to obtain a robust evaluation of most aspects of background climate.
7291	91	12	91	12	Only one simulation was used on each tested model (CMIP3, CMIP5 and CMIP6) together with the historical series. (Figure produced with ESMValTool v2.0.0b2.) [Julio Cesar Barreto da Silva, Brazil]	Accepted. We replaced the phrase with 'Only one simulation per model is used from each of the CMIP3, CMIP5, and CMIP6 “historical” experiments.'
40139	92	0			FAQ3.3 reads really well! [TSU WGI, France]	Noted. Thanks very much.
40391	92	0			I wonder if it wouldn't be worth mentioning, as another line of evidence the acidification of the ocean and other consequences which are not related to warming of the atmosphere but to the increase in GHG [TSU WGI, France]	Rejected. This is an example of a human influence on the earth system, but is not evidence that human's are responsible for climate change.
40513	92	0			maybe "solar brightness" is a bit too jargony but that's a detail [TSU WGI, France]	Taken into account. Changed to 'the brightness of the sun'.
17613	92	1	92	40	This argument does not hold because based on circular reasoning. The models are based and coded with formulas for which natural variability (internal and external) is small in magnitude and time scale, so it is normal that model output projections confirm this input. [ferdinand meeus, Belgium]	Rejected. Internal variability is an emergent property of models, it is not coded in directly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37399	92	1	92	52	Climate models don't provide evidence. They are created on what's known and what's assumed. Even they produce approximately correct results, that doesn't mean that they accurately embody every factor. (The analogy is that there is more than a single pair of numbers that sum to 10.) In other words, every claim in this FAQ about models producing evidence is false. (I'm just puzzled as to why any proper scientist would make such a claim.) [John McLean, Australia]	Rejected. As described in this FAQ, climate models are based on physical principles and embody our current understanding of how the climate system is expected to respond to changes in forcings.
21561	92	1			I am not sure that both FAQ 3.1 and FAQ 3.3 are required. They end up sounding a bit repetetive of each other. I think FAQ 3.3 is probably marginally the better of the two presently. [Peter Thorne, Ireland]	Taken into account. FAQ 3.1 and FAQ 3.3 have been re-written to make them more distinct.
106509	92	1			For FAQ 3.3, this FAQ would be greatly strenthend by including the key fingerprints in the patterns of climate change that implicate GH gases as driver of recent warming: e.g. (1) winter warminer more than summer (2) night warming more than day and (3) surface warming and upper atmosphere cooling - all pointing to increased retention of warmth rather than other drivers. I find the public responds well to this additional line of evidence for humans causing the current global warming trends [camille parmesan, France]	Taken into account. An additional sentence on other changes in the climate system including warming of the troposphere and cooling of the stratosphere has been added.
38711	92	3	92	6	Can exemples for "paleoclimate records" be given or the expression be simplified or explained even more? I am thinking of something like "data obtained from rocks, sediments, corals, shells, tree rings". [Maike Nicolai, Germany]	Taken into account. Tree rings are now given as an example.
37391	92	4	92	4	I think you are referring to climate models (i.e. models of climate) rather than "computer models", which logically are models of computers. [John McLean, Australia]	Taken into account. Changed to 'computer simulations of past climate change'.
7293	92	5	92	5	Exclusion of the words "allows us to clearly": [...] principles, help us to identify the dominant [...] [Julio Cesar Barreto da Silva, Brazil]	Taken into account. This has been re-phrased.
38713	92	5	92	6	The way I interpret "allows us to clearly identify...", it implies that scientists (I think they are referred to by "we" here) seem to aim to attribute the dominant role to humans. So this does not sound like an unbiased and open-ended process. My suggestion would be to put this more neutrally, for example: "If information from observations of climate change, from paleoclimate records that can show changes over the past thousands of years, and from computer models that can simulate past climate change based on physical principles is synthezised, it becomes obvious that humans have the most dominant role in driving recent climate change." I also replaced "we" because it is not clear if this refers to the scientific community or society in general, the readers of the FAQs or another group of people. Btw. the different uses of "we" in the FAQs might need checking. Sometimes it seems to refer to the authors, sometimes to society. The references to society might be perceived as prescriptive in some cases. [Maike Nicolai, Germany]	Taken into account. Re-written as 'the dominant role of humans in driving recent climate change is clear'.
7295	92	7	92	7	Add the text that justifies deleting the previous expression: Based on chaos theory, it is evident that it is not always possible to predict the behavior of a system, since there are deterministic, continuous and discrete systems, whose behavior is practically unpredictable due to the great sensitivity to changes in initial conditions (LUFFIEGO GÁRCIA et al., 1994). Therefore, the power of predictability in the study of Sciences is limited, as it is considered the property that certain supposedly nonlinear functions have in exponentially amplifying any deviation (or error), however small, preventing any long-term prediction, and leading to erratic behavior, which seems to obey only the rules of chance, despite the strict determinism of these functions (BERGÉ, 1996, p. 74). [Julio Cesar Barreto da Silva, Brazil]	Rejected. While the proposed text is reasonable, it relates to initial condition predictability, of the kind done in weather forecasting, rather than to projection of changes in the mean climate (the attractor, in the language of chaos theory), in response to a change in boundary conditions.
7297	92	8	92	8	The climate is influenced by a number of factors, whether natural or anthropogenic. [Julio Cesar Barreto da Silva, Brazil]	Noted. This is also explained in the box.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37389	92	11	92	15	This claim is unsubstantiated because no climate models have been validated. [John McLean, Australia]	Rejected. Evaluation of models is assessed in the chapter, and covered in much of the cited literature.
39173	92	14	92	40	Do not use the term "multiple lines of evidence" repeatedly in explaining why recent climate change in human-driven. [Lourdes Tibig, Philippines]	Accepted. One instance of this phrase has been modified, leaving only a single instance.
102923	92	15	92	15	add: The continued destruction of ecosystems leads to incresaed greenhouse gas emissions and weakens the natural carbon sink. [Philippe Tulkens, Belgium]	Rejected. The FAQ already indicates that increases in GHGs are the result of human activities. The proposed addition would be too much detail for this FAQ.
7299	92	17	92	17	First, the current rates on the increase in the concentration of the main greenhouse gases are, without precedent, the highest ever seen in the last 22,000 years. Several authors show that these increases are the result of human activities, the well-known anthropic effects (Boden; Marland; Andres, 2009; WRI, 2014; EPA, 2016). [Julio Cesar Barreto da Silva, Brazil]	Rejected. We are not allowed to add citations to FAQs.
112673	92	17	92	17	First of all, instead of "firstly" [Melissa Jiménez Gómez Tagle, Germany]	Taken into account. 'Firstly' deleted.
127361	92	17	92	18	Seems worth mentioned that pCO2 is unprecedented in > 800,000 years, and that the Seuss effect shows conclusively the increase is due to fossil fuels and deforestation. [Trigg Talley, United States of America]	Taken into account. This has been added.
114737	92	18	92	18	check consistency with what ch2 says about this [Jan Fuglestvedt, Norway]	Taken into account. Revised to 800,000 yrs on the basis of ch2 assessment.
37395	92	24	92	33	AR5 said of CMIP5 climate models "... an analysis of the full suite of CMIP5 historical simulations (...) reveals that 111 out of 114 realisations show a GMST trend over 1998–2012 that is higher than the entire HadCRUT4 trend ensemble ...." [WGI contribution, chapter 9, text box 9.2, page 769, and in full Synthesis Report on page SYR-8]. It also said "There may also be a contribution from forcing inadequacies and, in some models, an overestimate of the response to increasing greenhouse gas and other anthropogenic forcing (dominated by the effects of aerosols)." [WG I SPM, section D.1, page 15, bullet point 2, and in full Synthesis Report on page SYR-8], and "This difference between simulated [i.e. model output] and observed trends could be caused by some combination of (a) internal climate variability, (b) missing or incorrect radiative forcing and (c) model response error". [WGI contribution, chapter 9, text box 9.2, page 769] Where is your evidence that CMIP6 models don't have the same flaws? Such information is critical to your claim that models only replicate observations when GHGs are included. [John McLean, Australia]	Noted. Please see Cross-Chapter Box 3.2 which assesses this.
7301	92	26	92	26	These climate models show a dominant effect of warming on the increase in greenhouse gases (gray band, which shows the effects of heating greenhouse gases by themselves), which was partially offset by the cooling effect on increases in aerosols. atmospheric (blue band). [Julio Cesar Barreto da Silva, Brazil]	Rejected. We find the existing phrasing clearer.
38715	92	29	92	31	If I understood correctly from FAQ3.1, El Nino counts as internal while variations in solar brightness and emissions from large volcanoes are external. Would it make sense to clarify this here again? [Maike Nicolai, Germany]	Rejected. The reviewer is correct, but our purpose here is just to explain which affects are including in the simulations with natural forcings.
7303	92	32	92	32	Replacing the term "much smaller": reproduce the observed warming – they simulate much lower temperature trends, indicating that these [...] [Julio Cesar Barreto da Silva, Brazil]	Taken into account. This text has been re-phrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38717	92	32	92	33	This is an important message that could probably highlighted more strongly, for example by starting a new sentence instead of adding it as some kind of hyphenated afterthought to the previous one and spelling out the conclusion more clearly, risiking a repetition: "...reproduce the observed warming. The fact that simulations including only natural processes show much smaller temperature trends (or: "much smaller temperature increases" if that is correct, too?) indicates that natural processes alone cannot explain the strong warming rate observed. The observed rates can only be reproduced when human influence is added to the simulations." [Maike Nicolai, Germany]	Accepted. Text similar to that proposed added.
7305	92	33	92	33	Correction of the expression "strong warming rate observed": natural factors cannot explain the strong rate of warming observed. [Julio Cesar Barreto da Silva, Brazil]	Accepted. Suggested change made.
37397	92	35	92	40	Laughable. A correlation does not prove cause. I remind you that AR5 said "... the rate of warming over the past 15 years (1998–2012; 0.05 [–0.05 to 0.15]°C per decade) ... is smaller than the rate calculated since 1951 (1951–2012; 0.12 [0.08 to 0.14] °C per decade)." [WG I SPM, page 5, section B.1, bullet point 3, and in full Synthesis Report on page SYR-6] and that this uncertainty that any warming had occurred was despite a definite increase in atmospheric CO2. [John McLean, Australia]	Noted. Please see Cross-Chapter Box 3.2 which assesses this.
38719	92	35	92	40	Some of your readers might recall there were warmer periods and different climatic conditions in the more distant past. Have you considered clarifying that the comparison of the different 50-year periods is only true for the past 2000 years? [Maike Nicolai, Germany]	Rejected. Our focus here is on the rate of warming, and the comparison with the past 2000 years is based on the assessment of Chapter 2.
7307	92	39	92	39	Together, this evidence shows that humans are the dominant cause of the global warming observed in recent decades. [Julio Cesar Barreto da Silva, Brazil]	Rejected. We prefer 'Taken together'.
7309	92	49	92	49	FAQ 3.3, Figure 1: The models show a significant increase in global averages in surface air temperature, mainly in response to greenhouse gases (red stripe), mainly combined with human and natural forcing (gray stripe), whose increase expanded since 1960, with possible causes of the accelerated urban and industrial process (packed by the automotive industry) that occurred in this decade. [Julio Cesar Barreto da Silva, Brazil]	Rejected. The attribution of emissions to particular sectors is outside the scope of WGI.
40185	93	0			Fig FAQ3.1: the spatial influence on natural variability is maybe missing in here [TSU WGI, France]	Noted, the spatial influence of natural variability is considered in the revised FAQ text.
40187	93	0			fig FAQ3.1: would it be clearer to use different colours to discriminate the component of the temperature change (natural vs human) Instead of showing warming vs cooling with different colours? [TSU WGI, France]	Accepted, this figure has been substantially updated and this comment has been considered during these updates.
13399	93	3	93	4	Wrong way to reference [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. This has been resolved in the final version.
13401	93	17	93	17	Misuse of } { [Maria Amparo Martinez Arroyo, Mexico]	Taken into account. This has been resolved in the final version.
40189	94	0			fig FAQ3.2: alternative observation is a bit confusing here [TSU WGI, France]	Accepted. This comment has been misplaced. We have removed the "alternative observations" from the figure.
40191	94	0			fig FAQ3.2: the caption/label needs to be simplified : a lay audience doesn't know what CMIP refers to [TSU WGI, France]	Accepted. We have simplified the caption and labels.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115547	94	5	94	5	Arazagüena et al 2018 has appeared in ACP 2018: How to cite. Ayarzagüena, B., Polvani, L. M., Langematz, U., Akiyoshi, H., Bekki, S., Butchart, N., Dameris, M., Deushi, M., Hardiman, S. C., Jöckel, P., Klekociuk, A., Marchand, M., Michou, M., Morgenstern, O., O'Connor, F. M., Oman, L. D., Plummer, D. A., Revell, L., Rozanov, E., Saint-Martin, D., Scinocca, J., Stenke, A., Stone, K., Yamashita, Y., Yoshida, K., and Zeng, G.: No robust evidence of future changes in major stratospheric sudden warmings: a multi-model assessment from CCMI, Atmos. Chem. Phys., 18, 11277–11287, https://doi.org/10.5194/acp-18-11277-2018, 2018. [Rolf Müller, Germany]	Editorial. Revised in final version.
32673	99	1	99	55	Page 99 is more than half empty [sadegh zeyaeyan, Iran]	Editorial. Revised in final version.
33003	99	1	99	55	Page 99 is more than half empty [Sahar Tajbakhsh Mosalman, Iran]	Editorial. Revised in final version.
109021	99	5	99	5	Change 'mediterranean' to 'Mediterranean' [Belen Martrat, Spain]	Editorial. Revised in final version.
79485	99				Page 99 is more than half empty (comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Editorial. Revised in final version.
109019	101	58	101	58	Change 'Mcgregor' to 'McGregor' [Belen Martrat, Spain]	Editorial. Revised in final version.
41891	103	34	103	36	It is my understanding that the journal Scientific Reports abbreviates to Sci. Rep. (i.e. Nat should be deleted) [Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Revised in final version.
13403	108	37	108	41	Incomplete References [Maria Amparo Martinez Arroyo, Mexico]	Editorial. Revised in final version.
109023	110	13	110	13	reference Kaufman et al., submitted Sci Data: please update [Belen Martrat, Spain]	Editorial. Revised in final version.
35911	111	3	111	5	Reference info updated to: Lee, J., Sperber, K. R., Gleckler, P. J., Bonfils, C. J. W., and Taylor, K. E. (2019). Quantifying the agreement between observed and simulated extratropical modes of interannual variability. Clim. Dyn. 52, 4057–4089. doi:10.1007/s00382-018-4355-4. [Jiwoo Lee, United States of America]	Editorial. Revised in final version.
32131	111	30	111	32	delete repeated reference and change the labels in the text deleting a/b [Anja Wendt, Germany]	Editorial. Revised in final version.
4171	112	32	112	32	There is a citation mistake and one of "Li, G. and Xie, S.-P." should be deleted. [Wenqi Zhang, China]	Editorial. Revised in final version.
30683	113	19	113	20	Please include article number for this paper. Lovenduski, N. S., N. Gruber, and S. C. Doney (2008), Toward a mechanistic understanding of the decadal trends in the Southern Ocean carbon sink, Global Biogeochemical Cycles, 22, GB3016, doi: 10.1029/2007GB003139. [Ian Simmonds, Australia]	Editorial. Revised in final version.
104403	118	47	118	48	Please update publication citation to: Parsons, L. A., M. K. Brennan, R. C. Wills, and C. Proistosescu (2020), Magnitudes and Spatial Patterns of Interdecadal Temperature Variability in CMIP6, Geophys. Res. Lett., 47(7), e2019GL086588. [Luke Parsons, United States of America]	Editorial. Revised in final version.
30685	121	26	121	27	Bibliographical details of published paper (please note slight change of title): Roach, L. A., J. Dörr, C. R. Holmes, F. Massonnet, E. Blockley, D. Notz, T. Rackow, Marilyn N. Raphael, S. P. O'Farrell, D. A. Bailey and C. M. Bitz, 2020: Antarctic sea ice area in CMIP6. Geophysical Research Letters, 47, e2019GL086729, doi: 10.1029/2019GL086729. [Ian Simmonds, Australia]	Editorial. Revised in final version.
32133	124	28	124	29	delete repeated reference and change the labels in the text deleting a/b [Anja Wendt, Germany]	Editorial. Revised in final version.
30687	124	35	124	38	Reference to Shepherd paper is inserted twice. [Ian Simmonds, Australia]	Editorial. Revised in final version.
32135	124	37	124	38	delete repeated reference and change the labels in the text deleting a/b [Anja Wendt, Germany]	Editorial. Revised in final version.
127363	133	1	133	11	Figure 3.1 is okay, but not overly compelling. Suggest labeling the x-axis (degrees K/C?) and clarifying in caption the 0/0 "current day" time period (if this is paleo work, does current day refer to 1950 or 2020?) [Trigg Talley, United States of America]	Noted. Figure 3.1 has been remade entirely.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32675	133	1	133	55	The resolution of the figures are not enough [sadegh zeyaeyan, Iran]	Noted. These were draft figures which have been updated and improved.
33005	133	1	133	55	The resolution of the figures are not enough [Sahar Tajbakhsh Mosalman, Iran]	Noted. These were draft figures which have been updated and improved.
21563	133	3	133	3	The years denoted inline in each panel need either explaining or, if not material to the interpretation of the figures should be deleted. [Peter Thorne, Ireland]	Noted. Figure 3.1 has been remade entirely.
96311	133	6	133	6	Caption of Fig. 3.1: Please explain the phrase 'anomaly in mean annual temperature' for laypeople, e.g. with 'variations in mean annual temperature'. [Nicole Wilke, Germany]	Rejected. Anomaly is not a technical term, and "variations" is not an appropriate synonym.
21565	133	6	133	11	I think I am right in inferring that the model runs are identical and the difference relates to our updated understanding of LGM conditions. But the caption is insufficiently clear in this regard. I suggest redrafting the figure caption for clarity. This may be symptomatic of the issue of caption material being present instead in the text raised in various other comments. [Peter Thorne, Ireland]	Noted. Figure 3.1 has been remade entirely.
79235	133	6	133	11	Figure 3.1: Is the line width of MIROC-ES2L and MPI-PMIP4 larger because these are CMIP6 models? What are the numbers behind the proxy-reconstructions? Please clarify in the caption. [Martin Stolpe, Switzerland]	Noted. Figure 3.1 has been remade entirely.
96313	133	6	133	11	It is confusing to see symmetric crosses as measures of temperature differences, since it would be expected that the crosses to represent some kind of uncertainty or variability, e.g. to be the visualization of an RMS. [Nicole Wilke, Germany]	Noted. Figure 3.1 has been remade entirely.
96315	133	6	133	11	The idea of this graph certainly is to show improvements. But to discover the improvements between CMIP5 and CMPI6 is left to the reader. Please mention them in the text. [Nicole Wilke, Germany]	Noted. Figure 3.1 has been remade entirely.
96317	133	9	133	11	The sentence "The coloured crosses show long-term modelled mean differences …. where there are temperature reconstructions" is hard to understand and it should be considered to replaced it by "The coloured crosses show differences between average temperatures from models and pre-industrial conditions, as determined from places, where there is information from temperature reconstructions." [Nicole Wilke, Germany]	Noted. Figure 3.1 has been remade entirely.
99871	133		133		The meaning of the values in brackets in the legend of the black points is not stated. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure 3.1 has been remade entirely.
79487	133				The resolution of the figures are not enough (comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Noted. These were draft figures which have been updated and improved.
21567	134	1	134	1	It is really unhelpful to have three identically titled panels all showing different things. Can you not use different titles to denote what each are? Remember that the figure may be lifted and used in lectures / public outreach but shorn of the captions. [Peter Thorne, Ireland]	Accepted. Panel titles have been improved.
2547	134	1	134	13	Especially in the data-sparse Southern Ocean, the bias and rmse maps need some sort of assessment as to how large is important. As it is, I am not sure any bias is significant outside of the upwelling zones. Only with a careful reading of the corresponding text was it clear that the low and high resolution maps correspond to the same subset of models, whose size is only apparent in Fig. 3.3. [Bryan Weare, United States of America]	Accepted. Stippling has been added to indicate where biases are statistically significant.
2549	134	1	134	13	the in-figure labels need to make clear the differences between b),d), and e). The negative values in c) are clearly not possible. [Bryan Weare, United States of America]	Accepted. Panel titles have been improved. Issues with panel (c) have been solved.
68061	134	1	134	13	Fig 3.2, add ERA surface T mean for comparison and evaluation of bias and random error amplitudes. [MIchael Evans, United States of America]	Rejected. ERA5 temperatures are not the subject of discussion here.
2939	134	1	135	20	Please add 32 CMIP6 models results. Other figures in this chapter also need to provide 32 CMIP6 models results, or as many as possible CMIP6 models. [Zong Ci Zhao, China]	Accepted. Figures have been remade from all CMIP6 models available at the data cut-off date.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44043	134	1	176		the reviewer suggested that all the figures use the same number (as more as possible) of CMIP models and use the same model name. For example, Figure 3.35-36 use the institution ID to represent the model name. [Lijuan Li, China]	Taken into account. We try to use more consistent model names.
96319	134	1			Figure 3.2: Please adjust the colour bar range in order to increase the resolution of the information. [Nicole Wilke, Germany]	Rejected. The main biased areas are clearly highlighted by the chosen colour scale.
96321	134	4	134	11	The figure caption should explain what parameters are shown here. E.g. the phrase "Multi model (ensemble) mean" should be explained with "average over all CMIP6 models". Also, the term 'bias' could be explained with 'systematic deviation'. Please try to explain what is meant by "c) Multi model mean of the root mean square error of the seasonal cycle ...ERA5". Readers without a scientific background don't know about RMSE... [Nicole Wilke, Germany]	Taken into account. We have revised captions for greater clarity.
26783	134	9	134	10	Figure 3.2 : does it really make sense to compute and compare the multi-model means of LR and HR HighResMIP simulations? In some models the resolution of the LR simulation is actually higher than than the resolution of some HR simulations from other models. The same in true for Figure 10. [Eric Brun, France]	Noted. The number of models is small but the two ensembles are based on the same models in their low and high resolution versions, so this gives information on possible impacts of high resolution. But the discussion remains cautious.
26785	134	9	134	10	Figure 3.2 : we suggest to indicate the nimber of models considered [Eric Brun, France]	Accepted. Ensemble member numbers are now given in the caption.
26781	134		134		Figure 3.2 : we suggest to mention "low resolution" and "high resolution" in the title of Panels (d) and (e) respectively [Eric Brun, France]	Accepted. Panel titles have been improved.
99873	134		134		More precise sub-figure titles are required, i.e. it is 'surface AIR temperature' in panels a,b,d,e, and the seasonal cycle aspect needs to be in the title of panel c. Panels d,e also have the same title when they are showing different things. A figure should be as clear as possible without having to read the caption. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Panel titles have been improved.
10669	134		136		I am sorry to say that Figures 3.2, 3.3 and 3.4 are not great adverts for "ESMValTool v2.0.0b3". [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
111079	134				figure 3.2 it would be nice to see from the figure where the models encompass observations (ie where some models get to them) and where they are all offset systematically [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Stippling has been added to indicate where biases are statistically significant.
64657	135	1	135	2	Figure3.3 legend and low accuracy is unclear [Eman Abdelazem, Egypt]	Accepted. The labels are now more legible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96323	135	1	135	3	Fig. 3.3 amalgamates two substantially different types of information in each of the three sub-figures in a highly problematic way: The main parts show time series of annual temperature anomalies. In contrast to that, the insets on the right side of the three diagrams show something different instead of time series, namely absolute values of mean model temperatures, i.e. time-means in a certain period (1850-1900). So the figure merges a temporal development with time-averaged absolute baseline temperatures. A very similar figure in the respective chapter of the AR5 has led to tremendous confusion and numerous disputes about the question of what would be the "correct" GMST in absolute degrees Celsius for the pre-industrial period, or, as a best substitute for the latter, for the 1850-1900 period. Based on Fig. 3.3, laypersons could think that 13.0°C from the right y-axis corresponds to the 0.0°C value of the anomalies on the left y-axis. So, if at all, the y-axis of the inset would have to be shifted downwards in order to match the multi-model mean GMST of 13.7°C with the zero line from the left y-axis. But obviously, also this would be strongly misleading. Therefore, we entreat the authors vividly to separate the insets from the time series [i.e. in each sub-figure a), b) and c)] and shift them into an own sub-figure d) – or remove them completely. Please also add an explanation of these different types of information in the caption. [Nicole Wilke, Germany]	Rejected. It is best to show the disagreement in absolute temperature -- in fact, the text has been revised to point out that absolute temperatures remain different in models, citing Palmer and Stevens (PNAS, 2019).
2969	135	1	135	10	In Fig.3.3 (a), Amplify of temperature by CMIP6 looked larger than CMIP5. Why? Please give explanations. [Zong Ci Zhao, China]	Noted. This was discussed in the text, and that discussion has been revised: this is probably linked to ESMs having fewer prescribed elements than the previous generation of climate models. The figure now compares to CMIP5 more clearly.
127365	135	1	135	13	In Figure 3.3, suggest putting (c) on same time frame of other x-axes. Consider explaining that the vertical lines are volcanic eruptions, etc. [Trigg Talley, United States of America]	Noted. Panel (c) has been deleted.
2555	135	1	135	14	This critical figure MUST be completely redone. The large number of curves completely obsures the means that are discussed in the text. The observations need to have an appropriate 95% window to judge agreement. I suggest figures with the observational mean and 95% brackets, the model mean, 95% range, and total range. The individual model results should be in an online supplement along with other model output. Is frame b) necessary? If so, why only a subset of the models in a)? Surely the masked means are easy to produce for all models. Frame c) should include only the low res mean, high res mean and the observational mean with its 95% range. Only plotted something like this can clear conclusions be made in the text. [Bryan Weare, United States of America]	Taken into account. A panel comparing CMIP6 and CMIP5 envelopes has been added. Original panels (b) and (c) have been deleted.
10653	135	1	135	14	I think the reference to "Jones et al (2012)" in panel a is incorrect. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The panel now reads 2013. The caption is correct.
10655	135	1	135	14	What the asterisk is needs mentioning in the caption. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now clarified in the caption.
10657	135	1	135	14	Was there any measure of 'quality control' of the models used in these plots? For instance the equivalent figure in AR4 had a criteria for piControl 'drift', that if exceeded meant the model was excluded. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. All historical simulations available at the cut off date have been included.
10659	135	1	135	14	The inset parts of the panel show the absolute temperatures for the reference period. Was the data masked? This will cause funny things to happen to data that has not been taken as an anomaly of a reference period. I would recommend checking that this is a reasonable approach. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The data is not masked but the HadCRUT5 observational data mask is much more globally complete than in HadCRUT4, alleviating the issue mentioned by the reviewer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10661	135	1	135	14	There really is no need to add text pointing out a small selection of volcanoes on here. Why not point to where GHGs were increasing, or where solar activity increased as well? It is odd to only point out one forcing factor then not mention it in the main text. They are not the dominant forcing factor after all. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Volcanoes are highlighted because they cause clearly visible short-term changes in the warming timeseries.
10665	135	1	135	14	If the same sample of models can't be used in both panels a and b, I would recommend a 4th panel also showing panel a with the same sub selection that is in panel b. This would demonstrate quite nicely the issue of sampling from an ensemble of opportunity can influence results. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Panel (b) has been revised completely and that issue disappeared.
10667	135	1	135	14	If figure gets updated to 2020, the caption should note how historical experiments were extended beyond 2014. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure has been updated to 2020, and the SSP used for the simulations is indicated in the revised caption.
79263	135	1	135	14	Figure 3.3 (c): The multi-model mean for HighResMIP is missing. [Martin Stolpe, Switzerland]	Noted. Panel (c) has been deleted.
32677	135	1	136	55	The resolution of the figures are not enough and the graphs guide are not readable. [sadegh zeyaeyan, Iran]	Noted. These were draft figures which have been updated and improved.
33007	135	1	136	55	The resolution of the figures are not enough and the graphs guide are not readable. [Sahar Tajbakhsh Mosalman, Iran]	Noted. These were draft figures which have been updated and improved.
113667	135	3	135	4	"All anomalies are differences from the 1850–1900 time-mean of each individual time series" except (c), right? [Agnieszka Kowalczyk, Poland]	Noted. Panel (c) has been deleted.
21571	135	3	135	5	This is not the case in the final panel where the reference period differs from this. Suggest revise this opening to state that in each panel the reference period is shaded. [Peter Thorne, Ireland]	Noted. Panel (c) has been deleted.
113669	135	4	135	5	"The reference period 1850–1900 is indicated by grey shading" should read "The reference periods 1850–1900 and 1950-1979 are indicated by grey shading" [Agnieszka Kowalczyk, Poland]	Noted. Panel (c) has been deleted.
26791	135	10	135	10	The same models than in (a) should be used in the final figure [Eric Brun, France]	Noted. Original panel (b) has been deleted.
10663	135	10	135	11	Which of the different blending techniques described in Cowtan et al (2015) is being used here. They give very slightly different results. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Original panel (b) has been deleted.
50717	135	figure 3.3			Would it be possible to increase the font size for the names of the models, especially in plots b & c where there is more room available to do so. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The labels are now more legible.
26787	135		135		Figure 3.3 The quality of the labels of subfigures should be improved. [Eric Brun, France]	Accepted. The labels are now more legible.
26789	135		135		We suggest to draw lines from 1970 in graph b to the upper left corner of graph c to highlight that c is on a reduced time scale. [Eric Brun, France]	Noted. Panel (c) has been deleted.
99875	135		135		Why are fewer models shown for panel b? Do you really need to have each model shown in a different colour? Its virtually impossible to pick out individual lines anyway and this confuses rather than clarifies. Yellow lines on a white background is also unclear and overall it is not a very accessible figure. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure has been revised extensively and the panel (b) has been changed. Individual models are still shown with a different colour for consistency with past figures.
116259	135		135		Could this figure also show the radiative forcing in model simulations? [Valerie Masson-Delmotte, France]	Rejected. A timeseries of modelled radiative forcing is not available for all forcers. More information on time-dependent forcings in CMIP6 has been added to Chapters 6 and 7, with links from Chapter 3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79489	135		136		The resolution of the figures are not enough and the graphs guide are not readable. (comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Noted. These were draft figures which have been updated and improved.
21573	136	1	136	1	Use of a linear scale in latitude rather than cos(lat) heavily over-emphasises high-latitude features and heavily under-emphasises low latitude features. Using cos(lat) weighting for this and all similar plots would better give an idea of the areal extent and should be considered. [Peter Thorne, Ireland]	Accepted. Figure revised accordingly.
64659	136	1	136	2	Figure 4.3 legend and low accuracy is unclear [Eman Abdelazem, Egypt]	Accepted. The labels are now more legible and the figure quality has been improved.
127367	136	1	136	6	What is the time period for Figure 3.4? [Trigg Talley, United States of America]	Accepted. The caption now gives the time periods used in the figure. Observed datasets are now analysed over the 1995-2014 period.
2571	136	1	136	8	Again, the indivicual model curves overwhelm and confuse. I was not sure which were the very divergent obs until blowing up considerably. Some uncertainty reange on the obs is needed. Then simplify to model mean and range. [Bryan Weare, United States of America]	Rejected. Taking the model mean dampens variability, which substantially lessens the interest of the comparison.
10671	136	1	136	8	I presume the model data has not been masked by HadCRUT4, which probably explains why high latitude variability is lower than HadCRUT4. The reason Cowtan/way(?) has low variability in same regions may be a consequence of its infilling into observationally sparce regions (Jones AAS, 2016). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Upgrading to HadCRUT5 means that datasets are more globally complete and more comparable among each other, which partly solves the coverage issue. The text now more clearly cautions about interpreting variability at high latitudes where coverage is incomplete.
79249	136	3	136	6	Figure 3.4: Add the period over which the standard deviation was computed for the observations. I'm also surprised by the large differences between the observational products. Could this be an artifact of incomplete coverage (i.e., only few grid cells available and then large variability?). If this is the case, I would suggest to compute the standard deviation only over a period with high coverage or to omit datasets with limited coverage. I also suggest to use the same period for the models (instead of the control simulation) to make it more comparable to the observations. [Martin Stolpe, Switzerland]	Accepted. The caption now gives the time periods used in the figure. Observed datasets are now analysed over the 1995-2014 period. Upgrading to HadCRUT5 means that datasets are more globally complete and more comparable among each other, which partly solves the coverage issue. The text now more clearly cautions about interpreting variability at high latitudes where coverage is incomplete.
50719	136	figure 3.4			Again, a larger font for the model key would be really helpful. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The labels are now more legible.
26793	136		136		Figure 3.4 Which period is used? What is shown precisely: the interannual standard deviation? [Eric Brun, France]	Accepted. The caption now gives the time periods used in the figure. Periods depend on dataset but are all in the 1850-1900 averaging period. The caption also clarifies that interannual variability is shown.
88947	136		136		Figure 3.4 There are large differences between the different observational products at high latitudes. This is presumabely due to how they have masked / infilled spatially when observations are sparse. This makes it very difficult to compare to the models. I think it would make more sense to mask all models and observations to where observations are available (e.g. HadCRUT4 coverage) so that a like for like comaprison is possible. [Schurer Andrew, United Arab Emirates]	Taken into account. The figure shows different observational datasets, and showing a masked version of the modelled timeseries for each dataset would make the figure too heavy. However, upgrading to HadCRUT5 means that datasets are more globally complete and more comparable among each other, which partly solves the issue. The text now more clearly cautions about interpreting variability at high latitudes where coverage is incomplete.
99877	136		136		Same comment as for Fig. 3.3. Why label the lines when they can't be distinguished anyway? The legend is unreadable. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The labels are now more legible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116261	136		136		Missing information in caption (for which period?) [Valerie Masson-Delmotte, France]	Accepted. The caption now gives the time periods used in the figure. Periods depend on dataset but are all in the 1850-1900 averaging period.
111081	136				is this figure annual? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The caption now clarifies that interannual variability is shown.
104405	137	1	137	1	Please update figure to final published, corrected version from Parsons et al., 2020 [Luke Parsons, United States of America]	Rejected. That figure has been completely remade, in a style that differs from Parsons et al. (2020).
21575	137	1	137	6	Has GISTEMP been detrended prior to calculating the sigma? If not how is that comparable to remaining panels? [Peter Thorne, Ireland]	Rejected. The point of comparing to GISTEMP is that internal variability in some models is a substantial fraction of forced variability in GISTEMP.
10673	137	1	137	7	What reference period is used to calculate anomalies? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The caption now clarifies that anomalies are with respect to the long-term mean GMST in CMIP6 simulations and the 1951–1980 mean in the GISTEMP data.
10675	137	1	137	7	At least several of these panels show piControl 100 or so years into the experiments. Why was the start of the picontrols rejected? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure used the last 400 years of each piControl. This has been changed to all available years.
2965	137	1	137	10	Please add CIESM model results. Reference: Lin et al., Community Integrated Earth System Model (CIESM): description and evaluation, Journal of Advances in Modeling Earth Systems, submitted. [Zong Ci Zhao, China]	Noted. The figure now includes all models that provide the data needed in the CMIP ESGF at the data cut-off date.
127371	137	3	137	3	Authors may want to define the control (unforced) simulations in the caption of Figure 3.5. [Trigg Talley, United States of America]	Accepted. The caption now clarifies that the control simulation is CMIP6's piControl experiment.
127369	137	3	137	7	Some of the models shown here (e.g., EC-EARTH3) show quite large centennial-scale variability which, if accurate, would raise significant questions about GMST attribution. This point should be addressed if it is not elsewhere. [Trigg Talley, United States of America]	Noted. That very point is discussed in the text, and that discussion has been revised following comments made there.
50721	137	figure 3.5			This would be easier to read if the red colour was a stronger red and therefore easier to differentiate from the blue. I think the combination of a purple/burgundy and blue is harder to differentiate for colour blindness. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. That figure has been completely remade.
26795	137		137		Figure 3.5 : Some models have very long pi control experiments. it would be interesting to show an additional 400 year period and discuss, if it is the case, that characteristics of variability can change between 400 year periods due to low frequency variability or transition between chaotic and organised behaviour. [Eric Brun, France]	Accepted. Time series are now shown for the whole duration of each piControl experiment available at CMIP.
99879	137		137		This is an important figure but it is hard to see the dark red lines on top of the dark blue lines. Why not just show the blue lines and quote the interannual standard deviation? The title is also incorrect - it is global surface temperature from NASA GISTEMP, not surface air temperature. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. That figure has been completely remade.
21577	138	1	138	1	Inline keys should be placed under the panel pairs to avoid the keys overlapping the data in each top panel. It would be even better if the forcings could be spelt out in full in a below the figure key - Natural, Anthropogenic, Greehnouse gases, other anthropogenic, so that the reader did not have to guess what these codes meant. Panels need addition of self-describing titles so the figure stands alone without the caption or the text. [Peter Thorne, Ireland]	Accepted. Labels are now friendlier.
127373	138	1	138	1	Figure 3.6 is not the most intuitive or informative graphic. Why is the GHG and OTH the same color family as NAT and not ANT? [Trigg Talley, United States of America]	Accepted. Labels are now friendlier.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10677	138	3	138	14	What experiments were used to deduce these signals? I am guessing that ANT/NAT were deduced from historical/hist-nat experiments. Are GHG/NAT/OTH deduced from historical/hist-GHG/hist-Nat? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The caption now clarifies the pair of experiments that were used to isolate each single-forcing contribution.
99881	138		138		The black and dark blue bars are very hard to distinguish from each other. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We used the IPCC colour scale.
21579	139	1	139	1	Figure needs a title. There is room below to write out the forcings in full rather than speaking in code. Just minor tidies could greatly increase accessibility of this figure as a stand-alone item. [Peter Thorne, Ireland]	Accepted. Labels are now friendlier.
10679	139	2	139	17	How is the uncertainty range in the observations calculated? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. In the SOD, it was the assessed warming in GSAT in 2009-2018 from Chapter 2, with its 5-95% uncertainty range. This has been updated to the 2010-2019 assessed warming in GSAT. The description of how the uncertainty range was derived is in Chapter 2.
10681	139	2	139	17	If the observational uncertainty represents the spread across different datasets, were the attribution studies also done with the different observational datasets? If not then the attributed trends will not have enough uncertainty spread (Jones and Kennedy, Journal of Climate, 2017) when being compared to multiple observational datasets. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Studies use slightly different datasets, and in some cases account for a measure of observational uncertainty, which is generally found to be small. But we took a conservative approach and assessed the directly calculated 5-95% ranges as likely ranges to account for this and other sources of error not directly accounted for.
10683	139	2	139	17	If I understand this plot correctly, internal variability is not factored into any of the trend uncertainties. It probably should be included, it often is added to the observed trend uncertainties. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have added error bars to show the amplitude of trends associated with internal variability, as was done in AR5.
99883	139	16	139	16	Coloured 'symbols' rather than 'circles' [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Caption reworded as suggested.
26797	139		139		Figure 3.7 :it is not obvious from this figure to understant that GHG and OTH are the decomposition of ANT. A solution could be to reorder the x axis Obs, NAT, ANT then have a blank or a dash line and GHG and OTH [Eric Brun, France]	Accepted. An arrow and bracket now clarify that Anthropogenic is decomposed into Greenhouse Gases and Other Human Forcings.
2967	140	1	140	5	Please provide the numbers of CMIP6. All figures of Chapter 3 should give the numbers of CMIP6. [Zong Ci Zhao, China]	Accepted. The caption now gives the number of CMIP6 models used.
2583	140	1	140	9	The "historical" model means in brown are nearly impossible to see even though they are by far most important. This would be far more interpretable if the shading for the nat, GHG and aer were removed at least in the lower frames. The obsevations might also be smoothed since there is no expected relation with the model output of year-to -year variations. [Bryan Weare, United States of America]	Accepted. The figure has been revised entirely to improve its legibility.
10685	140	1	140	10	I admire the ambition of this figure (better that anything I could do), but I think it is struggling to succeed to clearly communicate what the different regions are showing. The vertical range is too small for some of the panels. There are too many colours to sucessfully show what is going on. Smoothing from annual to decadal means might help. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure has been revised entirely to improve its legibility.
10687	140	1	140	10	If figure gets updated to 2020, the caption should note how historical experiments were extended beyond 2014. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure has been updated to 2020, and the SSP used for the simulations is indicated in the revised caption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35029	140	1	140	10	This figure arrangement, based on color matching between regions and map is not color-blind effective. It would be better to have the map in the center of the regional panels, so that arrows as well as colors and names can be used to match. [Baylor Fox-Kemper, United States of America]	Taken into account. The figure has been revised entirely to improve its legibility.
26801	140	4	140	4	Figure 3.8 : Is one member per model used? If not, how is the range computed? [Eric Brun, France]	Accepted. The caption now clarifies that the range gives the minimum and maximum of the CMIP6 range.
26799	140		140		Figure 3.8 : It would be good to change the scale of the y-axis so that the entire evolution could be seen. For example, for hist over Europe even the multi-model mean is not visible for many years. [Eric Brun, France]	Accepted. The figure has been revised entirely to improve its legibility.
99885	140		140		This figure is hard to parse and could be simplified. Do you really need to show all the different shadings to make the points in the text? The regions map and coloured outlines could be removed with just a reference to Figure 1.15 on regional definitions. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure has been revised entirely to improve its legibility.
2587	141	1	141	7	The observed trends for these arbitrarily chosen time periods differ by so much to be nearly useless. What are the corresponding confidence regions for the observations? I question using fixed ocean models in such an analysis. These are not those used for the future. The Mitchell et al. results should be used to formulate a more useful figure. I suggest this figure be replaced by smoothed yearly values at say three sample levels, 850, 200, 30mb. [Bryan Weare, United States of America]	Taken into account. The periods were chosen to cover the whole instrumental era (1979-2014), ozone depletion era (1979-1997), and (ozone recovery era) 1998-2014. The split into two sub-periods was done because of ozone depletion then recovery, meaning that a single linear trend over the whole period is of limited use. Observational uncertainties have now been added. Fixed-SST simulations are useful to isolate upper air temperature biases that are due to an upward propagation of SST biases, as discussed in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52831	141	1	141	7	I commend the authors for including this figure. I had advocated for this figure to be included in AR5 and found it had been eliminated from the main text and placed without comment in the supplementary material even though its meaning for model evaluation was substantial. The most important panel of Fig. 3.9 is the left panel because it denomstrates models are running too hot and that (via the fixed SST comparison vs. interactive ocean) the models have unrealistic positive feedbacks. This should be stated in the text. However, the right panel is inapprorpate because the short 17-year time period allows natural variability to have a large impact due to the major ENSO in 1998. My recommendation is to modify these three panels by including the extra 5 years of 2015-2019 using the ssp245 (any scenario would do as they do not differ in this period). This is done in the State of the Climate 2019 for the 1979-2019 period. I hope to attach the result as a plot, but if unsuccessful, I have forwarded it to Blair Trewin who can supply it to the team. (Note that inthe 1998-2019 period, the observations and model average have the same surface trend - so you can see the strong tendencies in models to show too much lapse-rate adjustment above 400 hPa. The main point is the 22-year period 1998-2019 will have major ENSOs at both ends, to damp this short-term variaiblity impact on the trend. [Also note that the radiosonde trends are certainly too warm due to the issue mentioned in the previous comment.) However, the main utility of the right panel, if it remains, is the comparison of the tropospheric temperature trend between models with prescribed SSTs and with their interactive oceans - providing the clear result that the models contain unrealistic positive feedbacks (but this is seen in the left panel too) This, as noted above, is a simple and obvious conclusion from the analysis you have done. The other conclusion is that something is evideintly wrong in the stratospheric processes since the actual temperatures up there did not decline while they did in the models. Very interesting result. [John christy, United States of America]	Taken into account. The text has been revised to point out the upward propagation of SST biases, but also that biases remain even in fixed-SST simulations, indicating other issues with the model responses. The middle and right panels have short periods, but with monotonous changes in stratospheric ozone, which makes linear trends more meaningful. Unfortunately it is not possible to extend the analysis to after 2014 because CMIP6 AMIP simulations have not been extended further than that year. The figure now includes observational uncertainties to ensure a fairer interpretation.
127375	141	1	141	7	[PRECISION] Are these all on the same baseline time period, or is each x-axis representative of the time period for Figure 3.9? [Trigg Talley, United States of America]	Accepted. The caption has been revised to clarify that the trends shown are linear trends over each period, rather than anomalies.
21581	141	1	141	8	I have major issues with this figure in that it is looking in absolute trend space and not at the constrained aspect of behaviour - the amplification of surface warming with height. See my comments on the main text around this fiigure. Figure should be modified to include panels looking at the ratio of warming at individual levels to the surface or replaced with such a figure. [Peter Thorne, Ireland]	Taken into account. The text discussing that figure has been revised to make a similar point. However plotting ratios does not work well because of the extension into the stratosphere, where trends are of opposite sign.
99887	141		141		I like the decision to use just red and blue lines for all the models, but the important black lines would be clearer if the red and blue lines were slightly thinner. The reasons for the three periods are given in the caption (good!) but could also be added to the titles of each sub-panel to improve clarity. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The figure has been revised entirely to improve its legibility.
33283	141				Page 141. Fig. 3.9. In the figure. Change “a.)” by “a)”, “b.)” by “b)”, and “c.)” by “c)”, In the legend: L5-6. Change “..Panel a), b) and 6 c) show trends over the periods 1979-2014, 1979-1997 (ozone depletion era), and 1998-2014 (ozone....” by “Panel a) and b) show trends over the periods 1979-2014 and 1979-1997 (ozone depletion era), and c) 1998-2014 (ozone...” [Guiomar Rotllant, Spain]	Accepted. Figure labels and caption changed as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17109	142	1	142	13	In relation to Figure 3.10, I suggest the in figure titles to be changed as: a) No Change, b) "Precipitation Multi Model Mean Bias" to be "MMM vs. GPCP v2.3 Bias", c) "Multi Model Mean Root Mean Square Error" to be "MMM vs. GPCP v2.3 Root Mean Square Error", d) "Precipitation Multi Model Mean Bias" to be "Low Resolution MMM vs. GPCP v2.3 Bias", d) "Precipitation Multi Model Mean Bias" to be "High Resolution MMM vs. GPCP v2.3 Bias", [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure 3.10 titles were changed to a) CMIP6, b) CMIP6 bias, c) CMIP6 root mean square error, d) CMIP5 bias, e) HighResMIP (high) bias, f) HigResMIP (low) bias. MMM was removed in all titles as it is understood and mentioned in the caption; the titles also clarify the sources of the models.
21583	142	2	142	2	Again, 3 panels the same title. How on earth is this figure going to stand alone if three figure panels have an identical title? [Peter Thorne, Ireland]	Taken into account. Figure 3.10 titles were changed to a) CMIP6, b) CMIP6 bias, c) CMIP6 root mean square error, d) CMIP5 bias, e) HighResMIP (high) bias, f) HigResMIP (low) bias. MMM was removed in all titles as it is understood and mentioned in the caption; the titles also clarify the sources of the models.
26803	142	6	142	6	Figure 3.10 : How is regridding performed? (algorithm, resolution of the destination grid, treatment of land and sea points etc.). It may be important, especially for a variable like precipitation [Eric Brun, France]	Taken into account. All data sets were regridded at the beginning of the process to a 1x1 degree grid.
104977	142	8	142	8	Why do you have negative values on color scale for a positive definate quantity in panel C ? [Peter Gleckler, United States of America]	Taken into account. Scale was modified to consider only positive values.
99889	142		142		Similarly to Figure 3.2, the titles on each sub-panel could more clearly state what is shown. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Titles of panels were modified to better reflect what is shown.
52951	142				Comments about figures. Fig. 3.10: ensemble mean percentage errors could also be shown given the space available for one more panel. Fig. 3.12: ensemble mean results ovr the whole domain (no masking with available observations) could be also shown as dashed lines to check the spatial representativity of the available observations. Figure 3.13: could show several observed datasets rather than just GPCP. Fig. 3.15: could be improved in order to better distinguish the various observed datasets. Fig. 3.16: could show both low-resolution and high-resolution results for the two HighResMIP models (and superimpose the same observations as in panel a). Fig. 3.20: clarify why model results differ between left and right panels. Fig. 3.32 & 3.33: could also show GMMIP results (+ clarify what is the period chosen for assessing trends in Fig. 3.32). Fig. 3.40: would it be possible to use this figure to suggest that human activities have already perturbed the whole climate system, including for instance the NH snow cover, global atmospheric humidity, global ocean pH, as well as sea surface salinity, vegetation phenology and (extreme) precipitation at the regional scale? [Hervé Douville, France]	Taken into account. Figure 3.10 now includes CMIP5 bias to compare with CMIP6 bias, as it was missing and now allows comparison. Figure 3.12 was redrawn and includes map of data availability.
21585	143	1	143	1	Figure title? Room to spell out all the regions in full over multiple lines below the x-axis to not speak in unnecessary code. [Peter Thorne, Ireland]	Taken into account. Now includes a better legend/title of the figure and codes were changed to full names of the regions.
17111	143	1	143	8	In refers to Figure 3.11, please explain and state about 6k, 0k, and 6k-0k in the figure comments. Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the caption we refer explicitly to the time period considered (mid-Holocene).
127377	143	1	143	8	Include the time period (the Holocene) in the Figure 3.11 caption. [Trigg Talley, United States of America]	Taken into account. Included.
26805	143	2	143	2	Figure 3.11 : Please precise the period in the legend, not only on the y-axis. We suggest to add "mid-holocene warm period" somewhere. [Eric Brun, France]	Taken into account. Included "mid-Holocene" in caption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17113	144	1	144	13	In refers to Figure 3.12, please put degree sign in the in figure title, such as 60°N-90°N, 30°S-30°N, etc. Pease enlarge the Precipitation Anomaly Axis bounds as some data are outside current bounds, for example, use 0.5 to -0.5 mm/day as vertical axis bounds. Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken partially into account. Time series were low-pass filtered and now the vertical axes include all range.
45321	144	1	144	13	Having a legend that indicates the meaning of each colored line would be better, even the caption did explain it. [Anson Cheung, United States of America]	Taken into account. Labels included.
26807	144		144		Figure 3.12 : We suggest to include labels for the color of the different curves [Eric Brun, France]	Taken into account. Labels included.
26809	144		144		Figure 3.12 : As precipitation is very noisy, it is hard to see something without low-pass filtering. [Eric Brun, France]	Taken into account. Time series have been filtered with a low-pass filter.
21589	145	1	145	1	Again, only one, this time distinct estimate is used here. Why when chapter 2 highlights substantial observational spread. Also, a self describing figure title please! [Peter Thorne, Ireland]	Taken into account. The new Figure now includes estimates of GPCP and ERA5. Title included.
17115	145	2	145	4	I suggest these changes: Wet (top) and dry (bottom) region tropical region (30°S-30°N) mean annual precipitation anomalies within 1988-2019 period. Observed data were in black lines (GPCP), single model simulations results were in light blue/red lines (CIMP6), and multi-model-mean results are in dark blue/red line (CIMP6). with respect to 1988-2018 (mm) for observations (GPCP - in black) and CMIP6 model simulations (single simulations light blue/red with multi-model-mean in dark blue/red). [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Caption has been modified.
17117	146	1	146	10	In refers to Figure 3.14, I suggest to change the vertical axis title to "subtropical edge shift (°/decade)". Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added a general title of the figure for self-descriptiveness.
99891	146		146		Figure very small and therefore hard to extract the message. Would be more usual to swap the axes on each panel? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
2599	147	1	147	11	The top frames are very difficult to compare, although it seems the comparison is good. The monsoon regions need to be defined only by their peripheries, so one can see the rainfall coloring. The arrows are so blunt to make comparison difficult. The lower frames need to highlight the mean much more and greatly lighten or remove the lines for the individual models. [Bryan Weare, United States of America]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
45323	147	1	147	11	the lines (quivers, coastlines, and equatorial line) in subplots a and b are too thick. It's very hard to read. [Anson Cheung, United States of America]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
17119	147	1	147	12	In refers to Figure 3.15, a and b, please change the wind arrows colour from black to other lighter colour. This is necessary because the precipitation contour colours are in dark theme already. Sufficient lighter arrow colour will make the graph to be more visible. Please mention the precipitation unit nearby the contour scalebar. For Figure 3.15, c and d, please differenciate more between CIMP6 MME Mean lines and CIMP6 Individuals lines, e.g. by using different darker line colour for one of them, maybe blue or violet. Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility. We think that in the current figure, black arrows are visible on the shading.
69909	147	1	147	12	Figure 3.15 (a & b): The plot represents the climatological summer-winter range of precipitation (shaded) rate and 850 hpa wind velocity (arrows) for GPCP and MME CMIP6 historical simulations. The geophysical boundaries and the wind velocity depiction are too much thicken which restrict the other information in the plot. The reduction in thickness will improve the concern of understanding for the general public. [SAHIL SHARMA, India]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69911	147	1	147	12	Figure 3.15 (c & d): It is very hard to understand to differentiate the CMIP6 MME and CMIP6 individual members because the color representation for both are almost similar. Also, the color bar used for the different observational datasets are similar which increase the complexity to apprehend the different time series. [SAHIL SHARMA, India]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
3661	147		147		· For the a and b of Figure 3.15, they have too heavy wind velocity (arrows); [Jiafu Mao, United States of America]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
3663	147		147		· For the c and d of Figure 3.15, It's hard to distinguish the MME mean from the individual members; [Jiafu Mao, United States of America]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
99893	147		147		Figure too small. Top panels utterly unreadable. Axes lines too thick. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
2605	148	1	148	10	Even blown up the inset is nearly impossible to read. The two lower frames can be easily combined. The obs should be black like above and nearly all of the other figures. ERA5 should be substituted for ERAint. However, I doubt the value of the lower frames given there are only two models and we apparently are seeing only the high res, not the corresponding low res results. [Bryan Weare, United States of America]	Accepted. After revision this problem is now resolved.
21591	148	2	148	2	Nowhere in caption or figure is what TM90 is exaplained. Either replace the titles with a self-describing title (first preference) or at the very least describe what this metric is in the figure caption. [Peter Thorne, Ireland]	Accepted. The revised version of the figure no longer uses this acronym.
99895	148		148		Red and green lines on top panel not distinguishable to those who are colour blind. Bottom panels too small to read properly. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. After revision this problem is now resolved.
21593	149	1	149	1	panel titles are scientific gobbledygook to your average reader and need to be replaced with self-describing titles or, at the very least, the caption needs to describe what these mean. But it wiould be far better to redraft the figure so it was self describing [Peter Thorne, Ireland]	Accepted. We now use a more descriptive title.
99897	149		149		Please add 'DJF Zonal Mean Winds' to figure title to make it more clear what is shown. Panels could be made larger and more readable by not repeating the axes on each panel. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now use a more descriptive title.
11319	150	1	150	1	I'm not sure if the plots are the best in assessing past sea ice trends. Are simple time series better for discussing models' ability to reproduce the observed trend, as well as the inter-model spread? The current plot rather looks like an emergent constraint, which however does not work well because of a lack of linear relationship (NH Sep in CMIP6) or an ensemble not covering observations (SH Feb in CMIP5 and 6). [Masahiro Watanabe, Japan]	Taken into account. Note that this plot is for evaluating climatology and trend. Time series comparison for D&A is displayed in another plot.
99899	150		150		Do we need different colours and symbols for each model when it is very hard to distinguish between them? Clearer labels on what is shown would be helpful. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We improved labelling for a better look.
2607	151	1	151	12	This figure must be updated through 2019. There is only a single time point for which there is apparent agreement between obsrvations of Antarctic SIE loss for the recent time. Is this continuing as suggested by the models? [Bryan Weare, United States of America]	Noted. As stated in the caption, the figure shows SIA anomalies for 1979-2017. The figure shows three-year means, ending with 2015-2017. The complete observational data for 2018-2020 were not available at the time the figure was finalised. As already described in 3.4.1.2 Antarctic SIA has remained anomalously low since 2016. The detailed evolution of Antarctic SIA up to 2020 is shown in Figure 2.20b.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2635	151	1	161	18	In the all important frame a) the individual results obscure the means; replace with a simple measure of range. I missed the grey bars, they are so light. I see no measure in the uncertainties in these 8-year trends, which I expect are large. In frames d)-f) what are the obseved values? Do they correspond to the historical runs? I see little of value in this figure except frame a). [Bryan Weare, United States of America]	Taken into account. Considered in revision.
99903	151		153		Red and green colours are indistinguishable to those who are colour blind. Do not use. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Colours changed.
54467	152	1	152	1	In Figure 3.20, the green solid line assigned to the pre-industrial control simulations (CTL), which it is mentioned in the corresponding figure caption, seems to be absent in all four panels. Please verify. [Maria del Pilar Bueno Rubial, Argentina]	Accepted. Clarified.
21595	152	2	152	2	There have been several versions of NOAA 20CRv2. It is important to note which specific version here. [Peter Thorne, Ireland]	Noted. We removed 20CR results for simplicity.
21597	153	1	153	1	Again, two identically titled panels with no explanation. Why the greay stripe in panel b? 0.001 is not a unit. The figure as submitted clearly could not be used in standalone form and requires substantive remedial work. [Peter Thorne, Ireland]	Taken into account. Panel titles were modified. The grey stripe does not exist any more in the updated figure.
110951	153	1			Figure 3.21 shows a suspicious feature which needs attention. The grey color starting from the south of India up to Antarctica [Ruksana Rimi, Bangladesh]	Taken into account. Grey stripe does not exist any more in the updated figure.
26811	153		153		Figure 3.21 :Why is there a grey line in the entire Indian Ocean? [Eric Brun, France]	Taken into account. Grey stripe does not exist any more in the updated figure.
99905	153		154		Panel titles are not clear about what bias is shown. Easy to make small changes to allow the figure to be read without having to read the caption. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Panel titles were modified.
21599	154	1	154	1	Figure lacks an overall title. Numbers on contours are too small to be easily legible. As are numbers under colour bar. Why are basins written in code? This unnecessarilly reduces figure accessability. Basins could be e.g. spelt out in fulll vertically to the right of each panel pair. Small tweaks could make this figure far more accessible to the reader. [Peter Thorne, Ireland]	Taken into account. Figure revised
127379	154	4	154	6	Surely the colors are not the differences between models and observations (on order of 20°C). The caption needs rewording. [Trigg Talley, United States of America]	Taken into account. Figure revised
71913	154	4		6	Surely the colour are not the differences between the models and the observations - that is of order 20C. The caption needs reqording. [John Church, Australia]	Taken into account. Figure revised
127381	155	1	155	7	CMIP6 models will be added? [Trigg Talley, United States of America]	Figure revised. CMIP6 models are now used
2617	155	1	155	8	This figure is mentioned only once on page 46 and is therefore out of order. Are there no CMIP6 results? If not, then this figure can be elminated. If so, this figure should show the observations, CMIP5 and CMIP6 model means, a measure of model spread, and one of observational uncertainty. [Bryan Weare, United States of America]	Taken into account. Figure updated to CMIP6. Text updated accordingly
79271	155	1	155	9	Figure 3.23: I assume this figure will be replaced by a CMIP6 version. If not, please use the corrected figure (CMIP5 mean is the same in both panels): http://www.climatechange2013.org/images/figures/WGI_AR5_Fig9-17_errata.jpg [Martin Stolpe, Switzerland]	Figure revised. CMIP6 models are now used
32679	155	1	155	55	the graphs guide is not clear enough. [sadegh zeyaeyan, Iran]	Noted. Caption updated.
33009	155	1	155	55	the graphs guide is not clear enough. [Sahar Tajbakhsh Mosalman, Iran]	Noted. Caption updated.
71915	155	1		7	I presume CMIP6 models will be added? [John Church, Australia]	Figure revised. CMIP6 models are now used
83089	155		155		Figure 3.23. I think it would be great to have a bit more consistency with Ch2 here - e.g. by including the depth layers reported in the OHC / ThSL section? In addition, it might be useful to show the vertical profile of warming from observations and models - this would introduce some new and I think useful information to the reader. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99907	155		155		Does each model really need a different colour? Does the text discuss specific models? [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Different colours in individual models changed into shading
79491	155				the graphs guide is not clear enough.(comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Taken into account. Figure revised
11321	156	1	156	1	I suggest replacing the panel (c) with the raw latitudinal profile of SST as in (d). [Masahiro Watanabe, Japan]	Rejected. Our intention was to focus on the differences between models and observations here. Absolute latitudinal variations in SST are so large, that differences between models and observations would be obscured if we were to show absolute values.
21601	156	1	156	1	The return of the identically titled panels. Needs to be addressed so the figure can be used standalone. An overall figure title would also help. [Peter Thorne, Ireland]	Taken into account. Figure updated
113671	156	1	156	1	I would specify the (d) label: "Equatorial mean SST" instead of "Equatorial SST" [Agnieszka Kowalczyk, Poland]	Accepted. Label corrected.
2613	156	1	156	9	This important figure must be completely redone. It is nearly impossible to see the observed values or the model means in any of the frames. Frames a and b should only include the observations and model means with some simple measure of model spread and uncertainties in the observations. Frame c should be a simple measure of the model spread; the means are in a. Frame d should have the longitudes shifted so that Indian, Pacific and Atlantic are contiguous and identified. Again, it is nearly impossible to see the model means. "Equatorial" should be defined. The high resolution results are of limited value without the corresponding low resolution ones. [Bryan Weare, United States of America]	Taken into account. Figure revised
32681	156	1	156	55	the graphs guide is not clear enough. [sadegh zeyaeyan, Iran]	Noted. Figure updated
33011	156	1	156	55	the graphs guide is not clear enough. [Sahar Tajbakhsh Mosalman, Iran]	Noted. Figure updated
113673	156	4	156	4	"HighResMIP" instead of "HighresMIP" [Agnieszka Kowalczyk, Poland]	Taken into account. Text revised.
99909	156		156		Does each model really need a different colour? Not very accessible. Bottom panels extremely difficult to see differences between shadings and lines. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Different colours in individual models changed into shading
79493	156				the graphs guide is not clear enough.(comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Noted. Caption updated
2625	157	1	157	15	This needs to be redone. This figure has no CMIP6 results and a stange y-axis. The inset seems the most useful. I do not understand the gray triangles after 2000. Are there really several observed volcanoes of VEI compaabalbe to Krakatoa in the time period? [Bryan Weare, United States of America]	Noted. Figure 3.25 and Figure 3.23 are now combined to be the new Figure 3.26, using CMIP6 models
12525	157	1	157	16	The firgure should be updated for CMIP6, observational datasets should also be updated and expanded, to be consistent with chapter-2. [Lijing Cheng, China]	Noted. Figure 3.25 and Figure 3.23 are now combined to be the new Figure 3.26, using CMIP6 models
38311	157	1	157	16	Figure 3.25 uses data from CMIP5. It is suggested to use CMIP6 data instead. As at present many observation data have been extended to 2018, it is suggested to update. [Yaming LIU, China]	Noted. Figure 3.25 and Figure 3.23 are now combined to be the new Figure 3.26, using CMIP6 models
130485	157	1	157	16	The firgure should be updated for CMIP6, and observational datasets also be updated and expanded, to be consistent with Chapter 2. [Panmao Zhai, China]	Noted. Figure 3.25 and Figure 3.23 are now combined to be the new Figure 3.26, using CMIP6 models
113675	157	4	157	4	I would explicitly mention the colors of "The three shaded wedges" as it is not obvious at first which wedges we should look at. [Agnieszka Kowalczyk, Poland]	Noted. Figure 3.25 and Figure 3.23 are now combined to be the new Figure 3.26.
113677	157	10	157	10	There should be a ")" after "(volcanic explosivity index [VEI]". [Agnieszka Kowalczyk, Poland]	Editorial. Revised as indicated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113679	157	10	157	10	"century volcanic eruptions with magnitude (volcanic explosivity index [VEI] represented by symbol size"; I can't see any difference in size in the Figure to be honest. Could you please make it more visible? [Agnieszka Kowalczyk, Poland]	Noted. Figure 3.25 and Figure 3.23 are now combined to be the new Figure 3.26.
10875	157	13			It is not possible to get "observed volcanic forcing". Radiative forcing is not something you can observe! The Ridley et al (2014) reference uses indirect observations of stratospheric aerosol optical depth in a simple climate model to estimate radiative forcing. To use an estimate from a simple climate model to adjust a forcing estimate from a more sophisticated model seems rather bold! At very least get rid of the "observed" association with volcanic forcing. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure 3.25 and Figure 3.23 are now combined to be the new Figure 3.26.
50723	157	figure 3.25			This might be easier to read if it was two separate plots rather than one inside the other. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure 3.25 and Figure 3.23 are now combined and updated to be the new Figure 3.26
83091	157		157		I wonder if Figure 3.25 and 3.23 could be combined? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure 3.25 and Figure 3.23 are now combined and updated to be the new Figure 3.26
99911	157		157		Inset panel is very squashed. Could just be a separate panel for clarity. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Figure 3.25 and Figure 3.23 are now combined and updated to be the new Figure 3.26
116265	157		157		I find this figure quite hard to understand (one needs to read carefully several times the caption to try to understand what is represented and what is means). Could it be made more self explanatory? [Valerie Masson-Delmotte, France]	Noted. Figure 3.25 and Figure 3.23 are now combined and updated to be the new Figure 3.26
2623	158	1	158	7	the color bar refers only to the left maps and so should be moved [Bryan Weare, United States of America]	Accepted, this figure has been substantially updated and this comment has been considered during these updates.
21603	159	1	159	1	Another figure requiring substantial work to make sure it can be used standalone. Panel titles. Overall figure title. Colour bar font size. Etc. etc. With a little effort this could be vastly improved. Also, standard projection suggested throughout AR5 is Robinson not Mercartor [Peter Thorne, Ireland]	Accepted, this figure has been substantially updated and this comment has been considered during these updates.
2627	159	1	159	15	the a3,b3, c3 labels are uncnessarily confusing; just state the observation names and model mean [Bryan Weare, United States of America]	Accepted, this figure has been substantially updated and this comment has been considered during these updates.
12527	159	1	159	16	Figure should be updated (Model: CMIP6; observation: most recent data extending to 2018). Change halosteric to salinity, if you decided to keep this plot. [Lijing Cheng, China]	Accepted, this figure has been substantially updated and this comment has been considered during these updates.
12529	159	1	159	16	The Pacific ocean is not all salting and Atlantic Ocean is not all getting saltier, so this way figure has important caveats. A very important signature is North Atlantic (>40N) freshening partly due to ice melting. So contrasting Atlantic vs Pacific has important limitations. Also the Indian Ocean is missing in this picture, which also shows a "salty gets saltier and fresh gets fresher" pattern. Please consider using salinity-contrast metric between lower and higher salinity regions. [Lijing Cheng, China]	Rejected, while we agree with this observation, this chapter has the mandate to work on global to basin scale changes. Anything at smaller scales than this is beyond the scope of our chapter.
38313	159	1	159	16	Figure3.27 uses data from CMIP5. It is suggested to use CMIP6 data. As at present many observation data have been extended to 2018, it is suggested to update. [Yaming LIU, China]	Accepted, this figure has been substantially updated and this comment has been considered during these updates.
2629	160	1	160	7	the individual models results obscure the means; substitute a simple measure of the spreads [Bryan Weare, United States of America]	Accepted, this figure has been substantially updated and this comment has been considered during these updates.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79085	160	3	160	7	Figure 3.28: would it be worth adding in the CMIP5 thermosteric too, for comparison (e.g. from Slangen et al, 2014/2016)? Or are these indeed the CMIP5 models (if CMIP6 is a typo), in which case there should be a larger set available than currently shown in the figure. Just to make sure: are the zostoga's dedrifted using the PIcontrol runs? [Aimee Slangen, Netherlands]	Rejected. At present we have concerns with the amount of data plotted already and we believe adding a further data set may further obscure this figures main messages. Also, note that his is CMIP6 data.
98019	161	1	161	1	(Fig. 3.29). Are the distributions of 8-yr AMOC trends in panel b from CMIP6 model control runs? The caption says historical runs, but that doesn't make much sense, as all of the model distributions seem to have a mean at or near zero, as one might expect for control runs but not historical runs (with All Forcings). [Thomas Knutson, United States of America]	Noted, this source data has been checked and historical simulations have been utilised as described in the caption.
113681	161	1	161	1	the numbers above the boxes in (d-f) are not explained in the Figure caption, I presume these are mean values? [Agnieszka Kowalczyk, Poland]	Accepted, these numbers are now explained in the caption.
113683	161	1	162	1	CESM2 model is not mentioned in the Figure caption. [Agnieszka Kowalczyk, Poland]	Rejected, individual models are not named in the caption, they are listed in the figure itself. We now also note that the number of CMIP6 models have been updated to include over 20 models.
26813	161	9	161	10	Figure 3.29 : The comparison between interannual variability and the observed difference between two consecutive years does not seem appropriate. The distribution of differences between consecutive years in the models (which will be much larger) should be shown in order to have an apples to apples comparison. [Eric Brun, France]	Rejected. The interannual variability of models and the observations is calculated with the same method so this comparison is appropriate. The caption has been revised to further clarify this point.
67723	162	1	162	1	It is hard to distinguish the color of lines. [Hiroaki Kondo, Japan]	Accepted. The figure has been revised entirely to improve its legibility.
10957	162	1	162	4	For plot a. The y-axis label for units in ppm, but in the figure description it is labels as ppmv. It is also decribed as ppmv in the text section 3.6.1 (page 56, line 40). [Joseph Thomas, United States of America]	Accepted. Inconsistencies have been corrected.
2641	162	1	162	8	Like nearly all other figures the observational curves should be black. The colored lines don't seem to correspond the models listed. I count seven models but at most four lines. The colors of the first three models and the last four are too similar. What are the vertical columns in b) and c)? FLN and FL are not defined. [Bryan Weare, United States of America]	Accepted. The figure has been revised entirely to improve its consistency with others, and legibility.
96325	162	2	162	2	Please add a source for FLN, FL (LeQuere et al). [Nicole Wilke, Germany]	Noted. The figure has been revised and the new GCB source is given.
96327	162	2	162	2	Please explain what the subscript "LN" stand for - the F_L flux suggests L is for land-use, but this is explicitly not part of it… [Nicole Wilke, Germany]	Noted. Figure has been revised and there is no need for that notation.
96329	162	2	162	2	Please update the figure with more models. [Nicole Wilke, Germany]	Accepted. The figure has been updated.
96331	162	2	162	2	The annual global carbon budget provides estimates since 1850 - please consider including previous decades as well. [Nicole Wilke, Germany]	Taken into account. We show GCP from 1960, where the estimates are most informed by observations, but Chapter 5 does the comparison from 1850.
96333	162	2	162	2	Why is the reference the 2016 annual global carbon budget? The 2019 budget was published in November 2019 and presumably is an improvement of quantifications as well as method, with the important update that the FNL flux is no longer calculated as residual, accumulating all errors, but as independent bottom-up model estimate. Please use the more recent information. [Nicole Wilke, Germany]	Accepted. The figure now uses updated data.
64491	162	4	162	4	red -> orange?; (panel b) bars? [Julia Nabel, Germany]	Accepted. The figure has been revised entirely to improve its legibility.
64493	162	6	162	8	reference for FLN and FL (Le Quere, X)? [Julia Nabel, Germany]	Noted. The figure has been revised and the new GCB source is given.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64495	162	7	162	7	depending on which GCB: FLN is not the residual but the estimated land sink [Julia Nabel, Germany]	Accepted. A new GCB is used and the caption has been revised accordingly.
64497	162	14	162	14	please check if the grey shadings of the one sigma model spread might be swapped (i.e. currently light grey for 2005-2014 and darker for 1961-1970), since the amplitude should have increased for 2005-2014? [Julia Nabel, Germany]	Noted. This was checked in the revision.
3665	162		162		· For the Fig. 3.30, the lines cannot be easily distinguished from each other; [Jiafu Mao, United States of America]	Accepted. The figure has been revised entirely to improve its legibility.
2643	163	1	163	18	Again the observations should be black, like most other plots. [Bryan Weare, United States of America]	Accepted. The figure has been revised entirely to improve its consistency with others.
67725	163	11	163	13	It is difficult to distinguish light-grey from grey. [Hiroaki Kondo, Japan]	Accepted. The figure has been revised.
2645	164	1	164	19	The verticl lines in frames j) and k) are not consistent with the statement on page 61, lines 25-27 that the observed trend in NAM and NAO are "not statistically significant." The distance between the two black lines implies just the opposite. [Bryan Weare, United States of America]	Accepted. Thanks for having pointed out this inconsistency. The sentence has been changed accordingly.
65679	164	11	164	11	Is this a reference to Lee et al 2018? Suggest discussing how these statistics change in other seasons (see Ch4 p44 for a discussion of projected changes in SAM in all seasons), or more generally, we suggest adding a comment on SAM representation in seasons outside of summer. [Kushla Munro, Australia]	Not applicable for the 1st part of the comment. The Lee et al. (2018) reference has been removed because in the revised figure for FGD, we show only the zonal index. Taken into account for the 2nd part of the comment: Regarding the choice of the seasons for the plot, we decided to limit to two seasons instead of 4 for sake of simplicity and also based on the robustness of the assessment for the different reasons. Most of the literature deals with summer and winter seasons and this is why we chose these two. Some words are included for autumn in the text. Chap4 should have removed those two seasons from their plot for consistency.
2647	165	1	165	15	Again on page 61 line 25 it is stated the the observed NAM trend is" not significant." This strongly suggests that the horizontal gray bars do not properly identify a reasonable significance region. The box and whiskers should be ordered such the CMIP6 "historical" results are most prominent and in red as most previous fitugres [Bryan Weare, United States of America]	Accepted. Thanks for having pointed out this inconsistency. The sentence has been changed accordingly.
33285	165				Fig. 3.33. I will add the units in the y axis, hPa decade–1. [Guiomar Rotllant, Spain]	Rejected. The SAM index is based on a difference between normalized zonal anomalies of SLP and therefore has no unit.
21605	166	1	166	1	Another figure that without remedial work on title and also axis labels cannot be used standalone. [Peter Thorne, Ireland]	Accepted. Title and axis labels have been added.
15245	167	0	167	0	shouldn't the figures have titles. It would help the reader. [Sergio Aquino, Canada]	Accepted. The title has been added.
2651	167	1	167	14	In frames c) and d) the width between the two horizontal lines cannot represent the variability of the durations for ENSOs in this 60 year period. Simply displaying the means for two highly related data sets does not do this, [Bryan Weare, United States of America]	Taken into account. Instead of showing the ensemble averages of individual models, we have added the statistics based on individual ensemble members, so that it can be compared with the observational results.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
113685	167	7	167	7	"(a, b) Composites of Niño" should read "(a, b) Composites of El Niño and La Niña" [Agnieszka Kowalczyk, Poland]	Taken into account. We have rephrased the "Nino 3.4 SST" to "ENSO index" in the figure and caption, with the definition of the ENSO index (i.e. SST anomaly averaged over the Nino 3.4 region) at the end of the caption.
113687	167	9	167	9	"for which Niño" should read "for which El Niño and La Niña" [Agnieszka Kowalczyk, Poland]	Taken into account. We have rephrased the "Nino 3.4 SST" to "ENSO index" in the figure and caption, with the definition of the ENSO index (i.e. SST anomaly averaged over the Nino 3.4 region) at the end of the caption.
2653	168	1	168	9	Again no appropriate measure of the uncertainties in these highly variable factors indicated.There is no indication of what time period this represents. The vertical lines in b) are distracting and unnecessary. [Bryan Weare, United States of America]	Taken into account. Instead of showing the ensemble averages of individual models, we have added the statistics based on individual ensemble members, so that it can be compared with the observational results.
26071	168		168		The boxers and whiskers mentioned in the caption are missing in Figure 3.36 b). [Don Alfonso Pino Maeso, Spain]	Taken into account. Boxes and whiskers have been shown but were not clearly visible due to the low quality of the SOD figure (occurred due to a processing error). We have revised the figure for clear visibility.
70309	168		168		Figure 3.36, the grey grid in the lower panel has been pulled to the front and overlays the dots you want to see. [Shayne McGregor, Australia]	Taken into account. The low quality of the SOD figure was due to a processing error. We have revised the figure for clear visibility.
21607	169	1	169	1	As drafted this figure cannot make sense as a stand-alone item. Considerable use of titles along with actually e.g. putting units on the two colour bars is required to give this figure a chance of being understood by the user in its own right. [Peter Thorne, Ireland]	Taken into account. In the original figure, units and label titles were missing which made the plot not easily readable. The layout has been improved in the revised version.
17121	169	1	169	13	In refers to Figure 3.37, please mention which scale bar is for temperature and which one is for precipitation. Please state the unit too. Thanks [Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the original figure, units and label titles were missing which made the plot not easily readable. The layout has been improved in the revised version.
32663	169	1	169	13	Add information about the middle east in figure 3.37. [sadegh zeyaeyan, Iran]	Rejected. The regions shown in this plot are the ones where there are recent literature to assess the ENSO teleconnection and where the latter is the most significant. To provide a complete and detailed view, a summary table for teleconnection associated with all the MoVs addressed in AR6 has been added in the Chapter Atlas and in Technical Summary.
32993	169	1	169	13	Add information about the middle east in figure 3.37. [Sahar Tajbakhsh Mosalman, Iran]	Rejected. The regions shown in this plot are the ones where there are recent literature to assess the ENSO teleconnection and where the latter is the most significant. To provide a complete and detailed view, a summary table for teleconnection associated with all the MoVs addressed in AR6 has been added in the Chapter Atlas and in Technical Summary.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26815	169	3	169	3	Figure 3.37 : The results of a significance test for observations should be shown. How are potential anthropogenic signals taken into account? [Eric Brun, France]	Taken into account but in the Technical Annex. The maps for teleconnections are duplicated from the Technical Annex but with boundaries of AR6 regions displayed in this Chapter 3 version. Because this is already a busy plot, we have decided to add significance on the Technical Annex maps for all the modes. In addition, a summary table for teleconnection associated with all the MoVs addressed in AR6 has been added in the revised version of the Technical Summary including significance information as well as in the Chapter Atlas.
113689	169	5	169	6	"for precipitation from GPCC over land and GPCP over ocean (contour, period: 1979-2014)" please indicate that it is for the bottom panel [Agnieszka Kowalczyk, Poland]	Accepted. Revised accordingly.
26073	169		169		Figure 3.37. Explanation of colours is a bit confusing [Don Alfonso Pino Maeso, Spain]	Taken into account. In the original figure, units and label titles were missing which made the plot not easily readable. The layout has been improved in the revised version.
70287	169		169		This is a fantastic figure. However, the thick black lines outlinning and connecting map regions to the dirstibutions appears to be very fat (they dominate the image), while the color is hard to differeintiate from the darkest colours represented in the colorbar. I suggest either changing the colour of the lines and boxes, or removing the darkest colours from the colourbar. [Shayne McGregor, Australia]	Accepted. The layout of the figure has been improved accordingly.
116267	169		169		There is a need for improved x chapter coordination on ENSO teleconnections. [Valerie Masson-Delmotte, France]	Noted. Thanks. Teleconnections are now assessed in Chap4 in FGD and also better characterized from a summary table for all the MOVs teleconnection given in Technical Summary and Atlas
113691	170	14	170	15	"The thick red and light blue lines are the MME mean for the historical simulations in CMIP5 and CMIP6, respectively" -- it is the other way round blue for CMIP5 and red for CMIP6 [Agnieszka Kowalczyk, Poland]	Accepted. Thanks for having pointing out this inversion in the legend.
2657	171	1	171	18	Again the horizontal bars in d) and e) greatly understate the uncertainties in these observations. Similarly, many features in a) are not likely to be significant at 95%. This is highligted in c) where onw of the two alternate data sets has about half the variability of the standard. What would happen if the data in f) were the aveargе for each time of the estimates from the three data sets? [Bryan Weare, United States of America]	Taken into account. The layout has been improved considerably. In d) and e), individual observations are now shown which better address the observational uncertainties. Significance has been added on maps and in f), three observational datasets are displayed and the correlation between the average observational timeseries and the MME is indicated.
113693	171	15	171	16	"The thick red and light blue line are the MME mean for the historical simulations in CMIP5 and CMIP6, respectively" -- it is the other way round blue for CMIP5 and red for CMIP6 [Agnieszka Kowalczyk, Poland]	Accepted. Thanks for having pointing out this inversion.
15243	172	0	172	0	Figure 3.4 missing information [Sergio Aquino, Canada]	Taken into account. Figure revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21609	172	1	172	1	The map here is in my view a distraction. This figure would be better if arranged thematically either by region or variable and the figure panels made much bigger so that the panel contents were much more clearly discernible. The panel titles say where the series are and I think a degree of geographical competence can be assumed. I would run all the red as one block background shaded red, then all the green, the purple etc. Use the colour to denote the variable. And use the space to show the fundamental assessment finding and not your cartographical skills at being able to find a world map (which anyway isn't shown in the Robinson projection standard for AR6) [Peter Thorne, Ireland]	Taken into account. Layout is modified for the FGD.
2661	172	1	172	9	Even the completed portions of this very incomplete figure are impossible to read even blown up many times. The black lines are nearly invisible as are both axes. I do not believe that the yearly observations can be filtered only with a "2-year running mean." They are just too smooth. [Bryan Weare, United States of America]	Taken into account. Layout is modified for the FGD.
127383	172	1			Figure 3.40 is clearly incomplete, but is it accurate to say that global ocean will show ocean heat content rather than sea surface temperature? While its important to include, it's also a bit odd to show surface temperatures for the land and global but OHC for the ocean in the same figure. Also, why is precipitation only shown for 60N to 90N? The authors should brainstorm a bit about how to make this figure clearer, as model/observation comparisions on a regional basis across all these metrics are important, but the current formulation is rather unclear. [Trigg Talley, United States of America]	Taken into account. Use of OHC over ocean reflects underlying attribution literature. Precipitation is shown for the northern mid to high latitudes because this is where a detectable anthropogenic response is seen.
54471	172	2			Figure 3.40. Summary…. "Additional variables shown will be added in the final draft. (Figure produced with ESMValTool v2.0.0b2.)". Please clarify the meaning of empty rectangles. ¿Will be the figure complete? [Maria del Pilar Bueno Rubial, Argentina]	Taken into account. They were filled for the FGD.
26075	172		172		Figure 3.40. Some boxes are empty. [Don Alfonso Pino Maeso, Spain]	Taken into account. They were filled for the FGD.
99913	172		172		Figure as designed is going to be unreadable at printed size. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Layout is modified for the FGD.
116269	172		172		While this is an iconic IPCC figure, could the authors of the chapter also consider other options (eg table) to report the key findings of the assessment and help integrate outcomes for large scale climate variables across ch 2, 3, 4 (observed, projected, emergence), and with the regional chapters for regional trends? [Valerie Masson-Delmotte, France]	Taken into account. The main findings of the chapter are now integrated in a table in TS.2.
33287	172				Fig. 3.4. Why there are empty boxes? [Guiomar Rotllant, Spain]	Taken into account. They were filled for the FGD.
2663	173	1	173	14	This complex summary figure must be simplited as much as possble. The y-axis legends should spell out the variable name, excluding the word global, since all but one variable is global. The color coding needs to be explicitly explained at the beginning that it is the relative rmsd accross al three modeling cycles. The frames should be put into landscape mode so on has some idea what the x-axis names are. The color bar need only be included on frame b) [Bryan Weare, United States of America]	Accepted. We have revised the figure, making it more readable.
127385	173	1			In Figure 3.41, the x-axis model names are exceedingly hard to read. [Trigg Talley, United States of America]	Accepted. We do our best in the revised version to make these names more intelligible.
113695	173	2	173	2	the very first upper left label in panel (b) is covered by white rectangle and hence unreadable [Agnieszka Kowalczyk, Poland]	Accepted. We have revised the figure; this is fixed now.
44045	173	2	173	15	There are many white color boxes in Fig. 3.41. What do these mean? Missing value? [Lijuan Li, China]	Accepted. Indeed these are missing values. This is now made explicit in the caption. The new version contains fewer white spaces.
113697	173	6	173	6	there should be a coma between "PSL" and "ZG500" [Agnieszka Kowalczyk, Poland]	Accepted. This has been fixed.
113699	173	7	173	7	the first "RSDS" should read "RLDS" instead [Agnieszka Kowalczyk, Poland]	Rejected. We think the label is correct.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50725	173	figure 3.41			The model names on these two plots are very hard to read. It's very tricky to fit so many on a plot clearly but maybe they would be easier to read if they were on the diagonal like fig 3.36. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised the figure, making it more readable.
96335	174	1			Figure 3.42: Please introduce a colour code for the individual CMIP6-models in order to allow an own assessment by the reader. (Similar to Figure 3.24) [Nicole Wilke, Germany]	Rejected. There are so many models that colour-coding them would render the figure unreadable.
127387	174	1			In Figure 3.42, why is a 20-year period (1980-1999) chosen rather than a more conventional 30-year climatology? Presumably all of these datasets are available until closer to present. Or is this simply constrained by the period over which CMIP3 historical runs are available (and the beginning of satellite TOA data circa 1980)? [Trigg Talley, United States of America]	Accepted. Indeed the CMIP3 simulations end in 1999. This is now made explicit in the text.
99915	174		174		Given this figure is being raised up, please ensure it is readable. E.g. impossible to tell from figure what the correlation is between. A simple title addition would improve clarity. FAQ version is better but still needs clearer title. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now explain better what the figure shows.
2671	175	1	175	16	Even enlarged greatly this is nearly impossible to interpret. Where are the multi-model means. I have a hard time believing that the uncertainty bars are indicative of the full set of uncertainties in these proxy data. [Bryan Weare, United States of America]	Accepted. We have revised the figure.
32683	175	1	175	55	the figures are not clear enough. [sadegh zeyaeyan, Iran]	Accepted. We have revised the figure.
33013	175	1	175	55	the figures are not clear enough. [Sahar Tajbakhsh Mosalman, Iran]	Accepted. We have revised the figure.
96337	175	1			Figure 3.43: Please improve the recognizability substantially. [Nicole Wilke, Germany]	Accepted. We have revised the figure.
99917	175		175		More thought into figure design required. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised the figure.
116271	175		175		There seems to be some duplication with one of the first chapter figures also showing results of CMIP6 simulations, check the flow of information and consistency. [Valerie Masson-Delmotte, France]	Noted. It is unclear which figure the reviewer is referring to. This figure is not duplicating any other figures in the chapter.
79495	175				the figures are not clear enough.(comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Accepted. We have revised the figure.
26817	176	3	176	3	Cross-Chapter Box 3.1, Figure 1: Multiple members are used for some models: how is the distribution computed in order to avoid to give more weight to the models with more members? [Eric Brun, France]	Taken into account. We have weighted individual ensemble members by the inverse of the ensemble size of the same model, so that individual models are equally weighted irrespective of the ensemble size. This has been clarified in the caption.
113701	176	13	176	14	"and composited trends of subsampled CMIP6 simulations" should by followed by "(d)" [Agnieszka Kowalczyk, Poland]	Editorial. Revised as indicated.
2963	177	1	177	4	Please add more CMIP6 models in upper-left figure. [Zong Ci Zhao, China]	Accepted. More CMIP6 models added.
37375	177	1	177	16	You have nothing more than belief to sustain these graphs because you haven't audited the temperature data as I have. I reported more than 70 problem areas in the HadCRUT4 data, many of which also apply to other near-surface temperature datasets because they share the same data. See McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset". [John McLean, Australia]	Noted. Data quality issue are dealt with by Ch2.
127389	177	1	177	16	Include definition of metrics? [Trigg Talley, United States of America]	Taken into account. Explained in the text.
2677	177	2			"TXx" and "Rx1day" need to be spelled out. Especially in Cross-chapter and FAQ boxes the figures need to stand alone just as much as possible. [Bryan Weare, United States of America]	Taken into account. Explained in the text.
113703	177	7	177	8	"SSP2-4.5" instead of "SSP2-45" [Agnieszka Kowalczyk, Poland]	Accepted. Corrected.
11323	178	1	178	1	Please explain dashed lines in the right panel. [Masahiro Watanabe, Japan]	Accepted, this comment has been considered when significantly revising this figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37377	178	1	178	1	This Figure should be filed under "Fiction" because (a) you haven't audited the temperature data and corrected the errors and (b) it is based on output from unvalidated climate models, which incidentally have been tuned to match near surface temperature data that is flawed. [John McLean, Australia]	Rejected, all observational data and numerical models used through this chapter have been validated.
8937	178	1	178	7	FAQ 3.1, Figure 1: Maybe explain how the zero degree C line was set in this figure. Isn't the comparison normally to 'long-term average' or 'pre-industrial levels'. Measured by this baseline we are at 1.1C warming now; but the Figure shows a different level. [Thomas Wiedmann, Australia]	Accepted, this comment has been considered when significantly revising this figure.
96339	178	1	178	8	FAQ 3.1, Figure 1: the way natural cooling/warming is presented in the figure is misleading. The added regression implies that peaks and dips of the curve would mean something and that it is reasonable to connect a peak and a dip to assess long-term trends. This is not the case, because most of the signal is superimposed by natural variability. We request the authors to delete these regressions as they do not help to understand the nature of natural variability. In fact such an added regression was one of the reasons of the long discussion about the "climate change hiatus" since the end of the 1990s and early the 2000s. Please find a way to make your point without these dangerous regression lines. [Nicole Wilke, Germany]	Accepted, this comment has been considered when significantly revising this figure.
7311	179		179		Include at the end of the subtitle (text in red): Yes, climate models have improved thanks to technological progress and a better understanding of climate processes. In this process, the performance evaluation of the model is highlighted, as shown in the Figure. This performance will vary according to the complexity of the model (in particular, under the number of variables; as well as, types of equations and attenuation functions used), which will directly impact on its high resolution and efficiency (IPCC, 2018). [Julio Cesar Barreto da Silva, Brazil]	Noted. We thank the reviewer for this alternative formulation. The text has been comprehensively revised, also in response to other review comments.
21611	180	1	180	1	It is a pity that such a clear figure can only be found in the FAQ and is not present in the chapter as well / instead. The figures in the chapter are much less clear and concise than this figure is. Both its simplicity but also the use of titles and understandable labels makes it a showcase of what so many of the figures within the chapter should be but fail to be. [Peter Thorne, Ireland]	Noted.
83599	180	1			"How do we know humans are causing climate change?" Note the green line "natural causes" is rather flat. However the PAGES2k paleotemperature set over past 2ka is not the only data to consider. A large number of data sets world wide show very clear cyclic variations (especially the 1000yr and 200yr cycles). If we take the G7 global proxy data set of Ludecke and Weiss 2017 and account for the observed natural cycles (without necessarily knowing what physical causative mechanism applies) then we can most certainly replicate the warming of 1850-2000 without including a dominant anthropogenic signal. See eg Lüdecke H-J and , C.O.Weiss, 2017, Harmonic Analysis of Worldwide Temperature Proxies for 2000 Years. The Open Atmospheric Science Journal, 11, 44 -53. See my further discussion on Section 7.5.4. The comment in page 3-22 lines 50-55 is also very pertinent – I suspect internal climate variability probably plays a larger role in global temp variations than the current AR6 portrays. [michael asten, Australia]	Noted. The green line shows simulations over the historical period with natural forcings, not paleo data.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7315	180	2	180	2	FAQ 3.3, Figure 1: Average global observational changes in surface air temperature (HadCRUT4), compared to simulated data, by climate models, in response to: 1) all natural and human forcing (gray band), 2) only greenhouse gases greenhouse effect (red band), 3) only aerosols (blue band) and 4) only natural forcing agents (green band). Dashed colored lines show the simulated average temperature (°C) for the tested models (1 to 4); and colored bands show the confidence interval of 5% to 95% on the supposed temperature variability for each simulated model (1 to 4). [Julio Cesar Barreto da Silva, Brazil]	Taken into account. Taken into account when revising caption.
7313	180		180		Include at the end of the subtitle (text in red): Observed warming (1850-2018) is only reproduced in simulations including human influence, especially, the greenhouse gases. [Julio Cesar Barreto da Silva, Brazil]	Accepted.
64697	189	1	189	3	I would propose to add a figure as Figur 3.42 to show the evoluation of the ability of climate models to reproduce a climate changes. This can be done using the MidHolocene and Last Glacial maximum simulations run for PMIP2, PMIP3 and PMIP4 in paralle (or within) CMIP3, CMIP5 and CMIP6. The model used are a subset (about 20) models used for the reference CMIP simulation used in this figure. It is important for and IPCC assessment to not only show mode evolution in reproducing modern conditions, but also in reproducing a changes that was not consider during the model developing phase and that allow to test feedacks that do not necessarily dominant in the current climate variability. The MidHolocene and Last Glacial Maximum are key well lnown periods, for which good paleoclimate reconstructoins and sythesis are availabe (with uncertainty estimates) that can be used for model benchmarking [Pascale Braconnot, France]	Noted. Unfortunately at this late stage we cannot come up with completely new, un-reviewed figures. Figure 3.43 covers these periods well.
19297	3(38)	31	38	53	Packed with snow and glaciers, mountains of the Hindu Kush Himalayan region seve serve the world as a global water tower.The recent flagship study https://link.springer.com/book/10.1007/978-3-319-92288-1 sheds excellent light on the 'human influience' on the overall cryosphere especially the snow cover, and ice in this part of the world.It is highly important to mention of human influiences on cryosphere, snow cover and glaciers references from this highlyimportant region of the would, highly vulnerable to climate change. [Ghulam-Muhammad Shah, Nepal]	Noted. We agree with the reviewer that this is a very important region with critical trends occurring in the cryosphere. Discussing this region in detail is however beyond the scope of this chapter. We have forwarded the comment to Ch9 for consideration.
19299	3(40)	35	40	45	Above comment equally applies [Ghulam-Muhammad Shah, Nepal]	Not applicable. It is unclear what change the reviewer wants to see.
26077					General Comment on the whole Chapter: please revise references, there are many inconsistencies between the text and the list of references. [Don Alfonso Pino Maeso, Spain]	Accepted. This has been done in the final draft.
29313					very good work. The COVID-19 pandemic proves beyond any doubt human influence on the climate [Zangari del Balzo Gianluigi, Italy]	Noted. COVID-19 impacts on climate are assessed in a new cross-chapter box in Chapter 6, which is now cited in Chapter 3.
32969					Anything is absolutely outstanding!!! There is nothing what could be improved except that one should perhaps also point out which activities of people lead to which climatic effects, so far that can be judged at all. [David Novak, Germany]	Noted. Where literature is available we assess relative contributions of GHG and aerosol emissions to climate change.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35921					There is a large body of literature of empirical (econometric) attribution studies that is missing from the report. These models use econometric methods to model observations while consistent with physical principles and complement the existing evidence. Specifically, these empirical papers provide an alternative line of evidence beyond simulated general circulation and earth system models. Suggested papers include: Chang, Y., Kaufmann, R. K., Kim, C. S., Miller, J. I., Park, J. Y., & Park, S. (2020). Evaluating trends in time series of distributions: A spatial fingerprint of human effects on climate. Journal of Econometrics, 214(1), 274-294.; Pretis, F. (2020). Econometric modelling of climate systems: The equivalence of energy balance models and cointegrated vector autoregressions. Journal of Econometrics, 214(1), 256-273; Dergiades, T., R.K. Kaufmann, T. Panagiotidis, 2016, Long-run changes in radiative forcing and surface temperature: the effect of human activity over the last five centuries Journal of Environmental Economics and Management, 76:67-85.; Stern, D.I. and R.K. Kaufmann, 2014, Anthropogenic and natural causes of climate change, Climatic Change, 122:257-269; Estrada, F., Perron, P., & Martínez-López, B. (2013). Statistically derived contributions of diverse human influences to twentieth-century temperature changes. Nature Geoscience, 6(12), 1050-1055. [Felix Pretis, Canada]	Taken into account. Relevant references added to methods Section 3.2 and Section 3.3.1.
35925					Empirical papers assessing the existence of the 'warming hiatus' are missing from the report, such as: Pretis, F., Mann, M. L., & Kaufmann, R. K. (2015). Testing competing models of the temperature hiatus: assessing the effects of conditioning variables and temporal uncertainties through sample-wide break detection. Climatic Change, 131(4), 705-718. This paper finds that the hiatus as such was not unique and that the conclusions in Kosaka and Xie (2013) were not supported. [Felix Pretis, Canada]	Rejected. We have enough citations showing the non-uniqueness of warming slowdowns which further argue the role of internal variability. Existence or lack of breakpoints are not related to the PDV influence on GSAT trends.
79477					It seems that it would be better to bring the figures in the text as before for better understanding, or only indicate the figure`s number in the text and not to write the subtitle so just refer to the end of the text.When the figures in the text is not sshown, there is no need to write subtitles in the text (comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Noted. The published version of the chapter will contain the figures included with the text.
79479					In adddition of the text, the refrences should be indicate in their figures`s subtitles.(comment by: mirzapourb@yahoo.com) [Hanieh Zargarlellahi, Iran]	Rejected. We follow standard IPCC style.
93511					The chapter is excellent! No revisions noted. [Rahab KINYANJUI, Kenya]	Noted. Thank you and appreciated.
99901					Please ensure figures are as understandable as possible without having to read the caption to work out what is shown. Design the figures for their printed size. Ensure all the information presented is necessary. Some figures are excellent, but others are very poorly designed. Clear labels and plot titles are essential throughout the Chapter figures. I have given example comments on many figures but many apply to several others. [Ed Hawkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figures have been revised for clarity.
114733					figure 3.17 is obviously a key figure tht will be importnat in the communication of the report. [Jan Fuglestvedt, Norway]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115881					FAQ3.1 this FAQ question and text is quite unclear. What does "recent" mean here? (since 1850 ? Since 1950?). The statement "orbital forcing creates radiative forcing" is misleading (the net effect is small, what matters is the seasonal/ latitudinal distribution of insolation + feedbacks. I think that it would be better to explain what is natural variability (response to external natural forcing + internal variability including modes), then how you make the difference (methods of detection, methods of attribution, explaining these terms), then how results differ for the global scale and the regional scale, temperature and precip. Also, the last paragraph is incorrect : in the case of ambitious mitigation, one does not expect surface warming to continue, but to stabilize, and interrnal variability can also obscure the detection of the effect of mitigation. I think that this FAQ needs more work. [Valerie Masson-Delmotte, France]	Accepted. We have revised the FAQ to address these issues.
115883					FAQ3.2 explain how you measure the skills of a climate model, and how an improvement is measured, and the fact that a better match to present day or recent trends is not directly linked to responses to large perturbations of the Earth"s energy budget (as feedbacks can depend of the climate state). Missing reference to feedbacks in the whole FAQ, and confidence in the representation of feedbacks. Explanations why the assessment relies on a multi model ensemble is needed (just stating that no single climate model is better at all aspects). The notion of "centreed pattern correlation" needs to be explained in the text. [Valerie Masson-Delmotte, France]	Accepted. This is a duplicate comment (see 115809).
115885					FAQ3.3 Figure not clear on what is shown (GSAT? GMST? CMIP6?). I suggest to reconsider the flow of information. We are certain human activities are responsible for emissions of heat trapping gases. That this leads to an imbalance of the Earth's energy budget. That energy is accumulated leading to warming etc. Plus no natural factor can explain the pattern of what is observed, which by contract is in agreement with what is simulationed and theoretically expected when you add GHG. Moreover the observed warming is emerging from natural variability as well as other aspects. Please reconsider the insights obtained x chapters, not just from attribution methods. [Valerie Masson-Delmotte, France]	Accepted. This is a duplicate comment (see 115813).
116167					Congratulations for the maturation of the draft chapter, including more results from CMIP6. I also appreciate work done to consider evidence from paleoclimate. [Valerie Masson-Delmotte, France]	Noted. Thanks and appreciated.
116169					Several parts of the chapter still need work to shift from a discussion of the literature to a sharper, more concise assessment. [Valerie Masson-Delmotte, France]	Taken into account. We have revised the chapter to be more an assessment, less a review.
116171					What is missing in the ES is an assessment of the time of emergence as an outcome. There are attribution statements for specific time intervals, but they do not communicate when the human induced signal is detected above background variability for a given variable. [Valerie Masson-Delmotte, France]	Taken into account. New material has been added to chapter 1, following cross-chapter discussion, to discuss links between detection and emergence, which we now cite.
116175					Several aspects related to model evaluation are missing in the ES (systematic biases; effects of very high resolution; major mismatches between observed and simulated trends). [Valerie Masson-Delmotte, France]	Taken into account. Effects of high resolution are discussed in the final bullet.
116187					For sections starting at 3.3, the flow of information in each section is often hard to follow. It would be good to introduce the approach at the start of section 3.3 (as a preamble). There is a need to check the coherency of paleoclimate information across chapters, and also avoid duplication within the chapter (eg with section 3.8.2 on model evaluation using paleoclimate information), and to integrate insights from paleoclimate information where relevant in key findings (it is almost completely absent in the chapter ES). [Valerie Masson-Delmotte, France]	Taken into account. Structure of sections has been further clarified in the introduction. Additional checks on coherency of paleo information carried in collaboration with chapter 2, and duplication has been removed. Paleo assessment has been added to the ES.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116195					It is hard to find, in this chapter, how the use of CMIP6 models differs from the use of CMIP5 models for attribution (how to relate the outcomes of model evaluation with the results of attribution, explicitely). [Valerie Masson-Delmotte, France]	Taken into account. Fitness for purpose for attribution now more explicitly assessed. Added where support from the literature for the assessment is available, for example on model variability.
116201					It could make sense to have a paragraph in this chapter on forcing prescribed to models for the historical period (comparison between CMIP5, CMIP6, and estimations from chapter 2 on RF). It seems very relevant for model evaluation and other aspects addressed in this chapter (to coordinate with ch 2, 6, 7). [Valerie Masson-Delmotte, France]	Taken into account. CMIP6 radiative forcings are assessed in Chapter 7, and are now better cross-referenced in Chapter 3.
116211					I suggest to define and explain what is a model bias. [Valerie Masson-Delmotte, France]	Taken into account. Definition added to the introduction.
116215					For sections 3.4 and 3.5, please use SROCC too as a starting point, not just AR5, and check literature assessed in SROCC to avoid duplication. [Valerie Masson-Delmotte, France]	Taken into account. SROCC now added as a starting point.
116263					Most figures provide a depiction of evidence but there is also a need for figures that display the outcome of the assessment with a clear visual message. The chapter also would benefit from a visual abstract (also showing links to other chapters, typically on model evaluation, or for the attribution of extreme events, or the use of insights from model evaluation and attribution for confidence in projections, bias corrections etc). [Valerie Masson-Delmotte, France]	Taken into account. A visual abstract has been added, and the figures have been revised for clarity.
127391					[PROGRESS] Throughout this chapter, the general format of sections followed the pattern of first describing what was in AR5, then listing studies and findings since AR5, then finally, often in the very last sentence, providing the author's finding. There are numerous sections (and some executive summary sections) that begin with "The AR5...". While an explanation of what has changed since AR5 is valuable, always beginning each section with a lengthy explanation of AR5 findings goes beyond context-setting. Much of this could be placed in a simple table demonstrating what has changed since AR5. The second part of this pattern, where the authors list new studies and findings, is often tiresome to read when it does not clearly lead up to a conclusion. Much of this "x says this, y says this, z says this" structure would benefit from summaries, syntheses, and assessments rather than a simple listing of references. Finally, it is not always clear what the author's conclusions for AR6 are. Placing the conclusion at the every end of the sections buries the lede of the section and the findings of the authors, making it very difficult for readers to understand what the synthesis or assessment of the authors is in AR6, or whether the listing of all the references that preceded the conclusion led the authors to that conclusion. Suggest re-ordering sections to place the important conclusions up front and then clearly explain how studies that have been pubished since AR5 support that up-front conclusion. [Trigg Talley, United States of America]	Taken into account. We conclude each section with a clear summary assessment. However, the AR5 content is important for context and has been asked for in other review comments. In revising the chapter, we did more assessment and less a review of literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127393					Overall this chapter is appropriate and well worded; however, some sections are inadequate. This relates particularly to the ocean, glacier, and ice sheet sections. There is a section at the end of the chapter on model evaluation. But a more important question than have the models improved (as asked in this section) is are the models fit for purpose. This section does not address that. A more appropriate way to address this question would have been to have this correlation assessment at the start of the chapter, then address the individual phenomena (as in existing sections) for both the model adequacy and the human influence question. Also, really surprising to find no discussion of climate sensitivity: It remains one of the most important parameters, and it needs to be assessed with observations as well as models across the full range of observations. Part of the shortcomings are likely the result of a narrow authorship list and too large a focus on CMIP. [Trigg Talley, United States of America]	Taken into account. We have substantially revised the sections on oceans, glaciers and ice sheets. We have revised to focus on fitness for purpose for attribution. Climate sensitivity is assessed in Chapter 7, and we have added additional cross-chapter references to ch7.