Comment ID	From Page	From Line	Topage	To Line	Comment	Response
56120	0	0	0	0	Both chater 2 and 3 show a Global monsoon discussion: make sure they are consistent [Rolf Müller, Germany]	Taken into account.
27718	0	0	0	0	et al in italics, bibliographical citations in chronological order. [Poot Delgado Carlos Antonio, Mexico]	Editorial. We are following standard IPCC formatting for references.
27720	0	0	0	0	Unify the use of "and" in bibliographic citations [Poot Delgado Carlos Antonio, Mexico]	Editorial.
28802	0	0	0	0	Chapter is well written, realtively concise with great box on slowdown. Good balance of attribution and evalution. Great figures [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
48018	0	0	0	0	Scoping Outline Check: All bullets from approved outline are covered in the first order draft. [WGI TSU, France]	Noted.
29884	0	0	0	0	Difficult to make a sound review since the "Erreur ! Source du renvoi introuvable" is also in Chapter 3 corrections! [Christoph Marty, Switzerland]	Accepted. This has been corrected in the SOD.
28864	0	0	0	0	FAQ3.1 - original and good FAQ3.2 I would perfer something like What are climate models and are they any good?, or are climate models fit for purpose? People still confuse them with statistical models etc. so I think some clarity would help. On some levels CMIP6 may well be worse in terms of historical match... FAQ 3.3 great title, but if you keep it we need to give the lines of evidence equal weight - not just saay theory encapsulated in models and then base evidence on attribution. I think this could be merged usefully with FAQs in cHapter 1 and 2 to make a really nice FAQ [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have kept FAQ 3.2 title unchanged as it was developed by the TSU with stakeholder consultation. We accept the point regarding statistical models and address this comment in the revised version of FAQ 3.2. There is now a sentence there explaining that climate models generally solve equations based on the laws of physics, i.e. they are not statistical models. On the performance re CMIP5: Noted. Indeed we do not expect CMIP6 to be better across the board for all variables. Just on balance, weighing up the performances across different metrics, we do find progress. We feel this is reflected in the new formulation. Regarding FAQ 3.3, we have revised giving more equal weight to all lines of evidence.
49436	0				This chapter should not repeat, nor contradict, information given in the previous chapter. So when the previous chapter details paleo-records, a reference to that section of the previous chapter can be made here, rather than having a long paleo section again. This chapter should focus on the attribution of the observed changes listed in the previous chapter, not on the changes themselves. [Sonya Legg, United States of America]	Taken into account. We have revised the paleo discussion to re-focus on attribution and model evaluation, and ensure appropriate referencing of Chapter 2 paleo assessment.
47172	0				Congratulations for this well-written and comprehensive draft of chapter 3. I only have minor comments (see hereafter). [Hervé Douville, France]	Noted.
53332	0				A well written FOD, with good structure. Looking forward to further development where the authors go beyond review and do more assessment. (e.g. often the text tells what the CMIPs did but without really assessing) [Jan Fuglestvedt, Norway]	Noted.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
7290	0				This chapter is extremely difficult to review in its present form due to the lack of CMIP6 results. I have kept my comments to concerns about clarity and balance. [Bryan Weare, United States of America]	Noted. More CMIP6 results are assessed in the SOD.
14468	0				The chapter gives a good account of human influence on various components of climatic parameters, cyosphere, biosphere, and oceans. The influence is mostly negative. However, which aspect of humankind such as their demography, consumptive pattern, dietary habits, social norms or physical activities play rolein such influences is not explained. It might be difficult if not impossible; its elaboration can guide towards regulation of per capita carbon foot prints. [Muhammad Mohsin Iqbal, Pakistan]	Rejected. The topics mentioned here are WGIII topics.
51844	0				This chapter overall is extremely well written and clear. The key findings are well supported. [Peter Thorne, Ireland]	Noted.
51856	0				ES leads each statement with attribution then talks about model assessment. Text tends to lead each section with model assessment then delve into attribution. There is a risk this could jar with readers. Consider making consistent for narrative continuity? [Peter Thorne, Ireland]	Rejected. In the chapter we start with model evaluation, since this is a pre-requisite for attribution. In the ES we start with the key conclusions, and follow up with supporting evidence.
51890	0				In general the figures would benefit from work on adding self-descriptive titles so that it is immediately obvious what is being shown in each. Just axis labels alone often is insufficient. Consider adding titles to all figures / panels in figures which make explicit what they are showing in each case to aid readability. [Peter Thorne, Ireland]	Taken into account. Some improvements to figure presentation made in SOD. Additional improvements to be considered for FGD.
51896	0				I was a bit surprised not to see a Stott et al., 2000 style map comparing observed trends to ANT, NAT and ALL and showing the degree of agreement or otherwise across the globe. I think these are accessible to policymakers and due consideration should be given to inclusion in the subsequent drafts. [Peter Thorne, Ireland]	Noted. We considered the suggestion, but due to limited simulations and time did not add in the SOD.
45002	0				CH3 is an excellent comprehensive analysis of the human influence on recent climate change. Although it's not the focus, I believe that, to fully address its goal of evaluating the responses to forcing, and discerning internal from unforced variability, CH3 needs a more substantial treatment of paleoclimate modeling and data-model comparisons. I suggest that the sections on many of the major climate variables – particularly those large-scale indicators that are assessed based on paleoclimate observations in CH2 -- should include a statement about how well the paleoclimate model simulations compare with paleo observations for multiple paleo reference periods, with a formal assessment of confidence. I am a LA of CH2 and would be happy to help bridge between the paleo content of our two chapters. [Darrell Kaufman, United States of America]	Taken into account. Additional assessment of paleo-attribution and evaluation have been included in the SOD.
45004	0				It would be helpful to know whether CH3 will be using: (1) Annex III to list which models were used for the paleoclimate runs (like AR5 Table 5.A.1); (2) the Atlas to display the major results of paleoclimate experiments (maps of simulations for different time slices); (3) Annex II to list the primary datasets used to evaluate the PMIP model output. Thank you [Darrell Kaufman, United States of America]	Noted. Chapter 3 now assesses CMIP6 PMIP simulations. CMIP6 models are described in Annex III. We have added references to Annex II for relevant paleo data used in our figures. To our knowledge paleoclimate simulations are not shown in the Atlas.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
26134	1	1	163	1	The whole chapter reads like its biassed and is written to promote the “predictions” of the models, rather than be an honest attempt to describe what its title says “ Human Influence on the Climate System” [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Chapter 3 concentrates on the assessment of human influence on the climate system, and evaluation of fitness for purpose of climate models to simulate future changes in large-scale indicators is now covered in Chapter 4 where the projections are presented.
42924	1	1	163	14	This chapter is very ambitious, combining elements of multiple chapters from AR5, lots of great synthesis of literature published since AR5. I would like to see an extended description of the various and especially the new detection and attribution approaches to complement the existing literature review that is drafted now. I am planning to review the chapter carefully in the SOD once the CMIP6/PMIP4 results are available, figures are updated, figures made more easily readable (resolution made it difficult for me to analyze them carefully.), more literature in review available for synthesis. [Michael Evans, United States of America]	Taken into account. Description of methods in Section 3.2 has been extended. Note that some discussion of attribution methods is included in Cross-Chapter Box 1.4, which we cite.
51446	1	1	163	50	no reference is made to the Atlas chapter of AR6, which probably should be included in the SOD [Bart Van den Hurk, Netherlands]	Taken into account. Reference to Atlas added to Section 3.1.
26252	1		163		I enjoyed reading the FOD, and congrats to the author team who have done a great job at this stage! [Masahiro Watanabe, Japan]	Thanks and noted.
45254	1				In the review, a certain number of critical studies is missing and they should also be included. In addition, the review should have more quantitive results with uncertainties as well. [Jianping Guo, China]	Taken into account. Additional references have been added for the SOD. More quantitative results based on CMIP6 model analysis have been added in the SOD.
38056	2	1	4	8	I find hard to understand the rationale behing changing the treated variable in each of the main sections,; e.g. no Sea-level/ AMOC in the near term, mid-to-long term. Ocean temperature in the mid-to long but not in the 21st century. No ocean in the climate policy. I would recommend to harmonize, or at least, explain the rationale in the choices of variables, and for the need to change from one section to another. More generally, I find the ocean aspect in this chapter could (should) be more substantial. [Jean baptiste SALLEE, France]	Taken into account; comment applies to Chapter 4. SOD explains more explicitly the choice of variables displayed. Note that ocean aspects are covered comprehensively in Chapter 9; while some overlap is deemed useful, it should not be extensive.
45006	2	1			An assessment of the ability of climate models to accurately simulate key large-scale climate variables under forcings of previous climate states should be represented in the ES. [Darrell Kaufman, United States of America]	Rejected. The chapter follows its mandate and takes a holistic approach that uses multiple lines of evidence from paleo archives, observations, and simulations of the past, present and thus uses paleo information in the individual key conclusions of the executive summary. and future to assess understanding of key processes.
28072	2	20	2	21	The display of page number has a formatting issue [Gan Zhang, United States of America]	Editorial.
39092	2	21	2	21	The number of the page "28" is not aligned as the others; ti should be at the right of the page. [JACQUES ANDRE NDIONE, Senegal]	Editorial.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
49028	2	38			Inconsistent use of capitals [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
28784	3	7	3	9	Please please please don't repeat the mistake of Ar5, by saying "that human activities caused more than half of the observed increase in global mean surface temperature over the 1951-2010 period." This will be taken out of context to assume that changes before 1950 were not anthropogenic and we can only be certain that around 0.3C of warming was. I've had to address this so many times. Just say the first phrase or give a number. - and/or give upper bound as well, and say something about longer period, based on other lines of evidence if attribution studies are inconclusive [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised text avoids 'more than half' formulation, and specifies the best estimate and likely range of the anthropogenic contribution to observed warming.
26254	3	15			remove a false content of 'Further, AR5 … (IPCC, 2013)' [Masahiro Watanabe, Japan]	Editorial.
42306	3	27	3	31	Long sentence taht may not be so clear to policy and decision makers. Need to be rephrased in more simple words... [LUCATELLO SIMONE, Mexico]	Rejected. Page or line numbers in the comment are wrong, because there is no long sentence at the location referred to.
28792	4	0	7	3	ES is good, could be more quantitative but bullets are right length, could maybe add more model evalutation for better balance? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. More model evaluation has been added in the SOD now that we have more CMIP6 data available.
26136	4	1	7	4	The title of this chapter does not correspond with its contents. This chapter is based mainly upon the results of models NOT observations and it should clearly say so. Or change the title to Models: Predictions of Human Influence on the Climate System. [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The chapter considers attribution and evaluation based on comparisons of model simulations and observations. Note that the primary assessment of observations of large-scale indicators is in Chapter 2.
53362	4	1	7	5	I miss more about sea level rise in ES [Jan Fuglestvedt, Norway]	Taken into account. A sea level attribution statement was already included in the ES, but this has now been strengthened and quantitative information on observed SLR has been added.
7292	4	1	7	5	This should lead with something like the lines on page 6, lines 42-47. Overall, this is a good summary. [Bryan Weare, United States of America]	Taken into account. The ES now leads with a synthesis attribution statement across the climate system.
56224	4	1	7	5	Could add a sentence in the executive summary pointing to relevant material from chapter 11, e.g. "Assessments regarding human influence on trends in climate extremes is provided in Chapter 11". [Sonia Seneviratne, Switzerland]	Rejected. ES focussed on summarising the content of Chapter 3. But Section 3.1 provides a reference to Chapter 11 for attribution of extreme events. And assessment of human influence on trends in precipitation and temperature extremes is summarised in X-Chapter Box 3.2, which is referenced here, and which itself references the underlying assessment in Chapter 11.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
8296	4	1	7	6	See the above comments. As a critical reader, I want to have an unbiased description of the evidence on which the report's conclusion are based. It is thus crucial that the authors' clearly distinguish between personal assumptions, hypotheses, objective evidence, and expert opinions. This information is currently not well described. Furthermore, when empirical evidence is referred to, the inferential uncertainty should be presented to the reader if it has been quantified. If it hasn't this should also be made clear. [Jonas Ranstam, Sweden]	Taken into account. Traceable accounts, explaining the reasoning underlying the uncertainty assessments have been strengthened in the chapter.
14826	4	3	4	53	Chapter 7 includes an attribution assessment that doesn’t belong in the ES of that chapter but which needs to be absorbed into the overall assessment provided here. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Chapter 7 has removed observation-based attribution statements from their ES.
7350	4	3		5	this is a long statement. Make sentence simple by breaking or making short what the paragraph is communicating. I suggest: AR5 concluded that human influence on the climate system is very clear. An indication is the accumulation of greenhouse gas concentration in the atmosphere and the positive radiative forcing in the energy budget. Also is ssen in the physical understanding of the climate system like the observed warming. strong confidence {3.3-3.8} [Chukwuma Anoruo, Nigeria]	Rejected. We prefer to keep these statements in a single sentence and use almost the same wording as the AR5 to maintain clear traceability to the AR5 WGI SPM.
45414	4	3			The word conclusion is used at the beginning of the paragraph, chapter and subheadings, which is inappropriate. Perhaps, the author could reduce using the word "concluded" at the beginnings of sections in the entire chapter. [David Baguma, Uganda]	Rejected. The word 'conclusion' is not used in the headings or subheadings of Chapter 3.
26138	4	5	4	5	The evidence continues to support the AGW hypothesis.. [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
27328	4	5	4	5	The sentence "This evidence has strengthened" is not fully justified because of the uncertainties indicated in the relevant literature. Also Hegerl (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 millenial temperature changes (chapter 3.7) is not sufficiently taking into account. [ferdinand meeus, Belgium]	Rejected. Overall the evidence has strengthened, as assessed in the sections of the chapter, even if some uncertainties remain, which we assess.
27184	4	7	4	9	Half of the observed warming since 1945, beginning of the acceleration of CO2 emissions, is about 0.2°C as seen in Figure 2.12. Does this modest increase justify any alarmism? [François GERVAIS, France]	Noted. No edits or response required.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
47174	4	7	4	9	There may too much emphasis on the attribution of post-1950 climate changes compared to the whole « historical » period which is not fully justified by the lack of (homogeneous) observations in the early 20th century. This specific sentence is a good illustration of this potential « bias » which may be misleading for a wide audience. In this specific case, I would rather suggest to provide an assessment about the post-1850 global warming caused by human activities (in line with the first part of the sentence). Generally speaking, it may be important to emphasize the variable-dependent difference between the actual and net discernible human influence on climate before the 1950s or late 20th century, due to the potential competing effects of anthropogenic GHG and aerosols. This key message should be further supported by CMIP6 models, at least those having participated to the dedicated DAMIP experiments. [Hervé Douville, France]	Taken into account. We have revised the ES to focus on attribution relative to the 1850-1900 base period. More assessment of attributable warming since 1850 based on new literature is included in the SOD.
8286	4	7	4	9	The statement that "The best estimate of human-induced warming since pre-industrial is approximately equal to observed warming and it is virtually certain (P ≥ 99%) that human activities caused more than half of the observed increase in global mean surface temperature over the 1951-2010 period" is problematic. First, it is unclear what the letter "P" refers to. I find it unlikely that it refers to a p-value reflecting the risk of a false positive outcome of a null hypothesis test (showing statistical significance when P<0.05). It seems more likely that the letter P reflects an estimated likelihood that the statement is correct, i.e. the probability that human activities have caused more than half of the observed increase in global mean surface temperature over the 1951-2010 period. However, this is not a frequentist probability, i.e. something that could be said to be true in 99% of the cases. The P could perhaps represent a Bayesian interpretation of probability as a degree-of-belief interpretation. If this is the case, it should be clarified to the reader whether the "P ≥ 99%" represents a prior or a posterior probability. Otherwise, if it just expresses a subjective opinion with an uncertainty that cannot be objectively verified, this should be described to the reader. [Jonas Ranstam, Sweden]	Taken into account. We have removed the reference to 'P'.
28786	4	7	4	9	Could add upper stratosphere and co2 cooling dominates? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We don't have quantitative attribution studies which separate out the CO2 contribution to upper stratospheric cooling to assess.
38024	4	7	4	9	This would read better: It is virtually certain (P ≥ 99%) that human activities caused more than half of the observed increase in global mean surface temperature over the 1951-2010 period, and the best estimate of human-induced warming since pre-industrial is approximately equal to observed warming` [Jean baptiste SALLEE, France]	Rejected. This is a general statement indicating that the main features of the circulation are 'broadly reproduced'. No confidence assessment is needed.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
51852	4	7	4	9	While this framing is better than in AR5 it still includes the more than half statement that has been serially misinterpreted by a huge number of vested interests. I don't, personally, see what the stating more than half buys you in terms of assessment outcomes for a policymaker audience. It may be more useful, and less open to abuse to include both the lower and the upper bound rather than solely the lower bound if you wish to retain this information. At least that way the two-tailed nature is made explicit. Equally, can this not be made as a fact based statement without confidence / likelihood language? Would that be more powerful and yet still consistent with the assessed evidence? [Peter Thorne, Ireland]	Taken into account. We have re-framed as 'It is virtually certain that human influence is the main driver…' to avoid the 'more than half' formulism. Also we have added a fact-based statement to the ES 'Human influence has warmed the climate system' based on assessment across climate system components.
26256	4	7	4	9	This statement (especially 'caused more than half') does not change much from AR5; could you give a little quantitative assessment for the human controbution to global mean surfave temperature change? [Masahiro Watanabe, Japan]	Taken into account. The ES now reports the assessed contributions of anthropogenic warming, GHGs, and other anthropogenic forcings to GMST change relative to 1850-1900.
56222	4	7	4	9	It is a bit strange that the 2nd part of this assessment is only for "more than half" of the observed increase in global mean temperature, while the first part of the sentence mentions that "the best estimate of human-induced warming since pre-industrial is approximately equal to observed warming", i.e. approximately equal to 100% of the total warming. Provide a confidence assessment for the first half of the sentence (high?) and consider providing an assessment for the likelihood that a larger fraction of overall warming is due to human influence (e.g. 80%, or 90%)?. Else possibly rephrase 2nd half of sentence by replacing "more than half of" with "the major part of" [Sonia Seneviratne, Switzerland]	Taken into account. We have replaced this text with 'It is virtually certain that human influence is the main driver…' to avoid the source of confusion around 'more than half'.
27322	4	7	4	9	can cause confusion(numbers do not add up) because present total temperature increase is about 1°C compared to pre-industrial. And according to AR5 only since 1950 °t increase, about 0,6°C, is casued by human activity for more than 50% (also 52% is by AR5 definition possible). AR5 implies that any °t increase before 1950 is not antropogenic. Gabriele Hegerl (Clim Change 2018) on the warming between 1901-1950 : about 50% of global warming from 1901 to 1950 forced by combination greenhouse gas and natural forcing. Hegerl conclusion: exact contribution of each factor to large scale warming remains uncertain. Some extra clarification related to attribution for 1901-1950-2018 would be beneficial [ferdinand meeus, Belgium]	Taken into account. ES has been revised to focus on attribution relative to preindustrial, which removes this source of confusion. Attribution of early 20th century warming is assessed in Section 3.3.1.1.
26140	4	7	4	16	This is entirely based upon “new attribution” studies. “It should thereforebegin with “Based upon new attribution and modelling studies, the best estimates of…..... [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is not only based on new attribution studies, but also on literature already available at the time of the AR5.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
32648	4	7	4	16	I think it is a bit confusing to have three different likelihoods indicated (i.e., virtually certain, very likely, and extremely likely). In particular, I think it hard for the reader to identify what the factor is that is different between virtually certain and very likely. I also find it a bit unusual that there is no indication that human activities also induce some cooling influences that offset some of the warming influence. [Michael MacCracken, United States of America]	Taken into account. The assessed cooling contribution from aerosols and other non-GHG forcings is now reported in the ES. Justification for likelihood levels is in the chapter body.
9134	4	7	4	16	There are contradictory statements in this paragraph as to whether it is "virtually certain" or "very likely" that human activities caused "more than half" of warming since 1951 (this assertion appearing to originate from AR5, and which would not hold up if solar and other natural forcings were fully evaluated). Based on comments 2 to 7 above, it is likely that human activities account for less than half of recent warming. [Jim O'Brien, Ireland]	Rejected. The evidence supporting this ES statement is assessed in Section 3.3.1.1.
15328	4	7	4	16	Please include information about what has caused the other half of the observed increase in global mean surface temperature over the 1951-2010 period. It is helpful to understand that it is virtually certain (P>99) that human activities caused more than half of the observed warming. But the next question is, is the other half also likely attributed to human activities (and if so, how likely?), and if not, what are the other factors? [Lia Cairone, United States of America]	Taken into account. Statement has been re-phrased to clarify that the best-estimate of the human-induced warming is approximately equal to the observed warming.
57330	4	7	4	16	Really important to give an estimate with an uncertainty range of the current level of human-induced warming in terms both of GMST and GSAT (if people insist on continuing to discuss the latter in subsequent chapters). This is much more policy relevant than further increasing the confidence that more than half the warming is due to human influence. I'm sure all these things will be tidied up, but just to prove I'm awake, there seems to be a mismatch between the evidence provided and the headline "virtually certain" confidence qualifier. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Ranges of human-induced warming in GSAT and GMST are now given in the ES. The increase in likelihood from 'extremely likely' to 'virtually certain' for the statement that anthropogenic forcing is the main driver of the observed warming is based on new attribution literature appearing since the AR5, as well as additional observations of ongoing global warming following the slowdown period, and is justified at the end of Section 3.3.1.1.
57332	4	7	4	16	In addition to the current level of human-induced warming, the current rate is very policy-relevant, since it determines the time left to nearby temperature thresholds. Haustein and Ribes methods could both be used to estimate this -- strongly suggest this is brought out. It was a high-impact finding in SR1.5 [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Based on this comment we have added assessment of the anthropogenic attributable trend to the chapter text.
7352	4	7		9	although this is a backward projection. There is need to support this evidence with litereature or a confidence level. [Chukwuma Anoruo, Nigeria]	Rejected. The literature assessment is in the body of the chapter. We use a likelihood indicator in the sentence, and as per the AR5 uncertainty guidance note, a confidence indicator need not be explicitly given if a likelihood indicator is given - the confidence level is assumed 'high' or 'very high'.
49030	4	7			missing words? Perhaps should be "the pre-industrial period" [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have replaced the reference to 'pre-industrial' with a reference to the years 1850-1900.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
7354	4	9		12	is there no need to give one or two examples of such studies? I think this will better the understanding of readers and as well serve as a reference [Chukwuma Anoruo, Nigeria]	Rejected. The evidence is in the body of the chapter, and the ES contains references to the relevant chapter sections.
54786	4	11			I was wondering what the sentence about variability refers to as it sounds like in contrast to older approaches variability is now more robustly expressed - I am not sure this is the case when reading the supporting evidence, the improvement is in estimating the covariance matrix which affects more the ability to optimize than the estimated significance. Maybe that bit could be rephrased. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reference to internal variability has been removed here.
16016	4	12	4	13	Without clearly specifying the concerned period, the statement "It is very likely that greenhouse gas increases from human activities caused more than half of the observed increase in global mean surface temperature" is likely to cause confusion with the headline statement "it is virtually certain (P>=99%) that human activities caused more than half of the observed increase in global mean surface temperature over the 1951-2010 period". [SAI MING LEE, China]	Taken into account. Note that the first sentence described concerns greenhouse gases, and the second concerns combined anthropogenic forcings whose combined response can be more closely constrained. The corresponding text has been re-phrased.
7356	4	12		13	this is an open statement, because there is no evidence to support the claim. The previous statement will only balance this if only is referenced. [Chukwuma Anoruo, Nigeria]	Rejected. The evidence is in the body of the chapter, and the ES contains references to the relevant chapter sections.
53334	4	13	4	13	perhaps obvious since you give the period a few lines up, but still i think you could add "since..." in the end of the setence after "surface temperatre" [Jan Fuglestvedt, Norway]	Taken into account. The revised text states explicitly that the estimate of the GHG-attributable warming is for the same period as the estimate of anthropogenic-attributable warming.
39094	4	14	4	22	This paragrah is scientifically very strong. [JACQUES ANDRE NDIONE, Senegal]	Noted.
29786	4	15	4	16	CO2 increase, while heating the troposphere, causes a cooling of the stratosphere. This may be stated here. [Govindasamy Bala, India]	Rejected. While it is true that CO2 increases cool the stratosphere, the assessed attribution studies identify ozone depletion as the dominant driver of lower stratospheric cooling.
26142	4	18	4	25	Should read “Since AR5, the CIMP5 model has been revised and we assess with medium confidence that most CMIP5 models have overestimated observed warming in the tropical troposphere. [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This paragraph deals with more than just evaluation of tropical tropospheric warming, and the bold headline statement should reflect this.
32650	4	19	4	19	I don't think it is the case that the CMIP5 and CMIP 6 ensembles have been explained--how they differ--it would be helpful to give a sense of how they are different, their general messages, etc. [Michael MacCracken, United States of America]	Taken into account. The CMIP5 and CMIP6 ensembles are introduced and described in Chapter 1 of this report, which we reference.
14828	4	21	4	21	Most models overestimate tropospheric warming. To a significant degree? [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We include a confidence assessment here, but not a likelihood statement, based on limited knowledge of the role of forcing uncertainty (see assessment in 3.3.1.2).
38406	4	25	4	25	I do not understand why you only assign „medium confidence“ to the finding that „some differences remain“ between observed and modelled temperature. Isn‘t that certain? [Dirk Notz, Germany]	Noted. The 'medium confidence' applies to the whole sentence.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
8288	4	27	4	29	The statement that "There is very high confidence that the observed slower global mean surface temperature increase in the 1998-2012 period was a temporary event induced by internal and naturally-forced variability that partly offset the anthropogenic warming tendency over this period" shoule be clarified with respect to the term "confidence", which has a special statistical definition in relation to estimation uncertainty. Again, does this represent a subjective opinion or is it based on statistical inference? In the latter case, what is the uncertainty? [Jonas Ranstam, Sweden]	Noted. It is an expert assessment, and we follow the IPCC guidance note in our use of confidence language here (https://wg1.ipcc.ch/AR6/documents/AR5_Uncertainty_Guidance_Note.pdf). The rationale underlying our confidence assessment is provided in X-chapter box 3.1.
26144	4	27	4	31	You cannot simultaneously claim very high confidence in an increase based upon 5 years of data, whilst at the same time rejecting the previous 12 recent years of data. May I suggest “The observed slower rate of global mean surface temperature increase (or even decrease) between 1998 to 2012 is not inconsistent with CMIP5 or CMIP6 models, since in the following 5 years the observed temperature warmed strongly with (2014 to 2018) being the hottest five year period in the instrumental record. However, climate science should be based upon long-term averages of 30 years (see IPCC 5 Glossary “Climate” ref WMO) and it remains to be seen, with the benefit of hindsight, whether or not these recent fluctuations up or down are statistically significant. [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The statement concerning the past five years is a statement about observed climate change rather than attribution. It is included here because it is relevant to our assessment of the causes of the slower rate of warming over the 1998-2012 period, and the role of internal variability.
27186	4	27	4	31	The pause observed from 1998 to 2012 continues if the strong El Niño peak of 2016 is not taken into account since it is natural, as seen in Fig. 2.12. [François GERVAIS, France]	Noted. We focus on the 1998-2012 period for consistency with AR5.
26258	4	27	4	31	It is nice that this sort of statement is included in ES, but I wonder the hiatus period is better defined with 1999-2014 but not 1998-2012. [Masahiro Watanabe, Japan]	Noted. The period was chosen for consistency with AR5 and with the minimum 15-year GMST trend starting since 1981 in most of observational products.
28788	4	27	4	38	I support this bullet but I don't think comparison to cMIP is best as their trends likely have wrong forcing. Would be better to use an impulse response model based on known forcings, or similar [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Reject. We include comparison of CMIP6 models, which used updated forcing compared to CMIP5 models. The role of forcings is discussed in X-Chapter Box 3.1.
27330	4	27	4	38	The analysis for the short time period 1997/1998 to 2018 depends strongly on the start and end date. For instance Hadcrut4 anomaly for 1998=0,539 and for 2012= 0,470. and for 2018=0,580; The statements given for these small time periods are in conflict with Hadcrut4 data and WMO 30 year climate time window advice. Should be revised. We need to wait another extra 10 year for a more reliable detection&attribution climate trend . [ferdinand meeus, Belgium]	Reject: Points raised are reasonable, but we focus on this period for consistency with AR5. Also note that the 'hiatus' period is included in the annotated outline of Chapter 3. https://wg1.ipcc.ch/AR6/documents/AR6_WGI_Background_information.pdf
6269	4	30	4	30	Energy is one of the main climate change drivers. Energy balance and energy consumption pattern is also needed to be considered in all calculations or estimation, in local, national and global levels (Jafari, M. and Smith, P., (2018). Climate Change as a Driving Force on Urban Energy Consumption Patterns. In Encyclopedia of Information Science and Technology (4th ed., pp. 7815-7830). IGI Global. https://doi.org/10.4018/978-1-5225-2255-3.ch680) [Mostafa Jafari, Iran]	Noted. This is assessed by WGIII.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
29788	4	31	4	36	This statement is inconsistent with what is written on page 70, lines 20-27: the models overestimate the warming trends. The percentile range given on page 70 is only ~2-16 but the text gives much larger range [Govindasamy Bala, India]	Rejected. The statement correctly indicates that most of the observed trends are within the simulated 2.5 - 97.5 percentile range.
51440	4	33	#REF!	34	A 2.5 - 97.5% confidence range is pretty wide to conclude that observations are not inconsistent with models [Bart Van den Hurk, Netherlands]	Taken into account. Revised to use a 5-95% range.
9136	4	40	4	40	Where is the evidence of large-scale precipitation changes since 1950? Most rainfall records (eg UK and Ireland) show considerable inter-decadal variability with a stable long-term average. [Jim O'Brien, Ireland]	Rejected. The evidence is assessed in the body of the chapter in section 3.3.2.1, and several attribution studies find a significant anthropogenic influence on large-scale averages of precipitation over the high latitudes and tropics.
7358	4	40			please indicate condidence level, which will certainly serve as literature [Chukwuma Anoruo, Nigeria]	Rejected. Don't need to give confidence level if likelihood is given as IPCC AR5 Guidance Note https://wg1.ipcc.ch/AR6/documents/AR5_Uncertainty_Guidance_Note.pdf.
7360	4	41			is there no need to indicate the new studies. This will balance the mind of readers since there is great gap between 1950 to 2019 [Chukwuma Anoruo, Nigeria]	Rejected. Meaning of comment is unclear.
8290	4	46	4	47	Regarding the statement "There is high confidence that greenhouse gas increase and stratospheric ozone depletion has caused acceleration of the Brewer-Dobson circulation in the lower stratosphere", see the previous comment. [Jonas Ranstam, Sweden]	Not applicable. Assessment of B-D circulation changes has been removed from the chapter.
7362	4	46		47	ofcourse the BD circulation is driven by extratropical wave forcing and the seasonal variation of ozone in tropics is driven by the variation of BD circulation. But there is need to make reader understand more clearly this BD circulation scenariao. You can add: the stronfger the BD circulation, the lower the total ozone in tropics. [Chukwuma Anoruo, Nigeria]	Not applicable. Assessment of B-D circulation changes has been removed from the chapter.
28898	4	47	4	47	This is very inconsistent with chapter 2, and I think chapter 2 is right. Refer WMO 2018 Ozone Assessment figure 5-9 [Matt Tully, Australia]	Not applicable. Assessment of B-D circulation changes has been removed from the chapter.
49032	4	50			missing word? "the frequency" [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised accordingly.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
54788	4				Model uncertainty: Maybe I missed it but I didn't find much on it. Andrew Schurers 2018 (referenced but not for model uncertainty) paper shows eg in his figure 6 that the multimodel mean is very overconfident in estimating the greenhouse gas attributable warming. this to me seems worth flagging and discussing in text. its not a neat story but my sense is that the attributable greenhouse warming is, despite more uncertain than in the MM mean, quite well constrained (and if doubling the noise variance robust even to using MM) but the aerosol/other anthropogenic contributions are quite uncertain. this isnt an easy thing to discuss but i think you should be upfront with it. i wonder if anybody else did individual model results? Andrews results are certainly very consistent with Gareths. I see that figure 3.5 seems to plan to address this but it would be good to also have a section on this topic which is an important uncertainty [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We cite additional studies which test sensitivity of multi-model results using perfect model tests, such as Ribes et al. (2020) and Gillett et al. (2020).
29790	5	1	5	7	The depth of penetration of heat may be specified. [Govindasamy Bala, India]	Taken into account. Depth ranges for the upper ocean and deeper ocean are now given in this bullet point.
11500	5	7	5	7	Add "ocean" before heat uptake. Not clear if this is the total or just ocean part. [Roanld Stouffer, United States of America]	Not applicable. Sentence has been re-phrased, and it is now clear that it is the total ocean heat uptake being referred to.
51442	5	7	#REF!	#REF!	I assume that "contribution" refers to deep ocean heat uptake (and not ocean heat uptake in general)? [Bart Van den Hurk, Netherlands]	Taken into account. The reviewer is correct and the text has been clarified.
8292	5	9	5	11	Is the statement that "It is extremely likely that there is discernible human influence on observed surface and subsurface oceanic salinity changes since the mid-20th century, with the broad-scale changes assessed in AR5 consistently reproduced in all subsequent studies" based on direct empirical evidence, i.e. a meta-analysis of published evidence, or does it, again, represent the authors' subjective opinoin? Can the uncertainty of the statement be described? [Jonas Ranstam, Sweden]	Rejected. Statement is based on expert assessment. The basis for the confidence/likelihood is explained in the body at the end of 3.5.2.2.
29792	5	9	5	18	The qualitative pattern of changes in zonal mean salinity may be discussed so readers can get an idea of how P-E changes under global wamring. [Govindasamy Bala, India]	Rejected. The spatial pattern of the changes is discussed in the body of the chapter, but there is insufficient space to describe it in the ES.
7364	5	9		18	should state the page. There is a great jump from the human influence to precipitation to model simulations. Page 5 line 1-7 should be interchanged with line 9-18. this will logically link the statement. [Chukwuma Anoruo, Nigeria]	Rejected. Structure of ES follows that of the chapter, and attribution of model evaluation of precipitation are dealt with together. Although precipitation changes and ocean salinity changes are linked, we think prefer to deal with these variables in the ES in the same order as in the chapter.
51444	5	15	#REF!	16	the subsentence on CMIP5 is a bit unclear: do you imply that CMIP5 only reproduces upserved patterns when aggregated to coarse basin scale resolution? [Bart Van den Hurk, Netherlands]	Taken into account. The reviewer misinterpreted the sentences. 'only'moved to before 'in simulations including greenhouse gases' to clarify.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
38408	5	17	5	17	In my view, any qualitative assessment of „model agreement“ must be relative to the level of internal variability. Hence, there is no clear logical link between „high internal variability“ and „less good model agreement“ - high internal variability only changes the definition of „good“ in this context (see, for example, Notz, 2015, https://doi.org/10.1098/rsta.2014.0164) [Dirk Notz, Germany]	Taken into account. This text has been deleted.
14830	5	17	5	17	Strong natural variability does not automatically imply less good model agreement. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This text has been deleted.
49034	5	17			less good - poorer? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been deleted.
28798	5	19	25	19	Need to check consistancy with chapter 8 [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have discussed with Chapter 8 and aimed for as much consistency as possible with Chapter 8, though some issues remain.
8294	5	20	5	22	It should be clarified to the reader whether or not the statement that "It is virtually certain that anthropogenic forcing is the dominant term in observed changes to global mean sea level, with simulations that exclude greenhouse gases unable to capture the increasing trend in thermosteric sea level rise over the historical period" represents the authors' expert opinion or actual empirical evidence. If it represents empirical evidence, what is the uncertainty of the finding? [Jonas Ranstam, Sweden]	Taken into account. The justification for the assessed conclusion on SLR has been strengthened in Section 3.5.3. 'Virtually certain' implies a probability of P>99%, as per AR5 guidance on uncertainties: https://wg1.ipcc.ch/AR6/documents/AR5_Uncertainty_Guidance_Note.pdf
26146	5	20	5	24	Diagram (c) IPCC AR5 Ch13 Page1147 shows a uniform rate of sea level rise from 1880 to 2010. In my experience this agrees with what is observed by many port and harbour hydrographers worldwide. In 1880 CO2 and global temperature had not started to rise although sea-level had - and yet there is an accepted lag between global temperature and sea-level. Yet now you state you are vritually certain that the bulk of the sealevel rise is from CO2. It just doesn't fit and you should say so. And the continued reference to historic studies, models, simulations is not helpful. In fact it could be that the bulk of sea-level rise since the inductrial is non-anthropogenic because of the delay. If / when the anthropogenic effects take hold it could of course be worse. [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The text has been revised to focus on the sea level increase itself, rather than any changes in its rate of increase.
9138	5	20	5	24	See comment 19 above to the effect that there is little evidence of anthropogenic influence on sea level rise since 1850. [Jim O'Brien, Ireland]	Taken into account. The justification for the assessed conclusion on SLR has been strengthened in Section 3.5.3.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
55230	5	20		24	[pt 1 of 2] The text says, "It is virtually certain that anthropogenic forcing is the dominant term in observed changes to global mean sea level, with simulations that exclude greenhouse gases unable to capture the increasing trend in thermosteric sea level rise over the historical period. Since the AR5, further studies have highlighted that model simulations that include all forcings (anthropogenic and natural) most closely match observed estimates of global mean sea level rise. {3.5.3}" That is the opposite of the truth. The 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. Those GHG emissions and the concurrent warming have caused no significant, detectable, sustained acceleration in the rate of sea-level rise. [cont'd] [David Burton, United States of America]	Taken into account. The justification for the assessed conclusion on SLR has been strengthened in Section 3.5.3.
38410	5	21	5	22	I find the notion of an „increasing trend in sea-level rise“ somewhat confusing. Is this really meant to imply that the models fail to simulate the increase in the trend in the rise of sea level? I think that the much stronger statement can be made that not even the sea-level rise itself can be captured in models that exclude greenhouse gases. [Dirk Notz, Germany]	Taken into account. The text now focusses on the sea level increase, rather than changes in the trend.
51616	5	21	5	22	Do you mean, 'with simulations that exclude ANTHROPOGENIC GHG unable to capture the increasing treand…'? Because other simulatitions would need to include norma;/natural GHG in their calculations, I think? [Lindsey Cook, Germany]	Taken into account. This sentence has been revised.
38026	5	26	5	27	Assessment/confidence is missing here [Jean baptiste SALLEE, France]	Taken into account. This paragraph has been re-written. Parts of the paragraph describe general assessment results which may not require confidence language. Confidence language has been added for the assessment of model circulation strength.
47176	5	26	5	31	What about following here R. Sutton’s recommendation about low-probability but physically plausible high-impact « scenarios » and add a final sentence to this paragraph such as: « A recent anthropogenic slowdown of the AMOC cannot be ruled out given the competing effects of anthropogenic GHG and aerosols before the late 20th century, the debated cause of the recent warming hole over North Atlantic and the possible overestimation of the AMOC stability in CMIP5 models ». [Hervé Douville, France]	Rejected. Existing assessment is correct as it stands. The reviewer's recommendation is more relevant to projections.
46724	5	26	5	31	no confidence assigned [WGI TSU, France]	Rejected. This is a general statement indicating that the main features of the circulation are 'broadly reproduced'. No confidence assessment is needed.
26260	5	26	5	31	There is no statement to confidence. [Masahiro Watanabe, Japan]	Rejected. This is a general statement indicating that the main features of the circulation are 'broadly reproduced'. No confidence assessment is needed.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
55232	5	30		24	[pt 2 of 2] 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]	Taken into account. See above.
38412	5	33	5	33	Both section 3.4.1 and chapter 9 state that anthropogenic forcing has „substantially“ contributed to Arctic sea-ice loss, strengthening the respective finding from AR5. I think the wording here should be consistent and thus include the term „substantially“. [Dirk Notz, Germany]	Accepted. Reviewer is correct and suggested change has been made.
53336	5	33	5	33	Why are you saying "in particular greenhouse gas increases" ? Is it to separte from any possible BC contributions? [Jan Fuglestvedt, Norway]	Rejected. This is to distinguish the role of greenhouse gases from the role of aerosols, which are treated in the following sentence. The separate influence of BC on Arctic sea ice had not be addressed in the attribution literature.
9140	5	33	5	34	See comment 17 above on Arctic sea ice melt having substantially halted since 2007. [Jim O'Brien, Ireland]	Noted. Not clear what 'comment 17' is. We focus on long-term trends in sea ice extent here.
7366	5	33		34	this statement is not clearly in agreement with page 4 line 40. what year is really the pre-industrial year? Please be consistient in the year. Also refer to page 5 line 41-42 [Chukwuma Anoruo, Nigeria]	Rejected. There is no inconsistency in assessments for precipitation and sea ice. Period considered depends on availability of observations.
49036	5	33			spelling mistake - particular [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected.
32652	5	34	5	35	Is it not also likely that over the last several decades black carbon aerosols from China and elsewhere have been carried into the Arctic and contributed to the amplified warming? [Michael MacCracken, United States of America]	Noted. No formal attribution studies of BC influence on Arctic SIE exist to our knowledge.
52564	5	38			this statement implies that we have low confidence in our science, which is different from low confidence in the causes. Please rephrase. [Douglas Maraun, Austria]	Taken into account. The word 'scientific' has been deleted here.
11502	5	46	5	48	Type-o CO2 [Roanld Stouffer, United States of America]	Accepted. This has been corrected.
27188	5	48	5	48	The 2 of CO2 is incorrectly written as an exponent instead of an indice. [François GERVAIS, France]	Accepted. This has been corrected.
14448	5	48	5	48	In CO2, 2 should be subscript rather than being a superscript. [Muhammad Mohsin Iqbal, Pakistan]	Accepted. This has been corrected.
46726	5	48	5	48	CO2 [WGI TSU, France]	Accepted. This has been corrected.
26262	5	48	5	48	correct a supersprict for CO'2' to a subscript. [Masahiro Watanabe, Japan]	Accepted. This has been corrected.
26946	5	52	5	52	The last sentence in this paragraph ("Earth system models …") concerns oceans and is out of place in a paragraph on photosynthesis and plant growth, so it should be deleted. [Joachim Rock, Germany]	Rejected. The paragraph concerns land and ocean carbon sinks, so this statement is in scope and is linked to the land assessment.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
29794	5	55	6	5	The increase in acidity is caused by an increase in CO2, not by warming. T increase should reduce the ocean uptake of CO2 by increased stratification and hence reduce acidification. [Govindasamy Bala, India]	Noted. The text already states that acidification is driven by CO2 uptake.
49038	6	2			missing words? "part to be associated" [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. 'is likely in part associated' is OK.
28900	6	7	6	9	Why would you put the SAM and the QBO in the same bullet? The SAM has a direct effect on weather and its changes are well established, completely different to the QBO. [Matt Tully, Australia]	Taken into account. The discussion of the QBO has been removed.
7368	6	7		9	ofcourse the duration of the decrease phase of total ozone depends on the sequence of QBO cycles. I think there is need to explain more about QBO and SAM before highlighting on it. [Chukwuma Anoruo, Nigeria]	Rejected. The statement is focussed on effects of ozone depletion, not on causes of changes in ozone. Also note that the QBO discussion has been removed.
28902	6	8	6	9	This is very inconsistent with chapter 2. The amplitude of the QBO seems to have increased again in recent years. [Matt Tully, Australia]	Taken into account. The discussion of the QBO has been removed.
47178	6	14	6	15	One « mode of variability » is missing that may deserve a particular attention given its potential sensitivity to global warming: the COWL (Cold Ocean Warm Land) that can be diagnosed as the second EOF of the extratropical Z500 in winter (e.g., He et al., Clim. Dyn ., 2014). [Hervé Douville, France]	Rejected. Our assessment of modes of variability is not exhaustive. We focus on a subset of key large-scale modes introduced in Section 2.4 of Chapter 2.
44646	6	14	6	22	"AO" also is a important mode, here should add AO. [Liang Zhao, China]	Rejected. AO is the same as the NAM, which is discussed. Section 3.7.1 indicates that the NAM and AO are two different names for the same thing.
46728	6	14	6	22	no confidence assigned [WGI TSU, France]	Taken into account. Level of evidence is reported for the statement that anthropogenic forcing has affected the principal modes of interannual climate variability ('no robust evidence'). As per IPCC confidence guidance note, for findings with limited evidence/agreement, we should assign level of evidence and agreement, rather than a confidence level. Middle two sentences are general, and don't require a confidence assignment in our view. Confidence level has been added to the final sentence.
15552	6	24	6	35	It is unclear which anthropogenic influences on Atlantic Multidecadal Variability are more evident. It is necessary to describe more details with a clear evidence. [SANG-WOOK YEH, Republic of Korea]	Taken into account. Text revised to note that the anthropogenic influence concerned is mainly aerosol forcing.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
29144	6	24	6	35	I am not sure if we can say with certainty that proxy reconstructions suffer from too high uncertainty while instrumental data are sparse and not long enough, but in general correct. Proxy reconstructions use the same spare and uncertain pre-1950 or pre-1970 data to calibrate the proxies, assuming that instrumental reconstructions better reflect reality. I would speak of observations and reconstructions based on incomplete instrumnetal data infilled with statistical models. Those models often assume stationarity, as do proxy-instrumental data calibrations. My conclusion is that proxy data might not be associated with high uncertainties as we think and that instrumental reconstructions might be as affected by uncertainties as the proxies. I think its important to value the proxy data and what they tell us about longer term cycles without over-interpreting. I agree to say that multi-decadal processes and their drivers and global teleconnection pathways are poorly understood. [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been revised to discuss inconsistencies between proxy reconstructions.
51618	6	37	6	40	Is there any way to connect this statement with existing research of increase in human suffering/loss of life due to extremes experienced over a period of years? Connecting the consequence to people/nature, with such statements, helps the reader to absorb the consequence of information presented. [Lindsey Cook, Germany]	Rejected. This is out of scope for WGI. Assessment of impacts of climate change belongs in WGII, and assessment of changes in physical climate and impacts will be brought together in the Synthesis Report.
47180	6	42	6	47	Although it relates to section 3.8.1, this paragraph could be merged with the first key message of the executive summary. [Hervé Douville, France]	Rejected. Executive summary contains assessment statements on human influence on climate. This introduction describes the scope of the chapter.
28790	6	42	6	47	We have a similar bullet in chapter 7 based on total heat content change. Could do with date range and use our energy budget data to give a magnitude - or leave as is so we can add details [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Focus here is on assessment of attribution of warming across the climate system, whereas Chapter 7 focussed on total heat content change.
26264	6	42	6	47	I do not understand why this bullet point is necessary because no quantitative assessment is given with this statement. [Masahiro Watanabe, Japan]	Rejected. This statement synthesizes across climate system components.
26266	6	49	6	50	I'd suggest rephrasing the bold sentences to 'Climate models have continuously been improved, with more high-resolution models that better capture small-scale phenomena, and more Earth system models that include additional biogeochemical processes'. [Masahiro Watanabe, Japan]	Taken into account. Reference to extremes has been removed as suggested.
29146	6	49	6	55	Yes, climate models have improved, we understand certain aspects better. But what about proxy reconstructions? I would like to see a separate paragraph here what the proxy community has achiveved in terms of attribution of human unduced climate change. I think there are ample papers to make a statement here (the PAGES2k community papers). Otherwise, the proxy data remain to be treated as secondary and uncertain realisations of the climate system and its variability. Proy outcomes are nicely mentioned in this chapter, but to my opinion not given the weight they deserve in the summaries. [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have strengthened the assessment of proxy-based attribution through the chapter.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
28904	6	49	7	3	I would tone down this bullet, somewhat, and make it sound more factual and less like marketing. [Matt Tully, Australia]	Taken into account. Evaluation of fitness for purpose for projections has been deleted from this bullet, and is now covered in Chapter 4.
44552	6	49			This ES item is quite similar to one from Chapter 1 (page 1-4 line 51, based on section 1.4.3). They're both relevant where they are, but let's coordinate the wording so they don't seem quite so overlapping. [Bjorn Samset, Norway]	Taken into account. The reviewer is correct that these two bullets were similar. The sentence on observational constraints has been deleted from our bullet, which reduces the overlap, and the first sentence has been revised based on assessment of more CMIP6 models.
7370	8	3		8	should be summarizied in a simple form instead of starting it differently. [Chukwuma Anoruo, Nigeria]	Rejected. Not clear what changes the reviewer is asking for.
52566	8	3			Is this good (and IPCC) language? A scientist can assess, not a chapter. Applies to similar cases below as well. [Douglas Maraun, Austria]	Rejected. Similar language was used in the introduction to AR5 WGI Chapter 10.
30570	8	6	8	10	results/conclusions from the 1.5C report should be mentioned as well [Annalisa Cherchi, Italy]	Taken into account. A reference to the SR1.5 has been added to Section 3.1.
30572	8	19	8	23	link is not clear: confidence in forcing is different from confidence in models (anyway models are considered later) [Annalisa Cherchi, Italy]	Rejected. This discussion is highlighting areas relevant to this chapter where confidence was low in the AR5, and understanding the role of forcing in the slowdown period was one of them.
45416	8	36		38	Check again accuracy of this sentence. [David Baguma, Uganda]	Taken into account. More paleo evaluation is included in the SOD.
45008	8	37	8	38	Please be specific about which PMIP time slices are evaluated in this chapter. This will help the reader, and (more urgently) the authors of other WG1 chapters, know what paleoclimate information is included in CH3. [Darrell Kaufman, United States of America]	Rejected. The introduction defines the general scope of the chapter, but does not give all the periods covered in the literature assessed.
39096	8	38	8	40	Please, this sentence needs to be reframed. [JACQUES ANDRE NDIONE, Senegal]	Taken into account. This sentence has been revised to indicate that the chapter focusses mainly on assessing studies using CMIP6 models, while building on the evidence in the AR5 and Special Reports.
7372	8	42			is it all the previous IPCC reporrts? If not, there is need to be specific in this statement or rather cross-ref those reports. [Chukwuma Anoruo, Nigeria]	Taken into account. It is the AR5 and IPCC Special Reports. The AR5 itself was based on previous IPCC reports, but we don't generally cite those earlier reports here.
47182	8	49	8	54	The tuning of global climate models for capturing the observed mean state and/or historical trends is a growing issue that may deserve a more thorough discussion in Chapter 3. How the suggested recommendation about the need to temper the results of tuned models has been implemented in this chapter and in the whole AR6 WG1 report? [Hervé Douville, France]	Taken into account. We now identify which models have used historical climate evolution as a tuning target in Fig 3.3, and assess the effects on pg 13 of the SOD.
30574	9	6	9	6	what surface? [Annalisa Cherchi, Italy]	Taken into account. Replaced with 'Earth's surface'.
30576	9	6	9	12	section 3.7 missing in the list [Annalisa Cherchi, Italy]	Taken into account. Section 3.7 now mentioned.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
46730	9	15	10	33	contents about D&A methods are too brief [WGI TSU, France]	Accept. We have added introductory sentences for better understanding based on AR4 and AR5.
8306	9	15	11	32	The methods section is largely unreadable because vital information is missing and other description is too detailed. This section seems to be written for impressing the reader rather than for conveying important information, a sign of silo mentality. I recommend re-writing the methods section or removing it. [Jonas Ranstam, Sweden]	Accepted. We have added introductory sentences to provide background information. Also we have better linked this section to other related parts.
43346	9	21	9	21	Not sure of this is editorial or substance: The word "casual" on this line looks wrong. Should it be "causal"? [James Renwick, New Zealand]	Accept. Typo has been corrected.
51448	9	24	#REF!	29	Will the Atlas online model evaluation tool be included in this chapter as well? [Bart Van den Hurk, Netherlands]	Rejected. Our chapter considers only large-scale observed changes.
53338	9	31	9	34	Very useufl with links like this - espcially with the new structure [Jan Fuglestvedt, Norway]	Noted. We now consider adding links to previous reports as well.
30578	9	37	9	37	section contains list useless for non-scientific readers, but it is just a list without specifying relevant differences among methods so it is useless also for scientific readers [Annalisa Cherchi, Italy]	Taken into account. We have provided more background information for better understanding by scientific readers.
8554	9	37	10	32	Very jargony and hard to follow. [Robert Kopp, United States of America]	Accept. We have provided background information to improve readability.
54792	9	37			Good methods section, but could cite Hasselmann and maybe crossrefer to earlier more detailed discussions in AR4 or AR5 of methods. Reading this the only people who have innovated is Oxford and IPSL… [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We added an introduction paragraph based on AR4 and AR5 as well as classical papers as needed.
9598	9	38	9	52	Lines 38-52 use a lot of complex statistical terms without really describing what those methods are doing or how they differ among each other. I would suggest either simplifying this section, so it is easier to follow or to include more detail for each method mentioned, describing what it does. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added more details.
8298	9	39	9	39	The phrase "Fingerprint methods are based on linear regressions" is vague and needs to be specified. I assume that the term "regressions" does not refer to regression but to regression analyses, regression models, or regression equations. [Jonas Ranstam, Sweden]	Accepted. We add introductory sentences for better understanding with citing classical papers including Hasselmann's.
52570	9	39	10	32	I am not sure whether this paragraph is so helpful for a non-expert in fingerprinting. Would it make sense to be less technical but rather list the underlying assumptions and to what extent these assumptions are met or can be relaxed by different approaches? This would enable one also to be much more precise compared to the currently technical but often still vague statements (e.g., “standard residual consistency test, or an improved one” is useless information). [Douglas Maraun, Austria]	Taken into account. We have added more explanations on method terms for better understanding by non-expert in fingerprinting.
52568	9	41			If you refer to classical papers, do also refer to the original papers by Hasselmann! [Douglas Maraun, Austria]	Accepted. We cite it.
51450	9	44	#REF!	46	The need to use this inverse covariance matrix is not trivial to me. Why is this needed to normalize with natural variability? [Bart Van den Hurk, Netherlands]	Taken into account. We have provided more background on this.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
9596	9	46	9	48	I found this section unclear. This sentence sounds vague and complicated. Perhaps it would be useful to clarify some terms used here: What is the residual consistency test? What was the improvement in RCT? [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added more details on the residual consistency test.
29336	9	46	11	42	The section title mentions International Governance. But are all the sub-sections 'international governance' ? IPBES, IPCC,e tc are not exactly governance. Suggest to think of a more appropriate title. Also might be useful to bring the IPCC AR6, SROCC and SRCCL together [Minal Pathak, India]	Noted. This look like a comment for other chapter.
7374	9	54			mention one or two of such recent studies which should be between 2016-2019 [Chukwuma Anoruo, Nigeria]	Rejected. Those studies are described subsequently.
49040	10	1	10	5	change in tense [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Applied past tense for the first sentence. We argue that using present tense is better for the parts describing methods.
54794	10	8			Schurer et al 2018 is actually optimized. Polson isnt as the signal to noise ratio didn't improve but the biggest factor is not the uncertainty in the covariance but the necessary truncation which can lead to ill represented signals that are hard to detect. so please dont cite Schurer here - but you could add him to the Bayesian section (infact its a bit unfair he is not there). [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accept. We remove Shurer et al. 2018.
8300	10	15	10	15	The phrase "new studies suggesting probabilistic approaches to the detection and attribution question" is vague, detection and attribution of what? [Jonas Ranstam, Sweden]	Accept. We revise it as "detection and attribution of climate change signals" or similar.
9600	10	15	10	32	This section is very interesting but difficult to read due to the use of many complex terms that are not explained. Perhaps briefly clarify the terms used. For example, specify that ;linear additivity' refers to model responses to different radiative forcings. Also what is what does 'discriminant analysis' refer to? Please explain briefly or clarify. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added more details.
7376	10	17			is there no other method that accounts for climate modelling? Then a clear comparison of the fit method should be accounted. [Chukwuma Anoruo, Nigeria]	Taken into account. We have added other studies that deal with the climate modelling uncertainties in detection and attribution.
7378	10	24			climate change signal is also not limited to seasons and geomagnetic latitude [Chukwuma Anoruo, Nigeria]	Taken into account. We have added an introductory sentence explaining time-fixed signal patterns.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
14578	10	35	16	31	I may have missed it but I have not found here or elsewhere in this chapter or in the other chapters a discussion of the recent temperature changes in Antarctica. If it is includes in another chapter, a reference here would be nice. If not, it must be included somewhere. The instrumental records is short. It shows substantial differences between East and West Antarctica while models tend to be more homogenous but this seems consistent with natural variability (Smith and Polvani 2017, Klein et al. 2019). Besides, most models suggest a large warming since 1850 due to anthropogenic forcing which is incompatible with reconstructions. This must be mentioned and discussed to my point of view (e.g., PAGES2k-PMIP 2015, Abram et al. 2016). This discrepancy may also have an impact on projection discussed in Chapter 9. References: Abram et al. 2016 already cited in the chapter. Klein F., N. J. Abram, M. A. J. Curran, H. Goosse, S. Goursaud, V. Masson-Delmotte, A. Moy, R. Neukom, A. Orsi, J. Sjolte, N. Steiger, B. Stenni and M. Werner, 2019. Assessing the robustness of Antarctic temperature reconstructions over the past two millennia using pseudoproxy and data assimilation experiments. Climate of the Past 15, 661–684, https://doi.org/10.5194/cp-15-661-2019 . PAGES2k-PMIP3 group 2015. Continental-scale temperature variability in PMIP3 simulations and PAGES 2k regional temperature reconstructions over the past millennium. Climate of the Past, 11, 1673–1699, 2015 www.clim-past.net/11/1673/2015/ doi:10.5194/cp-11-1673-2015. Smith, K. L. and Polvani, L. M.: Spatial patterns of recent Antarctic surface temperature trends and the importance of natural variability: lessons from multiple reconstructions and the CMIP5 models, Clim. Dynam., 48, 2653–2670, https://doi.org/10.1007/s00382-016-3230-4, 2017. [Hugues	Accepted. Antarctica has been added to Figure 3.6 and its discussion.
14450	10	37	10	38	The lines 37-38 are repetitive of lines 54-55 of the same page (Page 10). One of these could be deleted. [Muhammad Mohsin Iqbal, Pakistan]	Accepted. The section head has been deleted.
51846	10	37	10	38	Personally I see little value to the reader of such stub introductions given that this is implicit in the title anyway and already obvious from the introduction. [Peter Thorne, Ireland]	Accepted. This sentence has been deleted.
57832	10	44		35	Anthropogenic analysis of variability of climate change in relation to surface temperature must be included. This is to allow thevhuman influence on surface temperature to be simulated and integrated in reto surface temperature vulnerability. integrated approach alomg witgvthe simulated variability of surface temperature in relation to climate research, forcings, theoretical framework and temperature extreme must be stated. [Abiodun Adegoke, Nigeria]	Noted. Using models to attribute surface temperature changes is indeed the topic of that section.
45418	10	45			why good? is there bad. Perhaps, rephrase or delete [David Baguma, Uganda]	Editorial. Long-term temperature records are good compared to other variables, especially precipitation.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
8302	10	48	10	49	The statement that "AR5 assessed that it was extremely likely that human activities had caused more than half of the observed increase in global mean surface temperature from 1951 to 2010", and similar, need to be clarified with respect to whether or not this represents a general expert opinion or the outcome of a quantified synthesis (meta-analysis). [Jonas Ranstam, Sweden]	Rejected. This is the result of the AR5 assessment, which relies on an assessment of peer reviewed literature and expert judgment.
27190	10	48	10	50	Half of the observed warming since 1945, beginning of the acceleration of CO2 emissions, is about 0.2°C as seen in Figure 2.12. Does this modest increase justify alarmism? [François GERVAIS, France]	Rejected. Half is the fraction attributable at the extremely likely confidence level. Much larger attributable fractions come with lower, but still sizeable, confidence levels. The report also notes that observed warming matches the best estimate of anthropogenic warming.
7380	10	49			substantriate the observed global mean temperature with a literature. Then proceed with the certain account of warming. [Chukwuma Anoruo, Nigeria]	Rejected. That specific statement repeats the AR5 conclusion.
45010	11	3			I believe that this paragraph (“paleoclimate context”) serves as THE primary account in the WG1 report of the (1) comparison between temperatures from paleoclimate models and paleo observations; (2) the simulated effect of long-term orbital, volcanic and solar forcing on temperature; (3) estimate magnitude of internal versus forced temperature variability over multi-decadal and longer time scales. Each one of these topics seems important enough in the context of model evaluation and attribution to warrant a substantial treatment in CH3. If these topics are not going to be covered in some detail in CH3, then it would be helpful if the CLAs would decide where they belong in the WG1 report. [Darrell Kaufman, United States of America]	Accepted. The paragraph has been substantially improved for the SOD with the help of CAs with paleo expertise.
30580	11	4	11	6	useless preamble [Annalisa Cherchi, Italy]	Rejected. The preamble provides the structure of the paragraph that follows.
45420	11	6		8	Split the sentence to improve clarity. [David Baguma, Uganda]	Editorial. No changes made.
32654	11	10	11	10	Saying the year "801" sounds far too precise, how about saying since the beginning of the 9th century, or something similar. [Michael MacCracken, United States of America]	Accepted. The paragraph has been rewritten and that level of detail has disappeared.
56154	11	10	11	12	Can this sentence be more precise? McGregor et al., 2015 does not indicate 'a long-term period of cooling, attributed from year 801 onwards reversed around 1800 for ocean temperatures and them Abram et al., 2016 for ocean and land temperatures; both studies are not comparable in terms of resolution; McGregor et al., 2015 laid the foundation for understanding the broad strokes at bi-centennial time scales, whereas Abram et al., 2015 delved into the details and evaluated the interrelationship between climate change and societies at a decadal scale (<10-y slices) [Belen Martrat, Spain]	Accepted. The paragraph has been substantially improved for the SOD with the help of CAs with paleo expertise.
14452	11	11	11	12	The number '1800' needs to be qualified as 'year 1800' and '1850' as 'year 1850'. [Muhammad Mohsin Iqbal, Pakistan]	Editorial. Practice adopted in the section.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
29148	11	11	11	12	"…and 1850 for ocean an land temperature (Abram et al., 2016)." Abram et al. 2016 state onset of warming in the 1830's over tropical oceans and certain regions in NH, while SH and Antarctica show late emergence or none yet, respectively. The tropical E Pacific shows later onset of warming as already mentioned by Tierney et al., 2015 (PAGES Oceans2k). Please be correct here that onset of warming pre-dates 1850, thus onset of instrumental observations. This is important beacause that means that we need to capture earlier changes as pre-industrial to set the right baseline for the pre-industrial vs. industrial reference period. Maybe 1750 is better suited in model studies than 1850 to present to define strat of industrial period. That could be a suggestion for future studies. [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement has been improved as suggested.
6477	11	13	11	13	This a good point to mention that temperatures start to rise in 1800 although human emisions were still low. Point out that the forcing effect of CO2 may not be linear due to indirect effects. [Hugh Lefcort, United States of America]	Rejected. Global warming gets outside natural variability much later than 1800.
16248	11	14	11	15	It will be much suitable for readers if we provide an examples explaining paleoclimate reconstruction in longer-term context. [Tabassam Raza, Philippines]	Rejected. The reference to Schurer et al. (2013) is such an example.
54796	11	14			the largest point of that paper and highlighting it might be worth it is that solar forcing is unlikely to be as large as in Shapiro. Ie the solar response in reconstructions is small. There is since a host of new reconstructions that have been analyzed also with detection and attribution, most recently a Neukom et al overview paper that is just accepted, which push the time horizon back further but detect all forcing only. Schurer by the way is from ca 1400 on [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Schurer et al. (2013) is used at this stage as an example for longer-term context, but extensive rewriting of the paragraph has occurred for the SOD, and the paper mentioned by the reviewer are now assessed.
30582	11	19	11	19	"also" with respect to what? [Annalisa Cherchi, Italy]	Noted. With respect to polar amplification of warming.
45256	11	19	11	19	Errors appear throughout the chapter. [Jianping Guo, China]	Editorial. This is a known issue reported by the TSU.
29150	11	19	11	19	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
49042	11	19	11	21	Poor English - suggest rephrasing this sentence [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. No changes made.
45012	11	24	11	25	It's unlikely that many PMIP4 experiments will be completed in time for the WG1 report. Meanwhile, there are many studies of temperature based on PMIP3 models and their comparison with paleo observations that have been published since AR5. Please include the significant results from available literature rather than expecting that major new findings from PMIP4 will be published in time. [Darrell Kaufman, United States of America]	Accepted. References have been added for the SOD. Some PMIP4 analysis was ready by the submission deadline.
29152	11	24	11	25	say here " will provide further opportunities for model-proxy evaluation over the Last Glacial…" [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text changed as suggested.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
53366	11	30	11	30	"Mean Annual Temperature (MAT)" is only used in figure 3.1, as far as I can see. [Jan Fuglestvedt, Norway]	Noted. The Figure has been completely reworked for the SOD.
30584	11	30	11	42	Fig 3.1 has no black dots. And what is the meaning of the red crosses clustered in two specific sector of the phase space represented? [Annalisa Cherchi, Italy]	Noted. The Figure has been completely reworked for the SOD.
37554	11	30			It would be best to avoid the acronym MAT for Mean Air Temperature over land, as MAT is already in use as an acronym for Marine Air Temperature. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The Figure has been completely reworked for the SOD.
37556	11	38			HadCRUT3 is by now a rather dated version of the HadCRUTx datasets. HadCRUT4 is widely used. HadCRUT5 is expected before the end of 2019, and is intended for use in the SOD of Chapter 2. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. But time was too short to repeat the analysis with the new dataset in time for the SOD.
45422	11	48		50	What is the source of the information of this entire sentence? [David Baguma, Uganda]	Editorial. The sentence provides the structure for what follows, and is merely stating facts.
52572	11	53	11	55	I would stress that there are common biases across models (e.g., in the upwelling regions). One substantial bias that is missing here is the North Atlantic cold bias which appears along the Gulf Stream – I would guess that it was also highlighted in AR5, it clearly appears in the CMIP5 MMM. Please add it, as it has substantial consequences for regional climate simulations. [Douglas Maraun, Austria]	Noted. The bias was not highlighted in AR5, but is now highlighted as it is also present in CMIP6 simulations.
51460	11	53	12	18	Figure 3.2 suggests a shift from a cold to a warm bias in many of the SH ocean and most of Central Africa. Is that shift too small to be noted in the main text? [Bart Van den Hurk, Netherlands]	Rejected. Yes, such small and local shifts are not important enough to be highlighted.
45424	11	56			The words "Latest generation" could be questionable after many years when this document is published. This is because after e.g., 5 or more years the word latest may not hold. [David Baguma, Uganda]	Editorial. It should be clear to readers that mentions of time are relative to the publication date of the report.
51452	12	1	44	17	Would be good to state how many CMIP6 models are available here; in the caption of figure 3.2 I conclude that 11 CMIP6 models have been assessed, correct? [Bart Van den Hurk, Netherlands]	Noted. The figure has been updated with more CMIP6 models, listed in the caption.
29154	12	2	12	2	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
38414	12	5	12	5	Why is it not clear whether the biases have decreased? If it is clear, I suggest that the wording „biases may have decreased“ is changed against „biases have decreased“. [Dirk Notz, Germany]	Rejected. We are waiting for more exhaustive comparisons to have more confidence in model improvements.
51848	12	8	12	12	I assume you mean to imply the cause is some combination of these factors and is model dependent and not to imply that all models suffer from this list of issues? This perhaps also applies to some areas of text more generally where synthesis has possibly lost a degree of required nuance? [Peter Thorne, Ireland]	Accepted. Rephrased to make clear the list are examples of biases.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
49166	12	12	12	12	There is already evidence from several models involved in HighResMIP that horizontal resolution significantly reduces many of these key longstanding biases, e.g. Roberts CD et al. (2018), Roberts MJ et al. (2019, submitted), Sidorenko et al. (2019, submitted), Haarsma et al. (in prep), as well as a multi-model overview (Caron et al., in prep). Roberts, C. D., Senan, R., Molteni, F., Boussetta, S., Mayer, M., and Keeley, S. P. E., 2018: Climate model configurations of the ECMWF Integrated Forecasting System (ECMWF-IFS cycle 43r1) for HighResMIP. Geosci. Model Dev., 11, 3681-3712, https://doi.org/10.5194/gmd-11-3681-2018. Roberts, M. J., Baker, A., Blockley, E. W., Calvert, D., Coward, A., Hewitt, H. T., Jackson, L. C., Kuhlbrodt, T., Mathiot, P., Roberts, C. D., Schiemann, R., Seddon, J., Vannière, B., and Vidale, P. L.: Description of the resolution hierarchy of the global coupled HadGEM3-GC3.1 model as used in CMIP6 HighResMIP experiments, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-148, in review, 2019. Sidorenko, D., and co-authors: Evaluation of FESOM2.0 coupled to ECHAM6.3: Pre-industrial and HighResMIP simulations. J. Adv. Model. Earth Syst., submitted. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Those references have been assessed for the SOD.
30586	12	16	12	18	You may just conclude that main biases remain. Actually from the figure it is hard to see improvements in upwelling regions, and it is difficult to conclude from the figure improvements over Himalaya (as one of the elevated region of the world) [Annalisa Cherchi, Italy]	Noted. The current text agrees with the reviewer.
6481	12	17	12	17	I do not know what "This assessment is currently made with low confidence" means. This type of language, used throughout the ARs, is very odd. If you do not have evidence of something then state as much. Do not make a strong statement and then take it back by saying you have low confidence. [Hugh Lefcort, United States of America]	Rejected. IPCC Assessment Report use calibrated language to evaluate likelihood confidence, see Mastrandrea et al. 2010
30588	12	23	12	30	In the figure the resolution is too low, labels in panels are not reported, it contains 4 panels but just 3 are described. What CMIP5 models are considered in the figure? [Annalisa Cherchi, Italy]	Accepted. Figure and caption have been improved as suggested for the SOD.
37558	12	28	12	29	ERA5 has been released, and a journal publication will shortly be submitted. ERA5 results are expected to replace some of the ERA-Interim results in the SOD of Chapter 2. If and when figures such as Fig. 3.2 are updated to include more results from CMIP6 models, it would be good also if ERA-Interim values could be replaced by ERA5 values. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Time was too short to upgrade to the more recent dataset.
8262	12	35	13	18	In Figure 3.3, CMIP6 simulation looked worse than CMIP5. Please also give the linear trends of temperature for observation and simulations by CMIP5 and CMIP6 ensembles, respectively. [Zong Ci Zhao, China]	Noted. The assessment in the SOD makes similar points. Linear temperature trends would be not be helpful for the discussion.
29156	12	38	12	42	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
16250	12	40	12	40	A simple clarification regarding why 1850-1900 is set as baseline instead of other suggested baselines such as 1720-1800 (Hawkins, E., 2017) (retrieved on Jun 18, 2019 from: https://www.climate-lab-book.ac.uk/2017/defining-pre-industrial/) should be included for concerned readers and decision makers. [Tabassam Raza, Philippines]	Rejected. The baseline is agreed across the report, and the reasons for the decision discussed in Chapter 1.
52574	12	40	12	41	I guess you are also not showing the absolute values because you don't want to show the substantial biases in the individual models. I am not sure whether this is a wise decision. In Chapter 11 we do show individual model absolute values at the regional scale. I think it would be fair showing a similar plot about global mean temperatures in Chapter 3. [Douglas Maraun, Austria]	Accepted. The fact that anomalies are less uncertain, from an observation point of view, that absolute temperatures is the main driver for that decision, but individual model absolute temperatures are shown in the new version of the figure.
56000	12	44	12	47	I do not think that CMIP5 models tend to overestimate or underestimate the temperature response to volcanic eruptions. I would rather think that even if a Pinatubo-like eruption would induce a cooling of approximately -0.5°C, the internal variability of the climate system can overhelm partially such a signal. This modulation is obvious at the regional scale, but play also a role a the global scale, in particular if such an eruption happens simultaneously with an el-Niño. This was the case right after the last three major eruptions (Agung, el Chichon, Pinatubo). This reason explains why we can not expect a GCM to reproduce exactly the surface temperature observed after volcanic eruptions (Swingedouw et al., 2017). In addition, a large number of studies suggest that volcanic eruptions tends to favor a chain of el-Niño - la Niña events (e.g. Khodri et al., 2017) and would favor positive NAO events (with an associated winter warming in some areas of the NH) during one to several winters (Zanchettin et al., 2013, Ménégoz et al., 2018). The large number of members required to detect such volcanically-induced dynamical signals in these sutdies demonstrates however their small signal-to-noise ratio. This is recently well demonstrated and explained in Zanchettin et al. (2019). References: Zanchettin, D., Timmreck, C., Bothe, O., Lorenz, S.J., Hegerl, G., Graf, H.F., Luterbacher, J. and Jungclaus, J.H., 2013. Delayed winter warming: A robust decadal response to strong tropical volcanic eruptions?. Geophysical Research Letters, 40(1), pp.204-209. Zanchettin, D., Timmreck, C., Toohey, M., Jungclaus, J.H., Bittner, M., Lorenz, S.J. and Rubino, A., 2019. Clarifying the Relative Role of Forcing Uncertainties and Initial-Condition Unknowns in Spreading the Climate Response to Volcanic Eruptions. Geophysical Research Letters, 46(3), pp.1602-1611. Ménégoz, M., Cassou, C., Swingedouw, D., Ruprich-Robert, Y., Bretonnière, P.A. and Doblas-Reyes, F., 2018. Role of the Atlantic	Noted. New literature has been assessed for the SOD, but the corresponding discussion kept short.
53340	12	48	13	2	This is important and may need to be elaborated. [Jan Fuglestvedt, Norway]	Noted. For the SOD, we marked on the corresponding figure the models that are tuned to observed warming.
30590	12	51	12	56	bad wording for ESM and physical climate models. Aerosols and land-use changes may be "interactive" also in non-ESM models [Annalisa Cherchi, Italy]	Accepted. The text has been reworded from ESMs to models.
51462	12	54	13	2	this is a matter of subjective assessment: a striking cool bias in the 1960s-1990s and more rapid warming after this period is suggested by CMIP6 compared to CMIP5 [Bart Van den Hurk, Netherlands]	Noted. The paragraph has evolved in a similar way as suggested by the reviewer for the SOD.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
14832	12	55	13	2	This will need major revisiting at SOD with CMIP6 DAMIP runs. Story likely to be more complex with potentially a larger range of model responses in simulating 20th century global temperature evolution. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Noted. A first look at DAMIP runs has been included.
30592	12	56	12	56	From fig 3.3 it seems that CMIP6 models tend to underestimate mean surface temperature changes. But this could be just preliminary as just few of them are considered. [Annalisa Cherchi, Italy]	Noted. Comparing CMIP5 and CMIP6 has been done, and Figure 3.3 is now more complete.
30594	13	6	13	14	quality of figure it too low [Annalisa Cherchi, Italy]	Editorial. Noted.
37560	13	12			In similar vein, if HadCRUT5 is indeed released in time for use in the SOD of Chapter 2, it should be considered as a replacement for HadCRUT4 in Fig. 3.3. The implication of a remark in the FOD of Chapter 2 is that HadCRUT5 will have improved (perhaps complete) spatial coverage. This would this reduce (or remove) the need for masking, which for HadCRUIT4 limits the representation of the contribution to the global average from enhanced warming of the Arctic. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Time was too short to upgrade to the more recent dataset.
30596	13	21	13	24	how is this sentence related to the rest of the paragraph? [Annalisa Cherchi, Italy]	Rejected. The sentence summarises the AR5 conclusion.
44064	13	29	13	31	Suggested modification: "Studies based on large ensembles of 20th and 21st century climate change as well as long pre-industrial control runs confirm that internal variability has a substantial influence on global warming trends over a few decades (Dai and Bloecker, 2018; Kay et al., 2015; Knutson et al, 2016) (FAQ 31 3.1)." Additional reference: Knutson, Thomas R., Rong Zhang, and Larry W Horowitz, 2016: Prospects for a prolonged slowdown in global warming in the early 21st century. Nature Communications, 7, 13676, DOI:DOI:10.1038/ncomms13676. [Thomas Knutson, United States of America]	Noted. The reference has been considered for the SOD.
54222	13	32	13	33	See this paper: https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-12-00548.1 [Nicola Maher, Germany]	Noted. That reference has been considered for the SOD.
6271	13	33	13	33	Energy is one of the main climate change drivers. Energy balance and energy consumption pattern is also needed to be considered in all modeling practices, in local, national and global levels (Jafari, M. and Smith, P., (2018). Climate Change as a Driving Force on Urban Energy Consumption Patterns. In Encyclopedia of Information Science and Technology (4th ed., pp. 7815-7830). IGI Global. https://doi.org/10.4018/978-1-5225-2255-3.ch680) [Mostafa Jafari, Iran]	Noted but out of topic of that particular section.
11504	13	36	13	41	This paragraph starts by using the slow warming aound the year 2000 as an example of decadal variability. It then discusses causes of variability on decadal time scales in general. This is confusing to the reader. I recommend rewriting the first sentence and note that one could just delete the first sentence. [Roanld Stouffer, United States of America]	Rejected. The first sentence does not give an example of natural variability, rather it explains why there is a renewed interest in decadal variability and points to the cross-chapter box on warming slowdown.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
14516	13	36	14	2	What did the authors want to say in this paragraph? They should tell the readers that the current models in particular CMIP6 simulations are able or unable to reproduce the multi-decadal variability, so that the readers could be confident with the results of the detection and attribution. The statement is also inconsistent with the corresponding point of the summary (Fourth Point on page 4 line 27-38). (CUG, Guoyu Ren) [Guoyu Ren, China]	Noted. The CMIP6 analysis is ongoing, which precludes stronger conclusions in this paragraph. And there is no inconsistency with the fourth point of the ES, which is only about the recent slowdown, not variability of temperatures in general.
26268	13	40	13	41	Please provide references that show that 'variability in these modes may be underestimated by CMIP5 models'. [Masahiro Watanabe, Japan]	Accepted. References are discussed in Section 3.7, and a clearer link with that section has been made in the SOD.
45426	13	41		43	In the current form using the word "coming" could imply that evidence walks. Improve the sentence [David Baguma, Uganda]	Editorial. "Come from" means "originate".
30598	13	42	13	42	what do you mean by "mixed"? [Annalisa Cherchi, Italy]	Editorial. Means that different studies reach different conclusions.
44638	13	43	13	43	"find" corrected to "found" [Liang Zhao, China]	Editorial. Change made.
29158	13	43	13	43	Papers by Laepple and Hybers 2014 (PNAS) and Hybers et al. show also important insights into the value of paleo-data for understanding of the full continuum of climate frequencies and point to deficiencies in CMIP 5 models. Those should be cited as well. [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Findings of Laepple and Huybers (2014) now added to the discussion.
44640	13	46	13	46	"report" corrected to "reported" [Liang Zhao, China]	Editorial. Change made.
44642	13	50	13	50	"report" corrected to "reported" [Liang Zhao, China]	Editorial. Changes made.
29160	13	54	13	54	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
49044	14	1			2 spelling mistakes: shown & generally [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Fixed.
27192	14	6	16	38	Among the 1°C of average temperature increase since the pre-industrial period, it is seen in Fig. 2.12 that about 0.6°C has been achieved between 1910 and 1945 when the emissions were much lower than nowadays. As a result, Ring, M.J., Lindner, D., Cross, E.F., Schlesinger, M.E., 2012 (Causes of the global warming observed since the 19th century. Atmos. Clim. Sci. 2, 401–415) consider that this increase was mainly natural. This is confirmed in Fig. 1 of FAQ 9.2 with only 15 % of human driver in the period 1900-1950. About +0.4°C since 1945 is far from the total earth warming, in contradiction with some sentences of the paragraphs. [François GERVAIS, France]	Rejected. The reviewer confuses internal variability with natural drivers of climate change. This confusion is admittedly not helped by the poor phrasing of the Ring et al. (2012) abstract. In short, internal variability causes variation around the mean, but no long-term warming or cooling. Natural drivers (sun irradiance, volcanoes) can cause longer-term warming or cooling, but cannot explain current warming. Fig 1 of FAQ9.2 is about sea level rise, not surface temperatures -- but the point is that most of anthropogenically-attributable warming is hidden by internal variability before the last 1980s.
30600	14	8	14	10	what about using large ensembles to measure internal variability? That would be more appropriate [Annalisa Cherchi, Italy]	Noted. That could be a valuable addition but the data is not available on main repositories.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
11508	14	15	16	38	This section seems very weak and narrowly focused. To make an assessment of the likelihood of the impact of human activites on the observed warming, one needs a discussion of the uncertainties in the estimates of the aerosol forcing and of the uptake of heat by the oceans. The aerosol part is only briefly mentioned and the oceanic heat uptake is missing. Both need included here with references to the other chapters discussing this point...or the discussion should be moved some place else. It seems incomplete here. [Roanld Stouffer, United States of America]	Rejected. This section is on attribution of total warming to human causes in general, where the main issues are internal variability and observational uncertainties. Errors in aerosol forcing and ocean heat uptake also matter but can be dealt with by attribution methods. This of course does not hold true for attribution to specific warming agents, which is discussed later.
52576	14	16	14	42	Again, I found this paragraph very technical but little useful. I would suggest to clearly state the underlying assumptions (as explained above). This would be of much more relevance than merely listing technical details. [Douglas Maraun, Austria]	Accepted. The paragraph has been rewritten to be less jargony.
52582	14	16	14	42	Please consider adding the new paper by Bellprat et al., Nature Communications, 10:1732, 2019. If I am not mistaken it has consequences for D&A, not only for event attribution. [Douglas Maraun, Austria]	Noted. The study has been considered for the SOD where it is used to frame the assessment of Hannart and Naveau and Haustein studies.
8556	14	16	14	42	Very jargony and hard to follow. [Robert Kopp, United States of America]	Accepted. The paragraph has been rewritten to be less jargony.
39098	14	16	14	42	Please this paragraph needed to be summarized [JACQUES ANDRE NDIONE, Senegal]	Accepted. The paragraph has been rewritten to be less jargony.
44644	14	16	15	18	The past tense and the present time in these paragraphs are mixed together, e.g Page 14 Line 20 : "propose" but Line 24 "built". I think we should use the past tense. [Liang Zhao, China]	Editorial. That paragraph has been heavily edited and tenses made consistent.
16018	14	16	15	18	Suggest including the study " A limited role for unforced internal variability in 20th century warming" (https://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-18-0555.1) in the discussion. [SAI MING LEE, China]	Accepted. The study is now assessed.
57834	14	16		42	the influence of human in the changing in climate variability between the 19th and 20th century has pose a great threat on the internal and surface temperature. However, the contribution of human and influence has invariably increased since the AR5 last draft to the first draftcof AR6 of IPCC. Thr global mean temperature in relation to the surface temperature and Air temperature (Atmospheric temperature) have impos changees both on the surface and Air temperature. [Abiodun Adegoke, Nigeria]	Noted. No changes made.
45428	14	19			The word "new attribution studies" could be questionable in future. This is because it may be difficult to know when literature seizes to be new [David Baguma, Uganda]	Editorial. It should be clear to readers that mentions of time are relative to the publication date of the report.
30602	14	21	14	21	not clear, please rephrase [Annalisa Cherchi, Italy]	Accepted. The paragraph has been rewritten to be less jargony.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
27950	14	26	14	28	Page 3-14, lines 26-28 give specific information about how Hannart (2016) got to their results. This information does not contribute much to the overall story and could be excluded. Page 3-15, lines 39-51: This paragraph does not say anything about the role for anthropogenic forcing in driving warming over Antarctica, while AR5 concluded that there was low confidence that anthropogenic influence has contributed to the observed warming averaged over the available stations. Furthermore, AR6 does not mention Antarctica in this context. We suggest that it should be included for completeness. [roderik van de wal, Netherlands]	Accepted. The paragraph has been rewritten to be less jargony. Antarctica has been added to Figure 3.6 and discussed there.
14834	14	28	14	42	This description of conclusion by Hannart and Naveau of “quasi-certainty” is an example of where the chapter needs to continue to improve its assessment aspects (not just a review). The impact of the Hannart and Naveau (2018) paper on the overall conclusions of this chapter needs to be assessed. Taken at face value, it could be argued, as the authors have done, that previous attribution assessments have been conservative. On the other hand, this conclusion of quasi-certainty potentially implies a strong degree of model reliability, particularly in estimating the deltaT between non-industrial (eg as estimated by “NATURAL” siumulations) and current global mean temperature values. The quasi certainty of Hannart and Neveau is conditional on a perfect (or near-perfect) model assumption, particularly in estimating the warming effects from natural factors not included (or even thought of) in current climate model simulations. The probability of this eventuality might well be rather low but still needs to be folded into the overall assessment. It would be good I think if the chapter came to a view on the Hannart and Neveau conclusion of “quasi certainty”. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The discussion has been firmed up in the SOD and confidence level has been downgraded back to extremely likely.
52580	14	31	14	32	This statement is in effect a zero (trivial and uninformative) statement, as it does not quantify anything (neither which region nor which fraction). [Douglas Maraun, Austria]	Accepted. The statement has been clarified.
52578	14	31			quasi certainty is not calibrated IPCC language [Douglas Maraun, Austria]	Noted, but it does not mean to be at this stage of the assessment.
51850	14	38	14	42	It would be worth adding a cross-link to the chapter 2 consideration of the issue particularly so given the close congruence on the assessed numbers of the impact here in the SOD. [Peter Thorne, Ireland]	Accepted. Reworded to make the link to Chapter 2, and lines 14.40-42 were a chapter 2 statement and have been deleted.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
37562	14	40	14	42	Several comments on this topic have been made with regard to the entire report, and to chapter 2. The FOD of Chapter 2 is incorrect in its reference to reanalyses in this regard. Reanalyses provide global SAT data, so can be used in direct model evaluation of air temperature, especially if the focus is on the past forty years when satellite data have been plentiful and reanalyses are of highest quality. All other things being equal, SAT rather than SST comparison is preferable, as it tests both a model's simulation of SST and its simulation of the near-surface atmospheric parameters that determine the relationship between SST and marine air temperature. Reanalyses do not generally assimilate marine air temperature observations, but they do assimilate information on marine surface winds, so their linkage between SST and marine air temperature does have an observational constraint. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The comment applies to Chapter 2.
30604	14	44	14	44	how do this statement combine with the paragraph before? [Annalisa Cherchi, Italy]	Accepted. Rephrased.
29162	14	47	14	50	the new paper by Haustein et al. 2019 in J. of Climate provides further evidence that early 20th warming was already anthropogenic forced [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Haustein et al. (2019), published since the SOD, has been added to the discussion, noting however that their method may underestimate the role of internal variability.
54798	14	47			True its shown in Hegerl et al 2018 but the analysis is based on Schurer et al 2018 [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The correct reference is cited for the statement.
45430	14	51		56	Confirm if this sentence is okay [David Baguma, Uganda]	Editorial. Yes, the sentence is correct.
30606	15	1	15	4	unclear, please rephrase [Annalisa Cherchi, Italy]	Editorial. The statement is clear in context of the paragraph. No changes made.
49048	15	4			additivity?? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. No changes made.
39100	15	6	15	18	The analysis looks fine; why not refering to AR5 [JACQUES ANDRE NDIONE, Senegal]	Rejected. That statement refers to Fig 3.5, noting that it is only preliminary but supports statements made previously by the AR5.
14454	15	7	15	7	'- - - based on the (Blindoff et al., 2013) assessment - - -' is suggested to be written as '- - -based on the assessment by Blindoff et al., (2013) - - -'. [Muhammad Mohsin Iqbal, Pakistan]	Editorial. Accepted.
11506	15	7	15	7	Change "warming" to "temperature changes". Volcano eruptions do not cause "warmings". [Roanld Stouffer, United States of America]	Accepted. The text has been changed as suggested.
52584	15	8	15	9	I don't believe the approach by Haustein properly accounts for internal variability as it is an Energy Balance model. Please check this carefully to avoid substantially misleading statements here. [Douglas Maraun, Austria]	Accepted. The SOD is tighter in its assessment.
52586	15	15			“using the methods...”: this is of little help. Please give some detail. [Douglas Maraun, Austria]	Accepted. Rewritten to "using their new attribution approach"
30608	15	23	15	34	not clear how does this looks for CMIP5 models. It should be explained/discussed [Annalisa Cherchi, Italy]	Noted. We now compare to CMIP5 in the SOD.
37564	15	23	15	34	See comment 151 about possible eventual use of HadCRUT5, which applies here also. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Time was too short to upgrade to the more recent dataset.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
42978	15	23	15	34	Two observational series available from 1850 now have extended coverage beyond HadCRUT4. Berkeley Earth should be used for masking, and averaged with Cowtan and Way to produce a more appropriate observational average. Berkeley Earth should also be used to mask blended model series, following Cowtan et al (2015) method. HadCRUT5 could also be used in SOD to produce a common mask and average if (as expected) it is also interpolated. [David Clarke, Canada]	Noted. A decision on observational datasets to include followed broadly the suggestion of the reviewer.
16254	15	43	15	44	The statement in the lines mentioned is confusing due to the reason that scientific studies either use accuracy (quantitative) or the study should be descriptive (qualitative). Thus, such kind of statement create ambiguity and got my reservation on that matter. Either remove this statement or provide a clear statement to justify the proper observation of the forcing vis-a-vis response. [Tabassam Raza, Philippines]	Accepted. Statement rephrased for clarity.
45432	15	44		48	New litearture?? [David Baguma, Uganda]	Editorial. The sentence specifies that "new literature" means "since the AR5".
29164	15	48	15	48	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
49046	15	48	15	49	This sentence does not make sense. [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. No changes made.
51466	15	50	16	17	"too incomplete" means that the natural only simulations are missing? [Bart Van den Hurk, Netherlands]	Rejected. Means that the CMIP6 database is still being built, with models, simulations, ensemble members, etc. still missing.
9602	15	55	16	6	Are the models also showing the blended air/sea temperatures as HadCRUT4 does? I think it is important to emphasize in this section that output from the models needs to be processed in the same way as observations for the comparison to be robust and like for like. Also, the baseline period for these calculations in Figure 3.6 should be specified in the figure caption. [Katarzyna (Kasia) Tokarska, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The caption now makes clear that the models using blended GMST/GSAT.
30610	15	56	16	6	Fig 3.6 indicates that the warming with all forcings in CMIP6 is less than in CMIP5, and it tends to overlap with CMIP5 natural forcings. Let's see what happen when including CMIP6 natural forcings. Likely to eventually discuss if the forcings are largely different (in ch 6?). [Annalisa Cherchi, Italy]	Noted. The CMIP5/CMIP6 comparison was very preliminary and has improved now that more models have been added.
37566	15	56	16	6	Comments 151 and 153 apply here also. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Time was too short to upgrade to the more recent dataset.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
14836	16	6	16	18	The chapter will need to assess the relative roles of optimal detection on CMIP6 (CMIP5) simulations, simple models like that used in chapter 7 and that of Haustein and Otto (not cited in FOD; apparently showing almost no role for internal variability), and the Hannart and Naveau quasi-certainty conclusion. It seems to me these conclusions all need to be taken into account but no one type of analysis provides the silver bullet result trumping all the others. For example, the Haustein and Otto approach is potentially subject to over-fitting to forcings thereby under-estimating the role of internal variability. And the Haustein and Otto result has implications for model verification – implying perhaps that models have too much internal variability if correct - else overfitting can lead to over-confidence if not. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The SOD considers several lines of evidence, and the implications of the studies cited.
54992	16	19	16	20	Noticed examples that require proof-reading are not commented while it may be useful to share the need to correct "ocean" in this line. [Kilkis Siir, Turkey]	Editorial. That was placeholder text that has been replaced.
45434	16	19		20	Confirm if the tense is fine for readers of the document. [David Baguma, Uganda]	Editorial. Yes, this is a placeholder that has been replaced.
49050	16	20			Spelling mistake - anomalies [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Fixed.
14838	16	22	16	38	This is where the synthesis is carried out and the next iteration in the SOD will be able to include Haustein and Otto as well as CMIP6 analyses. A key point here for me is assessing not just whether P > 99% but whether P is greater than a value even closer to 100% (or 1-P/100 much smaller). This underpins the eventual attribution assessment and whether even “virtually certain” is being conservative. Personally I don’t believe it is because of the uncertainties inherent in a model based analysis (as attribution inevitably is) which preclude (in my opinion) an unequivocal attribution statement. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The SOD considers several lines of evidence, and the implications of the studies cited. This work was part of the motivation to downgrade to extremely likely.
14840	16	22	16	38	Also it will be necessary, I believe for this assessment here to fold in the information on attribution of temperature trends from chapter 7 (and for chapter 7 to delete its attribution assessment or else couch these numbers in suitable provisional terms subject to the overall synthesis carried out here in chapter 3). It is important in my view for the IPCC to keep separate an appreciation that GHGs cause and have caused warming (unequivocally) from an attribution assessment that considers whether that GHG warming has been the dominant cause of the observed warming (in my personal view this is not unequivocal but this would need to be carefully assessed in the light of all the evidence including the information on model fidelity and the Hannart and Naveau paper). [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The findings from Chapter 7, which builds surface temperature warming from the bottom up, are mentioned in the Executive Summary of Chapter 3. It is a strong result that the two approaches (bottom up and top down) agree reasonably well on attributable warming.
45436	16	22			The document is likely to be read in future. The use of the word "new literature' may not be appropriate in future after many years [David Baguma, Uganda]	Editorial. It should be clear to readers that mentions of time are relative to the publication date of the report.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
6479	16	23	16	23	Going from 95 to 99% certainty is welcome but the fact that we can still only attribute more than half of the warming to anthropogenic causes is worrying. This needs to be discussed. [Hugh Lefcort, United States of America]	Accepted. Statistically, an increase in certainty necessarily requires a decrease in the amount of warming that can be attributed to human activities. But we agree that describing only part of the probability function is not the best way to present the finding. Instead, the follow the changes made to the Executive Summary and now say "It is extremely likely that human influence is the main driver of the observed increase in global-mean surface air temperature in 2009-2018 relative to 1850-1900."
49052	16	25	16	28	This sentence is difficult to follow and should be rephrased [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Rephrased slightly.
43348	16	27	16	27	Replace "simply P > 99%" with "found with probabilities above 99%" [James Renwick, New Zealand]	Editorial. Changed to "imply probabilities over 99%"
14456	16	27	16	27	'- - - also simply P>99% that - - -' is not clear. If it is probability level, it must be placed at proper context in the sentence. [Muhammad Mohsin Iqbal, Pakistan]	Editorial. Rejected. There is different confidence level for these two sentences.
29166	16	27	16	27	change "simply" to imply [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Changed as suggested.
14518	16	31	16	35	"Although there is mixed evidence that models underestimate internal variability, there is no evidence for the severe underestimate that would be needed to challenge the conclusions of the attribution studies assessed in this section." The significant underestimate of the multi-decadal variability by the models is a fact, which we have to face up to, I think. This would not fully support the confidence "virtually certain (P ≥ 99%)" given to the following attribution conclusion. I would prefer to "very likely" rather than "virtually certain". Overall, the better understanding of the warming slowdown during the last two decades and the current disability of models to reproduce the natural multi-decadal variability seem not support a stronger attribution assessment. (CUG, Guoyu Ren) [Guoyu Ren, China]	Accepted. The assessment made by this section implied that models do not miss important sources of internal variability. The paragraph has been heavily edited.
14440	16	34	16	34	The first word 'underestimate' is suggested to be written as 'underestimation'. [Muhammad Mohsin Iqbal, Pakistan]	Rejected. "underestimate" is also a noun.
49054	16	43	16	48	This paragraph is unclear and poorly expressed [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account.
45438	16	43		43	The place where this reference i.e., Bindoff et al., 2013 is placed may be questionable. [David Baguma, Uganda]	Editorial. Accepted.
30612	16	47	16	48	what do you mean? And how is it relevant here? [Annalisa Cherchi, Italy]	Editorial. This introduces the rest of the section, but rephrased for clarity.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
51854	16	50	17	25	In Chapter 2 we are using GNSS-RO which has the advantage of vertical resolution combined with spatial sampling but is only available since 2000. We are also using ERA reanalysis. The studies discussed here make use of radiosonde and MSU products. That mismatch may or may not be important but likely should be noted. It would be worth checking whether any of the RO groups have done such studies. I believe tht Uni. Graz may be in process of writing such a study presently. [Peter Thorne, Ireland]	Taken into account.
16256	16	50	17	26	Although the overestimation of the observed warming was sufficiently explained, the significance of this overestimation was not mentioned appropriately. There is ample discussion about the model. However, there is a lack of significance of the findings of the model, on how it will be used for decision making; or what kind of mitigation should be promoted considering those findings. [Tabassam Raza, Philippines]	Taken into account. Implications are discussed in the SOD.
39102	16	51	16	51	The reference "(Flato et al, 2013)" should be put at the end of teh sentence. [JACQUES ANDRE NDIONE, Senegal]	Editorial. Accepted.
29168	16	55	16	55	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
49056	16	56			Unclear - should it be "models suggesting more substantial warming than observations." [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Accepted.
28448	17	3	17	3	suggest -> suggested [HIDEO SHIOGAMA, Japan]	Editorial. Accepted.
30614	17	9	17	9	"of 2000" should be "2000" [Annalisa Cherchi, Italy]	Accepted.
49058	17	9			grammar - the year 2000 [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Accepted.
14458	17	16	17	16	'- - - that CMIP5 models warmed more than - - -' can better be written as '- - - that CMIP5 models indicated more warming than - - '. [Muhammad Mohsin Iqbal, Pakistan]	Accepted. Rephrased as suggested.
14842	17	16	17	26	I find the framing here a bit problematical. A discrepancy between models and reality is something to assess AFTER you've taken account of the possible effects of observational error and internal variability. So I don't see internal variability and observational error as contributing to any discrepancy (although uncertainties in these factors can contribute to the confidence as to whether or not there is a discrepancy). [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence rewritten to reframe as suggested.
45440	17	16			On the use of the word "New studies" see the comment on page 3-16, line 22 ie., The document is likely to be read in future. The use of the word "new studies' may not be appropriate in future after many years [David Baguma, Uganda]	Editorial. Accepted.
45442	17	20			The words "recent work" . see the comment on page 3-17, line 16 [David Baguma, Uganda]	Editorial. Accepted.
45444	17	22		23	The use of the word "we" could be questionable. Rephrase the sentence. Also, the word "they" could be interpreted as weak information for internal readers. [David Baguma, Uganda]	Editorial. Taken into account.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
14460	17	23	17	23	The sentence ending with 'satellite data.' is suggested to be joined with the next sentence by deleting 'We assess' and adding 'and'. [Muhammad Mohsin Iqbal, Pakistan]	Rejected. Text is clearer as is.
26270	17	24	17	24	Please specify if the overestimation of the SST trend is about the global mean value or the pattern. [Masahiro Watanabe, Japan]	Accepted
30616	17	25	17	26	where is a figure/reference showing this? [Annalisa Cherchi, Italy]	Taken into account. The sentence has been deleted.
30618	17	31	17	34	what do dashed line represent in fig 3.8? [Annalisa Cherchi, Italy]	Noted. Dashed lines indicated ultra-high horizontal resolution models, but the figure has been changed in the SOD.
39104	17	39	17	39	The reference "(Bindoff et al, 2013)" should be put at the end of teh sentence. [JACQUES ANDRE NDIONE, Senegal]	Editorial. Accepted.
51858	17	39	18	4	Is it really the case that only Santer has published on this subject since AR5? If not cite others. Lott et al may have been post-AR5 literature cut off? If there is other relevant literature is there a risk of accusations of over-reliance? May this reliance on papers arising from a single lead author cause challenges to the increased likelihood assessment conclusion here? [Peter Thorne, Ireland]	Taken into account. We contacted Fraser Lott but he hasn't published any relevant paper since 2013.
51468	17	41	43	45	I read this statement as a notion that the MSU dataset of TMT is very different from earlier data sets of this quantity. I don't read a notion that this MSU is warmer or colder, or more or less reliable. In other words, this very pronounced difference between MSU and earlier datasets does not support the "extremely likely" statement about antropogenic warming at the end of this paragraph. I don't think I read the intended message very well, but I wonder what this intended message is [Bart Van den Hurk, Netherlands]	Taken into account.
27952	17	44	17	45	Page 3-17, lines 44-45 give the information that five standard deviations is the standard required in particle physics for discovery of a new particle. This information does not contribute to the section as any reader that understands this statement should be familiar with the fact that five to eight standard deviations is a lot. Page 3-18, lines 42-44 mention that temperature change in response to volcanic forcing is larger than previous studies, but page 3-18 lines 32-33 mention that upper stratospheric temperature changes were not assessed in the context of attribution or model evaluation in AR5. So apparently the results from these previous studies were not robust enough to be mentioned in AR5. We therefore suggest to not make the comparison with previous studies in lines 42-44. If these previous studies were conducted after AR5, then they should be mentioned. Page 3-18 lines 40-42: It is not clear whether the 2-3 degrees cooling is with respect to pre-industrial or a difference within the time period 1979-2005. If it is within the time period, it should be mentioned how internal variability is dealt with. If it is with respect to pre-industrial, it should mention this. [roderik van de wal, Netherlands]	First one, accepted. Second one, accepted. Third one, partially accepted.
16258	17	44	17	45	The significance of the statement seems to be stand-alone, there is need for transition for the upcoming content. [Tabassam Raza, Philippines]	Accepted.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
30620	17	44	17	45	so how is relevant sentence about what noted by santer et al 2019? [Annalisa Cherchi, Italy]	Taken into account. That statement has been deleted.
28906	17	44	17	45	I would definitely remove the comment about 5 sigma. It sounds like pub talk. Particle physics is quite different because of quantum (ie probabilistic) indeterminism. The significance of observed temperature changes should not be assessed in a throwaway conversational line. [Matt Tully, Australia]	Accepted.
28908	17	47	17	52	This discussion about the seasonal cycle seems randomly inserted and out-of-place, it doesn't connect to anything before or after. [Matt Tully, Australia]	Accepted.
45446	17	47			The use of the word “recently” could be questionable. Rephrase. [David Baguma, Uganda]	Editorial. Accepted.
37568	17	48			"observationally-based datasets" is perhaps better than "observed datasets". The datasets in question are sets of observed data, perhaps with some processing such as gridding. It is not the datasets that are observed. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Accepted.
14844	18	3	18	4	I wonder whether it is worth re-considering assessments that don’t include some additional information about the significance or substantial nature of the attributable component. The difficult with statements such as “extremely likely .. contributed to” is that this contribution could be very small in the context that we know there is a very strong (potentially unequivocal) expectation that GHGs contributed some warming (so the statement is not provide any additional attribution information). The question rather is whether that warming was significant (ie detectable or “discernible”) and/or substantial (eg more than half or dominant etc). I realise this "contributed to" formulation was one used in AR5 but it occurs to me whether this is something that might be worth revisiting this time, given the key policy question is not whether GHGs caused some warming but whether that warming is significant and substantial. A reconsideration would also aid the framing of the overall assessment - with an appreciation that warming (both observed and ghg caused) can be concluded upon unequivocally, whereas an attribution of a significant (ie detectable) or substantial (as defined quantitatively in some way) component is going to be concluded upon with some likelihood (potentially up to virtually certain level as is currently concluded for global mean surface temperatures) which takes account not just of observational and model errors but the structural uncertainies associated with known and unknown unknowns. [Peter Stott, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. If we give a quantitative evaluation about warming contribution, the confidence level may need to be reduced.
45448	18	3			The word “we” is inappropriate. Rephrase the senetence. [David Baguma, Uganda]	Editorial. Accepted.
49060	18	7	17	8	grammar - were generally realistic [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Accepted.
39106	18	7	18	30	What about the added value of CMIP6? [JACQUES ANDRE NDIONE, Senegal]	Noted. This is now discussed.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
45450	18	25			The authors need to be consistent with the tense used. [David Baguma, Uganda]	Editorial. Accepted.
27260	18	27	18	28	It would be good to clarify here how ODS affect stratospheric temperatures, i.e. primarily via their chemical effect on ozone (which then affects heating) [Gabriel Chiodo, Switzerland]	Accepted.
52588	18	27			write out ozone depleting substances [Douglas Maraun, Austria]	Taken into account.
37570	18	32	18	42	It could be added here that it has been shown (Simmons et al. (2014; doi: 10.1002/qj.2317; apologies for self-citation) that reanalyses indicate an upper-stratospheric cooling from 1979 to 2009 of about 3C at 5hPa and 4C at 1hPa that agrees well with the cooling in AMIP-type simulations using CMIP5 forcings.The cooling in the reanalyses results from the assimilation of SSU and AMSU data. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
48436	18	35	18	35	Maycock et al 2018b should be Karpechko & Maycock et al 2018 [Julie Arblaster, Australia]	Accepted. The reference has been corrected.
49062	18	38	18	52	Repetition of findings of Mitchell (20160 [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This reference was shown here for the first time.
27262	18	48	18	48	It would be good to clarify what is meant here by "step-like" changes [Gabriel Chiodo, Switzerland]	Accepted.
28794	18	50	19	10	Add upper strospheric to summary of section and consider confidence statement and place in ES? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account.
43176	18	53	19	1	Chemistry-climate model results further show that the relatively rapid decreases in global upper stratospheric 53 temperatures in the early 1980s and early 1990s are likely to be due to the combined influence of 54 temperature decreases after the warming from major tropical volcanic eruptions and the declining phase of 55 Document for Expert Review 99d40ad1 First Order Draft Chapter 3 IPCC AR6 WGI Do Not Cite, Quote or Distribute 3-19 Total pages: 163 For a better learn of the readers could be mention the major tropical volcanic eruptions and the principal gases on tons of contamination [Sebastian Naranjo Silva, Ecuador]	Taken into account.
50966	19	1	19	5	Lehmann et al. 2015 Clim Ch. Show that the increase of record rainfall can only be explained with recent global temperature increase. [Kai Kornhuber, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference has been considered for the SOD. (Note that the comment is in fact about Section 3.3.2)
16260	19	3	19	8	This area needs more explanation to justify why the lower stratosphere has cooled rather than warmed as a result of anthropogenic forcing. [Tabassam Raza, Philippines]	Taken into account.
39108	19	3	19	8	We'll be happy to know more about CMIP6 data regarding this issue. [JACQUES ANDRE NDIONE, Senegal]	Noted.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
46702	19	13	23	14	Moonsoon is assessed in section 3.3.3.2; Section 4.4.1.4, 4.5.1.5; 8.2.1.3, 8.3.1.3.2, 8.3.2.2, 8.3.2.4 , 8.4.2.3, 8.3.2.1.1 , 8.4.2.7, 8.5.1.1.2; Section 9.5.4.7; Section 10.4.2.2.1, 10.4.2.2.2, 10.4.3.2.1, 10.4.3.2.2, 10.6.3; Section 11.1.5, 11.4.1, 11.4.4, 11.4.5, 11.7.1, 11.9.5, 11.10.2, Cross-Chapter-box-11.1.1, Section 12.4.1.3, 12.4.2.3, 12.4.2.4, 12.4.2.6, Cross-chapter box 12.1; Atlas.2.2, Atlas.2.3, Atlas.5.2.2, Atlas.5.3.1, Atlas.5.3.1, Atlas.5.3.1, Atlas.5.3.2, Atlas.5.3.3, Atlas.5.3.3, Atlas.5.5.1, Atlas.5.5.2.2, Atlas.5.11.1.3, in the form of ES in chapter 3,4,8,11, box in chapter 8 and above-mentioned subsections [WGI TSU, France]	Noted.
45014	19	13			Please start the section on “paleoclimate context” with a statement of purpose, otherwise, it’s not clear why the information in this paragraph is not presented along with the simulations and modern observations of the same climate variables. In my view, it should be, but I might have overlooked the reason to separate the paleo content. [Darrell Kaufman, United States of America]	Accepted, text has been revised to make this clear from the start of the section.
45016	19	13			I did not check in detail, but some of the information about drought might overlap with information in CH8. Chapters 2-4 are focused on “large scale” climate, so the sub-continental-scale information probably belongs in CH8. [Darrell Kaufman, United States of America]	Noted, however hydroclimate changes are always going to be regional. We were asked to provide some broad context for Chapter 3 so focused on the most robust findings. Ch. 8 goes into much more detail - we know reference Ch. 8 where appropriate.
52596	19	15	19	51	I don't quite get the structure of this section. Could this be briefly motivated in the beginning, and could (maybe) a synthesis be added? [Douglas Maraun, Austria]	Accepted, text has been revised to make the paleoclimate relevance clear from the beginning
45452	19	15			Why do authors begin a major sentence with weak words like "since"?, which is even used with time. [David Baguma, Uganda]	Not Applicable, text has been removed.
6483	19	17	19	17	Since it earlier states that cooling occurred during most of the CE until 1800, why state that recent warming in the Levant is "unprecedented in the last millennium"? If there was an earlier period of cooling that stopped in 1800, then wouldn't it be expected that any rise would be "unprecedented in the last millennium"? Your statement makes a predicted phenomenon sound dramatic. [Hugh Lefcort, United States of America]	Rejected. The text here concerns drought, not temperature, and the statement that "recent prolonged dry spells in the Levant and Mongolia are unprecedented in the last millennium" is supported by the cited literature.
35278	19	17	19	17	It should say: oxygen or hydrogen stable isotopes [eugenia gayo, Chile]	Not Applicable, text has been removed.
45454	19	18			These recorders...Which? improve the sentence! [David Baguma, Uganda]	Not Applicable, text has been removed.
52590	19	22			Give reason (I guess it is just the expected Hadley cell expansion). [Douglas Maraun, Austria]	Accepted, text modified to clarify it is due to increased evaporative demand.
16262	19	23	19	26	There is need to clarify and provide at least one example on what historical observations can be attributed to anthropogenic forcing. [Tabassam Raza, Philippines]	Rejected. This sentence concerns paleoclimate data, not historical data, and simply sets the stage for the sentences that follow.
16264	19	23	19	34	Most of the people reading about this chapter may not know how dry spells can be calculated using tree ring method. It will be better if there is a small explanation on the process. [Tabassam Raza, Philippines]	Taken into account. Full explanation of tree ring paleoclimatology is outside the scope of the assessment; however the sentence has been modified to specify, "Records of tree ring width".

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
35280	19	23	36		Garreaud et al. 2017 in Hydrol. Earth Syst. Sci. also present a centennial context for the Mega-drought in Chile by using tree-rings, and how it could be related to anthropogenic warming. [eugenia gayo, Chile]	Accepted. The Chilean tree ring record is now mentioned here.
52592	19	27	19	30	You use “unusual” twice. This is very unspecific. Be more precise here (could one use IPCC calibrated language?). [Douglas Maraun, Austria]	Taken into account - the first unusual has been replaced, the second kept since this is the best description of the literature findings.
32114	19	32	19	34	which regions are affected by these megadroughts? Are they simultaneous in all the regions? [Marie-France Loutre, Switzerland]	Accepted. Wording has been changed to specify that sentence refers to western North America, and not the Mediterranean where the recent drying can be attributed to human activities.
31932	19	32	19	34	this sentence is not specific enough and could be understood inmany ways. Moreover, it seems contradictory with the previous sentence. East Africa has been drying over the recent decades. Is it a megadrought? Is the prolonged dry spell in the Levant and Mongolia a megadrought? In these latter case drought is attributed (at least in part) to human activities, while megadrought cannot be attributed to human activities. Please clarify. [Marie-France Loutre, Switzerland]	Accepted. Wording has been changed to specify that sentence refers to western North America, and not the Mediterranean where the recent drying can be attributed to human activities.
52594	19	35	19	36	As the statement are “likely” and issued with “medium confidence”: please replace “demonstrate” with “indicate” and “can occur” with “may occur”. [Douglas Maraun, Austria]	Accepted, fixed
45456	19	35			Confirm the style of referencing [David Baguma, Uganda]	Rejected, style of referencing is automatically set report-wide
7382	19	38		39	substantiate with literature [Chukwuma Anoruo, Nigeria]	Rejected. This sentence reiterates the AR5 assessment, so no literature review is needed.
35282	19	46	19	51	At least for me, it seems ilogical not mention Ruddiman´s discussion on prehistorical emissions of well-mixed gases by about 8 ka. [eugenia gayo, Chile]	Rejected. This sentence refers to older time periods with substantially higher CO2.
27722	19	48	19	48	replace with published article (Tierney et al) [Poot Delgado Carlos Antonio, Mexico]	Accepted. Citation has been updated.
39110	19	48	19	48	The year is missing in the reference Tierney et al [JACQUES ANDRE NDIONE, Senegal]	Accepted, fixed
33318	19				There is a vast paleoclimate record available concerning past precipitation; suggest expanding that portion of this section. [Erika Wise, United States of America]	Noted. That section has been extensively rewritten for the SOD. (Note that the comment is in fact about Section 3.3.2)

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
33330	19				There have been paleoclimate reconstructions of humidity that could be considered for inclusion in Section 3.3.2, including: Liu, Y., Wang, Y., Li, Q., Song, H., Zhang, Y., Yuan, Z., & Wang, Z. (2015). A tree-ring-based June-September mean relative humidity reconstruction since 1837 from the Yiwulü Mountain region, China. International Journal of Climatology, 35(7), 1301–1308. https://doi.org/10.1002/joc.4057 Liu, N., Liu, Y., Bao, G., Bao, M., Wang, Y., Ge, Y., … Tian, H. (2015). A tree-ring based reconstruction of summer relative humidity variability in eastern Mongolian Plateau and its associations with the Pacific and Indian Oceans. Palaeogeography, Palaeoclimatology, Palaeoecology, 438, 113–123. https://doi.org/10.1016/j.palaeo.2015.08.003 [Erika Wise, United States of America]	Noted. The references have been considered for the SOD. Note that the comment is in fact about Section 3.3.2)
52598	20	4			what are “broad-scale” features? Please replace by large-scale if this is meant. [Douglas Maraun, Austria]	Editorial. Changed to “large-scale”.
52600	20	5			This is again a completely useless statement as it does not carry any information. What is “modest agreement”? Is this good? Bad? [Douglas Maraun, Austria]	Rejected. We use the same wording as it is in the summary paragraph of section 9.4.1.1 of AR5.
54430	20	7	20	8	It is necessary to point out that CMIP5 models failed to reproduce the zonal mean trends of seasonality as well as the linear trends of annual precipitation and seasonality. [Reynold Stone, Trinidad and Tobago]	Accepted. A new sentence with that statement is now included.
52602	20	7	20	23	You should link to Chapter 10 Section 3 here at least once. We capture regional performance in terms of reproducing surface variables and also capture the performance in simulating phenomena of relevance for regional climate. [Douglas Maraun, Austria]	Editorial. Included link to chapters 8 and 10.
29170	20	8	20	8	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted
16266	20	8	20	9	"Some extent" is a loose terminology in this kind of scientific study, I think there should be a proper range on the observed zonal mean precipitation trends. [Tabassam Raza, Philippines]	Accepted – text revised as: “Since AR5..., as well as reproduce qualitatively the observed zonal mean land precipitation trends: models capture the drying trends in the tropics and along 45S, as well as the wetting trend in the NH mid to high latitudes, but the amplitude of the changes are much smaller.”
47184	20	12	20	15	This sentence refers to studies about recent precipitation trends rather than biases. This may need a clarification or a later discussion. [Hervé Douville, France]	Accepted. Clarified.
29172	20	19	20	19	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
49168	20	23	20	23	The statement with the Vanniere et al. (2018) citation seems somewhat dismissive. It shows an extremely consistent reduction in many of the key, longstanding precipitation biases across a range of CMIP5/CMIP6 era models from enhanced horizontal resolution, including the tropical Atlantic and the double ITCZ in the Pacific. As in the comment above, there are more papers coming from HighResMIP in addition to the Vanniere et al 2018 study that will increase the robustness of this result. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account.The discussion has been improved in SOD.
37572	20	33			Our monitoring of ERA5 production at ECMWF has indicated that ERA5 provides a distinct improvement over ERA-Interim in its representation of precipitation, so there is a case for updating Fig. 3.9 to use ERA5 not ERA-Interim. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Figure 3.9 does not use ERA-Interim, but GPCP. Maintained use of GPCP in SOD.
52604	20	40			Again, I fdon't get the structure here. Why paleo again? Wouldn't it make sense to move this up? [Douglas Maraun, Austria]	Taken into account - this section is actually being moved to 3.8
40484	20	42	20	43	Are the underestimated rainfall changes during these periods specific to particular regional monsoons (i.e. WAM/Sahel) or can they be generalised to the hemispheric or global monsoon? [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable, text has been removed.
30622	20	43	20	43	not clear how regions are represented in fig 3.10 [Annalisa Cherchi, Italy]	Figure 3.10 was replaced with an evaluation of model (CMIP5 and CMIP6) representation of mid-Holocene precipitation in several regions.
29174	20	43	20	43	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted
27724	20	48	20	49	delete comma [Poot Delgado Carlos Antonio, Mexico]	Accepted, fixed
27726	20	52	20	53	delete comma [Poot Delgado Carlos Antonio, Mexico]	Accepted, fixed
39292	20	52	21	5	Some key work should be included here in citation with respect to model-proxy agreement at the continental scale. This includes Lora (2018, J. Climate), Ibarra et al. (2018, Geology), Scheff et al. (2017, J. of Climate) and other work by Juan Lora and Tripti Bhattacharya. [Daniel Ibarra, United States of America]	Accepted, some references were added.
45458	21	25		29	The sentence is too long. There are four words in pat tense. Perhaps, improve the communication by spliting in short sentences [David Baguma, Uganda]	Editorial. Sentence split.
30624	21	27	21	28	what period/time was affected? Not clear how it is related with sentence just before [Annalisa Cherchi, Italy]	Accepted. Moved the sentence to another paragraph.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
44058	21	29	21	29	Suggest to insert here: "A gridpoint based analysis of precipitation trends over land regions since 1901 (Knutson and Zeng, 2018) comparing observed and model simulated trends finds that detectable anthropogenic increasing trends have occurred prominently over many middle to high latitude regions of both hemispheres. The observed trends in many cases are significantly stronger than modeled in the CMIP5 historical runs for the 1901-2010 period (though not for 1951-2010), suggesting possible deficiencies in models with capturing past forced trend behavior over the past century in precipitation trends. " Reference: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, https://doi.org/10.1175/JCLI-D-17-0672.1 (see fig. 3c). [Thomas Knutson, United States of America]	Accepted.
49064	21	29	21	31	is positive tendency the same as increase - unclear [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Yes, it is the same in this case.
52606	21	29			references are missing (“new results”) [Douglas Maraun, Austria]	Taken into account. Sentence modified and references to figure.
29176	21	33	21	33	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted
39506	21	52	21	55	On the other hand, Vera and Diaz (2015) describe significant positive trends in summer rainfall over southeastern South America using observations and CMIP5 Historical simulations, being the latter statistically significant from the negligible trends obtained for the natural-forcing-only experiments. Vera, C., L. Díaz, 2015: Anthropogenic influence on summer precipitation trends over South America in CMIP5 models. Int. Jou. of Climatol. 35, 3172-3177 DOI: 10.1002/joc.4153. [Carolina Vera, Argentina]	Accepted. Reference is now included.
54800	21	52	22	3	in the ES I got the impression there are new papers attributing changes in high latitude rainfall to forcing (like in Zhang et al 2013 - might be worth linking to this not sure that was in AR5 - but this is about the SH maybe check ES wording? (maybe I misunderstood) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. ES bullet was rewritten.
45460	21	52		55	The sentence is too long. There are four words in pat tense. Perhaps, improve the communication by spliting in short sentences. [David Baguma, Uganda]	Editorial. Sentence split.
45462	21	57			These…..who are these? [David Baguma, Uganda]	Accepted. Clarified.
52608	22	1	22	3	We have a whole paragraph dealing with the simulation of fronts. I would suggest to move the whole sentence here to Chapter 10. [Douglas Maraun, Austria]	Rejected. Ch 10 considers this topic in detail. Here is only introduced to give context to changes. Otherwise the reader cannot interpret results. We include a link to chapter 10.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
28796	22	1	22	34	This section could consider citing the range of PDRMIP studies that improve understanding considerably on reasons for precip change compared to CMIOP5, even if I say so myself :) Seach PDRMIP publications in google, but examples are below. I think it is clear that humans have influenced precip, especially intenstity, and when you take the wider literture into account and theory. As it stands this somewhat contradicts chapter 8. Hodnebrog, Ø., Myhre, G., Forster, P. M., Sillmann, J., and Samset, B. H.: Local biomass burning is a dominant cause of the observed precipitation reduction in southern Africa, Nat. Commun., 7, 11236, https://doi.org/10.1038/ncomms11236, 2016. Drivers of Precipitation Change: An Energetic Understanding T. B. Richardson, P. M. Forster, T. Andrews, O. Boucher, G. Faluvegi, D. Fläschner, Ø. Hodnebrog, M. Kasoar, A. Kirkevåg, J.-F. Lamarque, G. Myhre, D. Olivié, B. H. Samset, D. Shawki, D. Shindell, T. Takemura, and A. Voulgarakis Journal of Climate, December 2018, https://doi.org/10.1175/JCLI-D-17-0240.1 Quantifying the Importance of Rapid Adjustments for Global Precipitation Changes G. Myhre, R. J. Kramer, C. J. Smith, Ø. Hodnebrog, P. Forster, B. J. Soden, B. H. Samset, C. W. Stjern, T. Andrews, O. Boucher, G. Faluvegi, D. Fläschner, M. Kasoar, A. Kirkevåg, J.-F. Lamarque, D. Olivié, T. Richardson, D. Shindell, P. Stier, T. Takemura, A. Voulgarakis, D. Watson-Parris Geophysical Research Letters Volume 45, Issue 20, Pages 11,399-11,405,https://doi.org/10.1029/2018GL079474 Understanding Rapid Adjustments to Diverse Forcing Agents C.J. Smith, R.J. Kramer, G. Myhre, P. M. Forster, B. J. Soden, T. Andrews, O. Boucher,	Taken into account. Results of PDRMIP are important but more related to other chapters that focus on understanding physical processes and not on human attribution of observed changes, which is chapter 3's focus. For example, PDRMIP results are relevant to Chapter 8 as they focus on hydrological sensitivity and on regional precipitation changes. PDRMIP is also relevant to chapter 7 as they consider fast and slow climate feedbacks. Nevertheless, we have included the reference Richardson et al (2018) that uses the results of PDRMIP and CMIP5 to interpret the role of anthropogenic forcing on global mean land/ocean rainfall trends.
30626	22	5	22	20	I suggest to leave discussion of mechanism to ch 8 and keep here only what related with attribution assessment [Annalisa Cherchi, Italy]	Rejected. Ch 8 explains mechanism in detail. Here is only introduced to give context to changes. Otherwise the reader cannot interpret results. We include a link to chapter 8.
54802	22	5			Polson et al analyzes 30N-30S not quite the tropics only (tropics and subtropics?) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Included subtropics.
54690	22	13	22	13	should be Undorf et al. (2018b) [Sabine Undorf, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Corrected.
40486	22	13	22	14	I still struggle with how one decides which literature to cite in IPCC. There are much earlier papers than Undorf (2018) that have shown the same thing. For example Polson et al. (2014) GRL: https://doi.org/10.1002/2014GL060811 attributed declines in the NH monsoons to aerosol emissions, by comparing GHG-forcing with AA-forcing historical experiments. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence modified and Polson et al 2014 included.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
54804	22	13			Polson et al (Polson D., Bollasina M., Hegerl G. C. and Wilcox, L.(2014) Decreased monsoon precipitation in the 1 Northern Hemisphere due to anthropogenic aerosols. GRL, 41, 6023–6029, doi:10.1002/2014GL060811.) detects aerosol influences in overall monsoons. Undorf et al 2018 is about source regions and detects the influence of North American and european aerosols in the african monsoon (e it cant be reproduced without that in CESM runs) and but shows that for reproducing Asian monsoon changes the local emissions are important too - so it detects fingerprints from aerosols from two separate emission regions. (i would cite them both but of course i am biased!) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence rewritten and Polson et al 2014 included.
49366	22	14	22	20	New Satellite observations of Sea Surface Salinity in the Arctic may reduce the uncertainty link to the limited availability of in situ saltinity observations (see website Arctic+ Salinity project: arcticsalinity.argans.eu). Hence Satellite observations have a [Rafael Catany, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Abrupt end to sentence, not clear what the reviewer suggests.
45464	22	30			One study found….How do you there was one study? Rephrase [David Baguma, Uganda]	Editorial. Sentence rephrased.
7384	22	30			list one or two of such studies [Chukwuma Anoruo, Nigeria]	Rejected. This is the summary of the section.
43178	22	39	22	40	With reference of the atmospheric water vapour the AR5 concluded that the contribution has found medium confidence, is seems general data, it don't seem too technical or which is the range of the confidence. The description could be better. [Sebastian Naranjo Silva, Ecuador]	Rejected. The paragraph summarizes closely the findings of AR5.
37574	22	40			This paragraph needs revising and briefly extending. The revision should note that Bindoff et al. (2013) in fact concluded that the mechanism needed "to be better understood". They did not characterise understanding at the time as poor - it could have been regarded as moderate. This is not surprising, as the levelling off of water vapour (decline in relative humidity) over land was a finding published only in 2010. The extension to the paragraph should cross-reference chapters 4, 7 and 8 of this AR6 report, which discuss post-AR5 work on this topic that has indeed provided better understanding - and confirmed the identification in the 2010 publication of the warming differential between land and sea as a key factor. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence rewritten and link to chap 4, 7 and 8 included.
52610	22	44	22	47	Aren't the first and second sentence explaining exactly the same phenomenon (namely the water-vapour feedback)? Currently it reads as if two different issues are being addressed. [Douglas Maraun, Austria]	Taken into account. Second sentence shortened to avoid repetition.
7386	22	44		45	substantiate with literature [Chukwuma Anoruo, Nigeria]	Accepted. Included reference Hartmann et al (2013).

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50056	22	49	22	51	Are you reffering to water vapour errors in the CMIP5 models and/or observations here? It should also be noted that the study by Sherwood et al. (2014) demonstarted that ~50% of the observed variability in the CMIP5 models was attributed to vertical/convective mixing between the planetary boundary layer and the free troposphere. Therefore, climate sensitivity will also be a source of error in models. (Sherwood, S.C., Bony, S. and Dufresne, J.L., 2014. Spread in model climate sensitivity traced to atmospheric convective mixing. Nature, 505(7481), p.37.) [Tim Trent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence refers to biases in models and has been modified. Also, we included that errors in relative humidity are closely related to errors in the large scale circulation. More detailed analysis on climate sensitivity is found in ch 7.
45466	22	49		51	The word "errors" is repeated many times in the sentence. Rephrase or improve the sentence [David Baguma, Uganda]	Editorial. Change one “errors” by “biases” for legibility. The other two “errors” stay as they are needed to convey the message.
27728	23	6	23	6	delete comma [Poot Delgado Carlos Antonio, Mexico]	Accepted
52612	23	6	23	8	I don't find this statement helpful. Why do models not capture the behaviour? Because of model errors? Then this is an issue. Or because of internal variability? Then it is trivial and not an issue. [Douglas Maraun, Austria]	Taken into account. Models cannot reproduce the relative humidity trend even when forced with observed SST. Thus, they ascribe the failure to representation of land surface processes and response to CO2 forcing. The sentence has been modified to reflect this.
37576	23	6	23	8	This sentence is not wrong, but it would be fairer to others who worked previously on this topic to write: "Dunn et al. (2017) confirmed earlier findings that global mean … approximately constant, and showed that none of the CMIP5 models captured this behaviour. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
47186	23	6	23	10	A two-step attribution study has however emphasized a recent anthropogenic decrease in relative humidity over the northern mid-latitude continents in summer, which was underestimated by most CMIP5 models with potential implications for the projected 21st century drying in these regions (Douville and Plazzotta, 2017 ; doi:10.1002/2017GL075353) [Hervé Douville, France]	Accepted. Sentence included.
16268	23	6	23	14	It was noted that there had been a steep decline after 2000. However, the conclusion does not depict a similar observation. The conclusion should be based on overall descriptive analysis. [Tabassam Raza, Philippines]	Rejected. The summary is concerned with attribution and there are not enough studies to attribute.
52614	23	19	23	51	You sometimes use the term streamflow, sometimes discharge. Is there a reason? If not stick to one, as it is confusing otherwise. [Douglas Maraun, Austria]	Editorial. Accepted: only use streamflow.
16270	23	19	23	51	Considering the evidence explained (from line 26 to 45) it is not prudent to reclaim that there is "medium confidence" when there is lack of significant evidences available to justify this reclaim. Thus, there is need to explore other evidences or rearrange the confidence level into lower category such as "low confidence"; although streamflow is also subject to human interventions... [Tabassam Raza, Philippines]	Rejected. This is the summary of AR5.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
45468	23	21			…attribution studies…which studies [David Baguma, Uganda]	Rejected. This paragraph is the summary of the AR5 report.
45470	23	24			where? [David Baguma, Uganda]	Rejected. This is the statement in AR5. Observational uncertainties are large worldwide.
44060	23	26	23	37	Here is another reference on difficulty in finding detectable/attributable changes in streamflow (in this case winter-spring streamflow timing over North American regions): Kam, J., T.R. Knutson, and P.C. Milly, 2018: Climate Model Assessment of Changes in Winter–Spring Streamflow Timing over North America. J. Climate, 31, 5581–5593, https://doi.org/10.1175/JCLI-D-17-0813.1 [Thomas Knutson, United States of America]	Taken into account. Reference included.
30112	23	29	23	37	As a reader, I feel like this entire paragraph is leading me to another sentence or even a figure after this introduction (or at the end of line 37) which would assist the layman reader in grasping concepts introduced here such as how and why aerosols in the environment can cause increases or decreases in streamflow, regionally. I've now pulled up the Gudmundsson et al. article to inform my own reading of this, as it piqued my curiosity. I recommend making an addition to this section which clearly details these processes accessibly for the reader, IE, comparing curves of reflectivity to heat retention or detailing how aerosols impact cloud formation. Otherwise, this could likely be cross-referenced with a heading that recommends referring to Figure 8.21, which explains aerosol interaction and impacts on cloud formation and precipitation well enough for my understanding. [Zachary Lubow, United States of America]	Accepted. A link to chapter 8 has been included.
52616	23	34	23	35	Please be precise here. The trend is not uncertain because of the choice of method, it is the trend estimate. [Douglas Maraun, Austria]	Editorial. Corrected as suggested.
49066	23	35	23	35	Spelling mistake - different [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Corrected.
51472	23	35	34	16	typo in "differnet" [Bart Van den Hurk, Netherlands]	Editorial. Corrected.
49068	23	35			Would contrasting be better than opposite? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Rejected, “opposite” reflects better what the authors found.
54806	23	39	23	45	As an evaluation of model regional precipitation through streamflow changes it might be worthwhile to cite Iles and Hegerl 2015 Ngeo which shows that streamflow in postvolcanic years shows detectable changes that are of the sign expected from models in some regions - wet tropics and some dry regions show increases. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference is now included in a sentence.
52618	23	41	23	42	Which diagnostics are used here (this is much more relevant than the fact that a Bayesian approach has been used)? What does “decadal simulations” mean? Please check this paper carefully, it doesn't sound convincing to me and it looks a bit as if internal variability has been mapped onto each other for model weighting, which would not make sense at all. [Douglas Maraun, Austria]	Taken into account. We have checked the details and revised the text accordingly.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
51474	23	43	43	44	"interannual runoff" probably needs to be "interannual variability of runoff". Is the seasonal cycle of runoff well described by the CMIP5 models? [Bart Van den Hurk, Netherlands]	Accepted. Corrected “interannual variability of runoff”. Alkama et al (2013) show that CMIP5 models simulate the climatological streamflow reasonable well, except for over South America and Africa.
27954	23	47	23	51	Page 23, lines 47-51: It is not clear whether the human interventions and water withdrawals that may interfere with the attribution of streamflow changes are in fact accounted for in the "medium confidence" in the effect of anthropogenic climate change on streamflow. [roderik van de wal, Netherlands]	Accepted. Yes, effects of other factors than climate change are accounted for the "medium confidence" statement. For clarity, we have revised the sentence.
38518	24	1	25	3	The assessment of the observed change of the Hadley circulation is inconsistent between Chapter 2, 3, and 8. In Chapter 2, "In summary, there has been a very likely widening of the Hadley Circulation since the 1980s, although there is only medium confidence in the magnitude. This has been accompanied by a likely strengthening of the Hadley Circulation, particularly for the northern hemisphere cell (medium confidence)."(p.51, L46-49). While in Chapter 3, "observed zonal mean Hadley cell expansion since the 1970s and changes in the Pacific Walker circulation strength are within the range of internal variability."(Chap 3, p.4, L47-49 & p.24, L53-54). In Chapter 8, "Multiple observational evidences indicate that in most seasons the Hadley cell expanded in both hemispheres, but its intensity remained almost unchanged (Nguyen et al., 2013). A poleward shift in the 23 subtropical highs of both hemispheres has been identified, consistently with the observed poleward expansion of the Hadley circulation and widening of the tropical belt." (p.54, L21-25). [Masaki Satoh, Japan]	Taken into account. There is no inconsistency that there have been long-term changes in observations but these changes have not yet emerged out of the range of internal variability. However, in SOD, we have emphasized the attribution and avoid unnecessary confusion.
49070	24	3	24	8	Hadley and Walker circulations had already been mentioned in the chapter so perhaps this useful explanation should be moved elsewhere. [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. For better readability, description of each large scale indicator should be given at the corresponding section. Instead of moving it, we have referred to this section when mentioning the Hadley and Walker circulation especially at Section 3.3.2.
52620	24	8			The trade winds (their meridional component) are also associated with the Hadley circulation. [Douglas Maraun, Austria]	Taken into account. This section focuses on zonal component of the trade winds. We have clarified this point.
28910	24	10	24	10	It should be either "stratospheric ozone depletion" or "the depletion of stratospheric ozone". [Matt Tully, Australia]	Accepted. Rephrased as suggested.
30628	24	15	24	31	Fig. 3.12 is organized to evidence differences among seasons, but in this paragraph seasonality is not discussed [Annalisa Cherchi, Italy]	Accepted. We have revised the figure to show NH annual mean, SH annual mean and SH summer only.
52622	24	31			You should add to the sentence that the detection does not refer to the PDV but to the Hadley circulation, if I got this paragraph correctly. [Douglas Maraun, Austria]	Accepted. Added "on the Hadley cell expansion".

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27956	24	53	24	55	Page 3-24, lines 53-55: Saying the observed zonal mean Hadley cell expansion is within the range of internal variability and then mentioning that studies confirm the contribution of human influence on the Hadley cell expansion is contradictory. Please clarify [roderik van de wal, Netherlands]	Taken into account. It is not contradictory that there is human influence but the resultant change has not yet emerged out of the range of internal variability. However, in SOD, we have emphasized the attribution to avoid unnecessary confusion.
52624	24	53	25	3	I would emphasize the difference with AR5 here. [Douglas Maraun, Austria]	Taken into account. This paragraph has been revised considerably.
37578	25	14			In addition to adding the listed reanalyses to the figure for the SOD, it would be appropriate to replace ERA-Interim by ERA5. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ERA-Interim has been replaced by ERA5.
30634	25	19	25	19	"Global monsoon" instead of "Global monsoons" [Annalisa Cherchi, Italy]	Editorial. Accepted.
39522	25	19	26	23	The following review paper could be useful for the assessment of the human influence on global monsoon changes: Seth et al. 2019: Monsoon Responses to Climate Changes—Connecting Past, Present and Future. Current Climate Change Reports https://doi.org/10.1007/s40641-019-00125-y [Carolina Vera, Argentina]	Taken into account. We have revised the paleo monsoon assessment considering this paper.
40488	25	19	26	40	Generally I think this global monsoons section is well written and appropriately referenced, and it makes good links back to Chapter 2 and forward to Chapters 8 and 10. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
30630	25	21	25	23	I would avoid these introductory lines, too simplicistic and ignoring bunch of literature arguing on the role of the land-sea thermal contrast (i.e discussing energy constraints like Biasutti et al 2018, or circulation regimes like Bordoni and Schneider 2008) [Annalisa Cherchi, Italy]	Taken into account. We have rewritten this paragraph to highlight the energy framework.
30632	25	21	25	25	I would write instead "Global monsoon groups all the tropical regions affected by monsoon systems and it has been identified with specific metrics (Wang and Ding 2008) that are assessed here. On the other hand, assessments of regional monsoons changes are made in Ch 8 and Ch 10." [Annalisa Cherchi, Italy]	Taken into account. We have rewritten this paragraph to highlight the energy framework.
39112	25	21	26	25	I totally agree with the analysis done in this section. It would be good to have more information about CMIP6 inputs in this section. [JACQUES ANDRE NDIONE, Senegal]	Noted. We have made the figure based on CMIP6.
45472	25	23			Include the source of this information [David Baguma, Uganda]	Taken into account. This paragraph has been rewritten.
16272	25	27	25	28	Because of the language used to quantify the performance of the model, it might be confusing with the confidence level. [Tabassam Raza, Philippines]	Accepted. We have changed the statements on model performance.
45474	25	28			Check the accuracy of this sentence [David Baguma, Uganda]	Accepted. We have rephrased this sentence.
30636	25	29	25	29	"global"to include before "monsoon circulation" [Annalisa Cherchi, Italy]	Editorial. Accepted.

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40490	25	29			I think it would be better to replace "there were no detection and attribution assessments" with "there were no detection and attribution assessment statements". This is to avoid ambiguity - of course studies had been done on this at the time of AR5 - just not sufficient to lead to an assessment statement in the report. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rephrased as suggested.
31946	25	31	25	31	Paleoclimate information on monsoons was mostly regional'. There is not much about global paleomonsoon from paleoclilmate information. Maybe a starting point Wang et al (2017) https://doi.org/10.1016/j.earscirev.2017.07.006 [Marie-France Loutre, Switzerland]	Noted. Thanks.
29178	25	37	25	38	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
16274	25	38	26	23	The conclusion is not clearly stated corresponding to the information portrayed regarding various models and combination of model assessment about global monsoons. Indeed, It lacks comparison or integration of the mentioned model outputs. It should consider and correlate the outputs of these models in terms of individual confidence in order to provide justification for the declaration of global monsoon confidence level from anthropogenic aerosol contribution. [Tabassam Raza, Philippines]	Taken into account. We have revised to conclusion paragraph to clarify links of each justification statement with the assessment statement.
45476	25	39		40	There are many past tense words in this sentence. Rephrase [David Baguma, Uganda]	Taken into account. We have rewritten this paragraph.
40492	25	42			For "higher resolution" what are we talking about here? Is it a subset of CMIP5 models (so e.g. 100 km), or experiments outside of CMIP5 e.g. at 50 km , 20 km etc.? The term high resolution means different things to different communities. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised this paragraph and added typical model resolutions used in the studies cited here.
40494	25	42			The better reproduced land surface climate sounds very vague and general. Does it apply to hemispheric monsoon precip and circulation? A counter to this (although just for the Asian monsoon) is the study of Johnson et al., which finds resolution/orography improvements in the local "detail" of the monsoon but not fundamental improvement in the bias. http://dx.doi.org/doi:10.1007/s00382-015-2614-1 [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised this part and mentioned that the improvement with resolution is not universal among models.
40496	25	43	25	45	I disgagree with the statement that, "Consistently, the simulation of annual mean and the seasonal cycle of global monsoon precipitation and circulation improves in AGCMs with higher resolutions (Zhang et al., 2018c). The study of Johnson (see comment above) shows that this is not a consistent finding, even when getting to resolutions of ~30 km. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised this part and mentioned that the improvement with resolution is not universal among models.
29180	25	48	25	49	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
40498	25	55			The phrase, "outside the 90% range" is ambiguous, although I think what you are saying is that the historical trend is greater in magnitude than 90% of artificial trends measured in a pre-indsutrial control run. Consider rewording this. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased this sentence.
40502	25	56	26	1	As in my earlier literature comment, a much earlier study (yet post-AR5) that noted these findings was Wang et al. (2003) (already in reference list) but even then this was not a new idea. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This sentence does not mean general AMV influence on global monsoon but quantification of its contribution to recent global monsoon intensification, which is new since AR5. Yet we found that one of citations is not relevant for this assessment and has therefore removed it.
40500	25	56			"…contribution of AMV on the subsequent…" --> "…contribution of AMV to subsequent…" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Accepted.
40504	26	12			The meaning of "by nature" may not be undertsood by some readers. I suggest phrasing something like, "However, by definition, proxy data represent local changes, and …" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This paragraph has been rewritten entirely.
40506	26	12			Insert "the" before "paleo" [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This paragraph has been rewritten entirely.
11510	26	15	26	23	A fact hindering D/A precipitation studies is the large precipitation variability relative to the signal….and the fact that there can be increases and decreases in P. This is not stated plainly. [Roanld Stouffer, United States of America]	Accepted. This point has been mentioned at the beginning of Section 3.3.2.
40508	26	16	26	18	The statement that, "There is no evidence that the influence of GHG increases has emerged out of internal variability since the late 20th century.", seems rather strong. The upturn may not be unequivocally attributed to GHG, but GHG-only experiments of the 20th century clearly show increases in monsoon precipitation, and would be consistent with the recent increases in monsoon rainfall mentioned earlier in this section. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Even though there is GHG contribution to the recent upward trend, it is unclear whether the GHG-forced trend is detectable out of internal decadal variability due to lack of literature.
40510	26	20			Is the implication of the inadequate representation of aerosol-cloud interactions mentioned here that aerosol-cloud interactions are being under or over represented (and therefore the effect on monsoons is under or overestimated) or is this not known? Guo et al. showed that those (very limited sample) of CMIP models containing indirect effects suppressed the monsoon more than those which did not. http://dx.doi.org/doi:10.5194/acp-15-6367-2015 [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This important literature is considered in the assessment together with more CMIP6 models.
51860	26	22			medium performance is an ambiguous term to use here. Can a better term be used? This also applies in several other spots. [Peter Thorne, Ireland]	Accepted. We have changed the statements on model performance.
30638	26	23	26	23	"monsoon" instead of "monsoons" [Annalisa Cherchi, Italy]	Editorial. Accepted.
52626	26	45	26	49	I don't find this introduction to the point. E.g., is “baroclinic activity” a technical term? Also “linked to” is a but vague. [Douglas Maraun, Austria]	Accepted. We have rephrased the sentence to make this clearer. "Baroclinic activity" is now longer used.

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7388	26	45		46	substantiate with literature [Chukwuma Anoruo, Nigeria]	Rejected. These are textbook-style definitions of the concepts that can stand without references.
52630	26	49			You should link to Chapter 10. There we discuss the relevance of performance in simulating large-scale features for regional climate. [Douglas Maraun, Austria]	Accepted. We have added a reference to Ch10.
52628	26	52			Please add “biases in” before “cyclone frequency” [Douglas Maraun, Austria]	Accepted. We are following the reviewer's suggestion.
15554	26				3.3.3.3 Extratropical jets, storm tracks and blocking: There is no description how the characteristics of extratropical jet in Asia-North Pacific including its intensity and position are influenced by anthropogenic forcing. [SANG-WOOK YEH, Republic of Korea]	Noted. This would probably best be covered in Chapter 10 (which focusses on regional climate change). In fact the chapter does not cover any trends in the position of the jet which is relevant here mainly because of its linkage with blocking events and cyclones.
45478	27	1			The authors intend to present a sentence that begins with the words ’New research….’ But its not clear when it was new or when it will stop being new and then old. Perhaps, consider rephrasing the section. [David Baguma, Uganda]	Accepted. We have inserted "Since AR5" at the start of the sentence to make this explicit.
13912	27	2	27	9	The discussion on blocking biases doesn't mention Europe, where biases are still unfortunately systematic, even at high resolution (Schiemann et al 2017; http://dx.doi.org/10.1175/ JCLI-D-16-0100.s1.; Woollings et al 2018; https://doi.org/10.1007/s40641-018-0108-z). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now rebalance the discussion, mentioning some persistent issues with blocking particularly for the Euro-Atlantic sector. We now include both references in the discussion. The "Atlantic sector" was meant to include Europe; this is now made explicit.
49170	27	3	27	3	suggest addition "…blocking in some regions (Zappa et al. 2013) and seasons (Schiemann et al. 2016), …". Aim is for a paper based on CMIP6 HighResMIP models to be written before the deadline. R. Schiemann, M.-E. Demory, L. C. Shaffrey, J. Strachan, P. L. Vidale, M. S. Mizielinski, M. J. Roberts, M. Matsueda, M. F. Wehner, T. Jung, 2016: The resolution sensitivity of Northern Hemisphere blocking in four 25-km atmospheric global circulation models. Journal of Climate, doi: https://doi.org/10.1175/JCLI-D-16-0100.1. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have adopted the reviewer's formulation.
43180	27	5	27	8	There are many announcers on the chapter that say "Erreur! Source du renvoi introuvable". I suppose that the references are not right vinculated [Sebastian Naranjo Silva, Ecuador]	Accepted. This has been corrected for the SOD.
29182	27	5	27	8	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
56442	27	6	27	7	Davini et al. 2017 (JAMES) found an improvement of blocking frequency with resolution in an ensemble of AMIP simulations. However it is worth mentioning that the blocking frequency improvement does not translate/derive in/from an improvement in the model bias [Corti Susanna, Italy]	Accepted. We now include this paper in our discussion.
49072	27	6	27	9	Unclear - these sentences should be reworded [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased the sentence.

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52632	27	6			which aspects of blocking have been investigated? In which seasons? [Douglas Maraun, Austria]	Accepted. We now make explicit that we are talking about the "blocking frequency" in the annual mean.
49172	27	9	27	9	Given the evidence above about resolution increase and improved blocking, is this last sentence (no improvement since CMIP3) simply saying that the resolution has not advanced far enough between those model generations? [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We do not know what would happen in a hypothetical model with extremely high resolution. At 25 km, there is improvement but some biases also remain. This has been clarified also in response to comment 13912 by Tim Woollings.
6285	27	11	27	20	Here trends in extratropical cyclone frequency are mentioned, but not the trends in the associated weather fronts, which are known to trigger, for example, heavy precipitation. New research has identified an upward trend in the nummber of extreme fronts over Europe, mainly during summer, but no such trend over North America. The trend is strongest during summer, and due to an increase in atmospheric humidity (Schemm et al. 2016, doi:10.1002/2016GL071451; Research highlight in Nature Climate Change: https://doi.org/10.1038/nclimate3218). [Sebastian Schemm, Switzerland]	Noted. We agree that fronts are an important aspect of cyclones. However, literature on this topic is much more scarce than for cyclones. The remit of this chapter is to use climate models to attribute any changes to human activity or other drivers. To our understanding, such attribution studies don't exist for fronts. Schemm et al is an observational study; the comment should really refer to Ch2 which covers the observational evidence.
52634	27	11	27	20	Please add recent literature on the use of storylines (e.g. Zappa and Shepherd, J Climate, 2017, and related literature). It does not make sense here to look only at the multi model mean response, there are opposing but plausible changes projected. [Douglas Maraun, Austria]	Noted. We now cite this literature. We had not however attempted to form a multi-model mean response when models produce inconsistent responses.
45480	27	11			The authors intend to present a sentence that begins with the words 'New research….' But its not clear when it was new or when it will stop being new and then old. Perhaps, consider rephrasing the section. [David Baguma, Uganda]	Rejected. Here is clear that "new research" refers to the post-AR5 period.
48452	27	22	27	29	A time period is needed over which 'substantial climate change' is assessed. Add references to Schneider et al 2015:10.1175/JCLI-D-15-0090.1 and Clem et al 2017: 10.1007/s00382-016-3329-7 which suggest a role for tropical Pacific SSTs in driving changes in the jet since the early 2000s. Ensure check consistency with SROCC and WMO/UNEP 2018 ozone assessment [Julie Arblaster, Australia]	Accepted. We now make explicit that this is climate change since the start of anthropogenic ozone depletion. A sentence is added on the linkage with tropical SST anomalies. Formally, the chapter is concerned with the role of humans in driving any changes, so for the purpose of attribution the linkage with SSTs would fall into the "natural variability" category.
7390	27	22			there is need to cite other models. The statement should also capture climate variability [Chukwuma Anoruo, Nigeria]	Part1: rejected. The cited literature references already cover CMIP5, ACCMIP, and CCMI models, i.e. the major model evaluations of recent times. Part 2: Accepted. We now also mention one new study concerned with climate variability associated with ozone depletion.
48450	27	24	27	24	Maycock et al 2018b should be Karpechko & Maycock et al 2018 [Julie Arblaster, Australia]	Accepted. This has been corrected.

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7392	27	24		27	it is certain that maximum total ozone concentration coincides with westerly maximum. this makes the ozone concentration to be dependent on density height integral of ozone at latitude and also to the solar cycle oscillation phase. Since the solar cycle influences BD circulation as the result of height, the profund wind QBO influence on ozone is only observed only during winter and spring seasons. [Chukwuma Anoruo, Nigeria]	Noted. It is unclear how the reviewer's comment relates to the text.
52636	27	24			please add “austral” before “summer” [Douglas Maraun, Austria]	Accepted. We follow the reviewer's suggestion.
48448	27	27	27	28	The precipitation changes have not been reproduced by these models (only the timeslice runs in Kang et al 2011), rephrase to ensure 'This' refers to atmospheric circulation only [Julie Arblaster, Australia]	Accepted. Precipitation changes were not explicitly considered by any of these authors. This is now corrected.
29184	27	28	27	29	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
52638	27	31			“only one” - this rings the wrong bell as it is not clear that “contiguous” implies a full latitudinal circle. [Douglas Maraun, Austria]	Accepted. "Contiguous" does not imply a full latitudinal circle. This is now made explicit.
54994	27	38	27	39	SR15 had included similar statements on findings on trends on the number of cyclones. In contrast, it was mentioned that "the occurrence of very intense tropical cyclones (category 4 and 5 hurricanes on the Saffir-Simpson scale) over recent decades" has been reported to increase in most studies with 7 references ranging from (Emanuel, 2005) to (Walsh et al., 2016) as provided in section {3.3.6} on page 203 of SR15. These findings may be better represented in WG1, possibly with a focus beyond extratropical cyclones. [Kilkis Siir, Turkey]	Noted. Tropical cyclones are out of scope for this chapter. It is unclear how the reviewer's comment relates to the content of the chapter.
13914	27	45	27	46	This statement should be revisted once more CMIP6 results are out. In CMIP5 at least there were still many models with very poor blocking over Europe. The statement on resolution could be softened to 'often coresponding to better performance'? The improvements are often quite small and there are several other important model aspects (Woollings et al 2018; https://doi.org/10.1007/s40641-018-0108-z). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased the sentence to accommodate the reviewer's concern. The evaluation is now also taking into account newly published research on CMIP6 results.
50102	28	18	28	42	How will ENSO change in the future is quite important. However, due to the limited models, it is not possible to make any conclusion so far. [Hong-Li Ren, China]	Noted. It is unclear how this relates to the text.
28912	28	18	29	7	This is extremely repetitive with chapter 2 but even worse, not very consistent in the overall assessment. [Matt Tully, Australia]	Not applicable. The discussion of the QBO has been dropped from Ch3.
7394	28	20		22	substantiate with literature [Chukwuma Anoruo, Nigeria]	Rejected. This is an uncontroversial textbook-style introduction of the concept.
28914	28	34	28	37	The amplitude of the QBOat 70 hPa has gone back up again from the late 1990s. [Matt Tully, Australia]	Accepted. I don't think the strengthening of the QBO has been published. We have weakened our assessment, in line with Chapter 2.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
37580	28	35	28	37	Some caution is needed in quoting the findings of Kawatani and Hamilton (2013). These authors did find a downward linear fitted trend in QBO amplitude particularly at 70hPa. However, looking at the unfitted data presented in Fig.1 of their paper, which cover the period only to 2008, it is clear that at low levels the linear trend comes mainly from one period of above-strength QBO peaking in 1964 and one period of much below-strength QBO peaking in 2006. It is questionable whether this can really be regarded as a trend over recent decades, the more so as in the current decade the QBO amplitude appears to have increased, examining by eye time series of radiosonde and reanalysis data. The unusual behaviour of the QBO after 2015 does not help interpretation. The paper certainly should be referenced, but the wording used should include an element of caution. Note that a similar comment (no. 123) has been made with regard to a reference in Chapter 2. If there is a response to these comments, it should be coordinated between the chapters. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased the assessment, also considering a comment by Matt Tully.
39114	28	42	28	52	A figure to highlight results from this section be good. [JACQUES ANDRE NDIONE, Senegal]	Noted. We agree with the reviewer. However, due to space constraints we are unable to include such a figure.
7180	28	47	28	52	Please insert a sentence like "Presently one high-vertical resolution GCM has successfully simulated the disruption event using observed SST and sea-ice concentration (Watanabe et al., 2018)." Watanabe, S., Hamilton, K., Osprey, S., Kawatani, Y., & Nishimoto, E. (2018). First successful hindcasts of the 2016 disruption of the stratospheric quasi-biennial oscillation. Geophysical Research Letters, 45, 1602– 1610. https://doi.org/10.1002/2017GL076406 [Shingo Watanabe, Japan]	Accepted. We now cite this paper.
28916	28	50	28	50	"raises the bar" is too informal an expression. [Matt Tully, Australia]	Accepted. We have rephrased the sentence.
28918	28	50	28	52	Are climate models genuinely "expected" to reproduce the 2016 event? I don't think this is realistic. [Matt Tully, Australia]	Rejected. Of course a free-running climate model will not "reproduce" this singular event. The anomalous behaviour has been linked to unusual tropospheric wave forcing. In the presence of such forcing, yes, climate models would now be expected to produce this unusual behaviour. We think the formulation conveys that.
51862	29	9	29	28	It feels odd to have this text without trying to draw any assessment conclusion finding. Consider expansion and formulation of an assessment finding. Particularly given recent interest in the phenomena it feels too short and lacking a definitive final conclusion as it stands here [Peter Thorne, Ireland]	Accepted. However, in the face of disagreements in the literature and a somewhat weak statement also in Chapter 2, it is hard to come up with a well-founded assessment statement on causes for any changes in SSWs. It is better not to assess anything than to come up with unfounded statements.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
54996	29	11	29	12	The important referral to the weakening of the Arctic polar vortex may include related references, such as Smith et al. (2019) International Journal of Climatology 39(4): 2080-2095 on "The influence of atmospheric circulation patterns on cold air outbreaks in the eastern United States." There are multiple other recent studies, which may be used to provide empirical evidence over "an absence of studies specifically focusing on simulated trends" as mentioned later in line 27. [Kilkis Siir, Turkey]	Noted. The cited paper does not refer to climate modelling. The "absence of studies" refers to papers assessing causes for any trends in SSWs. We maintain there are no such studies.
45482	29	14			The author may consider putting the source of the information [David Baguma, Uganda]	Not applicable. SSWs are now defined in Chapter 2.
7396	29	32		34	This section should be rephrased for easy understanding. A simple definition of BD circulation should be adopted. Also, it should be added that BD circulation has inverse relationship with ozone. This will make more clearer the mechanism. [Chukwuma Anoruo, Nigeria]	Not applicable. Discussion of the BDC is dropped for the SOD.
28920	29	42	29	46	The Karpechko et al 2018 reference I think is supposed to be to chapter 5 of the WMO 2018 ozone assessment, not the one given. [Matt Tully, Australia]	Not applicable. Discussion of the BDC is dropped for the SOD.
53344	30	19	30	20	The sentence "Hardiman et al. (2017)'s quantification … statement." doesn't work well. [Jan Fuglestvedt, Norway]	Not applicable. This section has been dropped.
13794	30	23	30	42	The current text in this paragraph would benefit from an assessment of the ability of CMIP models to reproduce the historical record (and reference to 9.3.1.1) [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Added the link to Ch. 9 as 9.3.1.1.
39384	30	23	31	41	I find the structure of this section not necessarily logical. It starts with "sea-ice loss", then goes to "attribution and detection" then "differences between models" then "internal variability" then "attribution of extreme events" then "conclusion". I think the section would read better if we start the same way with "sea-ice loss" but then introduce possible uncertainties such as "metrics and observational uncertainty" (not mentioned much yet), "differences between models", "internal variability" and then end on a strong statement: despite these uncertainties, we can conduct "detection and attribution" and "attribution of extreme events" [Clara Burgard, Germany]	Taken into account. This section has been revised and the structure is now AR5 summary, model evaluation, attribution.
39116	30	23	36	17	This section 3.4 should consider results and conclusions coming from SR on Cryosphere and Oceans [JACQUES ANDRE NDIONE, Senegal]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD
31456	30	23	36	17	A general reamark on this section:It is very good that this section systematically refers to Ch 9. This ensures consistency. Thank you! What I found a bit annoying is that there are very many references to CMIP6 and its subprojects. These parts of the text are not always very helpful for the assessment (or are these some implicit placeholders for real CMIP6 results to come?) [Gerhard Krinner, France]	Noted. The coordination with Ch9 is a work in progress, and we expect a considerable revision in collaboration with Ch9 for the SOD which should homogenize content among the chapters

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
13792	30	23	47	11	Well done to the authors on putting together sections 3.4 and 3.5. I think in some places the text would benefit from greater synergy with chapter 9 as there is currently quite a lot of repetition which we should avoid. I think 3.4 and 3.5 read a little more as a review rather than an assessment and I would recommend that every paragraph aims to make a clear assessment statement. This is reflected in my more specific comments. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. See our specific responses below.
45484	30	29			The use of the word "concluded" at the beginning of a section could be questionable or be misleading. Please, find another word. [David Baguma, Uganda]	Rejected. We follow the common structure that should apply to all Ch 3 sections and first sentence starts from "AR5 concluded that ...".
29186	30	33	30	37	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD.
39386	30	35	30	37	This sentence is unclear. I do not get the message here. [Clara Burgard, Germany]	Accepted. We have clarified this.
39388	30	38	30	42	It is unclear why this information is given here. I suggest creating a new paragraph, talking about uncertainties introduced by metrics (Ivanova et al.,2016) and uncertainties introduced by observational uncertainty (e.g. Ivanova et al., 2014, Niederdrenk and Notz, 2018) [Clara Burgard, Germany]	Taken into account. We have checked with Ch. 9 and added information on uncertainties as needed.
38418	30	44	31	3	There are a number of non-formal attribution studies that suggest a contribution of internal variability of 30-50 % to the observed Arctic sea-ice loss (see page 9-38, first paragraph). You might want to include these studies here. [Dirk Notz, Germany]	Accepted. Added those studies and revised the text accordingly.
39390	30	44	31	3	I suggest highlighting first clear attribution in observations (e.g. Notz and Stroeve, 2016). Then, the models can be used to further underline detection and attribution in comprehensive context. [Clara Burgard, Germany]	Rejected. We follow the common structure that should apply to all Ch 3 sections.
13796	30	44	31	3	I would expect this paragraph to make an assessment statement about detection and attribution of Arctic sea ice change. I suggest that detailed description of specific studies is changed to a concise assessment. Also, should the correlation with Co2 high confidence in 9.3.1.1) be discussed in this paragraph? The next two paragraphs could be summarised at the end of this paragraph with an assessment statement on processes and internal variability linking to 9.3.1.1 for details. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added the link to 9.3.1.1.
38416	30	44	32	49	I enjoyed reading the section on sea ice, and find that it gives a nice overview of recent developments. I find, however, that the assessement-character of the text should be strengthened. The text in my view currently reads too much like a review rather than an assessment. [Dirk Notz, Germany]	Taken into account. We have added some confidence statements to the text, using a consistent format with other subsections in Ch3.
45486	30	52			The word "…They…" do authors mean reserchers sited? Rephrase and elimite the word "they [David Baguma, Uganda]	Accepted. Revised it as "The study" or similar.
27730	31	1	31	1	delete point [Poot Delgado Carlos Antonio, Mexico]	Taken into account. Edited as "Gagne et al. (2017b)".

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
26178	31	5	31	13	Representativenes of snow depth on sea ice is very critical for the sea-ice growth, in particular under recent moistened situtation over the Arctic Ocean. Following paper would be relevant to cite: Sato, K. and J. Inoue (2018), Comparison of Arctic sea ice thickness and snow depth estimates from CFSR with in situ observations. Climate Dynamics, 50, 289-301. [Jun Inoue, Japan]	Rejected. The reference is more on process and is forwarded to Ch. 9.
13798	31	5	31	13	This paragraph could point to 9.3.1.1 for process discussion and have a common assessment. I suggest that discussion of projection sensitivity should be postponed to chapters 4 and 9. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added the link to 9.3.1.1. We keep discussion of observational constraint considering its brevity and importance.
52640	31	12			a study cannot find anything difficult. It is the authors. [Douglas Maraun, Austria]	Accepted. Revised it as "suggest that this is difficult .."
39392	31	13	31	13	I suggest adding "observational uncertainty" as an additional difficulty. [Clara Burgard, Germany]	Taken into account. We have added it.
48134	31	15	31	25	There needs to be a discussion of the growing body of literature that is finding that longwave surface and atmospheric effects and the lack of their realistic representation in climate models can explain a large fraction of model biases. See and cite Huang et al, 2017 (doi:10.1175/JCLI-D-17-0125.1), Kuo et al, 2017 (doi:10.1002/2017JD027595), Kuo et al, 2017 (doi:10.1002/2017MS001117) and Feldman et al (2014) (doi:10.1073/pnas.1413640111). The warming at high latitudes is most significant in the winter where snow- and ice-albedo feedbacks are not directly relevant, and this chapter needs to acknowledge that the study of controls of temperature and frozen-surface extent during polar winter is central to understanding polar amplification. [Daniel Feldman, United States of America]	Rejected. Forwarded it to Ch. 9 that focuses on physical processes.
31156	31	15	31	25	The Ding et al. 2018 paper should also be cited on the role of internal variability on Arctic sea ice loss (https://doi.org/10.1038/s41561-018-0256-8) [François Massonnet, Belgium]	Accepted. We now cite the study.
13800	31	15	31	25	This paragraph needs an assessment statement (see 9.3.1.1). I think that this paragraph and the preceding one could be merged to provide a summary and reference 9.3.1.1 for details. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Cited 9.3.1.1 in the beginning para.
35312	31	15	31	25	The role of internally generated decadal varibility in contributing to variations in the trends in Arctic sea ice needs to be mentioned. This reference (currently cited) actually shows that the accelerated rate of Arctic sea ice loss in the early 2000s had contributions from remote forcing from the negative phase of the IPO in the Pacific during the cold season and positive AMO in the warm season. Therefore, instead of vague references to "internal varibility", there is actual evidence of what it producing that internal variability and how it affects the rate of Arctic sea ice loss; this makes a more convincing case for how observed changes are a mixture of internal variability and externally forced response: Meehl, G.A., C.T.Y. Chung, J.M. Arblaster, M.M. Holland, C.M. Bitz, 2018: Tropical decadal variability and the rate of Arctic sea ice retreat, Geophys. Res. Lett., 10.1029/2018GL079989. [Gerald Meehl, United States of America]	Accepted. Revised the paragraph accordingly.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
52642	31	15			This is a very general statement that belongs to the introductory sections of the chapter (or even chapter 10). It should be deleted here. [Douglas Maraun, Austria]	Taken into account. We have revised the sentence to represent Arctic sea-ice cover.
49074	31	21	31	25	Unclear - do they mean attributed? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have clarified the point.
44062	31	25	31	25	Suggested text to add: Li et al. (2017) conclude that regional internal variability may have played a leading role in producing the subtantial decrease since 1979 in observed sea ice over the Barents Sea SIE in winter. Reference: Li, D. R. Zhang, and T. R. Knutson, 2017: On the discrepancy between observed and CMIP5 multi-model simulated Barents Sea winter sea ice decline. Nature Communications, 8: 14991. [Thomas Knutson, United States of America]	Rejected. Our chapter considers large-scale changes only. Forwarded to Ch.9.
13802	31	27	31	33	Detection and attribution of extreme SIE events should definitely be in this section. I would suggest that the paragraph produces an assessment. I would also query using extremely unlikely in the text (as I don't think that it is meant as an assessment statement which could be confusing to the reader). [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Just single study available to make an assessment.
45488	31	29		41	This section is weak. [David Baguma, Uganda]	Taken into account. We assume that the reviewer points out the final assessment part. We have revised them using more studies based on CMIP6 models.
29188	31	32	31	32	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD.
39394	31	35	31	35	This sentence is an important conclusion. I suggest making it stronger: "The new attribution studies confirm the conclusion of AR5, that it is very likely....." [Clara Burgard, Germany]	Rejected. We have added quantitative information here "more than half loss", which is more than confirmation of AR5 conclusion.
53342	31	35	31	36	"mainly due to greenhouse gases" is confusing. Is it to separate from BC? [Jan Fuglestvedt, Norway]	Taken into account. Now defined as causing more than half of the change. Intended to highlight that GHGs are assessed to be the main driver of the decrease.
13804	31	35	31	41	I like the idea of having a summary assessment but I think this needs to build from previous statements. Also, please check against both chapter 9 and chapter 3 of SROCC for consistency. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added some confidence statements in preceding paragraphs.
46636	31	55	31	55	No mention of Antarctic sea ice thickness in this section. It's briefly covered in 2.3.2.1.2, however. [WGI TSU, France]	Rejected. No d&a study is available.
45490	32	1		45	The section is expected present about human influence but they is limited or nothing about it [David Baguma, Uganda]	Noted. We assess why human influence is not detectable on the Antarctic sea ice changes.
13806	32	2	32	13	This paragraph would benefit from the addition of assessment statements and reference to 9.3.2.1. Eg, the decrease being caused by atmospheric circulation is assigned medium confidence in 9.3.2.1 [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Added the link to 9.3.2.1.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
38420	32	4	32	4	I don‘t think that the increase has been „linear“. I suggest to drop this word. [Dirk Notz, Germany]	Accepted. Deleted it.
48458	32	4	32	5	Is this true? A trend line through e.g. Meehl et al 2019 Fig 1a suggests otherwise. [Julie Arblaster, Australia]	Accepted. Revised it accordingly.
37582	32	4	32	5	It is not correct to write "Antarctic sea extent has been increasing linearly in all months over the 1979-2017 period." It hasn't. Firstly, it has a big seasonal oscillation. Secondly, deviations from its climatological seasonal cycle (anomalies) have oscillated up and down, with a small upward trend when the data are fitted with a straight line.And although the linear trend to 2017 is for an increase, the current extended spell of below-average ice extent began early in 2016. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised the sentence accordingly.
29190	32	5	32	7	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD.
37584	32	6	32	6	It is incorrect to write that the strong decrease in Arctic sea-ice extent started in November 2016. November 2016 is the month with the lowest recorded value of the Antarctic sea-ice anomaly, for both area and extent, and both from NSIDC and from the OSISAF data as processed by ECMWF for use in ERA5. The decrease began earlier in 2016. Values have remained below average most if not all of the time since then. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised it as suggested.
48462	32	6	32	7	Add Purich and England 2019:10.1029/2019GL082671 [Julie Arblaster, Australia]	Accepted. We cite it.
35314	32	6	32	13	The big story for the AR6 regarding Antarctic sea ice is the rapid retreat starting in 2016. There is evidence that we understand why that happened, and this should be beefed up in this discussion which now is very vague. The two references cited (Meehl et al., 2019; Wang et al., 2019) actually show why and how the Antarctic sea ice retreated. It had to do with a confluence of factors that involved convective heating anomalies from the tropical eastern Indian Ocean, a negative SAM and weak westerlies around Antarctica, and most importantly for why the sea ice retreat has been sustained for nearly three years (and counting) are changes in the ocean linked to tropical decadal variaiblity of the IPO (negative in the early 2000s and positive SAM; contributed to strong westerlies around Antarctica). The warm subsurface water was moved upward on decadal timescales by those forcings so that the ocean was preconditioned for the negative SAM in later 2016 such that the entire upper ocean column was then anomalously warm and the ice retreated and has stayed retreated. [Gerald Meehl, United States of America]	Accepted. Revised the paragraph accordingly.
38422	32	7	32	7	There are also studies suggesting an oceanic explanation for the trends (see p 9-41) [Dirk Notz, Germany]	Accepted. Added those studies and revise the text accordingly.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
14580	32	7	32	8	The atmosphere played a role in the sea ice extent decrease in 2016 but the ocean too (see the references cited in this section, in particular Meehl et al., 2019) (medium). Underlining the role of the ocean is also more consistent with the discussion in Chapter 9. [Hugues Goosse, Belgium]	Accepted. Revised it accordingly.
13808	32	15	32	35	This paragraph could be a summary of the process discussion in 9.3.2.1. which assigns low confidence to the CMIP5 model capability to simulate the evolution of Antarctic sea ice and low confidence to the possible mechanisms. The AABW mechanisms should be reflected in 9.3.2.1. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Added the link to 9.3.2.1
29192	32	16	32	16	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD.
35316	32	17	32	39	This entire discussion of Antarctic sea ice can be strengthened by evidence that ties why these trends have been occurring rather than the vague references to internal variability. The following reference actually shows that convective heating from the negative IPO in the tropical Pacific produced an anomalous Rossby wave response with surface winds that were conducive to expanding Antarctic sea ice. Thus we can say a lot more as to why the sea ice was expanding, and a lot had to do with decadal variability in the tropical Pacific: Meehl, G.A., J.M. Arblaster, C. Bitz, C.T.Y. Chung, and H. Teng, 2016: Antarctic sea ice expansion between 2000-2014 driven by tropical Pacific decadal climate variability. Nature Geoscience, DOI: 10.1038/NGEO2751. [Gerald Meehl, United States of America]	Accepted. Revised the paragraph accordingly.
48460	32	29	32	29	Add Purich et al 2018:10.1175/JCLI-D-17-0092.1 [Julie Arblaster, Australia]	Accepted. We cite it.
29194	32	34	32	34	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD.
13810	32	37	32	45	Should this paragraph be merged with the paragraph above? [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This paragraph is for attribution while previous one is for model evaluation.
45492	32	37			Which studies? [David Baguma, Uganda]	Noted. We provide details from the next sentence.
7398	32	37			list such studies and should be maintained [Chukwuma Anoruo, Nigeria]	Rejected. Already provided below.
52644	32	43			Hasn't this been discussed above? If so, either link or delete. [Douglas Maraun, Austria]	Taken into account. We already linked it, saying "as discussed above".
13812	32	47	32	49	As for Arctic sea ice, I think the summary assessment is good but would be better if supported by statements in preceding paragraphs [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added some confidence statements in preceding paragraphs.
31444	32	48	32	48	"There is low confidence in the attribution of this Antarctic sea-ice extent increase". Is there any attribution at all of this increase? Or do you mean "attribution to internal variability"? [Gerhard Krinner, France]	Rejected. Studies suggest diverse factors as described in the previous paragraphs.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
6485	33	5	33	5	Mention that increased Antarctic ice is not inconsistant with the models. A warmer, wetter world can result in more snowfall and ice accumulation as long as temperatures remain below freezing. [Hugh Lefcort, United States of America]	Rejected. Consistency of simulations and observations is assessed in Section 3.4.1.2. On average the CMIP5 models did not simulate the increase in Antarctic sea ice, even though these models should simulate overall increases in precipitation due to warming. Internal variability is discussed as a possible source of the discrepancy.
43182	33	5	33	5	On the number in bracket represents the number of models used, I recommend that the models have the similar number of applications. [Sebastian Naranjo Silva, Ecuador]	Taken into account. We have used more models particularly for NAT, which decreases the influence of inter-model differences.
45494	33	16			provide examples of such activities [David Baguma, Uganda]	Accepted. We now cite one such activity.
47188	33	27	33	52	There is here an apparent paradox (an underestimated negative trend in spring snow cover despite an overestimated snow albedo feedback in CMIP5 models) that may deserve a plausible explanation and an edit of the sentence starting in line 46. [Hervé Douville, France]	Noted. We agree that the paradox is "apparent" in that there is not a clean contradiction between the two findings. The underestimated negative trend in snow cover is associated with the underestimated warming trend over the northern continents. This is there despite the overestimated snow-albedo feedback, implying a presence of other, poorly understood feedbacks. The sentence is being reassessed in the light of any CMIP6 evaluations on the topic.
29196	33	28	33	28	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD..
31446	33	28	33	28	"AR5 found that... (... Thackeray et al., 2016)". That's a shortcut. AR5 cannot possibly have cited a 2016 paper. [Gerhard Krinner, France]	Accepted. We have added "see also" to indicate that these papers make the same point.
29198	33	50	33	51	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD..
31448	33	54	33	56	Thank you for citing these papers here, but I think this isn't really helpful for th assessment. I'd suggest cutting this sentence. [Gerhard Krinner, France]	Accepted. We replaced the sentence with an assessment of SnowMIP and LSMIP results.
47190	34	9	34	10	Too cautious ? Replace « likely » by « very likely » (does any model show a significant increase ?) and « low confidence » by « only medium confidence » ? [Hervé Douville, France]	Noted. We reassess what confidence level is appropriate once CMIP6 results are in.
31450	34	13	34	13	"low-confidence statement on the quality of these simulations": The confidence assessments usually are there to say how confident we are in the *findings*. It might be better to write "Allow only low confidence in these findings..." (or sth similar). [Gerhard Krinner, France]	Accepted. We follow the reviewer's suggestion.
53346	34	14	34	15	The statement here seems vaguer than what is necessary, when seen in light of previous statements. [Jan Fuglestvedt, Norway]	Noted. We disagree that the statement is vague, but reassess the situation versus CMIP6 results.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
53348	34	15	34	15	I suggest replacing "activties" by "emissions of greenhuse gases" [Jan Fuglestvedt, Norway]	Accepted. Although GHG increases are not the only mechanism to affect snow extent, we have added "particularly GHG increases".
51864	34	18	34	44	Such a long and detailed introduction feels out of place with the style elsewhere in the chapter. Consider editing for intra-chapter consistency [Peter Thorne, Ireland]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD.
31452	34	20	34	22	This sentence only talks about glaciers, but I suppose it is meant to talk about glaciers and ice sheets, as this is the topic of the section. But then, I think it would be better to write that ice sheets are represented only as static components, not as dynamic ones (there is, I hope, no climate model that represents Antarctic as an ice-free continent!). As written, the statement would only be true for mountain glaciers which are not represented at all because they are to small. Similar remark applies to the following sentence: ice sheets were there before CMIP6, albeit only as static components (like in most CMIP6 simulations). [Gerhard Krinner, France]	Rejected. The first two sentences describe the status of the simulation of glaciers (sentence 1) and ice sheets (sentence 2) in current generation models. In subsequent text further descriptions elucidate a reader updates from CMIP5/AR5 to CMIP6/AR6
42322	34	27	34	44	Note to what extent abrupt changes are considered, as in DeConto and Pollard (2016) Nature, doi:10.1038/nature17145. [Gabrielle Dreyfus, United States of America]	Noted. This reference is more relevant to ice sheet projections, which are covered in Chapter 9.
12638	34	27	34	44	Important to note how integral the changes to ice sheets are to fully understanding the potential for SLR, especially when considering that SLR will be even more dramatic for non-linear changes to ice sheets, and why the incorporation into the models is crucial to drawing a more complete picture of climate change projections. [Kristin Campbell, United States of America]	Taken into account. We anticipate considerable updates relating to the ISMIP6 component of CMIP6. From these simulations more quantitative insights should be available to address this comment in the SOD
12792	34	27	34	44	Important to note how integral the changes to ice sheets are to fully understanding the potential for SLR, especially when considering that SLR will be even more dramatic for non-linear changes to ice sheets, and why the incorporation into the models is crucial to drawing a more complete picture of climate change projections. [Durwood Zaelke, United States of America]	Taken into account. This comment directly duplicates #12638
42806	34	31	34	41	Glacier contribution to SLR is 21% here, and it was 29% in AR5, so it is not proper to say "broadly consistent". And need a little more interpret for the difference. [Xiao Cunde, China]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD
27958	34	31	34	44	Page 3-34, lines 31-44: This part of the text mentions a multitude of results from observations. This information could be covered in chapter 2 and 9, so it could either be moved there or removed if it is already covered. [roderik van de wal, Netherlands]	Noted. We expect a considerable revision in collaboration with Ch9 for the SOD and so cross-chapter duplication should be minimized with appropriate pointers to specific sections in other chapters
32946	34	37	34	41	Perhaps check which numbers are used in Ch9? [Aimee Slangen, Netherlands]	Noted. The text in this section was provided by Ch9, and we expect a considerable revision in collaboration with Ch9 for the SOD
13816	34	39	34	41	I think it is sufficient to say that the results are braodly consistent with AR5. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
13814	34	41	34	44	I suggest removing this last sentence as it appears to be an afterthought [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We expect a considerable revision for the SOD
51866	34	49	35	22	While laudible to try to coordinate across chapters ceding in toto the assessment to chapter 9 feels very odd. It is surely necessary for chapter 3 to undertake a meaningful assessment of the aspects within the chapter's charge? It cannot be expected of a reader to refer to chapter 9 for the assessment and it is important that chapter 3 undertake an assessment suffiucient to support a resulting assessment finding. [Peter Thorne, Ireland]	Taken into account. On glaciers and ice sheets, we agreed that model evaluation, belonged in Chapter 9, since it relies on distinct models from the cliamte models used in the rest of the chapter, and is process-based. Attribution is covered in Chapter 3.
52646	34	51			Isn't the accumulated snow the firn? Currently it reads as if it is additionally transformed to become firn. [Douglas Maraun, Austria]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD.
31454	35	2	35	2	GlacierMIP outside of CMIP: I am not sure whether it ais really a relevant information to add that this or another project is part of CMIP or not. Who really cares? But if you think that this kind of information is relevant, you might add in this case that GlacierMIP is a WCRP project that has been aligned with CMIP. [Gerhard Krinner, France]	Noted. An issue with GlacierMIP is that simulation output are not readily available for analysis, unlike the growing CMIP6 archive which is replicated across the global ESGF federation. It is this reason that more quantitative assessment was not included in AR6 FOD
50856	35	11	35	22	There is evidence on anthropogenic impact on Siachen glaciers (mostly due to armed forces activity at a geo-politically sensitive border). Sample reference - Gilany, S.N.A. and Iqbal, J., 2016. Geospatial analysis of glacial dynamics in Shigar and Shayok Basins. [Sejuti Basu, India]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD. Thanks for pointing out this additional study
50858	35	11	35	22	There is also evidence of permafrost melting as a result of 40 years anthropogenic impact along oil and gas pipelines in Siberian Arctic. Sample reference - Petrzhik, N., Matyshak, G., Myshonkov, A. and Petrov, D., 2017, April. Arctic ecosystem reaction on permafrost melting as a result of 40 years anthropogenic impact. In EGU General Assembly Conference Abstracts (Vol. 19, p. 1560). [Sejuti Basu, India]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD. Thanks for pointing out this additional abstract
7294	35	25	36	17	The new estimates of ice sheet changes must be related to CMIP6 output in a figure and/or table. This should not be left to Chapter 9. [Bryan Weare, United States of America]	Taken into account. We anticipate considerable updates relating to the ISMIP6 component of CMIP6. From these simulations more quantitative insights should be available to address this comment in the SOD
51868	35	25	36	17	This section does not clearly state either what the AR5 findings were or what the new assessment finding is which is at odds with style elsewhere. At a minimum it would be good to more clearly pull out one or more conclusions couched in the uncertainty / confidence language here. [Peter Thorne, Ireland]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD.
47192	35	25			Suggested additional reference : Mouginot et al. (2019) about recent trends in the Greenland ice sheet mass balance [Hervé Douville, France]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD. Thanks for pointing out this new study

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
13852	35	27	36	17	I am a little confused about the content of this section and I think it would benefit from some increased synergy with 9.4. I think that the statement relevant for chapter 3 is that detection and attribution for ice sheets is challenging (paragraph 2). In the first paragraph, I think the introduction to ice sheets is covered in chapter 9, the historical projects before ISMIP6 are not necessary for the reader and the details of coupled ice sheet models will be in the modelling annex. In paragraph 2, I think the discussion of ice sheet models is covered in 9.4 and could be referenced here. I'm not sure what paragraph 3 is trying to assess. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section was re-written to better align with Chapter 9 in the SOD.
7400	35	27			substantiate with literature [Chukwuma Anoruo, Nigeria]	Taken into account. Considerable updates were applied during the SOD and FGD preparation, with more comprehensive literature coverage.
46640	35	43	35	43	No conclusions given on D&A for ice sheet mas loss [WGI TSU, France]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD
49076	35	53	36	1	This sentence is difficult to follow and should be rephrased [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD.
54998	36	3	36	17	Several references of Aschwanden et al. from 2013 and 2016 are used while more recent references on the Greenland ice sheet may also be integrated into the synthesis of scientific findings as additional evidence, particularly Aschwanden et al. (2019) Science Advances 5(6), eaav9396, DOI: 10.1126/sciadv.aav9396 on “Contribution of the Greenland Ice Sheet to sea level over the next millennium” that is published 19 Jun 2019. [Kilkis Siir, Turkey]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD. Thanks for pointing out this additional study
45496	36	7		17	Chech the accuracy of the text. Is it about evluation? [David Baguma, Uganda]	Accepted. We expect a considerable revision for the SOD
31458	36	17	37	17	It would be nice if this section could end with an assessment of what (little) we can say about the human role in observed ice sheet changes. [Gerhard Krinner, France]	Taken into account. The human role in ice sheet changes was more thoroughly assessed in the SOD (and subsequently in the FGD).
29200	36	22	36	35	Indian Ocean missing, could be mentioned why [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted. No CORE-II publications focused on the Indian Ocean, and therefore no appropriate citation exists
13818	36	22	36	47	I think that the content of these two paragraphs could be reduced referring to chapter 9 on OMIP and resolution. For example, discussion of resolution is a key point in 9.2 and highlighted at the start of 9.2.1. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We expect a considerable revision in collaboration with Ch9 for the SOD and so cross-chapter duplication should be minimized with appropriate pointers to specific sections in other chapters

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
49174	36	37	36	47	As well as OMIP, HighResMIP introduces coupled models with ocean resolutions down to 8km resolution (and more broadly 25km), also with higher vertical resolution. I'm not sure why the focus is only on ocean models here. In addition, the paragraph talks about ocean-only models, and then cites Griffies et al (2015) which is a coupled model resolution hierarchy. I think this should be made clearer, and hence it could then include other papers that have done similar studies - Small et al. (2016), Roberts MJ et al. (2019, submitted). Small, R. J., Justin Small, R., Bacmeister, J., Bailey, D., Baker, A., Bishop, S., Bryan, F., Caron, J., Dennis, J., Gent, P., Hsu, H.-m., Jochum, M., Lawrence, D., Muñoz, E., diNezio, P., Scheitlin, T., Tomas, R., Tribbia, J., Tseng, Y.-. H., & Vertenstein, M. (2014). A new synoptic scale resolving global climate simulation using the Community Earth System Model. Journal of Advances in Modeling Earth Systems, 6, 1065– 1094. https://doi.org/10.1002/2014MS000363. Roberts, M. J., Baker, A., Blockley, E. W., Calvert, D., Coward, A., Hewitt, H. T., Jackson, L. C., Kuhlbrodt, T., Mathiot, P., Roberts, C. D., Schiemann, R., Seddon, J., Vannière, B., and Vidale, P. L.: Description of the resolution hierarchy of the global coupled HadGEM3-GC3.1 model as used in CMIP6 HighResMIP experiments, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-148, in review, 2019. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These Small et al. and Roberts et al. studies are now cited in the SOD.
45498	36	37			what do authors intent to inform readers by presenting that "…with the coming of age…? be specific [David Baguma, Uganda]	Noted. The text for this section was heavily revised, and the queried text is no longer present.
45500	36	43			What is recent comparison? Is there old comparison? Rephrase! [David Baguma, Uganda]	Noted. The "recent" refers to Griffies et al 2015 published after AR5, which contrasts responses across a hierarchy of horizontal resolutions
45502	36	52		53	check the sentence and improve [David Baguma, Uganda]	Noted. We expect a considerable revision in collaboration with Ch9 for the SOD and so cross-chapter duplication should be minimized with appropriate pointers to specific sections in other chapters
38028	37	1	37	1	It might help the reader to expose somewhere in the first two paragraphs the general structure of the section, ie why Tropical ocean is treated separtely before treating the wide global picture. [Jean baptiste SALLEE, France]	Noted. An introduction paragraph is added
13820	37	1	38	16	I think that teach of the paragraphs needs an assessment statement on model capability to represent the mean state. I suggest adding a subheading at the start on Zonal Mean Temperature. I think the ability to simulate the mean state needs to be linked to D&A [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Statement on the model capability to represent mean state is added. A subheading is also added for surface temperature and zonal mean temperature

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
45230	37	1	40	7	Section 3.5.1 Temperature: I think the ordering of this section is a bit "muddled" in places. For example, biases in OHC are discussed on page 37, lines 24-32, and only later in Figure 3.21 are biases in surface temperature discussed/shown. I would suggest organising the sub-sections by variable, starting with surface properties and then discussing the deeper layers - I think this will help the reader follow. It also seems logical to discuss the model limiations for each variable before going on to state the assessed findings (since the evaluation provides context and presumably directly informs the confidence statements?). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The section has been re-arranged accordingly. Model limitation (CMIP6) is added.
46978	37	1	42	46	In SROCC Chapter 5, one of the key findings is that increased upper ocean stratification is one of the important anthropogenic drivers of many of the changes to the ocean biogoechemistry and ecosystems. Increased upper ocean stratification is a robust result in both observations and models of surface-intensified warming and increasingly strong high latitude haloclines. This is such a robust result as to almost be boring, but given that increased stratification is such an important driver of other oceanic consequences, it seems that at least a brief discussion is warranted here. Note that while AR5 discussed observed increases in "thermal stratification" due to limitations in salinity measurement, SROCC Chapter 5 discusses simply "stratification" including both temperature and salinity changes. [Robert Hallberg, United States of America]	Taken into account. Enhanced stratification is now briefly discussed in 3.5.2. This topic is also assessed in more detail in Chapter 9.
11512	37	24	37	32	Seems like the ARGO float data should be explicitly mentioned here particularly for improving the observations of the Southern Ocean. [Roanld Stouffer, United States of America]	Noted. The structure of the text has been changed. The Argo float data is mentioned in section 3.5.1.4 now.
45232	37	27	37	32	The lines dealing with under-sampling of the Southern Ocean and OHC trends estimates being biased low. This is an important point, but I think this discussion might more logically appear in Chapter 2 or Chapter 7? Some repitiion of key points is useful and it could be touched on in the summary assessment paragraph(s). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The lines with under-sampling has been removed
50104	37	38	37	45	The content on the different flavours of ENSO needs to be consistent with the other parts which didn't include the different flavours. [Hong-Li Ren, China]	Rejected. This comment may belong to the modes of variability section
46986	37	42	37	46	The improvments in the ocean simulations have also resulted from improvements in the ocean model numerics and parameterizations, not just adding forcing. Please consider discussing efforts to control common ocean biases via improved ocean models, such as improving parameterizations or reducing numerically induced diapycanl mixing, when describing the improvements in some of the CMIP6 models. The Adcroft et al. description of the ocean in GFDL's new CM4 climate model (2019, submitted to JAMES) discusses this issue explicitly, but I believe that such efforts to reduce ocean model biases with improved numerics or vertical coordinates are common to many other modeling centers. [Robert Hallberg, United States of America]	Discussion on the efforts to control biases is added.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
47194	37	49			What about removing « tropical » from the title and adding a paragraph about the extratropical mean state and the potential biases that may alter the human influence on the extratropical ocean (e.g., North Atlantic warming hole, AMOC) ? [Hervé Douville, France]	Accepted. Suggested change made.
49078	38	5	38	9	This sentence is unclear and should be rewritten [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text for this section was heavily revised, and the queried text is no longer present
29202	38	5	38	13	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected in the SOD.
49176	38	9	38	9	Some evidence from HighResMIP models that tropical SST biases are reduced with increased resolution. Roberts MJ et al. (2019, submitted, reference as above). Caron (in prep) will produce a multi-model figure. The indication from the Vanniere et al. (2018) reduction in tropical Atlantic precipitation bias suggests that a similar reduction in SST bias will be evident. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text has been updated. Assessment of HighResMIP is added
45504	38	9		16	Perhaps, authors may include a period or comma sign for readers to known whether ended or is continuing [David Baguma, Uganda]	Editorial.
29204	38	12	38	16	The Indian Ocean text is very short and besides one 2017 reference, all are pre-2010. there is more recent results out I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The Indian Ocean text has been updated and extended with the CMIP6 results.
51870	38	34	39	41	This segment reads perhaps too much like a review and not an assessment per se and couldlikely be tightened. I'm not clear why the final assessment is only at the very likely level given the complete absence of alternative hypotheses that may plausibly explain the observed increases. When redrafting this I would advise trying to more clearly integrate and compare / contrast lines of evidence so that the final assessment statement is much more directly traceable to your underlying assessment text. Particularly focus on why very likely and not virtually certain is the appropriate finding here and perhaps highlight that explicitly in the revised statement (if, indeed, very likely is the appropriate finding) [Peter Thorne, Ireland]	Accepted/ Noted. The text has been rewritten. Assessment statements are updated.
39118	38	36	38	40	Is it possibel to give more details on the most affected oceans? [JACQUES ANDRE NDIONE, Senegal]	Noted. However, the paragraph has been removed to as it fits better in Chapter 2/7
38030	38	36	38	50	This paragraph should start at line 46 "Observed OHC changes are discussed ..." [Jean baptiste SALLEE, France]	Accepted and updated
45234	38	36	38	50	This paragraph repeats quite a lot of information summarised elsewhere in the report - including Chapter 2 and Chapter 7. I think the text here could be reduced substantially, cross-referencing other chapters, while retaining the attribution statements. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The text has been updated.
13822	38	36	38	50	I think this paragraph could be shortened to avoid repetition [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The paragraph has been removed.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
46982	38	38	38	40	This is an assessment, not a review, and undifferentiated lists of more than about 4 references generally do not add credibility to a statement, but do degrade thre readability of the text. Out of consideration for your intended audience, please consider selecting only the 4 most relevant, readable, or recent references to support this point. [Robert Hallberg, United States of America]	Accepted. However, the text has been removed to avoid repetition.
38032	38	46	38	46	about the virtually certain: note that this is consistent with Chap 2 and 9, but inconsistent with Chap 7 (very high confidence) [Jean baptiste SALLEE, France]	Noted. Primary assessment of observed large-scale OHC changes is in Chapter 2, which we report here.
55000	38	46	38	47	Noticed examples that require proof-reading are not commented while it may be useful to share the need to correct "that that" in this line. [Kilkis Siir, Turkey]	editorial
11514	38	47	38	50	Given the extreme lack of observations below 2km, I am surprised by the high confidence statement for the partioning of the warming vertically in the ocean. [Roanld Stouffer, United States of America]	Noted. The text here is reporting the assessment of Chapter 2. We passed this comment on to Chapter 2 to consider in the preparation of their SOD.
38034	38	52	38	52	Would be great to clarify "upper" (e.g. upper 700 m) [Jean baptiste SALLEE, France]	Accepted and updated
27194	38	52	38	53	Figure 10 of Laloyaux et al (2018) doi: 10.1029/2018MS001273, shows that the ocean heat content seems to follow the 60-70 year cycle, possibly related to Atlantic Multidecadal Oscillation, already published by Schlesinger and Ramankutty, 1994; Ogurtsov et al., 2002; Klyashtorin and Lyubushin, 2003; Loehle, 2004; Zhen-Shan and Xian, 2007; Carvalo et al., 2007; Swanson and Tsonis, 2009; Scafetta, 2009; Akasofu, 2010; D'Aleo and Easterbrook, 2010; Loehle and Scafetta, 2011; Humlum et al., 2011; Chambers et al., 2012; Lüdecke et al., 2013; Courtillot et al., 2013; Akasofu, 2013; Macias et al., 2014; Ogurtsov et al., 2015, Ollila 2017. The anthropogenic contribution, therefore, is questionable. [François GERVAIS, France]	Rejected. Analyses using natural and anthropogenically forced climate models have shown robust anthropogenic caused increase in OHC.
38036	39	3	39	6	Is that chap 2 matters rather chap 3? Might be better to leave in chap 2/7/9 to avoid additional possibilities of inconsistencies, as it does not appear needed here [Jean baptiste SALLEE, France]	Rejected. OHC changes through the past two decades are relevant to Chapter 3 and X-chapter Box 3.1, so we have kept this text here.
11516	39	5	39	6	Given the extreme lack of observations below 2km, I am surprised by the high confidence statement for the partioning of the warming vertically in the ocean. [Roanld Stouffer, United States of America]	Noted. The text here is reporting the assessment of Chapter 2. We have passed this comment on to Chapter 2.
13824	39	8	39	37	I think that these paragraphs need to make assessment statements to support the final summary assessment paragraph [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Assessment Statements are added at the end of the paragraphs.
6487	39	18	39	18	"found" or "predicted"? [Hugh Lefcort, United States of America]	Rejected, the term “found” is more appropriate here. However the structure of the sentence has been changed and “found” is removed
38038	39	18	39	18	About the "is mainly due": Would be good to have an assessment here (confidence level) [Jean baptiste SALLEE, France]	Accepted and updated
49080	39	26			Spelling mistake - greenhouse [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
14462	39	33	39	33	'Whereas,---'. This should be joined with the previous sentence as '- - - fluctuations (Weller et al., 2016) whereas - - -' [Muhammad Mohsin Iqbal, Pakistan]	Accepted, text corrected as suggested
45236	39	39	39	41	I think this statement on Anthropogenic influence on OHC needs to be consistent with the headline summaries from Chapter 2, Chapter 7 and elsewhere. Since OHC accounts for >90% of the total warming, this point is closely related to our understanding of radiative forcing. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text updated.
45238	39	44	40	5	I can find no reference to Figure 3.22 in the text. This is a useful figure that should be discussed. I would also like to see some brief summary of the related Gleckler et al paper and the closely related works by Cheng and co-authors. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. All figures were regenerated for the FGD, and all new figures are now discussed in text. Gleckler et al. (2016) and Cheng et al. (2017) are now assessed.
49082	39	51			om? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Comment not clear
37586	40	24	40	25	SSS does provide a direct feedback on the atmosphere through sea-salt aerosols, which act as condensation nuclei and also have some direct effect on radiation. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text updated to include reference to the indirect radiative feedbacks of sea-salt aerosols
49368	40	24	40	25	To revise statement "Unlike sea surface temperature (SST), simulated sea surface salinity (SSS) does not provide feedback to the atmosphere". Recent studies (e.g. Grodsky 2012, Reul, et al. 2014) suggested there might a feedback between SSS and the atmosphere in Hurricane conditions. This was later supported by model studies suggesting that Hurricanes crossing over freshwater plumes (i.e. low SSS) may experience hurricane intensification of about 50% (Vincent, et al, 2012, Balaguru, et al., 2014). Suggested references: Balaguru, K., G. R. Foltz, L. R. Leung, and K. A. Emanuel (2016), Global warming-induced upper-ocean freshening and the intensification of super typhoons, Nature Communications, 7, 13670, doi: 10.1038/ncomms13670 http://www.nature.com/articles/ncomms13670#supplementary-information. Balaguru, K., P. Chang, R. Saravanan, L. R. Leung, Z. Xu, M. Li, and J.-S. Hsieh (2012), Ocean barrier layers' effect on tropical cyclone intensification, Proceedings of the National Academy of Sciences of the United States of America, 109(36), 14343-14347, doi: 10.1073/pnas.1201364109. Grodsky, S. A., N. Reul, G. Lagerloef, G. Reverdin, J. A. Carton, B. Chapron, Y. Quilfen, V. N. Kudryavtsev, and H.-Y. Kao (2012), Haline hurricane wake in the Amazon/Orinoco plume: AQUARIUS/SACD and SMOS observations, Geophysical Research Letters, 39, doi: 10.1029/2012gl053335. Reul, N., J. Tenerelli, B. Chapron, D. Vandemark, Y. Quilfen, and Y. Kerr (2012), SMOS satellite L-band radiometer: A new capability for ocean surface remote sensing in hurricanes, Journal of Geophysical Research-	Noted. Text updated to include reference to the indirect radiative feedbacks of sea-salt aerosols.

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13826	40	24	40	34	I think the process discussion in this paragraph would be better in 9.2.2.2 [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The coordination with Ch9 is a work in progress, and we expect a considerable revision in collaboration with Ch9 for the SOD which should homogenize content among the chapters, in addition to balance out page counts
49084	40	25	40	27	This sentence is very difficult to follow - in fact the whole paragraph should really be re-written. [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text for this section was heavily revised, and the queried text is no longer present.
46980	40	36	40	44	Zika et al (2018) identify increased upper-ocean thermal stratification inhibiting vertical mixing as being responsible for a significant portion (Chapter 9 says 1/3) of the observed changes in sea-surface salinities. This a new driver of salinity changes that is not discussed in AR5, but the physics behind this is robust, and it should perhaps be noted here despite appearing in just a few papers (so far as I know). The reference is Zika, J. D. et al., 2018: Improved estimates of water cycle change from ocean salinity: the key role of ocean warming. Environmental Research Letters, 13 (7), 074036, doi:10.1088/1748-9326/aace42. [Robert Hallberg, United States of America]	Noted. The Zika et al 2018 reference is assessed in section 3.5.2.2.
51872	40	47	41	28	Is it intentional that there is no summary assessment statement to this sub-section? [Peter Thorne, Ireland]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD
29206	40	52	40	52	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This was a formatting issue and won't be repeated in the SOD
13828	41	4	41	28	These pargraphs need confidence statements on model capability to represent mean state (see also comment on temperature regarding mean state and D&A) [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The coordination with Ch9 is a work in progress, and we expect a considerable revision in collaboration with Ch9 for the SOD which should homogenize content among the chapters and add statements about fitness for purpose, in addition to balance out page counts

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
49370	41	18	41	28	The Climate Change Initiative (CCI) is an ESA funded programme, which aim to create the best quality satellite data products from all the existing Essential Climate Variables (ECV), incluting both SST and SSS. This newly produced dataset may aid the understanding of the current model biases, i.e. especially those primarly linked to the precipitation field. For more information and reference see http://cci.esa.int ; Suggested reference: Sea Surface Salinity. Suggest citing the following (also to clarify the differenece between Salinity and Sea Surface Salinity): Reul N., Arias M., Boutin J., Catany R., Chapron B., D'Amico F., Dinnat E., Donlon C., Fore A., Fournier S., Grodsky S.A., Guimbard S., Hasson A., Kolodziejczyk N., Lagerloef G., Lee T., LeVine D., Lindstrom E., Maes C., Mecklenburg S., Meissner T., Olmedo E., Sabia R., Turiel A., Tenerelli J., Thouvenin-Masson C., Vergely J.L., Vinogradova N., Wentz F., and Yueh S. (2019) Sea Surface Salinity estimates from Spaceborne L-band radiometers: an overview of the first 9 years of observations (2010-2018). Remote Sensing of Environment, Special issue on 50 years of Sea Surface Salinity, in review [Rafael Catany, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Extensive assessment of observations is out of scope for Chapter 3.
45506	41	33			See the comment about the word "conclution" at the beginning of the section. [David Baguma, Uganda]	Comment not clear. Ignored
13830	41	40	42	4	These pragraphs need assessment statements to support the summary paragraph [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Now that the SROCC has been released, we expect a considerable revision in collaboration with Ch9 for the SOD
29208	41	42	41	45	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This issue was resolved in the FGD production
29210	41	55	41	55	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This issue was resolved in the FGD production
49086	41	55	42	4	Very unclear. [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The text for this section was heavily revised, and the queried text is no longer present.
29212	42	3	42	4	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This issue was resolved in the FGD production
49372	42	6	42	7	Add reference and evidence that near surface and subsurface salinity change driven by human activities have occurred across the globe since the mid 20th century. What data it aws used? And how reliable this data was? [Rafael Catany, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The previous paragraph outlines numerous detection and attribution (D&A) studies that have positively detected human influence on near-surface and subsurface salinity changes across the globe. Please see Ch2 for further insights for observed changes and the data that underpins these estimates
7402	42	6		7	substantiate with literature [Chukwuma Anoruo, Nigeria]	Taken into account. Considerable updates were applied during the SOD and FGD preparation, with more comprehensive literature coverage.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
37588	42	22	42	23	In the caption of Fig. 3.23, there is reference to "analysis period 1950-2008" following a reference to the "1950-2000 climatological mean". It is not clear why this is mentioned. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The presented analysis was published in Durack and Wijffels (2010; https://doi.org/10.1175/2010JCLI3377.1) and this analysis assessed the observed period from 1950 to 2009 (April), but presented observed changes as a 50-year trend so these could be directly compared to simulations from CMIP3 (1950-1999).
52648	42	56			add “only” before “partially” [Douglas Maraun, Austria]	Accepted. Revised as suggested.
52650	43	1	43	6	Please rewrite in an accessible style. This sentence is insulting [Douglas Maraun, Austria]	Accepted. Rewritten now.
49088	43	1	43	6	This sentence is too long so the meaning is lost. [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rewritten now.
13832	43	11	43	18	I think that this text belongs either in chapter 4 or chapter 9 [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentences focused on regional changes deleted.
32948	43	12	43	14	An additional reference might be Carson et al (https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-14-00359.1) who looked at several CMIP5 models [Aimee Slangen, Netherlands]	Noted.
29214	43	22	43	22	write sea as Sea [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
13834	43	22	43	34	Please refer to box 9.2 for the components. The historical context of GLACE and GSWP3 seems unnecessary [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been dropped. A reference to Box 9.2 is included earlier now.
12640	43	24	43	34	Important to note how integral the changes to ice sheets are to fully understanding the potential for SLR, especially when considering that SLR will be even more dramatic for non-linear changes to ice sheets, and why the incorporation into the models is crucial to drawing a more complete picture of climate change projections. [Kristin Campbell, United States of America]	Rejected. The implications for projections is beyond the scope of this chapter.
46984	43	24	43	34	Some mention of simulations of thermosteric sea level rise is warranted in this paragraph, even if just to note that uncertainties in CMIP5 were relatively small. [Robert Hallberg, United States of America]	Accepted. Text added
12794	43	24	43	34	Important to note how integral the changes to ice sheets are to fully understanding the potential for SLR, especially when considering that SLR will be even more dramatic for non-linear changes to ice sheets, and why the incorporation into the models is crucial to drawing a more complete picture of climate change projections. [Durwood Zaelke, United States of America]	Rejected. The implications for projections is beyond the scope of this chapter.
27732	43	32	43	32	the year is missing (Hock et al.) [Poot Delgado Carlos Antonio, Mexico]	Editorial.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
51874	43	39	44	5	Similar to the comment on the ES, this focus upon the lower bound of the attributable change has been abused by misinformers serially. I would strongly suggest redrafting this to concentrate upon the best guess of the attributable change (presumaly close to that observed) and also mentioning the upper-bound of the attributable component (presumably greater than the observed change) to avoid the potential for abuse. Personally I find the repeated allusion to percentages unhelpful to the assessment here and would remove but I fully recognise this is a matter of personal taste. The section also feels unduly short and may benefit from being given more space. [Peter Thorne, Ireland]	Accepted. Text revised accordingly.
45240	43	39	44	16	Section 3.5.3.2 - Sea Level Change. Cross-check against Chapter 9 to avoid duplication. I can find no reference to Figure 3.25 in the text. I think we need to decide where the attribution of sea level should sit in the report and simply state the summary findings in Chapter 3 if the discussion more logically sits in Chapter 9? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account.
13836	43	41	44	5	This paragraph needs to be compared with section 9.6.2.4 with a summary produced here. The statements are inconsistent with virtually certain here and very likely in 9.6.2.4, half observed change here and dominant cause in 9.6.2.4. [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised as suggested.
52652	43	42			“successfully” sounds a bit prejudiced to me. Or is this justified because slr is such a clear indicator of climate change? [Douglas Maraun, Austria]	Accepted. The word "successfully" has been deleted.
55234	43	50		53	[pt 1 of 2] The text says, "Slangen et al. (2016) considered all quantifiable components of the sea level budget and showed that anthropogenic forced changes account for 69 ± 31% during 1970 to 2005, whereas natural forcings combined with internal climate variability have a much smaller impact only contributing 9 ± 18% of the change over the same period." It disappoints me that many people take seriously these "studies" done solely with unverifiable computer models. That paper caused many chuckles. Since the authors were from CSIRO, I annotated a NOAA graph of sea-level at Australia's longest tide gauge, to illustrate the findings of that paper: http://sealevel.info/680-140_Sydney_2016-04_anthro_vs_natural.png Now, why do you suppose they didn't didn't include a graph like that in their paper? :-) [cont'd] [David Burton, United States of America]	Rejected. This section deals with global mean sea-level trends which are not directly comparable to trends from individual tide gauges . The words "global mean" have been added after "components of the" to improve the clarity.
55236	43	50		53	[pt 2 of 2] Seriously, though, the climate models are NOT good enough for this sort of analysis. It shouldn't be in the Report, but if you include it you should at least note that its conclusions are unverifiable from actual evidence. ### [David Burton, United States of America]	Rejected. The section 3.5.3.1 addresses the model simulations of the components of the sea level budget.
32950	44	1	44	14	Thanks for including this figure, as we won't have a SL D&A figure in Ch9. Just need to check that figure, numbers and confidence assessments are agreed upon with Ch9 (Kopp/Slangen), to ensure cross-report consistency [Aimee Slangen, Netherlands]	Taken into account.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
55238	44	3		5	The text says, “we assess that it is virtually certain that anthropogenic activities are responsible for more than half the observed sea level change since the 1970s.” (it's nonsense -- see cmt immediately above) [David Burton, United States of America]	Taken into account. This sentence has been revised.
53350	44	4	44	4	I suggest replacing "activties" by "emissions of greenhuse gases" [Jan Fuglestvedt, Norway]	Taken into account. 'anthropogenic activities' replaced by 'anthropogenic forcings'. Note that anthropogenic forcings includes aerosol changes as well, so changing to 'anthropogenic emissions of greenhouse gases' would change the meaning and is not directly supported.
13838	44	21	44	25	Reference 9.2.4 here [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	This chapter subsection has now been referenced and the following text has been appended to an existing sentence ", while noting that the dynamical understanding of these circulation changes and circulation changes occurring at smaller scales are reported in Chapter 9.2.4"
7404	44	21		22	substantiate with literature [Chukwuma Anoruo, Nigeria]	Accepted, an appropriate citation has been added (e.g. Buckley and Marshall 2016).
46644	44	28	44	28	Ch2 and Ch9 are referenced in general. More precise references are 2.3.3.4.1 (Oceanic Overtuning Circulation) and 9.2.4.1 (Atlantic Meridional Overtuning Circulation) [WGI TSU, France]	Accepted and updated
33110	44	28			It might be worth mentioning the problem that the models, for the most, part do a poor job at simulating the AMOC for the Last Glacial Maximum (e.g. Muglia and Schmittner, 2015, GRL, 42, 9862-9869). The CMIP5/PMIIP3 models predict stronger and deeper AMOC, and the paleo proxy data is not consistent with that (e.g. Lynch-Stieglitz et al., 2007, Science, Lynch-Stieglitz, J., 2017, Annual Reviews of Marine Science, 9, 83-104). This is a long-standing problem, with the previous model intercomparison (PMIP2) also showing strong, deep AMOC during the LGM (Otto-Bliesner et al., 2007, GRL, 34). [Jean Lynch-Stieglitz, United States of America]	Accepted. An additional sentence has been added to the revised comment that briefly describes perceived model biases during past climatic periods.
27010	44	28			This section has a lot of overlaps with similar sections in chapters 2, 4 and 9. Please reduce overlap and check for contradictions. It seems to me that this section should focus on evidence for forced changes [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Noted
52654	44	28			Consider adding the paper Reintges et al., Clim Dynam, 49:1495-1511, 2017. It demonstrates the huge uncertainties in projections, which is relevant also for the simulation of past trends. [Douglas Maraun, Austria]	Noted, but not directly relevant for our chapter. It has been passed onto CH9.
13840	44	30	44	32	This description of the AMOC seems unnecessry here [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We believe this very brief introduction to the AMOC is required for the non expert that decides to read our chapter without reading Chapter 2.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
49090	44	34	44	36	This sentence could be simplified. [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, this sentence has been simplified and now reads "AR5 concluded that while climate models suggested that an AMOC slowdown would occur in response to anthropogenic forcing, the short observational AMOC record precluded it from being used to support this model finding. "
49178	44	39	44	39	Within PRIMAVERA-HighResMIP we aim to submit a multi-model assessment of AMOC and model resolution before the deadline. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted
38040	44	45	44	45	That seems inconsistent, or at least too strong statement compared with chap 2/9, e.g. Chap 9 says "The timeseries is still too short to discern a trend, given the surprisingly large variability (Figure 9.9) displayed compared to CMIP5 models (Roberts et al., 2014). Smeed et al. (2018), however, argue that between 2007 and 2011 the AMOC shifted to a state of reduced overturning, now transporting 2-3 Sv less water northwards than in 2004-2007 (Figure 9.9)." I would rephrase :"these measurements have shown a shift in AMOC strength with a decrease transport of 2-3 Sv from 2004-2007 to 2007-2011. [Jean baptiste SALLEE, France]	Accepted, the text has changed in the manor suggested but has also been updated.
49180	44	46	44	46	I presume this text will be updated to reflect the update on RAPID-MOCHA data where the last few years have shown a slight recovery in AMOC. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted, yes this text has been updated to reflect the most recent data.
29218	44	49	44	51	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this was a formatting issue that won't be repeated in the SOD.
49438	44	50	44	54	One hypothesis for the shallowness of the AMOC in most CMIP5 models is the numerical mixing in overflows in z-coordinate models. This is supported by a study (Wang et al, 2015) which compares the overflows and AMOC in GFDL's CM2M and CM2G models (the physical components of the ESM2M and ESM2G models). The isopycnal coordinate model CM2G has a deeper AMOC, which becomes shallower if mixing is explicitly increased. See Wang, He, Sonya Legg, and Robert Hallberg, February 2015: Representations of the Nordic Seas overflows and their large scale climate impact in coupled models. Ocean Modelling, 86, DOI:10.1016/j.ocemod.2014.12.005. [Sonya Legg, United States of America]	Noted, this comment has been passed onto Chapter 9 who present a more process based understanding
29216	44	53	44	54	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this was a formatting issue that won't be repeated in the SOD.
38042	44	53	44	54	I don't think chap 2 states it this way as a trend per decade. Chap 9 neither. More as a shift in state with no sign of a trend given the short record. I think it would be better (and probably more accurate anyway) to stay on Chap2/9 line. [Jean baptiste SALLEE, France]	Accepted, the wording has been altered to remove reference to the trend per decade

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
13842	45	1	45	25	Each paragraph needs an assessment statement. The paragraph on stability could be combined with preceding paragraph. Please check consistency with 9.2.4.1 [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	However, the comment about each paragraph having an assessment statement has been rejected. Our assessment statements are all provided in the final paragraph of the subsection.
29220	45	5	45	8	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this was a formatting issue that won't be repeated in the SOD.
27012	45	6	45	6	it is thought' suggests to me that there is evidence in publications - please provide reference. Alternately use 'it may be' [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, sentence has been modified as suggested.
54692	45	12	45	12	should be Undorf et al. (2018a) [Sabine Undorf, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, reference has been corrected
38044	45	16	45	17	Isn't that Chap 4 matters? [Jean baptiste SALLEE, France]	Accepted, this sentence has been modified to refer to historical simulations
38046	45	18	45	20	This sentence by itself is chap 2 matters. Please consider with merging with attribution comments made para below (line 34-38 same page). [Jean baptiste SALLEE, France]	Accepted, the sentence has been altered to better fit with the current scope of our chapter
49182	45	22	45	22	Roberts MJ et al (2019, submitted) shows how large the Atlantic salinity bias with depth can be in a low resolution model with potential consequences for AMOC stability and strength. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, we discuss potential sources of the reported systematic biases of subsurface salinity.
27014	45	27	45	28	I don't think 2.3.4.1 say this. I think the weakening has been shown to be statistically significant, however it is unclear whether the changes seen are from variability [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, however, this sentence has been removed to avoid repetition
38048	45	27	45	28	This is confusing. It is coming back to RAPID observation as 4 para above, plus appears inconsistent with what is said above [Jean baptiste SALLEE, France]	Accepted, this sentence has been removed. Substantial changes have been made to the text to improve the flow and to avoid repetition.
49092	46	6	46	9	Unclear [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text has been modified.
38050	46	6	46	17	All this appears to be chap 2 matters. [Jean baptiste SALLEE, France]	Noted, we have updated our text taking into account CH2 text, trying not to be overly repetitive and only providing information required for our assessment.
49094	46	16	46	17	Poor grammar [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, text has been altered accordingly
13844	46	26	46	45	This seems to have a lot of repetition with 9.2.4.2. I suggest that this paragraph focuses on detection and attribution studies, referring to 9.2.4.2 for process discussion [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this paragraph has been removed.
38052	46	26	46	45	the title of the section is Southern Ocean *circulation*. I would advice to change the title or restructure the section to aligh with the title. [Jean baptiste SALLEE, France]	Noted, this paragraph has been removed.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
13846	46	47	46	53	I think that this paragraph could just be a sentence referring to 9.2 where resolution is discussed [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, we believe that this very brief description provides information for the reader that does not want the level of details provided in Chapter 9.
49184	46	49	46	49	"ocean mesoscale is poorly represented" - it would be much more accurate to say that the ocean mesoscale is wholely parameterised in the Southern Ocean for almost all CMIP5/6 standard models. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Suggested text change accepted
46648	46	49	46	49	Ch9 is referenced in general. More precise reference is 9.2.4.2 [WGI TSU, France]	Accepted and updated
13848	46	55	47	4	Ice shelves seems unnecessary detail here given amount of text devoted to this in 9.2 and 9.4 [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted, reference to ice shelves has been removed.
38054	47	6	47	7	The reference is not appropriate. Chap 2 does not cover SO circulation in FOD. (though I hope they will change and address it in future drafts) [Jean baptiste SALLEE, France]	Noted, Chapter 2 does now briefly introduce Southern Ocean circulation changes, so is referenced accordingly.
13850	47	8			Discussion of future projection is chapter 4 [Helene Hewitt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is a brief mention intended as a fitness for purpose statement. The sentence has been refined in an attempt to make this clearer.
52656	47	14			Wouldn't it make sense to move this section behind the modes of variability? [Douglas Maraun, Austria]	Rejected. The order of the section has been agreed by Chapters 2, 3, 4 and follows multiple considerations.
27338	47	22	47	24	Out of all issues with land surface model, the choice of highlighting temperature responses seems arbitrary to me, given uncertainty in land-use, permafrost dynamics, nutrient cycling etc.. The statement that ESMs to not properly account for temperature responses is ambiguous because it remains unclear here what is the underlying observation for this assessment, or the consequence. [Sönke Zaehle, Germany]	Accepted. This was a mistake, and some sentences may have gone astray. We now write: "Other routine omissions from terrestrial carbon cycle models of components expected to interact with climate change forcings are representations of permafrost thaw (Comyn-Platt et al., 2018), the Nitrogen cycle (Thomas et al., 2015) and its impact on vegetation dynamics (Jeffers et al., 2011), the Phosphorus cycle (Fleischer et al., 2019) and accurate implications of carbon store changes for a range of land use options (Harper et al., 2018)."

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
27340	47	32	47	49	This paragraph seems exclusively focussing on the evaluation of ESMs, whereas the purpose of the chapter is to present evidence AND evaluate ESMs. I suggest to include a couple of key references to observed patterns and dedicated attribution studies to provide more background on attribution of trends, and the (regional) causes of interannual variability for the evaluation of the ESMs. These could include: Sitch, S., Biogeosciences, 12, 653-679, https://doi.org/10.5194/bg-12-653-2015, 2015., Jung et al. Nature 541, 516-520, Humphrey et al. Nature 560, 628-631, but of course there are alternative papers as well. [Sönke Zaehle, Germany]	Accepted. This advice has been adopted in full. We now write: "Data-led studies demonstrate that regional variations in both the trends and the yearly strength of the terrestrial carbon sink are considerable. Datasets of LAI and atmospheric inversions from point CO2 concentration measurements, as well as land simulations, imply that the majority of current terrestrial carbon accumulation is in the tropics (Sitch et al., 2015). Extrapolation of eddy-covariance point measurements of land-atmosphere CO2 exchanges illustrates that for local to regional scales, the dominant control of yearly sink strength is water availability, while at continent to global scale, the main driver is whether it is an anomalously hot or cold year (Jung et al., 2017). The major role of levels of water stored in the ground in influencing land-atmosphere CO2 exchange is reconfirmed through simultaneous analysis of satellite gravimetry and atmospheric CO2 levels (Humphrey et al., 2018)"
27344	47	32	47	49	In addition to what is discussed, I think it is worth highlighting that airborne fraction of CO2 has remained constant between 1960 and 2018, implying that both the land and ocean carbon sink have increased roughly in proportion to the anthropogenic emissions (See Chapter 5 FAQ 1 for example), desite significant interannual variability on land [Sönke Zaehle, Germany]	Accepted. Such an observation-based analysis is in the remit of Chapter 2, but the text has been modified to hint at the result, writing "carbon sinks to be an on-going substantial fraction of emissions".
29222	47	34	47	41	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
52658	47	37			“centered on”, not “centered around” [Douglas Maraun, Austria]	Editorial. Rejected. "Around" implies a degree of approximation that "on" does not.
27346	47	39	47	42	Hoffmann et al, 2013, JGR Biogeo, doi:10.1002-2013JG002381, also assesses this and highlights next to nitrogen, uncertainties in the photosynthetic response to CO2 at global scales, shifts in carbon allocation and turnover, as well as land-use change as other important factors. [Sönke Zaehle, Germany]	Accepted. Reference now discussed.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
28154	47	42	47	49	Wenzel et al. (2016) using in-situ observations of CO2 concentration and Winkler et al. (2019, also supplementary information file) using satellite LAI observations show that the spread in CMIP5 models with respect to photosynthetical carbon fixation (key process in land carbon cycle) is large and many models deviate from the observational constraints. At least for the land carbon cycle, the statement that "ESMs simulate global carbon sinks within the range of observation-based estimates with high confidence" should be revised unless the new CMIP6 ensemble suggests otherwise. [Alexander Winkler, Germany]	Accepted. The CMIP6 simulations give a stronger result and the text has been edited to: "New CMIP6 simulations reconfirm that ESMs estimate global mean land and ocean carbon sinks to be a substantial fraction of emissions. These simulated sinks fall within the range of observation-based estimates with high confidence, provided land models include representation of nutrient limitation."
27342	47	42			Add "net" before carbon uptake, or reword to sink. [Sönke Zaehle, Germany]	Accepted. Sentence rephrased as suggested.
26948	47	43	47	46	By "climate fluctuation" you mean "weather"? "Climate" as a concept strethes over more than a year and thus "interannual variability" in carbon uptake by vegetation is rather not due to climate fluctuation, but influenced by weather, e.g. summer drought. [Joachim Rock, Germany]	Noted.
27362	48	7	48	10	This paragraph should link to AR6 FOD Section 2.3.4.1 [Sönke Zaehle, Germany]	Accepted. Cross-chapter reference added.
27364	48	7			Can there be an assessment of the CMIP projection of growing season length changes highlighted in AR6 FOD Section 2.3.4.6? [Sönke Zaehle, Germany]	Accepted. Although there does not seem to be literature splitting the increasing magnitude of seasonal variation into the components of (1) more growth and (2) long phenology i.e. season length, text has been edited to ensure the reader knows about the two components: "Changes have been observed in both vegetation productivity as well as longer growing seasons (Park et al., 2016).
29666	48	21	48	22	I suggest to support these words by adding the following reference: Winkler, A. J., Myneni, R. B., Alexandrov, G. A. and Brovkin, V.: Earth system models underestimate carbon fixation by plants in the high latitudes, Nat. Commun., 10(1), doi:10.1038/s41467-019-08633-z, 2019. [Georgii Alexandrov, Russian Federation]	Accepted. This is an important new paper that is now cited.
27360	48	30	48	31	This paragraph should link to AR6 FOD Section 2.3.4.5 [Sönke Zaehle, Germany]	Accepted. Cross-chapter reference added.
27348	48	30	48	35	While Mao and Zhu agree, the statement that the large variability in the trend makes a firm attribution difficult is true. Therefore I do not think that the word "confirm" is adequate here, and would suggest using "support". Also the placement of the last sentence in the middle of the abstract is confusing and should appear either upfront, or at the end of the paragraph, where the assessment is formed [Sönke Zaehle, Germany]	Accepted. Paragraph modified as suggested.
39120	48	30	48	41	What's the connexion between this issue and Shael regreening? [JACQUES ANDRE NDIONE, Senegal]	Rejected. The chapter looks at continental and global changes only.
27352	48	30	48	41	note that Chen et al., Nat. Sustainability 2 122-129 (2019) show that greening in India and China is related to land-use change [Sönke Zaehle, Germany]	Accepted. Reference now discussed.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
28156	48	30	48	41	You may want to cite Chen et al. (Nature Sustainability, 2019, doi:10.1038/s41893-019-0220-7), who showed that many regions exhibiting significant greening trends are associated to human land-use management. Partly, they challenge the results by Zhu et al. (2016) who reported that most of the observed greening can be explained by CO2 fertilization. [Alexander Winkler, Germany]	Accepted. Reference now discussed.
36684	48	33	48	33	Mao et al. (2013) should also be added to better support the dominant effects of CO2 concentrations on the land greening. The paper is “Mao J, Shi X, Thornton PE, Hoffman FM, Zhu Z, Myneni RB (2013) Global latitudinal-asymmetric vegetation growth trends and their driving mechanisms: 1982–2009. Remote Sensing, 5, 1484–1497.” [Jiafu Mao, United States of America]	Accepted. This reference is highly appropriate and is now cited.
26950	48	36	48	38	This sentence is incomprehensible. How should LAI changes be influenced by stomatal closure? This feature would be in place anyway and allow plants to profit from increased CO2, but it would only be a factor on ist own if the composition of the plant community changed, too. [Joachim Rock, Germany]	Accepted. The sentence has been rewritten to: "LAI increases attributed to CO2 fertilisation is due to a direct raised physiological response. However, for drylands, CO2-induced stomatal closure may operate to conserve soil moisture to aid photosynthetic capability, and where higher water use efficiency can offset such closure impacts on photosynthesis (Lu et al., 2016)"
27350	48	39	48	42	It is true that these trend attribution cannot be used to evaluate CMIP models, but the attribution of the LAI trend to driving forces does not require a coupled ESM simulation, and can be done (as evidenced by the papers cited in this paragraph), from offline simulations and satellite observation. [Sönke Zaehle, Germany]	Accepted. Wording has been corrected accordingly.
49096	48	43			Grammar - to rather than on [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Accepted.
47196	48	47	48	50	(…) that represent ACCURATELY the whole suite of (…) especially WATER and nutrient limitation and crop intensification, (…) [Hervé Douville, France]	Accepted. However, the suggestion change the meaning because many models to not even have nutrients and crop representation. So the statement has been extended to include "or irrigation effects, with the latter identified through deficiencies in simulated terrestrial water cycling (Yang et al., 2018)"
6489	48	49	48	49	Why low confidence? The data are robust . Just because something can't be modeled does not mean it is not occuring. [Hugh Lefcort, United States of America]	Rejected. The chapter does more than assess whether something is occurring -- it is about attribution to human activities. And modelling is required for attribution.
27196	48	55	48	55	The 2 of CO2 is incorrectly written as an exponent instead of an indice. [François GERVAIS, France]	Editorial. Accepted.
45508	49	9			What do authors intent to information readers by saying “…a great deal of research…” Be specific. [David Baguma, Uganda]	Rejected. “a great deal of research” is a way to say “there has been a lot of work”, we cannot be more specific than that, considering that there is no exact count of the number of work that has been done.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
43304	49	11		14	The sentence should be shortened. [Onema Adojoh, United States of America]	Rejected. Although long, the sentence is clear and would lose its meaning if shortened.
46988	49	34	49	34	Increased upper ocean stratification was identified as one of the primary drivers of oxygen changes in SROCC Chapter 5. This increased time-mean upper ocean stratification is a directly observed signal, and one that directly explains why mixing (for example) is expected to change. Please see SROCC Chapter 5 (5.2.2.4) for a more complete discussion of decreasing oxygen and its primary drivers (including increased stratification) along with useful references. [Robert Hallberg, United States of America]	Taken into account. SROCC reference has been added to the text. However the stratification as a primary driver of the deoxygenation part is not included. Claiming stratification alone as primary driver can be misleading, given the, although connected, impacts of the change in temperature and circulation.
57836	49	37		47	Surface parameters, pCO2(the patial pressure of CO2) has been great contribution to warming of the equatorial biomes, mid Atlantic ocean an area close to the gulf of Guinea, study shown since 2008from the Nigeria, stated and concur or strongly agreed that; there have been persistent warming in the mid Atlantic ocean for the past two decades. An integrated approach to ocean chemistry and oceanographic techniques and research must be implemented to run the actual process of the temperature rise and warning of the ocean. [Abiodun Adegoke, Nigeria]	Noted. Observed warming in SSTs and ocean heat content in assessed in Chapters 2 and 9, including observed patterns of trends. In Section 3.6.2 our focus is on assessing attribution of large-scale changes in ocean biogeochemistry. Recommendations regarding future research are not within scope of IPCC assessments.
55002	50	8	50	15	There are multiple recent studies that could be assessed in relation to statements on ocean acidification, including Kawahata et al. (2019) Progress in Earth and Planetary Science 6(1),5 on "Perspective on the response of marine calcifiers to global warming and ocean acidification-Behavior of corals and foraminifera in a high CO2 world hot house." Referral to section {5.3} on "ocean acidification and de-oxygenation" may also be useful. [Kilkis Siir, Turkey]	Rejected. The suggested paper would be more appropriate for Chapter 2 than chapter 3.
27960	50	26	50	26	It would improve this chapter if a FAQ is spend to include an explanation on what teleconnections are. [roderik van de wal, Netherlands]	Rejected. However, this has been documented in Glossary and Technical Annex.
42026	50	26	62	12	I noticed that the discussions on each mode of variability starts with a textbook style definition in the first paragraph. I imagine that such definition is probably somewhere else in the report such the observational annex. If that is the case, it may be more appropriate to leave out the definition paragraph and start with the summary of the AR5 report. [Hyacinth Nnamchi, Germany]	Rejected. We have the Technical Annex which describes individual modes. Yet, each chapter should be minimum self-descriptive, which means each subsection should begin with a short introduction of the mode.
51876	50	26			Note that the order in which these modes are assessed varies from that in chapter 2. The two should be synchronised? [Peter Thorne, Ireland]	Noted. A coordination framework has been set up to ensure better consistency in the assessment of the modes of variability across Chap2/Chap3/Chap4.
39512	50	28	53	43	Notice that observed changes of both NAM and SAM are assessed in CH2 (sections 2.4.5.1 and 2.4.5.2) while the projected changes are assessed in CH4 (sections 4.3.3.1). Check consistency and coherency across chapters regarding definitions and associated references. [Carolina Vera, Argentina]	Noted. At SOD we have a Technical Annex that defines individual modes and each chapter refers to it.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
46668	50	28	60	12	Assessment on modes of variability occurs in Section 1.3.3; Section 2.4; Section 3.7; Section 4.4.3, 4.5.3; Section 6.2.2.5.1; Section 7.1.1/2 ; Section 8.3.1.3.2, 8.3.2.2, 8.3.2.4.1, 8.3.2.9.1, 8.4.2.5,8.5.2.2.1, 8.3.2.9.2, 8.4.2.5, 8.3.2.9.3, 8.4.2.5, 8.3.2.9.4, 8.4.2.5, Figure 8.43, 8.5.2.2.1, 8.5.2.2.1; Section 9.2.2.1, 9.2.2.3, Section 9.4.3.2, BOX 9.2, 9.2.3.1, Table 9.1, Section 9.2.1, Cross-Chapter Box 9.1, BOX 9.2, 9.6.2.1.1, 9.6.2.1.2, 9.5.4.7, 9.2.5; Section 10.1.4.2, 10.4.2.2, 10.6.3.3; Section 11.3.1, 11.7.1.1, 11.6.2, 11.1.5,11.4.1, 11.6.1, Table 11.4; Section 12.4.1, 12.4.4.3, 12.5.2.3; Section Atlas.5.2.1.2, Atlas.5.3.1.1, Atlas.5.3.2.1, Atlas.5.5.1.1, Atlas.5.5.2.1, Atlas.5.6.2.1, Atlas.5.6.3.1, Atlas.5.10.2.1, Atlas.5.10.2.2. This topic is addressed in ES of Chapter 2, 3, 4, 7, 11, addressed in box in chapter 9, and broadly addressed in above-mentioned subsections in chapter 1, 2, 3, 4, 6, 7, 8, 9, 10, 11, 12. [WGI TSU, France]	Noted. Thanks for this information.
47198	50	28			There are also several model studies suggesting that the positive NAO trend observed in the late 20th century could be partly driven by the observed warming in the tropical Indian Ocean (e.g., Bader and Latif , 2003 & 2005 ; Hurell et al., 2004 ; Douville et al., 2018) although there is only low confidence in such a causal relationship and about the relative contribution of natural variability and anthropogenic climate change to the observed SST warming. [Hervé Douville, France]	Accepted. This possibility is mentioned in the text and there are few sentences about the potential role of the SST upon the variability of the NAO. References prior AR5 are not mentioned though.
52660	50	28			The NAO is closely linked to the mean jet position. I am wondering whether it therefore makes sense to merge this section with the earlier sections on the atmospheric circulation. Then you could move the coupled and ocean modes of variability just after that section. [Douglas Maraun, Austria]	Rejected. It is absolutely true that the NAO is tightly linked to the position of the jet. However, the structure of the assessment is such that the description of modes of variability includes more than dynamical aspects and we prefer to have a separate section devoted to global-scale dynamical entities (like the jet. Hadley cell, storm tracks etc.). That said, a technical annex has been added in SOD including a more complete description of the mode. We denoted the relationship between the NAO and the jet in the annex.
29224	50	51	50	52	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
45510	50			70	Use these comments to improve the rest the chapter [David Baguma, Uganda]	Not applicable
13918	51	7	51	26	A related concern is that climate models systematically underestimate the level of multidecadal variability in the observed NAO / jet stream (Simpson et al 2018; https://doi.org/10.1175/JCLI-D-18-0168.1 and Bracegirdle et al 2018, https://doi.org/10.1029/2018GL078965) [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The underestimation of the NAO at multidecadal timescale as simulated in CMIP model has been discussed and taken into account in the model evaluation. References have been added accordingly.
52662	51	8			consider using “internal variability” instead of sampling issues. [Douglas Maraun, Austria]	Taken into account. Sentence changed as "internal variability" was indeed incorrect

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29226	51	11	51	11	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
27962	51	13	51	14	Page 3-51, lines 13-14 says that since the mid-1990 NAM/NAO trends are mostly negative. At first this seems contradictory to line 7 which says that the NAM and NAO have a positive trend over 1951-2011. To avoid this, the sentence on lines 13-14 can be expanded to say: 'Despite the overall positive trend over 1951-2011, the NAM/NAO trend since the mid-1990 are mostly negative.' [roderik van de wal, Netherlands]	Accepted. Revised as suggested.
52664	51	14			Trends in what? [Douglas Maraun, Austria]	Taken into account. This sentence has been revised.
49098	51	20	51	22	This sentence needs to be reworded [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This sentence has been removed.
30640	51	21	51	22	no need of those old references, as in fact you are assessing later origin of contradictory results [Annalisa Cherchi, Italy]	Accepted. We have removed the sentence.
43306	51	29		24	Same as page 49 [Onema Adojoh, United States of America]	Accepted. We have split the sentence into two.
29228	51	37	51	38	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
26272	51	40	51	50	It would be good to mention the signal-to-noise paradox (Scaife and Smith 2018: A signal-to-noise paradox in climate science. Npj Climate & Atmos Sci., 1, 28). [Masahiro Watanabe, Japan]	Accepted. The signal-to-noise paradox is an important issue as also raised by other reviewers. It is now highlighted in the model evaluation part (new paragraph added) and references have been added accordingly.
52666	51	43			“signification”: is this proper English? [Douglas Maraun, Austria]	Editorial. Accepted.
13920	51	47	51	48	Although models have medium to high performance in simulating the NAO and its teleconnections, there is an apparent signal-noise problem as evident in seasonal hindcasts. As discussed by Scaife and Smith (2018, https://doi.org/10.1038/s41612-018-0038-4), this has implications in that the NAO response to external forcing may be too weak in models. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The signal-to-noise paradox is an important issue as also raised by other reviewers. It is now highlighted in the model evaluation part and references have been added accordingly.
52668	51	47			Which statistical features? Please be more informative! [Douglas Maraun, Austria]	Taken into account. We have revised this sentence.
26274	52	37	53	43	It is a bit pity that this subsection reads descriptive and does not explain physical mechanisms by which ozone and/or GHG forcing can induce the positive SAM trend. It would be well recognized in the research community but not for non-experts. [Masahiro Watanabe, Japan]	Noted. Indeed the chapter does not address mechanisms, by design. In fact details of the mechanism are still unclear. Readers will need to examine the specialist literature on this.
37590	52	39			It would be better to change "consists of" to something like "involves". There are wind changes as well as mass changes associated with the SAM. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised as suggested.

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29230	52	40	52	40	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
39122	52	44	52	44	Please, delete the space at the end of the reference (Marshall, 2003) [JACQUES ANDRE NDIONE, Senegal]	Accepted. This has been fixed in the SOD.
29232	52	50	52	50	please provide reference for statement "Based on proxy reconstructions, there was medium confidence that the SAM trend 50 since 1950 was anomalous compared to the last 400 years." [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased the sentence to make even clearer that this was an AR6 finding.
29234	52	54	52	54	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD..
48442	53	4	53	4	and GHGs in other seasons' is incorrect. Most other seasons do not have significant trends so are not attributable. Rephrase to say GHGs act across all seasons [Julie Arblaster, Australia]	Accepted. We have rephrased the sentence to remove any attribution statements for seasons other than summer (when trends (in winter and spring) are insignificant or attribution is less clear.
29236	53	5	53	6	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD..
29238	53	9	53	9	what is high-top and low-top models, is that explained somewhere? [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have included a brief explanation what those concepts mean.
31936	53	9	53	11	The ENSO variability over the past 500-1000 years appears mor closely comparable to the recent decades ENSO variability. This is probably the importan point here. [Marie-France Loutre, Switzerland]	Rejected. This comment refers to Chapter 2.
31934	53	11	53	11	Figure 2.40. What does it refer to? [Marie-France Loutre, Switzerland]	Rejected. This comment refers to Chapter 2.
48444	53	11	53	12	cancel each other other' would indicate no influence of GHGs on the SAM? Add Solomon and Polvani (2016): 10.1175/JCLI-D-16-0034.1 [Julie Arblaster, Australia]	Accepted. There isn't a complete cancellation, but rather a partial offsetting. We have rephrased the sentence. We now cite Solomon and Polvani.
31938	53	12	53	13	What is the reference here? [Marie-France Loutre, Switzerland]	Rejected. This comment refers to Chapter 2.
48438	53	16	53	18	Tropical Pacific SST trends could be externally forced, rephrase. Also add Schneider et al 2015:10.1175/JCLI-D-15-0090.1 and Clem et al 2017: 10.1007/s00382-016-3329-7 [Julie Arblaster, Australia]	Accepted, we have rephrased the sentence. Schneider et al and Clem et al only discuss observational findings that have limited bearing for the attribution statements made here.
29240	53	26	53	28	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is corrected for the SOD..
12642	53	32	53	43	In the updates to come with the CMIP6 results, consider how the SAM changes are affected by the recovery of the ozone layer in the coming decades. [Kristin Campbell, United States of America]	Rejected. Discussing the future is the domain of Ch4. Here we address how ozone recovery (though in its infancy) may have affected the SAM.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
29552	53	32	53	43	The summary of the robustness of SAM trends is suprising to me as compared to statements about trends and variability in northern hemisphere variability pattern such as the NAM, the NAO, AMM, AMOC etc. I am aware of the literature on importance of ODs and GHGs on SAM trends, but I am surprised to see such a clear statement to SH trends as compared to NH trends. Observational records are very short and the distribution of paleo-sides is even more limited on the SH. Also, climate models have a hard time to reproduce the NH and SH variability correctly. I would therefore like to ask for a careful comparison of statements for NH and SH trends. How much is due to internal (natural) variability and how much is of anthropogenic origin? Please also check cross-chapter links, such as to chapter 4 (Figure 4.6). [Katja Matthes, Germany]	Noted. We agree that attribution of any trends in NH modes is more difficult than for the SAM. For the SAM, several evaluations of models participating in activities such as CMIP5, CCMI, and CCMVal have all shown that ozone depletion is a leading driver of a strengthening of the SAM in austral summer since ODSs started to take effect. See e.g. Son et al., Env. Res. Lett. (2018). As for the paleo context, we do not claim that the strengthening is unusual relative to any proxy reconstructions -- we agree these are uncertain. Note that figure 4.6 shows a clear strengthening of the SAM between ~1960 and 2000 but no significant trend for the NAM.
12796	53	32	53	43	In the updates to come with the CMIP6 results, consider how the SAM changes are affected by the recovery of the ozone layer in the coming decades. [Durwood Zaelke, United States of America]	Rejected. Discussing the future is the domain of Ch4. Here we address how ozone recovery (though in its infancy) may have affected the SAM.
48440	53	39	53	57	Ensure synthesis and reduce overlap with Chapter 2, some of this material might fit better there [Julie Arblaster, Australia]	Rejected. These lines are only concerned with model validation and attribution; there is no overlap with Ch2.
48446	53	44	53	44	Add Sherwood and Nishant, 2015: doi:10.1088/1748-9326/10/5/054007 [Julie Arblaster, Australia]	Rejected. Sherwood and Nishant is a purely observational paper; we do not see how a discussion of the paper would help here.
37592	54	4			It would be better to avoid phrases like "the Earth's largest source of interannual climate variability". These things depend on metric and region. Global-mean surface air temperature is lowered by large volcanic eruptions at least as much as it is raised by strong El Nino events. Local surface air temperature variations in the winter Arctic associated with interannual variations in sea-ice cover are larger than the temperature variations over the eastern equatorial Pacific associated with El Ninos. And so on. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence is removed.
29242	54	21	54	25	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
37594	54	22			Is the reference to Guilyardi et al.(2009) the correct one? It is dated too early to include CMIP5 results. If it refers to the models that were in operation prior to 2009, then if they did a reasonable job on El Nino one might expect the same to be true in the actual CMIP5 models. If this pre-AR5 reference is retained, perhaps the wording could be changed a little here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have cited a more relevant paper.
29244	54	44	54	47	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.

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15556	54				3.7.3 El Nino-Southern Oscillation There exist a number of wrong sentences to describe ENSO characteristics. For example, “Observed ENSO amplitude, as measured by the standard deviation of central Pacific SST anomalies, along with its 2-7 year time scale” (line 20-21) ENSO events are often synchronized to the seasonal cycle in the observations, with central/eastern Pacific El Nino and La Nina SST anomalies tending to peak in boreal winter (November-January) and tending to be at their weakest in the boreal spring (March-April) (Harrison and Larkin, 1998; Larkin and Harrison, 2002) (line 41-44) Therefore, I think this section should be carefully revised. [SANG-WOOK YEH, Republic of Korea]	Accepted. These sentences have been revised.
14442	55	8	55	8	The word 'of' before ENSO is suggested to be deleted. [Muhammad Mohsin Iqbal, Pakistan]	Editorial. Accepted.
54224	55	15	55	17	See: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2018GL079764 [Nicola Maher, Germany]	Accepted. This paper has been cited.
46676	55	15	55	18	Assessment on response of ENSO variability to external forcing is inconsistent with assessment in Section 12.5.2.3 [WGI TSU, France]	Taken into account. We have revised the text so that this assessment is on the past changes, while Section 12.5.2.3 discusses future changes.
13248	55	20	55	26	As for the asymmetry of ENSO: ENSO phase asymmetry is not limited for amplitude but for durations (Ohba and Ueda 2009; Okumura et al. 2011; Ohba et al. 2010). This point should be also expanded and enhanced in here. Ohba, M., and H. Ueda, 2009: Role of Nonlinear Atmospheric Response to SST on the Asymmetric Transition Process of ENSO. J. Climate, 22, 177--192, doi:10.1175/2008JCLI2334.1. Okumura Y.M., M. Ohba, C. Deser, and H. Ueda, 2011: A proposed mechanism for the asymmetric duration of El Nino and La Nina. Journal of Climate, 24, 3822-3829. 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. [Masamichi Ohba, Japan]	Taken into account. We have added a discussion of "duration" to the ENSO asymmetry metrics, citing a paper. Note that our focus is on recent literature since the AR5, therefore we have not added citations to the papers listed here.
49186	55	26	55	26	Roberts CD et al. (2018, as above) suggests that at least some CMIP6 models may be able to better represent the asymmetry. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have cited this paper.
29246	55	33	54	34	new 2019 paper Freund et al., 2019 Nature Climate Change by on CP type ENSO from corals past 400 years to cite here in next version [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have cited this new literature.
30642	55	44	56	8	how is this paragraph related with the scope of the chapter: human influence should be discussed [Annalisa Cherchi, Italy]	Noted. This model evaluation is necessary for detection and attribution.

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43198	55	47	55	49	In this context valuable to point out that the traditional PSA has some ambiguity around how it is defined. Irving et al., 2016: A new method for identifying the Pacific-South American pattern and its influence on regional climate variability. Journal of Climate, 29, 6109–6125, doi: 10.1175/JCLI-D-15-0843.1 have recently presented a dynamic definition for this SH phenomenon [Ian Simmonds, Australia]	Taken into account. We have cited this paper.
29248	56	5	56	5	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
30644	56	27	56	27	"clear" from what? [Annalisa Cherchi, Italy]	Taken into account. We have rephrased the sentence.
30646	56	27	56	36	How is this related to human influence? Not relevant in this chapter, better placed in ch 9, eventually [Annalisa Cherchi, Italy]	Rejected. Model evaluation is necessary as a basis for detection and attribution.
52670	56	27	56	36	This is a very important paragraph, and it should be placed in a more prominent position. It should also be discussed what the consequences of misrepresenting key feedbacks mean for projections of ENSO. Currently it reads a bit like the small print of an insurance contract. [Douglas Maraun, Austria]	Rejected. We intentionally place this paragraph immediately before the assessment summary on ENSO, to highlight its importance.
42020	56	27	56	56	The too weak Bjerknes feedback, too weak heat fluxes statement seems to imply that ENSO dynamics is localized to the equatorial Pacific. I think that more caution is needed here as there could be some roles for teleconnections within and outside the Pacific. I did not read the papers quoted but there could be some roles for multi-decadal modulations through for instance the phasing of (e.g. Atlantic Multidecadal Variability, etc) in the models that are not entirely consistent to observations. I think that some statement of uncertainty will be appropriate here. [Hyacinth Nnamchi, Germany]	Accepted. We have added some references to inter-basin connections including Cai et al (2019).
15558	56	38	56	44	3.7.3 El Nino-Southern Oscillation It is necessary to reflect more literature which showed more evidence on the increase of CP El Nino activity based on paleo-climate period (please see Freund et al. Nature Geoscience, 12, 450-455 (2019) [SANG-WOOK YEH, Republic of Korea]	Accepted. We have cited this new literature.
27964	57	24	57	25	Page 3-57, lines 24-25 say that a systematic bias is compensated by other biases, resulting in a realistic IOB magnitude. From the way it is worded, this makes it seem like this justifies using the unrealistic representation with the systematic bias included. Therefore, we suggest rewriting this such that it mentions clearly that despite showing a realistic IOB, it should not be mistaken for a correct representation since there is still a systematic bias (if this is the key message). [roderik van de wal, Netherlands]	Taken into account. The fact that the realistic representation of the IOB mode is due to compensation of biases is considered in the overall assessment of detection and attribution. This is clarified in the summary statement.
46670	58	5	58	6	‘there is no evidence that anthropogenic forcing has changed the interannual IOB and IOD’ , is no evidence a correct use of IPCC calibrated language? [WGI TSU, France]	Taken into account. Rephrased by "there is limited evidence" without italicizing.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
42022	58	14	59	14	There is a recent review on the tropical Atlantic climate system that should be relevant to this section (Foltz et al., 2019, The Tropical Atlantic Observing System. Front. Mar. Sci. 6:206. doi: 10.3389/fmars.2019.00206). The mean state biases are very topical question for this region and I think that is relevant to discuss the primary studies that assessed the biases in the CMIP3 (Richter, I. & Xie, SP. Clim Dyn (2008) 31: 587. https://doi.org/10.1007/s00382-008-0364-z) and CMIP5 (Richter, I., Xie, SP., Behera, S.K. et al. Clim Dyn (2014) 42: 171. https://doi.org/10.1007/s00382-012-1624-5) models. The lack of model fidelity on the tropical Atlantic mostly for the mean state; However the multi-model ensemble tends to look similar to observations in most studies. Indeed, it remains an open question the extent to which the mean state biases affect variability and predictability. So rather than attribute lack of confidence in the possible human influence to model biases, I would suggest a more cautious discussion that includes other sources of uncertainty such as aerosols (Booth et, 2012, Nature, 228-232,doi:10.1038/nature10946) and multi-decadal modulations (Svendsen et al., Clim. Dyn, (2014) 43: 2931. https://doi.org/10.1007/s00382-013-1904-8; Martín-Rey et al., 2018, J. Climate,31, 515–536, https://doi.org/10.1175/JCLI-D-16-0459.1). [Hyacinth Nnamchi, Germany]	Taken into account. All the references have been added except the Richter et al. (2008) related to CMIP3 following our editorial choice to limit our citation to the most recent ones past AR5, and the Svendsen et al. (2014) one dealing with interannual cross-basin interaction while the focus here is on multidecadal variability. Following the suggestion, the justification of the lack of confidence in attribution studies has been broaden and the original sentence has been rephrased accordingly.
49190	58	37	58	37	AMM also plays an important role in Atlantic tropical cyclone variability (many papers, but Roberts MJ et al. for example shows this). Roberts, M. J., P. L. Vidale, M. Mizielinski, M.-E. Demory, R. Schiemann, J. Strachan, K. Hodges, J. Camp, R. Bell, 2015: Tropical cyclones in the UPSCALE ensemble of high resolution global climate models. J. Clim special issue on Hurricanes, 28, 574–596. http://dx.doi.org/10.1175/JCLI-D-14-00131.1. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reference has been added together with some words on the representation of the tropical cyclones.
49100	58	42	58	44	This sentence does not make sense. [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The "cold (warm) north (south)" formulated has been changed to avoid confusion
27734	58	55	58	55	correct the following paragraph teleconnectivity over land (Sahel monsoon, Steinig et al., 2018), [Poot Delgado Carlos Antonio, Mexico]	Accepted. Sahel monsoon has been replaced by West African Monsoon throughout the text for consistency.
40512	58	55			Is the Sahel monsoon here the same as the West African monsoon? Stick to a uniform terminology if possible. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. "West African monsoon" is preferred to "Sahel" and changed accordingly for consistency throughout the entire section
49188	59	1	59	1	The individual HighResMIP papers Roberts CD et al. (2018), Roberts MJ et al. (2019, submitted) begin to confirm this, and the Caron et al. (in prep) will include the multi-model summary plots. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reference has been added and a placeholder note to check results from HighResMIP literature when available
46672	59	4	59	5	"no robust evidence" is not a correct use of IPCC calibrated language. [WGI TSU, France]	Taken into account. Italic has been removed to avoid confusion.
43308	59	19		24	Same as page 49 and 51 [Onema Adojoh, United States of America]	Accepted. We have shortened the sentence.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
33320	59	24	59	25	Not everyone would agree with the statement on lines 24-25. Importantly, the one cited reference is about PDO in particular, not PDV in general. [Erika Wise, United States of America]	Taken into account. We have revised the sentence.
29250	59	36	59	36	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
37596	59	43			"short instrumental observations" needs changing, as it is the time over which instrumental observations have been made that is short, not the observations. Something like "the short period for which instrumental observations have been made" would do, though "short observational records" (used later) could be used here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised as suggested.
29252	59	45	59	46	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
29254	59	51	59	51	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
15560	60	1	60	13	3.7.6 Pacific Decadal Variability As described in this paragraph, there exists an inter-basin interaction of PDV and AMV. However, there is no description on the change in the PDV-AMV relationship on the low-frequency timescales. This belongs a natural variability and it is necessary to mention how such changes are associated with human influences. [SANG-WOOK YEH, Republic of Korea]	Taken into account. There is possibility that human influence have contributed to the AMV (Section 3.7.7), which can indirectly drive the PDV. We have revised the sentence to clarify this.
49102	60	3			aliased?? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Rephrased as "projects onto".

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
7474	60	5			PDO controls regional climate in East Asia, so anthropogenic forcing contributing past PDO evolution is important. Boo et al(2015) showed anthropogenic aerosol emissions amplify PDO response by aerosol effect , reported that “The historical simulations using the Hadley Global Environment Model version 2 show that there is a common externally forced component in relation to the twentieth century North Pacific SST variability.” The process is explained by both direct and indirect aerosol influences, and the aerosol-cloud interactions affecting surface energy budget. It is consistent with NAO showing that anthropogenic aerosol emission could modulate the natural variability. Then it needs to be cited between Line5 and Line6. Line5-6 says “Anthropogenic aerosol emission has contributed to the negative PDV trend; however a response is not robust across models ”. But advanced result, even with one model, is needed to add. Then I suggest to add the following sentences at the end of Line 5; “Boo et al (2015) studied that there is a common externally forced component in relation to the twentieth century North Pacific SST variability.” reference) Boo, K.-O., B. B. B. Booth, Y.-H. Byun, J. Lee, C. H. Cho, S. Shim, and K.-T. Kim (2015), Influence of aerosols in multidecadal SST variability simulations over the North Pacific, J. Geophys. Res. Atmos., 120, 517–531, doi:10.1002/2014JD021933 [Kyung-On Boo, Republic of Korea]	Taken into account. Potential aerosol influence on the PDV has been already written, and this paper is additionally considered there.
26276	60	8	60	8	The term AMV appears here, before it is defined in 3.7.7 [Masahiro Watanabe, Japan]	Noted. Actually, AMV appears in Section 3.7.1, where we cite Section 3.7.7.
39124	60	15	60	19	This paragrah is scientifically very strong; it looks fine but needs some improvements. We'll be happy to know what CMIP6 brings as novel results regarding this issue. [JACQUES ANDRE NDIONE, Senegal]	Rejected. CMIP6 last millennium simulations have not become available yet.
55004	60	21	60	21	The statement on "any detectable changes in PDV" can be assessed considering such references as the invited review article of Henley (2017) in Global and Planetary Change 155: 42-55 on "Pacific decadal climate variability: Indices, patterns and tropical-extratropical interactions." The review synthesized studies on the role of PDV in the climate system while underlining the need for "intense research focus on PDV observations, palaeoclimate and modelling." [Kilkis Siir, Turkey]	Rejected. Henley (2017) is already cited and taken into account in the assessment.
54808	60	21			you conclude that there is low confidence in human influences on PDV- for me it sounds like you would expect some changes but are not sure you see them. Do we really expect changes? I don’t (but then I might have missed the paper) - if you want to say there is presently little evidence that human influences have significantly altered the PDV i would phrase it differently than you have [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the sentence as "low confidence on whether …"
52672	60	21			Please replace “that” by “on whether”. “That” sounds prejudiced. [Douglas Maraun, Austria]	Accepted. Revised as suggested.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
54812	60	28			appear to underestimate' - is it significant? I am not convinced that models underestimate NAO longterm variability Carley (Iles cite) found trends in controls over multiple decades but we didn't do a full significance test. Variance can be quite variable over a century or so so unless its statistically tested i would tune that down [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have tested significance of variance ratio for CMIP5 and CMIP6.
27198	60	55	62	25	Please cite and discuss the paper published recently by Gan et al doi.org/10.1029/2018EA000443 about the "slowing down", actually a cooling, in Figure 1 of the paper, related to AMO. [François GERVAIS, France]	Taken into account. This section does not address regional multidecadal teleconnections in detail. Instead this reference has been added in Cross-Chapter Box 3.1.
54810	61	11	61	20	There is also a paper by Mueller et al showing something similar: Müller W. A., D. Matei, M. Bersch, J. H. Jungclaus, H. Haak, K. Lohmann,G. P. Compo, and J. Marotzke (2014): A 20th-century reanalysis forced ocean model to reconstruct North Atlantic climate variation during the 1920s, Climate Dynamics. doi:10.1007/s00382-014-2267-5 [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. From ocean-forced simulation, it is impossible to extract/isolate the forced versus internal signals because both are included in the forcing by construction. This reference is more relevant for ocean processes tackled in Chap 9.
42024	61	11	61	20	Further support for internal ocean dynamics for the Atlantic Multidecadal Variability is provided by some observational analyses (McCarthy et al., 2015, Nature, 508-510;doi:10.1038/nature14491) and (Gulev et al., 2013; Nature,464-499, doi:10.1038/nature12268). [Hyacinth Nnamchi, Germany]	Rejected. Discussions based on observation only are treated in Chap2. Papers dealing with observations are cited in Chap3 only when relevant for model evaluation, but not for processes per-se.
27016	61	11	61	20	It may be worth mentioning somewhere that Will et al, 2019 show some of the disagreement between forced/internal variability comes from different ways of defining AMV [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A sentence has been added to question the relevance of the traditional definition of the AMV metrics based on the most recent studies of Wills et al (2019) + the Haustein et al (2019)
13916	61	15	61	16	The evidence for a crucial role of ocean dynamics is broader than this, eg Zhang et al (2013; https://doi.org/10.1175/JAS-D-12-0331.1) and O'reilly et al (2016; https://doi.org/10.1002/2016GL067925). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. References added in the text. Thanks for your suggestion.
39126	61	20	61	20	Please, add ";" between these references "Brown et al, 2016" and "Martin et al, 2014" [JACQUES ANDRE NDIONE, Senegal]	Editorial. Accepted.
52674	61	20			This is very euphemistic. If feedback representations are specific to each model, than most models will have biases in these representations. Please state that clearly, and also spell out the consequences. [Douglas Maraun, Austria]	Taken into account. The word "representation" was misleading and we replaced it by "feedbacks (cloud, barotropic versus baroclinic local reponses to SST anomalies, etc.), whose respective weight…"
52676	61	27			Give reference for the variability statement. [Douglas Maraun, Austria]	Taken into account. References added (Bracegirdle et al. 2018) and sentence changed. "In addition" was misleading and replaced by "For instance". Additional recent references related to coupled processes have been also added (baker et al. 2017, Woollings et al. 2018)

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
29256	61	31	61	32	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. This is a known issue reported by the TSU.
29258	61	34	61	48	new paper by Haustein et al. 2019 in Journal of Climate confirms role of anthropogenic forcing since early 20th century on AMO/AMV [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Ref added in the paragraph describing the forced component of the AMV. A sentence has been also added to question the relevance of the traditional definition of the AMV metrics based on the most recent studies of Wills et al (2019) + the Haustein et al (2019)
26278	61	34	61	48	There is a more recent paper that attempts to reconcile the external driving of AMV and an internally generated AMO (Watanabe and Tatebe 2019: Reconciling roles of sulphate aerosol forcing and internal variability in Atlantic multidecadal climate changes. Clim. Dyn., doi:10.1007/s00382-019-04811-3) [Masahiro Watanabe, Japan]	Accepted. Reference added in the text + a sentence introducing the probable interplay between internally and external-driven response in the Atlantic
51718	61	44			Spelling mistake - atmosphere [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Accepted.
51720	61	45			would question the word stationarity -possibly reword this sentence [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The word "stationarity" has been removed and the sentence rephrased in part.
51878	62	28	63	8	Use of both virtually certain and clear in such a cheek-to-jowl fashion is not necessarily helpful for clarity here? [Peter Thorne, Ireland]	Rejected. "Clear" is a quote from AR5. "Virtually certain" is our assessment of the likelihood that humans have caused global warming. The two statements are not quite referring to the same assessment statement, and are not in contradiction. In our ES statement, human influence on climate is still "clear".
6333	62	28	63	8	How do we measure the effectiveness of initiated regional and global climate models? [Isaac Sarfo, Ghana]	Noted. As the chapter does not assess "initiated" climate models used for short-term predictions, it is unclear what this comment is referring to.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
27326	62	32	62	36	Sentence "evidence has grown….observed changes since 1950's...attributable to antropogenic influences" is not a fair summary of the literature. The important role of natural variability as key driver for the observed temperature changes is not enough taken into consideration. In chapter 3.7, temperature variations-oscillations of 1-2 °C are described as being caused by mostly natural variability due to mainly ocean circulations (and teleconnections with atmosphere) . In chapter 3.7 we find statements that observed temperature variations-oscillations cannot be attributed to antropogenic effects (low confidence) . In Chaper 3.7 there is no mention that CO2/GHG signal is main driver for the temperature changes. [ferdinand meeus, Belgium]	Rejected. Surface temperature is indeed subject to some variability, although the direction of change is clear. Variability is much reduced when global ocean heat content is considered. For this there is much reduced variability and a clear warming trend during the instrumental period. Climate models can only reflect those changes if anthropogenic drivers are taken into account, i.e. the global-mean temperature cannot be explained in terms of natural variability. The statements about temperature oscillations are taken out of context. Indeed there are natural modes of variability that cause such regional variations, that are not forced by human influence. On a technical point, the lines in question refer to earlier parts of the assessment. Therefore they are cited here without giving full evidence.
29260	62	37	62	38	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
7406	62	42		43	substantiate with literature [Chukwuma Anoruo, Nigeria]	Noted. Reference to earlier parts of the chapter is added where this conclusion is reached. The sentence does not refer to external literature but to the assessment of temperature trends earlier in the chapter. A specific reference to this effect is added.
7408	62	52		55	the statement is not far from line 32-37. the pages convey the same. Is there still need for the paragraph? [Chukwuma Anoruo, Nigeria]	Rejected. This is a formal assessment statement which we feel is still needed (and is repeated in the Executive Summary).
53352	63	6	63	6	Chapter 5 should be corrected to Chapter 7 [Jan Fuglestvedt, Norway]	Accepted. We have corrected this error.
30648	63	13	63	15	CMIP6 historical cover spread of changes including CMIP5 hist-nat: sensitivity is lower or differences in forcings? [Annalisa Cherchi, Italy]	Noted. The figure quality has been improved so any differences between CMIP5 and CMIP6 can be more easily discerned. We do not know whether it is differences in models or differences in forcings that drive this.
51882	63	20	65	51	There is a potential issue here in that this section is without reference pretty much at all to the literature. Given the task of IPCC is to synthesise and assess this is a potential issue. It could be argued that this is new and novel research which may be construed as out of scope. It would be better I think to at least more fully reference literature about the underlying methods but also, of course, having results papers would help. I just flag this issue here for due consideration by the chapter team. [Peter Thorne, Ireland]	Noted. Reference to literature is now improved. The calculation is using an established method used before for AR5 (which is fine according to the IPCC guidelines). We anticipate more literature using CMIP6.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
27966	63	45	63	46	Page 3-63, lines 45-46 say that CMIP5 multi-model mean generally performed better than individual models. It would improve this section to have a discussion on why this is and if there are no studies on this subject a sentence could be included explaining that we do not know the cause of this. [roderik van de wal, Netherlands]	Rejected. This is a general finding that AR5 and AR4 have already demonstrated (so we have understood the cause of such behaviour for several years now). We now additionally cite Annan and Hargreaves (2011) who discussed this for CMIP3.
51880	63	55	64	56	I assume it is some relevant subset of the datasets linked. Is this made obvious in the annex? Should the text be modified here to make it explicit? [Peter Thorne, Ireland]	Noted. Which datasets are actually used is now made explicit in the figure caption. Discussing this in detail in the text would run counter to the idea of this section, which is to assess model performance in a multivariate sense.
8658	63		65		"3.8.2.1 In agreement with previous assessments, the CMIP5 multi-model mean generally performed better than individual models (Rougier, 2016). ... In accordance with AR5, the multi-model mean, with one notable exception, is better than any individual model (Rougier, 2016). ... ...performs better than most individual models (Rougier, 2016)." I think it would be a good idea to remind readers that this is the expected result. This was already proven before AR5 so need to cite the old papers - just a nuance in the language required (In case it's not clear what I am referring to: eg. Annan and Hargreaves 2011, JClim). [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We assume that added the reference (Annan and Hargreaves, 2011), conveying the idea that this is well established.
29262	64	8	64	8	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
29264	64	19	64	19	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
29266	64	25	64	25	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
37598	64	34			It is a bit strange to read "Little progress was found for fields that were well simulated". This is hardly a surprise, as if something is well simulated there is less room for improvement than for something that is poorly simulated, and model devlopers will give priority to more problematic matters. Perhaps the word "quite" could be inserted before "well simulated". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. But the point is valid that improvement is only possible for measures that are not well simulated. We have rephrased this point.
53354	64	39	64	39	typo: rlut, [Jan Fuglestvedt, Norway]	Accepted. Comma removed after "rlut".
30650	64	45	64	56	fig 3.38 panels a,b,c are described but not clear what is what (also resolution is very low) [Annalisa Cherchi, Italy]	Accepted. We are improving the labelling so it becomes a little clearer what is displayed here.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
45018	65	4	65	22	Much of this information about the boundary conditions for the paleo reference periods (PMIP time slices) is already in cross chapter box 1.3, which was developed so that multiple chapter could refer this information. I suggest omitting most of this section, referring to box 1.3, and instead using the space for other priorities. [Darrell Kaufman, United States of America]	Noted. Actually box 1.3 does not cover two of the four periods introduced here. If box 1.3 actually covers all of the four periods, a shorter section would indeed suffice. Or alternatively, if the latter two periods are dropped because insufficient simulations are available to cover them.
37600	65	8			For consistency of terminology with Chapter 1, "preindustrial levels" should be changed to "the pre-industrial baseline". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We follow the reviewer's recommendation.
31940	65	9	65	10	the cross reference should be checked here. Reconstruction of past evolution of Greenland (9.4.2.1), Antarctica (9.4.3.1), sea level (9.6.2.1) [Marie-France Loutre, Switzerland]	Accepted. We have added a reference to section 9.4.3.1, covering Antarctica.
29268	65	35	65	36	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
29270	66	13	66	14	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
49192	66	16	66	16	HighResMIP-PRIMAVERA is working on a manuscript on tropical cyclones and model resolutions, as well as other extreme events (with Chapter 11). Caron et al. (in prep) will give more evidence when overviewing the HighResMIP simulations, using standard metrics packages. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We are looking forward to assessing this paper when ready.
39128	66	26	66	32	We've strong added value in this paragraph thanks to CMIP6 data. [JACQUES ANDRE NDIONE, Senegal]	Noted. It is unclear which change the reviewer is advocating for.
52678	66	34			Why does this paragraph appear in a section on processes? [Douglas Maraun, Austria]	Noted. It is not clear which change the reviewer is advocating for. Nowhere does this chapter deal directly with processes -- these are considered in later chapters.
37602	66	36	66	37	See comment 170 regarding the reference to "little apparent improvement" in CMIP6 to something that was "well simulated" in CMIP5. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have removed this half-sentence as it is redundant.
29272	66	52	66	52	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is fixed in the SOD.
51884	67	13	68	49	I'm not sure that a reader wouldn't expect this section to be in Chapter 4 rather than chapter 3. Is some discussion between the two chapters in this regard warranted? [Peter Thorne, Ireland]	Accepted. This topic is now covered in Chapter 4.
47200	67	13			Recent attributable trends in the instrumental record may also represent increasingly powerful emergent constraints on climate projections (e.g., Knutti et al., 2017 for September sea-ice or Douville and Plazzotta, 2017 for boreal summer mid-latitude surface relative humidity over land) although internal climate variability generally remains a major obstacle at the regional scale. [Hervé Douville, France]	Not applicable. This topic is now covered in Chapter 4.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
52680	67	13			I found this section a bit weak. It should link explicitly to the discussions of adequacy for purpose in the other Chapters (I guess in Chapter 1, definitely in Chapter 10 for regional aspects). We somewhere (Chapter 1, here?, Chapter 4?) need an in-depth discussion on how we get from present-day performance to credibility of projections. Emergent constraints are just one idea with a lot of caveats. There is much more scope for discussing process-based evaluation. [Douglas Maraun, Austria]	Not applicable. This topic is now covered in Chapter 4.
37604	67	25			In this line, and in line 27, reference is made to "observations". Maybe this is OK, but model performance is often evaluated against griided data , including reanalyses, that are derived from observations. So I wonder whether "observational datasets" would be better than "observations" in these two lines. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This topic is now covered in Chapter 4.
52682	67	29	67	32	Does this sentence only apply to past changes? Then it could be justified. But it is so unspecific that it could also apply for future changes. Then it would not be justified without substantial additional discussions and a clear line of argument how one gets from performance in present climate to credibility of projections. [Douglas Maraun, Austria]	Not applicable. This topic is now covered in Chapter 4.
29274	68	6	68	6	error message start of sentence in my pdf "Erreur ! Source du renvoi introuvable" Ref missing I believe [Jens Zinke, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been corrected for the SOD.
28800	68	6	68	10	Our assessment of ECS doesn't support the emergent constraint appraoch as valid for the FOD, this may change of course! But need to check consitency [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This topic is now covered in Chapter 4.
31674	68	8	67	10	This Chapter cites a single study on ECS to "propose a best estimate of 4 K." when Chapter 7 current gives a range with 4 at the upper end. This discussion must be consistent with Chapter 7 [John Dunne, United States of America]	Not applicable. This topic is now covered in Chapter 4.
49194	68	27	68	27	"…more high-resolution models that capture small scale processes and extremes". Since through the chapter we have evidence that higher resolution can address key, longstanding model biases, to then seemingly relegate this to improving small scales and extremes seems very strange. [Malcolm Roberts, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This topic is now covered in Chapter 4.
27332	68	30	68	32	Sentence "….suitable for policy-relevant climate projections" is only valid if also more explicit information is given about the tuning-calibration-parameterisation of the CIMP5-6 . In order to make informed and better decisions, policymakers need more transparancy on this complex modelling aspect. The modelling community (Voosen, Science 2016 and Hourdin 2017) has clearly stated that nearly every model has been tuned-calibrated to the 20th century climate records. Same model outcome with often conflicting tuning-parameterisation technique and with perfect fit to historical climate records is not a good basis for confidence in the future projections for 2050 and later. [ferdinand meeus, Belgium]	Not applicable. This topic is now covered in Chapter 4.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
53356	68	31	68	31	I would not only mention CO2. Instead of mentioning more components, you can just say "emissions" [Jan Fuglestvedt, Norway]	Not applicable. This topic is now covered in Chapter 4.
54814	68	32			here is a sad little gap about imlications on predictions from detection and attribution see earlier comment [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This topic is now covered in Chapter 4.
31676	68	37	68	47	I am concerned that the current discussion highlights proposed emergent constraints in single papers rather than focussing on what can be said about our confidence through synthesis of literature. In this figure (3.42) I was expecting to see plots of robustly/mechanisticly linked observational and models properties or other directly connected observables such as in Using the current seasonal cycle to constrain snow albedo feedback in future climate change. Geophys Res Lett. 2006;33:L03502. Here the left plot shows ECS which is not an observable, and the right plot a CO2 metric with a highly speculative relation to climate sensitivity without any strong mechanistic relation. Rather than pointing out that speculative hypotheses have been proposed, I would suggest the authors highlight emergent constraints that have been confirmed as robust derive from multiple studies and that relate observables mechanistically (e.g. clouds, ice, etc) to observable sensitivity and leave the more speculative connections to idealized sensitivity to Chapter 7. [John Dunne, United States of America]	Not applicable. This topic is now covered in Chapter 4.
27736	68	43	69	46	subscript (CO2) [Poot Delgado Carlos Antonio, Mexico]	Accepted. The font is corrected.
8260	68	52	69	30	It should meantion the effects of urbanization on surface sir temperature and surface wind speeds. [Zong Ci Zhao, China]	Rejected. This chapter's scope is restricted to global / continental scale indicators.
37606	69	2	69	3	"austral summers of 2017 and 2018" can now read "austral summers of 2017, 2018 and 2019". At least that's what's found from calculations based on either NSIDC data or OSISAF data as processed for use in the ERA5 reanalysis. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
37608	69	19			Change "Short instrumental observations" to "Short Instrumental data records" or something similar. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
52684	69	31			This might be a place to add a discussion about the difficulties in simulating and attributing anything related to circulation aspects as discussed by Shepherd, Nature Geoscience, 2014. This is a key knowledge gap. [Douglas Maraun, Austria]	Noted.
57334	69	33	72	46	Really? Do we actually need another "hiatus box"? With hindsight, it was given far too much weight in AR5 -- there must be a temptation to devote even more space to it to try and undo the damage, but it just gives hostages to fortune, feeding the notion that "we don't really know why it happened". Yes we do. Variability. VERY dangerous to do some kind of poor-man's initialised forecast by looking at auto-correlation of trends in CMIP6. Strongly urge you not to do this, it is a hostage to fortune. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. AR6 Annotated outline suggests Chapter 3 to re-assess the hiatus. Several other reviewers make supportive comments on this box, while no one else suggests its removal.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
28804	69	36	70	20	Great box, good to mention GSAT, but how siginificant is GSAT for attribution in rest of chapter and cited studies, I would like to see this discussed. However, as ES never really quantifies trends you are likely fine :) [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We compare model GSAT and GMST trends in the text.
35148	69	36	71	39	Consider replacing existing text with the following: Slower Surface Global Warming over the Early 21st Century 37 38 Contributors: Christophe Cassou (France), John Fyfe (Canada), Nathan Gillett (Canada), Edward Hawkins 39 (UK), Yu Kosaka (Japan), Blair Trewin (Australia) 40 41 The observed rate of global mean surface temperature (GMST) increase was lower from the late 1990s to early 42 2010s compared to the preceding decades and to the ensemble mean of historical simulations produced by climate models of the process-based type from both CMIP5 (extended by RCP scenarios beyond 2005) and CMIP6. (See Chapter 1 Section 1.4.3 Climate models for a description of the range and typology of models used in climate science). This apparent slowdown of surface global 44 warming, often called the “hiatus”, was assessed with medium confidence to have been caused in roughly equal 45 measure by a cooling contribution from internal variability and a reduced trend in external forcing (particularly 46 associated with solar and volcanic forcing) in the AR5 (Flato et al., 2013). In the AR5 it was assessed that 47 almost all CMIP5 simulations did not reproduce the hiatus, and that there was medium confidence that the 48 difference in trends was to a substantial degree caused by internal variability with possible contributions from 49 forcing error and model response uncertainty. This Cross-Chapter Box assesses new findings from both statistical and process-based models on trends over	same as #28132

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
28132	69	36	71	39	Consider replacing existing text with the following: Slower Surface Global Warming over the Early 21st Century 37 38 Contributors: Christophe Cassou (France), John Fyfe (Canada), Nathan Gillett (Canada), Edward Hawkins 39 (UK), Yu Kosaka (Japan), Blair Trewin (Australia) 40 41 The observed rate of global mean surface temperature (GMST) increase was lower from the late 1990s to early 42 2010s compared to the preceding decades and to the ensemble mean of historical simulations produced by climate models of the process-based type from both CMIP5 (extended by RCP scenarios beyond 2005) and CMIP6. (See Chapter 1 Section 1.4.3 Climate models for a description of the range and typology of models used in climate science). This apparent slowdown of surface global 44 warming, often called the “hiatus”, was assessed with medium confidence to have been caused in roughly equal 45 measure by a cooling contribution from internal variability and a reduced trend in external forcing (particularly 46 associated with solar and volcanic forcing) in the AR5 (Flato et al., 2013). In the AR5 it was assessed that 47 almost all CMIP5 simulations did not reproduce the hiatus, and that there was medium confidence that the 48 difference in trends was to a substantial degree caused by internal variability with possible contributions from 49 forcing error and model response uncertainty. This Cross-Chapter Box assesses new findings from both statistical and process-based models on trends over	Taken into account. We have revised the introductory paragraph considering the suggested edits.
7410	69	41		43	substantiate with literature [Chukwuma Anoruo, Nigeria]	Rejected. The trend differences are assessed throughout this box and do not need references here. Instead we cite the corresponding figure.
42980	69	47	69	47	Change "hiatus" to "slowdown" for consistency. As noted in Fyfe et al 2016, "hiatus" was unfortunate framing. The first reference to ' "hiatus" ' should stay as it was the term used in AR5 and other publications. [David Clarke, Canada]	Taken into account. We have rephrased it as "observed slower warming"

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
37610	70	1	70	18	This paragraph highlights the mess created in this FOD by the handling of GMST/GSAT. See comments 5, 6 amd 7 on the entire report, and several comments made on section 2. The numbers quoted from Simmons et al. (2017) are for GSAT from renalyses and GMST from the datasets using gridding directly from observations (HadCRUT4, GISTEMP etc). This is partly because in some comparisons of GMST and GSAT from reanalysis made in that paper the differences were small, albeit systematic when it came to trends. Differences among the various GMST datasets were larger than the differences between GMST and GSAT for the reanalyses. Also, we viewed the gridded GMST datasets as approximations to GSAT datasets, consistent with the view of the creators of GISTEMP, as noted in comment 6. See also comments 179 and 180. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have provided the trend difference between GMST and GSAT in this Box, while citing Cross-chapter Box 2.3 for their definitions.
37612	70	7			The range of 0.06ºC to 0.14ºC/decade could be recomputed for the SOD, based on the updated datasets promised in Chapter 2. Already we know that ERA5 gives 0.14ºC/decade, the same as ERA-Interim, and the new v4 of GISTEMP gives 0.13ºC/decade compared with 0.10ºC/decade for the version used in Simmons et al. (2017). The new v5 of NOAAGlobalTemp gives an unchanged value of 0.08ºC/decade. We still await a new version of the HadCRUT dataset.The 0.06ºC/decade lower limit came from HadCRUT4, and HadCRUT5 will probably give a higher value, but we have to wait a while. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have re-evaluated the trends based on updated observational products.
54818	70	8	70	19	I would give examples for these multiple lines of evidence. Also at the end of this section, I would explicitly mention that even if we allow the response to natural forcings to be much larger than in the models, they still cannot explain the pattern of warming without human influences (ie obliquely relate to scaling) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is a comment on FAQ 3.3. At the end of the section we now state that the response to natural forcings is much smaller than the observed trends.
37614	70	12			The reference to "non-observed locations in the Arctic" should be revised. There are observations there and many are used by the reanalyses to infer surface air temperature. In particular reanalyses use sea-ice cover data along with SST data, so represent the strongly positive surface air temperature anomalies that occur (other than in summer) in regions that had ice-cover in the past but not today. Other observations too constrain the reanalyses. It is the more traditional datasets that combine only SST data with surface air temperature from fixed land stations, and which require sufficient past data to construct station climatologies, that suffer seriously from "non-observed locations". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have rephrased as "locations of missing records in observational products".

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
13082	70	13	70	13	To be comprehensive, text on the impact of temporal sampling (besides incomplete global data coverage) will be placed after '...than the global average (Cowtan and Way, 2014; Huang et al., 2017b).': [A high-frequency sampling of daily mean air temperatures over globe is revealed to improve the description of the recent warming trend (Zhou and Wang, 2016).] Reference: Zhou, C., and Wang, K., (2016). Spatiotemporal divergence of the warming hiatus over land based on different definitions of mean temperature. Sci. Rep., 6, 31789. doi: 10.1038/srep31789. [Zhou Chunlüe, United States of America]	Accepted. We have cited this paper and assessed its influence on GMST trends.
53358	70	13	70	18	Even if given somewhere else in the report, it would be good to indicate magnitude of the contribution form temperature definition to the discrepancy between models and obsevatsions. [Jan Fuglestvedt, Norway]	Accepted. We have compared GSAT and GMST trends in models and provide their difference in the text.
30652	70	15	70	18	not clear: GSAT and GMST have differences in terms of hiatus? [Annalisa Cherchi, Italy]	Taken into account. This sentence explains the preceding sentence that the methodological difference in evaluating global temperature calculation contributed to the model-observation discrepancy. We have clarified this.
37616	70	17			GMST is more complicated than defined here, as what happens over sea ice and around coastlines is dataset-dependent. See comment 102 on chapter 2. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This issue has been assessed in Cross-chapter Box 2.3. Here we cite this box.
37618	70	18			The text needs changing here, as the operational observationally-based reanalysis datasets provide GSAT. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This sentence applies to observational data sets only.
30654	70	20	70	27	not clear: do the models have hiatus? In models GSAT and GMST differ? What is eventually more realistic? What are the main differences between CMIP5 and CMIP6 in terms of hiatus and performance of GSAT and GMST? [Annalisa Cherchi, Italy]	Taken into account. We have provided differences of GSAT and GMST trends in models. Also we compare changes in ensemble-mean trends and ensemble spread between CMIP5 and CMIP6 to assess their relative importance in capturing the observed slowdown.
42984	70	20	70	27	Three of five land-ocean datasets are now spatially complete over 1998-2012, and all five are expected to be so by the time of SOD, according to AR6 Ch 2 FOD. It is therefore more appropriate to forego masking of model series. The main analysis should only cite the percentiles of the spatially complete observational series; it is quite likely that these will all lie within the 5-95 percentile range of the blended model trends. See next comment for further elaboration of the implications of these changes. [David Clarke, Canada]	Taken into account. We have updated HadCRUT4 to HadCRUT5 and included Cowtan and Way data. Yet HadCRUT5 still has data-missing regions, although very narrow.
37620	70	20			"updated observations" should be replaced by "updated observational datasets" or something similar. Some additional observations are involved for some datasets, but it is also the processing of the observations (including in some cases bias adjustments) that has changed. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have revised as suggested.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
30656	70	29	70	29	why not using large ensemble to measure internal variability? [Annalisa Cherchi, Italy]	Rejected. The warming slowdown is partly induced by temporary compensating effects of external forcing and internal variability, and the use of large ensemble simulations of a single model would suffer from biases in forced warming in the particular model, which cannot be assessed in detail here.
30658	70	30	70	30	how/where the deviation is seen? Fig 1 is not so clear in this sense [Annalisa Cherchi, Italy]	Taken into account. We have improved the figure visibility.
35326	70	30	70	34	Another bit of evidence to quantify the relative contributions of internal variability associatd with the IPO to the slowdown is shown by: Meehl, G.A., A. Hu, B.D. Santer, and S.-P. Xie, 2016: Contribution of the Interdecadal Pacific Oscillation to twentieth-century global surface temperature trends. Nature Climate Change, 6, DOI:10.1038/nclimate3107. [Gerald Meehl, United States of America]	Accepted. We have cited this paper in the text.
28120	70	40	70	46	Representation of clouds in climate models. The text refers here to the issue of "resolution of about 1 km needed to realistically represent clouds" which is the only reference I found in the document to the issue of representing subgrid effects of clouds andmore broadly of characterizing clouds at high resolution, although it is possible I missed this elsewhere. AR5 referred to the concern "CMIP5 models continue to exhibit the 'too few, too bright' low-cloud problem (Nam et al., 2012), with a systematic overestimation of cloud optical depth and underestimation of cloud cover." [Sec 7.2.3.4]. It seems that a follow-up discusion is required of this. Characterization of the distribution of cloudiness (cloud fraction, cloud optical depth) is probematic in observations as well as models. As pointed out by (Stubenreich et al 2013) the cloud fraction has been showing apparent increase with time not because it is really increasing, but because of improvement in methods of detection, their figure 1, top.Comparing six surface and satellite products of cloud fraction Wu et al. (2014) showed similar trends in monthly or annual values despite differences of up to 10% or more absolute cloud fraction. Inevitably cloud fraction and cloud optical depth depend on resolution and threshold. Schwartz et al. (2017) showed variation in cloud depth and cloud presence at scales down to a few centimeters. It thus seems that the issue of characterizing the spatial variation of clouds presence and optical depth and obtaining accurate representation of the radiative effects of clouds in climate models remains a first-order problem that should at least be noted and progress reviewed. I am not sure that this is the best place for this, but I call attention to it here because of the above quoted language. Stubenrauch, C. J., et al. (2013), Assessment of global cloud datasets from	Noted. Due to the complexities around evaluating clouds in model simulations and the strong linkages with climate sensitivity, this topic was defined to be out of scope for chapter 3 and is treated in depth in chapter 7.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
13084	71	11	71	11	Providing full citations here: [(Zhou and Wang, 2016; Section 3.7.1)] Reference: Zhou, C., and Wang, K., (2016). Spatiotemporal divergence of the warming hiatus over land based on different definitions of mean temperature. Sci. Rep., 6, 31789. doi: 10.1038/srep31789. [Zhou Chunlüe, United States of America]	Noted.
30660	71	30	71	30	what do you mean by "dynamical GCM"? [Annalisa Cherchi, Italy]	Accepted. We have rephrased as "process-based climate models".
42982	71	34	71	34	Change "hiatus" to "slowdown" for consistency. [David Clarke, Canada]	Taken into account. We have rephrased it as "slower warming"
51886	72	2	72	9	Firstly, I really like this box. Here, however, it would be nice to circle back explicitly to the AR5 finding and note whether other than the massive hike in confidence / likelihood this fundamentally differs from the AR5 finding (o my view it doesn't). It may also be worth more explicitly stressing how the AR5 assessment, being based upon a relative paucity of peer-reviewed analyses was heavily expert judgement based. That may be better earlier in the introductory piece though. [Peter Thorne, Ireland]	Taken into account. We explicitly mention that the AR5 assessment was based on expert judgement.
43310	72	2		5	Same as page 49, 51, and 59 [Onema Adojoh, United States of America]	Rejected. This sentence is not splittable, and the text is clear as it is.
37622	72	2			"GMST observations" should be changed to "observationally-based datasets". GMST is not an observable, so one should not talk about GMST observations. The observables are SST and land-surface air temperatures, and the other variables that reanalyses use to infer surface air temperature. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have rephrased it as "observation-based GMST data sets".
37624	72	3			"GMST" on this line could equally well be "GSAT". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We have mentioned that in and after "internal variability" parts, arguments on GMST trends also applies to GSAT trends.
53360	72	9	72	9	Would be good refer ro more than one paper for this statement. [Jan Fuglestvedt, Norway]	Accepted. We have cited additional papers.
37626	72	11			We are almost half-way through 2019, and it is shaping up warmer than 2018. So as things stand "three" could be changed to "four" in the SOD. But there's still time for the unexpected to happen. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have updated the sentence to include 2019 record.
37628	72	11			"GMST" on this line could equally well be "GSAT". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We have mentioned that in and after "internal variability" parts, arguments on GMST trends also applies to GSAT trends.
37630	72	21	72	22	Should "GMST" be "GSAT" here? Should both be mentioned? Meehl et al. (2013) is dated before the GMST/GSAT distinction became fashionable, and the abstract of the paper begins: "Globally averaged surface air temperatures …". But the abstract of Roberts et al.(2015) refers to "global mean surface temperature". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have mentioned that in and after "internal variability" parts, arguments on GMST trends also applies to GSAT trends.
9278	73	14	73	14	change "corrected", probably to "connected" [philippe waldteufel, France]	Accept. Revised as "correlated".
28922	73	14	73	14	"corrected" should be "correlated" [Matt Tully, Australia]	Accept. Revised as suggested.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
51722	73	22			Spelling mistake - influence [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected.
51724	73	31			Unclear - what are nearly land areas? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected as "nearly all land areas"
52686	73	42			This statement is not true in its generality (see, e.g., Pfahl et al., Nature Climate Change, 2017). Please add “some” or “most” (not sure, please check) before “areas”. [Douglas Maraun, Austria]	Accept. Revised as suggested.
42986	74	1	74	17	Cross-Chapter Box 3.2, Figure 1: Since spatial interpolation of observations is now recognized as best practice (see AR6 ch 2), observations should be compared to blended SAT-SST model series without masking. All spatially complete observational series should be included. The exclusion of Cowtan-Way is particularly puzzling and there appears to be no possible justification for this, especially considering it was the only new dataset included in SR1.5. The main analysis should exclude non-interpolated datasets such as HadCRUT4 and NOAA GlobalTemp, but these could be shown in the figure as dotted vertical lines, alongside the solid lines of the main observational datasets. If the forthcoming HadCRUT5 is spatially complete as seems to be expected according to Ch 2 FOD, the PDF of the ensemble trend could be shown; in this case, HadCRUT4 could still be retained as a salutary reminder that the "hiatus" was largely an artifact of coverage bias. (HadCRUT4 is the only dataset that retains such a slow warming trend over 1998-2012, at 0.05C/decade). Otherwise, the ensemble of Cowtan-Way should be used. [David Clarke, Canada]	[This comment refers to Cross-Chapter Box 3.1] Taken into account. We have included Cowtan & Way dataset, and HadCRUT4 has been replaced by HadCRUT5. HadCRUT5 still has data-missing regions, although very narrow.
45020	75	5	75	18	In paleoclimatology, the influence of natural variability is greater at longer time scales, not shorter. In fact, other than daily and seasonal changes, a spectral analysis of climate time series based on proxy climate observations shows that the amplitude of climate variability scales negatively with the frequency (more power at longer time scales). I think that the issue could be resolved simply by stating that this FAQ addresses climate changes during the industrial era (since 1900). As such, natural forcing by orbital changes and tectonic processes and feedbacks involving long-term changes in ocean circulation and carbon cycling, are negligible. To clarify this, I suggest changing “climate change” to “recent climate change” in the FAQ question itself. [Darrell Kaufman, United States of America]	Accepted. Suggested change made.
7412	75	5		18	Also solar activities influnce on climate possess natural variability like the variation observed in sunspot, aurora and geomagnetic storm. The variation on the navigation systems also natural variability and could be included [Chukwuma Anoruo, Nigeria]	Noted. Solar influence is already included in the discussion of natural influences on climate.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
57838	75	5		36	Anthropogenic process and Natural variability pf climate models and forcing which occurs naturally or by human influence should be drawn or show in a comprehensive table of climate indices or model. A clear understanding of natural variability seperated from the forcing of climate model simulation in relation to anthropogenic or natural origin must be analyse in a table showing the past and future occurences and trends. we have seen occurences where natural variability fluctuates and are greater than the human influence on our climatic system; for example ; 2010 eruption of EYJAFLLAJOKULL in iceland caused serious disruption tovair travel across western and northern europe for more than 10 days between April 14-20th . In view of the natural occurences in relation to human influence, we have fluctuations in majors occurences and future trends of natural variability. [Abiodun Adegoke, Nigeria]	Noted. Aspects of natural variability are assessed throughout Chapter 3, including in Sections 3. 3-3.6, Section 3.7 which assesses changes in modes of variability, Cross-chapter box 3.2, which assesses the role of internal variability in early 21st century temperature change, and Figures 3.4 and 3.5, which show internal variability in temperature.
29292	75	7	75	11	"Natural variability …. Plays a smaller role …" This sentence is obviously incorrect. In the longer timescale, i.e. the glacial-interglacial timescale, the natural variaiblity is dominant. The sentence should state that human induced changes are important at the timescale that is relevant for hte social and economic impacts. [Fabio D'Andrea, France]	Taken into account. This FAQ has been extensively re-written in the SOD.
53364	75	7	75	11	When you say that natural variability plays a small role in the long term, i think you should indicate what long term means here [Jan Fuglestvedt, Norway]	Taken into account. It is now clarified that this refers to the 100-year timescale.
43350	75	8	75	9	Need to be careful about saying natural variability playing a smaller role in "long-term variations", as my immediate thought was the ice age cycles which are all about natural variability. Can oyu make it clear that the discussion is about the last few centuries only? [James Renwick, New Zealand]	Taken into account. Revised to indicate that this applies to the 100-year timescale, as shown in the figure.
14464	75	8	75	16	'- - -, it plays a smaller role - - - precipitation and wind.' seems to be related to 'Changes in natural variability - - - human-induced change.'These are suggested to be linked as '- - -, it plays a smaller role in long-term variability (on centennial time scale) in globally averaged temperature in the ocean and atmosphere, and a relatively larger rolein short-term (on decadal to inter-decadal time scale) fluctuations in other variables such as precipitation and winds'. [Muhammad Mohsin Iqbal, Pakistan]	Taken into account. This FAQ has been extensively re-written in the SOD.
51726	75	13			Spelling - averaged [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested change made.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
15562	75	14	75	18	FAQ3.1: Changes due to natural variability are small compared to human induced change on centennial time scales. On decadal to inter-decadal time scales, there is a middle ground in which natural variability can have similar magnitudes to human-induced change. On this temporal range, natural variability can substantially enhance or diminish global warming; creating periods of faster warming or periods with little to no global warming at all.“ -> ->I am not sure whether there is clear evidence that change due to natural variability are small compared to human induced change on centennial time scales. A paleo-climate record already showed that the variation of surface temperature due to natural variability is comparable to that due to human’s anthropogenic forcing. Therefore, it is necessary to add more clear evidences on this issue in the main text. [SANG-WOOK YEH, Republic of Korea]	Taken into account. The text here is referring to variations over the past 100 years as shown in the figure, not all natural variations in climate over periods longer than 100 years, which are large for example over glacial-interglacial cycles. The text has been revised to indicate this. Note also that the underlying assessment of the relative influence of natural variability and human-induced change in temperature is in Section 3.3.1.1, not in the FAQ.
49318	75	20	75	20	FAQ 3.1 line 20, the phrase that currently reads "(indirect measurements that span back thousands of years)" should instead read "(indirect measurements that span back hundreds to millions of years)" for consistency with other chapters and to better reflect the full range of paleodata [Yarrow Axford, United States of America]	Accepted. Suggested change made.
51728	75	21			have been? - this sentence is poorly phrased [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Re-phrased.
16328	75	23	75	23	It might be helpful to define 'atmospheric forcing' [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Re-phrased, so that 'atmospheric forcing' is no longer used.
43352	75	25	75	29	This paragraph could be better expressed. I suggest "The El Nino-Southern Oscillation is a phenomenon that is a large source of natural climate variability, affecting winds and sea surface temperatures over the tropical Pacific Ocean. An ENSO event, as they are often referred to, lasts around a year and can change tropical ocean temperatures and rainfall patterns. ENSO also modulates global mean temperatures and affects winds, rainfall and temperatures across much of the globe. The Northern Annular Mode is another example of natural variability, which can affect weather in the high northern latitudes." [James Renwick, New Zealand]	Taken into account. This FAQ has been rewritten, but a long description of ENSO is outside its scope.
16330	75	31	75	32	It might be helpful to explain how scientists actually separate these forcings in the climate model - e.g., how do scientists know whether an observed climate change has anthropogenic or natural origins? [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Re-phrased to clarify how the simulations are carried out. The paragraph already describes how observed changes can be compared with naturally-forced simulations to assess whether or not observed changes are consistent with naturally-forced variability. A full discussion of detection and attribution is beyond the scope of this FAQ.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
43354	75	31	75	36	This following paragraph could also use work. I suggest "To understand which aspects of observed climate change have been caused by natural variability, scientists seperate the influences on climate model simulations due to their anthropogenic or natural origins. When only natural influences are used, the resulting simulations are generally called natural forced similations, and they can be used to assess the range of climate variations expected from natural climate variability alone. Comparison between such simulations and those that include anthropogenic influence allow us to assess which aspects of observed climate change are consistent with responses to human influence, which are consistent with natural variability alone." [James Renwick, New Zealand]	Taken into account. An edited version of the proposed text was used.
14444	75	32	75	32	The word 'there' is suggested to be changed to 'their'. [Muhammad Mohsin Iqbal, Pakistan]	Accepted. Change has been made.
40638	75	32	75	32	should be "their" [Olga Solomina, Russian Federation]	Accepted. Suggested change made.
51730	75	32			separate [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Re-phrased.
51732	75	32			their [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested change made.
50354	75	32			there should be their [Tirthankar Banerjee, India]	Accepted. Now corrected.
50356	75	32			liguistic error [Tirthankar Banerjee, India]	Accepted. Now corrected.
14446	75	34	75	34	The word 'expects' is suggested to be changed to 'expected'. [Muhammad Mohsin Iqbal, Pakistan]	Accepted. Change has been made.
14466	75	34	75	36	'Use of these - - - natural variabilty' is repetitive of first sentence of the paragraph; suggested to be deleted. [Muhammad Mohsin Iqbal, Pakistan]	Rejected. Repeating this term increases clarity.
51734	75	34			expected [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested change made.
43356	75	38	75	39	Suggest rewording to "It is clear that on the interannual time scale, natural variability can have a much larger impact on global mean temperatures than does year-to-year changes in human influence (FAQ 3.1, Figure 1)." I understand the point of what you're trying to say here, but it is not well expressed and I don;t think the figure is helpful. [James Renwick, New Zealand]	Taken into account. The figure has been replaced, and the text revised, though the proposed wording was not directly used.
8304	75	44	75	45	It is unclear what the statement that "As we move to temporal windows between 50-100 years in length, the impact of natural forcing is so small that it is dwarfed by the mean warming rate observed over the last 100 years" is based on. What is the empirical evidence? Given the large number of references to papers evaluating anthropogenic effects, the lack of references to natural variation is striking. [Jonas Ranstam, Sweden]	Rejected. Note that literature citations are not allowed in IPCC FAQs, but the material in the FAQs is based on assessment in the chapter. In this case, this statement is based on the assessment in Section 3.3.1.1, which assesses many attribution studies quantifying the natural influence on long-term temperature changes.
45022	75	54			FAQ 3.1 Fig 1. Using control simulations to represent natural climate variability neglects the important role of natural forcing, especially by volcanoes. [Darrell Kaufman, United States of America]	Taken into account. This figure has been replaced with a more easily understandable schematic version.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
40640	75		75		The orbital forcings are not mentioned in the natural variability list of factors. Even though they are irrelevant to the modern temperature changes they probably should be mentioned here. [Olga Solomina, Russian Federation]	Accepted. Suggested change made.
7414	76	3		7	I think it will be nice to highlight a brief history of the climate models, starting from the very first model to most recent. [Chukwuma Anoruo, Nigeria]	Rejected. This is a comment on FAQ 3.1, Figure 1, but this FAQ does not deal with model evolution. Note that the topic of model evolution is dealt with extensively in Section 1.3 of Chapter 1.
28134	77	1	77	46	Consider replacing existing text with the following: FAQ 3.2: Are Climate Models Improving? 2 3 Climate models are of two main types (See Chapter 1 Section 1.4.3 Climate Models): statistical and process-based. For statistical models… (Overview of statistical studies needed here – an approach could be made to the authors of the review Granger Causality Analyses for Climatic Attribution Atmospheric and Climate Sciences, 2013, 3, 515-522 http://dx.doi.org/10.4236/acs.2013.34054, Alessandro Attanasio, Antonello Pasini1, Umberto Triacca) Turning to process-based climate models, these have improved and continue to do so. Models are now more suitable for capturing the 4 complexities and small-scale processes of the climate system and they compare better with many observations for 5 key climate variables. For decades, models have shown that changes to the climate comes from man-made 6 greenhouse gas emissions, but now, our understanding of the impacts of these changes, and of the changes 7 yet to come, have improved. 8 9 Since the 1950s, scientists have used process-based computer models to understand the Earth's climate. Fundamentally, 10 such models have improved due to advances in technology that allows for greater sophistication and more	Rejected. The chapter does not go at all into statistical models. Here would be the wrong place to start this discussion.
16332	77	1	77	55	FAQ3.2 seems to overlap with parts of FAQ1.1 on whether we understand climate change better now compared to when the IPCC started. FAQ1.1 also includes two paragraphs on how climate models have improved, and has a proposed Figure on climate models in 1990 compared to AR6. It might be helpful to reconcile these overlaps (perhaps by merging the two FAQs), or have both FAQs in the same chapter. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We reduce the overlap for the SOD.
26280	77	35	77	46	FAQ7.1 refers to the model improvement in terms of cloud and cloud-aerosol interaction. It'd be nice that this paragraph is linked to it. [Masahiro Watanabe, Japan]	Accepted. We now refer to FAQ 7.1.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
26286	79	1	79	26	I'm sorry to say that the FAQ3.3 is not well written and sentences are unclear. This is probaly caused by mixing different time scales in a paragraph. Readers may not follow what is intended to say; e.g. D&A for the 20th century climate change or a comparison between proxy and models at millenial time scales. [Masahiro Watanabe, Japan]	Taken into account. This FAQ has been extensively re-written for the SOD.
28116	79	1	79	44	This discussion is very weak. Just two examples: growth of CO2 and increase in temperature. Surely there must be more definitive examples that can be unambiguously ascribed to human activities. I would certainly include in this anthropogenic aerosols and theri direct radiative effect. But even with that much more evidence of attribution would seem required. [Stephen E Schwartz, United States of America]	Taken into account. This FAQ has been extensively re-written for the SOD.
26282	79	3	79	5	past hundreds of thounsands of years --> past thousands of years [Masahiro Watanabe, Japan]	Accepted. Suggested change made.
7416	79	7		8	substantiate with literature [Chukwuma Anoruo, Nigeria]	Rejected. IPCC Frequently Asked Questions do not include literature citations, though they are grounded in the scientific assessment of the underlying chapters.
36670	79	12	79	12	Please change "arming" to "warming". [Jiafu Mao, United States of America]	Accepted. Suggested change made.
14476	79	12	79	12	The word 'arming' needs to be changed to 'warming'. [Muhammad Mohsin Iqbal, Pakistan]	Accepted. Correction made.
49320	79	12	79	12	FAQ 3.3 line 12: typo in term "arming" (should be "warming") [Yarrow Axford, United States of America]	Accepted. Suggested change made.
26284	79	12	79	12	arming --> warming [Masahiro Watanabe, Japan]	Accepted. Suggested change made.
31944	79	12	79	12	warming' instead of 'arming' [Marie-France Loutre, Switzerland]	Accepted. Suggested change made.
37632	79	12			"arming" should be "warming" - I hope. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the sentence in respond to this and other review comments, and now lead with 'It is virtually certain' as suggested here.
51736	79	12			warming? [Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested change made.
49322	79	25	79	25	FAQ 3.3 line 25: typo "that that" [Yarrow Axford, United States of America]	Accepted. Suggested change made.
29294	79	42	79	42	I tblieve that a figure like that of Meehl et al (2004 Journal of Climate 17(19)), i.e. with a visible effect of volcanos etc, would be more convincing here, and would fit with the preceding text [Fabio D'Andrea, France]	Accepted. Suggested change made.
28044	117	1	117	1	The 10%-90% ranges overlap, which makes it hard to see the orange and blue ranges in particular. We suggest making the colors more transparent. It does not contribute to the figure to show CMIP5 results, as the CMIP6 results are clear enough. [roderik van de wal, Netherlands]	Noted. It is assumed the reviewer is in fact referring to Figure 3.6
28046	117	1	117	1	The model spread is lost in the representation of this figure. We suggest an additional column that shows the model spread for CMIP6, or have every cell split diagonally to show this. It is not clear why Arctic values are taken for March, while Antarctic values are taken for February. [roderik van de wal, Netherlands]	Rejected. It is unclear which figure the reviewer is in fact referring to.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
28048	117	1	117	1	This figure does not show any labels (e.g. a,b). The figure and the caption do not mention the unit of the second figure (salinity). The figure does not show units for the presented variables. The unit of the left figure (temperature) is mentioned in the caption, but in other figures in the chapter the unit is shown in the figure. For the sake of consistency, this could also be shown in the figure. None of the axes of the figure have any units mentioned. The choice of scaling range on the left figure (salinity) is not consistent with the values that are presented. For example, the scaling range goes up to 1.2, while the figure only shows values lower than 0.6. We suggest adapting the range to the maximum values shown in the graph. [roderik van de wal, Netherlands]	Rejected. It is unclear which figure the reviewer is in fact referring to.
26952	117	1	117	14	Figure 3.1: There are no black dots in this figure. Do you mean "blue crosses"? [Joachim Rock, Germany]	Noted. The Figure has been completely reworked for the SOD.
31930	117	1	117	14	hard to see the symobls. Red Crosses,bold blue crosses, and purple line are OK. I cannot identify black dots and finer lines [Marie-France Loutre, Switzerland]	Noted. The Figure has been completely reworked for the SOD.
51888	117	1			Mismatch between caption (black dots) and figure (no black dots) needs rectifying [Peter Thorne, Ireland]	Noted. The Figure has been completely reworked for the SOD.
7418	117	1			figures should be made more clearer and sbusequently to all. [Chukwuma Anoruo, Nigeria]	Taken into account. All figures have been revised to improve clarity. Also note that in many cases figures have been revised to only show CMIP6 results in the SOD, instead of CMIP5 and CMIP6, which simplifies the presentation.
16246	117	3	117	5	As per the caption, the explanation regarding the black dots are given. However, the figure 3.1 does not show display these black dots as compared to the red and blue crosses. [Tabassam Raza, Philippines]	Noted. The Figure has been completely reworked for the SOD.
45024	117				Fig. 3.8. Land vs sea temperatures is a great target for a data-model comparison, but probably is not the most important one for policy implications. I suggest that the result so additional data-model comparisons be included that attest more directly to the ability of models to simulate the effects of changing radiative forcing. AR5 chapter 5 includes several such examples. [Darrell Kaufman, United States of America]	Accepted. Fig 3.1 has been replaced with an update of AR5 Fig 9.11(c)
28050	118	1	118	1	The figure does not show any labels (e.g. a,b,c,d). Also, the caption of this figure does not include any information about figure d. [roderik van de wal, Netherlands]	Accepted. All figures were regenerated for the FGD
28052	118	1	118	1	It is not clear why the volcanic eruptions represented by grey triangles are not included in the forcings. The caption mentions that magnitude of eruptions is represented by symbol size, but all of the symbols have the same size. [roderik van de wal, Netherlands]	Rejected. It is unclear which figure the reviewer is in fact referring to.
28054	118	1	118	1	This picture would be clearer if there was a header over each of the Halo A3, B3, C3 figures that said what study was used to get the results, rather than mentioning it in the caption. [roderik van de wal, Netherlands]	Rejected. It is unclear which figure the reviewer is in fact referring to.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
45258	118	1	118	1	The figures are not clear enough. It is sugegsted to present the figures with 2 columns and 4 rows. [Jianping Guo, China]	Noted. Improvements have been considered for the SOD.
48132	118	1	118	11	Polar projections of these biases are also needed. The performance of models in CMIP5 and CMIP6 is difficult to gauge at high-latitudes otherwise. [Daniel Feldman, United States of America]	Rejected. Too difficult to accommodate many figures in the space allocated.
7296	118	1	118	11	As with the important Fig. 3.9, this is very difficult to interpret. One simplification is to remove the CMIP5 mean map and focus on the differences with observations. One key question, that is difficult to answer, is do the CMIP6 models produce observations better than in CMIP5. [Bryan Weare, United States of America]	Taken into account. CMIP5 results have been removed from this figure to simplify the presentation. Note that CMIP5 and CMIP6 model performance can be more quantitatively compared in Figures 3.38 and FAQ 3.2, Figure 1 (FOD figure numbers).
39130	118	1	118	11	This figure 3.2 needs to improved regarding the resolution issue, for better lisibility [JACQUES ANDRE NDIONE, Senegal]	Editorial. Noted.
32010	118	4	118	11	Isn't d missing in the caption? a,b, c, d are missing in the figure. [Marie-France Loutre, Switzerland]	Noted. The caption has been improved.
51454	118	5	0	0	I assume that "one realization" implies one member per CMIP5 model [Bart Van den Hurk, Netherlands]	Accepted. Caption has been clarified as suggested.
51456	118	9	0	10	The root mean square error expresses the difference between the multi-model and ERA seasonal cycle, expressed as monthly values (so with n = 12)? [Bart Van den Hurk, Netherlands]	Noted. The caption has been improved.
51458	118	11	0	0	caption for panels d is missing [Bart Van den Hurk, Netherlands]	Noted. The caption has been improved.
28056	119	1	119	1	This figure uses the abbreviation "tas" in the axes label, but this abbreviation is not explained in the chapter, nor in any of the section of this chapter. We assume it means surface air temperature, which would be "sat". [roderik van de wal, Netherlands]	Accepted. Figure 3.3 has been improved as suggested.
28058	119	1	119	1	The caption of this figure mentions D&A, but this abbreviation is not defined in the text, nor in the caption itself. It is not clear why it was chosen to exclude Antartica from the figure. The 10%-90% ranges overlap, which makes it hard to see the orange and blue ranges in particular. We suggest making the colors more transparent. [roderik van de wal, Netherlands]	Accepted. Figure 3.3 has been improved as suggested.
7298	119	1	119	13	The large number of newly available CMIP6 models makes all of these types of diagrams very difficult to interpret. The observations and mean model are hard to pick out. Some thought should be given to including figures like this in an appendix and including here only the mean model and perhaps some other relevant statisitics. [Bryan Weare, United States of America]	Taken into account. In revising the figure, we have ensured that the observations and mean model are easy to identify. This is a key figure, so we will keep it in the main chapter, and including individual models is important for the assessment.
51464	120	3	0	0	what kind of standard deviation is shown here? Over longitude areas and time mean? Or spatial mean std over time? And if so, is it years or decades? [Bart Van den Hurk, Netherlands]	Noted. The caption has been improved.
16252	121	1	121	14	For clarity purposes, the units for X- and Y-Axes should be properly indicated in the graph. It is due to the reason that the Y-Axis label states K yet caption shows degree C. Also, the X-axis units were not indicated. [Tabassam Raza, Philippines]	Accepted. The Figure has been improved as suggested for the SOD.
51892	121	1			Use model names on the x-axis in susbequent drafts? Also, should this figure not show the observed warming and its uncertainty for comparison purposes? [Peter Thorne, Ireland]	Accepted. The Figure has been improved as suggested.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
45260	122	1	112	1	The curve color for CMIP5_historial cannot be seen, especially when the background is yellow. [Jianping Guo, China]	Noted. Improvements have been made for the SOD.
28060	122	1	122	1	Antarctica is not included in this figure, but there is no reason mentioned why it should not be. It was included in AR5. The 10%-90% ranges overlap, which makes it hard to see the orange and blue ranges in particular. We suggest making the colors more transparent. [roderik van de wal, Netherlands]	Accepted. Figure 3.3 has been improved as suggested.
39132	122	1	122	12	This figure 3.6 needs to improved regarding the resolution issue, for better lisibility [JACQUES ANDRE NDIONE, Senegal]	Editorial. Noted.
42742	122		122		Fig 3.6 - Consider making uncertainty slightly transparent to de-emphasize, also maybe switch to less years labeled (just tick-marks) to reduce clutter. Every ~50 years would be sufficient. [Stephanie Courtney, United States of America]	Accepted. The Figure has been improved as suggested.
39134	124	5	124	5	Please, instead of writing "Mitchell, Thorne, Stott, & Gray, 2013", write "Mitchell et al, 2013" [JACQUES ANDRE NDIONE, Senegal]	Editorial.
26288	124		124		It'd be nice if the diagram is extended to the lower stratosphere. [Masahiro Watanabe, Japan]	Noted. The plot has been extended vertically in order to better show stratospheric cooling.
39136	125	1	125	11	This figure 3.9 needs to improved regarding the resolution issue, for better lisibility. It'll be better in landscape position. [JACQUES ANDRE NDIONE, Senegal]	Editorial. CMIP5 results will not be shown thus making figure larger.
42744	125		125		Figure 3.9 (also 3.2) - May be planned anyway, but just in case, it's impossible to make comparisons at this scale and layout, consider laying out CMIP maps side-by-side down a whole page [Stephanie Courtney, United States of America]	Editorial. CMIP5 results will not be shown thus making figure larger.
57926	126	1	126	1	in Figure 3,10, order the legend in the same colours as in the figure (so purple, blue, green, etc.). Would be better if the two names 'LGM MAP" and 'MH MAP' are written horizontal, on top of each panel. [Bas de Boer, Netherlands]	Editorial. Figure 3.10 was replaced with an evaluation of model (CMIP5 and CMIP6) representation of mid-Holocene precipitation in several regions of the world.
39294	126	1	126	18	These are global distributions to be included? Oncea available with CMIP6-PMIP4 models a map format would be much more useful with respect to the discussion on page 20/21. [Daniel Ibarra, United States of America]	Figure 3.10 was replaced with an evaluation of model (CMIP5 and CMIP6) representation of mid-Holocene precipitation in several regions.
39138	126	1	126	18	Why in this figure 3.10, we don't have CMIP6 data? [JACQUES ANDRE NDIONE, Senegal]	Figure 3.10 was replaced with an evaluation of model (CMIP5 and CMIP6) representation of mid-Holocene precipitation in several regions.
42746	126		126		Fig 3.10 - Very difficult to trace y-axis labels to data, consider color-coding model name to model suite color like in data. Also spell out MH and LGM, perhaps as titles [Stephanie Courtney, United States of America]	Editorial. Figure 3.10 was replaced with an evaluation of model (CMIP5 and CMIP6) representation of mid-Holocene precipitation in several regions of the world.
51470	127	1	0	0	this plot (and the equivalent Figure 3.6) has the disadvantage that the information shown depends on the order with which the different datasets are plotted. Some form of transparency should be considered to reveal the masked envelopes [Bart Van den Hurk, Netherlands]	Taken into account. Curves have transparency now.
39140	128	1	128	9	This figure 3.12 has a very good quality resolution; all figures should look like taht one. [JACQUES ANDRE NDIONE, Senegal]	Noted.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
7300	133	1	133	12	The Antarctic portion of this figure raises a number of concerns and the accompanying text on page 32 helps little. If the mean or internal variability of ice extent is important then the observations and mean model results should be shown. [Bryan Weare, United States of America]	Noted. The observations and mean model results are shown in the SOD version of this figure.
48456	133	3	133	3	This figure understates the potential role of internal variability in the sea ice trends by visually comparing a timeseries of one realisation of the observations with multimodel changes. It needs further thought. Perhaps some stippling could be added where the observed changes are outside the multimodel spread? [Julie Arblaster, Australia]	Taken into account. We have added inter-model agreement information to model results.
43184	133	3	133	11	On the Figure 3.17 the bars show a total difference of the multidecadal increase between the Antarctic and the Arctic ice extent for 1979-2015, a recommendation is introducing better the graphic about both scenarios and the reasons, the idea is general and is difficult of comprehension. [Sebastian Naranjo Silva, Ecuador]	Taken into account. We have rearranged panels to better represent forcing responses.
26954	143	1	143	7	Figure 3.27: Please indicate what the lines in the graphs represent (different models? observations?), what the red bars indicate, and what "F_L" and "F_LN" in panel c stand for. [Joachim Rock, Germany]	Noted. The figure was a placeholder and has been updated for the SOD, as well as its caption.
39142	148	1	148	10	This figure 3.32 has a very good quality resolution; all figures should look like taht one. [JACQUES ANDRE NDIONE, Senegal]	Noted.
39144	150	1	150	12	This figure 3.34 needs to improved regarding the resolution issue, including the labels on the X axis, for better lisibility [JACQUES ANDRE NDIONE, Senegal]	Accepted. We have reproduced this figure.
48566	150		150		Overall I liked the chapter however I feel that there is some discussion which are misiing/showing errors in section 3.7.6 (page 59) reagrding PDO. I would suggest to authors to include the discussion properly by removing errors in current report. Further labels of the Fig. 3.34 on Page 150 needs to be drwan nicely as it looks very blurred in current version of the report. [Pushp Raj Tiwari, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have reproduced Fig. 3.34. Figure citation errors are a known issue reported by the TSU.
51894	151	1	152	1	You need to be a little careful here in that ERSST itself is spatially filtered by ordered weighting of the 140 most substantial EOTs (truncated EOFs) and so the SST is pre-filtered to express large-scale modes. There is thus a risk of circularity in these figures. This may be unavoidable but it is worth due consideration. [Peter Thorne, Ireland]	Noted. We use ERSST as a reference observational dataset but have included other observational products in panel (c).
39146	154	1	154	14	This figure 3.38 will be better in landscape position. [JACQUES ANDRE NDIONE, Senegal]	Rejected. We have changed the layout and presentation of the figure. Landscape or portrait don't really change the readability of the figure, but rotating it would create an unnecessary break with AR5.
43358	156	0	156	0	Interesting diagram, but would it convey any useful information to the target audience? [James Renwick, New Zealand]	Accepted. We have reworked the diagram.
7302	159	1	159	18	Parts a) and b) are very confusing. The labeling is overly complex. Where are the white and blue lines? [Bryan Weare, United States of America]	Taken into account. We have improved the figure visibility by showing the outline only for CMIP5 histogram without infilling.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
43360	161	1	161	10	This diagram takes some thinking about (for me at least) to get the sense of what is shown. Could you perhaps have a line plot of natural variability on different time scales, from seasonal to centennial, along with the observed trend value/line? [James Renwick, New Zealand]	Taken into account. This figure has been replaced with a more easily understandable version.
7304	161	1	161	10	This is not a good summary figure. It is not what it says, box and whisker. It does not portray compatible observational trends. Is the information here important enough for a concluding figure? [Bryan Weare, United States of America]	Taken into account. This figure has been substantially revised to make it more easily understandable and suitable for a FAQ.
26956	162	1	162	13	FAQ 3.2, Figure 1: Please either add the correlations for the alternate observational data sets for rlut and swcre, or include a statement that there are no such datasets. [Joachim Rock, Germany]	Accepted. The figure is revised more generally to improve readability for a wide audience.
7306	163	1	163	11	This simple, clean summary figure should include important other variables like sea level and Greenland and Antarctic ice. [Bryan Weare, United States of America]	Rejected. Intention is to inform FAQ and provide info on main drivers of climate change, rather than to be comprehensive. Note that Figure 3.40 is a similar figure which shows a range of variables.
27200	163	1	163	12	Figure 10 of Laloyaux et al (2018) doi: 10.1029/2018MS001273, shows that the ocean heat content seems to follow the 60-70 year cycle, possibly related to Atlantic Multidecadal Oscillation. This changes complements the right part of Fig. 1 of FAQ 3.3 and its significance. [François GERVAIS, France]	Noted. The discussion in Laloyaux et al. (2018) makes clear that the changes shown in Figure 10 are mainly artefacts and not real climate change signals.
8820	43558	7		9	Consistency? These lines state that human activities caused more than half of the observed increase in global mean surface temperature over the period 1951-2010. Page 1-40 lines 35-37 state "...that it is extremely likely that human influence has been the dominant cause of the observed warming since the mid-20th century". Dominant cause implies something greater than more than half. [Dennis Paterson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the revised text we say human influence is the 'main driver' of the observed warming, and in the chapter text we define this as meaning having caused more than half.
8822	43619	1			Anthropogenic CO2 may have substantially contributed to the reduced alkalinity of the oceans, but they are not acid and are not becoming more acid. This difference is one of accuracy. [Dennis Paterson, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reviewer is correct that the oceans are alkaline. However, the chapter text refers to 'ocean acidification' and to an 'increase in acidity', but does not say that the ocean is acid. The terms above are standard and refer to a decrease in pH.
54782	ES - 4				The ES is well written and interesting, and clearly expressed, well done. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
54820	Fig 3.31				This seems a bit asymmetric - not much in figurs on last millennium but the SAM which is not that well reconstructed! I wouldn't trust it much. I hope you are also going to show the forced runs against global and hemispheric mean temperatures? [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The proxy reconstruction and its comparison with last millennium simulations are an important advancement since AR5. We have updated the figure to include another reconstruction, and revised the assessment to take into account the uncertainty of reconstructions.
54822	Fig. 3.40				Presently doesn't do a lot for me… [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have reworked the diagram.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
54790	Section on i				TCR estimates or ASK (stott and kettleborough) method results are not discussed in this section unless I missed it; only emergent constraints. I think this is a gap that needs plugging as arguably the trends attributable so far have at least as much reliability as emergent constraints (I would argue more as they only assume that presently underestimatedor overestimated greenhosue gas responses continue to be that). there are some tricky issues there around Aerosols that of course can be tricky (e.g. its more reliable to use ghg only forward than ANT as aerosols are presently decreasing and errors that cancel now could enhance each other in future) . TCR is discussed in chapter 7, but only using simple dynamical models and box models entirely leaving out the attribution literature! wherever it goes, it needs to be somewhere - for me, ch3 is in a great position to discuss ASK and TCR estimates and then can hand over results to chapter 7... [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. TCR is assessed in Chapter 7 and projected change in large-scale variables in Chapter 4.
54784					The chapter as a whole is in very good shape and reads well and interesting. The assessments seem sensible and well supported. I made a few suggestions, sometimes self serving (sorry - or rather group serving) but in cases when I think there is a gap. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
56832					Figure 3.19: color palette for temperature should be the one from the Visual Style Guide. For more guidance, contact the TSU graphics officer // labels/units on the color bars and axis are missing in the figure / it would be helpful to have the definition of the black lines in the figure (as a legend for example) [WGI TSU, France]	Taken into account. Figure has been revised for the SOD. The defined color palette for temperature is included for the FGD.
56834					Figure 3.20: the units should be moved to the yaxis, vertically [WGI TSU, France]	Taken into account. Was considered when figure was revised for the FGD.
56836					Figure 3.21: it would be nice to understand from the figure alone (without caption) that this is talking about sea surface temperature (SST), spelling out the acronym would help. [WGI TSU, France]	Accepted. Was considered when figure was revised for the FGD.
56838					Figure 3.22: this is a good figure but the panel could be reorganized spacially to give it more potential // the numbers in the black triangles could be related to the references // For more guidance, contact the TSU graphics officer [WGI TSU, France]	Taken into account. Was considered when figure was revised for the FGD.
56840					Figure 3.24: it would be nice to understand from the figure alone (without caption) that this is talking about sea level anomaly, provide a title above the first map // label/unit of the color bar is missing // it would be nice to have the "enhanced salinity" and reduce salinity also annotated close to the color bar for a more comprehensive figure. // in (a) and (b) legend could be in (a) top left corner, closer to the symbols that will be seen first. [WGI TSU, France]	Accepted. Was considered when figure was revised for the FGD.
56842					Figure 3.25: it would be nice to understand from the figure alone (without caption) that this is talking about sea level change (SLC), spelling out the acronym in the axis label would help. [WGI TSU, France]	Accepted. Figure has benn revised for the SOD.

Comment ID	From Page	From Line	Topage	To Line	Comment	Response
56844					Figure 3.26: label should indicate what diamonds are // there are no visible stars in (a) // rainbow colors should be avoided (For more guidance, contact the TSU graphics officer) // Y axis label on right plots are overwriting (a) // it would be good to spell out Sv as standard deviation // it would be good to spell out AMOC in caption and figure ideally [WGI TSU, France]	Accepted. The figure was revised for the SOD.
56846					Figure 3.33: it is not clear what a and b are in the dots of the (b) plot. [WGI TSU, France]	Taken into account. The figure has been revised.
56848					Figure 3.35: the colors from (a) should be revised [WGI TSU, France]	Taken into account. The figure has been revised.
56850					Figure 3.36: the colors from (a) should be revised [WGI TSU, France]	Taken into account. The figure has been revised.
56852					Figure 3.40: For creating an engaging mind map, graphics officer could provide some guidance if needed. [WGI TSU, France]	Noted.
56854					Figure box 3.2 figure 1: it seems like TXx and Rx1day are not being spelled out in the caption nor the main text. It would be good to spell it out in the caption and ideally in the label of Y axes. [WGI TSU, France]	Taken into account. Was considered when figure was revised for the FGD.
56856					FAQ 3.1 figure: this figure as it is is not suitable for a lay audience [WGI TSU, France]	Taken into account. Figure has been substantially revised for the SOD.
56858					FAQ 3.2 figure: this figure as it is is not suitable for a lay audience [WGI TSU, France]	Taken into account. Figure has benn revised for the SOD.
54816					Overall, a very nice chapter; I also like the box on the hiatus (and am pleased that it also mentions theinfluence of the NAO on NH winter trends which is missing in the shorter discussion earlier in the chapter; maybe better relate to the box than try explain it there) [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
43312					The figures labels should tidy - Check Fig. 3.2; Fig. 3.6 page 122; Fig. 3.9 page 125; Fig. 3.10 page 126; Fig. 3.11 page 127; Fig. 3.14 page 130; Fig 3.15 page 131; Fig. 3.28 page 144 [Onema Adojoh, United States of America]	Taken into account
52558					In general, I have the impression that the quality of the contributions still varies quite strongly from section to section. Often, the text paraphrases the literature rather than discussing it. An assessment should not only derive statements by integrating statements of individual papers, but also by assessing the quality of individual papers and by explaining at least to some extent differences in messages. This has not yet been achieved for all sections [Douglas Maraun, Austria]	Taken into account. We have strived for more assessment and less review in the SOD.
52560					I am not sure about the current structure of the chapter. In particular I was surprised reading about modes of variability only after the impacts on the biosphere. Also one could consider to better integrate the text on the atmospheric circulation and the text on the atmospheric modes of variability (e.g. NAO) which essentially describe the same thing but just from a simplified angle (e.g., the NAO representing the jet variability). [Douglas Maraun, Austria]	Taken into account. We follow the order of indicators agreed with Chapter 2. We have tried to better link the circulation and modes assessments.

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52562					Quite often I had the impression that technical detail was given, but not the really relevant information (e.g. on the underlying assumptions, the used diagnostics etc). Changing this would help to substantially improve the relevance of the Chapter. [Douglas Maraun, Austria]	Taken into account. We have aimed to include such additional information where possible in the SOD.
45908					(2) Need to give a bit more description about the CMIP evaluation tools, ESMValTool and CVDP. [Md. Habibur Rahman, Japan]	Rejected. Such approaches are now assessed in Chapter 1, Section 1.4.
45910					(3) Need to give a bit more description of the other probabilistics approaches. [Md. Habibur Rahman, Japan]	Taken into account. The discussion of detection and attribution methods has been expanded.
45912					(4) Figure 3.1: Where is black dots? Or black lines instead of black dots? [Md. Habibur Rahman, Japan]	Taken into account. Caption has been clarified and figure revised.
45914					(5) Figure 3.2: In my opinion the annual-mean surface (2 m) air temperature (°C) for the period 1986–2015~18 (instead of 1986-2005) could give a better understanding of the recent change of surface air-temperature. [Md. Habibur Rahman, Japan]	Rejected. Standard period agreed across all chapters.
45916					(6) Figure 3.3: Difficult to understand the dot lines of differenct colors. [Md. Habibur Rahman, Japan]	Taken into account. Colours were revised for the SOD.
45918					(7) Figure 3.10: It is very difficult to understand to black dots of the right side figure. [Md. Habibur Rahman, Japan]	Taken into account. This figure has been replaced by a new version in a different format.
45920					(8) Figure 3.12: In my opinion the 1980-2005 trend of subtropical edge latitude can be replace by recent data of 1980 to 2015~18. [Md. Habibur Rahman, Japan]	Taken into account. Date range updated to 1980-2013.
45922					(9) No need to write "Figure produced with ESMValTool v2.0a1" in each figure. [Md. Habibur Rahman, Japan]	Rejected. This was included to support reproducibility.
45924					(10) Figure 3.19: Very difficult to understand the message from the figure. Can be produce a simple figure for this. [Md. Habibur Rahman, Japan]	Taken into account. Figure was replotted to try to improve clarity.
45926					(11) Figure 3.23: Very difficult to understand the message from the figure. Can be produce a simple figure for this. [Md. Habibur Rahman, Japan]	Noted. Not yet updated in SOD, but figure to be replaced in FGD.
45928					(12) Figure 3.27: It would be better to produce a map from the data of 1860-2015~18 instead of 1860-2010. [Md. Habibur Rahman, Japan]	Taken into account. Extended to 2014 and updated using CMIP6 simulations.
45930					(13) Temperature can be calculated for the period of 1958-2015~18 instead of 1958-2010. [Md. Habibur Rahman, Japan]	Rejected. Not clear what this comment is referring to.
45932					(14) FAQ 3.1, Figure 1: It would be better to produce the figure by using theobserved global mean surface temperature trend value 1910-2015~18 instead of 1910-2010 period. [Md. Habibur Rahman, Japan]	Not applicable. This figure has been replaced in the SOD.
45934					(15) FAQ 3.2, Figure 1: This figure can be produce by the annual mean climatology data from 1980-2015~18 instead of 1980–1999 period. [Md. Habibur Rahman, Japan]	Rejected. Data past 1999 are not available for CMIP3 simulations.
45936					(16) FAQ 3.3, Figure 1: It would be better to prodcue map from the anomalies data of 1880-2015~18 instead of 1880–1919 for surface temperatures; and data of 1960-2015~18 instead of 1960–1980 for ocean heat content. [Md. Habibur Rahman, Japan]	Rejected. We choose early base periods in order to show the separation between the anthropogenic and natural responses.
46704					attribution results of moonsoon changes show overlap with section 8.2.1.3, needs coordination [WGI TSU, France]	Taken into account. Have resolved some overlaps on monsoons in the SOD.

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45938					(17) Sentences can be write by using less complex sentences rather than purely complex sentences. [Md. Habibur Rahman, Japan]	Taken into account. It is not clear which text this is referring to, but if it refers to the whole chapter,we have aimed to improve readability in the SOD.
45940					(18) It would be better for a policy makers if given csome country specific examples like in the case of salinity and sea level rise particularly for the climate affected countries like Bangladesh, Maldives and other countries. [Md. Habibur Rahman, Japan]	Rejected. Focus of the chapter is on large-scale indicators.
45942					(19) In the terrstrial carbon cycle, there is no description about the impact and role of tropical forests to climate change. [Md. Habibur Rahman, Japan]	Rejected. We focus on large scale indicators. See Chapter 5.
45944					(20) I have not found aby description about the role of woodfuel burning and greehouse gas emissions particularly in the developing countries because people are rely on woofuel for their burning and heating and obviously has great impact on the greenhouse gas emissions and ultimately to the changing climate. [Md. Habibur Rahman, Japan]	Rejected. This is out of scope for WGI. More a WGIII issue.
11642					There needs to be more discussion and assessment of the role of spatially heterogeneous human climate forcings in the report. As written in National Research Council, 2005: Radiative forcing of climate change: Expanding the concept and addressing uncertainties. Committee on Radiative Forcing Effects on Climate Change, Climate Research Committee, Board on Atmospheric Sciences and Climate, Division on Earth and Life Studies, The National Academies Press, Washington, D.C., 208 pp. https://www.nap.edu/read/11175/chapter/2 “Regional variations in radiative forcing may have important regional and global climatic implications that are not resolved by the concept of global mean radiative forcing. Tropospheric aerosols and landscape changes have particularly heterogeneous forcings. To date, there have been only limited studies of regional radiative forcing and response. “ As an example of how important this issue is, in the paper Matsui, T., and R.A. Pielke Sr., 2006: Measurement-based estimation of the spatial gradient of aerosol radiative forcing. Geophys. Res. Letts., 33, L11813, doi:10.1029/2006GL025974. http://pielkeclimatesci.wordpress.com/files/2009/10/r-312.pdf “Unlike GHG, aerosols have much greater spatial heterogeneity in their radiative forcing. The heterogeneous diabatic heating can modulate the gradient in horizontal pressure field and atmospheric circulations, thus altering the regional climate” In that paper the spatial gradients of diabatic heating from aerosols was much more than an order of magnitude larger than that due to human greenhouse gas forcing. A similar large effect should be expected from land use change/land management. Thus, this issue of spatial heterogeneity of human climate forcings needs	Taken into account. This is assessed in more detail in our SOD, cross-referencing Chapters 6 and 7.
45946					(21) I have not found any description about the negative impact of cluimate change on permafrost. [Md. Habibur Rahman, Japan]	Rejected. Out of scope. See Chapter 5 for discussion of changes in permafrost.

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45948					(22) Can use some tables instead of figures because sometimes a table can represent data easily than a figure. [Md. Habibur Rahman, Japan]	Rejected. This comment is too general identify an appropriate response. We do use some tables already in the chapter.
15256					It is dificult to reconcile the detailed discussions of model performance, and the review of results from the emergent constraints literature, with the high level statements put forward in the executive summary of the chapter about 1) models having improved, and 2) observational evidence being effective in constraining ECS and future projections. The evidence discussed does not seem to fully support these statements that are interpretable as unconditional and sweeping. When model output evaluation is described, in the body of the chapter, the conclusions much more often than not are that models' performance has not really changed substantially. Maybe some more limited point about the effects of resolution on representing some specific aspects/quantities can be noted, but between that more restrictive assessment and the much stronger statements now there (I think one of them has 'high confidence' attached) I think there is still a gap of evidence to be filled. There is a statement about the models "spanning" the observed behavior which is difficult to interpret, but perhaps it is just a matter of clarifying what it is meant. Without a bit of further explanation of what is meant here, a statement like that could be derived from a situation where really bad models are collected, that either grossly underestimate or grossly overestimate the observed. The literature cited about emergent constraints is interesting and developing and promising, but appears still limited in offering robust constraints on ECS or future projections, at least from what is right now described in the chapter, and given that nothing yet is available from CMIP6. [Claudia Tebaldi, United States of America]	Taken into account. The assessment of changes in CMIP6 models versus CMIP5 models has been updated based on new literature assessing CMIP6. The discussion on observational constraints on projections and fitness for purpose for projections has been transferred to Chapter 4, since it was decided that it is better to have this assessment in the place that projections are discussed.
45262					Many figures are not clear. Their resolution should be increased. [Jianping Guo, China]	Taken into account.
56808					Figure General comments Chapter 3: ideally, the figure should be a bit more independent from the caption => some crucial information in the caption should be included directly in the figurel (e.g. color legend) //Figures and caption should be more independent from the main text => spell out acronyms in figure and/or caption wherever possible (model acronyms are not expected to be spelled out). // you can add titles to your figure to enhance the understanding at first glance // plots should be labelled (a) (b) (c) etc. and refered as such in the caption too // figures should be uncluttered for SOD (e.g. remove unnecessary grids and frames) // some units are missing in the axis labels and they should ne in () and not in [ ], the unit should be standardized troughout the chapter (e.g. degrees celcius becomes °C) . please refer to the IPCC visual style guide (https://www.ipcc.ch/site/assets/uploads/2019/04/IPCC-visual-style-guide.pdf) [WGI TSU, France]	Taken into account. All figures were updated for the SOD.
56810					Figure 3.1: there are no black dots displayed (black dots were mentioned in the caption) [WGI TSU, France]	Taken into account. The figure has been substantially revised for the SOD.

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56812					Figure 3.3: It would be nice to have an annotation "reference period" in the yellow shaded area // according to style guide: units in () and not in [] and °C instead of degC // "tas" is not explained in caption and main text, Ideally it should be spelled out in the axis titles [WGI TSU, France]	Taken into account. The figure has been substantially revised for the SOD and all comments have been considered.
56814					Figure 3.4: in caption, is there a (b) panel? [WGI TSU, France]	Taken into account. We removed "a)" in the caption of the figure for the SOD.
56816					Figure 3.6: "reference period" for the yellow shade could be added to the legend [WGI TSU, France]	Taken into account. Figure has been revised for the SOD. The yellow shade has been removed and the reference period is mentioned in the caption.
56818					Figure 3.10: Figures and caption should be more independent from the main text => spell out LGM and MH in caption and ideally in figure (if enough space) // darklight grey shading should be added to the legend as well [WGI TSU, France]	Taken in to account. Figure has benn substantially revised for the SOD.
56820					Figure 3.11: It would be nice to have an annotation "reference period" in the yellow shaded area // according to style guide: units in () and not in [] // precipitation instead of pr in the axis titles (if enough space) [WGI TSU, France]	Taken into account. The figure has been substantially revised for the SOD and all comments have been considered.
56822					Figure 3.12: according to style guide: units in () and not in [] and "°" instead of deg [WGI TSU, France]	Taken into account. The figure has been revised.
56824					Figure 3.13: units and title of color bar are missing // hatching when "inside the monsoon domain" would allow a clearer view on the precipitation/wind data. At the moment it is quite cluttered // spell out acronym where possible (e.g. full name instead of GPCP) [WGI TSU, France]	Taken into account. The figure has been revised.
56826					Figure 3.15: the colors should be revised. For more guidance, contact the TSU graphics officer. // what the contour represent should be written in the figure (next to "contour from…") [WGI TSU, France]	Taken into account. Caption has benn revised for the SOD. The colors will be changed for the FGD.
56828					Figure 3.16: it would be nice to understand from the figure alone (without caption) that this is talking about sea ice extent (SIE), spelling out the acronym would help. [WGI TSU, France]	Taken into account. Was considered when figure was revised for the FGD.
56830					Figure 3.18: it would be nice to understand from the figure alone (without caption) that this is talking about snow cover extent (SCE), spelling out the acronym would help. [WGI TSU, France]	Taken into account. Was considered when figure was revised for the FGD.