

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27676	0	0	0	0	et al in italics, bibliographical citations in chronological order. [Poot Delgado Carlos Antonio, Mexico]	Taken into account. References adjusted to adhere to IPCC style guide.
17950	0	0	0	0	In general, the figures are in a really poor resolution. For some of them it's hard to read everything on them. [Gwenaelle GREMION, Canada]	Taken into account. See response to comment 43302.
38942	0	0	0	0	(also for other chapters) Methane is sometimes categorized to WMGHGs and elsewhere to SLCFs. Experts know why it is. However this might incur great confusion particularly for policymakers. Better explanation should be given at an early chapter, like Chapter 2, to deliver to the readers how to treat methane in this Assessment Report. [Yugo Kanaya, Japan]	Taken into account. We have coordinated with other relevant chapters and to assure better consistency in the SOD.
8752	0	0	0	0	Define accronyms at first occurrence (e.g. WMGHG, GSAT, GM, AMOC, etc) [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This has been checked more thoroughly in the SOD.
8754	0	0	0	0	For section 2.3.1.1.2: Can you say something about the forcing of the observed variability? For example, the global SST compilation by Mcgreggor et al 2015 (DOI: 10.1038/NGEO2510) suggests that the observed cooling from 801 to 1800 CE is not caused orbital forcing but at least in part by high frequency explosive volcanism [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - beyond the mandate of this chapter. Reviewer suggests including an analysis of the cause of observed climate change, thus addressing attribution issue, whereas CH2 is dedicated to observational evidence of the climate changes and climate forcing separately.
8764	0	0	0	0	For figures, have a consistent way to zoom in into different periods. For example, in fig 2.2 the zoom in is done with dashed lines, in Fig 2.3 with a combination of vertical rectangle and shaded areas of different colours, while in Fig 2.10 the zoom in is done with gray shaded areas. [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have attempted to be more consistent in how we do this in the next draft.
29538	0	0	0	0	The comment with respect to global mean signals refers to the whole chapter. It would be important to also mention regionally varying patterns and link to other chapters, in particular chapter 10! [Katja Matthes, Germany]	Rejected. Regional change is the domain of later chapters and we need to avoid overlap with these.
28778	0	0	0	0	This is my favourite chapter so far (aside from chapter 7). Very comprehensive and good read. Sections are well balanced and have same style, sections are mature and bang up to date with references. Good use of confidence language and cross referencing. Good figures, clear and easy to understand [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48016	0	0	0	0	Scoping Outline Check: All bullets in the approved outline are covered but there is limited focused on desertification, soil moisture, vegetation changes (as noted in bullet 4 of the ch2 approved outline). [WGI TSU, France]	Taken into account. Newly appointed LA, SRCL report availability and support from chapter 5 LAs have helped to remedy the situation. However, soil moisture measurements are very limited globally so have not been able to be included.
28862	0	0	0	0	FAQ2.1 - I prefer this framing to FAQ 1.2 but would like to see it combined with attribution more FAQ2.2 - merge with FAQ 1.1 [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted - The overlaps between FAQs 1.2 and 2.1 were discussed during LAM3 and a plan was developed to eliminate redundancy and maintain consistency.
47814	0	0	0	0	Chapters 2, 5 and 7 class methane as long-lived but chapter 6 classes it as short-lived. [WGI TSU, France]	See response to comment 38942
14280	0	0			A well organised and succinctly written chapter so far. The water vapour and precipitation changes sections can be checked for consistency with Chapter 8. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Cross-checking was undertaken with the result that some aspects of chapter 8 have been donated to chapter 2 for integration in the SOD which has now been done.
32006	0				This is a comment about the unit. There is (for example) both mm/decade and mm decade-1. Should that be made more uniform? [Marie-France Loutre, Switzerland]	Taken into account. We have tried to be consistent with the style guide in SOD.
53326	0				The chapter is quite heavy to read, but the summaries in the end are very helpful. The heavy material presented makes the figures and ES very important. [Jan Fuglestedt, Norway]	Noted. Covered by a range of other, more specific comments.
7258	0				Too much of the Executive Summary and the full text treats equally items important to the entire report and those which are tertiary or secondary. Items discussing climate over 100's of millions of years are given nearly equal weight to those in the past two centuries. Items, which are evaluated as low or medium confidence often appear to be given similar weight to those with high confidence or virtually certain. [Bryan Weare, United States of America]	Taken into account. The ES has been substantively rewritten for inter-chapter consistency. However, it is still important to capture the state of knowledge which requires highlighting areas of high and low confidence and placing recent observations in a long term context which is the given chapter charge.
52318	0				Be consistent across the chapter in how the Holocene is referenced. In some places (e.g. p. 8 line 34) it is not capitalized; terms like "mid-holocene" and "middle Holocene" occur throughout. Same for "preindustrial" "Pre-Industrial" and "pre-Industrial" - be consistent. [Katherine Glover, United States of America]	Taken into account. We have attempted to use a consistent style throughout in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54622	0				chapter's content overlap with several other chapters and whilst a bit of overlap is basically impossible to avoid, I think in many parts of this chapter should be removed or to be reduced considerably, redirecting the reader to the appropriate chapters. For example all of 2.3.1.2 (chapter 8 is devoid to that). And section 2.3.3 [Ruth Cerezo, Mexico]	Rejected. The chapter has in its charge assessing these aspects. It is actually other chapters that are tending to over-reach and these overlaps have been further managed at LAM3 with some donation of text between chapters .
54624	0				there is a need of consistency when discussing E-P (some other chapters use P-E) [Ruth Cerezo, Mexico]	Accepted. We have undertaken efforts to be consistent within and between chapters in SOD.
52324	0				Text is extremely wordy in places throughout the chapter, with lots of passive voice. There are numerous "...of the...by...by which...as such" sentence constructions that make syntax convoluted, detracting from readability. I've noted spots below where language and syntax could be easily rephrased to 1) make it more active, and 2) streamline it. [Katherine Glover, United States of America]	Taken into account. We have tried to tighten the language in the SOD.
35432	0				This chapter is in very good shape, complete and comprehensive and written in a consistent style, and the authors should be commended on a job well done. [Nathan Gillett, Canada]	Noted with thanks.
35434	0				Overall, paleoclimate information is well integrated into the chapter, and discussion of paleo information and instrumental observations together, as defined by the AR6 WGI outline, represents an improvement compared to their separate discussion in AR5. Examples of sections in which paleo information is particularly well integrated include the cryosphere and ocean sections, where paleo changes are used to set recently observed changes in context - see for example 2.3.2.3. However, in 2.3.1 the paleo information is less closely linked to the overall discussion, and the links between the discussion of paleo information and instrumental observations are less clear. [Nathan Gillett, Canada]	Taken into account. For both CO2 and temperature where this was an issue we have decided to restructure the sections to better integrate paleo and instrumental. However, given their importance and the breadth of evidence to be assessed some unique sub-structuring is necessary and required for readability and to support the key findings.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35436	0				I strongly recommend that in all places where numerical values and confidence ranges are given, that these represent observational uncertainties only. Many of the uncertainty ranges quoted in the chapter are observational uncertainties only. For example, the quoted uncertainty range of 0.86-1.14 C for 2009-2018 temperature relative to 1850-1900 is the observational uncertainty only. However, the uncertainty ranges quoted on most trends in the chapter represent the effects of internal variability only. They represent the uncertainty in the true underlying trend under the assumption that residuals from the trend are generated by an AR(1) process, fitted to the observations (see for example captions to Tables 2.3 and 2.19 and as explained in AR5 WGI Supplementary material 2.SM.3.2 which is referenced in this chapter). For example, the uncertainty in the 1880-2018 trend of 0.73-1.28C is calculated in this way, and is much larger than the uncertainty quoted in the 2009-2018 decadal mean for this reason. Firstly, the observational uncertainty and the uncertainty due to internal variability are fundamentally different, and it does not make sense to use one for quoted means and the other for trends. There is nothing special about trends, as compared to changes in means or any other diagnostic, which means it makes sense to calculate their uncertainties with respect to internal variability. Secondly, the confidence intervals accounting for the effects of internal variability represent a comparison of the observed change with an underlying model of a linear forced change with AR(1) internal variability superposed, which represents a detection	Taken into account. We have attempted to be more explicit as to what sources of uncertainty are being quantified where. In some cases it is necessary to include multiple sources of uncertainty simultaneously. For example we feel that the assessment of surface temperature changes should include both observational dataset choice and time series fitting uncertainties. Text has been added to the introductory section.
35438	0				I suggest using fewer acronyms for discussions of periods in Earth's history. These acronyms will only be familiar to paleoclimate specialists, and not to general climate scientists who would otherwise have to frequently refer back to the definitions to follow the discussion. I suggest only using acronyms for the most frequently-used terms (PETM, LGM, MCA, LIA), and writing the others out in full. Aside from their use in Table 2.1, LIG and EECO are used only two times, PI is only used three times, and MPWP is used only five times. [Nathan Gillett, Canada]	Taken into account. We are now much clearer upon the time periods via inclusion of Cross-Chapter Box 2.1, unfortunately space consideration means that use of acronyms is necessary to avoid substantial page count issues.
52344	0				Reconcile parentheses formatting across the report - many examples of two citations at sentence end that could be combined, parentheses nested within parenthesis that don't pair/resolve [Katherine Glover, United States of America]	Taken into account. We ran out of time to resolve all reference issues in FOD. All references should be included in SOD as submitted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56218	0				Definition of GMST: Why not use land surface temperature rather than air temperature over land for that definition? In that case, GSAT would be only based on air temperatures (on both land and oceans) and GMST would be only based on surface temperatures (on both land and oceans). [Sonia Seneviratne, Switzerland]	Rejected. Land surface temperature measurements of sufficient coverage and duration do not exist. We have tried to strengthen the GMST / GSAT issue via inclusion of a new cross-chapter box.
23460	0				I would note that overall the quality of this FOD is substantially better than the equivalent version in AR5. However, I am concerned with some of the editorial variation between sections (and also between the various chapters I have looked at) especially where this variation makes the science unclear. I have flagged some of these issues for clarity. One thing I am unsure about is the reference citation style in the text. In some places references are given chronologically, in others alphabetically, and in others it appears to be random. I would normally expect the first form to be used but have not formally reported such instances unless there is an issue of clarity involved. There are similar issues with the use of Latin terms (eg in-situ, per). I would normally expect these to be italicised (and they are more generally in other chapters) but, again, this is inconsistent in this chapter. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have tried to make the reference citations more similar by reordering in the next draft. The FOD had real issues with just getting all citations in so such aspects did not have time to be addressed. The cross-chapter aspects have been addressed by addition of an internal review point which has helped to improve inter-chapter consistency (although ambiguities undoubtedly remain still at SOD stage).
8656	0				I am generally suspicious of the modernity of the citations here. I find it hard to believe that comprehensive new synthesis papers of practically all these variables have been produced since the last IPCC report. It is not correct to only be citing new results based on a small number of measurements if previous syntheses exist. If no new synthesis exists, you can update an older synthesis with the new data points, but should not be throwing away the previous result. [Julia Hargreaves, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The charge is to consider the new evidence since AR5 and thus the stipulated requirement is to focus upon the literature since 2012. This was clarified at LAM3 by the Co-Chairs in plenary.
9464	0				Despite the very rich information provided in the report in general and in chapter 2 in particular it is suggested to address another relevant question - how long will it take for the carbon dioxide concentration to return to preindustrial levels - without additional human intervention. This is not only a theoretical question but a rather relevant question in the context of assessing the possible contribution of solar radiative forcing management to control climate change risks. This is because there is a lot of uncertainty about the time period for which such activity would have to be sustained. [Klaus Radunsky Radunsky, Austria]	Rejected. Out of scope of chapter. This is the domain of chapter 5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45412	1	1	10	1	The paragraph, section, and bolding of this executive summary is not consistent with that of other chapters or recent reports. I suggest each paragraph should have a bolded statement including at least one assessment, and that the division into sections of executive summary might be made more tacitly (although I admit that I like this sectionated exec. summary and would like that form to be adopted report-wide). [Baylor Fox-Kemper, United States of America]	Taken into account. We have completely redrafted the ES to be consistent with guidance provided by TSU following LAM3
50232	1	1	100	1	choose between century and Century and check it through the whole chapter [Sophie SZOPA, France]	Taken into account. We have adopted the IPCC style guide in the SOD drafting.
14514	1	1	197	6	A major concern of mine would be the possible overlap of contents of this chapter contents with those of other chapters of WG1 especially WG2, and the related overlook of the atmospheric component which is the fundamental one for understanding climate change. Different from the previous IPCC observational chapter, this one include too many components of the climate system. This procedure has its advance, but also brings the two big problems as mentioned above. I noted the obvious insufficiency of assessment of the atmospheric change. Overall, this approach is somehow defective. It increases the length of all the relative chapters, and at the same time induces the incompleteness for assessment of the key scientific issues. (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. Comment is inconsistent with scope adopted by the Parties and given to the chapter team.
7204	1	14	1	15	Michael Byrne (UK / Ireland) [Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Comment unclear and assumed to be in error on part of reviewer. Not actionable.
49352	1		197		Overall comment on Chapter 2: This chapter is rich with information from a wide range of relevant timescales. Well done. One quibble is that throughout the chapter I was wanting to see the discussed changes in eg CO2 and temperatures linked to their well-documented consequences (e.g., ocean acification and extinctions in the PETM, glacier and ice sheet changes in the LIG). Some of these links can be made by readers themselves, who for example read the temperatures section and the glaciers section and put these pieces together. But a few more sentences here and there could make these linkages more clear and certainly more explicit. It was very likely a conscious choice by the authors not to do so, to keep things concise perhaps. But I think adding a few things here and there would be useful to readers. I've made notes of a couple such ideas (pertaining to glaciers & ice sheets) above, and one more (pertaining to PETM CO2) below. [Yarrow Axford, United States of America]	Taken into account. We have considered this where space and narrative permits. Section 2.3.5 is the attempt to draw this together in precisely such a manner and we have attempted to strengthen this accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6273	2	5	2	5	General: (2.2) Energy is one of the main climate change driver. Energy balance and energy consumption pattern is also needed to be considered, 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]	Rejected. Energy is not a radiative forcing agent so is out of scope.
40902	2	21	2	21	The 6 in SF6 should be subscript. [Johannes Laube, Germany]	Editorial, the accepted report will undergo professional copy-editing before publication.
35486	2				According to the IPCC guidance note on uncertainties 'Likelihood may be based on statistical or modeling analyses, elicitation of expert views, or other quantitative analyses.' Therefore likelihood assessments should only be given where there is underlying quantitative analysis - for example if an ensemble observational dataset exists which is assessed to sample over all relevant uncertainties, and more than 90 of 100 ensemble members show a positive trend over a given period, a positive trend over that period could be assessed as very likely. In the ES most sections use exclusively confidence assessments, but the section on atmospheric circulation change is an exception. Is there really more quantitative analysis underlying the assessments in this section? (Ref: https://wg1.ipcc.ch/SR/documents/ar5_uncertainty-guidance-note.pdf). [Nathan Gillett, Canada]	Rejected. At LAM3 the bureau members clarified when and how confidence / likelihood should be used and our usage has been checked against this. The guidance is not as strict as implied here.
40904	3	4	3	4	Why are precipitation and evaporation starting with capital letters? Worth checking for the entire Table of Contents. [Johannes Laube, Germany]	Taken into account. Revised. See also comment 40902
17916	3		11		Some abbreviations are spelled out, i.e., explained, but some are not. That should be unified, i.e., all explained when used for the 1st time. [Branko Grisogono, Croatia]	Taken into account. Efforts have been made to consistently spell out on first use.
7818	4	23	4	25	we need say land surface air temperature/minimum/maximum first, then DTR because reader may conclude minimum reduce slower than maximum. [zhiyan zuo, China]	Rejected. The major piece of evidence here is the DTR papers of Thorne et al. 2016; there is no recent data set of maximum and minimum temperatures, and hence conclusions on maximum and minimum temperatures are inferred from the GMST trends combined with the (much smaller) DTR trends.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39478	5	1	11	5	Avoid the use of acronyms. If they are needed, define them. [Carolina Vera, Argentina]	Taken into account. The ES as submitted for SOD should not contain any undefined acronyms
56212	5	1	11	5	Executive Summary: Would be useful to include a sentence in the Executive Summary of Chapter 2 pointing to later chapters addressing regional changes, in particular Chapter 11, which addresses observed changes in extremes, e.g.: "Observed changes in extremes, including at regional scales, are assessed in detail in chapter 11". [Sonia Seneviratne, Switzerland]	Rejected. This is already done in the introduction where we feel it is more appropriate to do so.
53294	5	1	11	6	The ES should follow the guidance given; see eg ch 5 and 3. [Jan Fuglestedt, Norway]	Taken into account. See comment 47996
47996	5	1	11		Exec Summary has been fomatted like an SPM (bolded headline statments with bullets underneath) but an ES should be divided into subsections (not necessarily in order of the chapter) with the first sentence of each point in bold. Orbital forcing and palea are either missing or limited in the ES. Please refer to the SR1.5°C fo examples of this format. [WGI TSU, France]	Taken into account. See response to comment 45412
8522	5	3	5	3	"Historical changes in drivers of climate change" [Robert Kopp, United States of America]	Taken into account. See comment 35462
35458	5	3	6	4	The opening paragraph of this section starts by saying that radiative forcing is dominated by greenhouse gases and aerosols. But the paragraphs below start with changes in solar irradiance, and only discuss aerosols near the end fo the section. I suggest starting with WMGHGs, then aerosols, then other forcings. [Nathan Gillett, Canada]	Rejected. We discussed this at length and prefer to open with the changes in natural drivers in the detailed findings to avoid accusations of favouring anthropogenic drivers.
28770	5	5	5	6	"Changes in drivers of radiative forcing" is strange wording. Change to "Climate change is driven by greenhouse gas and aerosol changes over the 19thC"..? Radaitive forcing is already a change and forcing and driver mean #the same thing? I also don't like the imbalance word - as you are not talking about net energy balance here but forcing. I would be explict and say the radaitive forcing has grown more over the last decade than in preceeding decades? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. See comment 35462
24490	5	5	5	7	This headline summary of the chapter focusses too much on radiative forcing. "The net effect is a positive imbalance of the radiative budget" is too technical for the top summary point. Not least because this is also an ES point from Ch 7. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The summary was combined and reconciled with chapter 7 finding in the TS and SPM. However, our scope does include changes in radiative drivers so we do need to assess these.
56206	5	5	5	8	Very strong statement, well summarized. [Sonia Seneviratne, Switzerland]	Noted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28772	5	5	6	9	Different words used forcing effective radiative forcing: climate forcing, radiative forcing etc. Use ERF everywhere? Quantification overlaps with Chapter 7 but maybe not a problem as long as we say same? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now use ERF consistently.
35462	5	5			I suggest replacing 'Changes in drivers of radiative forcing' with 'Changes in radiative forcing'. [Nathan Gillett, Canada]	Taken into account. Wording has been changed to a more appropriate phrasing here.
44622	5	6	5	6	"higher than" corrected to "higher than that" . [Liang Zhao, China]	Taken into account. See comment 35462
35440	5	6	5	7	The meaning of 'is accelerating' is not clear. I suggest 'is increasing'. 'Accelerating' implies a second derivative of the radiative forcing, which I don't think is what is meant. [Nathan Gillett, Canada]	Taken into account. The key finding has been revised based upon the broad range of comments received.
11548	5	10	5	11	Authors write "Changes in total solar irradiance during the industrial era are not unusual in the context of at least the past 9000 years (medium confidence)". What do authors want to say with this? This statement hides the fact that the second half of the 20th century was actually one of the most active phases of the entire Holocene. See Steinhilber et al. 2012 (doi 10.1073/pnas.1118965109) and Solanki et al. 2004, https://www.nature.com/articles/nature02995 . In contrast to sun spots, the solar magnetic field reached its highest values in the late 20th Century. Readers need to know this information to place the second half of the 20th century in a meaningful context. [Sebastian Luening, Portugal]	Taken into account - reviewer requests additional context for this ES statement, which is presented in the text.
48774	5	10	5	12	formulation "not unusual" is not very clear: rather "in the range" ? [Sylvie JOUSSAUME, France]	Taken into account in ES revisions
28774	5	10	5	13	Need to say what changes are in solar and volcanic trends? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Reject - We think the reviewer is asking to quantify trends in solar and volcanic forcing. The details are in the text. The ES statements here are crafted to support of the headline regarding the unusualness of recent changes in a way that is simple and concise.
24494	5	10	5	13	The solar irradiance is less important than some of the other conclusions and so should be moved down the list somewhat. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Reject - The order of the statements matches the order of sections in the text.
47532	5	11	5	12	see comments 5 and 6 above. [Matthew Toohey, Germany]	Unclear - reviewer's comment numbers are not preserved.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8738	5	15	5	19	Time is expressed in years and kiloyears (ka), and CE. The units of time should be consistent for the entire report, and for the benefit of the public use mainstream terminology (i.e. thousands of years instead of ka). [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, in ES wherever possible years are used. Also x-chapter box 2.1 (Table 1) provides explicit info on time scales
51794	5	15	5	19	WMGHG was never defined in the text, but was used here. [Anson Cheung, United States of America]	Editorial
24492	5	15	5	19	This is more important point than solar radiance and should go above it. It could also be re-phrased to give more emphasis to the present WMGHG concentrations being the highest for x million years. Note that this ES point is very similar to the first ES point in Ch 5. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the ES has been completely restructured
8740	5	15	5	24	Both mixing ratios and concentrations are used to express the level of GHG in the atmosphere. The general public will be more familiar with concentrations expressed in ppm [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - as suggested "concentration" is better for readers, but also scientifically defined somewhere else.
35442	5	15			Instead of writing 'fluctated by at least 2000ppm' it would be easier to understand and more informative to say 'fluctuated between approximately x and yppm'. [Nathan Gillett, Canada]	Taken into account . ES has been completely redrafted
44628	5	16	5	16	"The last time CO2 levels were similar to the present-day was over 2 million 17 years ago" corrected to "The last time CO2 levels were similar to the present-day, which was over 2 million17 years ago". [Liang Zhao, China]	Editorial
52314	5	17	5	17	no comma after "While" [Katherine Glover, United States of America]	Editorial
40906	5	17	5	17	No comma necessary after "While". Also, shouldn't this be "prior to 800 ka ago"? [Johannes Laube, Germany]	Editorial
26124	5	17	5	20	Define WMGHG [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
32636	5	18	5	18	It seems to me that WMGHG needs to be defined in the text, and also why it is important, so a bit more than a definition-- and perhaps then indicate that there are then also other warming agents. [Michael MacCracken, United States of America]	Editorial
55972	5	18	5	18	The initial WMGHG could be detailed when it appears for the first time (same comments for GMST and GSAT). [Martin Ménégoz, France]	Editorial
23462	5	18	5	18	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23464	5	18	5	18	Please define WMGHG in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
53978	5	18	5	18	WMGHG should be in full at the first mention here in the ES (sadly, it may be the only section of the chapter that is read!) [Timothy Carter, Finland]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35444	5	18			At this point in the chapter 'pre-industrial' hasn't been defined as 1750-1900. In normal usage this term just means before the industrial revolution, so as written the statement that it is certain that pre-industrial WMGHG mixing ratios were lower than present day levels is not correct, since they have been higher at some points in Earth's history. Include a definition of pre-industrial here. [Nathan Gillett, Canada]	Accepted - We better define now. GHG concentrations in 1750 and 1850 are provided
8742	5	21	5	24	It would be useful to give an indication how does the increase of GHG concentrations since the preindustrial compare to the change from the Last Glacial Maximum to pre-industrial levels. [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The summary statement includes values on LGM for CO2; CH4; N2O).
24496	5	21	5	24	Suggest phrasing these changes as percentages, as it is not obvious to the non-expert how important these ppb are. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The ES has been completely redrafted
53980	5	21	5	24	Though it is probably more straightforward to report anomalies relative to pre-industrial than absolute abundances, given variations in observed values across measuring sites, it would be remiss if this ES did not report absolute values and their uncertainty ranges here. CO2 concentration is an iconic value that really should be included up front here (alongside the increase). [Timothy Carter, Finland]	Accepted. ES now mentions: reaching in 2018 levels of 407.4 (± 0.3) ppm, 1858.6 (± 3) ppb, and 331.2 (± 0.3) ppb, respectively.
17952	5	22	5	22	CH4 concentration should be 46 ppb. (in text 46.4 ppb) [Gwenaelle GREMION, Canada]	Rejected. ES statement is refocused and does not mention the increase in recent period anymore.
8524	5	22	5	23	Reads weirdly, as most are use to seeing actual concentrations, not just delta concentrations. Show both? [Robert Kopp, United States of America]	Accepted. Statement has been completely redrafted
15988	5	22	5	23	The figures of increases and abundances of CO2, CH4 and N2O are different from the values reported in the WMO Greenhouse Gas Bulletin (https://library.wmo.int/doc_num.php?explnum_id=5455). May need to double check. [SAI MING LEE, China]	Noted. The reference method used in this chapter is the NOAA marine boundary layer concentrations. WMO-GAW global mean mixing ratios are shown in Table 2.2, and agree with values shown in the WMO Greenhouse Gas Bulletin, 2018.
37394	5	22			"since 2011" is too vague, as the reader does not know when "now" is. "increase since 2011 by ... N2O." should be replaced by "increase, by ... N2O from 2011 to 20xx" or something similar. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Statement clarified and rephrased.
31102	5	23	5	23	Probably wanted to write 418, not 118. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The statement refers to the abundancies above pre-industrial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48776	5	23	5	23	First time I see using the difference with pre-industrial. I would rather recommend using absolute values since those values are better known than relative values. Radiative forcing are given for other forcing: would be good to add them for all forcings. [Sylvie JOUSSAUME, France]	Accepted. The ES statement has been revised, and all relevant sections provide an ERF estimate, derived from Chapter 7.
7820	5	23	5	23	punctuation wrong. Particularly. For the Southern Hemisphere [zhiyan zuo, China]	Editorial
26126	5	23	5	24	This sentence was written with the subject at the end. Better is "Their abundances above pre-industrial levels are now 118.1 ± 1.6 ppm, 1043 ± 6 ppb and 59 ± 4 ppb respectively (very high confidence). {2.2.4.2} [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Summary statement is modified.
8194	5	23			I don't quite understand the reference point here. According to Table 2.1, "pre-industrial" refers to the year 1750, which saw a CO ₂ abundance (according to various sources) of 277 ppm. Also according to Table 2.1, the "present" refers to the years 1995-2014. If I average CO ₂ abundances over the years 1995-2014, I get 379 ppm (according to data in http://www.johnstonsarchive.net/environment/co2table.html , also https://www.co2levels.org/). The difference is 379-277=102 ppm. But the text quotes an increase of 118.1 ppm above pre-industrial. So ... what am I missing? [Steven Neshyba, United States of America]	Noted. Different data compilations lead to slightly different changes. Here we base our estimates on the NOAA methodology. The numbers have been updated to quote the difference between 2018 and 1750.
35446	5	23			Specify somewhere what these confidence intervals are (5-95%, one sigma?). I suggest using 5-95% confidence ranges throughout the chapter. [Nathan Gillett, Canada]	Taken into account in redrafting ES
40908	5	26	5	26	This should be "Montreal Protocol". [Johannes Laube, Germany]	Accepted. Indeed Kigali will restrict emissions at a later point in time.
53284	5	27	5	27	I suggest you write "total direct" since this is an aggregate of gases, and since indirect effects via strat O ₃ are not included. [Jan Fuglestedt, Norway]	Accepted. We follow nomenclature in Chapter 7.
40910	5	27	5	27	It seems odd to state the radiative forcings of the halocarbons, but not those from the 3 main WMOGHGs. In general for this section I find that changes are reported in an inconsistent mixture of measures including ppm, ppb, W m ⁻² , and percentages. [Johannes Laube, Germany]	Accepted. ERF values in all relevant sections and reported changes more uniformly, based on calculations in Chapter 7.
53286	5	28	5	29	I don't think the last part of the sentence on imposed emission reductions is needed here. [Jan Fuglestedt, Norway]	Accepted.
8526	5	28	5	29	What is the germaneness of the Kigali Protocol in this context? Is it just a reference time point? If so, should this be "when" rather than "with"? I might delete, since this is not about the physical system. [Robert Kopp, United States of America]	Accepted. Reference to is removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35448	5	28	5	29	The meaning of the phrase 'with emissions restrictions imposed under the Kigali Amendment of the Montreal Protocol' is unclear as written. As written this could be interpreted as saying that the Kigali Amendment has limited the radiative forcing of HCFCs and HFCs, but this is not the case, since the Kigali Amendment only came into force in 2019. I suggest instead inserting after 'HFCs' - ', which are now regulated by the Kigali Amendment of the Montreal Protocol'. [Nathan Gillett, Canada]	Accepted. Sentence is rephrased.
53982	5	28	5	29	It isn't clear how the Kigali Amendment is supposed to be related to the values preceding its mention. Is this implying the the radiative forcing is constrained to those values because of the Amendment, or what? [Timothy Carter, Finland]	Accepted. Reference to Kigali is removed
17954	5	29	5	29	CFC confidence statement should refer to section 2.2.4.3 [Gwenaelle GREMION, Canada]	Accepted. Corrected
35450	5	31	5	35	The overall message of this paragraph is not clear. Is the paragraph saying that the satellite measurements of water vapour show no long-term increase, and the Boulder record is not representative of the global mean? Or is the paragraph saying that there is real disagreement between the satellite record and the Boulder record? Clarify. [Nathan Gillett, Canada]	Accepted. Statement is revised.
12610	5	37	5	39	Add that stratospheric ozone is starting to show signs of recovery, with noticeable improvements emerging expected by the 2030s and repair of the Antarctic ozone hole expected around 2060. World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58. Note also that the unexplained and unreported emissions of CFC-11 put at risk the continuing recovery of the stratospheric ozone. Montzka S. A., et al. (2018) An unexpected and persistent increase in global emissions of ozone-depleting CFC-11, NATURE 557:413–417; World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Executive Summary, Global Ozone Research and Monitoring Project-Report No. 58, ES.3; Rigby M., et al. (2019) Increase in CFC-11 emissions from eastern China based on atmospheric observations, NATURE 569:546–550. [Kristin Campbell, United States of America]	Partly accepted/noted. Chapter 2 exclusively deals with observations, and not with future concentrations, or emission attribution. For CFC-11 we include a more recent publication that was not available at the time of FOD submission (Rigby et al. 2019). We include a reference to the WMO 2018 Scientific Assessment of Ozone Depletion.
24498	5	37	5	39	The second sentence would be better phrased as a recovery i.e. an increase compared to the 2.5%. The two baseline timeperiods "pre-1980" and "1964-1980 average" need to be the same. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected to the 1964-1980 averages. The statement about 2.5% is actually from the WMO Ozone Assessment 2010 and the baseline there was 1964-1980.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12760	5	37	5	39	Add that stratospheric ozone is starting to show signs of recovery, with noticeable improvements emerging expected by the 2030s and repair of the Antarctic ozone hole expected around 2060. World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58. Note also that the unexplained and unreported emissions of CFC-11 put at risk the continuing recovery of the stratospheric ozone. Montzka S. A., et al. (2018) An unexpected and persistent increase in global emissions of ozone-depleting CFC-11, NATURE 557:413–417; World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Executive Summary, Global Ozone Research and Monitoring Project-Report No. 58, ES.3; Rigby M., et al. (2019) Increase in CFC-11 emissions from eastern China based on atmospheric observations, NATURE 569:546–550. [Durwood Zaelke, United States of America]	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations, or emission attribution.
53984	5	37	5	39	Wouldn't it be more logical for this point to follow the abundance information and precede the water vapour findings? Although these are presumably included here as radiative forcers as much as ozone destroyers, the latter link is obviously relevant and the decline still ongoing, in spite of reduced abundances. Would a comment on this be pertinent here, given that the this issue has been in the media recently, with suggestions of regional non-compliance to Montreal and Kigali? [Timothy Carter, Finland]	Rejected. The sequence of key-findings follows the chapter structure. Further information on non-compliance issues is out of scope for this chapter
37396	5	37			What is the non-specialist reader to make of the "Near global (60S-60N)" wording? It could imply there are insufficient observations to draw a conclusion poleward of 60N and 60S. But it could imply there are observations, but that ozone does not decline in the polar regions. But the non-specialist reader has probably heard about the ozone hole, so may wonder why the statement is limited to 60S-60N. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The revised text includes separate statements on non-polar and polar regions.
35454	5	41	5	43	The first sentence says 'while surface trends are variable' in the NH, which implies no significant trends. The second sentence says that ozone has *also* increased at the surface in the SH, implying that it has increased at the surface in the NH. Clarify whether or not ozone has increase at the surface in the NH. [Nathan Gillett, Canada]	Accepted. The word *also* has been deleted.
35452	5	41			Replace 'free tropospheric ozone' with 'ozone in the free troposphere'. [Nathan Gillett, Canada]	Rejected. The proposed change is more wordy.
23466	5	48	5	48	Please note the acronymy for Aerosol Optical Depth (AOD) here [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35456	5	48	5	53	I suggest starting this paragraph with the final sentence giving the long-term context - that aerosols showed increasing trends during the industrial era. Then follow with the current first sentence indicating that aerosol optical depth has decreased since 2001. [Nathan Gillett, Canada]	Taken into account in redrafting ES
23468	5	50	5	50	insert 'it' after Asia, [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
53986	5	51	5	51	Is the anthropogenic cause decreasing, or is there an anthropogenic cause for the decreasing AOD? Wording is ambiguous here. [Timothy Carter, Finland]	This sentence is not retained in the SOD.
23470	5	53	5	53	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
53288	6	1	6	1	"equivalent" is not the right word here. I suggest simply deleting it. [Jan Fuglested, Norway]	Taken into account - combined with comment 24500.
24500	6	1	6	1	"radiative forcing" should be "effective radiative forcing". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
8528	6	1	6	2	What is the uncertainty range here? Likely? Very likely? [Robert Kopp, United States of America]	Rejected - assessment performed using a confidence statement rather than a likelihood statement.
12612	6	1	6	4	Global albedo is also affected by changing sea ice coverage, particularly in the Arctic, where loss of reflective ice from 1979 through 2011 added 6.4 Wm ² to Arctic warming, which averaged globally is equivalent to 25% as much as CO ₂ added in this period.. Pistone K., et al. (2014) Observational Determination of Albedo Decrease Caused by Vanishing Arctic Sea Ice, PROC. NAT'L. ACAD. SCI. 111(9):3322–3326. [Kristin Campbell, United States of America]	Taken into account - text revised ("global" replaced with "terrestrial" to clarify that the assessment refers to land areas).
12762	6	1	6	4	Note the opportunity to further enhance global albedo, including reflective roofs and pavement. See e.g. Santamouris, Mattheos. "Cooling the cities—a review of reflective and green roof mitigation technologies to fight heat island and improve comfort in urban environments." Solar Energy 103 (2014): 682-703; and Kyriakodis, G. E., and M. Santamouris. "Using reflective pavements to mitigate urban heat island in warm climates-Results from a large scale urban mitigation project." Urban Climate 24 (2018): 326- 339. [Durwood Zaelke, United States of America]	Rejected - beyond the mandate of WGI (comment refers to mitigation).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12764	6	1	6	4	Global albedo is also affected by changing sea ice coverage, particularly in the Arctic, where loss of reflective ice from 1979 through 2011 added 6.4 Wm ² to Arctic warming, which averaged globally is equivalent to 25% as much as CO ₂ added in this period. Pistone K., et al. (2014) Observational Determination of Albedo Decrease Caused by Vanishing Arctic Sea Ice, PROC. NAT'L. ACAD. SCI. 111(9):3322–3326. [Durwood Zaelke, United States of America]	Taken into account - combined with comment 12612.
40912	6	1	6	9	The importance of cryospheric changes for the global albedo is not clear from these paragraphs. [Johannes Laube, Germany]	Taken into account - combined with comment 12612.
23472	6	2	6	2	Capital C for century (for consistency elsewhere in Chapter [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)])	Editorial.
7822	6	2	6	3	why the confidence is not high, but for medium since the observation after 2000s should be the best? [zhiyan zuo, China]	Rejected - the paragraph addresses confidence in changes since the late 19th Century, not confidence during the 2000s.
53290	6	4	6	4	re "no agreement on sign of effect": Please check SRCL and coordinate with ch7. [Jan Fuglestedt, Norway]	Rejected - statement is broadly consistent with SRCL and Chapter 7.
15990	6	6	6	6	The reference year for radiative forcing should be 1750, not 1850. See Ch.1, P.5, Line 17. Also see Ch.2, P.29, Line 54. [SAI MING LEE, China]	Accepted, and sorry for the confusion. The radiative forcing has to be defined with respect to 1750.
23474	6	6	6	9	This is a bit vague (or reads that way). Please quantify 'a few years' and also what a moderate to large volcanic eruption is viewed to be (perhaps with an example or a volcanic explosivity index rating [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)])	This statement is not retained in the SOD
35460	6	6			I suggest replacing 'net radiative effect' with 'net radiative forcing'. [Nathan Gillett, Canada]	Editorial
35464	6	14	6	15	The authors have tried to strength the AR4 assessment that warming is unequivocal by broadening focus to indicate that 'Directly observed changes in key large-scale atmospheric, oceanic, cryospheric and biospheric indicators of climate' are unequivocal'. But this is actually a much weaker statement, since all it is saying is that changes have been observed in these indicators, without saying what these changes are or whether they are consistent with warming. I suggest re-framing in terms of observed changes which are consistent with warming. Finally, are the authors convinced that observed changes in key large-scale biospheric indicators are unequivocal? [Nathan Gillett, Canada]	Taken into account. The key finding has been revised based upon the broad range of comments received.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12614	6	14	6	17	Additionally, it is important to point out that many of these indicators—like overall warming and rate of warming as well as carbon dioxide concentration—reflect an acceleration of climate change. [Kristin Campbell, United States of America]	Taken into account. The key finding has been revised based upon the broad range of comments received.
56208	6	14	6	17	Very strong statement, well summarized. [Sonia Seneviratne, Switzerland]	Noted with thanks.
29844	6	14	6	17	One message that the IPCC has not emphasized is the speed of change. Never in the history of the world (probably) that the CO2 concentrations increased by 120 ppm in a century. Also, never in the history of the planet, that the planet warmed by 1 deg C in a century. I think that this powerful message is not out there yet and it could be made here. [Govindasamy Bala, India]	Taken into account. The key finding has been revised based upon the broad range of comments received.
12766	6	14	6	17	Additionally, it is important to point out that many of these indicators—like overall warming and rate of warming as well as concentration of carbon dioxide and other climate pollutants—reflect an acceleration of climate change. The rate of global annual temperature increase has more than doubled in recent decades to 0.17 °C per decade. The rate of CO2 concentration in the atmosphere also is accelerating, growing to a rate of 2.48 ppm/year in 2018; for comparison, the average increase of CO2 in the 1980s was about 1.6 ppm/year and 2.2 ppm/year during the last decade (2008–2017). The accelerating warming is being driven not only by continuing emissions, but also by self-reinforcing feedbacks. Xu Y., et al. (2018) Global warming will happen faster than we think, NATURE, Comment 564:30–32; National Oceanic and Atmospheric Administration (NOAA), Global Climate Report - Annual 2018 (last accessed 15 June 2019) (“During the 21st century, the global land and ocean temperature departure from average has reached new record highs five times (2005, 2010, 2014, 2015, and 2016), with three of those being set back-to-back. From 1880 to 1980, a new temperature record was set on average every 13 years; however, for the period 1981–2018, the frequency of a new record has increased on average to once every three years. Nine of the 10 warmest years (listed below) have occurred since 2005, with the last five years (2014–2018) ranking as the five warmest years on record. The year 1998 is the only year from the 20th century among the ten warmest years on record, currently tying with 2009 as the ninth warmest year on record. The yearly global	Taken into account. The key finding has been revised based upon the broad range of comments received.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37398	6	14			The text states "atmospheric, oceanic, cryospheric and biospheric". Are there no terrestrial variables other than cryospheric and biospheric ones that give an unequivocal signal? Soil moisture and global river runoff are mentioned as indicators in Box 2.1, but are referred to as atmospheric indicators, not terrestrial ones. This is counter to GCOS ECV terminology, and bemuses me. Soil moisture and runoff depend on atmospheric inputs, but they also depend on terrestrial variables such as soil texture. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We discussed at length at LAM3 how we may do this and decided that splitting terrestrial off would be unhelpful to the reader and also necessitate changes in other chapters which would be hugely challenging. We have modified what was cross-chapter box 2.1 (now 2.2) for clarity.
35330	6	16	6	16	"some have exceeded conditions over many millennia" - not clear to me - the exceedence has taken many millennia or the exceedance is over values which have been stable for many millennia [Dunn Robert, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The key finding has been revised based upon the broad range of comments received.
32304	6	17	6	17	'...unusually rapidly...'. This text is imprecise (i.e. the reader needs to know what a 'usual rapid' change is and how 'unusual' the change being referred to is in order to gain information from it)... so can be it be rephrased? [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The key finding has been revised based upon the broad range of comments received.
48780	6	17	6	17	The unusual rapidity of change is I think crucial. It would deserve a specific sub-paragraph for temperature since it is only mentioned for oceans and biosphere. [Sylvie JOUSSAUME, France]	Taken into account. The key finding has been revised based upon the broad range of comments received.
28776	6	19	6	19	needs to do a better job of explaining GMSAT, GSAT - and defining them? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This issue has now been highlighted via a new cross-chapter box 2.3.
11560	6	19			Why are pre-industrial temperature changes and ocean cycles (AMO, NAO, PDO...) not being discussed here? [Sebastian Luening, Portugal]	Rejected. They are discussed in the appropriate places.
8744	6	21	6	21	add (GMST) after Global Mean Surface Temperatures. [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
8746	6	21	6	21	Deep past is an ambiguous term so perhaps better to mention something like "over the last xyz million years" instead [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified in the modified surface temperature key findings.
39474	6	21	6	21	Explain what "deep past" means [Carolina Vera, Argentina]	Taken into account. See response to comment 8746
55974	6	21	6	21	Global Mean Surface Temperatures -> Global Mean Surface Temperatures (GMST) [Martin Ménégoz, France]	Editorial
23476	6	21	6	21	Insert (GMST) to introduce acronym [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
53988	6	21	6	21	I'm not sure readers will be acquainted with the term "deep past" - should this be defined or referred to as geological or pre-instrumental or something more immediately identifiable? [Timothy Carter, Finland]	Taken into account. See response to comment 8746

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57912	6	21	6	26	Perhaps mention here that spatial variations in temperature are large (polar amplification), and that polar amplification is significant (see AR5 Box 5.1) [Bas de Boer, Netherlands]	Taken into account. Revised the surface temperature key findings
8530	6	21	6	26	What is the uncertainty range here for numbers presented as $x \pm y$? Likely? Very likely? [Robert Kopp, United States of America]	Taken into account in ES revisions.
29112	6	21	6	26	Are you sure that it is wise to place a summary paragraph worded like this here? I see no incorrect science in it, but it plays directly into the hands of those lobbyists who misrepresent palaeoclimate with "climate has changed before ... so current climate change isn't worrying". At a minimum, I feel you should provide layman context for the LGM and the Eocene as well as their dates (e.g. 'before farming was invented' and 'at the start of the Age of Mammals'). [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Noted - CH2 remit is to assess observational evidence for the changing state of the climate system, including temperature. Whether recent changes are worrying is not a criterion in CH2. The context for this information is in CH1 and less-technical summaries are in FAQs.
42886	6	21	6	45	Ambiguous: last sentence here is likely referring to global marine surface temperatures (GMST), but previously GMST is global mean surface temperatures. Clarify. [Michael Evans, United States of America]	Taken into account - last sentence specifies "GMST", which is correct and unambiguous.
57814	6	21		26	The Global mean surface temperature of the past and Pre-industrial era were both higher and lower. Therefore; An integrated approach on studie of pressure belts and prevailing winds movement should be properly checked. As this may affect the mean global temperatures of the earth in comparison with the early age and the early Eocene climate optimum. [Abiodun Adegoke, Nigeria]	Noted - Changes in surface pressure and winds are discussed elsewhere in CH2.
23478	6	22	6	22	Change to Pre-Industrial for consistency elsewhere in the text, and insert 'times' after 'Industrial' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised the surface temperature key findings
49346	6	22	6	22	"Within the Cenozoic, GMST relative to pre-industrial ranged..." (Suggest adding the context of the Cenozoic, because there are deeper-time pre-Cenozoic warm and cold extremes that are not mentioned here.) [Yarrow Axford, United States of America]	Taken into account. Revised the surface temperature key findings
13150	6	22	6	25	What is the confidence level for the Eocene temperature estimate and what is the uncertainty? [Nora Richter, United States of America]	Taken into account. Revised the surface temperature key findings
33104	6	23	6	24	Where does the 6 ± 2 degrees come from? Shakun et al 2012 seems to be about 3 degrees. Annan and Hargreaves (2013) based on the data in that compilation and in the MARGO SST compilation estimate 4.0 ± 0.8 . It is hard for me to imagine 6 degrees being consistent with the sea surface temperature data. [Jean Lynch-Stieglitz, United States of America]	Taken into account - see section 2.3.1.1.1 where both land and sea temperatures are discussed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8748	6	24	6	24	Specify the period of time covered by the last interglacial (it is given in Table 2.1 but perhaps useful to specify here as well) [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised the surface temperature key findings
40914	6	24	6	34	For consistency and understandability it would be good to give the periods of the last inter-glacial, the Holocene, and the Common Era as well. [Johannes Laube, Germany]	Taken into account. Revised the surface temperature key findings
39476	6	28	6	30	Avoid the use of "possibly". Locate the medium confidence level of the first sentence after "Little Ice Age" [Carolina Vera, Argentina]	Taken into account. Revised the surface temperature key findings
8750	6	28	6	34	What ages are you using to define the Little Ice Age and the Medieval Climate Annomaly [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised the surface temperature key findings
13152	6	28	6	34	Emphasize that LIA and MCA are mostly observed in the Northern Hemisphere, particularly in the North Atlantic region and the Arctic. [Nora Richter, United States of America]	Reject - This statement focuses on GMST rather than regional-scale differences. In addition, warm and cold periods are known from other regions during these intervals.
35466	6	28			The sentence is poorly phrased because it isn't clear how much confidence is assigned to the cooling in the SH. If the confidence level for cooling is different for the SH than the NH, write these out separately. 'There is medium confidence that surface temperatures over the NH have tended to gradually decrease over the past several thousand years, and low confidence that surface temperatures have decreased over the SH over the same period' or similar. 'Possibly' isn't part of the calibrated uncertainty language. [Nathan Gillett, Canada]	Taken into account. Revised the surface temperature key findings
11550	6	30	6	31	Authors write "...Little Ice Age, which was globally the coldest multi-century interval of the Holocene". This is true. On the other hand, why does the AR6 report in Chapter 1 choose the Little Ice Age then as baseline for temperature? A meaningful approximation for „pre-industrial global temperatures“ has to represent an average temperature over a longer (late) Holocene time span, e.g. the last 2000 or 10,000 years (until 1850). The choice 1850-1900 does clearly not fulfil this criterion. See Lüning & Vahrenholt 2017 (doi: 10.3389/feart.2017.00104) for details. [Sebastian Luening, Portugal]	Noted - Choice of the pre-industrial reference period and associated terminology is discussed in CH1.
35806	6	30	6	32	This high confidence statement seems to be in disagreement with chapter text on pg 38, ln 38-41, which describes ascribing 'mostly colder conditions' to the LIA as a 'well-documented oversimplification' and cites references which argue that there were not globally coherent warm or cold periods in the CE. [Nathan Gillett, Canada]	Accepted - Added, "averaged globally" to clarify the basis of the statement. While the text covers regional differences, the ES focuses on global metrics.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46796	6	31	6	31	In many regions this reversion (i.e. warming) actually first occurred by the turn of the 20th century and the late 19th century instead was rather cool with even glacier advances. What type of trend break analysis is the statement based on? [Charpentier Ljungqvist Fredrik, Sweden]	Taken into account. Revised the surface temperature key findings
11552	6	31	6	32	Authors write: "Surface temperatures over the Northern Hemisphere, and possibly also the Southern Hemisphere, have tended to gradually decrease over the past several thousand years (medium confidence) culminating in the Little Ice Age, which was globally the coldest multi-century interval of the Holocene (high confidence). This cooling trend was reversed during the mid 19th Century (high confidence)." This is not entirely true. Several millennial-scale temperature cycles occurred (Bond cycles, Bond et al 2001 in Science) which brought already previous brief warm phases of a few centuries which include e.g. the Medieval Climate Anomaly and the Roman Warm Period. Global Holocene long-term temperature reconstructions such as the ones by Marcott et al 2013 are not able to resolve these because data points are too widely spaced and age models too uncertain. A monotonous long-term cooling as is suggested in this chapter 2 does not represent current knowledge of the palaeoclimate community. [Sebastian Luening, Portugal]	Taken into account. Revised the surface temperature key findings
35468	6	31			The phrase 'was reversed during the mid-19th century' implies that the trend was only reserved in that period. 'reversed in the mid-19th century' would be better. [Nathan Gillett, Canada]	Taken into account. Revised the surface temperature key findings
7824	6	32	6	33	could give some name of the regions for the many place? [zhiyan zuo, China]	Rejected - Statement in report refers to global average, not to the regional changes.
16130	6	32	6	34	"...period of the Common Era..." -Please be more specific. [Branko Grisogono, Croatia]	Taken into account - Statement has been Omitted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11554	6	32	6	34	Authors write: “The GMST of the past 50 years was likely warmer than any 50 year mean of the Medieval Climate Anomaly (high confidence), and more likely than not was the warmest 50-year period of the Common Era (medium confidence).” A high confidence statement is being made about the MCA, even though the opposite is being claimed in Chapter 1 (page 69, lines 35-37: “Although Lüning and Vahrenholt (2017) suggest a much longer context for defining pre-industrial, estimates of natural radiative forcings and global temperature are too uncertain to allow a reliable estimate for longer periods.” How can a high confidence statement being made under those circumstances. In fact, in reality global and hemispheric temperature reconstructions are still unstable and change significantly from version to version. A series of regional syntheses for the MCA documents robust evidence that the MCA was predominantly warm also outside the North Atlantic region. See Lüning et al. 2017, and 2019a&b for MCA temperature syntheses for Africa, South America and Oceania. Lüning et al. (2019a): The Medieval Climate Anomaly in South America. Quaternary International, 508: 70-87. doi: 10.1016/j.quaint.2018.10.041; Lüning et al. (2017): Warming and cooling: The Medieval Climate Anomaly in Africa and Arabia. Paleoclimatology 32 (11): 1219-1235, doi: 10.1002/2017PA003237; Lüning et al. (2019b): The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012. [Sebastian Luening, Portugal]	Rejected - While the MCA was likely warm from place to place, a new study by Neukum et al. (2019, https://doi.org/10.1038/s41586-019-1401-2), which is based on the most compressive and recent data compilation available reaffirms the AR5 finding that 20th century warming has been more uniform/homogenous than previous warming or cooling events.
9114	6	32	6	34	As in comment 1 above, there is strong evidence that the Medieval Warming Period was warmer than now. [Jim O'Brien, Ireland]	Reject - Agree that medieval warm period was warmer than now in some regions. However, this statement explicitly refers to global temperature.
46798	6	32	6	34	It is unclear from the report how this is calculated and how the uncertainty is taken into account. I think it can be said rather safely, on a global scale, that the temperatures of the last 30 years exceed those any 30-year long period in medieval times. However, on a 50-year time-scale – including the less “extreme” temperatures of the 1970s and 1980s – I fell very uncertain is available proxy evidence (considering their biases and uncertainties) actually support this with “High confidence”. With a 50-year period I would only give it “Median Confidence” compared to the warmest 50-year period in the 10th century. [Charpentier Ljungqvist Fredrik, Sweden]	Taken into account. Revised the surface temperature key findings

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57320	6	32	6	49	Can it be correct that the GMST change to 2009-2018 is 0.99C, while the GSAT change is 1.0C, but they are supposed to be 5% different? And don't we need to mention the impact of incomplete coverage here? [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Changes have been recalculated and checked in SOD. A cross-chapter box has also been prepared dealing with GMST-GSAT issues.
26128	6	33	6	33	The accepted term is the Medieval Warming Period – why change its name? This is not a sales/marketing document, is it? [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Noted - AR5 preferred "Medieval Warm Period"
48778	6	34	6	34	"common era" is used in the executive summary but not much in the text and only defined in the legend of Fig 2.10. Would be good to have it defined in the text. Moreover, modern era is also used (p13, 128) but not defined (since 1950s?). Also industrial era (since 1750?)/ Adding those definitions in Table 2.1 would help. [Sylvie JOUSSAUME, France]	Accepted - Define "Common Era" along with other terms when first presented (term no longer used in ES).
9364	6	34	6	34	It is suggested to explain, e.g. in a footnote, the term "Common Era". [Klaus Radunsky Radunsky, Austria]	Accepted - "Common Era" is defined in x-chapter box 2.1, table 1 along with other terms when first presented (term 'Common Era' no longer used in ES).
53990	6	34	6	34	Not sure that the "Common Era" will be recognisable to many readers unless clarified here [Timothy Carter, Finland]	Accepted - Define "Common Era" along with other terms when first presented (term no longer used in ES).
39480	6	36	6	36	The right term is "It is virtually certain" and it associated with a 99-100% of likelihood of the Outcome. Check it. [Carolina Vera, Argentina]	Rejected. The view of our chapter is that this outcome is sufficiently certain that confidence/likelihood language is not warranted.
42884	6	36	6	36	There is no certainty in science; change to "Global mean surface temperatures... (high confidence)." [Michael Evans, United States of America]	Rejected. The view of our chapter is that this outcome is sufficiently certain that confidence/likelihood language is not warranted.
55976	6	36	6	36	global mean surface temperatures -> GMST [Martin Ménégoz, France]	Accepted. GMST acronym now defined at start of this section.
23480	6	36	6	36	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11556	6	36	6	37	Authors write “It is certain that global mean surface temperatures have increased since the late 19th century. Each of the last four decades has been warmer than any decade that preceded it”. Add “of the last 800 years”. New: “...warmer than any decade of the past 800 years that preceded it”. We can still not be sure what temperature level the Medieval Climate Anomaly reached. There is growing evidence that the MCA was predominantly warm on all seven continents. See Lüning et al. 2017, 2019a&b for MCA temperature syntheses for Africa, South America and Oceania. Lüning et al. (2019a): The Medieval Climate Anomaly in South America. Quaternary International, 508: 70-87. doi: 10.1016/j.quaint.2018.10.041; Lüning et al. (2017): Warming and cooling: The Medieval Climate Anomaly in Africa and Arabia. Paleoceanography 32 (11): 1219-1235, doi: 10.1002/2017PA003237; Lüning et al. (2019b): The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012. [Sebastian Luening, Portugal]	Taken into account. Surface temperature key findings in SOD have been revised.
51602	6	36	6	37	Critical message for decision makers and could be highlighted/bold. [Lindsey Cook, Germany]	Noted. The SPM has been completely redrafted for consistency with the style guide prepared by the TSU.
27166	6	36	6	41	Since the acceleration of CO2 emissions starting in 1945, the increase of temperature has been only about 0.4°C up to the plateau before (and after) the El Niño peak of 2016. +0.4°C since 3/4 of a century should be emphasized to tone down the alarmism. [François GERVAIS, France]	Rejected. Temperature changes are presented for a range of periods. The claim made in this comment is not supported by the data, unless the warmest individual year of the 1940s is being compared with more recent decadal averages.
8532	6	36	6	41	This is presented in a manner that is hard for anybody not used to looking at these numbers and different ways of estimating changes to follow. Simplify for ES. Pick either a delta method or a linear trend; I would suggest the former, since the linear trend is harder to explain concisely. Order the different time periods consecutively. E.g., 1995-2014, then 2002-2018, then 2009-2018. [Robert Kopp, United States of America]	Taken into account. These are reported comprehensively in the supporting text (section 2.3), and the revised ES is distilled from this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35472	6	38			Are these estimates of GMST warming for observed locations only? Or are they estimates of the warming in the true spatially-complete global mean? These are different - e.g. Cowtan et al. (2015). The latter will tend to be larger, and have larger uncertainties. This is separate (and comparable in importance) to the issue of air temperature over ocean vs SSTs discussed in the following paragraph. As well as clarifying which these are, some text should be included in the ES on this issue. [Nathan Gillett, Canada]	Taken into account. The present numbers include data sets with a range of levels of global completeness (which is reflected in the relatively large uncertainty range). The FOD text was drafted in anticipation that updates in the underlying data products during 2019 would allow globally complete estimates for most or all data sets by the time of SOD. This is largely the case but we have included explanatory text in SOD where required.
53992	6	39	2	39	There are several period averages quoted here; the first was a 20-year mean; the second a decadal mean; but the third is a 17-year period, which seems pretty arbitrary. Why is this shown here? In all cases, couldn't the wording also describe (concisely) that the first is a 20-year and the second a decadal mean, just to clarify why it is shown. However, the significance of a 17-year mean is perplexing. [Timothy Carter, Finland]	Rejected. The periods chosen here for period averages were based on either periods equivalent to those reported in previous ARs (the 10-year period) or those which are required by other chapters for their own purposes (the 20-year period is used as the 'modern' period in modelling chapters, while the 17-year period matches the availability of specific data sets, e.g. those based on GPS radio occultation).
11558	6	40	6	41	Need to mention a warming rate, e.g. 0.1°C per decade [Sebastian Luening, Portugal]	Rejected. Changes are presented as the change from one period to the next for conciseness. Sufficient data are provided for users requiring a change rate per decade to derive one themselves.
53994	6	40	6	41	Couldn't there be qualifying language here to explain that the first trend is for the longest possible instrumental record and the second for the recent record of accelerated warming (this could even be emphasised, because presumably that's the reason for citing it!) [Timothy Carter, Finland]	Rejected. This section is by definition a brief summary of key points. More detail is presented in section 2.3.1.1.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57328	6	40	6	41	The linear trend was always a bit daft, when it is obviously not linear over this period. It was shoved in right at the end of the AR5 process when people realised there wasn't any indication of total warming relative to anything recognisable as pre-industrial. Surely we can now do better. Rather than a linear trend, why not use the non-linear trend based on a regression onto the expected responses to anthropogenic and natural forcing (Haustein et al, 2017 etc). People may complain "that is attribution, so it can't be in chapter 2", but fitting to a straight line is attribution also -- it's an attempt to explain observed changes in terms of a linear trend. And an unsuccessful one. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. A decision has been made to retain the OLS trend calculation method with AR(1) correction. While alternative methods clearly exist all have their own issues. Time series which are clearly non-linear can be handled through judicious selection of time periods, or using delta rather than trends, but the GMST time series is not sufficiently non-linear to require such treatment.
49926	6	41	6	41	"trends" is the wrong word because it would require units of degrees C year-1. Because the temperature change is given in units of C, better phrasing would be: "...with the 1980-2018 total change of 0.70..." [Owen Cooper, United States of America]	Rejected. The text explicitly states that the quoted values are 'estimated total change from a linear trend fit'.
47534	6	41	6	41	To me, a trend should be in units of degC/time [Matthew Toohey, Germany]	Rejected. Whilst it is certainly possible to express trends in this way, we have taken the view that most users will regard total changes as being of more value. Users requiring trends in units of deg C/time can easily derive these values from the quoted data.
37400	6	41			Is it right to refer to a trend of 0.70°C? Would it not be better to write "with a 0.70°C increase from 1980 to 2018" rather than "with 1980-2018 trends of 0.70°C"? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified.
35470	6	41			Quote trend uncertainties based on observational uncertainty only, not uncertainty ranges based on internal variability. To do this calculate the trend in available range of datasets, and use this to assess uncertainty, rather than using the uncertainty in the trend based on the residual variance (possibly corrected for autocorrelation). See my general comment. [Nathan Gillett, Canada]	Taken into account. The quoted uncertainties at present incorporate both the range of datasets and uncertainty within each dataset - our view is that this best reflects the full range of potential uncertainties (albeit imprecisely). This is explained further in SOD.
55978	6	43	6	43	Global Surface Air Temperature -> Global Surface Air Temperature (GSAT) [Martin Ménégoz, France]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39482	6	43	6	49	The paragraph lacks of use of the uncertainty language. Is the first sentence needed in the ES? If that so, consider reversing the sentence order through moving to the paragraph top the last two sentences including the most important key findings of the paragraph. [Carolina Vera, Argentina]	Rejected. The view of our chapter is that this outcome is sufficiently certain that confidence/likelihood language is not warranted.
53996	6	43	6	49	This adjustment factor is new to me. I looked further in the chapter for an explanatin and references, but found identical text.and no citations. I would have appreciated some explanation of this apparently significant adjustment, and there is none in this chapter. If this is described in previous reports, then please reference it. If not, then this absolutely should be explained and properly cited in this chapter. [Timothy Carter, Finland]	Taken into account. The citation for this factor is Richardson et al 2018. Regardless, the new box better highlights this.
17912	6	44	6	45	Are you mean the atmospheric or oceanic marine boundary layer? [Branko Grisogono, Croatia]	Taken into account. Surface temperature key findings in SOD have been revised.
37402	6	45			There are many comments to follow on GMST/GSAT differences. The adjustment of 1.05 (1.03-1.07) is based on modelling. Published estimates for reanalyses (not cited in the FOD), and as-yet-unpublished estmates for ERA5, suggest the adjustment might be a little lower. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. GMST/GSAT issues are dealt with through a dedicated cross-chapter box in SOD, for which use of reanalyses is part of the brief.
49928	6	47	6	47	The GSAT change for 1850-1900 to 2009-2018 is reported as 1 C. But this value requies two decimal places (i.e. 1.00 C) so that it is consistent with the uncertainty, which is reported to two decimal places. [Owen Cooper, United States of America]	Accepted. Text changed.
35474	6	47			The authors don't seem to have applied the correction factor correctly. The warming for 2009-2018 in GMST is 0.99. $0.99 \times 1.05 = 1.04$ C, rather than the 1 C quoted here. [Nathan Gillett, Canada]	Accepted. This is an error in the text (also at p40).
23482	6	51	6	51	Insert (DTR) to introduce acronym [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
13154	6	51	6	54	Does DTR refer to diurnal temperature range? Make sure this is clear. [Nora Richter, United States of America]	Editorial
55980	6	52	6	52	diurnal temperature range - diurnal temperature range (DTR) [Martin Ménégoz, France]	Editorial
53998	6	53	6	53	I'm not sure that "hence" is the right word here. It doesn't automatically follow from the previous statement about DTR that the following statement about max and min temperatures should also be true, especially a statement with uncertainty language attached. [Timothy Carter, Finland]	No longer present in ES

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35476	6	53			The word 'hence' should be deleted here, since it implies that changes in minimum and maximum temperatures are inferred from observed changes in mean temperature and DTR, rather than being directly observed. [Nathan Gillett, Canada]	Rejected. While it is certainly possible in principle to make a direct assessment of maximum and minimum temperature, to our knowledge, there is no global paper on the subject more recent than the Thorne et al 2016 DTR papers, and hence the conclusions on maximum and minimum temperature are indeed inferred from the mean temperature and DTR results.
39484	7	2	7	2	Apply uncertainty language to the key findings of the first two sentences. [Carolina Vera, Argentina]	Rejected. The view of our chapter is that this outcome is sufficiently certain that confidence/likelihood language is not warranted.
23484	7	2	7	2	Capital T for troposphere and tropospheric [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
35478	7	2	7	3	I suggest writing the rate of warming and its uncertainty (based on observational uncertainty only) separately for each period. Even if the rate of warming and its uncertainty happen to be exactly the same for the two period, it is not clear if the ranges have been merged in some way with the current phrasing. [Nathan Gillett, Canada]	Taken into account. Text reworded for SOD.
57816	7	2		8	Exactly that is high confidence, that the temperature in the tropical upper troposphere have changed over the last two decades on the troposphere which have warmed faster than those at the surface since 2001. (Temperature range; 2006,2008,2009,2011 and 2018). Technology techniques and integrated analysis in relation to rise in temperature have to be stated. [Abiodun Adegoke, Nigeria]	Rejected. Our assessment is that (under IPCC guidelines in the use of uncertainty language) only medium confidence is warranted at this time. (This is upgraded from low confidence in AR5.)
23486	7	4	7	4	Change troposphere to Troposphere [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23488	7	4	7	4	This is a poor expression, and also poor science (although becoming increasingly common in the media!). Temperatures cannot warm, they increase/decrease or it gets warmer/cooler. I suggest replacing 'warmed' with 'increased' (and consider quantifying) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
47536	7	5	7	5	Here and in other places, this seems to me to be not quite logically correct way to phrase confidence statements. We can assign confidence to a statement (e.g., X>Y with high confidence), but I think it makes no sense to say we have low confidence in quantity X (or even the magnitude of X), rather X is *uncertain*. [Matthew Toohey, Germany]	Rejected. The use of 'low confidence' in this context is standard IPCC usage.
23490	7	10	7	10	Capital S for stratosphere [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23492	7	10	7	10	Capital C for century (for consistency elsewhere in Chapter [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)])	Editorial
23494	7	12	7	12	Capital S for stratospheric [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23496	7	13	7	13	Capital S for stratospheric [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
42888	7	16	7	49	Can a statement be made about upper tropospheric specific humidity? [Michael Evans, United States of America]	Rejected. Upper-atmosphere specific humidity is covered through the assessment of total column water vapour.
47538	7	18	7	18	evaporation "losses" is nonintuitive to me, its an increase in evaporation which balances an increase in precipitation. [Matthew Toohey, Germany]	Accepted. The ES was modified substantially to address this and other comments and suggestions
39486	7	18	7	21	The paragraph requires further elaboration including more precision, better specification of the conclusions and the use of the uncertainty language. In which period have the trends been assessed? All datasets considered agree with the trend sign? [Carolina Vera, Argentina]	Accepted. The ES was modified substantially to address this and other comments and suggestions
35480	7	18	7	21	This paragraph is missing calibrated uncertainty language. The hydrological trends discussed here are not certain. [Nathan Gillett, Canada]	Accepted. The ES was modified substantially to address this and other comments and suggestions
48782	7	18	7	49	no mention. To paleoclimate changes? [Sylvie JOUSSAUME, France]	Accepted. The ES was modified substantially to address this and other comments and suggestions
57818	7	18		21	The mean daily temperature of any regions by any area have a big and significance impacts on the Global land precipitation caused by increase or rise in global temperatures. The future trend of Global temperature increase in relation to the global land precipitation with evaporation and global steam flow. [Abiodun Adegoke, Nigeria]	Noted. Assessment of regional temperature changes is the purview of chapters 10 and 12 and is not covered here. Similarly, future projections of global temperature can be found in chapter 4.
31104	7	19	7	19	The reason why P-E would not remain zero is not trivial so suggest saying why here. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The ES was modified substantially to address this and other comments and suggestions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35482	7	20	7	21	In what way is the land P-E trend of close to zero consistent with observed increases in specific humidity and total column water vapour? A global mean P-E trend of exactly zero would be consistent with constant global mean total column water vapour. But I don't understand what the relationship is between constant land P-E and a global increase in column water vapour. [Nathan Gillett, Canada]	Taken into account. A global P-E trend close to zero does not imply that precipitation and evaporation remained constant with time. In this sense, both precipitation and evaporation might increase with time giving a P-E~0. Increase in evaporation translates to an increase in TCWV. The section and finding have been clarified.
40916	7	21	7	21	What radiative forcing does the total column water vapour increase translate to? Also, this statement appears at odds with the following paragraphs. [Johannes Laube, Germany]	Rejected. Radiative forcing assessment can be found in section 2.2. Tropospheric water vapour has always been considered a feedback and not a forcing in prior reports and we wish to maintain this for continuity.
54000	7	23	7	23	It's this "Common Era" again - it needs to be defined up front in the ES [Timothy Carter, Finland]	Editorial
39488	7	23	7	26	Which is the "Common Era"? Reconstructions of what? How long is "prior to common era"? [Carolina Vera, Argentina]	Accepted. The ES was modified substantially to address this and other comments and suggestions
17914	7	28	7	30	Why the increase in near-sfc. spec. humidity, but a decrease in relative humidity? - Is it because the air temp. goes up at faster rate, or? [Branko Grisogono, Croatia]	Noted. However, assessment of attribution of changes in humidity is the purview of chapter 3. We don't want to speculate on links to temperature, although it may have a predominant role.
54002	7	29	7	30	What about changes in RH over the oceans? [Timothy Carter, Finland]	Accepted. The ES was modified substantially to address this and other comments and suggestions
39490	7	32	7	34	Specify "satellite era". Check the use of the uncertainty language. Consider rewriting it as "Trends in global total column water vapour are likely positive ..., therefore, there is medium confidence in the estimation of the magnitude". [Carolina Vera, Argentina]	Accepted. The ES was modified substantially to address this and other comments and suggestions
23498	7	38	7	38	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
39492	7	38	7	49	For providing a more coherent and consistent discussion, consider combining and/or integrating these last 3 paragraphs with the first one in lines 18-21. [Carolina Vera, Argentina]	Accepted. The ES was modified substantially to address this and other comments and suggestions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23500	7	39	7	39	Insert 'the' after 'since' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
40918	7	39	7	39	This should be "the 1980s". [Johannes Laube, Germany]	Editorial
40920	7	42	7	42	This should be "averaged". [Johannes Laube, Germany]	Editorial
40922	7	47	7	49	This sentence seems to be missing a word. [Johannes Laube, Germany]	Accepted. The ES was modified substantially to address this and other comments and suggestions
23502	8	3	8	3	Insert (GM) to introduce acronym [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
47540	8	3	8	6	Does this text fit better with the Hydrological cycle changes section on the previous page? [Matthew Toohey, Germany]	Accepted. The ES was modified substantially to address this and other comments and suggestions
40924	8	3	8	6	Shouldn't this be in the "Hydrological Changes" section, especially since it seems entirely focused on precipitation? [Johannes Laube, Germany]	Noted. We consider that the global monsoon is a key aspect of atmospheric circulation, as well as being analyzed in the context of precipitation variability.
47542	8	8	8	8	Are the "extratropical" and "mid-latitude" jets referred to here different things or the same? In my experience, "extratropical jet" is not very commonly used, more common is "subtropical jet" and either "midlatitude" or "polar". [Matthew Toohey, Germany]	Accepted. The 'extratropical jet' is used in the SOD throughout the text of CH2, which refers to the subtropical jet and the polar jet.
7794	8	8	8	10	Please reconsider if it is really the case that the mid-latitude jetstream meanders more intense. Due to accelerated warming in the Arctic, the North-South temperature gradient is reduced, which might lead to less intensification. [Merja Tölle, Germany]	Noted. The statement of meandering has been modified and more different references has been included and assessed in a balanced way.
35484	8	8			The terms 'extratropical jet' and 'mid-latitude jet' are both used in the same sentence, but I believe these are referring to the same thing. I suggest using one term or the other, but not both. [Nathan Gillett, Canada]	Accepted. Text has been clarified and in the SOD only 'extratropical jet' is used, which refers to the subtropical jet and the polar jet.
23504	8	14	8	14	Insert 'the' after 'over' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23506	8	18	8	18	Space between 'the' and '1980s' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35488	8	18	8	19	It may be the case that the observed vortex weakening trend is larger than observational uncertainties, but the variability in vortex strength is large (and not normally distributed). I suggest adding something concerning the large internal variability in stratospheric vortex strength. [Nathan Gillett, Canada]	Accepted. The large internal variability has been considered in the SOD.
23508	8	21	8	21	Capital S for stratospheric [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28886	8	21	8	32	This seems too strong a statement given the contents of 2.3.1.4.2 [Matt Tully, Australia]	Not applicable. BDC is no longer included in Chapter 2.
43328	8	22	8	22	The phrase "somewhat uncertain magnitude" is woolly. Try saying just "uncertain magnitude" [James Renwick, New Zealand]	Not applicable. BDC is no longer included in Chapter 2.
47544	8	22	8	22	Unclear to me if "low confidence" refers to the conclusion of a strengthening of the BDC, or to the uncertain magnitude. [Matthew Toohy, Germany]	Not applicable. BDC is no longer included in Chapter 2.
54004	8	25	8	25	QBO should be written in full at this first mention [Timothy Carter, Finland]	Not applicable. QBO no longer included in Chapter 2.
8036	8	25	8	26	This statement would be largely based on Kawatani and Hamilton (2013) who however report a decreasing trend of the amplitude of the QBO at a 95% confidence. This makes the trend a lot more probable than expressed by "likely". Would it be possible to strengthen this statement? I agree that the number of studies finding this is limited (I suppose because there is essentially only one type of data to go by, radiosonde observations), but the evidence from these observations is pretty clear. [Olaf Morgenstern, New Zealand]	Not applicable. QBO is no longer included in Chapter 2.
28888	8	25	8	26	I am surprised that the change to the QBO is mentioned in the executive summary. Compared to almost all other points mentioned it is esoteric and of "low confidence" so why include it? [Matt Tully, Australia]	Not applicable. QBO is no longer included in Chapter 2.
45410	8	28	9	42	Links to the appropriate sections in Chp 9 should be added to the executive summary here, as well as within the subsections of Chp 2. [Baylor Fox-Kemper, United States of America]	Noted. Collaboration improved.
29854	8	30	8	31	What is the rate of volume change? [Govindasamy Bala, India]	Rejected. Because of space and data limitations, we focus in this chapter on observational data for sea ice area and thickness.
9116	8	30	8	34	Arctic ice built up during the cooling period from 1945 to 1978, and began to recede coincidentally with the commencement of satellite records in 1979; its melting has substantially halted since 2007, as in Box 10.1, Figure 1, and according to latest data from the Danish Meteorological Institute (DMI), see https://notalotofpeopleknowthat.wordpress.com/2019/02/09/january-arctic-sea-update/ . Shipping records point to a similar melt during the warming of 1920-1940. Previous predictions of the Arctic's demise have proven wrong. [Jim O'Brien, Ireland]	Rejected. The statements in this summary focus on published literature and data detailed in section 2.3.2.1.1. We note that the wording was changed/updated since the FOD.
52316	8	31	8	31	no comma after "become" [Katherine Glover, United States of America]	Noted. The wording of this part has changed, and the actual part is not anymore included.
23510	8	31	8	31	Delete , after 'become' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in major ES restructuring

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46800	8	33	8	34	Actually, rather much data supports relative ice-free mid-Holocene summer sea-ice conditions in the Arctic. Given the strong orbital forcing of the time ice-free conditions are also to except. [Charpentier Ljungqvist Fredrik, Sweden]	Noted. The statement is changed and does not anymore include the part addressed.
17956	8	34	8	34	Would suggest rephrase "as likely as not as low today as in the mid-Holocene" [Gwenaelle GREMIION, Canada]	Noted. This part was reworted, and the commented wording is not anymore included like that.
9366	8	34	8	34	Either delete "last" or rewrite the sentence. [Klaus Radunsky Radunsky, Austria]	Editorial
6653	8	34	8	34	The sentence about the mid-holocene sea-ice area is obscure. Some words are missing or should be removed. As a result, it is not possible to assess the validity of the mid-holocene statement. [Thomas Lavergne, Norway]	Noted. This part was reworted, and the commented wording is not anymore included like that.
35490	8	36	8	37	I recommend reporting the trend in Antarctic sea ice extent to the end of the observational record, rather than selecting the end point to maximise the positive trend. The text could still describe the evolution of Antarctic SIE over time. [Nathan Gillett, Canada]	Noted. More and updated information is now given in the respective subsection, while the ES statement needs to be kept brief.
54006	8	37	8	39	Message garbled - grammar and wording needs attention here [Timothy Carter, Finland]	Noted. This part was reworted, and the commented wording is not anymore included like that.
43330	8	38	8	38	Missing text or incomplete sentence. [James Renwick, New Zealand]	Editorial
40926	8	38	8	38	Delete "remains low". [Johannes Laube, Germany]	Editorial
13872	8	39	8	39	Please clarify what you mean by "contradictory": contradictory datasets or opposing regional trends ? [Samuel Albani, Italy]	Editorial
46802	8	41	8	42	For a long-term perspective of the AMV/AMO it would be advisable to refer to Wang et al. (2017): Wang, J., Yang, B., Ljungqvist, F.C., Luterbacher, J., Osborn, T.J., Briffa, K.R., and Zorita, E. 2017: Internal and external forcing of multidecadal Atlantic climate variability over the past 1,200 years. Nature Geoscience, 10, 512–517. [Charpentier Ljungqvist Fredrik, Sweden]	Is this the right page, line number? Comment is not relevant to text cited.
13156	8	41	8	43	What about shifts in seasonality? This would be a good place to mention changes in seasons in relation to snowfall and the timing of "snow melt." [Nora Richter, United States of America]	Noted - ES is summary of main text which focusses on large scale changes in key indicators .
17958	8	45	8	46	It would be useful to comment on the time period over which we have evidence that glaciers are retreating. The other paragraphs in this section all refer to the time interval for the observations of change. [Gwenaelle GREMIION, Canada]	Accepted. Additional information is added now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15992	8	45	8	46	The inclusion of the statement "In most places, mountain glaciers are currently more extensive than during the early-middle Holocene" in the Executive Summary because it may convey the wrong message to the audience that the current warming level is not significant. In face, the accelerated shrinking and retreat of glaciers in recent decades are very significant: e.g. P.62, Lines 28-30, Lines 33-34, Lines 50-51; P.63, Lines 25-26. Suggest to highlight them in the Executive Summary as appropriate. [SAI MING LEE, China]	Noted. The wording on glaciers was changed, this part is not anymore included here.
15640	8	45	8	48	The comparison between Holocene and present glacier extents needs to come with a statement about glacier response times (Johannesson et al. 1989) to make clear that present day's glaciers are out of balance with respect to current climatic conditions and, hence, committed to further ice loss. Jóhannesson, T., Raymond, C. F., & Waddington, E. D. (1989). A simple method for determining the response time of glaciers. In Glacier fluctuations and climatic change (pp. 343-352). Springer, Dordrecht. [Michael Zemp, Switzerland]	Noted. This information is given in the respective subsection of the report, but not in the ES, due to space limitations.
13158	8	45	8	48	Mention that in many regions, glacial melting is accelerating. [Nora Richter, United States of America]	Noted. The wording on glaciers was changed, including that glaciers are retreating and continue to retreat. More details are given in the respective subsection.
35492	8	45			I suggest avoiding the phrasing 'are currently shrinking' because the meaning is amiguous. Right now some glaciers will be shrinking and some expanding depending on the season. Year to year there will be some variability. The observed trends are based on trends observed over a period of years. I suggest re-writing 'glaciers worldwide have shrunk over the past x decades' or similar. [Nathan Gillett, Canada]	Noted. The sentence was reworded, and the commented wording is not anymore included like that.
40600	8	46	8	46	I would write "still" more extensive to underline the on-going and continuing processes. [Olga Solomina, Russian Federation]	Noted. The wording on glaciers was changed, including that glaciers are retreating and continue to retreat. More details are given in the respective subsection.
29856	8	46	8	46	"more extensive"? Or "less extensive" ? [Govindasamy Bala, India]	Noted. The statement is changed and does not anymore include the part addressed.
52320	8	47	8	47	add comma - "Some Arctic mountain glaciers, however, are now smaller..." [Katherine Glover, United States of America]	Noted. This part was reworded, and the commented wording is not anymore included like that.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40602	8	47	8	47	Probably it makes sense to indicate more directly the Canadian Arctic and Svalbard where these evidences were found. [Olga Solomina, Russian Federation]	Rejected/noted. More information is given in subsection 2.3.2.3. Here in the summary, length limits are limiting the degree of detail.
52322	8	50	8	53	Greenland Ice Sheet or Greenland Ice sheet or Greenland ice sheet? Be consistent in capitalization throughout (I have always seen the first one) [Katherine Glover, United States of America]	Noted. Greenland ice sheet is written consistent throughout the chapter now (as "Greenland ice sheet").
15994	8	50	8	54	According to P.64, Lines 28-31, the ice mass loss of Greenland increased fourfold from the 20th century (~70 Gt/year) to early 21st century (~270 Gt/year) which is a very significant change and should be reflected in the Executive Summary. [SAI MING LEE, China]	Accepted. This information is now included in the ES.
9118	8	50	8	54	The Greenland ice sheet mass remains relatively constant, see https://notalotofpeopleknowthat.wordpress.com/2018/08/26/greenlands-ice-mass-balance/ . Greenland temperatures since 1851 are relatively constant, as proven by KNMI data, see https://www.the-cryosphere.net/12/39/2018/tc-12-39-2018-supplement.pdf ; it was also lower in the 1920-1940 warm period, as evidenced in Fig 2.26. In 1992 and recently, two WW2 planes were recovered from under 82m and 91m of snow/ice respectively, a dramatic indicator of the snow/ice build-up since around 1942. [Jim O'Brien, Ireland]	Rejected. The statements given are based on the most recent research about the Greenland ice sheet from peer-reviewed scientific publications.
23512	8	51	8	51	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
51604	8	52	8	53	Do you mean '1840' since records began, or in 1840 there was a similar historic melting? Please clarify as this again is an important (and comprehensible) message to policy makers on urgency. [Lindsey Cook, Germany]	Noted. The statement is changed and does not anymore include the commented part.
37404	8	52			Total mass loss from the Greenland Ice Sheet is stated here to be 627+-89Gt during summer 2012. In Chapter 9, line 2 of page 9-47, the 2012 mass loss is stated to be 444 +52Gt. These different numbers are probably reconcilable - summer 2012 vs 2012 as a whole.? Loss through glaciers included or not? It would help the reader if there was more uniformity of presentation however. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The statement is changed and does not anymore include the commented part.
8534	9	2	9	5	Cross check with chapter 9. There should be a higher level of confidence for WAIS than for EAIS. [Robert Kopp, United States of America]	Noted. The wording for Antarctic ice in this summary was updated since the FOD, and new literature is taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51606	9	7	9	8	Could you extend this sentence with the consequence of melting permafrost, to highlight for non-science policy makers the additional release of GHG previous trapped in ice, and the consequences of this. [Lindsey Cook, Germany]	Reject - The consequence with respect to GHG is beyond mandate/scope of chapter. Release of GHG covered in Ch 5. See also response to #40606
40606	9	7	9	8	If the permafrost started to thaw 250 years ago this processes is hardly connected to the anthropogenic global change and, hence, a brief explanation of the forcings of this processes should be provided. [Olga Solomina, Russian Federation]	Noted - No statements are being made here regarding attribution, which is beyond the scope of the section and the chapter as a whole. Attribution is discussed in Ch 9. However, the statement has been clarified.
54982	9	7	9	8	It will be beneficial for the statement to be strengthened with possible examples for "many places." The referred section {2.3.2.5} on pages 66-67 concludes with the same statement while providing additional detail on "many places." [Kilkis Siir, Turkey]	Noted - Revisions made to section text including summary statements.
32928	9	12	9	16	Need to check that numbers and confidence assessments are agreed upon with Ch9 (Kopp/Slangen), to ensure cross-report consistency [Aimee Slangen, Netherlands]	Noted. Collaboration improved.
23514	9	13	9	14	Insert space between number and unit (700 m, 2000 m and 6000 m) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
29850	9	14	9	16	The speed of change in the industrial era could be emphasized [Govindasamy Bala, India]	Accepted. Accounted for in edits
49930	9	19	9	19	Here salinity changes are described as "etremely likely" and that these changes are supported by "all available observational studies". In this context the phrase "The salinity pattern implying that..." seems far too weak. With this much evidence why say that the data only imply that fresh regions are becoming fresher and salty regions saltier? Wouldn't a better phrase by "the salintiy pattern demonstrates that..."? [Owen Cooper, United States of America]	Accepted. Text revised.
23516	9	19	9	19	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23518	9	23	9	23	Insert (GMSL) to introduce acronym [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8536	9	23	9	25	Cross check with chapter 9 (9.6.2.1.1), which concluded with medium confidence that the 20th century rise was faster than during any century since at least 1000 BCE, and that "global mean sea level very likely rose on average by 1.2 [0.9-1.7] mm yr ⁻¹ over 1901-1990 and 1.7 [1.3-1.9] mm yr ⁻¹ over 1901-2015 and 3.1 ± 0.3 mm yr ⁻¹ over 1993-2017 (high confidence; 9.6.2)." Time periods must be standardized between the chapters and assessments must converge. [Robert Kopp, United States of America]	Noted. Collaboration improved.
29852	9	23	9	25	Again, the rapidity of the change could be emphasized. [Govindasamy Bala, India]	Accepted. Rate of change explicitly noted.
27168	9	23	9	28	Fig. 3.14 of the AR5 report shows that the sea level rise seems to follow a 60-70 years cycle which is documented in a number of papers: Schlesinger and Ramankutty, 1994; Ogurtsov et al., 2002; Klyashtorin and Lyubushin, 2003; Loehle, 2004; Zhen-Shan and Xian, 2007; Carvalo et al., 2007; Swanson and Tsonis, 2009; Scafetta, 2009; Akasofu, 2010; D'Aleo and Easterbrook, 2010; Loehle and Scafetta, 2011; Humlum et al., 2011; Chambers et al., 2012; Lüdecke et al., 2013; Courtillot et al., 2013; Akasofu, 2013; Macias et al., 2014; Ogurtsov et al., 2015, Ollila 2017. The apparent acceleration of sea level rise might be related to the last ascending phase of the natural cycle. See Fig. 2a of http://dx.doi.org/10.1016/j.earscirev.2016.02.005 [François GERVAIS, France]	Rejected. Please see discussion in AR5, Chapter 3, Section 3.7.4 and in SROCC, Chapter 4, Section 4.2.2.1.1.
9120	9	23	9	28	Sea level rise has been fairly constant at about 2mm/year since 1880; in fact it is so linear that no anthropogenic effect is implied, as in Fig 2.32. The latest satellite data puts the figure at about 3.3mm/year, the difference between these and land-based measurements being unexplained. A paper by Dr Judith Curry similarly found no anthropogenic signal in sea level rise. [Jim O'Brien, Ireland]	Rejected. No reference for suggested paper provided. Statement inconsistent with SROCC, Chapter 4's executive summary: "Global mean sea level (GMSL) is rising (virtually certain) and accelerating (high confidence)."..."The dominant cause of global mean sea level rise since 1970 is anthropogenic forcing (high confidence) (4.2.2.1.6, 4.2.2.5)."
32930	9	23	9	28	Need to check that numbers and confidence assessments are agreed upon with Ch9 (Kopp/Slangen), to ensure cross-report consistency [Aimee Slangen, Netherlands]	Noted. Collaboration improved.
35494	9	23	9	28	I suggest writing 'the rate of GMSL rise has increased' rather than 'accelerating'. 'Accelerating' implies a positive second derivative of the rate of GMSL rise. Also on line 26 I suggest replacing 'acceleration' with 'increase'. [Nathan Gillett, Canada]	Noted.
23520	9	24	9	24	Capital C for century (for consistency elsewhere in Chapter) x 2 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32420	9	25	9	27	These values of accelerating sealevel rise of 3.7 – 4.4 mm per year for the period 2006-2018 are highly speculative and should not be mentioned here. It is likely that they are completely wrong and based on faulty methods. The use of these figures should be postponed until they have been properly verified by surface methods. [Martin Hovland, Norway]	Rejected. The reviewer provides no sound basis for their assertions here and they are inconsistent with the substantial body of literature assessed.
54008	9	25	9	28	Another case of multiple time periods being cited but with not apparent logic to their selection. Some qualifying language is needed to clarify why these periods (and not some others) are reported, or this might appear to be cherry picking to suit a narrative of accelerating sea level rise. [Timothy Carter, Finland]	Noted. Better consistency in SOD
17946	9	26	9	26	"...acceleration from -0.002 - 0.019 mm yr-2);..." -If negative, then this is deceleration, not acceleration. [Branko Grisogono, Croatia]	Editorial
35498	9	26			It isn't clear what the phrase 'with a likely acceleration from - 0.002-0.019 mm/yr' is referring to. What period is this over? By inference this might be for a period before 1901, but this should be made clear. [Nathan Gillett, Canada]	Accepted. Reference period clarified.
35500	9	27			The phrase 'as much as 50%' is ambiguous. I would interpret this as any amount up to 50%. Better to give a range - 'There is high confidence that the seasonal cycle of atmospheric CO2 has increased by x%-y%..'. [Nathan Gillett, Canada]	Taken into account - text revised.
51608	9	31	9	31	Would it be possible to remind us of what AMOC is, otherwise the section is meaningless. I could only find reference to this again at the end of the Chapter, so helpful to spell out on page 9? [Lindsey Cook, Germany]	Editorial
23522	9	31	9	31	Define AMOC [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
54010	9	31	9	31	AMOC needs full description at first mention. [Timothy Carter, Finland]	Editorial
35496	9	39	9	40	The meaning of the first sentence is unclear. I suggest something like 'While the atmospheric extratropical jets have likely shifted poleward since the 1980s, no consistent response of the wind-driven ocean circulation has been observed' or similar. [Nathan Gillett, Canada]	Taken into account. This paragraph has been revised considerably. Note that mechanisms are out of scope for chapter 2, and can be found in chapter 9
48784	9	44	10	16	biosphere changes are usually described in WG II. Make sure how these statements are consistent with WGII. [Sylvie JOUSSAUME, France]	Noted.
54012	9	49	9	49	Ocean basins have experienced declining pH since the pre-industrial era - tighten the grammar here. If this is because of stored CO2, then state the presumed causal relationship. [Timothy Carter, Finland]	Taken into account.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42018	9	49	9	52	Just an inquiry. As a reader I was wondering whether the statements regarding "highest on geological record" stated strongly correspond to the PETM mentioned in chapter 5 p 11, and chapter 5 p38 line30-53. [Knud Boesgaard Christensen, Denmark]	Taken into account in major revisions
13160	9	49	9	52	How much is pH changing? It might also be important to mention that the largest changes in pH are occurring in the Arctic and in coastal upwelling zones [Nora Richter, United States of America]	Taken into account, and is combined with other comments and text modified.
51610	9	49	9	52	It would help to connect the level of deoxygenation in relation to greatest marine extinction 252 million years ago, so readers understand the consequence of what deoxygenation (and declining pH) means to current life forms. [Lindsey Cook, Germany]	Taken into account. The paragraph went through major revision, combining amongst others this comment with other similar comments.
49932	9	49			This sentence is not well phrased. Would be better as: "All ocean basins are storing anthropogenic CO2, which has caused declining pH..." [Owen Cooper, United States of America]	Taken into account, and in addition, further modifications added to be in-line with SROCC assessment outcomes.
23524	9	50	9	51	Lower case o for Oceans and Ocean [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
15996	9	51	9	52	Suggest including a rough indication of the length of the geological record to aid appreciation by the reader. [SAI MING LEE, China]	Taken into account, and combined with other similar comments.
40928	9	52	9	52	The "available geological record" is a very vague term and should be quantified. [Johannes Laube, Germany]	Taken into account, and combined with other similar comments (e.g. ID 15996).
35502	9	52			Mention how long this geological record is here. This is an important qualifier especially given the 'virtually certain' likelihood attached to this assessment. [Nathan Gillett, Canada]	Taken into account, and in addition, further modifications added to be in-line with SROCC assessment outcomes, and to add time periods specifically.
31106	9	54	9	54	"lifecycle events" is jargon [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. According to this and other comments, this paragraph underwent major revision.
40930	9	54	9	54	This is also a very vague statement. It could be misinterpreted to refer to all lifecycle events of all marine species. [Johannes Laube, Germany]	Taken into account in major revisions
13162	9	54	10	1	What is meant by "advanced" in this sentence, please clarify. [Nora Richter, United States of America]	Taken into account. According to this and other comments, this paragraph has undergone major revision.
51612	9	54	10	1	What does it mean when timing of lifestyle events advance? Hard to understand consequence here. [Lindsey Cook, Germany]	Taken into account in major revisions

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54014	9	54	10	16	Is this straying into WG II territory? Furthermore, there is no commensurate information on the timing of phenology and other lifecycle events in land-based species (growing season is not quite the same - this lengthens while phenological events advance with warming). However, these should be covered extensively as observed impacts in WG II so probably needn't feature here. I do accept that these results affect the carbon cycle, which is treated in Ch 5. [Timothy Carter, Finland]	Taken into account. Statement has been revised substantially.
37406	9	54			This conclusion is a little unclear when read in isolation. I think it would be better if words such as "seasonally-dependent" were inserted before "lifecycle events". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in major revisions
7826	10	1	10	33	there are not logical connection between the description in line 1-26 to the conclusion in line 28-33 [zhiyan zuo, China]	Rejected. The comment could not be properly allocated, and the lines given here refer to two different topics.
29848	10	3	10	7	Yes, the speed of change is unprecedented. This point could be emphasized. [Govindasamy Bala, India]	Taken into account - text revised.
9368	10	6	10	6	It would be helpful to specify the time period "current Holocene interglacial" also in number of years or explain since which point in time (e.g. since the last 11.7 thousand years). [Klaus Radunsky Radunsky, Austria]	Taken into account - text revised.
23526	10	6	10	6	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
30466	10	9	10	10	You wrote that "terrestrial vegetation across the globe has increased", but which indicator of vegetation is referred to? Vegetation height? Density? Phenology? This should be made clearer [Pieter De Frenne, Belgium]	Taken into account - text revised (definition of terrestrial vegetation clarified).
29846	10	9	10	10	I believe that there is evidence for vegetation "productivity", not for vegetation stock. Please check [Govindasamy Bala, India]	Taken into account - combined with comment 30466.
9122	10	9	10	10	The increase in global vegetation is in fact the good story of increasing CO2 levels in terms of feeding the planet's future 10 billion people! [Jim O'Brien, Ireland]	Noted. No explicit changes requested and such text is not within chapter scope.
35504	10	9	10	10	Does this assessment of terrestrial vegetation having increased apply to greenness, biomass, or some other variable? [Nathan Gillett, Canada]	Taken into account - combined with comment 30466.
17918	10	9	10	10	Vague statement; please explain it. [Branko Grisogono, Croatia]	Taken into account - combined with comment 30466.
51614	10	20	10	23	Could you write this in comprehensible English? [Lindsey Cook, Germany]	Taken into account. The key finding has been revised based upon the broad range of comments received.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56210	10	22	10	23	This sentence is unclear, a bit strangely expressed. Could be interpreted by layperson as meaning that whole AR6 assessment could be questioned, which I assume is not meant here by the authors. Include confidence assessment (low, medium or high) and a better framing of context: How large is the discussed uncertainty? What is the exact impact on confidence? Are the implications only quantitative, or also qualitative? Could it affect the sign of specific assessments; if yes, which ones? Clearly, more details are provided below, but also the summary statement should be clear enough that it can be taken out of context. [Sonia Seneviratne, Switzerland]	Taken into account. The key finding has been revised based upon the broad range of comments received.
23528	10	38	10	38	Insert (AMO/V) to introduce acronym [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
35506	10	38	10	39	The formulation 'It is very likely that the AMO/AMV index has not exhibited any significant trend during the instrumental period (high confidence)' is overly complex and unclear. This is saying that P>90% that the AMO has not exhibited a trend which is inconsistent with internal variability at some non-specified probability level (assuming 'significant' means 'statistically significant with respect to internal variability' in this context), with high confidence. Better to write something like 'No consistent trend in AMO/AMV indices has been observed over the instrumental period (confidence qualifier).' [Nathan Gillett, Canada]	Taken into account in redrafting the ES
23530	10	43	10	43	Define PDO [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
15998	10	44	10	45	Re: "Since AR5 a shift from positive to negative phase of the PDO has occurred". The timing of phase change of PDO depends on the period (e.g. 11 years) and method (e.g. centre average, trailing average) of averaging of the PDO index. Using monthly PDO index data from JISAO (http://research.jisao.washington.edu/pdo/), the centre-average of 11 years data shows an increasing trend from negative values to about zero at 2013 (i.e. the year when AR5 WGI report was published). In addition, the statement seems inconsistent with a statement in Ch.3, P.72, Lines 13-14: " This was accompanied by a PDV shift toward its positive phase". Please consider revision and alignment of the statements in Ch.2 and Ch.3. [SAI MING LEE, China]	Noted. Cross-chapter consistency regarding the statements of the PDV have been checked.
23532	10	45	10	45	I suggest changing paleo to palaeo: the latter form is also used in the Chapter and paleo is the American spelling (I have the sense that the style for the document is British English as this is used more commonly) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29114	10	48	10	48	This sentence (taken from the end of 2.4.5.1) does not seem like a valid summary of the preceding paragraphs. It isn't clear what is meant by the "positive trend weakening since the 1980s" given that the (winter) NOA peaks in the 1990s and then decreases to 2010ish. A conclusion about variations clearly existing, but probably not falling outside historic range seem more valid [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The ES was modified substantially to address this and other comments and suggestions
23534	10	48	10	48	Define NAM/NAO/AO [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. A definition of the main modes of variability is included in the new Technical Annex.
35508	10	49			1980s should be 1990s here. There were positive NAO values observed in the 1990s. The upward trend has partially reversed since the 1990s. [Nathan Gillett, Canada]	Accepted. The ES was modified substantially to address this and other comments and suggestions
23536	10	52	10	52	Define SAM [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
43332	10	52	11	1	Should mention the role of ozone depletion here as this is a distinct anthropogenic influence separate from GHG increase. [James Renwick, New Zealand]	Noted. Assessment of attribution is the purview of chapter 3 and is not covered here.
23538	11	3	11	3	Define Atl3 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The Atlantic Zonal Mode was adopted in the SOD to be consistent with the meridional mode. Definitions of all the modes are included in SOD now into the Technical Annex on modes of variability.
40932	11	3	11	4	for the entire Executive Summary: The "instrumental period" is a very qualitative term as various atmospheric, cryospheric, and oceanic measurements started their respective periods at different times. [Johannes Laube, Germany]	Accepted/noted. In the ES of SOD the whole summary paragraph related to the modes of variability has been re-written. Later in the corresponding text instrumental period is defined in the context of modes of variability.
31108	11	3	11	5	That paragraph is very jargony. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
23540	11	4	11	4	Define AMM [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Definition of the AMM is now in the Technical Annex on modes of variability .
50170	12	0	12	0	table 2.1 3rd row: typo in Eocene [Sophie SZOPA, France]	Noted. Table is now replaced by a new cross-chapter box 2.1 donated in part from Chapter 1 box 1.3 in FOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51796	12	3	12	16	A large part of this paragraph is a repetition of what was said in Chapter 1. So I think this can cut down to make it more concise. [Anson Cheung, United States of America]	Rejected. This assumes that the reader is reading all chapters sequentially. This is unlikely to be the case so it is worth spending some words reiterating the context even if it somewhat repeats the earlier chapter. Some very minor shortening of the paragraph has been undertaken.
24502	12	3	12	16	This paragraph should also refer to using radiative forcing data from chapter 7 [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This has been covered in Section 2.2 where it is more appropriate to do so..
53292	12	5	12	5	I don't think this is needed here. [Jan Fuglestedt, Norway]	Rejected. No reason given for this suggestion and we believe that for a first time user of the report this is necessary context tying it back to the prior report which acts as the starting point for the present assessment. Also, this is important in view of the scoping of AR6, which is fundamentally different from that for the previous reports (AR4/5)
23542	12	8	12	8	Change 'a' to 'an' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
43334	12	18	12	18	Waffly start: "The chapter starts out in Section 2.2 by assessing..." Shorten to "Section 2.2 assesses..." [James Renwick, New Zealand]	Noted. This has been redrafted and is now accompanied by a graphical abstract in the SOD for inter-chapter consistency.
54626	12	25			It needs to say that chapter 8 will make a more thoroughly discussion in the hydrological cycle and chapter 9 on oceans [Ruth Cerezo, Mexico]	Rejected. Already covered by Chapter 1 and by an earlier paragraph in the same section plus in the openings to relevant subsections of section 2.3. There is no need to state this again here.
23544	12	26	12	26	Change 'paleoclimate' to 'palaeoclimate' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. All prior reports have used paleo

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39304	12	34			<p>Section 2.1: P2-12 please insert in line 34:</p> <p>For scales longer than weather scales (about 10 days), instrumental and paleo temperature analyses display temperature fluctuations that systematically decrease with scale as the averaging period increases: the fluctuations largely cancel each other out, the temperature appears to be stable. Without external forcings (e.g. in GCM control runs), this continues to arbitrarily long time scales and characterizes the approach of the model to its long term climate (Lovejoy, Schertzer et al. 2013), (Lovejoy 2019), (Lovejoy, Varotsos et al. 2018). However, due to external forcings and/or very slow internal processes, at some critical time scale τ_c, the variability stops decreasing and starts to increase. τ_c punctuates the end of the high frequency macroweather regime. At lower frequencies - in the climate regime - temperature fluctuations grow with increasing time scale so that the temperature appears to “wander”, to be unstable (Lovejoy and Schertzer 2013), (Lovejoy 2013). At τ_c, the internal variability becomes dominated by the responses to external forcings (and in the pre-industrial epoch, possibly to new slow internal processes). In the last decades, τ_c is about 16-18 years (Lovejoy 2014). At shorter time scales, the internal variability is the dominant source of variability, at longer scales, the forced response is dominant. Over the late Pleistocene, the average τ_c was \approx 300 years although it varies at different phases of the glacial-interglacial cycle, and may be as long as several millennia in the pre-industrial Holocene (corresponding to an exceptionally</p>	Rejected. Out of scope of section and much is out of chapter scope. Also request for gross self-citation is entirely inappropriate.
35510	12	35	12	37	I strongly suggest that quoted uncertainties in trends should reflect observational uncertainty only, not uncertainty in the underlying trend given some model of interval climate variability. Box Chapter 2.2 of the AR5 explains 'The 90% confidence interval is solely that arising from sampling uncertainty in estimating the trend'. See my general comment on the whole chapter on this topic. [Nathan Gillett, Canada]	See response to comment 35436.
23546	12	36	12	36	Capital B for box [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
57404	12	37	12	37	It is stated that statistical significance is assessed at the two-tailed 90% or 95% confidence level. However, in the remainder of the chapter the actually applied level is typically not specified. Thus, I propose to either mention the applied confidence level together with actual value or to define a default here and mention deviations later. [Marc Schröder, Germany]	Accepted. We have attempted to be more explicit in the SOD. Also, recently there was a consolidated decision on 90% confidence to be used across the report (at least for key-indicators and variables). That explicit information is given in the SOD and has been co-ordinated across the chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35512	12	37			The significance level associated with a significance test is the probability of a given result occurring under the null hypothesis - often 5%. The significance levels included here should be quoted as 5% or 10%. Moreover, the authors should pick one level and apply it consistently throughout the chapter (in many cases the significance level is not specified for the results of significance tests in the chapter, so it is impossible to tell which level is applied in the current draft). Finally, the authors should assess significance of trends against a null hypothesis that the trends are consistent with zero based on observational uncertainty. [Nathan Gillett, Canada]	Taken into account. See response to 57404
57402	12	45	13	2	Even though it is noted in the caption that not all periods are considered I suggest to also include the Common Era and the Industrial Era. [Marc Schröder, Germany]	Taken into account in drafting of new cross-chapter box 2.1 including incorporation of material from cross-chapter box 1.3 in the FOD.
11562	12	47	13	1	Why is the Holocene Thermal Maximum not discussed here? Most of the HTM pre-dates the selected 6kyr [Sebastian Luening, Portugal]	Taken into account in drafting of new cross-chapter box 2.1 including incorporation of material from cross-chapter box 1.3 in the FOD.
13164	12	47	13	1	Table 2.1, It might be useful to include temperature estimates for each period and how they compare to today. As in how much "warmer" or "colder" was it relative to present-day climate. This would be useful for emphasizing how "unusual" current climate changes are in the context of the past, and also why we study certain periods of Earth's history to understand how our planet will change in a warming climate. [Nora Richter, United States of America]	Rejected. We prefer to show this in Section 2.3.5
41406	12	47	13	1	Table 2.1 is almost the same as the Cross-Chapter Box 1.3 Table 1. May be both tables could be unified in only one and Table 2.1 removed. [Lucas Bianchi, Argentina]	Taken into account. The material has been moved from Chapter 1 to Chapter 2.
31982	12	47	13	1	Has it been checked that the same definition for the timeslice was used throughout the report? [Marie-France Loutre, Switzerland]	Taken into account. The chapter now hosts a box concentrating upon defining the paleo period slices.
53296	12	47	13	2	useful table. You could split "pre-industrial" and "early industrial". (Please coordinate with Ch1) [Jan Fuglestad, Norway]	Taken into account in drafting of new cross-chapter box 2.1 including incorporation of material from cross-chapter box 1.3 in the FOD.
52326	12	49	12	50	wordy - what about "available evidence drives which variables were considered" [Katherine Glover, United States of America]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14992	12	51	13	1	Technically this interval is the 'mid Piacenzian warm period' or the late Pliocene warm period given the recent shift of the Pliocene/Pleistocene boundary. Whilst for many authors calling MPWP is not an issue others have taken issue with not using "Piacenzian" to describe this narrow time interval within the Pliocene. [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A new cross-chapter box on this period - 2.4 - has been produced for the second order draft and permits a greater exploration of this period.
47546	12	51	13	1	"Timeslice" is inconsistent with the useage of this term in model simulations. "Period" might be much better. Also, the periods given in the table should have a "BP": 18 ka is strictly speaking a length of time, not a point in time. [Matthew Toohey, Germany]	Taken into account in drafting of new cross-chapter box 2.1 including incorporation of material from cross-chapter box 1.3 in the FOD (the term "timeslice" was replaced by a different term, pointing to the period considered).
23548	12	51	13	1	Insert space between numbers and units in the 'timeslice' column. If the table is to be split across pages please copy the column headings across to the top of the second page. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in drafting of new cross-chapter box 2.1 including incorporation of material from cross-chapter box 1.3 in the FOD.
57914	13	1	13	1	Replace Pliocene with Piacenzian in Table 1 [Bas de Boer, Netherlands]	Taken into account in drafting of new cross-chapter box 2.1 including incorporation of material from cross-chapter box 1.3 in the FOD.
42890	13	1	13	4	Table 1: subdivide the Present as is done in the Executive Summary; eliminate gap between 1900-1995, adding "20th century"? [Michael Evans, United States of America]	Taken into account in drafting of new cross-chapter box 2.1 including incorporation of material from what was cross-chapter box 1.3 in the FOD.
27930	13	5	13	5	Throughout this section the subsections start with refering to the results of AR5. However, for good comparison it would be usefull to end the section with a comparison of the results found by AR6 and AR5. Maybe even with the use of a table. Now sometimes the conclusions of AR6 are not even on the same aspects as in AR5. Starting then with the results of AR5 is not really contributing. Furthermore, it is useful to consistently add a summary at the end of every subsection [roderik van de wal, Netherlands]	Taken into account. Further efforts to ensure a consistent approach have been undertaken.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35514	13	5	16	9	For both the solar and volcanic forcing, changes in forcing datasets are reported as being 'recommended for CMIP6' as though this in itself is a mark of quality, and they are assessed somewhat uncritically (e.g. pg 14, ln 12-15). Of course the purpose of this chapter is to critically assess the observational datasets, and datasets used in CMIP6 should receive particular scrutiny of their uncertainties and any differences with other datasets, given their importance in underpinning other results in the report. [Nathan Gillett, Canada]	Taken into account. Less emphasis is placed on CMIP6/PMIP4 recommendations and more emphasis is placed on the underlying data sources. Nonetheless, advances since AR5 are encapsulated, to some degree, by CMIP6 forcings, which are also on display in the figures.
17960	13	5	31	3	Section 2.2 is a good overall assessment of the changes in drivers. It effectively highlights where significant advances have been made since AR5 and highlights areas that are still poorly constrained or understood. That being said more consistency could be used throughout the section in listing radiative forcing values, evaluating uncertainties etc. The figures need some changing as well- formats are currently very different that can distract from the information itself. [Gwenaelle GREMION, Canada]	Accepted. We have included in a more consistent manner the Effective Radiative Forcing values corresponding to the drivers in the relevant sections, with the actual values evaluated in Chapter 7. We have attempted to further harmonize figures across sections/sub-sections.
43336	13	5	31	3	The structure and content of Section 2.2 is very nice. [James Renwick, New Zealand]	Noted with thanks.
28780	13	5	31	3	Section 2.2 is a tour de force and nicely integrates with Chapter 7, where you do the trends and we do the forcing. We might want to have same structure to sub sections for SOD? Somewhere here you could point to our definition of ERF and more explicitly say that our chapter takes these trends and computes the ERF?. I like that this section doesn't have too many ERF numbers [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Partially accepted. Addressing several other reviewer comments, we decided to include some numbers on RF connected to the drivers (taken from Chapter 7) in the relevant sub-sections (which is in contrast to the reviewer's suggestion). As there were other constraints the sections have not been synchronised.
24504	13	5			The ordering of the climate drivers in section 2.2 should be reconsidered. Most readers will be interested in the anthropogenic forcing agents. I appreciate there is a logic to building things up from the paleo first (e.g. orbital), but most readers will be looking for more recent changes and expecting CO2 to be more prominent. Similarly within each subsection, while again there is a chronological logic to discuss deep time first, it makes it less easy to spot the recent historical (which most readers will be looking for). [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This was discussed at considerable length and the authors felt that the current ordering made narrative sense and avoided placing undue prominence to anthropogenic influences.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15112	13	9	13	18	An important feature of orbital forcing is how the Earth's elliptical orbit combined with the significant asymmetries between hemispheres owing to the significantly different ratios of land to ocean tends to modulate change. For example, considering how perihelion aligns with the seasons, the N hemisphere receives less than average energy in the summer and more than average energy in the winter. When this reverses in about 11K years, the N hemisphere will experience a larger difference between summer and winter while the S will experience a smaller difference. The average difference in W/m^2 between perihelion and aphelion is about $20 W/m^2$ and based on the presumed ECS, this should be far more evident in the temperature record than it is. In order for the climate to respond slow enough that the effects of this are not present, it would need to respond so slowly, we would hardly notice seasonal change, much less diurnal change. [George White, United States of America]	Noted. No specific revision suggested.
51798	13	9	13	18	I think it is important to point out eccentricity changes the overall total insolation whereas the other two shifts the distribution of insolation. [Anson Cheung, United States of America]	Rejected. Details about the effect of each of the orbital parameters does not contribute to the assessment in this CH2.
51800	13	9	13	18	Since we know those cycles pretty well, why not just explicitly mention the periods associated to each orbital forcing? [Anson Cheung, United States of America]	Reject - The periodicities, especially for precession, are complicated. Furthermore, no new information since AR5 has been published to assess this point.
42402	13	9	13	18	The flow of the paragraph can be improved. It starts by talking about how insolation is affected by earth's orbit, then it explains that we have precise calculations of insolation, and the third sentence talks about orbital forcing changes. The direction the paragraph is going in is not clear. The link between the second sentence about calculations and the following sentence about orbital forcing changes can be stronger. Perhaps the calculations sentence can be moved down towards the last sentence creating a link between measurements and our understanding about the lack of discernible effects on annual mean global radiative forcing. [Elizabeth Fard, United States of America]	Accepted. Switched the order of the sentences, as suggested.
50172	13	11	13	13	Wouldn't it be relevant to also cite the work of Laskar (Laskaret al. 2004 and Laskar et al. 2010)? [Sophie SZOPA, France]	Accepted - replaced reference.
57916	13	12	13	13	Please replace Berger&Loutre, 1991 with Lasker et al., 2004, doi: 10.1051/0004-6361:20041335 [Bas de Boer, Netherlands]	Accepted - replaced reference.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14994	13	12	13	13	I think that the Berger and Loutre (1991) paper may have been superceded by the orbital calculations of Laskar et al..2004 and Laskar et al. 2011: Laskar et al. 2011 La2010: A new orbital solution for the long-term motion of the Earth. Astron. Astrophys., Volume 532, A89; Lasker et al. 2004 A long term numerical solution for the insolation quantities of the Earth. Astron. Astrophys., 428, 261-285. [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - replaced reference.
31984	13	12	13	13	additional reference to Laskar's work, which extends the time span for reliable orbitla parameter would probably be welcome here. [Marie-France Loutre, Switzerland]	Accepted - replaced reference.
55506	13	13	13	14	Suggest following suit of "obliquity (tilt)" that appears in text with "eccentricity (elliptical/circular)" and "precession (axis wobble)" [Wesley Fraser, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - add parenthetical phrases, as suggested.
50174	13	15	13	18	Is the reference still AR4 there? [Sophie SZOPA, France]	Noted - Reference is correct.
30472	13	16	13	18	As you are assessing different epochs I think it is worth briefly mention when the orbital forcing have been thought to have a large impact on the mean global radiative forcing [Annalisa Cherchi, Italy]	Rejected. Remit of CH2 is to present observational evidence. Attribution of climate changes to specific forcings is outside the scope.
24506	13	16	13	18	If this mentions "small impact on seasonality" it needs to follow this up to say what the small impact is. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Added quantified effect of orbital changes.
24508	13	16	13	18	You can't "discern" an affect on annual mean global radiative forcing (at least not before the satellite era). I suggest "no discernible effect on climate". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Change, "no discernible effect" to "no appreciable effect".
46804	13	17	13	18	Yes, while this is true it cannot be ruled out that feedback from the small changes in boreal summer insolation may still have had an impact. [Charpentier Ljungqvist Fredrik, Sweden]	Noted - The statement in the report refers to radiative forcing. Feedbacks are not considered here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9124	13	21	15	5	Section 2.2.2 needs reviewing in the context of the important research of Henrik Svensmark and Nir Shaviv, as in comment 2 above. [Jim O'Brien, Ireland]	Noted - Unclear which comment is "comment 2 above". Nonetheless, reviewer is requesting additional consideration of the links between cosmic rays and climate. CH2 remit is observational evidence for changes in climate drivers. The process and feedbacks associated with the climate-altering effects of these drivers are considered in subsequent chapters. In response to reviewer's comments, evidence for recent changes in the solar magnetic field and in cosmic ray flux is pointed to chapter 7.
44844	13	21	16	9	Section 2.2.2 and 2.2.3 discuss about solar forcing and volcanic aerosol forcing and Figure 2.1 shows time series of these two forcings. When one looks at Figure 2.1, there seems a little confusion on the changes in both forcings. Since the changes in TSI, by definition, is 4 times larger than the changes in radiative forcing of volcanic ashes, it should be mentioned in the text or in the figure caption. [Won-Tae KWON, Republic of Korea]	Accepted - Added a note to figure caption to explain that TSI values refer to changes in solar radiation and do not account for the spherical Earth, which diminish the forcing by a factor of four.
11564	13	23	13	26	Palaeoclimate case studies have produced numerous examples of a strong solar imprint on the climate development, yet models still struggle to replicate the climate of the past. The only solar effect that is considered in the models is TSI whilst likely amplifier processes associated with the magnetic field (and possibly cosmic rays) as well as UV radiation and effects in the stratosphere are being neglected. You should be discussing these parameters, too. How did the solar magnetic field and cosmic rays change? TSI is not the only representation of solar activity and is very likely that amplifier processes are active that help to explain the empirically documented solar control observed in many pre-industrial case studies. [Sebastian Luening, Portugal]	Taken into account (Accepted part and reject part) - Statements about the observational evidence for changing UV have been added. The reviewer is also requesting an assessment of solar changes beyond TSI to include feedbacks that might account for inferences of solar effects on long-term climate variability. CH2 remit focuses on observations. Feedback processes are covered in other chapters. Also note that Solar forcing for CMIP6 (v3.2) paper in GMD by Matthes et al. (2017, https://doi.org/10.5194/gmd-10-2247-2017) addresses most of these issues and this formulation further is used in the modelling chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32638	13	23	13	32	I think it would be worth clarifying how to compare TSI to the forcings being talked about for GHGs. Am I correct that the TSI baseline is for the incoming radiance, and so to compare to the GHG forcing that is discussed requires dividing by 4 to account for the spherical Earth. It would also really help here to give the baseline amount around which the variations are occurring in order to make clear how small the solar variation is. [Michael MacCracken, United States of America]	Accepted and noted. Added statement regarding spherical Earth, as suggested. Regarding "baseline amount", stating values in W/m2 is an objective approach, similar to referring to degrees warming without referring to the very small change relative to the natural (baseline) greenhouse effect.
29894	13	24	13	31	It should be specified how the cited values of fluctuations were obtained/defined. The variability ranges depend on how they are defined. The range of daily fluctuations can be up to ~0.5% due to sunspots. 0.1% might refer, to e.g., a 3-month, 1-year, 11-year or several-decade smoothing, which are frequently used windows. The numbers will differ depending on the definition. [Natalie Krivova, Germany]	Accepted - clarified that this value represents for fluctuations averaged over multi-millennial time scales.
44842	13	26	13	26	Define TSI. [Won-Tae KWON, Republic of Korea]	Editorial
47548	13	26	13	26	TSI is not yet defined. [Matthew Toohey, Germany]	Editorial
40934	13	26	13	26	It would help readability to consistently spell out abbreviations such as TSI at their first mentioning in each chapter. [Johannes Laube, Germany]	Editorial
23550	13	26	13	26	Define TSI [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
41408	13	26	13	27	It mentions TSI acronym without defining it. In Page 30 Line 7, section 2.2.8 it says "Total solar irradiance (TSI)". This should be done in Page 13 Line 26 where TSI is mentioned for the first time in the text or in the Executive Summary (Page 5 Line 10). [Lucas Bianchi, Argentina]	Editorial
23552	13	27	13	27	Give dates in late 20th Century [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Text modified accordingly
17962	13	29	13	31	Does the range of values presented for the period 1750 - 2011 of 0.05 to 0.10 W m ⁻² require an absolute value as is presented for the period 1986- 2008 (-0.04 (-0.08 - 0.00))? [Gwenaelle GREMION, Canada]	Taken into account - The former cites a range of values, whereas the latter cites mean with ± 1 sigma range.
30474	13	30	13	31	these values in W/m2: what do they mean? And what impact do they have (i.e. in terms of temperature)? I think it is worth just to quickly repeat it [Annalisa Cherchi, Italy]	Reject - Explaining the meaning of units of measure and relating radiative forcing to temperature are outside the scope of CH2.
29896	13	30			Ch. 7, p.46, line 11 cites: 0.05 (0.00-0.10) Wm ⁻² [Natalie Krivova, Germany]	Noted - CH7 statement refers to a different time period than the one here ("... solar SARF from 1750 to 2011 to be 0.05 (0.00–0.10) W m ⁻² ").

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17964	13	31	13	32	It is not clear if this sentence is referring the likelihood of both of the values mentioned in the previous sentence (p29- p31) or just the latter value in the previous sentence? [Gwenaelle GREMION, Canada]	Accepted - revised sentence to clearly relate confidence levels with associated values.
42502	13	34	13	36	The section about solar activity reconstruction is biased and misses out relevant new developments. There was no real progress in the reconstruction of solar activity for more than 9000 years ago (i.e. beyond 1000 yrs BP). I urge the authors to compare the solar activity reconstruction by Steinhilber et al., (2012) with the “new” Wu et al. (2018) reconstruction. The curves are basically identical. There is no new data in Wu et al (2018) and no new approach is presented that actually resolves the existing differences between different radionuclide records (the differences have been normalized away). IPCC loses its credibility if such publications are used to show progress. References: Steinhilber, F., Abreu, J. A., Beer, J., Brunner, I., Christl, M., Fischer, H., Heikkilä, U., Kubik, P. W., Mann, M., McCracken, K. G., Miller, H., Miyahara, H., Oerter, H., and Wilhelms, F., 2012, 9,400 years of cosmic radiation and solar activity from ice cores and tree rings: Proc. Natl. Acad. Sci., v. 109, no. 16, p. 5967–5971, doi/5910.1073/pnas.1118965109, Wu, C. J., Usoskin, I. G., Krivova, N., Kovaltsov, G. A., Baroni, M., Bard, E., and Solanki, S. K., 2018, Solar activity over nine millennia: A consistent multi-proxy reconstruction ★: A&A, v. 615, p. A93 [Raimund Muscheler, Sweden]	Accepted - Included relevant new developments and clarified the "newness" of the 9000-year-long reconstruction and stated that it is similar to previous reconstructions. Importantly, the uncertainty has decreased significantly in the new reconstruction: (1) The reconstruction by Wu et al. is the only one which is not just a linear regression between the measured TSI variability and a solar activity proxy. Firstly, it has been shown that the relationship is actually not linear (Vieira et al. 2010). Secondly, if there is an uncorrected instrumental trend in the employed measured TSI (which is likely the case, as measurements by various instruments show somewhat different trends), this will be automatically taken over into the reconstruction. Both shortcomings are removed in the reconstruction by Wu et al., which significantly increases its

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42504	13	34	13	42	<p>There are important new developments since AR5 that need to be mentioned here. There were recent revisions in the sunspot numbers (Clette et al., 2014; Svalgaard and Schatten, 2016) that very much weaken earlier reconstructions that showed record high solar activity in recent decades (e.g. Solanki et al., 2004; Usoskin et al., 2003). The revised sunspot records also agree very well with earlier reconstructions of solar activity with radionuclide records (Muscheler et al., 2016; Muscheler et al., 2007). For the sun-climate debate this is an important development since these reconstructions of record high solar activity have been used to argue for a very strong solar influence on climate in the past century. References: Clette, F., Svalgaard, L., Vaquero, J., and Cliver, E., 2014, Revisiting the Sunspot Number: Space Science Reviews, v. 186, no. 1-4, p. 35-103., Muscheler, R., Adolphi, F., Herbst, K., and Nilsson, A., 2016, The revised sunspot record in comparison to cosmogenic radionuclide-based solar activity reconstruction: Solar Physics, v. 291: 3025. doi:10.1007/s11207-016-0969-z.</p> <p>Muscheler, R., Joos, F., Beer, J., Muller, S. A., Vonmoos, M., and Snowball, I., 2007, Solar activity during the last 1000 yr inferred from radionuclide records: Quat. Sci. Rev., v. 26, no. 1-2, p. 82-97, Solanki, S. K., Usoskin, I. G., Kromer, B., Schüssler, M., and Beer, J., 2004, Unusual activity of the Sun during recent decades compared to the previous 11,000 years: Nature, v. 431, no. doi:10.1038/nature02995, p. 1084-1087, Svalgaard, L., and Schatten, K. H., 2016, Reconstruction of the Sunspot Group Number: The Backbone Method: Solar Phys, p. 291: 3027-3042. doi:10.1007/s11207-016-0970-8</p>	Accepted - Included relevant new developments. Indeed, the presently known uncertainties of sunspot numbers are much greater than those given for the 'classical' GSN and WSN (they were heavily underestimated). At present, seven sunspot number reconstructions exist, being quite different in the 18th and 19th centuries. However, all of them still exhibit enhanced activity in the second half of the 20th century (see Fig. 8 in Chatzistergos, A&A, 602, A69, 2017). Moreover, as stated by Clette et al. (SSR, 2014) the Modern grand maximum is still evident, although the recalibrated series may indicate that a Grand Maximum needs to be redefined "as a tight repetition/clustering of strong cycles over several decades, without requiring exceptionally high amplitudes for those cycles compared to other periods." The comment, however, mixes the
55508	13	34	13	42	<p>New interesting work from Jardine et al. (2016) in Scientific Reports that present an alternative means of estimating/understanding TSI. [Wesley Fraser, United Kingdom (of Great Britain and Northern Ireland)]</p>	Noted - Jardine et al. (DOI: 10.1038/srep39269) report proof of concept for a new proxy for UV irradiance from Lake Bosumtwi, Ghana. The significance of this single study is difficult to assess in the absence of comparative analyses.
27678	13	35	13	35	check bibliographic citation [Poot Delgado Carlos Antonio, Mexico]	Accepted. Citation has been reviewed.
50176	13	36	13	36	Shouldn't it be " their improved models for production and deposition" [Sophie SZOPA, France]	Accepted - revised text as suggested.
35516	13	36	13	37	Indicate how long-term these changes are. [Nathan Gillett, Canada]	Accepted - Replace "long-term" with "millennial-scale".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42506	13	36	13	38	The sentence "Long term changes with inferred amplitudes up to 1.5 (1.4-2.1) Wm-2 have been identified in the 11-year averaged irradiance reconstructions from the cosmogenic time series." is very misleading since cosmogenic radionuclides provide information on solar shielding of cosmic rays. They do not contain direct information on solar irradiance changes [Raimund Muscheler, Sweden]	Noted. We refer the reader to chapter 7 for such an assessment.
30476	13	38	13	42	I see the need to mention how do these more recent findings combine with the choices done in terms of the forcing datasets chosen for CMIP6 [Annalisa Cherchi, Italy]	Reject - Assessing choices made for CMIP6 forcing is outside of CH2 scope.
42508	13	38	13	42	The longer-term solar cycles have been discussed since a long time ago (see e.g. Damon and Sonett, 1991) and one should give credit to the original authors. Reference: Damon, P. E., and Sonett, C. P., 1991, Solar and terrestrial components of the atmospheric C-14 variation spectrum, in Sonett, C. P., Giampapa, M. S., and Matthews, M. S., eds., The sun in time: Tucson, The University of Arizona, p. 360-388 [Raimund Muscheler, Sweden]	Reject - CH2 focuses on advances since AR5, while avoiding textbook-like coverage of climate science topics.
29898	13	42			I would suggest to add a note: A higher range of secular variability is considered unlikely but cannot be currently ruled out completely (Egorova et al. 2018, Karoff et al. 2018 [Karoff, C., Metcalfe, T. S., Santos, A. R. G., Montet, B. T., Isaacson, H., Witzke, V., et al. (2018). The Influence of Metallicity on Stellar Differential Rotation and Magnetic Activity. Astrophys. J. 852, 46. Available at: http://stacks.iop.org/0004-637X/852/i=1/a=46 .) Compre: Ch. 7, p.46, lines 21-35.: note, however, that the explanation given there for the higher variability range in Egorova is not correct. [Natalie Krivova, Germany]	Accepted - add note and citation as suggested.
50178	14	1	14	1	"is" should be "are" [Sophie SZOPA, France]	Editorial
35518	14	1			The focus is on observational datasets here, so describe the underlying observational dataset rather than referring to 'the CMIP6 historical forcing'. [Nathan Gillett, Canada]	Accepted - Include information about underlying observational datasets (assesses the current understanding of solar variability), but somewhere in AR6, the forcing that is actually used in CMIP6 needs to be discussed.
29524	14	2	14	3	Although not much difference can be seen in the TSI for the IPCC selected periods (MCA: 1360.91, LIA: 1360.63, Pre: 1360.69), there are much larger differences between the Maunder Minimum (1645-1715) and the Modern Maximum (1914-2007), i.e. 1360.4 vs. 1361.1 W/m2 or in comparison with the Medieval Maximum (950-1040): 1360.81 W/m2. Similar conclusions can be drawn for the UV and the VIS spectral regions. [Katja Matthes, Germany]	Accepted - added statement as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11566	14	2	14	4	You only present the TSI change between LIA and MCA. What would the change be in the solar magnetic field, cosmic rays and UV? Keep in mind that the minor change in TSI cannot explain the empirically documented solar control observed in many pre-industrial case studies. [Sebastian Luening, Portugal]	Taken into account - Regarding cosmic rays and solar magnetic field: Changes in TSI during the LIA and MCA are related to changes in cosmic ray flux, as modulated by the solar magnetic field, so the three are therefore not independent. Regarding UV: The contribution of the UV range (below 400 nm) to the TSI variability is about 50-60%. The change in the UV between MM and now strongly depends on the wavelengths. So, it would be roughly a factor of 10 of the TSI change in the near UV up to almost 1000 in Lyman-alpha (121 nm).
29526	14	3	14	5	I don't understand this sentence. I think it is confusing and needs rewriting to make the meaning clear. [Katja Matthes, Germany]	Taken into account - statement and its context has been rewritten.
29900	14	4			I suggest to specify "scaling to CMIP6" to avoid potential confusion [Natalie Krivova, Germany]	Editorial
27680	14	5	14	5	check bibliographic citation [Poot Delgado Carlos Antonio, Mexico]	Accepted. Citation has been reviewed.
30478	14	6	14	6	and what is the TSI range of the dataset udes for CMIP6? [Annalisa Cherchi, Italy]	Taken into account - in following paragraph.
50180	14	9	14	9	is it the TSI range as written or the amplitude of TSI change? [Sophie SZOPA, France]	Noted - The word 'change' is used to encompass all aspects of variability, including range, magnitude, and timing.
11568	14	9	14	15	You need to mention that the second half of the 20th century was one of the most active solar phases of the entire Holocene. See Steinhilber et al. 2012 (doi 10.1073/pnas.1118965109) and Solanki et al. 2004, https://www.nature.com/articles/nature02995 . In contrast to sun spots, the solar magnetic field reached its highest values in the late 20th Century. [Sebastian Luening, Portugal]	Noted - The conclusion about high solar activity by Steinhilber and Solanki are based on cosmogenic isotope data. Due to Suess effect. Isotopes should not be used in the 20th century.
29902	14	10	14	11	Sunspot [group] countings (which are also ground-based observations of solar activity) are actually available since the early 17th century, more regularones - since the second half of the 18th century. Late 19th century probably refers to more detailed sunspot observations (including area and positions) and full-disc photographs. [Natalie Krivova, Germany]	Taken into account - sentence Omittedted. The period between 1600 and 1900 is considered in the previous paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50182	14	12	14	12	"On the basis of the recommended CMIP6 forcing" should rather be "On the basis of these observations, a TSI timeserie reanalysis has been produced for CMIP6" [Sophie SZOPA, France]	Taken into account -- text revised.
35520	14	12	14	15	Don't just report the TSI changes in the dataset, but give confidence ranges and confidence qualifiers on assessed changes. [Nathan Gillett, Canada]	Accepted - Added uncertainty range and confidence qualifiers, as suggested.
30480	14	13	14	13	why do this reference (Matthes et al 2017) for the CMIP6 forcing is different from the one used above on line 2 (Jungclaus et al 2017)? [Annalisa Cherchi, Italy]	Taken into account -- text revised.
23554	14	26	14	26	Capital S for stratospheric [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
27252	14	37	14	48	I think it would be good to provide a quantitative estimate of the uncertainty in the TSI reduction back to Maunder Minimum in this paragraph, i.e. from about 1 W/m2 (the PMIP4 solar forcing) to about 4.5 W/m2 (Egorova et al., 2018). [Gabriel Chiodo, Switzerland]	Accepted - added statement to contrast small TSI change in Jungclaus et al. with larger TSI change in Egorova et al.
15172	14	38	14	42	While it's properly pointed out that there's significant uncertainty in the variability across all of the solar influences affecting the planet, none of this uncertainty is reflected by the implicit assumption that all (or most) change is due to industrialization, especially with regard to the recent Modern Grand Maximum (or more accurately, the Modern Grand Optimum). This illustrates another failure in this document which is to properly point out that neither warming or increased CO2 levels are particularly harmful to the biosphere and both paleo reconstructions and modern measurements tell us that biology always does better at warmer temperature and higher CO2 levels, moreover; higher temperatures mean more evaporation and rain which is also highly beneficial to biology. Ignoring exculpatory evidence is as bad for science as it is for justice. [George White, United States of America]	Noted - Attribution of the causes of climate changes are outside of the scope of CH2, as are the effects of those changes.
29528	14	45	14	47	I think this sentence is a bit confusing as it shortens the following points: 1) Recent satellite observations indicate that solar-cycle variability in the 200-400 nm range, which is important for ozone photochemistry and middle atmosphere heating, is larger than previously assumed (Yeo et al., 2015, 2017a; Coddington et al., 2016a). 2) the 16% increase in contribution to TSI variability in CMIP6 is relevant because its effect on the middle atmosphere can be transferred to the troposphere and the surface and hence need to be taken into account in climate model simulations in addition to the TSI. Therefore: It is necessary to highlight that UV changes need to be taken into account in models as well as TSI changes! There is enough evidence now, summarized in Matthes et al. (2017). [Katja Matthes, Germany]	Accepted part and reject part - Added suggested statement about recent satellite observations. However, factors that should be included in climate models is outside of the scope of CH2, as are the processes that involve the effects of the forcings on climate (i.e., photochemistry).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50184	14	45	14	48	It's difficult to see the causal link between the two part of the sentence. More generally is it difficult in this paragraph to distinguish what is relevant from a model need point of view (CMIP discussion oriented) from what is the signal of TSI variability in the observations [Sophie SZOPA, France]	Accepted - similar revisions suggested by other reviewers.
24510	14	45	14	48	While Matthes et al. 2017 does indeed say that the CMIP6 forcing is 16% stronger in the 200-400 nm band, I can't see where she says this increase is due to the effect on photochemistry. This increase will indeed affect photochemistry, but I don't think Matthes says it is caused by photochemistry. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - section rewritten for clarity, with the help of Matthes (a CA).
35522	14	45	14	48	The focus here should be on assessing observed changes, not describing the experimental design of CMIP6. Also the text here says that stronger UV TSI variability was included in CMIP6 than CMIP5 'because its effect on ozone photochemistry can be transferred to the troposphere and surface' - as written this seems to suggest that UV variability was artificially inflated to increase the solar signal in the troposphere and at the surface. Replace with an assessment of observations of UV variability, and how this assessment has changed since AR5. [Nathan Gillett, Canada]	Accepted - Statement now phrased in terms of increase in observed UV variability. The phrases regarding CMIP6 recommendations and climate effect of forcing were Omitted.
30482	14	47	14	47	TSI variability is quite generic. What timescale? And how is this number related to what shown in Fig 2.1b? [Annalisa Cherchi, Italy]	Accepted/taken into account - Timescale is now specified. Fig 2.1b caption now specifies that contribution of UV is included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24512	14	50	15	1	There should be a definite assessment statement here as to whether GCR are or are not important. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected- By "important", I assume that the reviewer is referring to their climate-altering affect. CH2 remit is to assess evidence in changes in forcing based on observations. Feedbacks and attribution is the focus of subsequent chapters. CH2 to refer to chapter 7 for discussion of GCRs, but limited treatment reflects the current prevailing understanding of role of GCRs. The cosmic-ray - cloud formation link via the ionization-induced/mediated nucleation of cloud condensation nuclei, initially proposed by Svensmark and co-workers, was studied theoretically, statistically and empirically. While former two methods did not provide any clear mechanism/relation for the proposed link, the result was clear in the experimental studies using dedicated experiments CLOUD (e.g., Kikby et al., Nature, 2011) and SKY (Enghoff et al., GRL, 2011). Both studied

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38386	14	50	15	2	<p>The potential amplification of solar forcing due to ionization changes caused by changes in the shielding of galactic cosmic rays (GCR) is described much less thoroughly in this report, compared to AR5 which had an entire subsection (7.4.6) dedicated to the topic along with subsection 8.4.1.5. Ionization caused by GCR is known to affect aerosol processes and in AR5 it was concluded that "Our understanding of the 'ion-aerosol clear air' mechanism as a whole relies on a few model investigations that simulate changes in cosmic ray flux over a solar cycle". However large improvements in that understanding have been achieved since then. [Martin Bødker Enghoff, Denmark]</p>	<p>Reject - part 1 of 3 part comment (38386, 38388 and 38390). CH2 remit is to assess evidence in changes in forcing based on observations. Feedbacks and attribution is the focus of subsequent chapters. CH2 to include a statement of the observational evidence for changing GCRs, but limited treatment reflects the current prevailing understanding of role of GCRs. The cosmic-ray - cloud formation link via the ionization-induced/mediated nucleation of cloud condensation nuclei, initially proposed by Svensmark and co-workers, was studied theoretically, statistically and empirically. While former two methods did not provide any clear mechanism/relation for the proposed link, the result was clear in the experimental studies using dedicated experiments CLOUD (e.g., Kikby et al., Nature, 2011) and SKY (Enghoff et al., GRL, 2011). Both studied</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38388	14	50	15	2	Continuing the above comment, I the following to be added either at the end of sect. 2.2.2 on solar forcing or as a separate subsection: "Regarding the proposed mechanism of solar modulated ionization affecting cloud cover significant advances have been made since AR5. Observationally the documentation for an effect of Forbush Decreases (short decreases in GCR due to solar coronal mass ejections) has been strengthened statistically in Svensmark 2016 where a Monte-Carlo bootstrap test is applied to 4 different datasets, revealing a >95% response in a large number of cloud parameters. Observational evidence of an amplified solar signal over the 11-year solar cycle is found in Howard 2014 building on the results of Shaviv 2008. There have also been a large number of detailed experimental investigations resulting in a multiparameter parametrization of the effect of ions on aerosol nucleation (Gordon 2017, Tomicic 2018). Experiments have also shown a strong effect on the growth of the aerosols, affecting CCN formation (Svensmark 2013). Finally the detailed physical mechanism behind the ion-induced growth to CCN has been described analytically and experimentally (Svensmark 2017), providing a consistant result between theory, experiments, and observations. Model results concluding that the effect is insignificant globally (Pierce 2009, Dunne 2016) do not include this growth mechanism which could change global CCN concentrations on the %-level over the 11-year cycle." [Martin Bødker Enghoff, Denmark]	Reject - part 2 of 3 part comment (38386, 38388 and 38390). See response in Part 1.
23556	14	55	14	55	Capital S for stratosphere and capital T for troposphere [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
30484	15	4	15	5	the sentence is too generic. Changes with respect to what and on what timescale? [Annalisa Cherchi, Italy]	Taken into account - The statement was revised
26902	15	4	15	5	The conclusion that TSI changes during the industrial are not unusual in the context of at least the past 9000 years is discussed earlier in 2.2.2 but now shown clearly for the 9000 years period. Figure 2.1 focuses over the last 2500 years and I think that the conclusion for the extended period of 9000 years period needs more justification. This is also a conclusion tha shows up at the executive summary. [Prodomos Zanis, Greece]	Accepted - Rather than TSI changes over the past 9000 years, the statement about the unusualness of recent changes is restricted to the past 1000 years, requiring less extrapolation beyond the observational period.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11570	15	4	15	5	Authors write: "To conclude, changes in TSI during the industrial era are not unusual in context of at least the past 9000 years (medium confidence)." What do authors want to say with this? This statement hides the fact that the second half of the 20th century was actually one of the most active phases of the entire Holocene. See Steinhilber et al. 2012 (doi 10.1073/pnas.1118965109) and Solanki et al. 2004, https://www.nature.com/articles/nature02995 . In contrast to sun spots, the solar magnetic field reached its highest values in the late 20th Century. Readers need to know this information to place the second half of the 20th century in a meaningful context. The cold Little Ice Age in turn was dominated by very low solar activity. Do not hide behind small TSI changes when it is clear that this does not explain the palaeoclimate record. [Sebastian Luening, Portugal]	Taken into account - Included statement about observational evidence for changes in solar activity. The conclusion about high solar activity by Steinhilber and Solanki are based on cosmogenic isotope data. Due to Suess effect, isotopes should not be used in the 20th century.
29530	15	4	15	5	Why is there a different time interval for solar and volcanic forcing even though Fig. 2.1 goes only back the past 2500 yrs? [Katja Matthes, Germany]	Noted - Differences in time intervals reflect the differences in the observational datasets.
41132	15	14	15	14	You must make clear here that the RF from volcanic eruptions is negative. Change to "with an amplitude greater than -1 W m ⁻² " [Alan Robock, United States of America]	Accepted - add negative sign to forcing values.
57820	15	14		23	An integrated table showing the past and Greenhouses gases concentration in the atmosphere should be drawn. Strongly agreed that; between the Eocene and Pliocene era, the concentrations of mixed gases of the greenhouse gas were unprecedented over the past 22,000 years. Therefore climate anomaly or graph should be plotted to quantified the variations and to show the past concentration pf greenhouses gases over the two era stated above. (Eocene and Pliocene). [Abiodun Adegoke, Nigeria]	Taken into account - these graphs and tables are included in CH2. Note that comment page number is incorrect.
47518	15	15	15	17	This conclusion from the AR%, by itself, undersells the importance of volcanic forcing. In the AR5, this comment was directly followed by "The natural forcing over the last 15 years has likely offset a substantial fraction (at least 30%) of the anthropogenic forcing." This aspect should be included here as well. [Matthew Toohey, Germany]	Taken into account - Agreed that this statement understates the importance of natural forcing. The statement was Omitted to avoid combining solar and volcanic in this section, and to leave the assessment of anthropogenic component to a subsequent section.
24514	15	16	15	16	Should reference Myhre et al. 2013 here. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - added reference, as suggested. In first sentence of this section.
23558	15	16	15	17	Don't split numbers and units across lines [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27682	15	16	15	17	edit paragraph [Poot Delgado Carlos Antonio, Mexico]	Unclear what comment refers to.
35524	15	17	15	17	Replace 'forcing' with 'volcanic forcing'. [Nathan Gillett, Canada]	Taken into account - sentence was deleted.
23560	15	22	15	22	Capital S for stratospheric [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
32186	15	26			"(...) the length of time between eruptions was large enough to (...)" instead of "(...) the length of time between eruptions large enough to (...)" [Isabel Trigo, Portugal]	Taken into account - The reviewer misunderstood the statement. The statement was revised for clarification.
41134	15	27	15	27	You must make clear here that the RF from volcanic eruptions is negative. Change to "with an amplitude greater than -1 W m-2" [Alan Robock, United States of America]	Accepted - add negative sign to forcing values, as suggested.
11572	15	28	15	31	Authors write "The reconstructed stratospheric aerosol optical depth averaged over the MCA (0.012) was lower than for the LIA (0.017) and similar to the PI period (0.011), although the estimated uncertainty is large: $\pm 30\%$ 1SD (Toohey and Sigl, 2017)." This is an oversimplification which is misleading. In reality, the second half of the MCA saw significant volcanic activity whilst about half of the LIA was characterized by low volcanic activity. One has to accept that changes in volcanic activity cannot explain the warm MCA and the cool LIA. [Sebastian Luening, Portugal]	Accepted - Clarified that volcanic activity varied within these periods.
23562	15	29	15	29	Capital S for stratospheric [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
47520	15	30	15	30	The 30% uncertainty quoted here is a systematic uncertainty in the absolute magnitude of the SAOD estimates, so it is not directly relevant to the confidence in the differences between those periods. Either this last part of the sentence should be removed, or it should be made clearer that the uncertainty is systematic in nature, or they should be replaced by estimates of the random uncertainty. [Matthew Toohey, Germany]	Accepted. Added statement that uncertainty is systematic.
47522	15	31	15	31	Please replace "loading of volcanic aerosols" with "stratospheric aerosol optical depth" as this is the quantity referred to by Toohey and Sigl (2017) [Matthew Toohey, Germany]	Accepted - wording revised as suggested.
50546	15	33	15	33	I suggest writing 'from Antarctic ice cores' [Frank Paul, Switzerland]	Reject - 'Glacier ice' is correct and consistent with 'tree rings' (rather than tree cores), 'marine sediment' (rather than marine sediment cores), etc.
30486	15	33	15	35	not clear how do this sentence is related to "dating and synchronization" discussed in this paragraph [Annalisa Cherchi, Italy]	Accepted - This development is now mentioned in the early part of the paragraph rather than at the end.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17966	15	33	15	35	The point that this last sentence does not seem to fit well as the last sentence. Although well related to the content of the paragraph could the reference be better used as an additional reference to Sigl et al., 2015 (P15, line 21). [Gwenaelle GREMION, Canada]	Accepted - This development is now moved to the early part of the paragraph rather than the end.
50186	15	37	15	52	Please add a reference to Figure 2.1 [Sophie SZOPA, France]	Taken into account - Fig. 2.1 is cited in this paragraph and in the previous one.
55982	15	41	15	41	of volcanic aerosol -> of volcanic aerosols [Martin Ménégoz, France]	Accepted - editorial correction made.
35528	15	45			I don't think any particular volcanic forcing dataset was recommended for CMIP5. The datasets cited here were chosen by the modelling groups themselves. [Nathan Gillett, Canada]	Accepted - Change "recommended for" to "used in".
35530	15	47	15	48	This text is describing the CMIP6 forcing dataset. Focus on assessing the underlying observations. [Nathan Gillett, Canada]	Taken into account - The statement about CMIP6 is retained because it satisfies the goal of featuring the major changes since AR5 (CMIP5). In addition, added a sentence to describe the underlying observations, as suggested by the reviewer.
42892	16	3	16	6	estimated systematic uncertainties are ~35% too large or too small? If so, is the bias corrected in the AR6 forcing sets? More generally, are the statistics of 2.2.3 Gaussian? If not, describe them nonparametrically in terms of percentile ranges (e.g. interquartile; 95%). [Michael Evans, United States of America]	Accepted - Added details about the distribution of error estimate. Consideration of details behind AR6 forcing sets is outside of the remit of CH2.
47528	16	8	16	8	Again, "loading" has not been defined, probably better to say "radiative forcing by volcanic aerosol" or "volcanic aerosol forcing" [Matthew Toohey, Germany]	Accepted - revise as suggested.
30488	16	8	16	9	same as for end of section 2.2.2 and meaning of "changes". Meaning of sentence should be that the recent (?) changes in the volcanic forcing are in the range of changes of the last 2500 years (in this case) [Annalisa Cherchi, Italy]	Accepted - clarified that both the magnitude and the variability are not unusual.
17968	16	8	16	9	This sentence is slightly unclear. "changes in volcanic forcing...are not unusual" does not appear to evaluate if the forcing itself is unusual, simply its changes. Perhaps this could be clarified. [Gwenaelle GREMION, Canada]	Accepted - clarified that both the magnitude and the variability are not unusual.
47524	16	8	16	9	This concluding statement, while appropriate, again downplays the importance of volcanic forcing. I would suggest adding something like "To conclude, volcanic eruptions cause short-term variations in radiative forcing and contribute to year-to-year climate variability. However, ..." [Matthew Toohey, Germany]	Accepted - added sentence as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47526	16	8	16	9	It is not clear if focus should be on changes within the industrial era, or differences between the industrial era and prior eras. Only the latter is really discussed in the above text. I would suggest something like "the average magnitude and variability of volcanic forcing during the industrial era are not unusual..." [Matthew Toohey, Germany]	Accepted - added phrase, as suggested.
17970	16	9	16	9	Upon reviewing the evidence presented here and in AR5 there appears to be no evidence in favour of the volcanic forcing being unusual, and considerable evidence for it being within normal bounds. While there are uncertainties remaining, perhaps this statement warrants a 'high confidence' qualifier. [Gwenaelle GREMION, Canada]	Rejected - Reviewer suggests that the confidence in reconstructions of volcanic forcing over millennia is "high" rather than "medium". However, the evidence is limited to sulfate from polar ice. Multiple lines of evidence are needed to increase the confidence.
27932	16	12	16	12	Maybe include figure with CO2 growth rate changes over time, to get good impression of nowadays extreme growth rates. [roderik van de wal, Netherlands]	Taken into account in new table 2.2
42894	16	12	18	10	section 2.2.4: would benefit from consistent summaries of means and rates of change over the smallest common resolved timescale (from ice core based to direct observations). This way, especially for rates, comparisons with past variations and statements in the executive summary could be most clearly made [Michael Evans, United States of America]	Noted/taken into account. Some high-resolution ice core records allows reliable calculation of centennial and decadal change rates at specific time intervals.
23564	16	14	16	14	change 'geologic' to 'geological' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
30490	16	14	16	14	"deep past" is not defined in Table 2.1. Probably better to have a definition for it [Annalisa Cherchi, Italy]	Editorial
50188	16	17	16	19	As Eocene/Pliocene and periods documented in ice are very different, the two ideas should be in separated sentences. [Sophie SZOPA, France]	Accepted - text revised - the time periods in separate sentences with different confidence levels (medium confidence for 500 Ma to 800 ka, and high confidence for the last 800 ka)
23566	16	20	16	20	Change 'gasses' to 'gases' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
50190	16	22	16	22	gasses => gases [Sophie SZOPA, France]	Editorial.
23568	16	22	16	22	Change 'gasses' to 'gases' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
8538	16	26	16	51	This meandering paragraph needs an assessment. [Robert Kopp, United States of America]	Taken into account - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51802	16	26	16	51	The time of focus jumps quite a bit around -- from past 50 million year, then to Triassic and then late Pliocene, E-O transition afterwards. I think it can be better organized. [Anson Cheung, United States of America]	Accepted - text revised
16000	16	26	16	51	Please consider including the results of this study: Mid-Pleistocene transition in glacial cycles explained by declining CO2 and regolith removal (https://advances.sciencemag.org/content/5/4/eaav7337) - More CO2 than ever before in 3 million years. [SAI MING LEE, China]	Rejected. The causes are not discussed in Chapter 2.
51684	16	26	16	51	§ 2.2.4.1.1. is quite confusing and lacks a logical progression. I would recommend the § to follow a chronological narrative [Samuel Jaccard, Switzerland]	Accepted - text revised
50192	16	27	16	31	are discussed the past 500 Ma, then the last 50 Ma and then the triassic.... Maybe consider to reorder the periods [Sophie SZOPA, France]	Accepted - text revised
40936	16	27	16	51	This section seems to be jumping between time periods and CO2 milestone mixing ratios somewhat erratically. Consistently ordering it by time could help here. [Johannes Laube, Germany]	Taken into account - text revised
23570	16	31	16	31	Insert , after Triassic [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
57918	16	34	16	34	late pliocene ends at about 2.6 so there's a discrepancy here between the time mentioned and the geological age. I would suggest to leave out 'late pliocene' from this sentence. [Bas de Boer, Netherlands]	Editorial.
14996	16	34	16	34	cite the Martinez-Boti et al. 2015 paper again here (cited in line 29) [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
23572	16	35	16	35	Define SSP [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
53298	16	35	16	35	I think you should other more clear words than "pessimistic". [Jan Fuglestedt, Norway]	Accepted - text revised - deleting "pessimistic scenarios"
17972	16	35	16	35	Do not use pessimistic here, it is subjective and not appropriate. 'high emissions scenarios' or name of scenarios themselves. [Gwenaelle GREMION, Canada]	Accepted - text revised
30492	16	35	16	51	I think it would be important to mention in the framing of this timing when the Earth started to be as we know it today (in terms of continental shape) [Annalisa Cherchi, Italy]	Taken into account in new cross-chapter box 2.1
35532	16	35			Avoid 'pessimistic' - the SSPs have different assumptions about development, technology etc. Replace with 'high emission'. [Nathan Gillett, Canada]	Accepted - text revised - "high emissions scenarios" instead of "pessimistic scenarios"
13874	16	41	16	42	It may be worth spending just a few words on our understanding of the possible causes of these changes in CO2 concentrations. Are the same mechanisms at play now or in the context of future climate projections ? [Samuel Albani, Italy]	Rejected. The causes are not discussed in Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17974	16	41	16	42	Highlight the uncertainties by giving the ranges rather than single values, instead of "~800 to ~2200", for example use e.g. "800±200 to 2200±400" and instead of "as little as 4000 years" use e.g. "4000-25,000 years". The estimated emission rates in the next sentence show how uncertain these values really are. [Gwenaëlle GREMION, Canada]	Taken into account - text revised and words are now better clarified.
42404	16	41	16	45	This section focuses on CO2 changes over the last 500 Ma. Here, the transition between the PETM and the MPWP is very quick. Perhaps a transitional sentence talking about CO2 change between the PETM and the MPWP would create clarity for this timeframe. If there was very little change, make that clear as well. The reader could feel there is a gap in time here. [Elizabeth Fard, United States of America]	Taken into account - text revised
49354	16	44	16	45	Suggest including here a few words about the consequences of that rate and magnitude of CO2 rise in the PETM, e.g. "around 7-35 times slower than current anthropogenic rates, yet fast enough to dramatically alter ocean pH with major consequences for marine ecosystems." [Yarrow Axford, United States of America]	Rejected - causes and results are not the main issue in chapter 2.
31992	16	45	16	45	This section does not provide estimate for CO2 concentration for MPWP (only 'followin the MPWP). However, chap9 indicates 'about 350-450 ppm; see 2.2.4'. Could this be made coherent, including references. [Marie-France Loutre, Switzerland]	Taken into account in new cross-chapter box 2.4
19180	16	49	16	51	I would suggest to rephrase this sentence, to make it sound less doubtful of the proxy records: "These results broadly agree with ice core records, however, detailed comparison awaits drilling of continuous ancient ice cores wit continuous stratigraphy." [Baerbel Hoenisch, United States of America]	Accepted - text revised
40938	17	2	17	2	A clear unit is missing in this caption. Is CO2 reported e.g. as a dry air mole fraction or a volumetric mixing ratio? A similar problem occurs in Figure 2.3 as "concentration" is ambiguous. [Johannes Laube, Germany]	Taken into account - It is a dry air mole fraction given that atmospheric pressure did not change for the last 500 Ma. However, we use "concentration" for readers who are familiar with "mole fraction" or "a volumetric mixing ratio."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19182	17	2	17	16	what is meant by "the basis" of a proxy? The Pagani et al. 2005 and 2011 data have been superceded by Zhang et al. 2013, 0-3.5 Ma should also include d11B data from Dyez et al. (2018), and the 0-450 ka panel d should include data from Hönisch & Hemming 2005. Also, I am not familiar with the paper of de la Vega et al. 2019 but the title promises Pliocene data, so that reference should probably be removed from panel d. In all panels the delta symbol needs to be inserted for d11B and d13C. [Baerbel Hoenisch, United States of America]	Taken into account - we reword to better cite the references
23574	17	3	17	3	Edit refrece to Foster et al. (2017) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
40940	17	11	17	12	I suggest moving this sentence to second position in this caption to increase its visibility, and also to ensure it includes references to all proxies. [Johannes Laube, Germany]	Editorial.
25552	17	15	17	15	Please check that the reference "Bereiter et al.,2015" is valid at this place to illustrate the interval 0 to 450 thousand years ago. I think that the good references for this interval are Petit, J. R., et al. (1999). "Climate and atmospheric history of the past 420,000 years from the Vostok ice core, Antarctica." Nature 399: 429-436 and Siegenthaler, U., et al. (2005). "Stable Carbon Cycle-Climate Relationship During the Late Pleistocene." Science 310: 1313-1317. The paper by Bereiter et al.,2015 deals with the 600-800 kyrBP interval. [Dominique Raynaud, France]	Taken into account - the original data for the last 400 ka are from Petit et al. (1999), but we used compiled data with updated chronology used in Bereiter et al. (2015).
27170	17	17	18	22	The value of 1.38E-05 W/m2/ppm for CO2 implies that the radiative forcing would be linear with concentration. This is misleading since actually it is generally recognized that the dependence on concentration is logarithmic. See for example Myrhe et al 1998. [François GERVAIS, France]	Rejected - the number does not exist in corresponding pages/lines.
35264	17	21	18	27	At least for me, it seems illogical not mention Ruddiman's discussion on prehistorical emissions of well-mixed gases. [eugenia gayo, Chile]	Rejected - The causes should be discussed in Chapter 5.
24516	17	21			In section 2.2.4.1.2 it might be useful to say whether paleo measurements of WMGHGs support or contradict Ruddiman 2003. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The causes of CO2 change should be discussed in Chapter 5.
32422	17	27	17	30	Gases trapped in buried ice, cannot be used as a proxy for the atmospheric concentration of CO2-variations from year to year. This has been shown in several studies. Therefore, the use of such information invalidates the conclusions given here. [Martin Hovland, Norway]	Rejected - Yes, the ice core records cannot tell about annual variations, but centennial variations are fine as long as the ice coring sites have high snow accumulation rates and sampling resolution for the analysis is sufficient.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51686	17	28	17	29	This statement is certainly correct. It may be worth adding than the most recent measurements, although more precise, do not change the overall picture with modern GHG concentrations higher than during any point of at least the last 800 kyrs [Samuel Jaccard, Switzerland]	Taken into account in edits
25546	17	28	17	38	Please add the following: Since AR5, estimates of the CO2 - Antarctic temperature phasing during the last deglaciation has been improved (Chowdhury Beeman et al., 2019). [Dominique Raynaud, France]	Rejected. The phase relationship between Antarctic temperature and CO2 concentration is out of scope of this chapter.
23576	17	30	17	30	No capital C for Centuries [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
25550	17	31	17	31	Please check that the reference "Schmitt et al ; 2012" has not been already used in AR5. [Dominique Raynaud, France]	Accepted - editorial -deleting "Schmitt et al., 2012"
50194	17	32	17	32	"than" is missing [Sophie SZOPA, France]	Editorial.
52328	17	32	17	32	seems like "than" is missing from the sentence [Katherine Glover, United States of America]	Editorial.
41410	17	32	17	33	Review the wording [Lucas Bianchi, Argentina]	Editorial.
49934	17	32			The word "than" is missing. Should be: "...an order of magnitude lower than increases observed in recent decades..." [Owen Cooper, United States of America]	Editorial.
17976	17	40	17	40	"the last 2000 years prior to 1850". Please clarify this statement, is this "the last 2000 years up to 1850" (i.e. CE 19 - CE 1850) or "the 2000 years prior to 1850" (i.e. BC 150 - CE 1850). The current formulation could mean either. [Gwenaelle GREMION, Canada]	Accepted - "the last 2000 years prior to 1850" changed to "0-1850 CE"
35534	17	44	17	45	What is the baseline relative to which these changes occurred? [Nathan Gillett, Canada]	Rejected - The causes should be discussed in Chapter 5.
40942	17	50	18	10	This paragraph has very little quantitative information on CH4 mixing ratios and its variability on longer timescales. [Johannes Laube, Germany]	Taken into account - CH4 change rate on timescales greater than multiple millennia can be considered.
23578	17	53	17	53	Capital E for earth's [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
23580	18	1	18	1	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
27684	18	2	18	2	check bibliographic citation [Poot Delgado Carlos Antonio, Mexico]	Editorial.
56074	18	9	18	10	These numbers are important (807+10) but there is no source/reference for these numbers. The way there are derived is not clear [Rolf Müller, Germany]	Accepted - references are added
23582	18	15	18	15	Insert 'that' after 'is' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
50196	18	15	18	17	shouldn't it be " is the last glacial termination during which N2O increased by..." [Sophie SZOPA, France]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50198	18	18	18	18	should be "a minimum of for 6-8 ka" [Sophie SZOPA, France]	Editorial.
17978	18	22	18	23	"at least three ice cores exhibit". It is not clear how significant this 'three' is. Listing the total number of ice cores, or better the number of ice cores that did not exhibit this short term minimum would greatly improve this sentence. [Gwenaelle GREMION, Canada]	Accepted - the list of the total number of ice cores is provided
18298	18	25	18	25	The statement "Multiple ice cores show N2O concentrations of 270.4 ± 3.4 ppb in 1750 and 271.2 ± 4.0 ppb in 1850. The industrial era values are higher than at anytime during the last 800ka" could be better placed in section 2.2.4.2. The values given in the first sentence are not higher than anytime during the last 800 ka according to Figure 2.4 a (e.g. values of ca. 300 ppb). Modern (from ca. 1980, Figure 2.4c) are though [Gwenaelle GREMION, Canada]	Taken into account - In summary of 2.3 section, we compare the glacial, pre-industrial and modern values.
56076	18	26	18	26	There is neither a reference for these numbers nor is it clear how the numbers are calculated here [Rolf Müller, Germany]	Accepted - references are added
17980	18	45	18	46	The word "slowly" here is rather vague. Either remove entirely or insert more detailed information about the rate of change. Another sentence could be used afterwards "the million year averaged rate of change did not exceed X ppm/Ma" [Gwenaelle GREMION, Canada]	Taken into account - deleting "slowly"
23584	18	47	18	47	delete , [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
52330	18	47	18	47	no comma after "While" [Katherine Glover, United States of America]	Editorial.
53300	18	48	18	49	I don't think you should talk about WMGHG as an aggregate here. Too imprecise. [Jan Fuglestedt, Norway]	Accepted. The definition of WMGHG is defined in the text.
50200	18	52	22	8	This part (and table 2.2) is difficult to read as the reference is not the preindustrial but the previous present period discussed in AR5 (actually 2011). What matters is how much did it change since the beginning of measurements or preind rather than since the last IPCC report [Sophie SZOPA, France]	Partly accepted. Both matter, as there is also a need to document how GHGs are recently changing. Table 2.2 and 2.3 now better handle this
23586	19	1	19	1	Insert ', respectively' after last 'ppb' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
50202	19	1	19	3	separate in 2 sentences [Sophie SZOPA, France]	Editorial
37408	19	7	19	8	The concept of "well-mixed" gases needs to be explained somewhere, even if it's relegated to the glossary. The sentence spanning these lines indicates that the gases are not well-mixed in some regions. The situation for the stratosphere merits a mention also. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Definition clarified, cf SOD page 14, lines 3 and 4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42896	19	15	20	21	To make the comparisons also across rows, would be good to make the concentration unit uniform in columns 1 and 2. (use scientific notation). Also, if the uncertainty in a quantity is 1.2, then the reported mean can't be reported to one or decimal place, can it? Round accordingly. [Michael Evans, United States of America]	Accepted. Table 2.2 is revised.
40944	19	17	20	21	I agree that all the main contributors to anthropogenic radiative forcing should be included here. However, for the smaller contributors such as halons, HFCs, PFCs, etc. there seems to be no obvious inclusion criterion. I strongly recommend making this more consistent, e.g. by following the approach of Rigby et al., 2014, who ordered by radiative forcing. This would result in the inclusion of other compounds such as C2F6, HFC-32, HFC-245a, c-C4F8, or HFC-227ea, all of which have comparable or even larger radiative impacts than e.g. the halons, CH3CCI3 or NF3 (see also the WMO SAOD 2018). This might make the table very long, so alternatively the less abundant gases could be bunched together as e.g. "minor" or "other" HFCs, PFCs, etc. with details shifted into an appendix as was the case for AR5. [Johannes Laube, Germany]	Accepted. We have modified this table, showing the top 20-25 gases in terms of ERF. One main point for this table is to show that observations from different groups give comparable results, and to show that the CMIP6 dataset generally agrees with observations in the yr 2011 (with some differences). The table is build on assessment in Chapter 7.
7288	19	17	20	22	Table 2.2 is much less useful than it could be. Instead of including the radiative effect per ppb, the authors should include the total direct radiative effects for the current concentrations. As it is, it is almost impossible to glean the relative importance of different species. To simplify the authors should remove the 2011 columns. The change column is sufficient. [Bryan Weare, United States of America]	Noted. Table 2.2 (now 2.3) is considerably modified, simplified and includes information on ERF (from Chapter 7), and information on pre-industrial. Year of 2011 is retained to provide information relevant to other chapters.
24518	19	17			Table 2.2 should use radiative efficiencies from Ch7, table 7.A.1 [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Also addressing various other review comments, table 2.2 has been updated to convey more relevant information, including ERF.
23588	19	23	19	23	Define 'Rad Eff' in table legend [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rad Efficiency is no longer shown in the table.
23590	19	23	20	1	If the table is to be carried over two pages please copy the column headings into the top of the columns on the second page [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted the final table will be copy-edited in the final stage of the report.
7828	20	49	20	49	wrong word LAnnually -- Annual [zhiyan zuo, China]	Noted. Copyedit to be completed prior to publication

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15114	21	3	23	31	While ozone is mentioned in a later section as declining, it's omitted here and in the grand scheme of things has a larger GHG influence than CH4. The distinction between well mixed GHG's and others is misleading and in the context of this report only serves to isolate water vapor and ozone as significant GHG's. CH4, N2O contribute less than 2% each to the combined GHG effect and along with the rest of the minor GHG's have a combined effect of less than 5%. Even more misleading is the omission of tropospheric water vapor as a contributing GHG which by itself accounts for about 2/3 of the total GHG effect, moreover; even incrementally, water vapor has a larger effect. Calling water vapor precipitable in the context of its GHG effect is a disingenuous distraction. The only precipitable water is the liquid and solid water in clouds. [George White, United States of America]	Noted. Section 2.2 is covering the drivers of climate change. For practical reasons (readability), it is organized into 'well-mixed' and 'short-lived components', without any presupposition on the magnitude of the Radiative Forcing. Overall ERF of the climate drivers is summarized in section 2.2.8. Tropospheric Water vapor response to climate change indeed has a larger contribution to the overall GHG effect, as evaluated in other chapters. Precipitable water in chapter 2 is considered as the depth of water in a column of the atmosphere if all the water in that column was precipitated as rain.
17982	21	5	21	6	This sentence is both unclear and possibly inaccurate. It is mostly the use of fossil fuels rather than their production that emits CO2. Additionally this sentence appears to claim that these causes are exclusively responsible for the changes, while the situation is considerably more complex. Update to add "among others" perhaps, or remove and simply refer to chapter 5 where this is discussed in more detail. [Gwenaelle GREMION, Canada]	Accepted. Changed to 'net balance between sources and sinks (Chapter 5)'.
37410	21	5			Is a word such as "primarily" needed before "reflecting", as there are parts played by changing sources other than fossil fuel and cement production? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. 'Primarily' added.
9276	21	7	21	7	The quoted annual mean CO2 increase seems 10 times too large [philippe waldteufel, France]	Rejected. The increase is not annual but an increase over 6 years.
16002	21	7	21	7	The CO2 concentration in 2017 is different from the value reported in the WMO Greenhouse Gas Bulletin (https://library.wmo.int/doc_num.php?explnum_id=5455). Please check. [SAI MING LEE, China]	Noted. The reference method used in this chapter is the NOAA marine boundary layer concentrations. WMO-GAW global mean mixing ratios are shown in Table 2.3, and agree with values shown in the WMO Greenhouse Gas Bulletin, 2018
24520	21	14			The colour legends need to be on the figure rather than in the caption. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
27686	21	16	21	26	subscript [Poot Delgado Carlos Antonio, Mexico]	Accepted. Corrected

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24522	21	25	21	26	But growth rate date are shown prior to 1995 for CO2 and CH4? [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The word N2O is added.
40946	21	25	21	26	Please add "of N2O" after "growth rates". [Johannes Laube, Germany]	Accepted.
16004	21	32	21	32	The CH4 concentration in 2017 is different from the value reported in the WMO Greenhouse Gas Bulletin (https://library.wmo.int/doc_num.php?explnum_id=5455). Please check. [SAI MING LEE, China]	Noted. The reference method used in this chapter is the NOAA marine boundary layer concentrations. For comparison Table 2.3 also includes WMO-GAW averages.
45632	21	32	21	33	Better to use the 2018 number: 1857.7 plus/minus 0.7ppb and to state this is for the remote marine surface. Also change the increase number [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The SOD the number is updated to 2018.
54984	21	33	21	33	Some of the measured data sources are not provided with additional referencing due to the identification of the data source in the statement. It may be checked if this does or does not require additional referencing. [Kilkis Siir, Turkey]	Rejected. It was not clear how this comment is related to the specific line numbers
45634	21	33			Perhaps cite recent update on the state of methane: Nisbet, E. G., M. R. Manning, E. J. Dlugokencky, R. E. Fisher, D. Lowry, S. E. Michel, C. Lund Myhre et al. "Very strong atmospheric methane growth in the 4 years 2014–2017: Implications for the Paris Agreement." <i>Global Biogeochemical Cycles</i> 33, no. 3 (2019): 318-342 [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. While, this is a valuable reference, it is not appropriate in the context of the discussion in this section.
50204	21	37	21	39	would need to explain that the sources and sinks are inferred from models (in particular OH) and not from observations. [Sophie SZOPA, France]	Accepted. See comment 35536
35536	21	37	21	48	For both methane and nitrous oxide the text describes the main sink, but does not describe the sources. I suggest either describing both sources and sinks or neither. [Nathan Gillett, Canada]	Accepted. We do not describe the sinks.
24524	21	38	21	38	Remove "by mainly the OH radical". It currently reads as if OH is the main source of variation. Causes of variation are discussed in Ch 5 and don't need to be here. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Deleted.
37412	21	38	21	39	There is an extensive discussion of methane in Chapter 6 also. If my suggestion (comment 2) to drop this is not taken up, then cross-reference to chapter 6 as well as chapter 5 will be needed. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There is already a very limited discussion on methane (9 lines), in support of Figure 2.4. The section refers for further information to cross chapter box 5.2 in Chapter 5.
23592	21	39	21	39	Delete brackets [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16006	21	44	21	44	The N2O concentration in 2017 is different from the value reported in the WMO Greenhouse Gas Bulletin (https://library.wmo.int/doc_num.php?explnum_id=5455). Please check. [SAI MING LEE, China]	Noted. The reference method used in this chapter is the NOAA marine boundary layer concentrations. WMO-GAW global mean mixing ratios are shown in Table 2.3, and agree with values shown in the WMO Greenhouse Gas Bulletin, 2018
24526	21	46	21	46	"increasing"->"accelerating". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The information in Figure 2.4 does not justify an increase of rate (or acceleration). Indeed, the word 'increasing' has been deleted.
24528	21	46	21	48	Suggest deleting sentence "The Pre-Industrial ..." as discussion of sources and chemical reactions is covered in Ch 5. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
27688	21	47	21	47	double parenthesis [Poot Delgado Carlos Antonio, Mexico]	Accepted. Corrected
24530	21	48	21	50	I don't think you can calculate an imbalance purely from the global annual average concentration and the lifetime. I suggest leaving budget calculations to Ch 5 as these are not discussed for other species in 2.2.4.2. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Budget calculations are presented in Chapter 5
56078	21	49	21	50	the budget is out of balance: unclear if this is shown in SPARC 2013; if not give a reference [Rolf Müller, Germany]	Accepted. The SPARC reference refers to the global lifetime, budget discussion is moved to other chapters.
35538	21	52	21	54	The text here is discussing global mean N2O changes. Here the text states that excellent precision and network consistency are extremely important 'because background spatial gradients are small'. For the global mean, wouldn't small spatial gradients make precision and network consistency less important? You could get a more representative value by averaging over fewer stations if gradients were small than if they were large. [Nathan Gillett, Canada]	Accepted. The reviewer is right, the interest of making accurate N2O is beyond making global averages. Due to page constraints discussion on this had to be removed.
24532	21	52	22	3	There is more detail on N2O measurements than for CO2 and methane. Is this a specific issue for N2O? If not, it should be covered for all species or not at all. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Assessing N2O gradients is in particular difficult. Due to page constraints this has been deleted.
50206	21	54	21	54	precise for what precision and consistency are important [Sophie SZOPA, France]	Rejected. To address other review comments we reduced this section, but more discussion can be found in the gaps in knowledge section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35540	22	2	22	3	The topic of determining emissions of N2O from concentration measurements by inverse approaches is out of scope of this chapter. [Nathan Gillett, Canada]	Accepted, we refer to inverse modeling discussed in Chapter 5.
50208	22	3	22	3	is it referring to inverse modelling? If yes, precise it [Sophie SZOPA, France]	Rejected. Due to page constraints there is no further mentioning of inverse modelling
37414	22	5	22	8	See comment 68. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is not possible to understand comment '68' reference and is thus unactionable.
17984	22	5	22	8	Perhaps a sentence about how the rate of change has increased would be useful here as well. [Gwenaelle GREMION, Canada]	Rejected. We have revised the sentence, but adding this information would distract from other key-point made in this sentence.
16008	22	6	22	7	The figures of increases and abundances of CO2, CH4 and N2O are different from the values reported in the WMO Greenhouse Gas Bulletin (https://library.wmo.int/doc_num.php?explnum_id=5455). Please check. [SAI MING LEE, China]	Noted. The reference method used in this chapter is the NOAA marine boundary layer concentrations. WMO-GAW global mean mixing ratios are shown in Table 2.3, and agree with values shown in the WMO Greenhouse Gas Bulletin, 2018.
40948	22	7	22	7	"Now" is a vague term. I recommend replacing it with "at the end of 2017" [Johannes Laube, Germany]	Accepted. Dates are included.
49936	22	7			In addition to the absolute increases in these trace gases, it would also be helpful to report the percent increases since pre-industrial times. [Owen Cooper, United States of America]	Rejected. Adding relative changes would make the analysis unnecessarily lengthy.
27934	22	11	22	11	Make clear distinction between gasses important for ozone depletion and gasses important for direct RF. [roderik van de wal, Netherlands]	Noted. All gases discussed in this section have a strong ODP as well as radiative effect.
40950	22	16	22	17	I suggest rephrasing this sentence to "Atmospheric abundances of (almost all) HFCs (replacements for HCFCs), PFCs, and other radiatively important gases were also increasing." to make it more consistent with the heading of this section. [Johannes Laube, Germany]	Accepted. Sentence rephrased
24534	22	20	22	22	The sentence "All man-made greenhouse gases..." isn't explained very well. Note that a few ODSs have natural sources as well as industrial ones and hence non-zero pre-I concentrations. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text includes an improved description and updated references.
23594	22	21	22	21	Change to Industrial Revolution for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37416	22	22			The wording here could be a little softer. The distribution of some of the "man-made" gases is to some extent determined by natural processes, as they are transported through the atmosphere to places where, in the case of CFCs at least, heterogeneous chemical reactions can take place. So if atmospheric temperatures and circulations change, the distributions of some of these gases will change. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence is adapted.
54628	22	25			why in this section there are not references to the several papers regarding the recent peak observed on CFC from China (there are several papers from S. Park regarding this) [Ruth Cerezo, Mexico]	Accepted. These publications were not available for FOD, but included in SOD
17986	22	26	22	28	It would be useful to know the percentage decrease over similar timeframes prior to 2011 to give these values some context. [Gwenaelle GREMION, Canada]	Rejected. While we agree that this could be relevant information, page limits do not permit to give this additional information
57822	22	26		39	Atmospheric abundance of chloroflourocarbons CFCs, has drastically reduced by the change made in-line with Montreal Protocol on industries producing the refrigerant R22 gas changing to R410 gas. A close watch and monitoring should be stated in the Montreal protocol guidelines on the declines of CFCs. Decrease in concentration of CFCs in the atmosphere should be plotted on a graphs or table showing the reduction and inventory by manufacturing industries producing refrigerant for cooling Units. A statement approach, response stated and technology techniques to combat or mitigate CFCs should be stated and implemented in the reports [Abiodun Adegoke, Nigeria]	Rejected. While the suggested analysis is relevant, it is beyond the scope of this chapter to provide this analysis.
35542	22	27			Insert 'and its ammendments' after 'Montreal Protocol'. The ammendments have been more important than the protocol itself in driving these declines. [Nathan Gillett, Canada]	Accepted.
50210	22	28	22	28	decrease "by" is missing [Sophie SZOPA, France]	Editorial
40952	22	29	22	29	I agree that it is important to mention the CFCs that are not decreasing, but this either needs a reference or to be made consistent with Table 2.2. [Johannes Laube, Germany]	Accepted. It was included to qualify earlier statement that abundances of "most" CFCs are decreasing. The sentence on minor CFCs is removed.
24536	22	30	22	31	This should cite Ch 7 where these ERFs are calculated. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. All ERFs will be taken from Chapter 7 henceforth.
40954	22	30	22	31	No radiative forcing was mentioned for the three main WMGHGs, so this is inconsistent. [Johannes Laube, Germany]	Accepted. ERF numbers are now consistently added in sections.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56080	22	33	22	33	do you mean „atmospheric“ abundance? [Rolf Müller, Germany]	Accepted. Added 'atmospheric'.
8038	22	33	22	36	A new Nature study unequivocally finds CFC-11 emissions in China. This could be cited here. https://www.nature.com/articles/s41586-019-1193-4 [Olaf Morgenstern, New Zealand]	Accepted. A reference to this observationally based study on unreported emissions of CFC-11 is made in Chapter 2 and 6.
24538	22	33	22	36	There have been additional papers published in the last year on the CFC-11 growth rate. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We include relevant new literature on CFC-11
56082	22	34	22	34	suggest to change to: >a slowing of the decline rate of CFC-11< [Rolf Müller, Germany]	Accepted. Includes new information from recent publications on CFC-11.
26904	22	36	22	36	Another recent article on the issue of CFC-11 increase could be also cited (Rigby et al., Increase in CFC-11 emissions from eastern China based on atmospheric observations, Nature 569, 546–550, 2019, https://doi.org/10.1038/s41586-019-1193-4) [Prodomos Zanis, Greece]	Noted. Literature reviewed and assessed for possible inclusion.
40956	22	36	22	36	Also Rigby et al., 2019. [Johannes Laube, Germany]	Accepted. Rigby 2019 was not available at the time of drafting the FOD.
56084	22	39	22	39	incomplete reference (Volk) [Rolf Müller, Germany]	Editorial
27690	22	39	22	39	the year of the bibliographic citation is missing (Volk et al.). [Poot Delgado Carlos Antonio, Mexico]	Editorial
23596	22	39	22	39	Date missing from reference [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
27254	22	39	22	39	typo in Volk et al paper (year missing) [Gabriel Chiodo, Switzerland]	Editorial
40958	22	39	22	39	This reference is for the original method, which has been improved since. Updated lifetime calculations were most recently published in the SPARC 2013 report and references therein as well as in Leedham Elvidge et al., 2018 and WMO SAOD 2018. [Johannes Laube, Germany]	Accepted. We include a reference to the WMO SAOD report 2018, which includes the earlier traceability.
50212	22	44	22	44	"recent years" be more precise [Sophie SZOPA, France]	Thank you this will be considered in the FGD
40960	22	45	22	45	This should be “mole fractions”. [Johannes Laube, Germany]	Accepted. Changed.
50214	22	50	22	50	increased "by" is missing [Sophie SZOPA, France]	Accepted.
40962	22	52	22	53	WMO SAOD chapters should be cited by naming the two lead authors: Engel and Rigby et al., 2018. [Johannes Laube, Germany]	Accepted. We follow the policy to cite the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42318	23	3	23	5	Integrate key findings on benefits of rapid transition from HFCs which avoid the creation of HFC banks, the HFCs embedded in products and equipment. World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58; and Velders et al. (2014), Growth of climate change commitments from HFC banks and emissions, Atmos. Chem. & Phys. 14:4563–4572, doi:10.5194/acp-14-4563-2014 [Gabrielle Dreyfus, United States of America]	Rejected. Chapter 2 does not discuss details on emissions and attributions.
12616	23	3	23	15	A rapid switch to climate friendly alternatives also helps avoid the creation of HFC banks, the HFCs embedded in products and equipment. World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58; and Velders et al. (2014), Growth of climate change commitments from HFC banks and emissions, Atmos. Chem. & Phys. 14:4563–4572, doi:10.5194/acp-14-4563-2014 (“If, for example, HFC production were to be phased out in 2020 instead of 2050, not only could about 91–146GtCO ₂ -eq of cumulative emission be avoided from 2020 to 2050, but an additional bank of about 39–64 GtCO ₂ -eq could also be avoided in 2050. Choices of later phaseout dates lead to larger commitments to climate change unless growing banks of HFCs from millions of dispersed locations are collected and destroyed.”). [Kristin Campbell, United States of America]	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations or emission attribution.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12618	23	3	23	15	Improving air conditioner energy efficiency and switching to lower GWP refrigerants as required by the Kigali Amendment to the Montreal Protocol could avoid even more warming, up to 100 Gt CO ₂ -eq cumulatively through 2050 (Shah et al., 2015; Purohit and Höglund-Isaksson, 2017). Shah et al. (2015), Benefits of Leapfrogging to Superefficiency and Low Global Warming Potential Refrigerants in Room Air Conditioning. Berkeley, CA, USA. (“While there is some uncertainty associated with emissions and growth projections, moving to efficient room air conditioning (~30% more efficient than current technology) in parallel with low-GWP refrigerants in room air conditioning could avoid up to ~25 billion tonnes of CO ₂ in 2030, ~33billion in 2040, and ~40 billion in 2050, i.e. cumulative savings up to 98 billion tonnes of CO ₂ by 2050.”). Phasing down HFCs can avoid 0.5C of warming, and the initial phasedown of Kigali will avoid up to 0.44C. World Meteorological Organization (WMO) (2019) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 2.40–2.41 (“With the Kigali Amendment and national and regional regulations, the future production and consumption of HFCs is strongly limited (Table 2-1). Under the provisions of the Amendment, the contribution of HFCs to the global average surface temperature is projected to reach a maximum around 2060, after which it slowly decreases to about 0.06°C by 2100 (Figure 2-20). In contrast, the surface temperature contribution from HFCs in the baseline scenario is 0.3–0.5°C in 2100 (based on Xu et al., 2013 and Velders et al., 2015). The	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations or emission attribution.
12768	23	3	23	15	A rapid switch to climate friendly alternatives also helps avoid the creation of HFC banks, the HFCs embedded in products and equipment. World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58; and Velders et al. (2014), Growth of climate change commitments from HFC banks and emissions, Atmos. Chem. & Phys. 14:4563–4572, doi:10.5194/acp-14-4563-2014 (“If, for example, HFC production were to be phased out in 2020 instead of 2050, not only could about 91–146GtCO ₂ -eq of cumulative emission be avoided from 2020 to 2050, but an additional bank of about 39–64 GtCO ₂ -eq could also be avoided in 2050. Choices of later phaseout dates lead to larger commitments to climate change unless growing banks of HFCs from millions of dispersed locations are collected and destroyed.”). [Durwood Zaelke, United States of America]	Rejected. Scenario analysis is beyond the scope of this chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12770	23	3	23	15	Improving energy efficiency of air conditioners and other cooling equipment and switching to lower GWP refrigerants as required by the Kigali Amendment to the Montreal Protocol could avoid even more warming, up to 100 Gt CO ₂ -eq cumulatively through 2050. Sachar et al. (2018) Solving the Global Cooling Challenge: How to Counter the Climate Threat from Room Air Conditioners. Rocky Mountain Institute, P. 24 (“The 5X solution saves up to 100 gigatons of cumulative emissions by 2050.”); Shah et al. (2015), Benefits of Leapfrogging to Superefficiency and Low Global Warming Potential Refrigerants in Room Air Conditioning. Berkeley, CA, USA. (“While there is some uncertainty associated with emissions and growth projections, moving to efficient room air conditioning (~30% more efficient than current technology) in parallel with low-GWP refrigerants in room air conditioning could avoid up to ~25 billion tonnes of CO ₂ in 2030, ~33 billion in 2040, and ~40 billion in 2050, i.e. cumulative savings up to 98 billion tonnes of CO ₂ by 2050.”). [Durwood Zaelke, United States of America]	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations or emission attribution.
12772	23	3	23	15	Phasing down HFCs can avoid up to 0.5C of warming by 2100, and the initial phasedown schedule of the Kigali Amendment will avoid up to 0.44C. World Meteorological Organization (WMO) (2019) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 2.40–2.41 (“With the Kigali Amendment and national and regional regulations, the future production and consumption of HFCs is strongly limited (Table 2-1). Under the provisions of the Amendment, the contribution of HFCs to the global average surface temperature is projected to reach a maximum around 2060, after which it slowly decreases to about 0.06°C by 2100 (Figure 2-20). In contrast, the surface temperature contribution from HFCs in the baseline scenario is 0.3–0.5°C in 2100 (based on Xu et al., 2013 and Velders et al., 2015). The difference in projected temperatures is relevant in the context of the 2015 UNFCCC Paris Agreement, which aims to limit the global temperature increase to well below 2°C relative to pre-industrial levels.”). Note that HFC-23 is not included in the SAP calculations, and in 2016, HFC-23 contributed 0.005 W/m ² forcing, approximately 17% of the total forcing from HFCs. World Meteorological Organization (WMO) (2018) EXECUTIVE SUMMARY: SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58, ES.39 (“The 2016 Kigali Amendment to the Montreal Protocol, assuming global compliance, is expected to reduce future radiative forcing due to HFCs by about 50% in 2050 compared to the forcing from HFCs in the baseline scenario. Currently (in 2016),	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations or emission attribution.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23598	23	4	23	4	Delete , after 'blowing' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
7830	23	8	23	8	add a unit after 2?? [zhiyan zuo, China]	Rejected - comment doesn't pertain to this line and is thus unactionable.
24540	23	10	23	11	Should refer to table 7.4 for HFC ERFs. "direct radiative forcing"->"effective radiative forcing". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. All ERFs will be taken from Chapter 7.
50216	23	14	23	14	"favor or" => "favor of" [Sophie SZOPA, France]	Editorial.
23600	23	14	23	14	Change 'favor' to 'favour' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
40964	23	17	23	31	According to the WMO SAOD 2018, "Collectively, the PFCs contributed 6.3 mW m ⁻² to global radiative forcing in 2016". Both in terms of global abundances as well as radiative impact they are more important than the two minor gases that are given preference here. I am also not sure why four sentences are devoted to these two minor gases, which have many competitors in terms of global mixing ratios and radiative impacts. Moreover, radiative forcings are discussed repeatedly, which is inconsistent with previous paragraphs. [Johannes Laube, Germany]	Accepted. This section has now an consistent approach as to components discussed and treatment of ERF.
24542	23	22	23	22	I don't think 0.3mWm ⁻² can be called "significant". Again cite table 7.4. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Table 7.4 is used, and also include in the chapter 2 table.
50218	23	26	23	26	increased "by" is missing [Sophie SZOPA, France]	Editorial.
23602	23	28	23	28	Capital P for protocol [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42320	23	28	23	28	Note that the observed forcing from HCFC and HFC are lower than they would have been absent policy, specifically: the Montreal Protocol has avoid climate emissions that otherwise would have equaled 24–76 Gt CO ₂ -eq/yr in 2010 and nearly that of the forcing from CO ₂ . Velders G. J. M., et al (2007) The importance of the Montreal Protocol in protecting climate, PNAS 104(12): 4814–4819. Further, additional warming could be avoided from a faster HFC phasedown schedule under the Kigali Amendment, which would be consistent with the “start and strengthen” history of past amendments where the parties often adjusted their initial phaseout schedule to accelerate a phaseout; an example is the 2007 adjustment to accelerate the phaseout of HCFCs. United Nations Environment Programme (UNEP) (2007) DECISION XIX/6: ADJUSTMENTS TO THE MONTREAL PROTOCOL WITH REGARD TO ANNEX C, GROUP I, SUBSTANCES (HYDROCHLOROFLUOROCARBONS), in REPORT OF THE NINETEENTH MEETING OF THE PARTIES TO THE MONTREAL PROTOCOL ON SUBSTANCES THAT DEplete THE OZONE LAYER, UNEP/Oz.L.Pro.19/7; World Meteorological Organization (WMO), United Nations Environment Programme (UNEP), National Oceanic and Atmospheric Administration (NOAA), National Aeronautics and Space Administration (NASA), & European Commission (2019) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 6.11 . [Gabrielle Dreyfus, United States of America]	Rejected. While the comment is valid, it is out of scope for this chapter to discuss counterfactual or future scenarios.
50220	23	28	23	31	maybe also add in the summary that some new species used in specific industrial use are increasing [Sophie SZOPA, France]	Accepted. The text includes a discussion of some new species, if relevant in the climate context.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12620	23	28	23	31	<p>It would be worth adding that the collective efforts to phase down CFCs, starting with consumer boycotts, some national and regional measures, and the Montreal Protocol, has avoid climate emissions that otherwise would have equaled 24–76 Gt CO₂-eq/yr in 2010 and nearly that of the forcing from CO₂. Velders G .J. M., et al (2007) The importance of the Montreal Protocol in protecting climate, PNAS 104(12): 4814–4819 (“The GWP-weighted emissions comparisons in Fig. 2 and Table 1 allow a direct comparison of the climate influences of ODSs and CO₂. In the baseline scenario, the annual contribution of ODSs to GWP-weighted emissions peaked in 1988 at a value slightly less than half that of global CO₂ emissions. After 1988, the contribution of ODSs falls sharply in contrast to increasing CO₂ emissions. By 2010, ODS emissions will have declined to 4–5% of Special Report on Emissions Scenarios (SRES) CO₂ emissions, which are projected to increase to 29–35 GtCO₂- eq-yr-1. In contrast, without the early warning of the effects of CFCs (MR74 scenario), estimated ODS emissions would have reached 24–76 GtCO₂- eq-yr-1 in 2010. Thus, in the current decade, in a world without ODS restrictions, annual ODS emissions using only the GWP metric could be as important for climate forcing as those of CO₂.”). [Kristin Campbell, United States of America]</p>	<p>Rejected. While the remark is valid, it is beyond scope for Chapter 2.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12622	23	28	23	31	Additional warming could be avoided from a faster HFC phasedown schedule, which would be consistent with the “start and strengthen” history of past amendments that often adjusted their initial phaseout schedule to accelerate a phaseout; an example is the 2007 adjustment to accelerate the phaseout of HCFCs. World Meteorological Organization (WMO), United Nations Environment Programme (UNEP), National Oceanic and Atmospheric Administration (NOAA), National Aeronautics and Space Administration (NASA), & European Commission (2019) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 6.11 (“Of course, adjustments to the HFC control schedules analogous to historical adjustments to the ODS control schedules could substantially reduce the climate impact.”); United Nations Environment Programme (UNEP) (2007) DECISION XIX/6: ADJUSTMENTS TO THE MONTREAL PROTOCOL WITH REGARD TO ANNEX C, GROUP I, SUBSTANCES (HYDROCHLOROFLUOROCARBONS), in REPORT OF THE NINETEENTH MEETING OF THE PARTIES TO THE MONTREAL PROTOCOL ON SUBSTANCES THAT DEplete THE OZONE LAYER, UNEP/Oz.L.Pro.19/7; Zaelke, Andersen, & Borgford-Parnell (2012) Strengthening Ambition for Climate Mitigation: The Role of the Montreal Protocol in Reducing Short-lived Climate Pollutants, RECIEL doi: 10.1111/reel.12010 (“Another important feature is the treaty’s ‘start and strengthen’ philosophy. Throughout its 25-year history, the Montreal Protocol has started by addressing a problem, learned by	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations, or emission attribution.
24544	23	28	23	31	It might be useful to say the the 0.25 W/m2 is a decrease from 0.26 in 2011 (Myhre et al. 2013). "Radiative forcing" should be "Effective Radiative forcing". These values should cite table 7.4. Consistency is needed in the number of significant figures. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We will use ERF and values from Chapter 7. A discussion of change of RF since 2011 has been included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12774	23	28	23	31	<p>It would be worth adding that the collective efforts to phase down CFCs, starting with consumer boycotts, some national and regional measures, and the Montreal Protocol, has avoid climate emissions that otherwise would have equaled 24–76 Gt CO₂-eq/yr in 2010 and nearly that of the forcing from CO₂. Velders G .J. M., et al (2007) The importance of the Montreal Protocol in protecting climate, PNAS 104(12): 4814–4819 (“The GWP-weighted emissions comparisons in Fig. 2 and Table 1 allow a direct comparison of the climate influences of ODSs and CO₂. In the baseline scenario, the annual contribution of ODSs to GWP-weighted emissions peaked in 1988 at a value slightly less than half that of global CO₂ emissions. After 1988, the contribution of ODSs falls sharply in contrast to increasing CO₂ emissions. By 2010, ODS emissions will have declined to 4–5% of Special Report on Emissions Scenarios (SRES) CO₂ emissions, which are projected to increase to 29–35 GtCO₂-eq-yr-1. In contrast, without the early warning of the effects of CFCs (MR74 scenario), estimated ODS emissions would have reached 24–76 GtCO₂-eq-yr-1 in 2010. Thus, in the current decade, in a world without ODS restrictions, annual ODS emissions using only the GWP metric could be as important for climate forcing as those of CO₂.”). [Durwood Zaelke, United States of America]</p>	<p>Rejected. While the remark is valid, it is beyond scope for Chapter 2.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12776	23	28	23	31	Additional warming could be avoided from a faster HFC phasedown schedule under the Kigali Amendment, which would be consistent with the “start and strengthen” history of past amendments where the parties often adjusted their initial phaseout schedule to accelerate a phaseout; an example is the 2007 adjustment to accelerate the phaseout of HCFCs. United Nations Environment Programme (UNEP) (2007) DECISION XIX/6: ADJUSTMENTS TO THE MONTREAL PROTOCOL WITH REGARD TO ANNEX C, GROUP I, SUBSTANCES (HYDROCHLOROFLUOROCARBONS), in REPORT OF THE NINETEENTH MEETING OF THE PARTIES TO THE MONTREAL PROTOCOL ON SUBSTANCES THAT DEplete THE OZONE LAYER, UNEP/Oz.L.Pro.19/7; World Meteorological Organization (WMO), United Nations Environment Programme (UNEP), National Oceanic and Atmospheric Administration (NOAA), National Aeronautics and Space Administration (NASA), & European Commission (2019) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58, 6.11 (“Of course, adjustments to the HFC control schedules analogous to historical adjustments to the ODS control schedules could substantially reduce the climate impact.”); Zaelke, Andersen, & Borgford-Parnell (2012) Strengthening Ambition for Climate Mitigation: The Role of the Montreal Protocol in Reducing Short-lived Climate Pollutants, RECIEL doi: 10.1111/reel.12010 (“Another important feature is the treaty’s ‘start and strengthen’ philosophy. Throughout its 25-year history, the Montreal Protocol has started by addressing a problem,	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations, or emission attribution.
53302	23	29	23	29	I suggest you write "total direct" since this is an aggregate of gases, and since indirect effects via strat O3 are not included. [Jan Fuglestedt, Norway]	Accepted.
17988	23	30	23	31	Perhaps state the radiative forcing from CO2 here or somewhere close by for comparison. [Gwenaelle GREMION, Canada]	Accept. We mention the ERF of well mixed GHGs, in agreement with the analysis in Chapter 7.
35544	23	30	23	31	The meaning of the phrase 'with emissions restrictions imposed under the Kigali Ammendment of the Montreal Protocol' is unclear as written. As written this could be interpreted as saying that the Kigali Ammendment has limited the radiative forcing of HCFCs and HFCs, but this is not the case, since the Kigali Ammendment only came into force in 2019. I suggest instead inserting after 'HFCs' - ', which are now regulated by the Kigali Ammendment to the Montreal Protocol'. [Nathan Gillett, Canada]	Accepted, the sentence is adapted..
23604	23	31	23	31	Capital P for protocol [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7748	23	42	23	42	Where is tropospheric water vapour? While stratospheric water vapour is included, tropospheric WV is not included here or elsewhere in the chapter, even though it's often designated as a short-lived gas. Yet the climate role of the water vapour column (which I have measured at or near solar noon for 29.4 years) dwarfs that played by stratospheric WV. While the IPCC traditionally downplays the role of tropospheric water vapour as a greenhouse gas, the positive feedback of its response to CO2 is several times more significant than CO2 alone. The IPCC requires that this report be "comprehensive, objective, open and transparent." Therefore, please include the tropospheric water vapour in this section. [Forrest Mims, United States of America]	Noted. Tropospheric water vapor responds strongly to climate change, but is less a driver. Assessment of surface tropospheric water vapor change is included in section 2.3.1.2.2 and column water vapor in 2.3.1.2.3, with an emphasis on dynamical aspects. In this section there is clear discussion of the increasing trend in tropospheric WV (total column) and its direct cause (observed increase in atmospheric temperature). Although similar to tropospheric water vapor, stratospheric H2O has some (thermo)dynamic feedback mechanisms and changes are partly driven by the contribution of changing CH4 to stratospheric H2O, which warrants inclusion in the drivers section.
15538	23	44			2.2.5 Short-lived gases While the water vapour and ozone is important short-lived gases, It is necessary to add more species of short-lived gases including sulphate, nitrate and ammonia, which play key roles to modulate the radiative balance in climate system. [SANG-WOOK YEH, Republic of Korea]	Rejected. We note that some information on aerosol composition is given in section 2.2.6, and more detailed information in Chapter 6.
7750	23	46	23	46	Please consider looking at other sources beyond Boulder-launched balloons and satellite data to increase confidence in stratospheric water vapour measurements. Frost point hygrometers are also launched from the Hilo Airport and a few other sites. Also, microwave measurements of stratospheric water vapour down to 26 km have been made since 1995 and before by the NRL team. Mike Gomez plays a major role in this long-term project. See Gerald E. Nedoluha et al. Validation of long-term measurements of water vapour from the midstratosphere to the mesosphere at two Network for the Detection of Atmospheric Composition Change sites. J. Geophys. Res.: Atmospheres, 2013, 118, 934–942. [Forrest Mims, United States of America]	Rejected. Unfortunately, Hilo and other sites do not provide time series of sufficient length for this analysis. Attempts to merge datasets from other instruments are dependent on bias-correction methods that increase uncertainty and make trend analysis unreliable. The microwave radiometer measurements at 26 km are noisy, and show significant offsets from both satellite and frost point hygrometers.
50222	23	47	23	47	"assessed" => "attributed" ? [Sophie SZOPA, France]	Rejected. The assessment statement was taken from AR5.
56086	23	49	23	49	mention >Boulder< here; single station sounds cryptic [Rolf Müller, Germany]	Rejected. For a summary statement this is overly precise.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24546	23	51	23	51	What was called "RF" in AR5 is now called "SARF" in AR6 (stratospheric adjusted radiative forcing). [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Terminology is employed as per IPCC guidelines.
29532	23	54	24	7	Wang et al. (2016) shows that recent stratospheric water vapor can still be part of internal variability! Wang, W., K. Matthes, N. Omrani, and M. Latif (2016), Decadal variability of the tropical tropopause temperature and its relation to the Pacific Decadal Oscillation, Nature Scientific Reports, 6, doi:10.1038/srep29537SREP-16-02848B. [Katja Matthes, Germany]	Accepted. We include a reference to the role of PDO, bearing in mind this is insofar the only published study and hence low confidence.
24548	23	54	24	7	This paragraph on feedbacks on SWV doesn't belong in a "Drivers" chapter, it should probably go somewhere else (Ch 7?). [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. SWV is included in this section, as anthropogenic increases in tropospheric methane (a climate driver) has likely increased the oxidation source of SWV. Also, surface temperature trends directly drive changes in the tropospheric WV but not in SWV, while conversely trends in SWV drive changes in surface temperature.
56088	24	1	24	1	add sudden stratospheric warmings to QBO and ENSO [Rolf Müller, Germany]	Accepted. Added.
56090	24	3	24	3	I think Smith et al., JGR, 2017 would also be a good reference here [Rolf Müller, Germany]	Rejected. Smith et al. (2017) conveys similar information as the Anderson et al. (2017).
27256	24	7	24	7	Some references on relevant studies focused on the SWV feedback in models are missing, besides Dessler et al., 2013. The very first paper showing a potential feedback from SWV was Forster and Shine, GRL 2002. Another relevant (and more recent paper) that revisits this issue is Banerjee et al., CDYN 2019 [Gabriel Chiodo, Switzerland]	Partly accepted. The references to Forster and Shine (1999) have been updated with more recent literature. We mention of the Banerjee paper about the relevance of mid-latitude SWV climate feedbacks.
56114	24	9	24	9	I suggest to mention here (or elsewhere) that the observational basis for SWV is problematic. Global observations lack the spatial resolution and local observations (Boulder) do not provide sufficient coverage (see e.g., Müller, Kunz, Hurst et al., Earth's future, 2016 [Rolf Müller, Germany])	Rejected. We think this is sufficiently clear from the current text. More in-depth discussion would be unnecessarily detailed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50224	24	9	24	15	Is this example statistically significant? [Sophie SZOPA, France]	Accepted. There is an uncertainty value (± 0.1) attached to each of the three monthly tropical mean anomalies. The uncertainty value is the 95% confidence interval of the standard error of the mean. These uncertainties show the statistical significance of the monthly tropical mean anomalies.
17920	24	20	24	34	What about other measurement sites for SWV besides Boulder, CO, USA? For instance, Scandinavia have used to have good SWV measurements. [Branko Grisogono, Croatia]	Noted. We have contacted reviewer requesting further information. Unfortunately no response has been received.
23606	24	21	24	21	Insert (USA) after 'Colorado' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
23608	24	24	24	24	Delete ', olorado (USA)' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
35546	24	24	24	34	The assessment conclusion is not clear here. Do the authors agree with Hegglin et al. (2014) that the increasing trend over Boulder is not representative of the global stratosphere, but there is no inconsistency with the satellite record? The text on lines 27-29 seems to say that there are no significant differences between satellite observations and Boulder observations, which agrees with this conclusion (even though the way it is introduced suggests it contradicts Hegglin et al). Also this section does not discuss the Hegglin et al. conclusion that overall stratospheric water vapour has decreased in the lower stratosphere and increased in the upper stratosphere. [Nathan Gillett, Canada]	Accepted. There are different outcomes from studies. Overall the assessment is low confidence in changes.
29966	24	25	24	29	This discussion is misleading, as it makes it sound as though Lossow et al. (2018) is in contradiction to Hegglin et al. (2014). In fact, Hegglin et al. (2014) already showed that there is no evidence from subsampling of a nudged model that the Boulder record is unrepresentative of the global stratosphere, but since this relies on the assumption that the models are credible in their variability, it cannot be considered definitive. Also, Lossow et al. (2018) certainly did not show there was no discrepancy between the Boulder record and satellite observations (which anyway do not go back to 1980), otherwise what is said in lines 31-34 would not be correct! [Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Hegglin states: "Our results suggest that the water vapour trends over Boulder should not be considered representative of the global stratosphere." In comparison, Lossow: "The main outcome of this study that the temporal behaviour at Boulder largely resembles that for the zonal mean around the Boulder latitude ...". We further clarify this in this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57406	24	25	24	29	Given the results presented in Hegglin et al. (2014) I propose to change this sentence into: "Lossow et al. (2018) showed insignificant differences between SWV trends at Boulder (40°N) and for the 35-45°N zonal mean from 1980 to 2010 using model simulations and satellite observations while Hegglin et al. (2014) observed a latitudinal dependence of SWV trends." [Marc Schröder, Germany]	Rejected. The sentence suggested by the reviewer is confusing because it mixes together the question of Boulder being representative of the 40°N zonal average (non-uniformity with respect to longitude) and the statement that SWV trends are dependent on latitude. We propose a revised sentence, based on actual quotes from the two papers.
7832	24	26	24	28	no relative content in last paragraphs about North Atlantic region and how to concluded? Do we need to say some conclusion about East Asia since we talk a lot about the AOD in East Asia in the last paragraph. [zhiyan zuo, China]	Rejected. We suspect the page and/or line numbers were misplaced and the comment is thus unactionable.
56092	24	26	24	29	It should be mentioned here that Boulder represents a mixture of a tropical water vapour trend and a mid-latitude water vapour trend; see Kunz et al., JGR, 2013 for details on this issue in the Boulder time series [Rolf Müller, Germany]	Rejected. It is difficult to see the added value of this, as the lower stratosphere over almost any mid-latitude site can sometimes be influenced by the tropics, depending on the position of the subtropical jet.
23610	24	31	24	31	No capital F for frost [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56094	24	34	24	34	I suggest discussing here whether such a thing as a >long term trend< in SWV exists at all. Is it expected to exist? [Rolf Müller, Germany]	Rejected. It is out of scope to discuss mechanism or (model) expected trends. Given the observed large long-term increase in tropospheric methane, it is intuitive that the oxidative source of SWV should have also increased. However, other factors that influence SWV could mask this expected increase, such as an acceleration of the Brewer-Dobson circulation. The upward trend in SWV is unfortunately based on one unique set of observations that began 7-8 years before the satellite era. Again, any trends deduced from satellite measurements do not include those 7-8 years when much of the increase occurred over Boulder. Claims that there is no trend (or even a negative trend) in satellite records of SWV since the late 1980s do not contradict the increase observed over Boulder, they simply cover a different time period.
17990	24	34	24	34	Add "due to insufficient and conflicting data" to the last sentence to explain the low confidence, there is almost equally low confidence in the lack of a long-term trend. [Gwenaëlle GREMION, Canada]	Reject. This is sufficiently explained in the preceding text.
35548	24	34			Avoid the formulation 'there continues to be low confidence in a long-term trend in SWV', since it's not clear if a trend has been observed but we only have low confidence in it, or if no trend has been observed. Something like 'No consistent long-term trend in SWV has been observed (confidence qualifier)' would be better. [Nathan Gillett, Canada]	Thank you this will be implemented in the FGD
40966	24	37	24	37	The Ball et al., 2018 paper caused much controversy by reporting the continuation of decreasing ozone levels in the NH mid-latitude lower stratosphere (i.e. over densely populated regions) and should not be ignored in this section. Also, what about the follow up paper by Chipperfield et al.? Finally, there needs to be more coordination with Chapter 6 to ensure a consistent message and minimise repetition. [Johannes Laube, Germany]	Accepted. References to Ball and Chipperfield are added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24550	24	39	24	41	This paragraph needs rephrasing. AR5 didn't assess that it was certain that stratospheric ozone was nearly constant. Hartmann et al. 2013 needs to be cited for AR5. Use "SARF" rather than "RF". For AR5 the SARF was -0.05+0.10 to 2011. Cite Myhre et al. 2013 for AR5 values. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Noted, there is still a conflict with what AR5 assessed and this will be resolved in the FGD.
35550	24	39			AR5 did not assess that it was certain 'that global stratospheric ozone from the mid-1990s to 2011 was nearly constant and about 3.5% lower than in the reference period 1964-1980'. This statement is included in the ES of chapter 5, but there is no likelihood qualifier attached. Delete 'it was certain that'. [Nathan Gillett, Canada]	Accepted. The opening sentence is revised..
50226	24	41	24	41	please complete the sentence, "that RF due to the stratospheric change since preindustrial is" [Sophie SZOPA, France]	Accepted. Sentence is further modified according to other reviewers' requests.
56096	24	43	24	48	References for all these statements about stratospheric ozone? [Rolf Müller, Germany]	Accepted. A reference to the Ozone Assessment (Braesicke et al, 2018) is added.
40968	24	47	24	47	This should be "ODSs". [Johannes Laube, Germany]	Accepted.
23612	24	47	24	47	Insert , after 'latitudes' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
37418	24	47	24	49	The text needs attention here. I assume "ozone" is the missing word before "declines" and "these gases" could be changed to "the ODSs" . [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The revised text now includes separate statements on non-polar and polar regions.
40970	24	49	24	49	"Emission of these gases" is confusing after a sentence about tropical ozone. [Johannes Laube, Germany]	Accepted. Changed in ODS.
56098	24	49	24	49	what is meant here? > these gases< ?? [Rolf Müller, Germany]	Accepted. Replaced by ODS.
24552	24	49	24	49	"these gases": make it clear this refers to ODSs, not ozone. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ODS is mentioned now.
35552	24	49			It isn't clear what 'these gases' refers to, since the previous sentence is discussing tropospheric ozone. Replace with 'ODSs'. [Nathan Gillett, Canada]	Accepted.
56116	24	50	24	50	Is there really consensus (see wmo ozone assessment 2018) on the ozone loss findings by Shepered et al., 2014? [Rolf Müller, Germany]	Accepted. We have softened the statement to 'some estimates suggest'.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56100	25	14	25	18	I think the papers by Ball et al (ACP, 2018) and Chipperfield et al. (GRL, 2018) on recent trends/changes in lower stratospheric ozone [Rolf Müller, Germany]	Rejected. Ball et al. 2018 says that there is still a decline in the lower stratosphere, while Chipperfield et al. 2018 argues that the decline is just due to high natural variability. This level of detail is too large for this section however, but we will change the word 'stabilisation' to 'near-stabilisation' in the FGD.
12624	25	20	25	26	Stratospheric ozone is starting to show signs of recovery, with noticeable improvements emerging expected by the 2030s and repair of the Antarctic ozone hole expected around 2060. World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58. However, continued recovery may be slowed due to the recent observation of unreported emissions of CFC-11, which are likely coming from eastern China. Montzka S. A., et al. (2018) An unexpected and persistent increase in global emissions of ozone-depleting CFC-11, NATURE 557:413–417; World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Executive Summary, Global Ozone Research and Monitoring Project-Report No. 58, ES.3; Rigby M., et al. (2019) Increase in CFC-11 emissions from eastern China based on atmospheric observations, NATURE 569:546–550. [Kristin Campbell, United States of America]	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations, or emission attribution.
12778	25	20	25	26	Stratospheric ozone is starting to show signs of recovery, with noticeable improvements expected by the 2030s and repair of the Antarctic ozone hole expected around 2060. World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Global Ozone Research and Monitoring Project-Report No. 58. However, continued recovery may be slowed due to the recent observation of unreported emissions of CFC-11, which are likely coming from eastern China. Montzka S. A., et al. (2018) An unexpected and persistent increase in global emissions of ozone-depleting CFC-11, NATURE 557:413–417; World Meteorological Organization (WMO) (2018) SCIENTIFIC ASSESSMENT OF OZONE DEPLETION: 2018, Executive Summary, Global Ozone Research and Monitoring Project-Report No. 58, ES.3; Rigby M., et al. (2019) Increase in CFC-11 emissions from eastern China based on atmospheric observations, NATURE 569:546–550. [Durwood Zaelke, United States of America]	Rejected. Chapter 2 exclusively deals with observations, and not with future concentrations, or emission attribution.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23614	25	22	25	22	Give dates/quantify 'recent years' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed to 'after 2000'
28890	25	22	25	22	For consistency, you should cite the specific chapter of WMO 2018, in this case Chapter 4 (Langematz et al) [Matt Tully, Australia]	Accepted. This will be implemented in the FGD
43860	25	24	25	26	I do not agree that event of ozone depletion in the Arctic in 2011 was not comparable to a typical event in the [Joanna Wibig, Poland]	Rejected. We compare the magnitudes, and do not use the term 'ozone hole'.
43862	25	24	25	26	Antarctic, it was deep depletion, but still the minimum was higher than 300DU and it should be clearly indicated that [Joanna Wibig, Poland]	Rejected. We compare the magnitudes, and do not use the term 'ozone hole'.
43864	25	24	25	26	this event was far from was is called "ozone hole" what is suggested by word "comparable" [Joanna Wibig, Poland]	Rejected. We compare the magnitudes, and do not use the term 'ozone hole'.
56102	25	26	25	26	Langematz (2018) is not the only citation for strong Arctic ozone loss in 2011 (see e.g. WMO 2018) [Rolf Müller, Germany]	Accepted. We added a reference to Manney 2011, as the main reference for Arctic ozone loss in 2011.
27258	25	26	25	26	Relevant reference missing here: Mannay et al., 2011 [Gabriel Chiodo, Switzerland]	Rejected. The reference request seems to be misaligned with the text here and it is unclear where it should refer to.
7834	25	27	26	15	no references in the subsection [zhiyan zuo, China]	Rejected. There are references in these subsections, looks mentioned page/line numbers are misplaced.
37420	25	28	25	30	See comment 69. Now some readers have been through the detailed text they will understand the earlier summary conclusion better, but it should nevertheless be rewritten so polar as well as extrapolar conditions are covered. It is important that summary statements can easily be read in stand-alone form, as there will presumably be a class of reader who reads only the headlines at the beginning of each chapter. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The summary statement refers to polar ozone and extra-polar ozone..
53304	25	28	25	30	Good to end the section with a summary like this. [Jan Fuglestedt, Norway]	Noted with thanks.
35526	25	31			What are the uncertainties in this difference of 14%? [Nathan Gillett, Canada]	Rejected. We have a statement that near-global ozone "increased slightly, but not significantly, during 2000-2017". There is no need to repeat this in the summary statement.
50228	25	35	25	35	"assessed" => "attributed" ? [Sophie SZOPA, France]	Reject. AR5 summary statements were assessment not attributions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23616	25	37	25	37	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
35554	25	37			Replace 'Low confidence was attributed to increases in the Southern Hemisphere.' with 'There was low confidence that increases in the Southern Hemisphere had occurred'. [Nathan Gillett, Canada]	Accepted.
50370	25	42	25	43	"collated" => "gathered"? [Sophie SZOPA, France]	Accepted. Changed into re-evaluated.
23618	25	43	25	43	Insert , after 'AR5' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
23620	25	44	25	44	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
50230	25	46	25	48	consider to include recent study of Yeung et al. Nature, June 2019 [Sophie SZOPA, France]	Accepted. Study included.
23622	25	48	25	48	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
23624	25	51	25	51	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24554	25	52	25	53	Better to quote 48% as 50% if it is +-30%. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The numbers reflect statistical analysis performed in TOAR, and is rounded to 47 %+-30 % spanning results from t-test and Wilkinson tests.
23626	26	2	26	2	Insert , after 'hemisphere' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
57408	26	7	26	34	Trends from ozonesondes and aircrafts are presented in Figure 2.7 and in Table 2.SM.1. As the figure already addresses (partly) this topic and because all information given in the table is seemingly available from the literature I propose to remove the Table 2.SM.1 and include the references here. [Marc Schröder, Germany]	The table is included in the Supplement to Cooper et al., 2019, which is submitted to Elementa by mid-August 2019.
24556	26	9	26	11	This needs to be specific about what period the 2-10% increase applies to. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Period included.
26906	26	12	26	12	Please specify the time period that the statement about hemispheric scale surface ozone trends refers. [Prodomos Zanis, Greece]	Accepted. All surface trends in the figure begin in 1995. The ending date varies from 2013 to 2018, but more than half end in 2017.
23628	26	14	26	14	Don't split numbers and units across lines [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35556	26	19	26	20	Are the Gaudel et al. results for the South Pole? [Nathan Gillett, Canada]	Noted, thank you. The results for specific regions are now summarised in the extensive table in Cooper et al. (submitted), August, 2019 which we cite.
7836	26	21	26	29	bad color in the figure is chosed [zhiyan zuo, China]	Accepted. A revision based on TSU recommended colours has been made.
35558	26	40	26	42	This statement that tropospheric ozone increased in the tropics seems to contradict pg 26 ln 2-3, which says that there is no evidence from historical data for ozone increases in the northern tropics. [Nathan Gillett, Canada]	Rejected. Page 26, lines 2-3, refer to surface trends in the tropics based on historical data (1954-1975). We then discuss tropical trends in the free troposphere since the mid-1970s.
7752	26	45	26	50	This section on aerosols is well done. It's good to see the Aeronet contribution to the global aerosol database. It's important for readers to understand that incorporating optical depth into climate models is complex and a work in progress, mainly due to substantial seasonal and regional variations. (I've measured the optical depth of Saharan dust events for 30 years. I also measured significant declines in temperature associated with huge increases in optical depth from biomass smoke during two campaigns in Brazil for NASA during annual burning seasons. While these events cover enormous geography, they are regional, not global. The Pinatubo aerosol cloud of 1991-93, which I also measured, was genuinely global.) [Forrest Mims, United States of America]	Noted. We thank the reviewer for this encouraging statement
41760	26	50	26	51	uncertainties/ranges from AR5 should be reported [Jan Cermak, Germany]	Accepted. The uncertainties are added.
24558	26	50	26	51	The -0.90 W/m2 includes both ARI and ACI, not just ACI. Cite Boucher et al. 2013 for this. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The mistake is corrected. We opted to cite Chapter 8 instead of Chapter 7 of AR5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13884	27	11	27	12	References should be Albani et al. 2016 (not 2015b) and Lambert et al. 2015. These are also currently missing from Annex II. LGM ; global ; atmo - dust emissions ; relative to 2-0 ka BP ; factor ; 2.2 ; ; ; (Albani et al., 2016) ; based on one model tuned to global dataset LGM ; global ; atmo - dust emissions ; relative to 12-0 ka BP ; factor ; 4 ; ; ; (Lambert et al., 2015) ; based on spatial interpolation of global dataset Albani S, Mahowald NM, Murphy LN, Raiswell R, Moore JK, Anderson RF, et al. Paleodust variability since the last glacial maximum and implications for iron inputs to the ocean. Geophys Res Lett. 2016;43(8):3944–54. [Samuel Albani, Italy]	Taken into account. The references are corrected in Chapter 2. Re. the annex question: Taken into account: At LAM3, Paleo BOG decided to omit values for each metric in this Annex.
23630	27	13	27	13	Change 'indicates' to 'indicate' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
23632	27	17	27	17	Change 'form' to 'from' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
30494	27	17	27	21	how do the ranges here mentioned combine with glacial/interglacial ratios? [Annalisa Cherchi, Italy]	Taken into account. The “broader scale” is now precisely referring to the Holocene.
23634	27	18	27	18	Change 'sediments' to 'sediment' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
13876	27	20	27	21	It would seem more accurate to state that volcanic emissions drive significant variability in sulphate concentrations in the few years following a major volcanic eruption, whereas biogenic sources can control large variations in emissions of sulphate aerosol and precursors on multi-millennial time scales. It should also be noted that these variations in non-sea salt sulfate concentrations are evident in Greenland, but not in Antarctica (e.g. Wolff et al., 2006). Wolff EW, Fischer H, Fundel F, Ruth U, Twarloh B, Littot GC, et al. Southern Ocean sea-ice extent, productivity and iron flux over the past eight glacial cycles. Nature. 2006;440(7083):491–6. [Samuel Albani, Italy]	Taken into account. The idea about volcanic contributions is that volcanic activity shows variability over longer times as well. Reworded to make this now more clear. Data for Antarctica has been added to the assessment.
13878	27	20	27	21	It might be interesting to comment briefly on what we know or do not know about other natural aerosol species variations on glacial/interglacial time scales (e.g. recently reviewed by Albani et al., 2018, and references therein). Albani S., Balkanski Y., Mahowald N., Winckler G., Maggi V., Delmonte B.: Aerosol-climate interactions during the Last Glacial Maximum. Curr. Clim. Change Rep., 4, 99-114, doi:10.1007/s40641-018-0100-7, 2018. [Samuel Albani, Italy]	Noted. However, since the previous part of the section is about natural aerosol, no extra text is added.
23636	27	21	27	21	Quantify 'significant variability' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Quantified as “(up to one order of magnitude)”

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17996	27	23	27	25	I suggest adding a sentence in the beginning defining the sources of BC since SO4 sources are discussed in the paragraph above and black carbon has very different sources. Perhaps " During the pre-industrial, black carbon is sourced from biomass burning, after industrialization, black carbon is sourced both from biomass burning and fossil fuel emissions. A recent study on the contribution of biomass burning to the arctic was shown to be 39+/- 10% (Winiger et al., 2019)." [Gwenaelle GREMION, Canada]	Noted. In SOD the description of the aerosol sources is omitted partly due to space limitations.
30496	27	23	27	31	how do these regions are defined is missing [Annalisa Cherchi, Italy]	Taken into account. The regions are defined in the references provided in the caption of Fig. 2.8.
13880	27	23	27	31	Please specify what is exactly meant by "sulfate" here, i.e. non-sea salt sulfate? Do there records also represent known volcanic signals ? [Samuel Albani, Italy]	Taken into account. This refers to Fig. 2.8, and in the caption the clarification is provided that this is nss sulphate. Some volcanic signals are identifiable.
17994	27	23	27	31	This paragraph lacks citations. [Gwenaelle GREMION, Canada]	Taken into account. This impression arises since the Figure now appears at the end of the document. Once it is placed in the text, the reader will see all the references in the Figure caption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12626	27	23	27	31	Depositions of black carbon in Arctic regions are particularly harmful because they decrease the albedo and enhance melting in the region that is already facing increased melting from warming. Tedesco M., et al. (2016) The darkening of the Greenland ice sheet: trends, drivers, and projections (1981–2100), THE CRYOSPHERE 10:477–496, 478 (“The presence of LAI such as soot (black carbon, BC), dust, organic matter, algae, and other biological material in snow or ice also reduces the albedo, mostly in the visible and ultraviolet regions (Warren, 1982). Such impurities are deposited through dry and wet deposition, and their mixing ratios are enhanced through snow water loss in sublimation and melting (Conway et al., 1996; Flanner et al., 2007; Doherty et al., 2013). Besides grain growth and LAI, another cause of albedo reduction over the GrIS is the exposure of bare ice: once layers of snow or firn are removed through ablation, the exposure of the underlying bare ice will further reduce surface albedo, as does the presence of melt pools on the ice surface (e.g. Tedesco et al., 2011).”); World Bank & International Cryosphere Climate Initiative (2013) ON THIN ICE: HOW CUTTING POLLUTION CAN SLOW WARMING AND SAVE LIVES, 2 (“Climate benefits for cryosphere regions from black carbon reductions carry less uncertainty than they would in other parts of the globe and are sometimes very large. This is because emissions from sources that emit black carbon—even with other pollutants—almost always lead to warming over reflective ice and snow.”); Arctic Monitoring and Assessment Programme (AMAP) (2017) ADAPTATION ACTIONS FOR A CHANGING	Taken into account. We now refer to Chapters 6 and 7 explicitly so the reader is pointed to this important aspect.
12628	27	23	27	31	At the same time, the removal of sulfates and other aerosols has unmasked warming. Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change; Ramanathan and Feng (2008) On avoiding dangerous anthropogenic interference with the climate system: Formidable challenges ahead, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.0803838105; Ramanathan and Xu (2010) The Copenhagen Accord for limiting global warming: Criteria, constraints, and available avenues, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1002293107. [Kristin Campbell, United States of America]	Taken into account. We now refer to Chapters 6 and 7 explicitly so the reader is pointed to this important aspect.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12780	27	23	27	31	Depositions of black carbon in Arctic regions are particularly harmful because they decrease the albedo and enhance melting in the region that is already facing increased melting from warming. Tedesco M., et al. (2016) The darkening of the Greenland ice sheet: trends, drivers, and projections (1981–2100), THE CRYOSPHERE 10:477–496, 478 (“The presence of LAI such as soot (black carbon, BC), dust, organic matter, algae, and other biological material in snow or ice also reduces the albedo, mostly in the visible and ultraviolet regions (Warren, 1982). Such impurities are deposited through dry and wet deposition, and their mixing ratios are enhanced through snow water loss in sublimation and melting (Conway et al., 1996; Flanner et al., 2007; Doherty et al., 2013). Besides grain growth and LAI, another cause of albedo reduction over the GrIS is the exposure of bare ice: once layers of snow or firn are removed through ablation, the exposure of the underlying bare ice will further reduce surface albedo, as does the presence of melt pools on the ice surface (e.g. Tedesco et al., 2011).”); World Bank & International Cryosphere Climate Initiative (2013) ON THIN ICE: HOW CUTTING POLLUTION CAN SLOW WARMING AND SAVE LIVES, 2 (“Climate benefits for cryosphere regions from black carbon reductions carry less uncertainty than they would in other parts of the globe and are sometimes very large. This is because emissions from sources that emit black carbon—even with other pollutants—almost always lead to warming over reflective ice and snow.”); Arctic Monitoring and Assessment Programme (AMAP) (2017) ADAPTATION ACTIONS FOR A CHANGING	Taken into account. We now refer to Chapters 6 and 7 explicitly so the reader is pointed to this important aspect.
12782	27	23	27	31	At the same time, the removal of sulfates and other cooling aerosols has unmasked warming. Well Under 2 Degrees Celsius: Fast Action Policies to Protect People and the Planet from Extreme Climate Change; Ramanathan and Feng (2008) On avoiding dangerous anthropogenic interference with the climate system: Formidable challenges ahead, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.0803838105; Ramanathan and Xu (2010) The Copenhagen Accord for limiting global warming: Criteria, constraints, and available avenues, Proc. Natl. Acad. Sci., doi: 10.1073/pnas.1002293107. [Durwood Zaelke, United States of America]	Taken into account. We now refer to Chapters 6 and 7 explicitly so the reader is pointed to this important aspect.
23638	27	25	27	25	Superscript ‘th’ [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35560	27	25	27	27	The text describes changes in 'Europe', in Russia and in Svalbard. Svalbard and much of Russia are in Europe, so the region or location currently described as 'Europe' should be described more specifically. [Nathan Gillett, Canada]	Accepted. Done as “continental Europe”.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23640	27	27	27	27	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
17998	27	29	27	31	Recent records from North America and Antarctica have not been included here. For North America, please see Chellman et al., 2017, ES&T which demonstrates a decline in BC to Fremont Glacier, Wyoming after mid-1900s. For Antarctica, please see Bisiaux et al., 2012a,b Atm. Chem. Phy and Arienzo et al., 2017 JGR . All three papers demonstrate recent trends in black carbon deposition to Antarctica. [Gwenaelle GREMION, Canada]	Taken into account. A sentence is added referring to the Antarctica results. We have not included the BC record from the Upper Fremont Glacier for two reasons: 1) this glacier is strongly affected by melting process with unknown consequences on the BC record; and 2) different age-depth relationships were published for this core (Schuster et al., 2000; Chellman et al., 2017) and it is not clear which one, if any, is correct.
23642	27	30	27	30	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
13882	27	31	27	31	While very significant uncertainties accompany the interpretations of those measurement and the potential causes, it was reported that at least in some regions dust emissions might have increased since the pre-industrial era and/or during the twentieth century (Mahowald et al., 2010; Hooper and Marx, 2018). These references have been also reprised in Chapters 8 and 12, so it could be worth to mention them here. [Samuel Albani, Italy]	Rejected. Since Chapter 2 is exclusively focused on observations, we would leave the discussion largely related to attribution to Chapter 6.
23644	27	40	27	40	Change 'is' to 'are' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
26908	27	43	27	43	The reference Cherian et al., 2014 does not corroborate stattelite data. A reveltant recent study by Georgoulias et al. (2016) could be also cited (https://doi.org/10.5194/acp-16-13853-2016). [Prodomos Zanis, Greece]	Accepted. The references were repeated here by mistake and are removed. The Georgoulias reference is added.
17992	27	44	27	45	"Li et al. (2014) analysed AERONET stations until 2014 and documented a trend over Southern Asia." This is confusing because higher up in this paragraph it is stated that the AREONET network data cannot be corroborated with satellite data in Southern Asia. Therefore it would be useful to clarify what type of trend was identified? The trend is clarified in lines 52 - 53, therefore, could line 44 (p27) be deleted or moved to the paragraph below? [Gwenaelle GREMION, Canada]	Accepted. It is true that this is confusing. The key reason is in the different time periods. The sentence is omitted.
50234	27	50	27	51	what is the reference there? [Sophie SZOPA, France]	Accepted. The reversal date was corrected to "around 2011" and a reference was added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26910	27	51	27	52	The recent study by Sogacheva et al., 2018 (https://doi.org/10.5194/acp-18-11389-2018) might be cited where the reversal of the trends is discussed comprehensively. A declining trend of PM2.5 is also discussed in a very recent study by Ma et al. 2019 (https://doi.org/10.5194/acp-19-6861-2019). [Prodomos Zanis, Greece]	Accepted. These very useful references are now added.
23646	27	52	27	52	Delete ; after 'data' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
27936	27	53	27	53	Page 27, line 53. A slight decrease which is not significant, is a not significant trend. Calling it a decrease in the first place, while it is not significant, might be misleading. A "trend, which is not significant", gives a different impression from saying "there is no significant trend found". This is a formulation which is used throughout the whole report. I do not understand why this formulation is chosen. [roderik van de wal, Netherlands]	Rejected. We think it is useful to provide the full information.
23648	28	10	28	10	Change 'micrometer' to 'micrometre' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23650	28	12	28	12	Delete the negative sign. The text as written does not make sense (technically a negative decline is an increase!) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted (corrected)
23652	28	13	28	13	Similar to previous point, delete +, you have already said the trend is upwards so by definition it is an increase. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted (corrected)
31110	28	14	28	15	Since there is no statistically significant decrease in total AOD, does that mean there was a compensating increase in coarse-mode AOD? Or is comparing trends from MODIS+MISR to MISR alone best avoided? [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reason is that for AOD there is additional natural variability so the decreasing trend is not statistically significant. No compensation needed.
50236	28	48	28	48	"in the global mean" => "at the global scale" ? [Sophie SZOPA, France]	Editorial
30498	28	49	28	49	add "globally" after "trends". I think you are referring to global values/changes [Annalisa Cherchi, Italy]	Editorial
23654	28	50	28	50	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56214	28	53	29	42	Only mentioning land-use effects on albedo is misleading, since the net radiative forcing of land use change is more likely to be overall positive (given combined effect on CO2 and thermodynamic effects of evapotranspiration), rather than negative. For instance, Lejeune et al. (2018) have recently shown that historical deforestation has led to an increased intensity of warm days in the northern mid-latitudes. [Reference: Lejeune, Q., et al, 2018: "Historical deforestation locally increased the intensity of hot days in northern mid-latitudes". Nature Climate Change, https://doi.org/10.1038/s41558-018-0131-z .] [Sonia Seneviratne, Switzerland]	Rejected - the section already considers factors other than albedo; likewise, an assessment of extremes is the purview of Chapter 11.
32192	28	53	29		Section 2.2.7 has also been significantly improved when compared to the corresponding section in the previous internal draft (2.2.2.4), being now clearer that the impact of LULC changes is much more than a change in surface albedo. Nevertheless, it still focus more impacts on the surface radiation budget and not so much on energy (and water) balance. It is acknowledged that further work may still be needed in this respect. [Isabel Trigo, Portugal]	Taken into account - text revised.
41054	28	53	31	3	should add the content about Anthropogenic Heat Release(AHR), although IPCC AR5 did not include AHR. My comments: Anthropogenic heat is a direct, external energy source to the Earth-atmosphere system impacting the energy balance of the Earth's surface as a result of global energy consumption (Chen et al., 2019). It is an important factor for urban heat island and urban climate (IPCC, 2007). The global mean flux of AHR is 0.03 W m-2, while it is geographically concentrated and fundamentally correlates with economic activity (Chen et al., 2014). With the rapid development of global urbanization, the effect of AHR on urban regional climate will be enhanced. It can reach high enough level to impact regional climate (Feng et al., 2012; Bohnenstengel et al., 2014; Nie et al., 2017), even global climate (Zhang et al., 2013; Chen et al., 2014; Chen et al., 2019). AHR Research shows that AHR increase the global annual mean surface temperature and land surface temperature by 0.02 ± 0.01 K and 0.05 ± 0.02 K, respectively. The global climatic effect of AHR varies with season: with a stronger climatic effect in the boreal winter, leading to global mean land surface temperature increases by 0.10 ± 0.01 K (Chen et al., 2019). AHR can increase the surface temperature in the mid- and high latitudes over North Hemisphere in the boreal winter (Zhang et al., 2013; Chen et al., 2016; Chen et al., 2019), which is probably a missing forcing for the additional winter warming trends in observations (Zhang et al., 2013). Modeling results show a possible new mechanism of AHR effect on global climate: it affects global atmospheric circulation and changes	Rejected. Chapter 2 assesses large-scale (i.e., global and hemispheric) rather than urban- and regional-scale changes. Integrated global effect of AHR presented in Chen et al. (2019) is based on the CAM atmospheric model, thus not directly observed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53306	28	56	28	56	"equivalent" is not the right word here. I suggest simply deleting it. [Jan Fuglestedt, Norway]	Taken into account - combined with comment 24500.
32188	28				The structure of section 2.2 is now much more balanced, when compared to that of the internal draft. [Isabel Trigo, Portugal]	Noted. We thank the reviewer for this encouraging statement
6275	29	1	29	1	change forest ecosystems should be highlighted- deforestation [Mostafa Jafari, Iran]	Rejected - deforestation is already addressed in the next paragraph.
7334	29	4	29	4	Land use and Land cover is an important criteria. Regarding this, the Thermal conductivity of rocks influences melting of the ice mass at the contact with the rocks and are governed by their mineral constituents and the structural fabrics within the rocks. For instance, a study by Swain (2019) suggests that the melting of the ice mass in the Schirmacher Oasis have a range of influence varying from 2.77 ± 0.18 Wm-1K-1 by lamprophyre dykes to 6.1 ± 0.37 Wm-1K-1 by metapelites. Reference: SWAIN, A.K. (2019): Influence of thermal conductivity of rocks on Polar ice sheet recession near Schirmacher Oasis, East Antarctica. Journal of Geological Society of India, V. 93(4), pp. 455 – 465. DOI: 10.1007/s12594-019-1200-2 [Ashit Kumar Swain, India]	Rejected. The chapter assesses large-scale (i.e., global and hemispheric) rather than local- and regional-scale changes which are the purview of later chapters and where such an assessment should take place.
53310	29	5	29	21	See SRCLL ch 2 [Jan Fuglestedt, Norway]	Noted.
35562	29	5	29	21	This paragraph starts with the statement that more than half of the global land surface has been modified by human activities, and ends with the statement that about 75% of the ice-free terrestrial surface is impacted by human endeavours. These two statements are separated and cite different sources. To avoid an appearance of inconsistency either cite both and describe the reason for the difference (presumably the amount of surface covered by ice) or just cite the SRCLL statement. [Nathan Gillett, Canada]	Taken into account - text revised (first sentence of paragraph rewritten to avoid the appearance of inconsistency).
23656	29	7	29	7	Delete , after 'records' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
7336	29	7	29	10	reconstructions and scenarios augmented with rock types, pollen data, archeological records, and other historical information (Gaillard et al., 2018; Goldewijk et al., 2017; Koch et al., 2019; Krausmann et al., 2013; Swain, 2019) have improved understanding of long-term changes in land use from agricultural applications (such as cropland and pasture) and other historical drivers of variability (such as human disease and conflict) and ice melting. [Ashit Kumar Swain, India]	Accepted - text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48992	29	8	29	8	Add Dawson et al. (2018) in PAGES Magazine 26/1 among the references. It is the first "multi-continental" synthesis of pollen-based REVEALS estimates of plant cover (land cover) for the Northern Hemisphere (N of 40 degrees) and, therefore, should be mentioned in this report. We'll hopefully have other publications before december 2019 on pollen-based land-cover change over the Holocene for N America, Europe, and China, and the Northern Hemisphere with curves of openland changes over the Holocene for entire continents and the entire northern Hemisphere. I'll inform you in due time. [Marie-Jose Gaillard, Sweden]	Taken into account. The suggested literature was reviewed but deemed out of scope.
48218	29	11	29	13	Klein Goldewijk et al. 2017 present scenarios, not empirical data. Instead of "recent studies", I would write "recent scenarios". Once the paper by Stephens et al. 2018 in Science is published (soon) I would add it with a sentence starting by "A recent global synthesis of archaeological data suggest etc...." [Marie-Jose Gaillard, Sweden]	Accepted - text revised.
6277	29	12	29	12	deforestation should be given with more details in local, regional and global levels - it is important for modeling estimation and adaptation action plan [Mostafa Jafari, Iran]	Rejected - the chapter assesses large-scale (i.e., global and hemispheric) rather than local and regional-scale changes.
35266	29	12	29	12	The Neolithic concept is far to a transversal concept for prehistoric societies. Actually, it describes the hunting-gathering – agriculture transition in EUROPE. In other regions it's called as Formative, because it didn't involve exactly the same technological innovations or socio-cultural processes. That's why I would say "when agriculture advected in some regions" in order to reflect a more generalized process that led to land-use changes: i.e the agriculture onset. [eugenia gayo, Chile]	Noted.
37422	29	12			See comment 11. A stray "BP" appears here. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
48220	29	13	29	14	Delete in references "Marquer et al., 2017" (this reference is not relevant here but should be used elsewhere, see comment below), and add after "Kaplan et al 2017", "; Roberts et al. 2018" (paper in Scientific Reports) [Marie-Jose Gaillard, Sweden]	Taken into account - text revised.
37424	29	13			"these improvements" should be changed to "these studies" or something similar. As it stands, the text reads as if neolithic deforestation is regarded as an improvement. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.
26936	29	14	29	16	What is meant by "land cover use"? [Joachim Rock, Germany]	Taken into account - text revised.
23658	29	15	29	15	Insert , after '1980s' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37426	29	15			Should "1980s" be "1970s"? The first Landsat was launched in 1972, and the accuracy of what it produced was unprecedented at the time. I assume the reference to the 1980s means that the data from the earlier Landsats is so much poorer than data from later ones that they are not used in studies of land-use change. Perhaps the text could be tweaked so that this is made clear. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.
6279	29	18	29	19	more practical details (figures) is needed on regional deforestation and even forestation and or tree plantation [Mostafa Jafari, Iran]	Rejected - the chapter assesses large-scale (i.e., global and hemispheric) rather than local- and regional-scale changes.
6281	29	21	29	21	here more negative impact on forest is to be seen and positive impact on forest as REDD, REDD+ (Reducing Emissions from Deforestation and Forest Degradation) and or forest management (C&I for SFM) should be presented separately [Mostafa Jafari, Iran]	Rejected - beyond the mandate of WGI (refers to mitigation).
32190	29	21			"SRCCCL " is referred in the references' list as "IPCC (2019)". Please ensure consistency [Isabel Trigo, Portugal]	Editorial.
24560	29	23	29	36	This paragraph needs to focus more on how the land use has changed and how we know this (modelling? Satellites? NDVI?). How are albedo changes assessed? - directly from SW radiances or inferred from land use? This would help structure the discussion more rather than the lists of radiative forcings. Radiative forcings (if used) should distinguish whether they are SARF or ERF. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.
41412	29	29	29	30	The reference to Andrews et al. 2017 is duplicated [Lucas Bianchi, Argentina]	Editorial.
23660	29	31	29	31	Delete , after 'dust' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
48222	29	36	29	36	The study by Strandberg et al. (2014) (Clim Past) should be referred to here. Although it is not a global study, but a European one, it shows clearly the spatial and seasonal variability of land-use change forcing on regional climate due to biogeophysical effects leading to either increases of decreases of temperature and precip depending of the region and season. The latter is essential to understand why, at the global scale, land-use forcing is difficult to study, and more importantly, can appear to be relatively small (as a sum of regional negative and positive effects) while it is significant at the regional scale; this is of great importance to know when land-use forcing is used in mitigation (i.e. reforestation). Not mentioning this in this report would be a shame. [Marie-Jose Gaillard, Sweden]	Taken into account - text revised (sentence added on implications of this study).
24562	29	38	29	39	This needs to use effective radiative forcing and cite 7.3.4.1 for the value. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30500	29	39	29	39	the change is exactly the same as in AR5. No update from SRCCL? [Annalisa Cherchi, Italy]	Rejected - SRCCL does not provide an update that differs from AR5.
53308	29	39	29	39	"equivalent" is not the right word here. I suggest simply deleting it. [Jan Fuglested, Norway]	Taken into account - combined with comment 24500.
23662	29	39	29	39	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
19310	29	45	29	45	This section appears to be more about changes in radiative forcing than about changes in radiative budget. The text discusses the TOA radiative impact of changes in: TSI, well-mixed greenhouse gases, short-lived gases (e.g., ozone), aerosols, and land use and land cover changes. Changes in the radiation budget are also influenced by the response to the forcings (i.e., feedbacks). Consider revising the title to something like Changes in radiative forcing. [Norman Loeb, United States of America]	Accepted. Good point, also raised by another reviewer.
14288	29	45			This subsection could signpost to observed radiative budget changes in Chapter 7. Should this section be radiative forcing rather than energy budget which includes also response? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Radiative forcing indeed is better.
24564	29	49	29	49	This should be "ERF" not "RF". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted (corrected)
24566	30	1	30	2	This section should use values from chapter 7, not just the methods and details. These should be used everywhere rather than "where possible". [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The "where possible" with respect to adjustments is now omitted. The values are from Chapter 2 and converted into ERF via methods and details from Chapter 7, so this is kept as was before.
50238	30	2	30	2	"as described" => "described" [Sophie SZOPA, France]	Editorial
23664	30	2	30	2	Insert , after 'possible' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This is omitted in response to another comment
18000	30	3	30	3	Define "short-lived" [Gwenaelle GREMION, Canada]	Noted. The sentence is omitted in the revised version. For completeness: "short-lived" is defined consistently with Chapter 6 as species with lifetimes less than 2 decades.
24568	30	3	30	5	I can't see any of the Ch 6 values used in this section. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The reviewer is right and this sentence is now omitted.
24570	30	7	30	7	This should be "effective radiative forcing" [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Indeed, corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23666	30	8	30	8	Superscript for rate [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
29534	30	8	30	9	multi-decadal trends in TSI sound strange, there are low-frequency variations (Gleisberg- and de Vries cycle), but I would not call that a multi-decadal trend! [Katja Matthes, Germany]	Accepted (corrected)
35564	30	9	30	10	Based on figure 2.1, TSI variations over a solar cycle are closer to 1 W/m ² than the 0.1 W/m ² figure quoted here. Do the authors mean to refer to the radiative forcing variation over the 11-year solar cycle? [Nathan Gillett, Canada]	Accepted. Indeed. Now specified that this is in terms of ERF.
23668	30	10	30	10	Superscript for rate [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
14286	30	13			small to moderate eruptions thereafter although radiatively significant with ERF of -0.08 W/m ² relative to an unusually quiescent interlude 1998–2002 (Schmidt et al. JGR, https://doi.org/10.1029/2018JD028776) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Added the Schmidt reference but didn't use the minor clarification in the text due to space constraints.
23670	30	18	30	18	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
40972	30	18	30	19	This is only correct for CFCs and HCFCs, but not for HFCs, PFCs, and other radiatively important gases. [Johannes Laube, Germany]	Accepted (corrected)
18002	30	25	30	38	Figure 2.9: The rates of change are also very important (as discussed in the last line of this section). Perhaps a second pannel could be added to this figure showing the rates of change of the different quantities. The uncertainty bounds on the 'Total' line would also be useful to show. [Gwenaelle GREMION, Canada]	Taken into account. Some indication of the changing rates of changes has been included.
24572	30	31	30	32	The aerosol forcings "include" ARI and ACI rather than being "the sum of". i.e. they are calculated as a total effect rather than being individually calculated and summed. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Good point, amended.
7754	30	41	30	43	Please revise to include the fact that IR absorption and positive feedback from short-lived (7-10 days) tropospheric water vapour makes it far more significant than surface ozone. Previous IPCC assessment reports openly declare the importance of water vapour. For example: "Water vapour is the most important greenhouse gas" and "As the largest contributor to the natural greenhouse effect, water vapour plays an essential role in the Earth's climate." (IPCC AR5: https://wg1.ipcc.ch/publications/wg1-ar4/faq/wg1_faq-1.3.html) [Forrest Mims, United States of America]	Taken into account. See response to comment 7748.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
53312	30	42	30	42	I suggest changing "most important" to "the component with the largest RF" or something like that. [Jan Fuglestedt, Norway]	Accepted. Changed as suggested
24574	30	47	30	47	What does this -0.35W/m2 refer to? Where does the number come from? It doesn't seem to come from chapter 6. Chapter 7 has an aerosol ERF of -1.1 W/m2 to 2017 not -0.35. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Mistake corrected.
35566	30	47	30	48	The chapter concluded that AOD has shown decreasing trends since 2001 with high confidence. But here the chapter concludes that the associated forcing has plateaued, rather than declined in recent decades. The reason for this difference should be briefly mentioned with a reference to Chapter 7 if discussed there - is it connected with changes in the distribution of aerosols? [Nathan Gillett, Canada]	Taken into account. The Chapter 7 ERF time series was a preliminary one; the updated one make use of the AOD assessment of Chapter 2.
48224	30	51	39	52	The land-use forcing is discussed here for recent times. It would be important to mention here that the equivalent of the anthropogenic deforestation of Europe between 6k and 0.2k (i.e. pre-industrial, before 1750) also has a significant biogeophysical effect on climate (albedo AND evapotranspiration effect depending on the region and season) (Strandberg et al., 2014). Although this study is regional (Europe, regional climate modelling), it is unique in its kind and should be mentioned here, or elsewhere in Chapter 2. [Marie-Jose Gaillard, Sweden]	Accepted. We refer to the relevant section here.
23672	30	53	30	53	Superscript for rate [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
40470	31	8	32	2	Perhaps the table entry on tropospheric circulation should also link to Chapter 8, since they consider monsoons, Walker & Hadley circulations and the MJO. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accept, chapters have again been invited to identify which entries they should be associated with.
30502	31	10	34	10	I suggest to revise the form of the box: it is written as to justify the choices, while it is supposed to state/summarize what have been decided to show in terms of "large-scale indicators" [Annalisa Cherchi, Italy]	Taken into account in the redrafting of the box.
50240	31	14	31	21	should be shortened by keeping only the first and last sentences of the paragraph [Sophie SZOPA, France]	Taken into account. This paragraph has been reconsidered. It is necessary to consider whether the box should stand alone or assume knowledge of overall structure of report.
53314	31	14	31	21	the box is useful, but I don't think this overview of AR6 WGI structure is needed here. Important to state earlier in the report. [Jan Fuglestedt, Norway]	Taken into account. See comment 50240
43338	31	14	31	21	Is this paragraph really necessary? [James Renwick, New Zealand]	Taken into account. See comment 50240

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43340	31	24	31	34	This paragraph is overly complex. Start with "Chapters 2, 3, and 4 focus on large scales while later chapters consider process-relevant and regional scales. The climate system involves process interactions from the micro- to the global-scale. Any threshold for defining "large-scale" is arbitrary." [James Renwick, New Zealand]	Taken into account. The paragraph has been edited accordingly.
30504	31	26	31	26	remove "that we define" and add "defined above" after "for AR6" [Annalisa Cherchi, Italy]	Taken into account. See comment 43340
30506	31	31	31	31	remove the sentence "For example, the El Nino phenomenon spans the tropical pacific basin" [Annalisa Cherchi, Italy]	Taken into account. See comment 43340
30508	31	31	31	31	add "these chapters" after "Thus, within" [Annalisa Cherchi, Italy]	Taken into account. See comment 43340
23674	31	31	31	31	Change Nino to Niño [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. See comment 43340
30510	31	32	31	32	remove "Chapters 2, 3 and 4 we define" [Annalisa Cherchi, Italy]	Taken into account. See comment 43340
30512	31	32	31	32	change "to constitute" with "includes" [Annalisa Cherchi, Italy]	Taken into account. See comment 43340
30514	31	33	31	34	remove whole sentence starting from "Although the assessment ..." [Annalisa Cherchi, Italy]	Taken into account. See comment 43340
32196	31	33	34		"Although the assessment shall be at these scales, where appropriate global maps etc. shall be shown and discussed". The word "although" suggest some sort of contradiction and I see none between the scales described above, the assessment of patterns/ changes at those scales and the discussion of global maps (and "etc"?) as appropriate. I like the description of the various scales and their inter-links presented in the paragraph, but the last sentence(s) need to be rephrased. [Isabel Trigo, Portugal]	Taken into account. See comments 18008 and 43340
18004	31	36	31	37	Consider: Determining a set of key indicators. "key indicator" is an abstract term that would benefit from clear and frequent explanations throughout the report. Please add a brief definition of a "key indicator" here. [Gwenaelle GREMION, Canada]	Taken into account. See comment 18008
18006	31	37	31	37	Repetition of information that Chapters 2, 3 and 4 aim to define key indicators on line 42 [Gwenaelle GREMION, Canada]	Taken into account. See comment 18008

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18008	31	37	31	44	The structure of this paragraph is difficult to follow and repeats information. Consider: (1) defining a key indicator. (2) "A limited set of observable key indicators across the atmospheric, oceanic, cryospheric and biospheric domains have been selected." (3) "Taken together, these key indicators provide a comprehensive assessment of changes within the climate system as a whole." (4) "The selected key indicators would be expected to have changed and continue to change in a coherent and consistent manner." (5) "Chapters 2, 3 and 4 shall each consider a subset of these key indicators, while avoiding overt overlap with later chapters." (6) The selected key indicators are summarized in Cross-Chapter Box 2.1, Table 1" [Gwenaelle GREMION, Canada]	Taken into account. The paragraph has been edited accordingly in the SOD.
30516	31	38	31	38	please rewrite "but avoid overt" as meaning is unclear [Annalisa Cherchi, Italy]	Taken into account. See comment 18008
50242	31	38	31	38	remove "overt" [Sophie SZOPA, France]	Taken into account. See comment 18008
53316	31	38	31	38	You may change "must enable" to "enables". (If it is a key variable it does so). [Jan Fuglestedt, Norway]	Taken into account. See comment 18008
30518	31	41	31	43	meaning not clear, I suggest to rewrite [Annalisa Cherchi, Italy]	Taken into account. See comment 18008
37974	31	48	31	52	You missed chapter 8 in table 1, Cross chapter Box 2.1, line Salinity [Jean baptiste SALLEE, France]	Accepted, we have added Chapter 8 with respect to salinity
30520	31	48	31	53	in the table MJO is included within "tropospheric circulation" phenomena but it is not assessed in sect 2.3.1.3. According to what decided on LAM2, MJO was not supposed to be part of ch 2 and it was decided to assess it fully in ch 8. If this is confirmed it has to be included and mentioned in the table. [Annalisa Cherchi, Italy]	Noted. MJO has been removed.
56216	31	48	32	2	Mention in a footnote to this table that global assessments regarding observed, attributed, and projected changes in weather and climate extremes are provided in Chapter 11. [Sonia Seneviratne, Switzerland]	Taken into account. We mention this explicitly at the end of the Introduction. We do not see a need to repeat here and complicate structure by introducing footnotes.
37428	31	53	32	1	In Table 1 of box 2.1, runoff should not appear under the heading "Atmosphere". It needs a separate heading "Land". It is also unclear in this table whether "surface humidity" refers to near surface soil moisture or near surface air humidity. If appears from later text to be soil moisture, so that too should be listed under the heading "Land". See also comment 70. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We discussed at length the issue of whether to include land as its own category and decided against this. We have clarified this in edits to the box text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23676	31	53	32	1	If the table is to be carried over two pages please copy the column headings into the top of the columns on the second page [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This should not occur in final layout
26830	32	0			Cross-chapter box 2.1 Table 1. The "Tropospheric circulation" is also included in chapter 8. It would be helpful to have this listed here to facilitate finding all the extra-tropical cyclone discussions. [Ruth McDonald, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
45758	32	1	32	1	What characteristic of permafrost in table. Extent? [Katja Mintenbeck, Germany]	Taken into account. This level of detail is likely too much for this box.
45760	32	1	32	1	Change to Seasonal cycle of CO2 to be clear what is meant and to be consistent with title of section 2.3.4.1 [Katja Mintenbeck, Germany]	Section 2.3.4 has been substantively redrafted leading to wholesale edits required here.
45762	32	1	32	1	phenology of what? Do you mean ocean colour (satellite) as a proxy for NNP and comparison for terrestrial growing season? [Katja Mintenbeck, Germany]	Editorial. Redrafted for clarity.
45764	32	1	32	1	land community assemblages - not assessed in chp 5, this is a WGII assessment issue [Katja Mintenbeck, Germany]	Taken into account. We have tried to adjust text accordingly.
45766	32	1	32	1	growing season onset and length - is this ocean or land, from satellite observations of greening? [Katja Mintenbeck, Germany]	Taken into account. See comment 45760
30522	32	6	32	6	I suppose that "global surface temperature" is "essential to include" as atmospheric indicator. Though the sentence is not well written [Annalisa Cherchi, Italy]	Taken into account. Edited for clarity.
30524	32	8	32	9	remove the sentence starting with "An overall...." [Annalisa Cherchi, Italy]	Editorial. Text redrafted
37430	32	11	32	12	Further to comment 85, soil moisture and runoff should not appear here under the heading "Atmospheric indicators". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We are including them as key indicators of the hydrological cycle which is considered under atmosphere. We discussed at some length comments around land as a category and decided not to pursue that option as we felt it would hinder and not help. Also issues associated with consistency with Ch. 3-4 were considered as arguments.
30526	32	12	32	12	add "with the rest of the processes and details of changes assessed in Ch 8" [Annalisa Cherchi, Italy]	Rejected. The prior sentence has already done this.
17922	32	14	32	14	While Chapter 1 uses, arguably, less typical names for major Jet Streams, this Ch. 2 uses more typical names, i.e., sub-tropical and polar jets. [Branko Grisogono, Croatia]	Noted. We are retaining current naming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17924	32	14	32	14	Nevertheless, in the line just mentioned, there is missing the connecting word 'and' between those Jet Streams. [Branko Grisogono, Croatia]	Editorial
35568	32				Cross-chapter box 2.1, Table 1: Solid fill should be added for the Chapter 3 cell against 'Terrestrial vegetation (global greening)' since Ch 3 assess LAI changes. Solid fill should be added for the Chapter 4 cell against 'Temperature/ Ocean Heat Content', since Chapter 4 assesses this (Section 4.5.2.1). [Nathan Gillett, Canada]	This comment has been deferred to the FGD
32932	33	2			Expert judgement is an option to estimate ice sheet contributions, so I wouldn't classify it as is the 'best' [Aimee Slangen, Netherlands]	Taken into account. Text revised accordingly.
18010	33	6	33	8	Snow cover is described out of order compared to the table. [Gwenaelle GREMION, Canada]	Taken into account. We have tried to better assure ensure narrative continuity.
23678	33	7	33	7	Change 'knock on' to 'knock-on' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35570	33	10			Replace 'were the permafrost to thaw' with 'as the permafrost thaws', since permafrost is already thawing as assessed elsewhere in the chapter. [Nathan Gillett, Canada]	Editorial. Accounted for in more general edits.
37432	33	14			The AR5 glossary includes the lithosphere as part of the climate system. Although it does not take up anywhere near as much of the excess energy from GHG increases as the ocean does, it is nevertheless a very large energy store. So the text here could be amended a little. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is too nuanced for this box which is not expected to provide such details. Comment also pertains more to the box in chapter 9. Note that the indicators related to land surface (lithosphere interface with the atmosphere (over the continents)) are included. Under Ocean lithosphere has very minor influence on ocean changes and is barely observed.
45768	33	21	33	27	suggest focusing this on satellite observations of ocean colour and land surface greening only [Katja Mintenbeck, Germany]	Rejected. Assessment charge given to chapter is to look more holistically at biospheric indicators than suggested here.
18012	33	22	33	27	Biospheric indicators are not described in the same order that are in the table. [Gwenaelle GREMION, Canada]	Taken into account. See comment 18010
18014	33	22	33	27	In table1: what's the different between "Growing season onset and length" and "Atmospheric Co2 inducing changes in seasonal cycles of growth rates"? It is not described in the text. [Gwenaelle GREMION, Canada]	Taken into account. We have integrated these sections in the revised SOD and text in table 2.2 has accordingly been amended.
18016	33	22	33	27	Not all biospheric indicators listed in the table are described in the text: "Ocean CO2/O2", "Terrestrial vegetation", "Ocean phenology" are not described. [Gwenaelle GREMION, Canada]	Taken into account. We have substantively edited this text section to reflect the revised sections 2.3.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23680	33	32	33	32	Change 'chapters' to 'Chapters' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
30528	33	39	33	43	in the table you should consider that ch 8 assesses the fingerprints of these modes of variability on the changes in the hydrological cycle [Annalisa Cherchi, Italy]	Taken into account. We have asked other chapters to ensure that they are correctly identified as to where they perform additional assessment in the two tables.
23682	33	43	33	43	Change Nino to Niño in first line of table [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35572	33				Cross-chapter box 2.1, Table 2: Add solid fill for Chapter 3 cell against 'Atlantic Meridional and Zonal Modes' since these are assessed in Chapter 3. [Nathan Gillett, Canada]	Accepted.
32318	34	17	34	17	Air-sea heat fluxes were considered in AR5 Chapter3 but are not covered here despite being an important component of the climate system. The conclusion in AR5 Ch.3 was that the 'detection of a change in air-sea heat fluxes responsible for the long-term ocean warming remains beyond the ability of currently available surface flux data sets.' Does this conclusion still hold in AR6? [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Rejected/noted. Air-sea fluxes are covered in Chapter 9 where the ability of surface flux time series to detect the changes in the ocean heat content is considered explicitly.
32320	34	17	34	17	Several studies since AR5 suggest that global mean net heat flux can now be determined at an accuracy sufficient to consider variations in heat uptake by the oceans (Liang and Yu, 2016; Liu et al., 2017; Ponte and Piecuch, 2018). Can the panel please assess these papers and provide an informed assessment regarding their significance/accuracy? Note this is not a case of cite my own work as I am not an author on these publications. Liang, X., & Yu, L. (2016). Variations of the global net air-sea heat flux during the "hiatusperiod" (2001–10). <i>Journal of Climate</i> , 29(10), 3647–3660. https://doi.org/10.1175/JCLI-D-15-0626.1 Liu, C., R. P. Allan, M. Mayer, P. Hyder, N. G. Loeb, C. D. Roberts, M. Valdivieso, J. M. Edwards, and P.-L. Vidale (2017), Evaluation of satellite and reanalysis based global net surface energy flux and uncertainty estimates, <i>J. Geophys. Res. Atmos.</i> , 122, 6250–6272, doi:10.1002/2017JD026616 Ponte and C. G. Piecuch. (2018) Mechanisms Controlling Global Mean Sea Surface Temperature Determined From a State Estimate. <i>Geophysical Research Letters</i> 45:7, 3221-3227. [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Air-sea fluxes are covered in Chapter 9, see also comment #32318
54630	34	17	43	38	all that section is already in chapter 1 [Ruth Cerezo, Mexico]	Rejected. The reviewer statement is not correct.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18018	34	19	34	19	"Holocene" is not defined in the chapter or in Table 2.1. [Gwenaelle GREMION, Canada]	Taken into account - Holocene is not a "reference period" and therefore might not fit in Table 2.1. However, it is now defined where first presented, as well as in the Glossary.
23684	34	19	34	19	This heading is confusing/misleading. 'Deep past' or 'Deep time' are terms used by geologists to refer to vast times back in the history of the Earth (hundreds of millions to billions of years). This section really only refers back over the last 56 million years (as per Table 2.1), with a focus on the last few million. For clarity and accuracy I would suggest modifying the title to 'the past 60 million years' or similar. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Reject - Agreed that "deep past" sounds similar to "deep time", which is generally much longer than "just" the Cenozoic (the focus here). To avoid confusion, added "Cenozoic Era" to the heading. This is appropriate for the general IPCC-report audience. Note also that this definition is essentially the same as the US National Research Council's, which published the book entitled, "Understanding Earth's Deep Past". https://doi.org/10.17226/13111 .
18020	34	19	35	54	This section compares temperatures alternatively to the Holocene mean, Holocene average, pre-industrial, and "modern". It would be easier to understand and make comparisons if it were more consistent. Furthermore, "modern" era is not defined in table 2.1 [Gwenaelle GREMION, Canada]	Noted - The literature is inconsistent. Attempting to normalize reported values to some common reference is hazardous and would lead to increased uncertainty.
50244	34	21	34	21	remind that all the acronym for periods are in the Table 2.1 by moving the first sentence of second paragraph at the beginning of section 2.3.1.1.1 [Sophie SZOPA, France]	Accepted - revise as suggested
31986	34	21	34	21	I assume that it is Global Mena Surface Temperatures (GMST). [Marie-France Loutre, Switzerland]	Accepted - Although AR5 considered more than just GMST, this paragraph focuses on GMST.
57824	34	21		44	High confidence in the surface temperature during the six studied intervals. Therefore an integrated Temperature Anomaly should be drawn to explained in detail the surface temperature between the January 2-1406 (-0.55°C) and cooler then 0.97°C of April 2019. and from October 2011(0.66°C) 0.33°C) cooler than April 2019. Temperature record.com. [Abiodun Adegoke, Nigeria]	Taken into account - Temperature differences between different reference intervals are stated in CH2.
14478	34	27	34	28	"...and likely to the maximum rate of global warming during the subsequent deglaciation", if we consider the rapid warming at the end of Younger Drays, the increasing rate of temperature would be likely to be larger 1° to 1.5°C per thousand years. (CUG, Guoyu Ren) [Guoyu Ren, China]	Noted - This paragraph refers to conclusions by AR5 only.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32640	34	30	34	31	This is a really important point to make--that the climate can change by large amounts. And the key point to make in saying this is that these changes are not just random (as science contrarians like to imply), but that they are caused by changes in particular factors. Were the climate stable in the face of such changes, then one might not be so concerned about the potential consequences of the rising CO2 concentration. However, the large changes in climate in the past were caused by changes in factors roughly comparable to the types of changes that human activities are inducing in radiative forcing. Thus, I'd urge finding some way to really make this point clearly and strongly. [Michael MacCracken, United States of America]	Rejected - The sensitivity of climate to various levels of radiative forcing is outside the scope of CH2 and is discussed elsewhere in AR6.
31988	34	36	34	36	"with the greatest increases at high latitudes". Strange to read this sentence on high latitudes while all the section deals only with GMST [Marie-France Loutre, Switzerland]	Accepted - Deleted as suggested
18026	34	37	34	37	The adjective "recent" may not be applicable for the year 2013 when this chapter is published in 2021 (also used on line 39 for the year 2015) [Gwenaelle GREMION, Canada]	Accepted - Omitted the word "recent", as suggested
37434	34	37	34	43	The term GMST as applied to the temperature datasets such as HadCRUT4 refers in fact to monthly anomalies in temperature. While the monthly anomalies in SST are quite a good approximation to monthly anomalies in marine air temperature (MAT) when averaged globally, the average difference between SST and MAT is much larger than the difference in their averaged anomalies. The conventional temperature datasets for today do not provide absolute values, though reanalyses do. In reanalyses, GSAT is typically close to 1°C colder than GMST (defined as air temperature over land and sea-ice) in boreal winter; differences in boreal summer are a little smaller. If numbers are going to be quoted as estimates for absolute values of GMST rather than as estimates of GSAT, as here in the FOD, then the reader needs to be told somewhere what the present-day absolute value of GMST is. Presenting the numbers as estimates of GSAT rather than GMST would be simpler. See comments 5, 6 and 7 relating to the entire report. This comment also applies to references to GMST in subsequent paleo sections. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - New cross chapter box on difference between GMST and GMAT.
50246	34	38	34	38	what is SE? [Sophie SZOPA, France]	Editorial - SE = standard error
18024	34	39	34	39	The MPWP is shown in figure 2.10b, not 2.10.a. In fact, Figure 2.10a should be referenced in the previous paragraph. [Gwenaelle GREMION, Canada]	Noted. Figures redrafted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18022	34	47	34	47	It's hard to follow the discussion of this figure because there's no indication of which time period is in which part of the figure. My suggestion would be to add them in the x-axis and/or with subtle background shading. It is especially difficult when the definitions of each time period in Table 2.1 are not in the same units as the relevant panel of Figure 2.10 (LIG, for example). [Gwenaelle GREMION, Canada]	Taken into account -Figure revamped for SOD
7838	35	6	35	6	DTR in N???? [zhiyan zuo, China]	Unclear what the reviewer is referring to here, but taken into account -- Figure no longer used
23686	35	19	35	19	Change text to 'Pre-Industrial times' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - CH2 wording conforms to AR6 convention
18030	35	22	35	25	This sentence is hard to read and "...by about a factor of approximately..." is redundant. Something like "New reconstructions (...) indicate that polar regions warmed approximately twice as much as the global mean" could read easier. [Gwenaelle GREMION, Canada]	Accepted - change wording as suggested
49342	35	25	35	25	Suggest adding "LIG proxy data indicate much larger local warming of air temperatures in some parts of the Arctic, including over the Greenland ice sheet." (e.g., CAPE 2006 doi:10.1016/j.quascirev.2006.01.033; McFarlin et al. 2018 www.pnas.org/cgi/doi/10.1073/pnas.1720420115) This is important because the text, as written, seems to imply a relatively small magnitude of LIG warming over Greenland that might not adequately explain LIG sea level [Yarrow Axford, United States of America]	Accepted - Although Greenland is a small region, the evidence for warmth during the LIA is remarkable and important to contrast and relevant to the ice-sheet extent which is assessed in this chapter.
18032	35	27	35	27	The LGM period is shown in figure 2.10c, not in 2.10b [Gwenaelle GREMION, Canada]	Accepted - Revise as suggested
18034	35	27	35	27	The text compares ocean surface vs. land surface during the LGM and references Figure 2.10b. However, in that figure, SST is shown from 2Ma and it's impossible to see what happens during the LGM (21ka to 19ka) because it's a very short timeframe relatively to the whole plot. [Gwenaelle GREMION, Canada]	Taken into account. Figures have been completely revised
33106	35	27	35	37	The LGM temperature estimate needs to be sorted out. I think the Snyder (2016) paper LGM estimate is an outlier compared to the others. I don't think it uses all of the available data for the LGM estimate since it is focussed on longer time scales. The other estimates (including the Annan and Hargreaves 2013 estimate mentioned above) seem much more in line with the extensive LGM sea surface temperature data sets. [Jean Lynch-Stieglitz, United States of America]	Taken into account - New LGM reconstructions are expected in time for consideration by AR6. Also, Snyder's reconstruction is for GMST, not SST, which explains why it is higher than the reviewer expects.
23688	35	28	35	28	Insert 'the' after 'on' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8644	35	28	35	28	I appreciate the desire to cite only the newest and shiniest research, but Snyder only used a handful of cores mostly from ocean margins (nothing at all on land) and it's hard to see how this approach can be considered an improvement specifically in relation to the LGM temperature compared to the analysis of Annan and Hargreaves (2013) who used about 400 data points widely distributed over land and ocean. Note also that the Shakun et al reconstruction in Fig 2.10c (also based on a small sample of poorly-distributed cores) is completely inconsistent with the Snyder reconstruction in 2.10b. If you want continuous time series, you could do worse than rescale both of them to 4C at the LGM. [James Annan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - New LGM reconstructions are expected in time for consideration by AR6. Also, Annan and Hargreaves (2013) is a blend of model output and proxy data. When available, CH2 default is to emphasize observations over models.
14998	35	29	35	29	The Snyder (2016) publication has received a lot of criticism for it's approach especially because it did not include all available data sets, and the compilation calculates a different set of temperature anomalies than compared to the LGM-Holocene transition (which was calculated using a much more spatially and temporally comprehensive data set than Snyder - see Shakun et al. 2012 at https://www.nature.com/articles/nature10915). Here it may be prudent to also include the Shakun 2012 compilation for comparison. [Erin McClymont, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - New LGM reconstructions are expected in time for consideration by AR6. Also, Shakun's reconstruction is illustrated in Fig. 2.10.
35574	35	33	35	35	It doesn't make sense to say LGM variability on century scale was about four times higher than during the Holocene 'especially at high latitudes' - it can't be especially 'about four times higher'. Either delete 'about four' or replace 'especially at' with 'and even more at high latitudes' or similar if the assessed evidence supports this. [Nathan Gillett, Canada]	Editorial
23690	35	34	35	34	Insert 'the' after 'on' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
48786	35	37	35	37	would be good to remind the range 1 to 1.5 per thousand years mentioned earlier (p34 line 28), to quantify what "rapid" here means ! [Sylvie JOUSSAUME, France]	Accepted - restate AR5 findings here.
23692	35	39	35	40	I suggest changing 'deep past' to 'geological past', or, better, quantifying [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in response to similar earlier comment. Also, added "within the Cenozoic" to be more specific about the period considered.
27938	35	41	35	42	Page 35, line 41-42. Why does magnitude of temperature change increase? [roderik van de wal, Netherlands]	Noted - no revision suggested. Also, causes of climate change are outside of the scope of CH2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23694	35	43	35	43	I suggest changing 'deep past' to 'geological past', or, better, quantifying [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in response to similar earlier comment. Also, added "within the Cenozoic" to be more specific about the period considered.
8540	35	49	35	54	Numbers here do not exactly agree with numbers earlier in the section. [Robert Kopp, United States of America]	Taken into account - final values are summary statements representing multiple values as reported individually in previous paragraphs.
23696	35	50	35	50	Change to Pre-Industrial for consistency elsewhere in the text and insert 'times' after 'Industrial' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
49344	35	50	35	50	"Within the Cenozoic, GMST relative to pre-industrial ranged..." (Suggest adding the context of the Cenozoic, because there are deeper-time pre-Cenozoic warm and cold extremes that are not mentioned here.) [Yarrow Axford, United States of America]	Accepted - revised wording, as suggested.
18028	35	52	35	54	There is not any reference in the LIG paragraph (Page 35, lines 16-25) to where the values of "1 ± 0.5°C" and "2°C" were obtained. So that their addition seems arbitrary [Gwenaelle GREMION, Canada]	Taken into account - final values are summary statements representing multiple values as reported individually in previous paragraphs.
13166	36	2	37	29	What about the Southern Hemisphere? The Southern Hemisphere is barely mentioned. There should at least be one or two sentences indicating what we currently know about post-glacial changes in the Southern Hemisphere and also what additional data is needed to understand this better. [Nora Richter, United States of America]	Accepted - Added statement about sparse evidence from the SH as reported in AR5.
13168	36	2	37	29	This section is very focused on the North Atlantic region, some mention should also be made about the Pacific. [Nora Richter, United States of America]	Taken into account - Actually the Atlantic Ocean is not mentioned in this section. Also, added a statement that this section focuses on the largest scale for which data are available. No ocean-basin-scale assessment is included.
35268	36	3	36	5	in the phrase "the period 1983-2012" the suffix CE should be indicated. Specially, considering that in the previous sentence authors define what CE means. [eugenia gayo, Chile]	Rejected - We assume that the reader will recognize calendar years as they are.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11574	36	6	36	8	<p>Authors write: "...the warm multi decadal periods prior to the 20th Century were not as synchronous across regions as was the warming since the mid-20th century (high confidence)." A high confidence statement is being made about the MCA, even though the opposite is being claimed in Chapter 1 (page 69, lines 35-37: "Although Lüning and Vahrenholt (2017) suggest a much longer context for defining pre-industrial, estimates of natural radiative forcings and global temperature are too uncertain to allow a reliable estimate for longer periods." How can a high confidence statement being made under those circumstances. In fact, in reality global and hemispheric temperature reconstructions are still unstable and change significantly from version to version. New evidence now indicates that the MCA was in fact more synchronous than thought. A series of regional syntheses for the MCA documents robust evidence that the MCA was predominantly warm also outside the North Atlantic region. See Lüning et al. 2017, 2019a&b for MCA temperature syntheses for Africa, South America and Oceania. Lüning et al. (2019a): The Medieval Climate Anomaly in South America. Quaternary International, 508: 70-87. doi: 10.1016/j.quaint.2018.10.041; Lüning et al. (2017): Warming and cooling: The Medieval Climate Anomaly in Africa and Arabia. Paleoceanography 32 (11): 1219-1235, doi: 10.1002/2017PA003237; Lüning et al. (2019b): The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012. [Sebastian Luening, Portugal]</p>	<p>Reject - This section reports on AR5 findings as they appear in the report. Also, the topic here is 20th century warming and how it compares with the MCA, not on the uniformity of the MCA. Finally, while the MCA was likely warm from place to place, a new study by Neukum et al. (2019, https://doi.org/10.1038/s41586-019-1401-2), which is based on the most compressive and recent data compilation available reaffirms the AR5 finding that 20th century warming has been more uniform/homogenous than previous warming or cooling events.</p>
23698	36	8	36	8	<p>Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Editorial</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11576	36	11	36	14	<p>Authors write: “SR1.5 highlighted the Holocene thermal maximum, the period between around 10 and 5 ka when GMST is inferred to have been up to 1°C higher than pre-industrial, but which occurred at different places at different times and therefore overestimates the GMST for any single period.” The SR1.5 mostly referred to a paper by Marcott et al. 2013 which is predominantly based on sea surface temperatures. Only about 10% of the proxies used in the paper originate from land sites. The warming of the HTM in this paper appears significantly underestimated because (1) the oceans warm slower and less intense than land, and (2) switch of currents leading to a colder HTM were misinterpreted as a cooling. The results of Marcott et al. 2013 therefore have to be treated with caution. It is very likely that the HTM on a global scale was much warmer, when reconstructed using a more balanced mix of land and oceanic sites. In many parts of the Arctic, summer temperatures were up to 4°C warmer than today. The Greenland ice sheet was smaller than today and many glaciers in the Alps were smaller than today or have disappeared altogether. [Sebastian Luening, Portugal]</p>	<p>Noted - The reviewer cites well-documented evidence for relative warmth from the Arctic and from mountains during the middle Holocene. The indicators largely reflect summer warming (driven by orbital changes). Much less is known about the global annual mean temperature (the focus on this section). Contrary to reviewer's comment, recent work (e.g. Marsicek et al., 2018; doi:10.1038/nature25464) shows much less mean annual warming over North America and Europe during the mid Holocene compared with Marcott et al. (2013). In addition a new GMST reconstruction is expected for consideration by AR6. Also, this section reports on AR5 findings as they appear in the report.</p>
35576	36	12	36	14	<p>The text here is confusing. The statement 'which occurred at different places and different times and therefore overestimates the GMST for any single period' seems to be a criticism of the assessment in the first part of the sentence that 'GMST is inferred to have been up to 1C higher than pre-industrial'. It isn't clear where this statement comes from - is it the SR 1.5, is it from the literature, or is it the authors' own assessment? If the authors are critiquing the assessment in the SR1.5 they should make this clearer, and explain and cite the new evidence which calls into question the SR1.5 assessment. Or if the 1C higher than pre-industrial is just the authors' own assessment, then just revise the assessment of the warming in GMST to take account of the fact that the warming occurred at different times in different places. If they are critiquing an assessment made in the literature, then cite the relevant studies. [Nathan Gillett, Canada]</p>	<p>Accepted - clarified stating that 1.5 report acknowledged that the HTM was different ages, and that this chapter focuses on a narrower time slice. If the HTM temperature cited by the 1.5 report could be reliably adjusted to account for the fact that the warming took place at different times, then I suspect the authors of that report would have done so.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11578	36	14	36	22	The choice of 6000 years for the HTM is unfortunate because the majority of the HTM warming occurs well before this time, as an ongoing global re-evaluation of the HTM indicates. HTM warming mostly occurred during two phases. In part of the sites the HTM warming occurred 11,000-9000 years BP, whilst in another part the warming happened 9000-6000 years BP. The chosen time of 6000 years BP is not even part of the first group of HTM sites, whilst it already starts cooling in the second group. For references click on sites colour-coded in red on this map: http://t1p.de/htm . The findings of this study will be published shortly and it would be a shame if the AR6 report was already out-of-date with respect to this subject shortly after it comes out. [Sebastian Luening, Portugal]	Noted - As stated, CH2 does not use the HTM as a reference period. 6 ka was chosen as a reference period because, like all other paleo reference periods, it is the focus of modelling studies.
50248	36	15	36	15	what is the "PMIP time slice"? [Sophie SZOPA, France]	Accepted - clarified as "paleoclimate modelling target"
23700	36	20	36	20	Change to Pre-Industrial for consistency elsewhere in the text and insert 'times' after 'Industrial' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23702	36	23	36	23	Insert , after) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18036	36	26	36	27	"..., with greater warming at 6 ka in the northern high latitude than to the south" Please clarify if "south" means the Southern Hemisphere or just lower latitudes in the Northern Hemisphere [Gwenaelle GREMION, Canada]	Editorial.
23704	36	27	36	27	This is vague, please give latitude values (or similar) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
11580	36	29	36	30	Authors write: "In aggregate, it shows the period between 8 and 5 ka was generally warmer in the Northern Hemisphere by about 0.5°C relative to pre-industrial, although the warming was likely greater regionally, especially when averaged over shorter periods." The HTM was surely warmer than only half a degree above pre-industrial. The study by Marcott et al. 2013 is predominantly based on sea surface temperatures. Only about 10% of the proxies used in the paper originate from land sites. The warming of the HTM in this paper appears significantly underestimated because (1) the oceans warm slower and less intense than land, and (2) switch of currents leading to a colder HTM were misinterpreted as a cooling. The results of Marcott et al. 2013 therefore have to be treated with caution. A more complete overview of HTM studies can be found on the following map. Click on sites colour-coded in red on this map: http://t1p.de/htm . [Sebastian Luening, Portugal]	Taken into account in response to similar earlier comment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42406	36	29	36	30	Highlight the paleo-relevance of the studies. "Numerous new site- and regional-level paleo studies generally agree...". Or "Numerous new site- and regional-level studies generally agree with the paleo/historical trends summarized in AR5". [Elizabeth Fard, United States of America]	Accepted - add "paleoclimate" as suggested
27940	36	32	36	32	Page 36, line 32. Higher than what? [roderik van de wal, Netherlands]	Taken into account - Higher than during the warmest millennium of the Holocene, as stated.
35578	36	37	36	38	The focus here should be on assessing observed changes - I don't think description of PMIP is relevant here (though paying particular attention to the PMIP period to inform later chapters makes sense). [Nathan Gillett, Canada]	Rejected - The focus is on observed changes, as suggested by reviewer. The reference to PMIP helps explain why observations during this period is of widespread interest in the paleo community, including in subsequent chapters of AR6.
42898	36	38	36	38	If it is oversimplified, explain why follow AR5, since we have an advanced understanding of MCA and LIA since then? [Michael Evans, United States of America]	Accepted - explained that two-fold period is useful construct and provides a target for data-model comparison.
35580	36	38	36	41	The text 'Despite its well-documented oversimplification.... this report follow AR5, which assigned mostly warm conditions from about 950 to 1250 to the MCA and' is confusing and seems to both criticise AR5 and draw an assessment conclusion in opposition to the evidence. Also I don't understand what 'which assigned' means in this context. I suggest re-formulating along the lines of 'New evidence since the AR5 indicates that there were no globally coherent warm or cold periods over the Common Era prior to the industrial revolution' or similar if this is what the literature supports. If this is indeed the case, then the authors might want to consider either not using the MCA and LIA terms, or if retained, at least adding a footnote to Table 2.1 to indicate that these periods were warm/cold in the NH midlatitudes, but not necessarily globally. [Nathan Gillett, Canada]	Accepted - clarified as suggested. Elsewhere, have added a statement that temperature fluctuations during these loosely defined periods varied regionally. Note that AR5 also found that these intervals were complicated.
27692	36	39	36	39	replace with published article (Neukom et al) [Poot Delgado Carlos Antonio, Mexico]	Accepted - update citation
6788	36	39	36	39	Dätwyler et al. year is 2018 [Raphael Neukom, Switzerland]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6776	36	41	36	43	Although see Brönnimann et al. (in Press, nature Geoscience) indicating that warming during the 1st half of the 19th century primarily reflects the recovery of the global climate system from a sequence of eruptions, with possibly a minor contribution from anthropogenic greenhouse gases. Suggest a balanced incorporation of both studies or removing the second part of the sentence (after the comma). [Raphael Neukom, Switzerland]	Accepted - updated to include Brönnimann et al's study about volcanic forcing at the end of the LIA. However, attributing the warming during the second half of the 19th century to reduced/recovery from volcanism is outside the remit of CH2.
23706	36	42	36	42	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
16118	36	45	36	46	New regional multi-proxy temperature reconstruction (Shi et al., 2015, Climatic Change) from Asia2K of PAGES 2k is encouraged to be cited, since PAGES 2k made a great improvement for the archives of proxy records (e.g. tree-ring measurements) in East Asia after AR5, which has substantive effect in the subsequent global compilation. [Feng SHI, China]	Accepted - added citation for Asian reconstruction
14480	36	48	36	50	For PAGES 2k summary, McGregor et al. (2018, Nature Geosciences) could be cited here. The paper indicates a higher temperature during AD 200-1200 than the last 900 years. [CUG, Guoyu Ren] [Guoyu Ren, China]	Accepted - added citation for marine reconstruction; assuming the reviewer is referring to 2015 article
42944	37	0			2.3.1.1.3 Temperatures during the instrumental period – surface. The following general issues with this section are discussed in more detail below: 1) Berkeley Earth discrepancy between gridded and average data over 1850-1950 (as well as other issues). 2) Cool bias in OLS trend estimates 1880-2018 (compared to recent decade or "flexible" trend estimates) 3) Coverage bias in HadCRUT4 and NOAA 4) Omission of discussion of slowdown (so-called "hiatus") as defined in AR5 chapter 2 Clarke (2019, in preparation) fully addresses issues 1-3 [David Clarke, Canada]	Noted. This appears to be an introduction to other comments by the same reviewer and requires no response on its own.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46806	37	1	37	8	Too much space is here devoted to, in detail, cite values from one single study where similar level of detail is not given for other studies. The MCA-LIA difference of this study is also almost certainly underestimated by a high noise level in the data collection (no signal on average in the proxies) and by the inclusion of individually detrended tree-ring series that cannot capture any centennial-scale variability as the maximum frequency retained is limited with such detrending method to periods shorter than the life-span of the individual trees used. [Charpentier Ljungqvist Fredrik, Sweden]	Rejected - The text focuses on the one study because (1) it is based on the most comprehensive and recent data synthesis; (2) it addresses methodological differences -- seven different statistical procedures are reported in the one study; (3) it is the only study that reports GMST, except for Tardif et al., 2018, which is not highlighted because it is more model-dependent; (4) the underlying data have been shown to yield reconstruction that are less method dependent (Wang et al., 2015); (5) as stated, the reconstruction agrees with the patterns reported in previous reconstructions; and (6) the length of text devoted to 2k temperature must be balanced against that devoted to all other reference periods in context of the exceedingly limited number of words available for assessing GMST of past climate states. The text now explains that the
23708	37	3	37	3	Very confusing as written. Delete the negative signs (you have said it is a cooling trend) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Reject - The use of a negative to signify cooling is standard practice.
23710	37	5	37	5	Change to Pre-Industrial for consistency elsewhere in the text and insert 'times' after 'Industrial' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
42902	37	7	37	8	add AR6 standard likelihood language to this sentence. [Michael Evans, United States of America]	Taken into account - "medium confidence" at the beginning of the long sentence applies to all four of the "major features of GMST" listed in this sentence
18038	37	8	37	8	50-year period within the past 2000 years? Or ever? [Gwenaelle GREMION, Canada]	Accepted - Changed "the reconstruction" to "the 2000 year reconstruction"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11582	37	10	37	21	While all limitations mentioned are true, regionally consistent pictures begin to emerge. See e.g. Lüning et al. (2019a): The Medieval Climate Anomaly in South America. Quaternary International, 508: 70-87. doi: 10.1016/j.quaint.2018.10.041; Lüning et al. (2017): Warming and cooling: The Medieval Climate Anomaly in Africa and Arabia. Paleoceanography 32 (11): 1219-1235, doi: 10.1002/2017PA003237; Lüning et al. (2019b): The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012; Lüning & Vahrenholt (2019): Holocene climate development of North Africa and the Arabian Peninsula. In: Bendaoud et al. (Eds.), Geology of the Arab World - An overview', Springer, 507-546, doi: 10.1007/978-3-319-96794-3_14. Climate models need to use available data for calibration – which is much better than uncalibrated models. Trust in the results of climate models are seriously eroding if modellers continue ignoring this big issue. More effort is necessary in this respect, even if initially based on far-from-ideal calibration palaeoclimate data. [Sebastian Luening, Portugal]	Taken into account - Two suggested references were added to the previous paragraph, which gives examples of some of the post-AR5 regional reconstructions (data syntheses in this case). In addition, this section was streamlined and integrated with the broader discussion of limitations of CH2 assessment. Issues regarding climate models are outside of the scope of CH2.
42900	37	10	37	21	needs support through citation and references (e.g. Emile-Geay et al 2017; St George (2014) for tree-rings; others for marine records/foraminifera, pollen, et al) [Michael Evans, United States of America]	Taken into account - this section was be streamlined and integrated with the broader discussion of limitations of CH2 assessment.
42408	37	10	37	21	Although it is very important to highlight the uncertainties within paleo-datasets, which this section does well, there are still many benefits to the data and paleo research in general (such as the information provided in this chapter and others). It is important to highlight the value of this type of research and I feel this paragraph lacks this point of view. I would add some sentences in the power position of the paragraph (at the end) highlighting again the usefulness of the paleo-datasets and showing support for continued paleo-research. This applies to all areas that use paleo-data and talk about its uncertainties within this report. It is important for people reading these documents to walk away knowing the value of paleoresearch regardless of the many uncertainties surrounding it. [Elizabeth Fard, United States of America]	Taken into account -this section was streamlined and integrated with the broader discussion of limitations of CH2 assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14482	37	11	37	14	The reconstructions of GMST based on tree-ring are also hampered by the likely bias related to CO2 fertilization effect on tree growth and observational surface air temperature data of modern calibration period. (CUG, Guoyu Ren) [Guoyu Ren, China]	Taken into account - this section was streamlined and integrated with the broader discussion of limitations of CH2 assessment. The goal is to feature a few of the major limitations rather than a comprehensive treatment of the weakness of all paleoclimate methods.
52332	37	11	37	17	extremely wordy - some complex sentences with 4-5 clauses and commas, and be edited to 2 or 3 sentences for clarity. One suggestion: "For example, tree ring data are the primary data source for CE temperature reconstructions, and only a small fraction extends to the first millennium CE." [Katherine Glover, United States of America]	Taken into account - this section was streamlined and integrated with the broader discussion of limitations of CH2 assessment.
44860	37	12	37	13	Moisture influence is not true to oceanographic temperature proxy. This line should be more specific. [Kaoru Kubota, Japan]	Taken into account. This paragraph has been Omittedted from this section and the content entirely rewritten.
6780	37	16	37	16	add "Northern Hemisphere" between "from" and "tree" [Raphael Neukom, Switzerland]	Taken into account. This paragraph has been Omitted from this section and the content entirely rewritten.
6778	37	17	37	19	Although new approaches exist, making use of model-based covariance patterns (e.g. Analog Method or Data Assimilation) are not prone to this stationarity assumption (Steiger et al. Papers, Gómez Navarro et al. 2017, Clim. Past, Neukom et al., in press). Possibly change to "Some Climate field reconstruction methods impose..." [Raphael Neukom, Switzerland]	Taken into account. This paragraph has been Omitted from this section and the content entirely rewritten.
35584	37	23	37	25	The sentence is poorly phrased because it isn't clear how much confidence is assigned to the cooling in the SH. If the confidence level for cooling is different for the SH than the NH, write these out separately. 'There is medium confidence that surface temperatures over the NH have tended to gradually decrease over the past several thousand years, and low confidence that surcace temperatures have decreased over the SH over the same period' or similar. 'Possibly' isn't part of the IPCC calibrated uncertainty language. [Nathan Gillett, Canada]	Accepted - clarified the difference in confidence in cooling in NH vs SH and Omittedted the word "probably"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32642	37	23	37	29	While there was earlier discussion that orbital forcing influences the climate, there really has not been discussion hear about how the trend in precession may well be the cause of this general cooling trend as the time of year of closest approach to the Sun shifts from summer to autumn and winter in the Northern Hemisphere. It seems to me it would be worth explaining this possibility along the way, making it clear that the LIA is not likely due primarily to diminished solar that then recovers into the 20th century as the contrarians like to say explains all warming prior to the mid-20th century. [Michael MacCracken, United States of America]	Reject - The cause(s) of the temperature trends is outside the scope of CH2.
23712	37	24	37	24	Change to 'to decrease gradually' to remove the split infinitive [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23714	37	24	37	24	quantify 'several thousand years' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Changed to "at least the past 5000 years"
46808	37	25	37	25	It is not enough evidence to support this statement given the decreasing, and more uncertain, proxy network back in time. Early cold events of the late Holocene, including the 8.2 event, may have been colder than the Little Ice Age. [Charpentier Ljungqvist Fredrik, Sweden]	Accepted - Change "Holocene" to "post-glacial period" because 8.2 ka might have been colder than LIA, as suggested. Also, expecting new Holocene GMST reconstruction for additional evidence.
49336	37	26	37	26	suggest "this multi-millennial cooling trend" instead of "this cooling trend" to clarify that recent warming reverses many thousands of years of overall cooling (rather than only the cooling into the LIA mentioned in the previous sentence) [Yarrow Axford, United States of America]	Accepted - revised as suggested.
11584	37	26	37	27	Authors write: "This cooling trend was reversed during the mid 19th Century (high confidence). This is not entirely true. Several millennial-scale temperature cycles occurred (Bond cycles, Bond et al 2001 in Science) which brought already previous brief warm phases of a few centuries which include e.g. the Medieval Climate Anomaly and the Roman Warm Period. Global Holocene long-term temperature reconstructions such as the ones by Marcott et al 2013 are not able to resolve these because data points are too widely spaced and age models too uncertain. A monotonous long-term cooling as is suggested in this chapter 2 does not represent current knowledge of the palaeoclimate community. [Sebastian Luening, Portugal]	Accepted - Omitted the word "gradually" to avoid confusion with reviewer's "monotonous trend". Added "multi-millennial" to specify the time scale and avoid confusion with the century-scale fluctuations highlighted by the reviewer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14484	37	27	37	28	"The GMST of the past 50 years was likely warmer than any 50 year mean of the MCA (high confidence). It would be safer to assign a medium confidence here, because many studies from Europe and East Asia show a higher regional average temperature during the period. (CUG, Guoyu Ren) [Guoyu Ren, China]"	Taken into account - While some regions were probably warmer during the MCA, this paragraph concerns GMST rather than regional temperatures. Nonetheless, the statement was revised to be more conservative. Rather than focusing on a "50-year period", the statement uses the more general "recent decades", which includes periods when average temperatures were higher than during the entire 50-year-long interval.
35586	37	27	37	29	The assessment statement here focusses on the past 50 years, whereas AR5 focussed on the most recent 20-year period. Would it be possible to make a stronger statement about the warmth of the 1995-2014 period (a common AR6 reference period) relative to the CE, or does the time-resolution of the proxies restrict this comparison to a 50-year average? [Nathan Gillett, Canada]"	Noted - AR5 millennial temperature reconstructions focused on 30- and 50-year-long intervals (AR5 Table 5.4). While it is certain that GMST has increased since 2000, and therefore statements about the unusualness of recent warmth are strengthened by including 21st Century temperatures, the assessment here generally addresses the reconstructions themselves. Most reconstructions, especially GMST reconstructions, end in 2000. For trends, a 50-year-long period was chosen because trends over shorter periods are generally less significant.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11586	37	27	37	29	Authors write: "The GMST of the past 50 years was likely warmer than any 50 year mean of the MCA (high confidence), and more likely than not was the warmest 50-year period of the CE (medium confidence)." A high confidence statement is being made about the MCA, even though the opposite is being claimed in Chapter 1 (page 69, lines 35-37: "Although Lüning and Vahrenholt (2017) suggest a much longer context for defining pre-industrial, estimates of natural radiative forcings and global temperature are too uncertain to allow a reliable estimate for longer periods." How can a high confidence statement being made under those circumstances. In fact, in reality global and hemispheric temperature reconstructions are still unstable and change significantly from version to version. New evidence now indicates that the MCA was in fact more synchronous than thought. [Sebastian Luening, Portugal]	Rejected - The statement in CH1 relates to the ability to detect a human influence on temperature (the onset date of industrialization), which is a question of detection and attribution. The statement here in CH2 relates to observations of temperatures over different periods. The CH1 and CH2 statements are not contradictory. In addition, contrary to the reviewer's comment, new studies increasingly show that temperature reconstructions of the past 1000-2000 are robust and that earlier warming/coolings are less homogenous than recent warming (e.g., Neukom et al., 2019).
48788	37	32	37	32	It would be important to add some elements in this section on the rate of temperature change. See for example: "Evolution of land surface air temperature trend", de Ji et al. 2014 (Nature CC) [Sylvie JOUSSAUME, France]	Rejected. We have decided to present changes as the total over different periods; warming rates can be derived from that information. Users requiring information on individual decades can obtain that from Figure 2.12.
27172	37	32	37	37	What is important to emphasize is that since 1945, beginning of acceleration of CO2 emission, the warming has been limited to about 0.4°C. [François GERVAIS, France]	Rejected. This assertion is only valid if the warmest year of the 1940s is compared with multi-year averages excluding 2016 in the recent period.
48772	37	33	37	37	Inconsistent with Chapter 7 p 5 line 26-27: Human induced surface temperature rise for the period 1750-2017 is 1.1 °C [0.9 to 1.3 °C 5% to 95% range]. And inconsistent with chapter 5 p66 line 33-34 We here apply a historical warming expressed in global average surface air temperatures (SAT) of 0.97°C between the 1850–1900 and 2006–2015 periods. A table with the different historic warming estimates, an explanation of the terms, why each is used where and the origine of the differences would help. [Birgit van Munster, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This line is stating what was reported in AR5 so there is no expectation that it would be consistent with values for more recent end dates reported in other chapters of AR6.
23716	37	40	37	40	Change 'have' to 'has' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50250	37	45	37	45	first reference to WWI, please explain. The beginning of the dataset in the 17th century wouldn't it be more informative? [Sophie SZOPA, France]	Taken into account. World War I was spelt out. While it is true that ICOADS goes back into the 1700s, to date the 18th century data have not been used in any global analysis to our knowledge.
49938	37	45			What is WWI? World War I? [Owen Cooper, United States of America]	Accepted. To be spelt out in SOD.
39306	37	47			<p>2.3.1.1.2 P2-37 Insert before line 47</p> <p>Using a new statistical technique, it was recently been possible to determine the uncertainty in estimates of global temperature change since 1880. By comparing 6 global average temperature series from 1880-2012, (Lovejoy 2017) showed that with 90% confidence that the temperature changes were correct to within ± 0.108 oC. The dominant source of error for these centennial scale estimates are the space-time scale reduction factors of each series that are each slightly different. This is a direct consequence of small differences between the nominal space-time resolutions of the gridded data (e.g. one month and 5oX5o) and the actual resolutions that depend on the amount of averaging in the space-time interval. The more averaging that is used, the smaller the variability, and small differences between the data sets lead to multiplicative differences in the estimated temperature changes since 1880.</p> <p>Lovejoy, S. (2017). "How accurately do we know the temperature of the surface of the earth? ." Clim. Dyn. [Shaun Lovejoy, Canada]</p>	Taken into account. The estimates of uncertainty already given at p40-41 are broadly consistent with the Lovejoy 2017 results.
23718	37	48	37	48	Remove hyphen and replace with : to tidy up the flow of the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23720	37	50	37	50	Remove hyphen [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35588	37	51	37	54	Given their importance for assessed estimates of warming, these two corrections deserve more discussion. Could the authors add some more discussion of the reasons for these biases and their assessed magnitudes? [Nathan Gillett, Canada]	Taken into account. This paragraph has been redrafted given the recent release of HadSST4 and other recent literature around SST uncertainties, although we believe that detailed discussion of the biases is best referred to the cited papers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32306	37	52	37	53	Which is more accurate, the buoys or the ships? i.e. Are the buoys biased cold relative to reality or are the ships biased warm or is it not possible to say? [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted. In the context of long-term changes, which one is most "true" is largely irrelevant - the important aspect is how each different source of data relates to the others.
23722	37	52	37	53	Don't split numbers and units across lines [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35590	38	4	38	6	This is a weak conclusion. Would be better to assess whether different SST datasets are consistent to within quoted uncertainties, and/or to assess the magnitude of the uncertainty in the global mean warming trend. [Nathan Gillett, Canada]	Noted. This part of the chapter is largely introductory. In FOD there were not separate assessments for global land and ocean temperatures in this chapter, but this has been done in SOD.
7142	38	6	38	6	It is stated that global-mean SST increased since 1900, but no number is given. Specific values for marine and land separately are not given later. In the context of the paragraph, placing this increase in context of the adjustments would be useful. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This part of the chapter is largely introductory. In FOD there were not separate assessments for global land and ocean temperatures in this chapter, but this has been done in SOD.
42946	38	13	38	13	Hausfather et al (2017a) should be Hausfather et al (2017) i.e. same paper referenced just above [David Clarke, Canada]	Accepted. Reference corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32644	38	14	38	17	<p>What has been particularly unfortunate about the almost certain warm bias in the ocean SST data during WWII is that this has bumped up the global average temperature and contrarians have attributed this to solar variability and recovery from the Little Ice Age, as they say. Were this bias not in the record, all that would be seen as mistaken. It is really interesting to take the observations record and use one's finger to cover over the WWII years--one gets quite a different impression of what has been happening, giving, in my view, much more visual credence to the GHG causal mechanism. It would seem to me that somewhere more needs to be said about this--if not here in the attribution discussion. But here, it really does need to be said more strongly that there does remain a bias in the record (land surface temperatures do not really show anything like the jump and what increase they do show may well be the result of how results are combined horizontally along coastlines). I've done a bit of work trying to identify where the bias is showing up, but not had the ability to finish the effort. I would urge showing results for the land and ocean areas separately and also showing some graphics of the spatial and seasonal patterns of the jump by somehow looking at departures from trend lines based only on years other than the war years, looking at differences from reanalyses, etc. Clearing all of this up would, it seems to me, really quash some of the remaining contrarian arguments. [Michael MacCracken, United States of America]</p>	<p>Taken into account. Land and ocean temperatures are discussed separately in SOD. There are ongoing developments in underlying data sets (particularly HadSST4) and consideration of potential data set biases is considered there, although given space constraints detailed discussion is referred to the cited papers.</p>
23724	38	15	38	15	<p>Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Editorial</p>
7144	38	17	38	17	<p>Is this correct? US naval sources may dominate overall during WW2, but for some periods of WW2 and just after most were UK naval sources. Odd to mention US naval sources, when Scott Woodruff has a document from the late 1970s saying that many US naval logbooks were physically destroyed in the late 1970s as being of no scientific value! Not sure whether this is mentioned in Kent et al (2017), but it is widely known. This publication has a list of datasets which were known to exist, but which can no longer be found. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Taken into account. According to Thompson et al (2008), 80% of the observations available at that time for the 1941-1945 period were of US origin. The Thompson et al 2008 citation has been added, and this section reviewed to incorporate the latest information from HadSST4.</p>
35332	38	27	38	27	<p>CRUTEM - missing citation - or is this in the wrong place given more detailed discussion in following paragraph [Dunn Robert, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Rejected. This is a generic reference to the CRUTEM family of datasets, with more detailed information and citations for specific versions in later paragraphs.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14486	38	28	38	31	This sentence could be modified as to read more accurately "A new global land data set, the China Meteorological Administration (CMA) Global Land Surface Air Temperature (GLSAT) dataset (Sun et al., 2017; Xu et al., 2018) has higher network density in some regions (particularly Asia) than previously existing datasets. It contains a total of 10,271 observational stations from continents all over the world with a length of records no less than 20 years for monthly mean temperature, and has been processed for data quality and homogenization. Global trends in GLSAT are generally consistent with other land datasets through 2014 (Sun et al., 2017; Xu et al., 2018). (Reference: Sun, X. B., G. Y. Ren, W. H. Xu, Q.X. Li, Y.Y. Ren, 2017: Global land-surface air temperature change based on the new CMA GLSAT dataset. Science Bulletin, 62: 136-238. doi: 0.1016/j.scib.2017.01.017). (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. The purpose of this paragraph is to provide a brief introduction to the existence of the new data set, noting that the primary data sets used in this section are those which cover both land and ocean. A CMA land-ocean product and a supporting paper are now available (which was not the case at the time of FOD) but as it starts in 1901, it is not suitable for assessment of changes from the pre-industrial period or a subset thereof.
50252	38	33	38	33	"lack" => "the lack" [Sophie SZOPA, France]	Editorial
7146	38	33	38	33	This sentence refers to land data in data sparse regions. Instead of portraying this as a limitation, it should be pointed out that almost all the data that can be found in regions like the Arctic is being used. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph has been substantially redrafted for SOD, now that the extent to which new data set versions spatially infill over the polar regions is clear.
7152	38	33	38	33	The point of looking at the Arctic and Antarctic extrapolation is that the Arctic has a warming period from 1920 to 1950, but there is little difference in the datasets, whether they extrapolate or not during this period. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Consideration have been given to discussing such issues as space and charge permit.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35592	38	33	38	42	<p>The issue of the effect of limited spatial coverage of temperature measurements on estimates of global mean warming needs a lot more discussion and assessment than is provided in this short paragraph. The question of how much global mean warming has occurred since pre-industrial is fundamental to the report and to informing the global stocktake. The fact that only certain regions have long-term observations means that there is substantial uncertainty in inferring the global mean from available observations. Fundamentally, we don't know how much non-observed regions have warmed, because we don't have direct observations. We can estimate this warming for some periods based on reanalyses, satellite records, or kriging from other stations, but these methods all introduce additional uncertainties which need to be thoroughly assessed. Moreover, it is not clear how well these approaches work for estimates of the global mean over the 1850-1900 period, which are needed to estimate warming relative to this baseline. If future versions of temperature datasets use infilling approaches, these approaches will need to be assessed here, and the additional uncertainties introduced quantified. As written, this paragraph seems to suggest lack of coverage in data-sparse areas is simply 'a limitation of conventional datasets' which will be addressed in 'spatially complete' datasets, rather than a fundamental source of uncertainty in estimating global mean warming. [Nathan Gillett, Canada]</p>	<p>Taken into account. This discussion was somewhat preliminary in FOD given the likelihood of major changes in some underlying datasets by the time of SOD. This section has been redrafted to reflect coverage bias issues in those versions used in SOD.</p>
7150	38	33	38	42	<p>To illustrate these polar coverage issues, it would be very informative to show time series for the Arctic and the Antarctic (poleward of say 65 degrees) from 1957, so these could additionally include the Reanalysis from ERA-5 and JARE-55 compared to the first 5 datasets shown in Table 2.3. Extrapolation techniques appear to work in the Arctic, but they also need to be shown in the Antarctic where less overall warming has taken place. It is a pity that you cannot have some Supplementary Figures in an IPCC Report. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Taken into account. Space did not allow regional analyses in Chapter 2 but it may be relevant for the regional chapters.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7154	38	33	38	42	Later with respect to extrapolation (for Humidity on p45, lines 39-41) you point out that the biggest source of uncertainty appears to be incomplete coverage, notably sampling ERA-Interim to the HadISDH mask (Dunn et al 2017). Why is this extrapolation an uncertainty for humidity, but the same extrapolation reduces uncertainty when it comes to air temperature? All you need to do is to state that the basic data input is essentially the same, but some groups extrapolate more than others. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This discussion was somewhat preliminary in FOD given the likelihood of major changes in some underlying datasets by the time of SOD. This section has been redrafted to reflect coverage bias issues in those versions used in SOD.
7148	38	34	38	35	CRUTEM4 in 2012 uses more stations than NOAA in 2012, but is referred as having limited coverage. The coverage in these datasets is much the same. What is being done in some datasets is to be much bolder (or possibly cavalier) in the extrapolation of what data there is to further afield. The issue in the Arctic is particularly important, as this extrapolation is very dependent on whether the values are from land stations or ships, and then whether the extrapolation is to regions of open water or sea-ice covered ocean (and the link to changes in sea-ice coverage through time). [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This discussion was somewhat preliminary in FOD given the likelihood of major changes in some underlying datasets by the time of SOD. This section has been redrafted to reflect coverage bias issues in those versions used in SOD.
37436	38	35	38	36	It could be added here (with apologies for self-citation) that Simmons and Poli (2015; doi: 10.1002/qj.2422) showed that the extended dataset of Cowtan and Way was in reasonable agreement with reanalyses over the Arctic, despite (understandably) lacking detail where reanalysis temperatures were influenced by anomalies in sea-ice cover. The paper also used discussed the general quality of temperatures over the Arctic from several reanalyses. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In FOD the Simmons et al 2017 paper was the primary citation for the similarity of Arctic trends between reanalyses and Cowtan/Way, but the additional citation is also of value.
42948	38	35	38	37	It should also be mentioned that NASA GISTEMP (as used in AR4 and AR5) did extend GHCN into polar regions (as well as extrapolate over Arctic sea ice) via distance weighting of nearby gridboxes up to 1200 km. Hansen et al 2005 clearly identified the coverage bias issue. NASA GISTEMP coverage is in fact quite similar to Berkeley Earth over 1880 to present. Additionally, NOAA technique of Empirical Orthogonal Transformation (EOTs) also provides increased spatial coverage in SSTs and undersampled land regions, including the Arctic. Clarke (2019, in preparation) shows the evolution of spatial coverage over 1850-2018 and such a figure might be relevant here. On the other hand, Fig 2.11 also illustrates the above as seen in the marked contrast in coverage between HadCRUT4 and NASA GISTEMP (with NOAA coverage in between the other two). [David Clarke, Canada]	Taken into account. This discussion was somewhat preliminary in FOD given the likelihood of major changes in some underlying datasets by the time of SOD. This section has been redrafted to reflect coverage bias issues in those versions used in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14488	38	36	38	38	This argument will be easily criticized. The problem is that you did not say that the Arctic region was lacking data coverage when the Arctic temperature was in low phrase or in decreasing period, but you did say so when the Arctic temperature was in high phrase or in a period with rapid warming trend. This is not scientific. Keeping the data coverage consistent throughout the time period analyzed is important. (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. The discussion of underestimation of warming trends refers specifically to the recent period.
42950	38	37	38	40	Consider adding: "Cowtan et al (2018) found that a simple GLS weighted averaging process similar in effect to infilling via kriging outperformed naïve simple hemispheric average (as in HadCRUT4) when tested against various levels of observational coverage". doi:10.1093/climsys/dzy003 [David Clarke, Canada]	Taken into account. This discussion was somewhat preliminary in FOD given the likelihood of major changes in some underlying datasets by the time of SOD. This section has been redrafted to reflect coverage bias issues in those versions used in SOD.
42952	38	38	38	39	Hausfather et al (2017) did not address this issue as we covered SST series only. Perhaps authors had a different paper in mind? [David Clarke, Canada]	Accepted. This is an error. The reference has been corrected.
18040	38	38	38	40	Would be good to include what the differences in warming trends is between data sets with/without polar coverage [Gwenaëlle GREMION, Canada]	Taken into account. Most of those data sets which had limited polar coverage at the time of FOD have more extensive coverage for SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13080	38	40	38	40	<p>To improve the comprehensiveness of the main text, one should consider to add the impact of temporal sampling (besides incomplete global data coverage) on mean temperature and its trend at regional or global scales. Specific text will be added after '...than those which do not.': [Conventional analysis on global warming were based primarily on the daily average air temperature from the daily maximum and minimum temperatures. Relevant studies have reported that observation time and temporal samplings do bias daily mean temperature (Baker, 1975; Zeng and Wang, 2012). Zhou and Wang (2016) reconstructed highly-sampling global land mean air temperature using the observations at 0, 6, 12 and 18UTC from the NCDC Integrated Surface Database (ISD-H) and revealed an under-estimation of recent warming trends.]</p> <p>References: Baker, D. G., (1975). Effect of observation time on mean temperature estimation. J. Appl. Meteorol. 14, 471–476. doi: 10.1175/1520-0450(1975)014<0471:EOOTOM> 2.0.CO;2. Zeng, X., and Wang, A., (2012). What is monthly mean land surface air temperature? Eos, Transactions American Geophysical Union 93, 156–156. doi: 10.1029/2012eo150006. 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]</p>	<p>Rejected. Biases from time of observation, or from changes in the method used in calculating daily means, are a well-known source of potential inhomogeneities in temperature data sets, but known issues of this type are dealt with in the process of developing major long-term homogeneous temperature data sets. The ISD-H data set quoted here is not homogenised.</p>
42976	38	40	38	40	<p>Add: "AR5 found a marked reduction (or "hiatus") in GMST linear trend over 1998-2012 relative to 1951-2012. Among the five conventional datasets, only HadCRUT4 now exhibits such a reduction in warming rate in that period (see Cross-Chapter Box 3.1 for further details)". Alternatively (if HadCRUT5 has enhanced spatial coverage as expected): "AR5 found a marked reduction (or "hiatus") in GMST linear trend over 1998-2012 relative to 1951-2012. None of the five conventional datasets now exhibit such a reduction in warming rate in that period (see Cross-Chapter Box 3.1 for further details)". It is important to provide this update here in AR6 Chapter 2, as the AR5 Chapter 2 statement was the official IPCC definition of "hiatus" cited in AR5 Chapter 9. [David Clarke, Canada]</p>	<p>Rejected. Information about the 1998-2012 period is covered in Cross-Chapter Box 3.1 and to include that information here would be duplication.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32198	38	40	38	42	I look forward to seeing the text to "be substantially modified here in later versions as new data products likely to be spatially complete". As referred in the revision of the Internal Draft, there has been a considerable effort to use satellite land surface temperatures to complement station observations and to provide more accurate interpolations. Many of these datasets are not yet fully matured and are obviously limited to the satellite era. It is worth noting that this may be the way forward to overcome limitations of data sparseness and interpolation techniques. [Isabel Trigo, Portugal]	Noted. Satellite land surface temperatures are referred to at p40 lines 4-7.
37438	38	40			Change "polar" to "Arctic". The period studied by Simmons et al. did not exhibit net warming over the Antarctic. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph was substantially revised for SOD given developments in underlying data sets.
14490	38	44	39	4	Sun et al. (2018) (Sun, X.B., Guoyu Ren, Qinglong You, Yuyu Ren, Wenhui Xu, Xiaoying Xue, Yunjian Zhan, Siqi Zhang, Panfeng Zhang, 2018, Global diurnal temperature range (DTR) changes since 1901, Climate Dynamics, doi.org/10.1007/s00382-018-4329-6) analyzed global land surface DTR change since 1901, and this work could be given a little bit more attention. They used a newly developed China Meteorological Administration (CMA) Global Land Surface Air Temperature (GLSAT) dataset, and showed an interesting multi-decadal scale shift of DTR from a rising trend for period 1901-1950 to a significant downward trend for period 1951-2014. Overall, the global land surface annual mean DTR significantly decreased at a rate of $-0.036^{\circ}\text{C}/\text{decade}$ over the 1901–2014 period, and the DTR decrease reaches $-0.054^{\circ}\text{C}/\text{decade}$ during 1951–2014. The early-1950s reverse phenomenon is more obvious in the Northern Hemisphere than that in the Southern Hemisphere. For the periods 1979–2014 and 1998–2014, the decreasing trends in DTR mainly occur in the Northern Hemisphere. Asia, Eastern North America, and Australia exhibited widespread decreases in DTR, although the trend pattern for global DTR is generally mixed during two recent periods of 1979–2014 and 1998–2014. (CUG, Guoyu Ren) [Guoyu Ren, China]	Taken into account. The Sun et al 2018 paper and its key results are already cited at p39 lines 1-4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14492	38	53	39	1	The regions with continuous decrease of annual mean DTR in recent decades actually mainly include East Asia, Central Asia and Southeast Asia, not merely Southeast Asia, with mainland China witnessing a more significant decline over the last five decades (Sun et al., 2018. Sun, X.B., Guoyu Ren, Qinglong You, Yuyu Ren, Wenhui Xu, Xiaoying Xue, Yunjian Zhan, Siqi Zhang, Panfeng Zhang, 2018, Global diurnal temperature range (DTR) changes since 1901, Climate Dynamics, doi.org/10.1007/s00382-018-4329-6). (CUG, Guoyu Ren) [Guoyu Ren, China]	Taken into account. This sentence refers to the findings of the Thorne et al. 2016 papers. More detail has been presented from the Sun et al. 2018 results.
32200	38		43		The sections on surface and free atmosphere temperature during the instrumental period are now very close to a final version. One of their main strengths is the diversity of data sources, from conventional observations, to satellite and reanalysis, that truly complement each other. [Isabel Trigo, Portugal]	Noted.
14494	39	1	39	2	"A second analysis by Sun et al. (2018), using the global CLSAT data set (Xu et al., 2018)..." should be changed to "A second analysis by Sun et al. (2018), using the GLSAT dataset (Sun et al., 2017; Xu et al., 2018)..." (Sun, X. B., G. Y. Ren, W. H. Xu, Q.X. Li, Y.Y. Ren, 2017: Global land-surface air temperature change based on the new CMA GLSAT dataset. Science Bulletin, 62: 136-238. doi: 0.1016/j.scib.2017.01.017). (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. The Xu et al 2018 reference is sufficient for the purposes of this report.
7156	39	6	39	10	This brief part of a paragraph correctly points out that urbanization is unlikely to affect long-term changes in land temperatures. However, the next sentence refers to two Chinese papers that indicate possibly greater urban effects in rapidly urbanizing areas of eastern China. The question is why these two papers, when there are probably a hundred to choose from. There are Chinese papers that show little urbanization effects, when looking at China as a whole, and that vast areas of China are not very well monitored. One of these is Wang, F., Ge, Q., Wang, S., Li, Q. and Jones, P.D., 2015: A new estimation of Urbanization's contribution to the warming trend in China. J. Climate 28, 8923-8938, doi:10.1175/JCLI-D-14-00427.1. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The purpose of this sentence was to give an example of a larger urban influence in a particular location, not to provide a comprehensive assessment of the topic.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14496	39	8	39	10	This issue has been examined frequently for mainland China and other countries of East Asia, and there should have been hundreds of publications in English and other languages since AR4. A balanced and brief assessment of the regional investigations is needed here. I suggest this part to be changed to read "....., although larger signals have been identified in some regions, especially rapidly urbanizing areas such as China (Yang et al., 2011; Ren and Ren, 2011; Li et al., 2013; Ren and Zhou, 2014; Liao et al., 2017). Recent studies applying more sophisticated procedures generally showed a large and significant contribution of urbanization of 20-40% to the overall annual mean warming of China as estimated from historical climate data of the national observational networks for the last four to six decades (Ren et al., 2008, 2015; Yang et al., 2011; Ren and Zhou, 2014; Sun et al., 2016; Wen et al., 2019). The effect of urbanization on the observed surface air temperature trends is also obvious in Korea and Japan (Chung et al., 2004; Fujibe, 2011) (References: Ren and Zhou, 2014. Ren, G. and Y. Zhou, 2014. Urbanization effects on trends of extreme temperature indices of national stations over mainland China, 1961-2008, Journal of Climate, 27 (6), 2340-2360, doi: 10.1175/JCLI-D-13-00393.1); Sun, Y., X. B. Zhang, G. Y. Ren, F. W. Zwiers, and T. Hu, 2016: Contribution of urbanization to warming in China. Nat. Climate Change, 6, 706–709, https://doi.org/10.1038/nclimate2956 ; Yang, X. C., Y. L. Hou, and B. D. Chen, 2011: Observed surface warming induced by urbanization in east China. J. Geophys. Res., 116, D14113, https://doi.org/10.1029/2010JD015452 ; Ren, GY, and	Rejected. This is more detail than is warranted in this section. The citations used were intended to be illustrative rather than exhaustive.
35594	39	10	39	13	Are these 'site specific data homogeneity issues' accounted for in assessed uncertainties in warming relative to 1850-1900 presented in this chapter? If so, explain this. If not, this should be done, because as written the text implies that these issues do contribute to uncertainties in this quantity. [Nathan Gillett, Canada]	Noted. This is largely carrying forward findings from AR5 (where the issue is discussed at much greater length). No known publications since AR5 substantially challenge the AR5 conclusions re: the influence (or lack thereof) of site-specific homogeneity issues on global-scale variables in global data sets.
26912	39	10	39	13	The sentence is long and not easily understandable. It needs rephrasing. [Prodomos Zanis, Greece]	Taken into account. Some rewording in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7158	39	10	39	17	The issue of shelter standarization is quite different from urbanization, and should have its own paragraph. The point about this effect being addressed in Europe is a good one. Elsewhere in this section, there is some discussion of adjusting marine data by comparing with land stations on islands and coasts. This argument can be used the other way around with data-rich regions around the North Atlantic. The adjusted marine data and the coastal/island land data agree since 1900, so exposure issues due to screen design changes are not that important. The important point is to emphasize the consistency between the various sources of temperature data, and to not ignore SST when looking coastal land stations (and vice versa). [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There is limited discussion of homogeneity issues in AR6 as these were extensively discussed in AR5, and because no known evidence has significantly challenged the AR5 findings in this area.
23726	39	11	39	11	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37440	39	13	39	17	This is an important result. The 0.1°C differences are around twice as large, or even more, than the estimated difference between GSAT and GMST changes. Add in something to account for the probable small rise in temperature from around the 1750 pre-industrial baseline to the 1850-1900 early industrial baseline, and the GMST/GSAT differences begin to look rather unimportant when it comes to estimating how close we are to reaching the 1.5°C or 2°C limits in the Paris Agreement. It could also be noted that there have been unusually large differences (well over 0.1°C in annual means) in the past two or three years among datasets (including reanalyses). These though are likely to come down when we see results from the new versions of the conventional datasets that are promised for the SOD. If they don't come down, there should be some discussion of this spread in the SOD. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The differences are substantially smaller in the dataset versions used in SOD. The uncertainties discussed here are incorporated in the uncertainty ranges reported for global temperature change later in this section.
37976	39	19	39	21	It would be interesting to also mention the high latitude Southern Hemisphere counterpart of slower warming than average. [Jean baptiste SALLEE, France]	Taken into account. This is largely implicit in figure 2.11.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7772	39	19	39	22	The text here and elsewhere cites only papers that demonstrate warming, while papers that show cooling are neglected or go unmentioned. For example, a solid reference here and nearby is Xiubao Sun, Guoyu Ren, Yuyu Ren, Yihe Fang, Yulian Liu, Xiaoying Xue and Panfeng Xhang,"A remarkable climate warming hiatus over Northeast China since 1998," Theoretical and Applied Climatology. 2017, Vol. 133, Issue 1–2, pp 579–594. Other papers also describe regional cooling. At least one such paper should be cited to fulfill the IPCC requirements that its reports be "comprehensive, objective, open and transparent." [Forrest Mims, United States of America]	Rejected. Whilst regional cooling over some timescales does exist, regional assessments are outside the scope of this chapter which deals largely with assessments at hemispheric and global scales. Regional information is available in Chapters 10 and 12, and in the Atlas.
37442	39	22			GMST could be replaced by GSAT here. The differences between GSAT and GMST are small compared with the difference between land and global averages. Note that Fig. 2.11 shows SST/LSAT trends for the conventional datasets, but the SAT (sea and land) trend for the ERA reanalysis. Trends over sea change a little for ERA5, which should replace ERA-Interim in the SOD. There appears to be quite a high level on consistency between the ERA MAT trends and the SST trends from the Hadley Centre, NOAA and GISS. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
37444	39	25	39	26	Agreement in Fig. 2.12 is not as robust as it might appear to the uninitiated reader. The datasets have some substantial interdependences, such as the use of common SSTs by GISS and NOAA, and by HadCRUT4 and Berkeley Earth, Cowtan and Way values taken from HadCRUT4 where the latter has data, and the use of a common set of quality-controlled station data by GISS and NOAA. This should be noted in the text. It is unfortunate that the figure does not include proven reanalysis datasets for the last few decades, as they would show the GISS dataset to be not such an outlier. This should change when the new datasets promised for the SOD are used. If not, there will be some extra explaining to do. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It is certainly true that there is a level of interdependence between the five datasets. In the current text, this figure is used to support the statement (p39 lines 25-27) that the five data sets are "in robust agreement that each of the last four decades has been the warmest globally since the beginning of the records". Given their short duration the renalyses cannot be used to support such an assessment.
26914	39	32	39	36	There is missing description in the caption of Figure 2.11 of the datasets shown. [Prodomos Zanis, Greece]	Rejected. Citations of the data sets are available in the text, as well as in the Observations Annex. In any case this figure has been merged with others and radically redesigned.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14498	39	32	39	36	Here, and also in next paragraph, I do not think it is proper to include reanalysis data series in this figure. All others are in situ observations, and reanalysis data have not been confirmed to well reproduce the observational trends though ERA-interim is somehow better than other reanalysis data. If this has to be included, then a detailed illustration should be given in the caption, showing that it is not the real observation. I also have some doubt about the possibility to calculate trends if the criterion for at least 20% of years during the first and last decile of the trend period are really guaranteed for GISS data. (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. The use of reanalysis data sets alongside conventional data sets (and the limitations thereof) is discussed separately in the text. In any case this figure is no longer in the report in its current form.
42954	39	32	39	36	Figure 2.11: Recommend using "flexible" trends (e.g. smoothing spline or LOESS with suitable span) rather than OLS linear. See comments #9-#13 for further details. [David Clarke, Canada]	Rejected. A decision has been made to retain the OLS trend calculation method with AR(1) correction. While alternative methods clearly exist all have their own issues. Time series which are clearly non-linear can be handled through judicious selection of time periods, or using delta rather than trends, but the GMST time series is not sufficiently non-linear to require such treatment.
37446	39	32			The caption needs to state what trends are being shown. As noted in comment 92, it is necessarily SST/LSAT for HadCRU, NOAA and GISS, and is SAT (MAT/LSAT) for ERA-Interim. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The GMST/GSAT discussion in general has been redrafted to reflect that the division between "GMST" and "GSAT" data sets is not as clear-cut as suggested in the FOD text. On the one hand, the reanalyses are nominally GSAT, but given the role of SSTs in forcing the model are conceptually similar to applying an inflation factor to the SST analysis; on the other hand, the use of NMAT in some SST homogenisation may alias an element of GSAT into nominally GMST analyses.
18042	39	33	39	34	Caption of Figure 2.11 describes a time series that is not actually on the figure [Gwenaëlle GREMION, Canada]	Taken into account. This figure is no longer in SOD in its current form.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57920	39	34	39	34	mPWP (Figure 2.10b) [Bas de Boer, Netherlands]	Unanswerable. Not sure what the reviewer is referring to -- maybe suggesting to use a lowercase "m". Current convention is to use upper case for abbreviations. For example, "MH" for mid-Holocene
57922	39	34	39	44	As mentioned later for the LIG, also for the mPWP spatial variations in temperature are significant, could be mentioned here too. Reference using Haywood et al (Nat comm, 2016), doi: 10.1038/ncomms10646 [Bas de Boer, Netherlands]	Noted - Unclear what section/line the reviewer is referring too, but reference suggested by reviewer taken into consideration for additional information on the MPWP.
14500	39	43	39	45	This, and Figure 2.11, should be explained in more details. At least you should tell readers what they are. Global land surface air temperature anomalies or global land and ocean surface temperature anomalies? It is also confusing for the calculation of anomalies. You say in the figure all the annual averages are relative to 1901-2000, but in the caption readers are told that all timeseries are rebased relative to a 1961-90 climatology. (CUG, Guoyu Ren) [Guoyu Ren, China]	Taken into account. The GMST/GSAT discussion in general has been redrafted to reflect that the division between "GMST" and "GSAT" data sets is not as clear-cut as suggested in the FOD text. On the one hand, the reanalyses are nominally GSAT, but given the role of SSTs in forcing the model are conceptually similar to applying an inflation factor to the SST analysis; on the other hand, the use of NMAT in some SST homogenisation may alias an element of GSAT into nominally GMST analyses.
26916	39	50	39	55	It misses from this sub-section 2.3.1.1.3 a discussion of the 20th century reanalysis datasets such as ERA-20C (Poli et al., 2016, 10.1175/JCLI-D-15-0556.1) and NOAA'S 20CR (Parker, JGR, 2011, doi:10.1029/2011JD016438). How these centennial reanalyses products compare with observational datasets in near surface temperature (https://climatedataguide.ucar.edu/climate-data/era-20c-ecmwf-atmospheric-reanalysis-20th-century-and-comparisons-noaa-20cr) and how valid they are for long-term climate studies? [Prodomos Zanis, Greece]	Rejected. These data sets do not assimilate temperature over land and are not intended to be a homogeneous product used for long-term temperature assessments. As the web page linked in the comment makes clear, there are substantial biases, particularly pre-1979, from changes in observing systems between the satellite and pre-satellite era.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7162	39	50	40	7	ERA-5 and JARE-55 are shown as global averages in Table 2.3. Given the correct discussion of the problems that both these Reanalysis have with the Antarctic, it would be useful to show the agreement of the various datasets without 65-90S. It would be better still to show the agreement between the all the datasets for 65S to 65N, to show that most of the differences relate to the polar regions. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Space did not permit this discussion in SOD.
35596	39	51			What is 'operational GMST monitoring'? Add a reference. [Nathan Gillett, Canada]	Rejected. This refers to products such as the annual WMO State of the Climate report, and monthly/seasonal/annual climate reports issues by various global monitoring centres. We believe this is sufficiently clear to readers.
37448	39	51			Change GMST to GSAT here. This is not optional. GSAT is what is monitored operationally. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The GMST/GSAT discussion in general has been redrafted to reflect that the division between "GMST" and "GSAT" data sets is not as clear-cut as suggested in the FOD text. On the one hand, the reanalyses are nominally GSAT, but given the role of SSTs in forcing the model are conceptually similar to applying an inflation factor to the SST analysis; on the other hand, the use of NMAT in some SST homogenisation may alias an element of GSAT into nominally GMST analyses.
23728	39	52	39	53	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37450	39	54			Kobayashi et al. (2015; doi: 10.2151/jmsj.2015-001) is a better reference than those given for JRA-55. See also comment 44 on Chapter 1. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37454	40	1	40	2	The behaviour of MERRA-2 is not explained correctly. MERRA-2 does not cool relative to ERA-Interim and JRA-55 "over recent years". Instead, its temperature drops sharply at one particular time in 2007, and after that varies over time similarly to ERA-Interim and JRA-55 again. So MERRA-2 can still be used to provide an estimate of interannual variability after 2007, but should not be used to calculate trends over a period that includes 2007. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text amended.
37452	40	1			GMST must be changed to GSAT. GSAT is what was compared. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text amended.
37456	40	3	40	4	Change "GMST" to "GSAT" in line 3, and change "those" to "GMST trends" in line 4. The second of these changes need not be made if the conventional datasets are interpreted (as Hansen intended for GISTEMP) as approximations for GSAT. See comments 5, 6 and 7 on the entire report. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The GMST/GSAT discussion in general has been redrafted to reflect that the division between "GMST" and "GSAT" data sets is not as clear-cut as suggested in the FOD text. On the one hand, the reanalyses are nominally GSAT, but given the role of SSTs in forcing the model are conceptually similar to applying an inflation factor to the SST analysis; on the other hand, the use of NMAT in some SST homogenisation may alias an element of GSAT into nominally GMST analyses.
37458	40	3	40	4	Nowhere in this chapter does the fact that GMST/GSAT differences have been documented for full-observing-system reanalyses (Cowtan et al. 2015 show a supplementary result only for a surface-pressure-only reanalysis). Either here, with a new sentence near the end of line 4, or in the following paragraph, it could be noted that Simmons et al.(2017) reported some reanalysis results for GMST as well as GSAT, and that these show that small differences between GSAT and GMST anomalies, but a slightly higher trend for GSAT than GMST, in qualitative agreement with the published results from model simulations. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
53322	40	9	40	9	As far as I can see, this is the first time (expect from in the ES) that GSAT is mentioned. It comes a bit abruptly and may need a bit more intro. [Jan Fuglestedt, Norway]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49106	40	9	40	9	This does not say why it is necessary to provide an estimate of GSAT from the available GMST estimates in order to aid later chapters. This point is now very dispersed across chapters and could usefully be nailed early on, even in chapter 1 (on which I have also commented). [Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
37460	40	9	40	33	The distinction between GMST and GSAT needs to be explained much earlier in the chapter, since some references to GSAT as well as GMST are needed earlier. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
8542	40	9	40	33	The case for preferring GSAT over GMST needs to be presented. [Robert Kopp, United States of America]	Accepted. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
42956	40	9	40	33	The GMST to GSAT adjustment is most welcome and I commend the authors for their rigorous treatment of the topic. This is great improvement over the approach in SR1.5 chapter 2, where GSAT estimate entailed a model-based adjustment on HadCRUT4 for both coverage and SST-SAT difference. It is also very good news that all conventional datasets are expected to have enhanced spatial coverage by the time of the next draft. [David Clarke, Canada]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
7160	40	9	40	33	To justify this large paragraph on marine data, it would be useful to remind readers that the oceans are 70% of the Earth's surface, and that adjustments to marine data are much more important than for land areas. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text modified.
53318	40	9	40	49	This presentation of the two temp definitions is important. Please continue to coordinate this closely with the other chapters. [Jan Fuglestedt, Norway]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37462	40	11	40	12	The definition of GMST is wrong. For the conventional observationally-based datasets the merging is applied to anomalies not absolute values, and anomalies in MAT are generally more similar to anomalies in SST than are the corresponding absolute values. The case of frozen sea must be discussed. When we calculate a GMST for reanalysis we use air temperature over sea-ice, and in GISTEMP it is anomalies over land that are extrapolated over sea-ice. In contrast the kriging infilling of HadCRUT4 by Cowtan and Way mixes both land and sea values from HadCRUT4 in constructing values over sea-ice and other regions where data are missing in HadCRUT4. The treatment of variations over time (seasonal and long-term) in sea-ice cover, which can mean changes in the proportions of SST and LSAT data used in the blending, is not well defined. And the merging near coastlines is not well defined: HadCRUT4 gives preferential treatment to land values from coastal or island stations when it does the merging. In short, GMST is not a well-defined quantity when it comes to detail. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The wording has been generalised to reflect the fact that different datasets handle temperatures over sea ice in different ways (and that the GMST/GSAT division in general is not as clear-cut as implied by the FOD wording)
35600	40	11	40	13	Clarify whether GMST includes SAT over ice or SST under ice, or is by definition masked out over sea ice. The present text does not say anything about what measurements are included in regions of sea ice. Cowtan (2015) put substantial emphasis on a definition of GMST as including SAT over sea ice, but it is not clear to me that GMST actually is defined in this way. [Nathan Gillett, Canada]	Taken into account. The wording has been generalised to reflect the fact that different datasets handle temperatures over sea ice in different ways (and that the GMST/GSAT division in general is not as clear-cut as implied by the FOD wording)
35598	40	12			The meaning of 'nominal depth' is not clear. Be more specific e.g. 'at a depth of less than x cm below the surface' or similar. [Nathan Gillett, Canada]	Taken into account. The word "just" was added to "below the surface". In practice the actual depth will vary between different types of data collection (one of the causes of biases between different data collection methods which are dealt with in the process of developing SST data sets) so an exact depth cannot be stated.
35602	40	14	40	15	The text needs to explain that this is because there are insufficient measurements of SAT over ocean. Also 'lower confidence' should not be italicised, since it is not calibrated uncertainty language. [Nathan Gillett, Canada]	Taken into account. This is covered by a separate annex on modes of variability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57324	40	14	40	15	"There will always be lower confidence in estimates of GSAT than of GMST." This is a key finding, and should be elevated to the Exec Sum and SPM. To be fair, it is in the ES, but not as clear as here. It should also be stated somewhere that there will always be lower confidence in globally infilled estimates than in estimates of average warming over regions in which observations exist, so the use of infilling is a compromise between policy relevance and confidence. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
35604	40	17	40	18	Given its importance for the estimate of historical warming, please provide some references and more discussion for air temperature over the oceans being expected to warm slightly faster than SSTs 'based upon adiabatic theory'. As far as I could see this theory is not discussed in any of the three references cited in this sentence. This behaviour is clearly seen in models, but in order to arrive at a full assessment of its robustness it would be helpful to understand whether there is a sound theoretical basis for this, or whether it might be just a feature of the model boundary layer schemes. [Nathan Gillett, Canada]	Taken into account. Additional text is used to explain this in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37464	40	18	40	33	The GMST/GSAT differences of around 6% (3% to 7%) and the subsequently applied adjustment may be on the large side. This is already hinted at in the FOD with the discussions of NMAT data. Further evidence comes from the uncited GMST/GSAT comparisons made for reanalyses. Simmons et al. (2017) showed that trends from 1979-2016 rounded to two decimal points were 0.17°C/decade for both GMST and GSAT from ERA-Interim, although the GSAT trend was slightly larger. For JRA-55 the rounded trends were 0.17°C/decade for GSAT and 0.16°C/decade for GMST. For more precision, I have calculated trends for 1979 to 2018, and taken values to three decimal points. The results are as follows: for ERA5 we have a GSAT trend of 0.182°C/decade and a GMST trend of 0.179°C/decade, for ERA-Interim we have 0.183°C/decade(GSAT) and 0.178°C/decade(GMST), and for JRA-55 we have 0.178°C/decade(GSAT) and 0.171°C/decade(GMST). Corresponding GMST trends from some of the conventional datasets used in the FOD range from 0.164°C/decade to 0.188°C/decade. In summary, the percentage reduction in the 40-year trend from reanalysis ranges from 5% for JRA-55, through 3% for ERA-Interim to 2% for ERA5. The GSAT/GMST differences for the reanalyses are much smaller than the spread between the various traditional GMST datasets, though the latter spread should come down for the new datasets promised for the SOD. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
35606	40	30			Provide references and assessment of the 'robust theoretical basis' for GSAT warming more rapidly than GMST. [Nathan Gillett, Canada]	Taken into account. Text on this aspect added for SOD.
37466	40	31	40	32	Applying a simulation-based adjustment to GMST to produce a GSAT equivalent that is then used in later chapters to compare, inter alia, with model simulations is open to question, particularly as lower values are suggested by reanalyses. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.
18044	40	31	40	48	There seems to be a rounding error in some of the numbers. Inflation of GMST to obtain GSAT is given as 5.8% on line 31, however, it's then rounded to 1.05 in line 45, when it should be 1.06. It's a small difference, but it is consistent with the later estimation of GSAT in line 48, which is given as 1.07. [Gwenaëlle GREMION, Canada]	Taken into account. There is a specific cross-chapter box for GMST-GSAT issues in SOD, in which these matters have been considered.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57322	40	33	40	33	The emphasis on in-filled datasets seems risky, remembering what happened with the infilled hemispheric temperature paleo-climate records in AR3. Since it is much easier and more robust to sample models where observations exist rather than trying to fill in observations where they don't, should this not be mentioned as a better alternative? [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We note the potential risks (and note the relevant uncertainties in the text). More infilled data sets are being used in SOD than was the case in FOD.
29536	40	35	40	35	I suggest to not only mention GMST, but also link to regionally varying patterns, probably explained in more detail in chapter 10? [Katja Matthes, Germany]	Rejected. Regional-scale information is generally outside the scope of this chapter. As the reviewer notes, it is within the brief of other chapters.
42958	40	35	40	39	The derivation of the uncertainties for the "period" estimates (e.g. 1850-1900 to 2009-2018) is unclear. Are they HadCRUT4 uncertainty range applied to provide a lower and upper bound to the range of three estimates found in table 2.3? [David Clarke, Canada]	Noted. The overall uncertainty range reflects the values and stated uncertainty for each individual data set.
12630	40	35	40	41	Note here that the rate of warming is accelerating and that many of the warmest years on record have occurred in recent years. The rate of global annual temperature increase has more than doubled in recent decades to 0.17 °C per decade. The rate of CO2 concentration in the atmosphere also is accelerating, growing to a rate of 2.48 ppm/year in 2018; for comparison, the average increase of CO2 in the 1980s was about 1.6 ppm/year and 2.2 ppm/year during the last decade (2008–2017). The accelerating warming is being driven not only by continuing emissions, but also by self-reinforcing feedbacks. Xu Y., et al. (2018) Global warming will happen faster than we think, NATURE, Comment 564:30–32; National Oceanic and Atmospheric Administration (NOAA), Global Climate Report - Annual 2018 (last accessed 21 March 2019) ("During the 21st century, the global land and ocean temperature departure from average has reached new record highs five times (2005, 2010, 2014, 2015, and 2016), with three of those being set back-to-back. From 1880 to 1980, a new temperature record was set on average every 13 years; however, for the period 1981–2018, the frequency of a new record has increased on average to once every three years. Nine of the 10 warmest years (listed below) have occurred since 2005, with the last five years (2014–2018) ranking as the five warmest years on record. The year 1998 is the only year from the 20th century among the ten warmest years on record, currently tying with 2009 as the ninth warmest year on record. The yearly global land and ocean temperature has increased at an average rate of 0.07°C (0.13°F) per decade	Noted. Warming amounts for a range of timespans are already quoted in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12784	40	35	40	41	Note here that the rate of warming is accelerating and that many of the warmest years on record have occurred in recent years. The rate of global annual temperature increase has more than doubled in recent decades to 0.17 °C per decade. The rate of CO2 concentration in the atmosphere also is accelerating, growing to a rate of 2.48 ppm/year in 2018; for comparison, the average increase of CO2 in the 1980s was about 1.6 ppm/year and 2.2 ppm/year during the last decade (2008–2017). The accelerating warming is being driven not only by continuing emissions, but also by self-reinforcing feedbacks. Xu Y., et al. (2018) Global warming will happen faster than we think, NATURE, Comment 564:30–32; National Oceanic and Atmospheric Administration (NOAA), Global Climate Report - Annual 2018 (last accessed 15 June 2019) (“During the 21st century, the global land and ocean temperature departure from average has reached new record highs five times (2005, 2010, 2014, 2015, and 2016), with three of those being set back-to-back. From 1880 to 1980, a new temperature record was set on average every 13 years; however, for the period 1981–2018, the frequency of a new record has increased on average to once every three years. Nine of the 10 warmest years (listed below) have occurred since 2005, with the last five years (2014–2018) ranking as the five warmest years on record. The year 1998 is the only year from the 20th century among the ten warmest years on record, currently tying with 2009 as the ninth warmest year on record. The yearly global land and ocean temperature has increased at an average rate of 0.07°C (0.13°F) per decade	Noted. Warming amounts for a range of timespans are already quoted in the text.
17926	40	35	40	49	There is a lot of repetition from e.g., Chapter 1. This unnecessarily lengthens the whole Report. The comment is rather general. [Branko Grisogono, Croatia]	Noted. Discussions at LAM3 should reduce the amount of duplication in SOD compared with FOD.
53320	40	35	40	49	I suggest that these GSAT and GMST numbers for the various period are presented systematically in a table. That would be so usefue! given the potential for confusion when there are two closley related but still sinificantly different definitions of such a key variable, [Jan Fuglestedt, Norway]	Taken into account. A table is presented in SOD.
28782	40	35	40	49	Can you make a confidence assesment of GSAT (and GMST)? [Piers Piers Forster, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It is unclear what is being asked for here - uncertainty ranges (which are already quoted) or the use of uncertainty language (which we believe to be unwarranted given the certainty of the sign of the change).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18046	40	36	40	36	"Each of the last four decades has been warmer than any decade that preceded it" include in specific period, e.g CE. Reads like they could be the warmest decades in history of earth. [Gwenaelle GREMION, Canada]	Taken into account. 'In the instrumental period' added to the end of the sentence to make clear what the decadal statement applies to.
23730	40	36	40	36	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23732	40	40	40	40	Don't split numbers and units across lines [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
7840	40	43	40	43	Figure 2.13, wrong figure? Temperature? Precipitation? [zhian zuo, China]	Unanswerable - could not identify where the comment refers to in the text.
23734	40	43	40	43	Insert (GSAT) after 'Temperature' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The report has been edited to a consistent style for the SOD
35612	40	43	40	46	According to Cowtan (2015) the effect of limited observational coverage, and the effects of using SSTs over oceans vs SAT have comparable effects on long-term trends in GMST versus globally-complete GSAT. But the effect of masking is not mentioned here, nor is it apparently included in the correction factor applied to GMST. Clarify. Is the targeted GSAT metric masked, or globally-complete? [Nathan Gillett, Canada]	Taken into account. Updated versions of most of the temperature datasets used are largely globally complete, which largely addresses this issue. There is further explanation in the SOD text.
37468	40	43	40	48	Again, the adjustment factor applied in this paragraph may be on the high side. But even a 5% adjustment factor translates to only an extra .05°C temperature rise from 1850-1900 to the present day. Note the other uncertainties discussed in comment 91, for which no adjustment is proposed. If 1850-1900 can be accepted as an approximation for 18th century temperature, then GMST can be accepted as an approximation for GSAT. Both have about the same implication for the error they introduce in estimating the date at which 1.5°C or 2°C warming is reached. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Whilst the reviewer is correct that the difference between GMST and GSAT is relatively small, it is known, systematic and quantifiable to a reasonable degree of certainty, and we therefore believe that its explicit treatment is warranted, notwithstanding the fact that other potentially larger uncertainties exist.
7260	40	43	40	49	This discussion of adjustments done in this way unnecessarily opens a huge hornet's nest. It is only necessary to combine land and sea temperature data when comparing to proxy data. One of the strengths of past reports is that the independent land and sea data show comparable patterns. They should not be combined in most circumstances. As it is, it appears that these "adjustment factors" are as large as the warming signal in the last century. I understand why this is not a concern, but many will not. [Bryan Weare, United States of America]	Rejected. The principal purpose of reporting on GSAT is for consistency with later chapters, as it is the primary variable used in climate model simulations. The reviewer statement that the adjustment factors are as large as the warming in the last century is not correct (possibly he has confused the 1.05 inflation factor, i.e. 5%, with 1.05 C?).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7842	40	43	41	52	the conclusion in line 43-44 page 40 and the conclusion in line 29-30 in page 41 are conflicted [zhiyan zuo, China]	Rejected. The p40 text refers to the surface layer over the oceans, p41 to the upper atmosphere.
35610	40	44	40	45	Provide references for these physically well-understood differences (in the rate of warming of SSTs and overlying air) to maintain marine boundary layer stability. [Nathan Gillett, Canada]	Taken into account. Additional text is used to explain this in SOD.
57410	40	45	40	45	Please provide a reference in which the estimation of the factor 1.05 is described. [Marc Schröder, Germany]	The citation (Richardson et al. 2018) is provided earlier (lines 19-21)
35608	40	45	40	46	Provide references and explanation for this adjustment factor and its uncertainties. In the current draft this number is just quoted without any explanation of its source. [Nathan Gillett, Canada]	The citation (Richardson et al. 2018) is provided earlier (lines 19-21)
49940	40	47			The GSAT change for 1850-1900 to 2009-2018 is reported as 1 C. But this value requires two decimal places (i.e. 1.00 C) so that it is consistent with the uncertainty, which is reported to two decimal places. [Owen Cooper, United States of America]	Accepted. Appropriate precision to be provided in SOD.
35614	40	51	40	52	There is no traceable account of the basis for the assessment that it is virtually certain that DTR decreased over the 1951-2012 period. Pg 38, ln 52 says only that 'Post-1950 there was broad agreement that DTR had decreased globally', which is not enough to support a 'virtually certain' assessment. The AR5 assessment was 'Confidence is medium in reported decreases in observed global DTR, noted as a key uncertainty in the AR4.', so this is an area where a much stronger assessment is being made than in the AR5. This needs a discussion of the basis of the much higher likelihood/confidence, and the authors could also consider including a figure on this. [Nathan Gillett, Canada]	Taken into account. Whilst this assessment is largely based on a single pair of papers (Thorne et al 2016a/b), supported by Sun et al 2018, the Thorne et al papers assess a broad range of different datasets, all of which give results consistent with the finding. The much stronger finding reflects the fact that at the time of AR5, there had been no new results published since AR4, meaning a weak confidence assessment there.
42960	41	3	41	12	Table 2.3 (1) 1850-1900 baseline: SR1.5 was careful to include all datasets in all GMST trend estimates. This requires an adjustment for NOAA and GISTEMP datasets. For the period estimates e.g.1850-1900 to 2006-2015, SR1.5 used trend ratios, whereas the anthro warming was based on matching average over 1880-2017. A more suitable technique is found in WMO climate report 2018. There NOAA and GISTEMP are matched to HadCRUT4 over 1880-1900 to create a pseudo 1850-1900 baseline. The same technique should be used here except of course NOAA and GISTEMP would be matched to the average of HadCRUT4, Berkeley Earth and Cowtan & Way over 1880-1900. [David Clarke, Canada]	Rejected. An intentional decision was made not to use trend ratios to report NOAA and GISTEMP from 1850 as this provided no additional information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42962	41	3	41	12	Table 2.3 (2) Trend estimation: AR5 ch 2 Box 2.2 outlined problems with long term linear OLS trends and presented an alternative in the form of smoothing spline. AR5 Box 2.2 Table 1 shows 9% trend increase for smoothing spline versus OLS for HadCRUT4 over 1901-2012. Visser et al (2018; doi:10.5194/cp-14-1-2018) shows a broad range of increases relative to OLS applying Integrated Random Walk (IRW) or smoothing spline (df = 7) to the five conventional datasets over 1880-2016. Increases ranged up to ~0.1C (Cowtan & Way), albeit with no increase in NOAA and a smaller increase in HadCRUT4. Clarke (2019, in preparation) examines "hybrid" trends to 2018, based on LOESS multi-decadal relative to 1850-1900 baseline (smoothing span +/- 20 years, polynomial degree = 1) and finds an average rise since 1850-1900 of 1.11C for three full global datasets (Berkeley, Cowtan & Way and NASA GISTEMP) versus 1.03C OLS trend over 1880-2018. [David Clarke, Canada]	Rejected. A decision has been made to retain the OLS trend calculation method with AR(1) correction. While alternative methods clearly exist all have their own issues. Time series which are clearly non-linear can be handled through judicious selection of time periods, or using delta rather than trends, but the GMST time series is not sufficiently non-linear to require such treatment.
42964	41	3	41	12	Table 2.3 (3) Trend estimation validation: Clarke (2019, in preparation) finds LOESS multi-decadal and smoothing spline outperform OLS-since-1880 when validated against 20 and 30 year average relative to 1850-1900 baseline. LOESS multi-decadal and smoothing spline are also much more consistent with the "latest decade" trend than the OLS-since-1880 method. The lack of consistency can be very clearly seen in comparing the two spatially complete series 2009-2018 estimates to OLS-since-1880. Since the decadal estimate is centred at the end of 2013, the equivalent 2018 point estimate would be 0.1C degree higher (assuming 0.2C/decade rise following SR1.5 chapter 2, Figure 2.2). For Cowtan & Way and Berkeley Earth that implies gap of 0.1C and 0.08C respectively between the two trend estimation methods, almost identical to the gap between LOESS multi-decadal and OLS-since-1880. HadCRUT4 shows a more moderate gap, but this will likely change in future, assuming a more spatially complete version is published. It should also be noted that anthro warming estimates are slightly *above* the latest decade trend estimate for all series (SR1.5 method following Hausteine et al 2017), implying an even greater inconsistency of the anthropogenic GMST metric with OLS-since-1880. [David Clarke, Canada]	Rejected. A decision has been made to retain the OLS trend calculation method with AR(1) correction. While alternative methods clearly exist all have their own issues. Time series which are clearly non-linear can be handled through judicious selection of time periods, or using delta rather than trends, but the GMST time series is not sufficiently non-linear to require such treatment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42966	41	3	41	12	Table 2.3 (4) Trend recommendation: Given clear deficiencies of OLS-since-1880, multi-decadal LOESS or smoothing spline should be used for trend estimation in long series. As noted above a fixed scale smoothing span of +/- 20 years is recommended. For smoothing spline over 1880-2018 this implies $df \approx 7.5$. [David Clarke, Canada]	Rejected. A decision has been made to retain the OLS trend calculation method with AR(1) correction. While alternative methods clearly exist all have their own issues. Time series which are clearly non-linear can be handled through judicious selection of time periods, or using delta rather than trends, but the GMST time series is not sufficiently non-linear to require such treatment.
42968	41	3	41	12	Table 2.3 (5) Trend uncertainties: Both LOESS and smoothing spline methods (like OLS) compute uncertainties under assumption of independent, identically distributed errors, necessitating correction for autocorrelation of residuals. For annual series, an AR(1) model corrects for autocorrelation of the residuals (IPCC AR5 Box 2.2; Visser et al, 2018). Alternatively, autocorrelation of trend residuals of monthly series can be corrected with an ARMA(1, 1) model (Clarke 2019, in preparation). Where available, observational trend uncertainties should also be calculated from ensembles provided by the data analysis providers, following SR1.5 Table 1.1. [David Clarke, Canada]	Rejected. A decision has been made to retain the OLS trend calculation method with AR(1) correction. While alternative methods clearly exist all have their own issues. Time series which are clearly non-linear can be handled through judicious selection of time periods, or using delta rather than trends, but the GMST time series is not sufficiently non-linear to require such treatment.
42970	41	3	41	12	Table 2.3 (6) Table structure: The three trend columns (1880-2018, 1960-2018 and 1980-2018) should remain, but with entries replaced with the appropriate non-linear trends (LOESS or smoothing spline) as recommended above. The reanalysis trends could be moved to a separate table, or else left as linear trends. It should be noted that the non-linear trends and OLS trends are virtually identical over 1980-2018 in any case. An additional column, "1850-1900 to 2018" should be added to the right of "1850-1900 to 2009-2018". This would show the non-linear trend 1880-2018 but relative to the 1850-1900 baseline, rather than the rise from 1880 to 2018 per se (which is typically 0.02C or 0.03C less). This metric would be more applicable than the 2009-2018 estimate in certain cases, an obvious example being the remaining carbon budget calculation in Chapter 5. [David Clarke, Canada]	Rejected. A decision has been made to retain the OLS trend calculation method with AR(1) correction. While alternative methods clearly exist all have their own issues. Time series which are clearly non-linear can be handled through judicious selection of time periods, or using delta rather than trends, but the GMST time series is not sufficiently non-linear to require such treatment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42972	41	3	41	12	Table 2.3 (7) Spatial coverage: The FOD notes more than once that all datasets are expected to be spatially complete by the next draft. We take this to mean extended coverage to at least 1200 km from existing observations, implying 80-90% coverage over 1880-1900 and 99%+ coverage from 1951 on. Currently three of five datasets meet this criterion. Unless all five do in fact meet this criterion, the "full global" average should be broken out as a separate row just above the "Average" row, and reported alongside the five-dataset averages in any summaries. [David Clarke, Canada]	Taken into account. Whilst the comment's definition of "spatially complete" is somewhat arbitrary, the data set which is clearly not globally complete (NOAA) is excluded from averaging in the table. This is explained in the caption.
42974	41	3	41	12	Table 2.3 (8) Berkeley Earth: There is a clear discrepancy between the area-weighted average of the Berkeley Earth gridded dataset and the published annual average series. The discrepancy over 1850-1900 is ~0.04C and can not be explained by any alternative averaging scheme. There is no discrepancy whatsoever however over 1955-2018, suggesting the issue is an error in averaging spatially incomplete data rather than the analysis itself. It is therefore recommended that the Berkeley Earth gridded dataset be used pending resolution of this issue. [David Clarke, Canada]	Taken into account. Gridded averages are used in SOD using a standard methodology across all the GMST data sets. The reviewer is correct that there is a difference between the values reported by Berkeley Earth and those calculated directly from the grids for SOD.
43866	41	6	41	8	Trend should be expressed as change (here in °C) per time (year, decade, other time). If the trend is per full period of data it should be clearly indicated in the table title [Joanna Wibig, Poland]	Taken into account. Table caption modified.
18048	41	6	41	9	Table 2.3 does not present confidence ranges for total changes in temperature (columns 3 and 4), nor to the average trends (column 4 and 6, row 7). [Gwenaëlle GREMION, Canada]	Taken into account. The confidence ranges are quoted in the text (p40 lines 35-41). The text includes both the confidence intervals for individual data sets and an indication of structural uncertainty given by the spread between data sets.
27174	41	6	41	11	This expert reviewer recommends adding a column from 1945, beginning of acceleration of CO2 emissions, to 2018 [François GERVAIS, France]	Rejected. The range of periods already presented is considered to be sufficient. Information on other time periods can be obtained from the time series and decadal means presented in Figure 2.12.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37470	41	10	41	11	How were the calculations shown in Table 2.3 for ERA-Interim and JRA-55 made. Are they really GMST not GSAT? Were (as discussed in Simmons et al., 2017) analysis values used over land but background values used over sea, and were ERA-Interim values adjusted for the well understood inhomogeneity in its SSTs? How was the uncertainty range estimated? It is surprising that a larger uncertainty range is quoted for ERA-Interim than for HadCRUT, which has uncertainty due to its limited spatial coverage. What is the explanation? Will ERA-Interim be replaced by ERA5 for the SOD? ERA5 does not need the adjustments that should be applied to ERA-Interim. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The GMST/GSAT discussion in general has been redrafted to reflect that the division between "GMST" and "GSAT" data sets is not as clear-cut as suggested in the FOD text. On the one hand, the reanalyses are nominally GSAT, but given the role of SSTs in forcing the model are conceptually similar to applying an inflation factor to the SST analysis; on the other hand, the use of NMAT in some SST homogenisation may alias an element of GSAT into nominally GMST analyses.
37472	41	10	41	11	"Change" would be better than "Trend" in the header line of the Table. Or change table entries to rates. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Table caption modified.
31308	41	15	43	55	In the chapitre " 2.3.1.1.4 Temperatures during the instrumental period – free atmosphere" it might be worthwhile to also add the results from a recent paper by Philipona et al., 2018. This paper compares different radiosonde datasets and satellite measurements in the troposphere and lower stratosphere, and was written in collaboration between members from the GRUAN, the radiosonde homogenization and the satellite communities. The citation of the paper is: Philipona, R., Mears, c., Fujiwara, M., Jeannet, P., Thorne, P., Bodeker, G., et al. (2018). Radiosondes show that after decades of cooling, the lower stratosphere is now warming. J. Geophys. Res: Atmospheres, 123. https://doi.org/10.1029/2018JD028901 . [Rolf Philipona, Switzerland]	Taken into account. Whilst the results in this paper are broadly consistent with the lack of significant post-2000 trends reported at p43 lines 3-6, this has been made explicit in the revised text.
23736	41	17	41	17	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
14502	41	19	41	21	Here observations are being assessed. Why do you turn to modeling and the bias of simulation? This should be the business of other chapter. (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. The reference to models here is to provide context as to why it is important to assess this variable observationally.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31310	41	23	41	30	To this paragraph the following sentence could be added: Recent comparisons made between different radiosonde and satellite data sets from 1979 to 2015 showed that temperature trends in the lower stratosphere are similar in the different datasets before the turn of the century, but that radiosondes show minor warming after 2000 while satellite measurements show no trends or rather cooling after 2000 (Philipona et al., 2018). [Rolf Philipona, Switzerland]	Rejected. This is covered at p43 lines 3-6. Since none of the datasets show significant trends post-2000 we do not think it necessary to distinguish small differences between datasets in non-significant trends.
37474	41	27			This reference to reanalysis problems near the 300hPa level needs qualification. Firstly, although it is indeed a problem that afflicts older reanalyses, lessons have been learnt and the biases of aircraft observations have been corrected before assimilation in MERRA-2 and ERA5. JMA did not have a suitable bias correction scheme at the time it started production of JRA-55, so it avoided the problem by simply not using any aircraft temperature data. Secondly, as discussed in Simmons et al. (2014; doi: 10.1002/qj.2317), the problem only affects ERA-Interim in regions and at levels where there are dense observations from aircraft. Elsewhere radiosonde and satellite data dominate aircraft data in constraining the data assimilation. Thus at the 300hPa level the problem prior to 2014 manifests itself predominantly over North America and to a lesser extent Europe. China may show up more today. At 200hPa one sees more along the transatlantic and Pacific flight paths. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In SOD, ERA-Interim is replaced by ERA5 and the discussion reflects that.
18050	41	35	41	36	It would be useful to provide approximate height ranges for the different layers both in the text and as background or other indication in Figure 2.13. Lines 25 to 45 in page 42 would be very hard to understand for someone with no intuitive feel of what those layers are. [Gwenaëlle GREMION, Canada]	Taken into account. This is best expressed through modifications to Fig. 2.12.
50254	42	1	42	1	what are the "two groups"? [Sophie SZOPA, France]	Taken into account. In fact two is an error - there are three (UAH, RSS, NOAA). Text modified to just say "groups".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57412	42	5	40	23	To me this paragraph is not in balanced with other, similar paragraphs. Thus, I propose to shorten this paragraph, e.g., into: "Global Navigation Satellite Systems (GNSS, such as e.g. GPS) radio occultation (RO) limb soundings provide a self-calibrated SI traceable satellite measurement (Anthes, 2011; Ho et al., 2010), yielding globally distributed, high vertical resolution profile information in the upper troposphere and stratosphere from the early 2000s (e.g. Angerer et al., 2017a; EUMETSAT, 2019). The measured time delay can be converted to temperature, water vapour density and other parameters (Kursinski et al., 1997; Scherllin-Pirscher et al., 2017); this processing does however add structural uncertainty to the data (Ho et al., 2012; Steiner et al., 2013). Data sets have been compared against MSU/AMSU channel 4/9 data records, as well as against several AMSU channels from the Aqua satellite, finding almost identical trends (Khaykin et al., 2017). Comparison of RO with collocated radiosondes, Vaisala RS90/92 and GRUAN data product (RS92-GDP), showed very good agreement with global annual mean temperature differences of less than 0.2°C in the upper troposphere and lower stratosphere." [Marc Schröder, Germany]	Rejected. We believe that it is warranted to cover the use of RO data in some depth as these data have not been used in detail in previous IPCC reports.
23738	42	10	42	10	Insert , after 'does' and after 'however' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37476	42	20	42	21	RO data also provide important data for assimilation in reanalysis systems, where they anchor bias corrections of AMSU and other types of observation. If you decide to add this point, a recent review paper by Ho et al. that has been accepted by BAMS subject to minor revision would serve as an up-to-date reference. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text will be reworded and reference added if available.
31312	42	20	42	23	One might be careful with this sentence. In our analysis we did not find agreement between radiosonde and satellite temperature trends in the lower stratosphere after 2000. [Rolf Philipona, Switzerland]	Rejected. This section is referring to RO observations which are not covered in the Philipona et al 2018 paper. (If it is a reference to other parts of the text, the difference between global-scale radiosonde and satellite trends reported in that paper is not significant, although it is in some regions).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31314	42	25	42	45	Our analysis shows temperature trends in the troposphere from the different radiosonde data sets that are similar to the trends reported in this paragraph. In the tropics the trends are on the order of 0.12 °C/dec before 2000 and 0.20 °C/dec after the turn of the century. Hence, they confirm a troposphere temperature trend increase in the tropics since 2000. [Rolf Philipona, Switzerland]	Noted. These results are consistent with the existing text.
35616	42	25			Replace 'warming continues' with 'warming has continued' because this is a statement about observed trends over the 1980-2018 period. [Nathan Gillett, Canada]	Accepted. Text modified.
18052	42	27	42	27	The reference to figure 2.13 doesn't specify which panel to look at. [Gwenaelle GREMION, Canada]	Rejected. Which parts of the figure are relevant should be clear from the caption (this will be more obvious in the final report when the figures are alongside the text). In any case this figure was substantially redrafted and simplified for SOD.
18054	42	27	42	44	Figure 2.13 has no uncertainty estimates so it is not possible to judge the magnitude of the difference between Radiosonde-based products and satellite-based products either in the lower troposphere (lines 27-28) or the upper troposphere (lines 42-45). [Gwenaelle GREMION, Canada]	Taken into account. Uncertainty estimates are provided where possible in SOD.
37478	42	29	42	31	Net warming of the tropical upper troposphere at a rate faster than net warming of tropical surface air was also demonstrated for ERA-Interim reanalysis data by Simmons et al. (2014; doi: 10.1002/qj.2317), who also noted a consistent signal in specific humidity and discussed fits to satellite as well as radiosonde observations. But this tropical amplification is quite a well-known feature (albeit with uncertainties in amplitudes - the "low confidence" expressed on line 21 of page 41 is appropriate). It has been discussed in several papers since AR5 as well as before, so it is puzzling to see the paper by Sherwood and Nigant quoted without linking it with earlier findings and understanding. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. FOD was cautious in the use of ERA-Interim given the identified issues with its temperatures near the 300 hPa level (critical in this context). We note the expectation (discussed earlier) that these issues have been largely addressed in ERA5 (once issues with the initial run are resolved) and anticipate giving reanalysis data more prominence in the final report.
18056	42	31	42	32	Here height is defined using hPa. This is not consistent with the rest of the text and, more importantly, not consistent with the figure. A reader not used to converting hPa to km would not know where to look. This could be solved by either using km in the text or providing a secondary y axis in hPa units. [Gwenaelle GREMION, Canada]	Taken into account. Pressure levels are a standard key of defining heights in upper atmosphere analysis but it is noted that many readers will be unfamiliar with the hPa to km conversion. We have done this through the figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23740	42	39	42	39	Edit for clarity: 'tropics, the lowermost Stratosphere is also warming' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Wording was changed.
35676	42	42	57	22	I suggest that the authors refer to and cite Karpechko et al. (2018) - WMO Ozone Assessment 2018, for another recent assessment of changes in the BDC. Karpechko et al. conclude in part that 'New studies using measurements provide evidence for structural changes in the stratospheric overturning circulation which is comprised of a strengthening in the lower stratosphere and a weakening in the middle and upper stratosphere.' (https://www.esrl.noaa.gov/csd/assessments/ozone/2018/report/Chapter5_2018OzoneAssessment.pdf) [Nathan Gillett, Canada]	Not applicable. BDC sub-section no longer included in Chapter 2.
37480	42	45	42	45	The sentence spanning these lines is misleading. As pointed out in comment 108 the latest reanalyses make allowance for biases in aircraft data, and in earlier reanalyses the aircraft temperature bias problem predominantly affects northern hemispheric regions and levels where the density of aircraft flights is high. There are plentiful satellite data to constrain temperature in the upper tropical troposphere (although analysing the sharp high tropical tropopause is a challenge, especially prior to the availability of RO data, and bias correction of the satellite radiance data is an ever-present issue). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This wording reflects the state of reanalyses at the time FOD was written (i.e. not including ERA5). If ERA5 has addressed issues with temperatures near the 300 hPa level (once initial biases are dealt with through ERA5.1) then we will be able to make a stronger assessment in later drafts. Whilst it may indeed be the case that aircraft measurements are densest at higher northern hemisphere latitudes, so are radiosondes, so it does not necessarily follow that aircraft are a smaller part of the observations mix at low latitudes.
57414	42	47	43	1	In Xian and Homeyer (2018) time series of tropopause height is not shown. It would support the consistency between radiosonde and GPS RO data if this time series is analysed in more detail, i.e., by assessing whether or not the actual trend occurs prior to 2006 and whether or not changes after 2006 are non-significant. [Marc Schröder, Germany]	Taken into account. IPCC can only draw on results (or data sets) which have been reported in the literature; it is regrettable that Xian and Homeyer 2018 did not report a time series (although note that this paper is still under review at the time of writing). Weak post-2006 changes in RO data are noted at p42 line 54.
7860	42	52	42	52	figure2.14, temperature humidity? [zhiyan zuo, China]	Rejected, unanswerable. This comment is unclear (and in any case does not relate to the quoted page/line number).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18058	42	52	42	53	This line references a slowdown of the rate of cooling of the lower stratosphere that hasn't been discussed up to that point. Maybe this whole paragraph could be moved? [Gwenaelle GREMION, Canada]	Rejected. The slowdown of the lower stratospheric cooling is discussed in the immediately following paragraph (the split across pages makes this less obvious).
18060	43	3	43	3	The period analysed in Figure 2.13 is 1980 to 2018, not 1979 to 2018. [Gwenaelle GREMION, Canada]	Noted. The statement in the text is correct (1979 is the first year of many of these data sets) but 1980-2018 was used in Figure 2.13 for comparability with trends reported elsewhere. In any case the 1980-2018 period is not used in the revised figure (now 2.12).
40974	43	3	43	6	This statement appears at odds with the one on P2-42 ("in the tropics, also the lowermost stratosphere is warming"). [Johannes Laube, Germany]	Taken into account. Start of paragraph reworded "Temperatures averaged through the lower stratosphere" to make it clear that the whole layer is being discussed here (a warming trend in a small part of the region does not negate this).
26922	43	3	43	6	There is no comment on the lower stratospheric temperature trends and their degree of uncertainty in the pre-satellite era (1958-1978) based on radiosonde data. Stratospheric temperature trends are important for the understanding of the LLGHGs effect on climate. There is still an open question whether stratosphere is more suitable than troposphere for the detection of man-made climate change signal from LLGHGs as in line with the theoretical expectations the equilibrium temperature in the stratosphere compared to the troposphere is more sensitive to anthropogenic GHGs and less sensitive to tropospheric water vapour and clouds (Manabe and Weatherald, 1967). Zerefos et al. (Atmos. Chem. Phys., 14, 7705–7720, 2014) pointed that the consistency of RICH temperature trends with the thickness calculated layer mean temperature trends from FU-Berlin and NCEP enhances our confidence for the cooling trend in the lower stratosphere in the pre-satellite era despite the documented trend uncertainties of the radiosonde data sets during this period (Randel and Wu, 2006; Free and Seidel, 2007; Randel et al. 2009). [Prodomos Zanis, Greece]	Rejected. 1960-2018 trends (which are drawn from radiosondes only) are discussed at p43 lines 34-37 in FOD, while the low confidence in pre-2001 details of vertical profiles is discussed at line 30.
50256	43	4	43	4	"even if" => "even when" [Sophie SZOPA, France]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18062	43	6	43	6	The period analysed in Figure 2.13 is 2002 to 2018, not 2000 to 2018. [Gwenaelle GREMION, Canada]	Rejected. This statement is true for the data sets available over this period. Figure 2.13 shows a separate view which takes advantage of the RO data (not available until 2002)
37482	43	13	42	17	The sentence spanning these lines is broadly correct, but too pessimistic. There have been more positive papers concerning what can be learnt from reanalyses in the upper stratosphere. For example, McLandress et al. (2014; doi:10.5194/acp-14-1547-2014) discuss how to remove the primary affect of unadjusted SSU-3/AMSUA-14 bias in ERA-Interim temperatures, and Simmons et al. (2014; doi: 10.1002/qj.2317) show mostly good consistency between the ERA-20CM model simulations and the ERA-Interim and JRA-55 reanalyses in the upper stratosphere, provided an adjustment of the type reported by McLandress et al. is applied in the case of ERA-Interim. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been reassessed before SOD in light of recent reanalysis developments.
27694	43	18	43	18	Unify the use of "and" in bibliographic citations [Poot Delgado Carlos Antonio, Mexico]	Taken into account. References are fully reconciled in SOD.
41138	43	18	43	24	I recommend citing results of Maycock et al. (2018) here. They show results of extended satellite data sets comprised of SSU with AMSU. Table 2 provides a summary of stratospheric trends for the three stratospheric SSU channels extended with AMSU until 2016 as well as a for the MSU4 channel for all investigated data sets. Results show that over 1979–2016 there is an increase in the magnitude of the cooling trend with height. Observations show weaker stratospheric cooling since about 1998 when ozone-depleting substances have been declining in the atmosphere. Reference: Maycock, A. C., W J. Randel, A. K. Steiner, A. Y. Karpechko, J. Cristy, R. Saunders, D. W. J. Thompson, C-Z. Zou, A. Chrysanthou, N. L. Abraham, H. Akiyoshi, A. T. Archibald, N. Butchart, M. Chipperfield, M. Dameris, M. Deushi, S. Dhomse, G. Di Genova, P. Jöckel, D. E. Kinnison, O. Kirner, F. Ladstädter, M. Michou, O. Morgenstern, F. O'Connor, L. Oman, G. Pitari, D. A. Plummer, L. E. Revell, E. Rozanov, A. Stenke, D. Visioni, Y. Yamashita, G. Zeng, (2018), Revisiting the mystery of recent stratospheric temperature trends, Geophys. Res. Lett., 45(18), 9919–9933, doi:10.1029/2018GL078035. [Andrea K. Steiner, Austria]	Taken into account. The results from this paper about the consistency between satellite and other datasets are already cited (page 42 line 2). Other results from the paper largely repeat results already reported elsewhere, either in the text or in figure 2.13. An explicit reference has been included.
23742	43	21	43	22	Don't split numbers and units across lines [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23744	43	26	43	26	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18064	43	26	43	27	There is no confidence language in the troposphere statements. [Gwenaelle GREMION, Canada]	Rejected. The view of our chapter is that this outcome is sufficiently certain that confidence/likelihood language is not warranted.
41140	43	26	43	27	"...lower tropospheric temperature has warmed at a rate of 0.13-0.23°C..." Please give a reference to these rates or to a figure. [Andrea K. Steiner, Austria]	Taken into account. These values are calculated directly from the various data sets cited in this section. Words "depending on the data set" have been inserted.
23746	43	27	43	27	Replace 'has warmed' with 'has increased' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is standard usage.
35618	43	28	43	30	The underlying text appears to support enhanced upper tropospheric warming since 1979 from a variety of observations (pg 42 In 29-36). Why only low confidence in changes prior to 2001? [Nathan Gillett, Canada]	Noted. The AR5 assessment was for low confidence throughout. The upgraded assessment to medium for the post-2002 period was due to the introduction of the radio occultation data. We are unaware of any post-AR5 work which would justify an increase in the confidence level for pre-2002 data.
23748	43	34	43	34	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
40976	43	34	43	35	This statement appears at odds with the one on P2-42 ("in the tropics, also the lowermost stratosphere is warming"). [Johannes Laube, Germany]	Rejected. The reference on p42 is to a different period (2002-2018), and does not in any case necessarily negate a conclusion at a global scale (it is possible that tropical warming could be offset by greater warming elsewhere).
35620	43	34	43	35	Is the trend and its uncertainties exactly the same for the two periods? Even if this is the case, I recommend writing the trend separately for each of the two periods referred to. [Nathan Gillett, Canada]	Taken into account. Text reworded for SOD. We have also reinstated a table of trends for different periods which should make this point clearer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31316	43	34	43	38	Our analysis shows that in the lower stratosphere temperature strongly decreased from 1979 to 2000. Averaged over the globe - except Antarctica - the radiosonde data sets show a decrease of 0.51 °C/dec and the satellites RSS a decrease of 0.46 °C/dec. However, from 2000 to 2015 the same global average shows an increasing trend of 0.14 °C/dec measured by the radiosondes, whereas the satellite data show almost no trend, RSS -0.01 °C/dec and NOAA -0.04 °C/dec. Hence, I do not think we should say that the lower stratosphere has cooled with a linear trend based cooling rate of 0.22-0.29 °C/dec from 1980 to 2018, if we clearly know that before the turn of the century the cooling is very strong and after 2000 almost no trends are measured by the satellites. Instead it would be better to say, that radiosondes and satellites measure a strong cooling from 1979 to 2000 of about 0.5 °C/dec and that radiosondes show a minor warming after 2000 which, however, is not observed by the satellites. [Rolf Philipona, Switzerland]	Taken into account. The chapter continues to use OLS with AR(1) correction, but suitable words are used in the text for assessment of time series which show clearly non-linear changes.
18066	43	35	43	36	Linear trend-based cooling rate over 1980-2018 is reported, and yet data shows stabilized temperatures/no significant cooling since the 1990s? [Gwenaëlle GREMION, Canada]	Taken into account. The chapter will continue to use OLS with AR(1) correction, but suitable words are used in the text for assessment of time series which show clearly non-linear changes.
41142	43	43	43	43	Suggest showing a comparison of the trend rates for all compared data sets in an additional subpanel. [Andrea K. Steiner, Austria]	Taken into account. We are showing a table of trend rates, in place of the time series panels in Figure 2.13, which are being removed for simplification.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37484	43	43	43	53	Fig. 2.13 includes trends for a rather short period, 2002 to 2018. Short-period trends must always be treated with caution, and they can give misleading results in particular if a dataset suffers a change during the period due to a change in the observing system. Trend over a longer period will be less sensitive to that change. This is what happens here for ERA-Interim (and would happen for ERA5 and probably any other reanalysis that assimilates RO data alongside other temperature data). There are too few CHAMP RO data from 2002 to 2006 to have much effect on the reanalysis, which utilizes other types of temperature data, but quite a big affect comes in at the end of 2006 when ERA-Interim starts to assimilate the much more plentiful COSMIC data (Poli, Healy and Dee, 2010; doi: 10.1002/qj.722), and a short-period trend that includes 2006/7 cannot be expected to give a very good result. A trend based on RO data alone may be OK, as there is nothing to compete with the CHAMP data for 2002-2006. I am unconvinced that including results from ERA-Interim in Fig. 2.13 will enlighten the reader. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Clearly short-period trends have higher uncertainties involved but this is a necessary consequence if we wish to show analyses including the RO data. We expect ERA-Interim will be replaced by ERA5 in the final report (no reanalyses are shown in SOD pending resolution of a lower stratospheric bias in the initial ERA5 run).
18068	43	43	43	53	Common vertical axis between plots should be aligned. [Gwenaelle GREMION, Canada]	Accepted.
43868	43	44	43	53	black lines (both thick and thin) in the right panel should be described in the legend [Joanna Wibig, Poland]	Accepted. This shows the tropopause height and is described as such in the caption in SOD.
18070	43	47	43	48	If Lat/altitude cross-section trends are adjusted by ENSO and QBO influence, then maybe the same should be done for the other trends (previous panels). [Gwenaelle GREMION, Canada]	Noted. ENSO/QBO influences will be less prominent over broad zonal means than they will be at specific points.
23750	44	6	44	6	Change 'a' to 'an' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23752	44	9	44	9	If you are talking about generic seasonal changes, 'monsoon' is correct. If you are referring specifically to the Asian cycle, this should be 'Monsoon' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence refers to generic monsoon characteristics.
23754	44	12	44	12	Change 'Paleo' to 'Palaeo' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - we use the IPCC style guide in SOD
13170	44	12	45	6	There is no mention of the Southern Hemisphere in this section. Include a few sentences of the current state of knowledge about hydroclimate variability in the Southern Hemisphere, and where more research is needed. [Nora Richter, United States of America]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42904	44	12	45	6	Discuss Steiger et al (2018) and the various tree-ring based drought atlas results (e.g. their Fig 3 and analyses based thereupon); given the uncertainties expressed in this section are valid. [Michael Evans, United States of America]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18106	44	12	45	6	Section 2.3.1.2.1: There is a large bias in cited papers whose first authors are from American institutions (8) vs all other nations (3) in this section. This may explain the overall bias towards discussing North American paleo hydroclimate. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
23756	44	13	44	13	Change 'Paleo' to 'Palaeo' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18076	44	13	44	27	The structure of this paragraph is difficult to follow. It begins by discussing regional-scale floods and droughts on interannual to centennial timescales (not clear if this is an AR5 conclusion?), before abruptly switching to generalized global-scale hydroclimate states over millennial to orbital timescales, and finally stating that AR5 provided no conclusions regarding large-scale hydroclimate states during the Holocene or CE. Consider adding a greater emphasis to (or outline this paragraph using) the range of temporal and spatial scales discussed in AR5 to summarize past hydroclimate change, in addition to the range of metrics used to define hydroclimate, which ultimately adds to the complexity of interpretation past large-scale states, variability, and abrupt changes (both in AR5 and moving forward). [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
23758	44	14	44	14	Change 'Paleo' to 'Palaeo' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
52334	44	15	44	24	Extremely wordy again - how can this be streamlined? Suggestion for lines 23 - 24: "A range of metrics define hydroclimate, lending complexity - these include..." [Katherine Glover, United States of America]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
23760	44	16	44	16	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
50372	44	19	44	19	P-E not yet defined [Sophie SZOPA, France]	Noted. We defined P-E in its first appearance.
23762	44	19	44	25	Move the definition of P-E from line 25 to line 19 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We defined P-E on the first appearance

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14290	44	19			Since P-E ~ 0 globally, this statement must be referring to land or regional P-E or the fact that P and E were larger so perhaps needs clarifying [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The statement was modified to clarify the changes in P-E. Given that the estimates to which this statement is addressed are essentially regional modification will put it to this context.
18078	44	20	44	21	It is not obvious if this statement is still referring to AR5 conclusions. Because of this, the statement appears to be later contradicted by the summary of hydroclimate changes during the Holocene starting on line 43. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
30530	44	29	44	34	this assessment about LGM is not proper as it does not include proper references. In fact, Williams 2014 is about climate change in deserts, while Putnam and Broecker 2017 inserted just after refer to lakes in the extratropics that have been identified wetter in the LGM. I don't see how they can refer to same issues [Annalisa Cherchi, Italy]	Taken into account. This section was extensively modified considering several suggestions and comments. New relevant references were added and a new/extended/revised interpretation was given to the results from previous references.
8756	44	29	44	40	It would also be useful to point out recent refinements in Asian hydroclimate since the LGM. While oxygen isotopes in speleothems from the East Asian Monsoon region indicate a weaker monsoon during periods (Heinrich events, Younger Dryas), the trace element reconstruction of precipitation indicate that local precipitation was higher at these times even under broadly weaker monsoon conditions. See Zhang et al 2018 DOI: 10.1126/science.aat9393 "East Asian hydroclimate modulated by the position of the westerlies during Termination I". Might also be useful for section 2.3.1.3.2. [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18080	44	30	44	31	The "LGM was drier than present" is a generalized statement that is not acknowledged as such. The proxy record indicates drier tropics and high latitudes during the LGM, while some extratropical regions (e.g. southwest USA) experienced maximum wet conditions. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18094	44	30	44	34	Consider re-arranging this argument to account for greater nuance and clarity, for example: Paleo evidence indicates sparse global vegetation cover and higher dust deposition during the LGM, which is in agreement with models and moisture-sensitive proxies suggesting an overall decrease in global precipitation relative to today, albeit regional-scale heterogeneity (Masson-Delmotte et al., 2013). Despite lower global precipitation amount, research since AR5 has indicated increased top-soil moisture in low- to midlatitudes relative to today (Scheff et al., 2017), thereby re-examining the characterization of the LGM as "dry." Low evaporation rates and increased top-soil moisture during the LGM may have contributed to elevated shorelines of large close-basin lakes located in the 35° to 40° latitudinal belts (Putnam and Broecker 2017; Scheff et al., 2017), such as the southwest US (e.g. Ibarra et al., 2018 Geology). [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
23764	44	31	44	31	Change 'vapor' to 'vapour' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18082	44	32	44	32	Inconsistent statement based on literature that is later cited: Scheff al. (2017) note that paleo researchers "should not assume hydrologic drying on the basis of vegetation decline alone." Numerous studies suggest that lower CO2 concentrations may have played a larger role. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18084	44	32	44	33	High lake levels discussed in Putnam and Broecker (2017) refer only to lakes located between 35 and 40 degrees lat. N and S, and is therefore not globally applicable (as suggested by the wording of this sentence). [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18086	44	33	44	33	Partially incorrect citation: the models used in Scheff et al (2017) produce P-E estimates but do not directly quantify lake levels (which require a close-basin setting in order to reflect regional P-E). [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18088	44	33	44	33	The statement "likely indicating greater runoff and wetter topsoils" is not a widely agreed upon conclusion, even amongst the aforementioned literature. Putnam and Broecker (2017) attribute elevated LGM shorelines to "past times of greater precipitation" in the close-basin region of the southwest USA, as opposed to greater runoff or soil moisture alone. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18092	44	34	44	34	Neither Putnam and Broecker (2017) nor Scheff et al (2017) claim increased streamflow during the LGM. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18090	44	34	44	36	This sentence is unclear to me. The phrase "large scale change" was previously used to describe continental-scale hydroclimate changes, yet here it appears to refer to boundary conditions (?), i.e. how the expansion and retreat of the Laurentide Ice Sheet influenced the trajectory and intensity of the eastern Pacific Storm track, as examined in Oster et al. (2015). [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
39296	44	34	44	40	Suggest that Ibarra et al. (2018, Geology) and Pound et al. (2014, CoP) be cited here as well for example of wet midlatitudes (during both LGM and Pliocene states) [Daniel Ibarra, United States of America]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18096	44	35	44	36	Morrill et al. (2018) GRL state "we infer that thermodynamical factors outweigh dynamical factors in determining moisture convergence by transient eddies at LGM" [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
33380	44	36			The importance of dynamic versus thermodynamic considerations is likely regionally-dependent [Juan Lora, United States of America]	Noted. Given that chapter 2 considers global changes in the hydrological cycle, we modified the assessment of the paleo aspects to try to avoid regional characteristics.
33382	44	36			These statements should cite the literature. At least over North America, many works have explored the dynamic aspect (e.g., Oster et al., 2015; Lora et al., 2017, doi:10.1002/2016GL071541). In addition, at least two papers have specifically investigated the dynamic versus thermodynamic components: Lora (2018, doi:10.1175/JCLI-D-17-0544.1) and Morrill et al. (2018, doi:10.1002/2017GL075807). [Juan Lora, United States of America]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
23766	44	38	44	38	Replace hyphen with : [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18098	44	38	44	39	Repetition of similar information, unless re-worded as "it was both globally wet and warm, particularly in the subtropics..." [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
11588	44	40	45	1	Authors write: "There is sufficient sampling during the CE to determine the "unusualness" of hydroclimate variability during the observational interval, given relatively stationary boundary conditions." An example of a very dry MCA in North Africa is then given. Palaeoclimatic mapping has revealed systematic hydroclimatic patterns for the MCA in Africa. Some regions got drier, others got wetter. The patterns and natural drivers of these changes are described in detail by Lüning et al. 2018 (doi 10.1016/j.palaeo.2018.01.025). It may be worth mentioning this continent-scale MCA study as an example for a pre-industrial baseline study. For further MCA hydroclimate references on other continents click on sites on this map: http://t1p.de/mwp . [Sebastian Luening, Portugal]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18100	44	42	44	42	While I understand the authors' message, I wonder if the word "sampling" may be confusing in a paleo context to the layman reader (one cannot sample the past). Term also used on line 49. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
39298	44	42	44	43	Is this really true? I think the speleothem community would argue differently. [Daniel Ibarra, United States of America]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
35270	44	42	44	54	There are evidences for changes in the hydroclimate for other regions, so is quite reductionist to center this paragraph on solely on North America. For instance, droughts during the MCA occurred also in some areas of South America. I won't suggest specific articles, but just searching in google scholar several articles will appear to sustain the notion that hydroclimate changed from the LGM onwards. [eugenia gayo, Chile]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39778	44	42	45	1	I suggest adding more content on the hydroclimate changes over the last millennium since there is a lot of reconstructions (PDSI,precipitation, runoff, etc.) For instance, Zuo et al. (2019) analyzed hydroclimate changes after volcanic eruptions based on large sets of PDSI and precipitation reconstructions over the last millennium, they found similar results between reconstructions, observations and model simulations. References:Zuo Meng, Tianjun Zhou*, Wenmin Man, 2019: Hydroclimate Responses over Global Monsoon Regions Following Volcanic Eruptions at Different Latitudes.Journal of Climate, 32, 4367-4385. DOI: 10.1175/JCLI-D-18-0707.1 [Meng Zuo, China]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
33384	44	42			"hydroclimate variability" here seems to be different from "variability" in line 35 above. Particularly by including the word "spatial," this is confusing. I assume the meaning here is temporal variability, but the distinction should be more clear. [Juan Lora, United States of America]	Noted. The word 'spatial' refers to the need of improvement of paleo data sampling.
44862	44	43	44	43	Typo. "to the in the Holocene" [Kaoru Kubota, Japan]	Editorial
30532	44	43	44	43	remove "in the" before "Holocene" [Annalisa Cherchi, Italy]	Editorial
50258	44	43	44	43	"in the" should be removed [Sophie SZOPA, France]	Editorial
14504	44	43	44	45	Similar change actually occurred in Northeast China, where the human activity was weak throughout the Holocene. A 1999 work (Ren, GY, 1999, Wetness changes of the Holocene in Northeast China, Geological Review, 45 (3): 255-264) showed that the climate of the early-to-mid Holocene was relatively drier, and it has been gradually become wetter since 8 ka BP. The last millennium may be the wettest period of the Holocene, and the driest period of the last ten thousand years may have occurred between 9 ka BP and 8 ka BP. This reconstruction is made on the basis of fossil pollen data, but strongly supported by the other lines of evidence from the buried soil, the bog formation, the loess deposition and black soil development and the lake evolution. (CUG, Guoyu Ren) [Guoyu Ren, China]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18102	44	43	44	46	An overgeneralization of North America without acknowledgement of regional heterogeneity. Shuman and Marsicek (2016) only refer to midlatitude North America. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7220	44	43	44	49	Is there a reason this paragraph focuses on North America and the Northern Hemisphere more broadly? What information is known (or not known) about the Southern Hemisphere? Also how is Northern Hemisphere "mid-latitude" defined? I'm thinking of regions of Asia at the limits of the monsoon boundary that have NOT gotten increasingly wet through the Holocene, but rather increasingly dry as monsoon strength has declined. [Hillman Aubrey, United States of America]	Taken into account. This section was extensively modified and shortened considering several suggestions and comments. New references were added and the text related to the previous references were re-interpreted.
18072	44	43	44	49	This section lacks information from the Southern Hemisphere. There is recent evidence for the onset of centennial-scale variability, with alternation of a series of droughts and humid phases in the mid-latitudes over the last 5800 years. Consider the following references: (1) Moreno, P.I., Vilanova, I., Villa-Martinez, R., Dunbar, R.B., Mucciarone, D.A., Kaplan, M.R., Garreaud, R.D., Rojas, M., Moy, C.M., De Pol-Holz, R. and Lambert, F., 2018. Onset and evolution of southern annular mode-like changes at centennial timescale. Scientific reports, 8(1), p.3458, and (2) Fletcher, M.S., Benson, A., Bowman, D.M., Gadd, P.S., Heijnis, H., Mariani, M., Saunders, K.M., Wolfe, B.B. and Zawadzki, A., 2018. Centennial-scale trends in the Southern Annular Mode revealed by hemisphere-wide fire and hydroclimatic trends over the past 2400 years. Geology, 46(4), pp.363-366. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18074	44	43	44	49	No information for the Southern Hemisphere. There is evidence that the mid-latitude of the Southern Hemisphere has become wetter after 8000 cal yr BP, as suggested in: Lamy, F., Kilian, R., Arz, H.W., Francois, J.P., Kaiser, J., Prange, M. and Steinke, T., 2010. Holocene changes in the position and intensity of the southern westerly wind belt. Nature Geoscience, 3(10), p.695. [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
51804	44	44	44	46	Shuman et al. 2018 also provided a synthesis on US hydroclimate over Holocene doi: https://doi.org/10.5194/cp-14-665-2018 [Anson Cheung, United States of America]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18104	44	52	44	52	Why is North America repeatedly used as an example while all other continents are ignored - does North America have the highest density of paleo data? Is it the only continent in which paleo data reveals consistent trends in paleo hydroclimate? [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46810	44	53	44	54	The MCA–LIA transition is actually not standing out as something special from the reconstructed hydroclimate. Moreover, the Monsoon Asia Drought Atlas (Cook et al., 2010) and the Old World Drought Atlas (Cook et al., 2015) should also be mentioned here. [Charpentier Ljungqvist Fredrik, Sweden]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
18112	45	3	45	6	Disconnected summary. The previous paragraph outlined large-scale hydroclimate variability and trends in the Holocene. The cited evidence uses compilations of up to 70 paleo moisture records per study to draw these conclusions. Yet rather than summarizing these conclusions with a low-confidence assignment (indicating little to no evidence(?) or agreement in studies), the conclusions are completely left out? [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments. New references were added and the results from previous references were re-interpreted
39494	45	9	45	45	This section 2.3.1.2.2 is the one underpinning the key findings about changes in specific humidity and relative humidity. Although confidence levels are provided to the main conclusions, the section lacks for publication references that support them. Is it only based on expert judgment? [Carolina Vera, Argentina]	Noted. We included the available literature on the topic to support the findings of this session.
18110	45	9	46	1	Section 2.3.1.2.2 Surface humidity: the definitions of “relative humidity” and “specific humidity” are not stated [Gwenaelle GREMION, Canada]	Noted. These definitions are included in the Glossary Annex.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57416	45	10	45	45	<p>This section refers to one observational data record and four reanalysis, two of which from ECMWF. The observational data record contains land based observations only. To me it seems that this subsection would benefit from 1) a more precise formulation (one observation over land, not multiple and whenever reference to the observation is given it should be emphasised that associated results refer to land only; in particular, the observational evidence for increasing trends over oceans is not provided, i.e., it is at present based on reanalysis) and 2) the inclusion of results based on observational data records over ocean. The latter should include in-situ observations and satellite data. It may be added: "Kent et al. (2014) show an increase in near surface humidity over the ocean using in-situ and reanalyses data records (though trend estimates were not computed due to the impact of ENSO variability) but further conclude that near surface humidity is poorly know over the oceans. Prytherch et al. (2015) conclude that microwave based data records do not reveal consistent trends.</p> <p>References: Kent EC, Berry DI, Prytherch J, Roberts JB. 2014. A comparison of global marine surface specific humidity datasets from in situ observations and atmospheric reanalyses. Int. J. Climatol. 34:10.1002/joc.3691. Prytherch, J., E. C. Kent, S. Fangohr, and D. I. Berry, 2015: A comparison of SSM/I-derived global marine surface-specific humidity datasets. Int. J. Climatol., Early View, doi:10.1002/joc.4150. [Marc Schröder, Germany]</p>	<p>Taken into account. We updated the section based on this and other suggested references.</p>
7844	45	11	43	19	<p>is the conclusion in line 11-12 conflicted with the conclusion in line 19-20? [zhiyan zuo, China]</p>	<p>Noted. Conclusion in line 11-12 is based on the period 2000 to 2012, while the conclusion in line 19-20 is based on the period 2012-2018.</p>
27696	45	16	45	16	<p>replace with published article (Willett et al) [Poot Delgado Carlos Antonio, Mexico]</p>	<p>Noted. The paper still under review.</p>
23768	45	17	45	17	<p>Insert , after 'static' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Editorial</p>
30534	45	19	45	21	<p>References for this assessment are missing [Annalisa Cherchi, Italy]</p>	<p>Noted. We included the available literature on the topic to support the findings of this session.</p>
50052	45	21	45	22	<p>"The abatement mentioned in AR5 is no longer apparent." - Is the suggestion that this now no longer apperent since 2012 or in the longer term specific humidity reord? Not clear to the reader. [Tim Trent, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Noted. We clarified the sentence</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14292	45	21			This subsection can be coordinated with 8.3.1.2. "The abatement mentioned in AR5 is no longer apparent.": make clear this is in relation to specific humidity due to the continued reduction in relative humidity over land (e.g. Dunn et al. (2017) [https://www.earth-syst-dynam.net/8/719/2017/esd-8-719-2017.html] which was also mentioned in AR5. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We modified the text to include the findings of Dunn et al. (2017).
30536	45	24	45	25	last sentence is too general: on what is it based? Averaged when? [Annalisa Cherchi, Italy]	Taken into account. The sentence was modified to include the period and region considered.
18108	45	24	45	25	Citation needed for the following sentence: "The relative humidity has remained well below average over land." [Gwenaëlle GREMION, Canada]	Noted. We included references to support this statement.
7164	45	24	45	25	What is the average period that RH has remained below? [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was modified to include the period and region considered.
14296	45	24			The physical causes of declining relative humidity over land can be signposted to 8.2.1.2 and 8.3.1.2 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We will refer the reader to those sections in the FGD.
14294	45	30			"increasing aridity" -->"decreases" (Incorrect usage). Also remove "therefore becoming more arid." at end of paragraph. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested corrections and edits were implemented.
30538	45	31	45	32	sentence to rewrite, partially ungrammatical [Annalisa Cherchi, Italy]	Editorial
32308	45	43	45	44	Text states a very likely increase in near surface specific humidity over the ocean. However, Fig.2.14 shows virtually no information over the ocean - some of which has a negative not positive trend. So, how are the authors able to reach a 'very likely' assessment of increase in near surface specific humidity over the ocean? [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We re-evaluated the statement based on your suggestion.
30540	45	43	45	45	this is the same conclusion as in AR5. If it should be based on fig 2.14, the trends are computed over the same periods [Annalisa Cherchi, Italy]	Rejected. Comment is unclear. Figure 2.14 was made with records from 1973 to 2018, covering most of the period assessed in AR5 (1973-2012).
14506	45	43	45	45	There is a need to specify the time period for the increase in near-surface specific humidity and the decrease in relative humidity. Also, as there is a large uncertainty with RH as mentioned above, "very likely" assigned to the RH decrease seems to be inaccurate here. (CUG, Guoyu Ren) [Guoyu Ren, China]	Taken into account. The time period is included (since the 1970s for trends in specific humidity and since 2000 for trends in relative humidity). We have considered the uncertainty in the surface humidity products to make our assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37486	45	48	45	53	This figure is based only on HadISDH. An earlier figure showing maps of temperature trends included a panel from ERA-Interim, to be replaced by one for ERA5 for the SOD. As the decline in RH was seen first in ERA-Interim and confirmed by a forerunner of HadISDH, I wonder whether Fig. 2.14 should be extended to show trends from ERA5 as well as HadISDH. Currently ERA5 trends would have to be from 1979, but by the time of the final draft they could be from 1973, as they are for HadISDH in Fig 2.14. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The development of new surface humidity figures is planned.
37488	45	51			The ordinary least squares method was used to calculate the temperature trends shown in Fig. 2.11 but the median of pairwise slopes is used here. A few words on the rationale for the choice of method could be given. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The Figure will be modified to include trends based on ordinary least squares method with AR1 correction.
30542	46	4	46	4	add the acronym "(TCWV)" directly in the title so you don't need to expand later the acronym in the text [Annalisa Cherchi, Italy]	Editorial
42906	46	4	46	42	Add discussion of upper tropospheric specific humidity retrievals (e.g. Soden et al 2005; Chung et al 2014 and others) - is the upper troposphere moistening and is this consistent with simulations with/without well mixed greenhouse gas forcing? Is there data onward from 2005? Could reanalysis products be used in this assessment, as is done in later subsections, given uncertainties in doing so? [Michael Evans, United States of America]	Rejected. Section 2.2.5.1 covers stratospheric water vapour. Consideration of surface and TCWV is sufficient for key indicators of the hydrological cycle. Chapter 8 may take on UT zone.
35622	46	9	46	19	The trend in total column water vapour in surface reanalysis products is model result rather than something close to direct observations. I suggest condensing this discussion. [Nathan Gillett, Canada]	Taken into account. We modified the assessment of total column water vapour according to your suggestion.
43342	46	11	46	11	TCWW should be TCWV [James Renwick, New Zealand]	Editorial
18114	46	11	46	11	The acronym TCWW is used on line 11 but defined in line 28. [Gwenaelle GREMION, Canada]	Accepted. Editorial
23770	46	13	46	13	Change 'twentieth century' to '20th Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23772	46	19	46	19	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23774	46	20	46	20	Change 'twentieth century' to '20th Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57418	46	29	46	36	I propose to change into: "data records, with global coverage but limited temporal resolution (Schröder et al., 2018). The various products generally exhibit a positive trend since 1979, which is particularly evident over the tropical oceans (Chen and Liu, 2016; Gu and Adler, 2013; Mieruch et al., 2014; Schröder et al., 2016, 2019; Wang et al., 2016a). The existence of apparent breakpoints in available TCWV records, which in most cases coincide temporally with changes in the observing system, affected trend estimates based on satellite, reanalysis and merged products over Central Africa, the Sahara and South America but also over global oceans (Schröder et al., 2016, 2019; Wang et al., 2016a). Moreover, data gaps over these regions in observations from ground-based GPS receivers and the radiosonde archive create low confidence in the TCWV estimations in these regions." Reference: M. Schröder, M. Lockhoff, L. Shi, T. August, R. Bennartz, H. Brogniez, X. Calbet, F. Fell, J. Forsythe, A. Gambacorta, S.-P. Ho, E. R. Kursinski, A. Reale, T. Trent, Q. Yang, 2019: The GEWEX water vapor assessment of global water vapour and temperature data records from satellites and reanalyses. Rem. Sens., 11(3), 251, https://doi.org/10.3390/rs11030251 . [Marc Schröder, Germany]	Noted. The reference was included in the assessment.
14298	46	30			A global TCWV trend of 0.84±0.2%/decade (Allan et al. 2014 doi:10.1007/s10712-012-9213-z.) was estimated for 1988-2008 by combining the SSM/I record over ice free oceans with reanalyses data, [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference was included in the assessment.
57440	46	31	46	32	I think it is adequate to mention here the observed change in TCWV in relation to C-C. I propose the following text that can be added here: "A subset of the TCWV products exhibit changes in TCWV per change in sea surface temperature around 7.5%/K (Gu and Adler, 2013; Schröder et al., 2019)." This way a link to the summary from AR5 is given. [Marc Schröder, Germany]	Taken into account. We modified the section to integrate this suggested citation.
50054	46	34	46	35	G-VAP reference missing: Schröder, M.; Lockhoff, M.; Shi, L.; August, T.; Bennartz, R.; Brogniez, H.; Calbet, X.; Fell, F.; Forsythe, J.; Gambacorta, A.; Ho, S.-P.; Kursinski, E.R.; Reale, A.; Trent, T.; Yang, Q. The GEWEX Water Vapor Assessment: Overview and Introduction to Results and Recommendations. Remote Sens. 2019, 11, 251. Paper includes an updated figure (from WRCF report) with dataset trends and relationship to clausius clapeyron (Figure 1 in paper). [Tim Trent, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference was included in the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40980	46	42	46	42	It might be worth adding that recent work has given little confidence in the ability of current reanalysis products to correctly reflect stratospheric circulation changes due to the wide spread obtained from different reanalyses (Chabrillat et al., ACP, 2018, Ploeger et al., ACP, 2019). [Johannes Laube, Germany]	Rejected. Section 2.2.5.1 covers stratospheric water vapour.
27942	46	45	46	45	“Centennial trends and trends over 1960-2017 hint on the “wet wetter – dry drier” pattern in annual precipitation totals which is not the case for the recent trends” - From the figure, one might conclude the opposite, i.e., that the recent trend has a more pronounced “wet wetter – dry drier” pattern than the centennial trends and trends over 1960-2017. [roderik van de wal, Netherlands]	Taken into account. We rephrased the sentence to remove any potential ambiguity.
39496	46	45	47	39	The section 2.3.1.2.4 supports the key conclusions regarding changes in global precipitation. But it does not include a single reference. The conclusions should be based on available literature, not only on expert judgment based on data analysis. [Carolina Vera, Argentina]	Noted. We included references to support the assessment of section 2.3.1.2.4
35628	46	45	49	30	There is a subsection on changes in precipitation over land, and a subsection on changes in P-E over land and ocean, but no section on changes in precipitation over ocean. A number of the datasets assessed in 2.3.1.2.4 are global in scope and include the ocean. Moreover there is literature analysing changes in precipitation at island locations e.g. Polson et al (2016) doi:10.1088/1748-9326/11/7/074024. Finally, if ocean precipitation measurements are too uncertain to warrant the inclusion of any assessment, what is the basis for including ocean measurements of P-E? I suggest including a section on changes in ocean precip, which could just say that ocean precip trends are very uncertain if that is the assessment. [Nathan Gillett, Canada]	Noted. We included the assessment of precipitation changes over the oceans during the satellite era, based on GPCPv2.3.
32310	46	45	49	30	The text goes from a subsection with an assessment of precipitation changes over land to the next subsection with precipitation minus evaporation over both ocean and land. However, there is no assessment of changes in precipitation over the oceans. Such an assessment is needed so please add an extra subsection on this topic or broaden 2.3.1.2.4 to include ocean as well as land precipitation. At present it is not clear whether precipitation has changed significantly over the ocean and this needs to be resolved. [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We included the assessment of precipitation changes over the oceans during the satellite era, based on GPCPv2.3.
32312	46	45	49	30	Likewise there is no assessment of whether evaporation has changed over the ocean so this needs to be assessed as well. [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We only assessed precipitation and P-E over the oceans.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14282	46	45			Feng et al. (2013) Nature Clim. doi:10.1038/NCLIMATE1907 Find increased variability in seasonality of tropical rainfall over the 20th century [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Given that the paper only evaluates changes in seasonality over the tropics and chapter 2 deals with global precipitation changes, this reference is better to be included in Chapter 8.
14300	46	45			Liu & Allan (2013) ERL doi:10.1088/1748-9326/8/3/034002 identify positive trends over global land across raingauge datasets (0.93-1.2 %/decade over 1979-2008) while significant increases in the wettest regime (2.0+-1.2 %/decade) and not significant decreases in the drier regime (-1.5+-2.5 %/decade) are identified for GPCP data over tropical land for the more 1988-2008 period that incorporates microwave data (Table 2). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference was included in the assessment.
7166	46	53	46	53	It is the Climatic Research Unit! It is odd how so many people get this wrong! [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Suggested correction implemented.
50260	47	5	47	5	over the 1901-2017 period [Sophie SZOPA, France]	Noted. Suggested correction implemented.
35624	47	5			Significant with respect to internal variability, or with respect to observational uncertainty? I recommend focussing on the latter. [Nathan Gillett, Canada]	Taken into account. Trends are significant with respect to observational uncertainty.
50262	47	8	47	8	what does "homogeneity" refer to? [Sophie SZOPA, France]	Taken into account. The sentence was modified to clarify the existence of uncertainties in precipitation records before the 1940s.
50264	47	8	47	8	global precipitation anomalies are.... [Sophie SZOPA, France]	Editorial
18118	47	8	47	8	"potential (lack of?) homogeneity in the early records." [Gwenaelle GREMION, Canada]	Taken into account. The sentence was modified to clarify the existence of uncertainties in precipitation records before the 1940s.
30544	47	8	47	9	sentence seems out of place as it is not clear what consequences we may expect from it [Annalisa Cherchi, Italy]	Taken into account. The sentence was removed.
23776	47	9	47	9	Change Nina to Niña and Nino to Niño [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
7168	47	20	47	25	I don't think the weights given here can be said to be an 11-year Gaussian-weighted filter. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We removed the filter for the SOD.
30546	47	30	47	30	not clear why the dataset GPCC V8 is used in Fig 2.16 to show the spatial patterns: looking at table 2.4 it is the only dataset considered whose trends are not significant [Annalisa Cherchi, Italy]	Taken into account. New maps and time series for the assessment of global precipitation were created.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18120	47	30	47	30	Previous paragraphs compared the different datasets. How does the spatial variability from GPCC V8 compared with the other datasets? [Gwenaelle GREMION, Canada]	Taken into account. New maps and time series for the assessment of global precipitation were created.
44636	47	31	47	31	"increases" corrected to "increasing". [Liang Zhao, China]	Rejected. In the context of the sentence, there is no need to change the word.
50266	47	34	47	34	increases of precipitation or trend magnitudes? [Sophie SZOPA, France]	Noted. Recent trends show increases in precipitation.
54424	47	37	47	38	This statement appears vague and subjective. What are meant by hint and recent? [Reynold Stone, Trinidad and Tobago]	Taken into account. The sentence was removed.
54426	47	37	47	38	Which geographical regions do this apply to? Is this for land or ocean? [Reynold Stone, Trinidad and Tobago]	Taken into account. The results are for land regions. We also included the assessment of precipitation changes over the oceans during the satellite era, with a map and global time series based on GPCPV2.3 data.
54428	47	37	47	38	It is also inconsistent with "An absence of wet/wetter, dry/drier over land is seen when considering historical observations of precipitation" in Chapter 8, page 42, lines 12-14. [Reynold Stone, Trinidad and Tobago]	Noted. We checked the cross-chapter consistency regarding the global hydrological cycle.
35626	47	37	47	39	This reads as rather vague and speculative. I suggest deleting this sentence. [Nathan Gillett, Canada]	Taken into account. The sentence was removed.
30548	47	37	47	39	better to refer to ch 8 for the assessment of the processes [Annalisa Cherchi, Italy]	Accepted
50268	47	37	47	39	shouldn't it be in the conclusion of this subsection also? [Sophie SZOPA, France]	Taken into account. The sentence was removed.
18116	47	37	47	39	The following sentence is not clearly explained: [Centennial trends and trends over 1960-2017 hint on the "wet wetter – dry drier" pattern in annual precipitation totals which is not the case for the recent trends] [Gwenaelle GREMION, Canada]	Noted. The sentence was rephrased.
49942	47	38			The word "hint" is vague and needs to be replaced. Try using IPCC uncertainty language instead. [Owen Cooper, United States of America]	Noted. The sentence was rephrased.
18122	48	1	48	3	Correcting for serial correlation is very important. I would suggest to apply the same methodology to the rest of the figures which don't mention it. For field regressions, it might be also important to control for multiple comparisons. There's not a lot of literature on the topic, but one could use FDR to adjust the p-value or follow the methodology of DelSole & Yang 2011(https://journals.ametsoc.org/doi/10.1175/2011JCLI4105.1) [Gwenaelle GREMION, Canada]	Noted. We used OLS trends with AR1 correction to calculate all the trends in Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43870	48	4	48	5	Are all digits in the numbers in trend equations significant? Only relevant digits should be in the table. [Joanna Wibig, Poland]	Accepted. We included only relevant digits
7170	48	4	48	10	Table 2.3 gave temperature trends to two decimal places. How can you justify giving precipitation trends to three places of decimals? [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We included only relevant digits
7846	48	7	48	10	for the season, boreal or austral? [zhiyan zuo, China]	Noted. The assessment of seasonal precipitation changes was not part of Chapter 2 charge and should instead be undertaken by chapter 8.
14508	48	15	48	28	Here the uncertainties of global land precipitation trend estimates should be briefly assessed. The uncertainties are mostly related to the incomplete coverage of data, the inhomogeneities of the observations, the usage of indicators for calculating temporal series, and the wind-speed relative under-catch bias. For the under-catch bias, the following publications could be referred: 1) Zheng, ZF, GY Ren, 2019, Effects of gauge under-catch on precipitation observation and long-term trend estimates in Beijing area, Advances in Water Science, 28 (5): 662-670; 2) Sun, XB, GY Ren, ZH Ren, et al., 2013, Effect of wind-induced errors on winter snowfall and its trends, Climatic and Environmental Research, 18 (2): 178-186; 3) Ye, BS, P Cheng, DQ Yang, et al., 2008, Effect of the bias-correction on changing tendency of precipitation over China, Journal of Glaciology and Geocryology, 30 (5): 717-725. (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. Note that most of the cited works are essentially regional and Ch. 2 focus is on large scale changes. These works are likely more relevant for the CH. 8 scope.
52976	48	16	17	48	An additional data set that is heavily downloaded and used is the original PERSIANN (0.25 degree) and should be added to this list. It is different than PERSIANN CDR. The reference for it is the highly cited BAMS paper: Sorooshian, S., K. Hsu, X. Gao, H. Gupta, B. Imam, and D. Braithwaite, "Evaluation of PERSIANN System Satellite-Based Estimates of Tropical Rainfall," Bulletin of the American Meteorological Society, 81(9): 2035-2046, doi: 10.1175/1520-0477(2000)081<2035:EOPSS>2.3.CO;2, September 2000 [Soroosh Sorooshian, United States of America]	Taken into account. We don't pretend to cover all the available satellite-based precipitation products. Therefore, only PERSIANN-CDR was mentioned in the text
23778	48	16	48	16	Change 'twentieth Century' to '20th Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
27698	48	18	48	18	double parenthesis [Poot Delgado Carlos Antonio, Mexico]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30550	48	21	48	23	this should be better detailed, so other table like 2.4 in SOD should be added and yes possibly inclusion of those results in fig 2.15 and 2.16 [Annalisa Cherchi, Italy]	Taken into account. We included new maps and time series for the assessment of global precipitation based on GPCPv2.3
37490	48	22			Some variations in global precipitation are almost certainly related to ENSO, but others are likely not. A minimum occurs in the early 1990s that may well be linked with tropospheric cooling associated with the 1991 eruption of Mt Pinatubo. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Attribution assessment is the purview of Chapter 3 and is not covered here.
14302	48	22			Liu & Allan (2013) ERL doi:10.1088/1748-9326/8/3/034002 show that this variability over the tropical wet regimes are related to an ENSO index ($r=-0.57$, 1979-2008) and atmosphere simulations forced with observed SST are able to capture much of this variability ($r=0.64$, 1950-2008). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence in SOD has been modified to address comment.
23780	48	26	48	26	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
32314	48	31	48	32	'AR5 concluded that the pattern of E-P over the oceans has been enhanced since the 1950s'. This statement is incorrect as the conclusion reached in AR5 Chapter 3 p.276 was that it is not yet possible to establish whether there are significant multi-decadal trends in mean E-P over the oceans. Please correct the text here to accurately report the conclusion of AR5. [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence was modified accordingly
57420	48	35	49	25	I propose to redo Figure 2.17 and associated discussions using ERA5 by assuming that the representation of P is of higher reliability in ERA5 than in ERA-Interim. In addition it would be interesting to compute the mean P-E trends separately over land, ocean and globally. [Marc Schröder, Germany]	Noted. New maps and time series for the assessment of global P-E were drafted.
35630	48	35			Clarify what global-scale P-E changes are being referred to here - presumably this is global mean land and global mean ocean P-E. True global mean P-E must be closely constrained to close to zero by the water budget of the atmosphere. [Nathan Gillett, Canada]	Taken into account. The assessment of P-E was improved based on several suggestions and comments.
30552	48	37	48	37	add "global" before "P-E trends". Change "by recourse to" with "using" [Annalisa Cherchi, Italy]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50270	48	38	48	38	please consider to mention that reanalysis are a combination of models and data. [Sophie SZOPA, France]	Rejected. Section 1.5 of Chapter 1 covers several aspects of reanalyses data. Therefore, the reader interested in the developments and applications of reanalyses might refer to that section.
37492	48	41			I would strongly urge that this figure be redrawn using ERA5 rather than ERA-Interim. The reanalysis team at ECMWF is aware that precipitation is too high in ERA-Interim over Africa, although it declines over time to be similar to values from GPCP and GPCP for the latest few years. Most of the excess comes from western tropical Africa. ERA5 is much better in this respect, and is an improvement over ERA-Interim in most other places - though there is still scope for improvement. We currently have only a working document on this from our own monitoring efforts - this is the sort of result that we would normally expect to be published in the literature by independent users of reanalysis as the depth of our expertise is limited. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. New maps and time series for the assessment of global P-E were drafted.
30554	48	42	48	43	remove the whole sentence starting with "Changes..." [Annalisa Cherchi, Italy]	Taken into account. The assessment of P-E was improved based on several suggestions and comments.
35632	48	42	48	46	This text is confusing as written. First, the text should be refocused on assessment of where P-E trends have been observed which are larger than observational uncertainty, rather than larger than internal variability based on an AR(1) model, which I think is the meaning of 'significant' here. Second, when bringing in evidence from runoff, the relevant question should be are the runoff records consistent with the assessed P-E trends from the reanalysis? There are three possibilities in each case - the runoff records for each continent are distinguishable from zero based on their observational uncertainty and are consistent with the P-E trends, or they are inconsistent with zero and inconsistent with the P-E trends, or their uncertainties are so large that they are consistent both with zero and the P-E trends. Whether or not individual rivers or P-E at individual locations on land show trends which are significant, in the sense of distinguishable from internal variability, is not important for this assessment. [Nathan Gillett, Canada]	Taken into account. The assessment of P-E was improved based on several suggestions and comments.
23782	48	43	48	43	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57422	49	1	49	1	Please change the first sentence into "Over the oceans, a negative global trend of P-E is observed since 1979 using MERRA-2 (Bosilovich et al., 2017)." And further I propose to add: "Using SSM/I based observations Andersson et al. (2011) also show a (small) decrease in P-E over the global ice-free ocean over the period 1988-2005." Reference: Andersson, A., Klepp, C., Fennig, K., Bakan, S., Grassl, H., and Schulz, J.: Evaluation of HOAPS-3 Ocean Surface Freshwater Flux Components, J. Appl. Meteor. Climatol., 50, 379–398, https://doi.org/10.1175/2010JAMC2341.1 , 2011. [Marc Schröder, Germany]	Taken into account. We included the suggested literature.
18124	49	1	49	1	This statement is in opposition with the AR5 conclusion summarized on pg. 48 line 31 (unless the verb "enhanced" is intended to indicate something other than positive/increase). If recent evidence contradicts AR5 conclusions, this should be clearly stated. [Gwenaëlle GREMION, Canada]	Rejected. The verb "enhanced" in AR5 was used to describe changes associated to regions of high salinity (where evaporation dominates) becoming more saline, and regions of low salinity (where rainfall dominates) becoming fresher. The statement is not opposing or contradicting the AR5 conclusion (AR5 assessed E-P instead of P-E).
50282	49	1	49	2	if the trends are deduced from reanalysis, they are not observed so please consider to replace "observed" by "found" [Sophie SZOPA, France]	Editorial
50278	49	28	49	29	"trends in globally averaged P-E are small" [Sophie SZOPA, France]	Taken into account. The assessment of P-E was improved based on several suggestions and comments.
35634	49	28	49	30	It is not clear what the reader is supposed to take from the assessment that observed trends in globally averaged P-E are small. Globally averaged P-E integrated over time must be equal to the trend in global average column water vapour. I suspect that P-E observations are not good enough to be able to resolve changes in column water vapour. The discussion seems to miss the fact that globally averaged P-E and its trend has to be close to zero from basic conservation arguments. [Nathan Gillett, Canada]	Taken into account. The assessment of P-E was improved based on several suggestions and comments.
37494	49	28			Continuing in the same vein, P and E are in much better global balance in ERA5 than ERA-Interim, indicating that the ERA5 values over sea are more realistic (or at least less unrealistic). I would not claim too much for ERA5 at this stage, but inserting the word "many" before "reanalysis" on this line would hardly weaken the conclusion. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The assessment of P-E was improved based on several suggestions and comments. We included ERA5-based P-E estimates in SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50280	49	29	49	29	"implied"=> "inferred" [Sophie SZOPA, France]	Editorial
30556	49	33	50	22	assessment of section 2.3.1.2.6 is quite weak. Probably better to coordinate with corresponding section in Ch 8 [Annalisa Cherchi, Italy]	Taken into account. The assessment of global streamflow was improved based on several suggestions and comments.
23784	49	35	49	35	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35636	49	39	49	48	This paragraph focusses on small-scale variations in streamflow, but for the large-scale indicators which are the topic of this chapter, shouldn't the focus be on continental-scale streamflow? In which case, shouldn't the focus be on streamflow measured close to river mouths? [Nathan Gillett, Canada]	Noted. The paragraph focusses on the development of new global streamflow datasets, which can be used for global or continental-scale assessments. The use of streamflow measured close to river mouths will maximize the anthropogenic effect, a fact that would preclude the use of streamflow as key indicator of a changing climate system.
23786	49	44	49	44	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35638	49	51	49	53	Is this conclusion for raw or naturalised flow? [Nathan Gillett, Canada]	Noted. The conclusion is for raw streamflow. We modified the conclusion to include this limitation.
37496	50	3	50	5	Consideration could be given to a changed warming. Precipitation tends to decrease in the early stage of an El Nino, which is consistent with a warmer atmosphere being able to hold more water. But water leaves the atmosphere in the form of precipitation once atmospheric temperatures start to fall in the decaying phase of an El Nino, when oceanic conditions may already be indicating a La Nina. So below/above average precipitation/runoff may not be simply a case of El Nino/La Nina. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section was extensively modified considering several suggestions and comments.
18128	50	3	50	5	I think that while the "... low (high)..." construction can be efficient at reducing wordcount, it's much harder to read. Please consider rephrasing it. [Gwenaelle GREMION, Canada]	Taken into account. The paragraph was rephrased.
35640	50	13	50	14	Describe briefly how streamflow is estimated from satellite data. [Nathan Gillett, Canada]	Taken into account. We only focused in reanalysis estimations of discharges given that they are more reliable than satellite-based estimates.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35642	50	19	50	22	As an observational assessment, this conclusion should focus on where trends are outside the range of observational uncertainty, rather than outside the range of estimated internal variability. The meaning of 'significant' here is not clear, but probably implies the latter. [Nathan Gillett, Canada]	Taken into account. We clarified the meaning of the significance of the trends.
35644	50	19	50	22	It does not make sense to say that there has 'likely' (i.e. P>66%) not been a 'significant' (trend inconsistent with internal variability at the 5%/10%? level) trend in globally averaged streamflow. Refocus on observational uncertainty. Something like 'Estimated trends in globally averaged streamflow are consistent with zero to within observational uncertainty (medium confidence)', or similar. Secondly, the assessment should clarify whether it applies to raw observed streamflow, or naturalised streamflow. [Nathan Gillett, Canada]	Taken into account. The summary statement of section 2.3.1.2.6 was rephrased.
18130	50	19	50	22	In light of previous discussion about non-climatic human influences in river streamflow, should there be a short confidence statement about how much the observed streamflow trends reflect climatic forcings? [Gwenaelle GREMION, Canada]	Taken into account. This section was extensively modified considering several suggestions and comments.
50284	50	27	50	27	"evaporation losses" => "increase of evaporation"? [Sophie SZOPA, France]	Editorial
39498	50	37	51	49	The following references might be useful for the section: Allen, R. J., and Kovilakam, M. (2017). The Role of Natural Climate Variability in Recent Tropical Expansion. Journal of Climate 30, 6329-6350. DOI: 10.1175/JCLI-D-16-0735.1. Kim, Y.-H, Min, S.-K., Son, S.-W., and, J. Choi. (2017). Attribution of the local Hadley cell widening in the Southern Hemisphere. Geophysical Research Letters. 44, 1015-2024. doi: /10.1002/2016GL072353 [Carolina Vera, Argentina]	Taken into account. The Allen and Kovilakam (2017) paper was included in the assessment of Hadley Circulation. Kim et al (2017) deals with attribution, which is the scope of Chapter 3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38516	50	37	51	49	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 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. We ensured consistency between chapters regarding the changes in the Hadley circulation.
23788	50	38	50	38	Insert '(HC)' after 'circulation' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35272	50	42	50	54	Evidences for past changes in the HC are also available for the Southern Hemisphere. See for instance, Villalba et al 2012 in Nature Geoscience. [eugenia gayo, Chile]	Noted. The suggested literature was reviewed and included in the assessment of Hadley circulation.
17928	50	42	51	49	HC is highly dependent on the way its averaging is defined, usually via Eulerian mean. Dynamic meteorology shows there is a better way to treat general circulation, e.g., via the Transformed Eulerian mean (Andrews & McIntyre, JAS 1976; Holton textbooks, etc.). Comment on that, importance and elusiveness of HC will be beneficial. [Branko Grisogono, Croatia]	Noted. However, IPCC is not a textbook and this discussion is out of scope.
23790	50	43	50	43	Change 'trade winds' to 'the Trade Winds' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18132	50	43	50	47	Why no mention of a southward shift of the tropical rainbelt (ITCZ) during Heinrich Stadials? A southward shift is well documented in Cariaco basin and Arabian sea sediments (Deplazes et al., 2013 Nature GeoScience), speleothem growth in NE Brazil (Wang et al., 2001 Nature; Wendt et al., 2019 EPSL), NE South American margin sediments (e.g. Zhang et al., 2017 QSR; Mulitza et al., 2017 Paleoceanography), etc. etc. [Gwenaëlle GREMION, Canada]	Taken into account. Several of the suggested papers were included in the assessment of Hadley circulation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18134	50	43	50	47	As it stands, this statement seems to underserve the important conclusions of recent research regarding past changes to HC. Consider re-phrasing, for example: Paleo evidence indicates past periods of hemispheric asymmetry in HC and associated latitudinal shift of the ITCZ in response to climate perturbations. For example, paleo rainfall and trade wind proxies show an intensification of the Northern Hemisphere HC in parallel with a weakening of the Southern Hemisphere HC (McGee et al., 2018a) and a southward shift of the ITCZ during Heinrich stadials (e.g. Deplazes et al., 2013). [Gwenaelle GREMION, Canada]	Taken into account. We included new literature for paleo evidence of Hadley circulation changes.
18136	50	45	50	45	Heinrich stadials are not defined in the text. [Gwenaelle GREMION, Canada]	Noted. We will suggest the inclusion of the definition in the Glossary Annex.
23792	50	45	50	45	Change 'trade winds' to 'Trade Winds' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35646	50	45			Explain what a Heinrich stadial is. [Nathan Gillett, Canada]	Noted. We will suggest the inclusion of the definition in the Glossary Annex.
18138	50	46	50	47	McGee et al. (2018b) reveals a possible link between Pacific HC and western US wet periods, but was not the first to determine wetter conditions in the western US during Heinrich Stadials (as suggested by this sentence structure). Evidence of this dates back to 2010. [Gwenaelle GREMION, Canada]	Taken into account. The assessment performed for AR6 is mainly based on literature published after 2013.
18126	50	47	50	48	Indicate if the following sentence refers to the Southern or the Northern Hemisphere westerlies: "An intensification of the HC and climatic conditions similar to La Niña-like state resulting in a more northerly position of both the Westerlies and the ITCZ..." [Gwenaelle GREMION, Canada]	Accepted. We modified the sentence to include the Hemisphere.
18140	50	48	50	48	ITCZ is not defined in the text. [Gwenaelle GREMION, Canada]	Editorial
18142	50	49	50	49	Why mention specific proxies used (i.e. nitrogen isotopes) for the first time here, while other proxies (i.e. oxygen isotopes or calcite lake deposits) are never specified in this chapter prior to this statement? To keep consistent, consider instead mentioning the archive (e.g. marine). [Gwenaelle GREMION, Canada]	Accepted. We ensured consistency in the description of the proxies.
18144	50	50	51	1	Ordering of sentences implies that significant warming is observed in the lower statospher (is this the case? It doesn't appear to be from fig2.13). Include clarification in second sentence as to where "significant warming" is observed. [Gwenaelle GREMION, Canada]	Not applicable. Comment is not assigned to the correct text and may not even pertain to the current chapter as far as we can tell from the limited context.
23794	51	2	51	2	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18146	51	6	51	9	The final sentence of this paragraph is too long and not properly structured. Perhaps a comma after "...(Nguyen et al., 2015)" it might help [Gwenaelle GREMION, Canada]	Editorial
23796	51	13	51	13	Change 'lat' to 'latitude' for clarity [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35648	51	13			Is this exactly 0.5 degrees/ decade, or is there an uncertainty range associated with this? [Nathan Gillett, Canada]	Taken into account. We included an uncertainty range associated with the expansion, considering new evidence based on recent publications.
23798	51	14	51	14	Replace hyphen with , [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
50272	51	14	51	15	change the punctuation [Sophie SZOPA, France]	Editorial
23800	51	15	51	15	Delete hyphen after 'HC' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23802	51	16	51	16	Change 'lat' to 'latitude' for clarity [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
14284	51	18			Feng et al., 2016 missing from reference list [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Reference was added.
23804	51	26	51	26	Delete 'seasons': summer and autumn are seasons, so the text is tautologous [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18148	51	31	51	37	It might be more intuitive either to use absolute values for intensity or flip the y axis in the southern hemisphere so that "more intense" means upward in both panels. [Gwenaelle GREMION, Canada]	Noted. The figure was corrected based on your suggestion.
26918	51	35	51	37	The key message for the GM precipitation increase over the last 40 years seems opposite to the AR5 conclusion with precipitation decrease as it is indicated in lines 53-54 of page 51. Please clarify this point. [Prodomos Zanis, Greece]	Noted. AR6 assessment is based on new evidence indicating that the GM precipitation trends are opposite to what was found in AR5.
13890	51	46	51	47	My understanding (eg from the Staten et al paper) is that a significant part of this trend is likely natural variability. Presumably this is discussed elsewhere in the report? [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Assessment of attribution of trends is the purview of chapter 3 and is not covered here.
35650	51	47	51	49	Of the six measures of HC intensity between the two hemispheres shown in Fig 2.18c and d, two seem to show an increase, three seem to show a decrease, and one shows no clear trend. This doesn't seem to support the assessment that there has likely been a strengthening of the Hadley Circulation. [Nathan Gillett, Canada]	Noted. The SH HC intensity needs to be interpreted in absolute values. Therefore, Figure 2.18d shows a strengthening in the SH HC intensity, which is significant considering ERA-Interim and MERRA-2. According to comment 18148, we modified Figure 2.18d to make its interpretation more direct to the reader.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40482	51	52	52	38	I like the way you have succinctly handled the past observed trends of the global monsoon in this chapter. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the feedback.
39520	51	52	52	38	The following review paper could be useful for the assessment of 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]	Noted. The suggested literature was reviewed and included in the assessment of global monsoon.
35652	51	52			Define the metrics (areas, variables, seasons) used to characterise global monsoons in this assessment at the beginning of this section. For example, what is the GM circulation index? What are the areas over which precipitation is averaged, and for which months? etc. [Nathan Gillett, Canada]	Taken into account. These suggestions were covered in Chapters 2, 3, 4 and 8.
30558	52	1	52	6	not about global monsoon, better to leave the details to Ch 8 [Annalisa Cherchi, Italy]	Accepted. We rephrased the paragraph to focus only on global monsoon changes.
52336	52	8	52	12	Also wordy and lots of passive voice - rephrase/edit [Katherine Glover, United States of America]	Editorial
40472	52	8	52	16	It would also be worth mentioning obliquity, which declined from the mid-Holocene onwards, reducing seasonality and consistent with the declining NH monsoons. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We included literature based on the impact of obliquity changes in the global monsoon.
37498	52	8	52	16	See comment 11 on the entire report. This is a paragraph where 100ka is used to mean 100k years, not a BP date. And it has a rare use of BP. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This has to be homogenised across the entire report.
23806	52	9	52	9	Replace 'a' with 'an' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18156	52	11	52	12	No mention of Cheng et al., 2016 Nature ? [Gwenaelle GREMION, Canada]	Noted. The suggested literature was reviewed and included in the assessment of global monsoon.
40344	52	11			Zhisheng et al. should be An et al., An is the family name [Chenxi Xu, China]	Accepted. The reference was modified.
40474	52	18	52	20	I think this sentence has mis-characterized the finding of Wang et al. (2013). He terms it a "mega-ENSO", which is really something like the IPO (and would have been better for everyone if he had just called it that). It is NOT an extreme El Nino event - the point is there is a measurable impact of decadal variability from the Pacific on the northern hemispehre monsoons. Thus I suggest changing the terminology of this sentence away from "extreme ENSO events". [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested correction implemented.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40476	52	23			Note that "in situ" is a borrowed phrase and doesn't need hyphenating. There is no ambiguity in the meaning of the sentence if the hyphen is removed. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
40478	52	25	52	27	This sentence should be reworded - at present the phrase "extended boreal summer" appears twice in the sentence. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was corrected.
50274	52	35	52	35	please consider to specify that it is the opposite of what was reported in AR5 [Sophie SZOPA, France]	Noted. The summary of the section was modified. However, AR5 points mostly to the East Asian monsoon (and with low confidence), and we point to the global monsoon with medium to low confidence.
40480	52	36			summer monsoon does not need capitals. [Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
39500	52	37	52	38	The discussion presented in lines 27-30 of page 52 concludes from many articles that positive trends in the SH were less evident and superimposed with large year-to-year variability. But in lines 37-38 the assessment gives low confidence to that. Is it correct? or what it has low confidence is the positive trend in the SH? [Carolina Vera, Argentina]	Accepted. What it has low confidence in is the positive trend in the SH. We rephrased the sentence to clarify the assessment
44840	52	40	54	18	The observational results on storm track changes seem quite diverse, which is partly because climatologies of cyclone track densities are sensitive to the detection method as noted in the section. Also, it is largely because long-term internal variability such as PDO and AMO affects the meridional position of storm track by modulating the atmospheric baroclinicity (Dong et al., 2013; Sung et al., 2014; Sutton & Dong, 2012). A crucial role of oceanic variability in the behavior of storm track has been identified by recent studies dealing with air-sea interactions around oceanic front regions (Foussard et al., 2019; Ma et al., 2017; Sun et al., 2018). [Won-Tae KWON, Republic of Korea]	Taken into account. The suggested literatures have been reviewed. Chapter 2 aims to assess the observed changes/trends in hemispheric/global scales. Avoided to talk the mechanisms and regional features due to limit page space.
50276	52	41	52	41	what is SREX? [Sophie SZOPA, France]	Noted. The SREX first appeared in Chapter 1.
50964	52	41	52	46	Lehmann et al. 2015 find a change in stormtracks almost throughout the CMIP5 models [Kai Kornhuber, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Assessment of simulations is the purview of chapters 3 and 4 and is not covered here.
23808	52	42	52	42	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18150	52	53	52	54	"A synthesis of evidence from the Southern Hemisphere indicates that the westerly winds were stronger 14-5 ka and zonally symmetric". The "14-5 ka" is better positioned after "symmetric" at the end of the sentence [Gwenaelle GREMION, Canada]	Accepted. Has been modified.
18152	52	53	52	54	Change "14-5 ka" for "between 14-15 ka" [Gwenaelle GREMION, Canada]	Noted. '14-5 ka', not '14ka-15ka'. In the SOD, changed to 'between 14-5ka'.
18154	52	53	53	1	Consider the following articles which provides independent evidence that the Southern Hemisphere westerly winds weakened between 11.2-7.2 ka (Saunders et al., 2018) and between 10.5-7.5 ka (Moreno et al., 2018). References: (1) Saunders, K.M., Roberts, S.J., Perren, B., Butz, C., Sime, L., Davies, S., Van Nieuwenhuize, W., Grosjean, M. and Hodgson, D.A., 2018. Holocene dynamics of the Southern Hemisphere westerly winds and possible links to CO2 outgassing. Nature geoscience, 11, pp.650-655, (2) Moreno, P.I., Vilanova, I., Villa-Martínez, R., Dunbar, R.B., Mucciarone, D.A., Kaplan, M.R., Garreaud, R.D., Rojas, M., Moy, C.M., De Pol-Holz, R. and Lambert, F., 2018. Onset and evolution of southern annular mode-like changes at centennial timescale. Scientific reports, 8(1), p.3458. [Gwenaelle GREMION, Canada]	Taken into account. The suggested literature have been reviewed and assessed. Moreno et al. (2018) has been included. Note there is doubt on the one-to-one relationship between changes in the SAM index and the poleward location of the jet. See Comments #35654
23810	52	54	52	54	Insert space between number and unit (5 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
33310	52		53		For additional paleoclimate background on pre-instrumental extratropical jets, storm tracks, and blocking, consider: Oster, J. L., Ibarra, D. E., Winnick, M. J., & Maher, K. (2015). Steering of westerly storms over western North America at the Last Glacial Maximum. Nature Geoscience, 8(3), 201–205. https://doi.org/10.1038/NGEO2365 Trouet, V., Babst, F., & Meko, M. (2018). Recent enhanced high-summer North Atlantic Jet variability emerges from three-century context. Nature Communications, 9(1). https://doi.org/10.1038/s41467-017-02699-3 Wise, E. K., & Dannenberg, M. P. (2017). Reconstructed storm tracks reveal three centuries of changing moisture delivery to North America. Science Advances, 3(6). https://doi.org/10.1126/sciadv.1602263 Wise, E. K. (2016). Five centuries of U.S. West Coast drought: Occurrence, spatial distribution, and associated atmospheric circulation patterns. Geophysical Research Letters, 43(9). https://doi.org/10.1002/2016GL068487 [Erika Wise, United States of America]	Noted. Most of the suggested works are regional, although we acknowledge the importance of regional estimates for paleo periods. Chapter 8 should cover regional changes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33386	52				Why are changes prior to the Holocene not included? Much work has suggested important shifts during various climate states (for example, Merz et al., 2015, doi:10.1175/JCLI-D-14-00525.1; Lofverstrom et al., 2016, doi:10.1175/JAS-D-15-0295.1; Lora et al., 2016 doi:10.1002/2016GL071244; Wong et al., 2016, doi:10.1002/2016GL068389). Even if "low confidence", these demonstrate the sensitivity of the jet position/disposition. [Juan Lora, United States of America]	Noted. Most of the suggested works are regional, although we acknowledge the importance of regional estimates for paleo periods. Chapter 8 should cover regional changes.
23812	53	1	53	1	Insert space between number and unit (5 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18158	53	1	53	4	Consider independent evidence for a long-term strengthening of the Southern Hemisphere westerly winds after 7 ka (Saunders et al., 2018) and after 7.5 ka (Moreno et al., 2018). References: Moreno, P.I., Vilanova, I., Villa-Martínez, R., Dunbar, R.B., Mucciarone, D.A., Kaplan, M.R., Garreaud, R.D., Rojas, M., Moy, C.M., De Pol-Holz, R. and Lambert, F., 2018. Onset and evolution of southern annular mode-like changes at centennial timescale. Scientific reports, 8(1), p.3458. [Gwenaëlle GREMION, Canada]	Noted. Suggested references have been reviewed and assessed.
33308	53	6	53	9	Check this reference; I do not think there is anything about poleward shifting storm tracks in Cook et al. 2014. [Erika Wise, United States of America]	Accepted. Modified.
51806	53	6	53	12	North Pacific storm track reconstruction: Wise and Dannenberg (2017) Sci. Adv.:10.1126/sciadv.1602263 [Anson Cheung, United States of America]	Rejected. The suggested literature does not cover the MCA period.
35654	53	12	53	15	Is this assessed poleward shift based on paleo data distinct from an increase in the SAM? Note that there is not a one-to-one relationship between changes in the SAM index and the poleward location of the jet - see e.g. Swart et al. (2015) - doi:/10.1175/JCLI-D-15-0334.1. [Nathan Gillett, Canada]	Taken into account. Suggested literature has been reviewed.
18160	53	12	53	15	There is recent evidence for an equatorial shift of the Southern Hemisphere westerly winds from western Tasmania (Fletcher et al., 2018) and Southern Patagonia (Moreno et al., 2018) between 0.5-0.2 ka and 0.6-0.15 ka respectively, coincident with the LIA. References: (1) Fletcher, M.S., Benson, A., Bowman, D.M., Gadd, P.S., Heijnis, H., Mariani, M., Saunders, K.M., Wolfe, B.B. and Zawadzki, A., 2018. Centennial-scale trends in the Southern Annular Mode revealed by hemisphere-wide fire and hydroclimatic trends over the past 2400 years. Geology, 46(4), pp.363-366, (2) Moreno, P.I., Vilanova, I., Villa-Martínez, R., Dunbar, R.B., Mucciarone, D.A., Kaplan, M.R., Garreaud, R.D., Rojas, M., Moy, C.M., De Pol-Holz, R. and Lambert, F., 2018. Onset and evolution of southern annular mode-like changes at centennial timescale. Scientific reports, 8(1), p.3458. [Gwenaëlle GREMION, Canada]	Rejected. Assessment of paleo jets/westerlies mainly focus on two warm periods of the middle Holocene and the MCA. A stipulated page limit precludes significant expansion of this subsection to include more details for the LIA. Section 2.4.1.2 in the SOD assesses the paleo changes of the SAM during the past centuries.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13892	53	17	53	19	Should state over what period the zonal winds have weakened. This does not seem consistent with the section on the NAO/NAM later in this chapter which reports weakened increasing trends over recent decades. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Time periods for which data are used and the consistency with the NAO/NAM have been clarified.
35656	53	17	53	19	Over what period have the mid-tropospheric extratropical zonal winds weakened? [Nathan Gillett, Canada]	Accepted. Clarified
35660	53	17	53	25	Over what period are the NH extratropical wind trends assessed here? If it's just since 2000, what about changes earlier in the record? Moreover, how robust are wind changes in the reanalyses, which seem to be underlying most of the studies cited here? [Nathan Gillett, Canada]	Accepted. Data periods and the robustness in reanalyses have been clearly stated.
35658	53	19	53	24	These findings of a robust increase in meandering in autumn and a decrease in summer do not appear to be consistent with Screen and Simmonds (2013), cited in the previous sentence. Screen and Simmonds (2013) find an increase in summer, and mixed trends depending on wavenumber in Autumn. [Nathan Gillett, Canada]	Accepted. The statement of meandering has been modified and take into account the differences and balance among results in the SOD.
13894	53	20	53	25	I think there is less agreement between studies on the trends in jet meandering than is reported here. For example the positive trends in autumn have been described as sensitive to the method (Barnes 2013), not significant (Screen and Simmonds 2013) and weak (Cattiaux et al (2016, GRL, doi:10.1002/2016GL070309). In my view the summary statement that 'jet meandering has intensified' is therefore not valid. This is especially clear for summer where some of the studies cited suggest a decreasing trend. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement has been modified and the differences among authors have been taken into account in a balanced way.
50962	53	22	53	25	I'm not sure if I would agree with this statement: 'most robust changes are detected for boreal autumn'. I agree with the decline in zonal winds Coumou 2015 we find an increase in a wave-7 pattern in the jet in Summer (Kornhuber et al. ERL 2019). This is in agreement with Wang et al 2013 (JGRA) who find an increase of synoptic scale wavenumbers in the NH mid.latis in Summer over 1979-2010 (reanalysis). [Kai Kornhuber, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement has been modified and the differences among the conclusions of different authors have been taken into account in a balanced way.
50286	53	27	53	27	SLP not defined prior this line [Sophie SZOPA, France]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6283	53	27	53	40	Extratropical cyclones are typically accompanied by cold and warm fronts. While the number and intensity of extratropical cyclones receive a lot of attention in this paragraph, changes in the frequency of weather fronts and their intensity is not mentioned. However, severe weather, such as hail, heavy precipitation and wind gusts typically occur along the fronts of a cyclone with the fronts often acting as trigger mechanism. Over Europe, there has been a significant increase in the number of extremely strong fronts, while there is no such trend over North America (Schemm et al. 2016, doi:10.1002/2016GL071451; Research highlight in Nature Climate Change: https://doi.org/10.1038/nclimate3218). Because this trends in front intensity is spatially heterogeneous it could help explain the inhomogeneous precipitation trend patterns seen over Europe. Although there is some degree of freedom in how to define and track a front, most methods agree on cold fronts, while there is less agreement on warm fronts (Hobe et al. 2014, doi: 10.1175/mwr-d-12-00252.1. and Schemm et al. 2014, doi: 10.1002/qj.2471). [Sebastian Schemm, Switzerland]	Rejected. A stipulated page limit precludes significant expansion of this paragraph to include more details for regional weather fronts, and extreme fronts have been assessed in chapters 11, and 10.
18162	53	29	52	31	It might be necessary to indicate if the mentioned Atlantic and Pacific storm track activity is sourced in the northern or southern regions of those oceans [Gwenaëlle GREMION, Canada]	Taken into account. Chapter 2 aims to assess trends of hemispheric/global scales. A stipulated page limit precludes significant expansion of this paragraph to include more details for basin storm tracks/cyclones. More regional features have been assessed in Chapters 8, 10, and 11.
37500	53	29			Change "reanalysis" to "reanalyses" as otherwise it looks as if the text is referring to "reanalysis observations", which covers all assimilated observations. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37502	53	35			The NCEP/NCAR reanalysis is based on a data assimilation system that is not far off thirty years old. Is the trend noted here found in other reanalyses? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Statement has been amended and assessment for southern hemisphere extratropical cyclones from multiple reanalyses has been clearly indicated in the SOD.
13896	53	36	53	40	Could also note the strong interannual-decadal variability as a reason for lack of confidence. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified.
48454	53	42	53	53	Add more explicit assessment of the SH jet shift e.g. Swart et al 2015, Ivy et al 2017 [Julie Arblaster, Australia]	Accepted. More information included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26920	53	50	53	51	Please clarify if the poleward shifting in the subtropical jet stream is consistent for all seasons. [Prodomos Zanis, Greece]	Accepted. Clarified in the SOD.
13900	54	1	54	12	Our recent review paper might be of use here, suggesting no robust hemispheric trends in blocking, and only a few regional trends which are not consistent between methods (Woollings et al 2018, Current Climate Change Reports). [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The suggested literature has been reviewed and included in the SOD.
23814	54	4	54	4	Insert 'the' after 'over' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
13898	54	8	54	9	This statement on enhanced variability of blocking does not seem well supported by the references given. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement has been modified and more references reviewed.
23816	54	14	54	14	Insert 'that' after 'likely' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
50288	54	14	54	14	"that" is missing [Sophie SZOPA, France]	Editorial
17930	54	14	54	16	Again, are we talking about two or three major jets on each hemisphere? Typically, one talks about subtropical and polar jet; the latter seems to be called extra-tropical, or? Anyhow, the same terminology should be used consistently, or stated otherwise. [Branko Grisogono, Croatia]	Accepted. Text has been clarified and in the SOD only 'extratropical jet' is used, which refers to the subtropical jet and the polar jet.
18164	54	14	54	18	There is evidence that the extra-tropical jets in the Southern Hemisphere shifted poleward between 1-0.5 ka in a record from western Tasmania (Fletcher et al., 2018), and between 1-0.8 ka in a record from Southern Patagonia (Moreno et al., 2018). Both intervals coincide with the MCA. References: (1) Fletcher, M.S., Benson, A., Bowman, D.M., Gadd, P.S., Heijnis, H., Mariani, M., Saunders, K.M., Wolfe, B.B. and Zawadzki, A., 2018. Centennial-scale trends in the Southern Annular Mode revealed by hemisphere-wide fire and hydroclimatic trends over the past 2400 years. <i>Geology</i> , 46(4), pp.363-366, (2) Moreno, P.I., Vilanova, I., Villa-Martínez, R., Dunbar, R.B., Mucciarone, D.A., Kaplan, M.R., Garreaud, R.D., Rojas, M., Moy, C.M., De Pol-Holz, R. and Lambert, F., 2018. Onset and evolution of southern annular mode-like changes at centennial timescale. <i>Scientific reports</i> , 8(1), p.3458. [Gwenaëlle GREMION, Canada]	Taken into account. The suggested literatures has been reviewed and Moreno et al (2018) has been included.
45618	54	15			About the recent increase in jet meandering, the authors could consider referencing Cattiaux, J., Y. Peings, D. Saint-Martin, N. Trou-Kechout and S.J. Vavrus (2016), Sinuosity of mid-latitude atmospheric flow in a warming world, <i>Geophysical Research Letters</i> , 43, 8259–8268. doi :10.1002/2016GL070309 [Julien Cattiaux, France]	Taken into account. The suggested literature has been reviewed and included.
23818	54	16	54	16	Italicise 'medium confidence' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23820	54	18	54	18	Quantify 'recent decades' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been clarified.
32316	54	21	55	38	Surface winds are considered but not wind stress. AR5 Chapter 3 concluded with 'medium confidence that Southern Ocean wind stress has strengthened since the early 1980s.' Does this conclusion still hold in AR6? [Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Changes in the wind stress along with the other surface fluxes are assessed in Chapter 9 (Section 9.2.2.3). See also #32318 and 32320. Wind speed is increasing over Southern Ocean since the middle 1980s, this has been indicated in this section in the SOD.
57424	54	21	55	38	Please add uncertainties of estimated trends. [Marc Schröder, Germany]	Accepted. The numbers of trends are taken from the published literature as they are. Only trend uncertainties, which have been provided in the published literature and are in accordance with IPCC guidance, can be cited in the SOD.
23822	54	22	54	22	insert 'the' after 'over' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
57826	54	22		40	When considering the level pr trend of surface winf and sea level pressure, exerts of Ocean current must be properly monitored. The northern Gulf of Guinea is a part of the eastern tropical Atlantic where oceanic conditions due to the presence of coastal upwelling may influence the regional climate and fisheries. This is monitored by A sensitivity experiments based on the Regional Oceanic Modelling System (ROMS) is carried out to assess the role of the detachment of the Guinea current as a potential mechanism for coastal upwelling. An integrated station based solution to the surface wind and sea level pressure must be implemented, as of the case of Gulf of Guinea in the Atlantic ocean. [Abiodun Adegoke, Nigeria]	Rejected. Page limit precludes significant expansion of this section, and assessment of ocean current and upwelling is the purview of chapter 9 and is not covered here.
23824	54	26	54	26	Delete 'time' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35334	54	30	54	30	Reference for HadISD is incomplete - Dunn et al (2014) is just the homogeneity assessment. C/Should also include Dunn et al, 2012 (https://doi.org/10.5194/cp-8-1649-2012) and Dunn et al, 2016 (https://doi.org/10.5194/gi-5-473-2016) [Dunn Robert, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Complete references cited in the SOD.
23826	54	31	54	31	Delete negative sign, a negative trend of a negative value is positive! [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13902	54	31	54	31	Can this trend be given in percent per decade? [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The number of trend is taken from the published literature as it is.
23828	54	35	54	36	Delete negative signs, a negative trend of a negative value is positive! [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18166	54	36	54	36	Figure 2.19 reports trends for the period 1988-2017, but the texts uses 1979-2016. [Gwenaelle GREMION, Canada]	Noted. Text cited published literature, updated results cited in the SOD.
18168	54	43	54	54	Changes in land surface wind from HadISD2 is discussed first in text, and therefore I suggest that it should be the first panel on this figure. [Gwenaelle GREMION, Canada]	Accepted. Sub-figures' order has changed.
35336	54	48	54	48	Reference to HadISD 2.0.2.2017f is misleading. Dataset named as the ISD (Smith et al 2011) but given a HadISD version number and acronym. I suspect the reference should be to HadISD, but hence should be as indicated on the dataset page (https://www.metoffice.gov.uk/hadobs/hadisd/v202_2017f/download.html) or minimally Dunn et al, 2016, Expanding HadISD: quality-controlled, sub-daily station data from 1931, Geosci. Instrum. Method. Data Syst., 5, 473-491, https://doi.org/10.5194/gi-5-473-2016 , 2016. [Dunn Robert, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Correct reference for HadISD2 cited.
23830	55	2	55	2	Insert 'the' after 'Over' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18170	55	2	55	13	None of the datasets discussed in this paragraph is shown in Figure 2.19 which, in turn, presents two ocean only wind products that are understandably not used in the previous paragraph discussing land trends. Analised periods are also not consistent with what's shown in the figure. [Gwenaelle GREMION, Canada]	Noted. In this paragraph the assessment for wind speed was based on the published literatures. Figure 2.19 was not taken from published literature, but produced using updated datasets from four different kinds of sources.
57426	55	7	55	9	Fig. 2.19 shows results from ERA5 while this part discusses results based on ERA-Interim (with unclear referencing). I propose to compute trends based on ERA5 over 1979-2015 (or for JRA55 and ERA5 over longer period) and potentially reconfirm agreement. I further propose to remove the sentence in line 9 on MERRA or include MERRA2 in above analysis. [Marc Schröder, Germany]	Noted. In this paragraph the assessment for wind speed was based on the published literatures. Figure 2.19 was not taken from published literature, but produced to show the wind trends using updated dataset from four different kinds of sources. The sentence in line 9 on MERRA has been removed in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57428	55	10	55	13	Please add source of these informations (no trend in microwave-based observations, spatial features). Young et al. (2011) is missing in the references and refers to altimetry data only. [Marc Schröder, Germany]	Accepted. Text modified and new literature cited.
35662	55	15	55	24	What about in situ measurements of SLP? Why only use reanalysis? Why not HadSLP? [Nathan Gillett, Canada]	Noted. No enough peer-reviewed literature since the AR5 available for an assessment of in situ SLP changes over the globe.
23832	55	18	55	18	Change to 'subtropical' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23834	55	21	55	21	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23836	55	29	55	29	Change 'Colors' to 'Colours' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer included in Chapter 2.
23838	55	37	55	37	Insert 'the' after 'Over' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23840	55	50	55	50	Insert 'of' after 'reanalysis' and change 'stratospheric' to 'Stratospheric' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
40978	55	51	55	51	This should be "stratospheric circulation". [Johannes Laube, Germany]	Editorial
56104	56	1	56	1	unclear if MERRA 1 or MERRA 2 is meant here. If Merra 1 is meant: is there really no MERRA 2 product available that would allow the outdated MERRA one product to be avoided? Compare Chap. 1, pg 48, I24 [Rolf Müller, Germany]	Noted. Clarified in the SOD.
35664	56	1	56	3	What does 'significantly' mean in this context? That this increase is larger than internal variability? If so, what noise model is used for internal variability, and does it account for non-normal variability of stratospheric polar vortex temperatures? As elsewhere, I recommend that the assessment in this chapter focusses on quantification of trends and their observational uncertainty, rather than attempting to compare with internal variability. Related to this, why focus on mid-winter at the outset of this paragraph, given that a few lines later it is noted that differing trends have been reported based on season, altitude and period considered. [Nathan Gillett, Canada]	Taken into account. Text has been revised. Here only assess the observed changes, and avoided to address the mechanisms, external/internal variability.
23842	56	5	56	5	Insert space between number and unit (10 hPa) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23844	56	6	56	6	Delete , after 'altitude' and insert , after 'period' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23846	56	10	56	10	northernm polar vortex' should probably be 'Northern Polar Vortex' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Not changed at this moment. May change later after the terminology consistency for whole chapter/report determined.
35666	56	17	56	21	What does 'significantly' mean in this context? That this increase is larger than internal variability? If so, what noise model is used for internal variability, and does it account for non-normal variability of stratospheric polar vortex temperatures? As elsewhere, I recommend that the assessment in this chapter focusses on quantification of trends and their observational uncertainty, rather than attempting to compare with internal variability. Also, what season is this analysis applied to? Finally, this paragraph only cites a single study - are there others? [Nathan Gillett, Canada]	Taken into account. This paragraph aims to assess the robustness of the observed change by comparing the different datasets, but not the internal variability. Text has been modified for clarification and more references have been included.
46652	56	23	56	32	No conclusion on SSW trends [WGI TSU, France]	Taken into account. The long-term changes in the occurrence of SSW events over years are hardly usable for computing meaningful trend and this is noted in the new summary, also due to the problem of interpretation of multiple (per year) events. However, the changes in the polar vortex strength is assessed.
29540	56	24	56	24	Here, original papers which first described SSWs should be cited - it is known since 1952 what the effect of SSWs on the stratosphere is and how often they occur. The correct references are: Scherhag, R. (1952), Die explosionsartigen Stratosphärenwärmungen des Spätwinters 1951/1952. Berichte des deutschen Wetterdienstes in der US-Zone, 6, Nr. 38, 51-63. Labitzke, K., and B. Naujokat (2000), The lower Arctic stratosphere in winter since 1952, SPARC Newsletter, 15, 11–14. [Katja Matthes, Germany]	Rejected. The AR6 focuses on recent progresses since the AR5.
23848	56	31	56	31	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23850	56	34	56	34	Capital letters for 'southern hemisphere stratospheric vortex' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Not changed at this moment. May change later after the terminology consistency for whole chapter/report determined.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56106	56	34	56	34	As far as I know there was only one SSW event in the southern hemisphere (2002) so why is there a discussion of a >trend<? [Rolf Müller, Germany]	Accepted. SSW in the southern hemisphere is no longer mentioned in the SOD.
18172	56	34	56	34	With only 1 recorded major SSW in the southern hemisphere, I think that it's not meaningful to talk about a "trend". I would not mention SSW trends in this paragraph. [Gwenaëlle GREMION, Canada]	Accepted. SSW in the southern hemisphere is no longer mentioned in the SOD.
46650	56	34	56	36	No conclusion on southern hemisphere trends [WGI TSU, France]	Taken into account. More literature sources have been assessed and the analysis of trends in the Southern Hemisphere vortex was included.
39502	56	34	56	36	The following reference might be useful to expand the assessment in the Southern Hemisphere: Vy, D. J., C. Hilgenbrink, D. Kinnison, R. A. Plumb, A. Sheshadri, S. Solomon, and D. W. J. Thompson, 2017: Observed changes in the Southern Hemispheric circulation in May. J. Climate, 30, 527–536, https://doi.org/10.1175/JCLI-D-16-0394.1 [Carolina Vera, Argentina]	Taken into account. Suggested literature has been reviewed and included.
35668	56	34	56	36	There are other studies of changes in southern hemisphere stratospheric vortex temperature. See for example Figure 4-3 in the 2014 WMO Scientific Assessment of Ozone Depletion. [Nathan Gillett, Canada]	Taken into account. Suggested reference and has been reviewed and included.
35670	56	38	56	39	The months on which these trends are calculated should be reported. As assessed in this section, while the vortex has weakened in mid-winter, it has strengthened in spring. [Nathan Gillett, Canada]	Accepted. Seasons clarified in the SOD.
50290	56	38	56	39	need a conclusion about the southern vortex [Sophie SZOPA, France]	Accepted. More references reviewed and the trends in southern hemisphere vortex have included in the SOD.
35672	56	46	56	49	Which heights/pressures is this for? [Nathan Gillett, Canada]	Not applicable. BDC sub-section no longer included in Chapter 2.
40982	56	47	56	47	This should be the "average time". Also, the Engel et al., 2017 record is only valid between altitudes of ~24 and ~35 km, so does not capture trends in the lower stratosphere. Thirdly, the Engel et al., 2017 paper is an extension of the 2009 one, largely using the same record based on measurements of CO ₂ , CH ₄ , and SF ₆ . Finally, there is some recent evidence casting doubt on the validity of SF ₆ as a tracer of mean age due to its shorter lifetime (Kovacs et al., 2017, Ray et al., 2017) and the largely aircraft-data based finding that it yields ~20 % higher mean ages than 5 new mean age tracers (Leedham Elvidge et al., 2018). [Johannes Laube, Germany]	Not applicable. BDC sub-section no longer included in Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35674	57	1	57	5	This is poorly explained. The text here reports that adjusting for a poleward shift of circulation changes the AoA trends from an increase to a decrease, but doesn't say how this adjustment was done. [Nathan Gillett, Canada]	Not applicable. BDC sub-section no longer included in Chapter 2.
18174	57	2	57	2	The BDC acronym is not defined. It should be added to line 43 p.56 [Gwenaëlle GREMION, Canada]	Not applicable. BDC sub-section no longer included in Chapter 2.
56108	57	5	57	5	The Brewer-Dobson circulation is also influenced by volcanic eruptions, see e.g. Diallo et al., GRL, 2017 [Rolf Müller, Germany]	Not applicable. BDC sub-section no longer included in Chapter 2.
23852	57	7	57	7	No capital O for 'ozone' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. BDC sub-section no longer included in Chapter 2.
23854	57	14	57	14	Change 'vapor' to 'vapour' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. BDC sub-section no longer included in Chapter 2.
7848	57	16	57	16	where is the evidence for the 13% pre decade in summer? [zhiyan zuo, China]	Not applicable. BDC sub-section no longer included in Chapter 2.
28892	57	21	57	22	Perhaps in the lowermost stratosphere this a defensible statement but not overall, given the discussion above. Also see WMO 2018 (Ozone Assessment) Chapter 5 fig 5-9 (update of Ray 2014) [Matt Tully, Australia]	Not applicable. BDC sub-section no longer included in Chapter 2.
23856	57	29	57	29	Insert space between number and units (15 km, 40 km) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. QBO sub-section no longer included in Chapter 2.
29542	57	31	57	31	Again, I would urge to use original literature or review papers summarizing the knowledge since the discovery of the QBO such as: Baldwin, M. P., et al. (2001), The quasi-biennial oscillation, Rev. Geophys., 39(2), 179–229. [Katja Matthes, Germany]	Not applicable. QBO sub-section no longer included in Chapter 2.
23858	57	36	57	37	Insert space between number and units (50 hPa, 10 hPa) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. QBO sub-section no longer included in Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41144	57	41	57	41	<p>I recommend including information on the use of climatologies from GPS radio occultation for characterizing the QBO (see e.g., Schmidt et al., 2010; Wilhelmsen et al., 2018). Due to the high vertical resolution of GPS radio occultation, the QBO can be well characterized with RO temperature. Wilhelmsen et al. (2018) provided atmospheric variability indices resembling the QBO and ENSO, with Figures 9 and 10 therein presenting detailed QBO features including the disruption of the QBO.</p> <p>References: Wilhelmsen, H., F. Ladstädter, B. Scherllin-Pirscher, and A. K. Steiner (2018), Atmospheric QBO and ENSO indices with high vertical resolution from GNSS radio occultation temperature measurements, <i>Atmos. Meas. Tech.</i>, 11, 1333–1346, doi:10.5194/amt-11-1333-2018 Schmidt, T., J. Wickert, A. Haser (2010), Variability of the upper troposphere and lower stratosphere observed with GPS radio occultation bending angles and temperatures, <i>Advances in Space Research</i>, 46, 2, pp. 150-161, http://doi.org/10.1016/j.asr.2010.01.021 [Andrea K. Steiner, Austria]</p>	Not applicable. QBO sub-section is no longer in Chapter 2.
35678	57	43	57	44	<p>This assessment is based on a single study. How much confidence do we have in this finding? [Nathan Gillett, Canada]</p>	Not applicable. QBO sub-section no longer included in Chapter 2.
37504	57	43	57	44	<p>Some caution is needed with regard to what is stated in the first sentence of this paragraph. Kawatani and Hamilton (2013) 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. 161) has been made with regard to a reference in Chapter 3. 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)]</p>	Not applicable. QBO sub-section no longer included in Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28894	57	43	57	44	Actually I would argue the amplitude of the QBO at 70 hPa has increased again since the early 2000s [Matt Tully, Australia]	Not applicable. QBO sub-section no longer included in Chapter 2.
50292	57	46	57	46	is the disruption event statistically significant? [Sophie SZOPA, France]	Not applicable. QBO sub-section no longer included in Chapter 2.
23860	57	47	57	48	Insert space between number and units (25 km, 40 hPa, 25 km) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. QBO sub-section no longer included in Chapter 2.
7178	57	49	57	49	Watanabe et al. (2018) should be cited along with Osprey et al. (2016). Watanabe, S., Hamilton, K., Osprey, S., Kawatani, Y., & Nishimoto, E. (2018). First successful hindcasts of the 2016 disruption of the stratospheric quasi-biennial oscillation. <i>Geophysical Research Letters</i> , 45, 1602–1610. https://doi.org/10.1002/2017GL076406 [Shingo Watanabe, Japan]	Rejected/noted. This reference is no more applicable as the whole QBO sub-section is no longer included in Chapter 2.
23862	57	50	57	50	Change Nino to Niño [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. QBO sub-section no longer included in Chapter 2.
18176	57	51	57	53	Unclear if the QBO is affecting ozone via the BDC or if the comment on the BDC is a separate comment. If they are linked perhaps re-write as one sentence. [Gwenaëlle GREMION, Canada]	Not applicable. QBO sub-section no longer included in Chapter 2.
23864	57	52	57	52	Quantify 'record low' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. QBO sub-section no longer included in Chapter 2.
28896	58	2	58	2	To me this is a fairer statement than that made on the previous page (lines 43-44) and I am surprised it has made the executive summary [Matt Tully, Australia]	Not applicable. QBO sub-section no longer included in Chapter 2.
38398	58	7	61	22	I enjoyed reading the section on sea ice, and find that it gives a nice overview of recent observations. I find, however, that the assessment-character of the text should be strengthened. The text in my view currently reads a bit too much like a review rather than an assessment. [Dirk Notz, Germany]	Accepted. We tried to strengthen the assessment character of the text.
23866	58	8	58	8	Change 'a' to 'an' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
45402	58	13	61	22	I find both the Arctic and Antarctic sea ice sections to be a list of papers rather than a coherent, readable account of the overall changes. As such, I am unable to easily assess these sections consistency with the discussions in Chapter 9.3. I would suggest that in collaboration with Chp 9, these discussions can be considerably shortened and refined, leaving more detailed discussion to the later chapter. [Baylor Fox-Kemper, United States of America]	Noted. We attempted to strengthen the assessment character of the text. We also kept close contact with chapter 9 authors to assure consistency and avoid unnecessary duplications.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49294	58	13	61	22	This section notes the overlaps in topic with that in Chapter 9 section 9.3 Sea Ice. Please ensure minimal repetition and suggest that since Chapter 2 is a summary chapter, that it should refer to the appropriate details to be stated in the respective sections in Chapter 9. As it is now, section 9.2.3.4 refers to section 2.3.2.1, and section 2.3.2.1, although to focus on the global and large-scale change, presents only the regional issues in each region and does not do a comparison between the amount of observations, events, and processes occurring in the two sea ice areas. [Zelina Zaiton Ibrahim, Malaysia]	Noted. Some overlap between chapter 2 and 9 is unavoidable, in order to have each chapter being comprehensive and understandable. In chapter 2, we deal with sea ice on a hemispheric scale. We do not see this as a regional scale. Since Arctic and Antarctic sea ice have some fundamental differences in nature and setting, we chose to not generate a global sea ice area dataset.
14510	58	13	61	22	It is strange that much better opportunity exist in Arctic than in Antarctic for an in-depth investigation and assessment of pre-1979 change in sea ice extents or area, but actually a more attention has been given to the latter. Is this because the observed decline of sea ice extent in the Antarctic region over the last 40 years is not consistent with the expectation? Could you feel here some bias in our studies and assessments of climate change? (CUG, Guoyu Ren) [Guoyu Ren, China]	Noted. We intend to treat both hemispheres equally.
11590	58	15			Need to integrate pre-industrial sea ice studies e.g. sea ice distribution during the MCA and HTM. For references, enter "sea ice" in the search windows of these two maps: http://t1p.de/mwp and http://t1p.de/htm . [Sebastian Luening, Portugal]	Noted. Assessment of studies on pre-industrial sea ice is included in more detail in chapter 9.
12632	58	16	58	25	Add specifics from final version of SROCC about how much sea ice declined during those years to provide numerical context of the change since AR5. [Kristin Campbell, United States of America]	Accepted. More information from SROCC is included now in this paragraph.
12786	58	16	58	25	Add specifics from final version of SROCC about how much sea ice declined during those years to provide numerical context of the change since AR5. [Durwood Zaelke, United States of America]	Accepted. More information from SROCC is included now in this paragraph.
50294	58	18	58	18	"decreasing" => "decreasing by" [Sophie SZOPA, France]	Accepted, "by" is now added.
7338	58	18	58	18	decreasing by 9.4 to 13.6% per decade [Ashit Kumar Swain, India]	Accepted, it is now corrected.
42410	58	18	58	19	The sentence ends stating "with the largest decrease in summer". If possible, can the year of the summer with the largest decrease be mentioned? If this information does not exist, then perhaps say "...with the largest decreases during summer". [Elizabeth Fard, United States of America]	Noted. The sentence was slightly reworded, but different to the suggestion.
7340	58	18	58	19	Reorient the sentence. It may be rewritten as 'The spatial extent had decreased in every season, with the largest decrease in summer (high confidence), in every successive decade since 1979.' [Ashit Kumar Swain, India]	Noted. The sentence has been rewritten, but in a different way compared to the suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32194	58	24			"SROCC" should be properly indicated in the references list. Please apply the same criteria to refer this report in the reference list and in the text used for "SRCL" (please see above). [Isabel Trigo, Portugal]	Noted. SROCC (IPCC special report) is commonly cited as SROCC throughout the chapter.
35680	58	27	58	29	Is there really high confidence in multi-millennial reconstructions of Arctic sea ice coverage? For comparison on pg 60, ln 10, we have only medium confidence in observed trends in snow thickness on sea ice, which are a directly observed variable. [Nathan Gillett, Canada]	Noted. The sentence was reworded and the confidence level is not listed anymore.
52338	58	27	58	29	Wordy [Katherine Glover, United States of America]	Accepted. The sentence is now reworded.
18178	58	29	58	32	Also in reference to comment no 6. Text states low sea-ice coverage in early Holocene and increasing from the mid-Holocene. To support executive statement and comparison with today, it should be made clearer that sea ice areas was low in the mid-Holocene specifically (as the referenecs that follow indicate) [Gwenaelle GREMION, Canada]	Rejected. More details on this are given in chapter 9, as it is also cross-referenced.
35682	58	30			To what does 'comparably low sea ice coverage' refer to? The present? [Nathan Gillett, Canada]	Accepted/noted. Comparably refers to the first part of the same sentence. At the end of the paragraph a sentence is added on the current pan-Arctic sea ice changes.
44634	58	31	58	31	"until" should be insteaded by "to". [Liang Zhao, China]	Accepted. This is written as "to" now.
23868	58	34	58	34	This is confusing as written. Change to sea-ice (I don't think you mean Arctic Sea here) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Arctic sea-ice area" is written when also SIA is introduced.
31382	58	37	58	37	in addition to OR instead of. Can't use sth in addition to AND instead of sth else. [Gerhard Krinner, France]	Accepted, the wording is changed accordingly.
37978	58	39	58	39	replace "minimum extent" by "annual minimum" or "summer extent" [Jean baptiste SALLEE, France]	Accepted, is replaced by "summer extent".
31384	58	39	58	39	Arctic sea-ice low in 2012: worth establishing link to Greenland summer melt that year? [Gerhard Krinner, France]	Rejected. We do not discuss possible links between Greenland summer melt and sea ice, since this is beyond the focus of chapter 2.
7342	58	39	58	42	Reframing the sentence is required. [Ashit Kumar Swain, India]	Accepted. The sentence is now split and reworded.
23870	58	40	58	40	Change to exponential syle [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The units are now written with consistent formats.
7344	58	40	58	43	Uniform unit is required. Hence in Line 43, the unit may be written as 7400 km2/year instead of yr-1. [Ashit Kumar Swain, India]	Accepted. The units are now written with consistent formats.
23872	58	42	58	43	Don't split negative value and units across line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. There is no such a split anymore.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37506	58	42	58	43	With regard to the sentence that spans these two lines, and also Fig. 2.21 and this paragraph more generally, it should be noted that the Arctic sea-ice maximum usually occurs in March, but it can occur in February. Likewise the minimum usually occurs in September, but it can occur in August. So one cannot demonstrate that the lowest or highest sea-ice area occurred in a particular year without looking at numbers for August and February as well as September and March. Figure 2.21 could be changed to show for each year the minimum and maximum area, rather than the results for the two months when area was most likely to be greatest and smallest. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We chose the months that are most common in regards with minima and maxima occur.
31386	58	44	58	44	"A longer baseline...": starting when? [Gerhard Krinner, France]	Accepted. The data published by Walsh et al. 2017 start in 1850. We have added that information.
35684	58	44	58	46	There are multiple studies and datasets examining sea ice extent changes before the satellite record beyond the single Walsh et al. (2017) study cited here. Additional studies should be assessed. See e.g. Gagne et al. (2017) https://doi.org/10.1002/2016GL071941 , Titchner and Rayner (2014) https://doi.org/10.1002/2013JD020316 , Mueller et al. (2017) https://doi.org/10.1175/JCLI-D-17-0552.1 ; Pirón, M. Á. C., and J. A. C. Pasalodos, 2016: Nueva serie de extensión del hielo marino ártico en septiembre entre 1935 y 2014 [A new time series of September Arctic sea ice extent: 1935–2014]. Rev. Climatol., 16, 1–19. These studies generally show an increase in Arctic SIE prior to 1975. Further these datasets generally show that the decrease in September SIE began before the 1990s. [Nathan Gillett, Canada]	Noted. The Walsh dataset is seen as a representative dataset for the assessment.
23874	58	47	58	47	Change 'longterm' to 'long-term' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
31388	58	47	58	47	"superimposed by interannual variability". Did that variability change? [Gerhard Krinner, France]	Noted. We did not analyse the potential change of interannual variability, and we are not aware of studies who had focus on that.
42802	58	47	58	50	Recent study indicates that liquid precipitation also matters (Dou et al., The Cryosphere, 13, 1233–1246, 2019 https://doi.org/10.5194/tc-13-1233-2019) [Xiao Cunde, China]	Rejected. This is more an aspect of attribution, which is not supposed to be part of chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52340	58		67		For the "Cryosphere" section, I often see the word "proxy data" and "significant" used to discuss RELATIVE changes, with no values attached. I have always learned that "proxy data" signals a transfer function was used (to obtain, say, temperature reconstructions) and that "significant" referenced statistical outcomes. Should use of these terms be consistent and reconciled across the chapter, and the whole report? [Katherine Glover, United States of America]	Noted. We looked into the use of proxy and find the use is justified. For significance, numbers are sometimes not listed if the statement is connected to a summary or a citation.
7850	59	6	59	18	the references not included in the references list [zhiyan zuo, China]	Accepted. The respective data are now more properly cited.
18180	59	10	59	11	The time series for ice sheet thicknesses is relatively short for making conclusions about long term forecasts. Maybe this is worth highlighting - and making readers aware this is due to the difficulty in obtaining sea ice measurements. Older, pre-satellite measurements, especially those taken from boats could give a misleading impression of ice thickness change as these earlier expeditions tended to go in accessible locations where ice was inherently thinner. If somebody wanted to make a 200 year time series of ice thickness, it could give a very misleading impression as early measurements tended not to be able to get to the very thick ice. [Gwenaelle GREMION, Canada]	Noted. Some of these aspects are discussed in detail in the cited literature. Our assessment of ice thickness are covering the time since the mid 1970s.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12634	59	10	59	24	<p>According to the Arctic Report Card, strong, multi-year ice is now down to just 1% of the Arctic sea ice, which is a 95% reduction over the last 33 years. Osborne E., et al. (2018) Executive Summary, in ARCTIC REPORT CARD 2018 (“As a result of atmosphere and ocean warming, the Arctic is no longer returning to the extensively frozen region of recent past decades. In 2018 Arctic sea ice remained younger, thinner, and covered less area than in the past. The wintertime maximum sea ice extent measured in March of 2018 was the second lowest in the 39-year record, following only 2017. For the satellite record (1979-present), the 12 lowest sea ice extents have occurred in the last 12 years. The disappearance of the older and thicker classes of sea ice are leaving an ice pack that is more vulnerable to melting in the summer, and liable to move unpredictably. When scientists began measuring Arctic ice thickness in 1985, 16% of the ice pack was very old (i.e., multiyear) ice. In 2018, old ice constituted less than 1% of the ice pack, meaning that very old Arctic ice has declined by 95% in the last 33 years. The pace and extent of the changes to summer sea ice cover, along with regional air temperatures and advection of waters from the Pacific and Atlantic oceans, are linked to the spatial patterns of late summer sea surface temperature. August mean sea surface temperatures in 2018 show statistically significant warming trends for 1982-2018 in most regions of the Arctic Ocean that are ice-free in August.”). [Kristin Campbell, United States of America]</p>	<p>Accepted. The statement on the amount of MYI is updated, and citation mentioned is added.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12636	59	10	59	24	<p>Emphasize that reduced Arctic sea ice allows greater radiation in the region and also greater swell of waves in the Arctic Ocean, which can further disrupt sea ice and accelerate breaking up of ice; all of which can be positive feedback loops. Thomson J. & Rogers W. E. (2014) Swell and sea in the emerging Arctic Ocean, GEOPHYSICAL RESEARCH LETTERS 41:3136–3140, 3136 (“Ocean surface waves (sea and swell) are generated by winds blowing over a distance (fetch) for a duration of time. In the Arctic Ocean, fetch varies seasonally from essentially zero in winter to hundreds¹⁰⁰ of kilometers in recent summers. Using in situ observations of waves in the central Beaufort Sea, combined with a numerical wave model and satellite sea ice observations, we show that wave energy scales with fetch throughout the seasonal ice cycle. Furthermore, we show that the increased open water of 2012 allowed waves to develop beyond pure wind seas and evolve into swells. The swells remain tied to the available fetch, however, because fetch is a proxy for the basin size in which the wave evolution occurs. Thus, both sea and swell depend on the open water fetch in the Arctic, because the swell is regionally driven. This suggests that further reductions in seasonal ice cover in the future will result in larger waves, which in turn provide a mechanism to break up sea ice and accelerate ice retreat.”). At the same time, reduced sea ice provides favorable conditions for cyclone development and increased intensity of cyclones, which can also facilitate break-up of sea ice; see Day J. J. & Hodges K. I. (2018) Growing Land-Sea Temperature Contrast and the Intensification of Arctic</p>	<p>Rejected. Attribution of changes and associated processes are discussed in more detail in chapter 9.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12788	59	10	59	24	<p>According to the Arctic Report Card, strong, multi-year ice is now down to just 1% of the Arctic sea ice, which is a 95% reduction over the last 33 years. Osborne E., et al. (2018) Executive Summary, in ARCTIC REPORT CARD 2018 (“As a result of atmosphere and ocean warming, the Arctic is no longer returning to the extensively frozen region of recent past decades. In 2018 Arctic sea ice remained younger, thinner, and covered less area than in the past. The wintertime maximum sea ice extent measured in March of 2018 was the second lowest in the 39-year record, following only 2017. For the satellite record (1979-present), the 12 lowest sea ice extents have occurred in the last 12 years. The disappearance of the older and thicker classes of sea ice are leaving an ice pack that is more vulnerable to melting in the summer, and liable to move unpredictably. When scientists began measuring Arctic ice thickness in 1985, 16% of the ice pack was very old (i.e., multiyear) ice. In 2018, old ice constituted less than 1% of the ice pack, meaning that very old Arctic ice has declined by 95% in the last 33 years. The pace and extent of the changes to summer sea ice cover, along with regional air temperatures and advection of waters from the Pacific and Atlantic oceans, are linked to the spatial patterns of late summer sea surface temperature. August mean sea surface temperatures in 2018 show statistically significant warming trends for 1982-2018 in most regions of the Arctic Ocean that are ice-free in August.”). [Durwood Zaelke, United States of America]</p>	<p>Accepted. The statement of the amount of MYI is updated, and a citation is added.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12790	59	10	59	24	<p>Emphasize that reduced Arctic sea ice allows greater radiation in the region and also greater swell of waves in the Arctic Ocean, which can further disrupt sea ice and accelerate breaking up of ice; all of which can be positive feedback loops. Thomson J. & Rogers W. E. (2014) Swell and sea in the emerging Arctic Ocean, GEOPHYSICAL RESEARCH LETTERS 41:3136–3140, 3136 (“Ocean surface waves (sea and swell) are generated by winds blowing over a distance (fetch) for a duration of time. In the Arctic Ocean, fetch varies seasonally from essentially zero in winter to hundreds⁽¹¹⁾ of kilometers in recent summers. Using in situ observations of waves in the central Beaufort Sea, combined with a numerical wave model and satellite sea ice observations, we show that wave energy scales with fetch throughout the seasonal ice cycle. Furthermore, we show that the increased open water of 2012 allowed waves to develop beyond pure wind seas and evolve into swells. The swells remain tied to the available fetch, however, because fetch is a proxy for the basin size in which the wave evolution occurs. Thus, both sea and swell depend on the open water fetch in the Arctic, because the swell is regionally driven. This suggests that further reductions in seasonal ice cover in the future will result in larger waves, which in turn provide a mechanism to break up sea ice and accelerate ice retreat.”). At the same time, reduced sea ice provides favorable conditions for cyclone development and increased intensity of cyclones, which can also facilitate break-up of sea ice; see Day J. J. & Hodges K. I. (2018) Growing Land-Sea Temperature Contrast and the Intensification of Arctic</p>	<p>Rejected. Attribution of changes and associated processes are discussed in more detail in chapter 9.</p>
18182	59	10	59	25	<p>Difficulties in making conclusions on the state of ice thickness in the Arctic - could expand on methodological issues. E.g. with difference between ICESat and CryoSat-2 data, the latter of which can penetrate snow cover, while ICESat largely uses predicted snow depths. This can cause problems in making a time series of ice thickness change as there is a methodological change- how accurate are the ICESat thicknesses? Methodological issues would also apply to thicknesses from the pre-satellite era. For example point measurements are very accurate but have poor spatial coverage, so are they representative? [Gwenaëlle GREMION, Canada]</p>	<p>Rejected/noted. The questions mentioned are of interest, but we chose not to go into detail due to space limitations and balance within the chapter. Respective information can be found in the literature cited.</p>
18184	59	10	59	25	<p>Perhaps worth mentioning that while sea ice is decreasing regionally, it is subject to local variability - in the central Arctic it appears to be showing little to no change (Richter-Menge and Farrell, 2013., Perovich et al., 2014). [Gwenaëlle GREMION, Canada]</p>	<p>Rejected. Chapter 2 is focused on the larger spatial scales. Regional to local scale aspects are included in chapter 9.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50296	59	11	59	11	the sea ice terminology shouldn't it be rather in the glossary? [Sophie SZOPA, France]	Noted. The reference to the WMO nomenclature is seen as helpful as a background. Several of the relevant terms are also part of the Glossary.
7346	59	12	59	12	the ice cover, but has nearly disappeared, now making up less than 3% of the area. [Ashit Kumar Swain, India]	Accepted/noted. The sentence is now updated and reworded, and information on the area is added in the sentence.
7348	59	17	59	17	Remove bracket. (e.g., King et al., 2018) [Ashit Kumar Swain, India]	Editorial.
23876	59	18	59	18	Insert , after 'Nevertheless' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete. "Nevertheless" is removed now.
23878	59	18	59	18	Quantify 'recent decades' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now reworded to "since the 1990s".
7420	59	19	59	21	The sentence 'It should be noted that data collected in Fram Strait are connected to a larger geographical area upstream of the transpolar drift.' may be given at the end of the paragraph. [Ashit Kumar Swain, India]	Noted/obsolete. The text was changed, the wording is not included anymore.
48896	59	21	59	24	A conclusion on very high confidence on faster ice motion requires more than one reference. The faster ice drift was already observed by Hakkinen et al. (2008), Spreen et al. (2011) and Vihma et al. (2012). Further, the faster ice drift is not only due to thinner ice cover, but partly due to stronger winds. References: Hakkinen, S., A. Proshutinsky, and I. Ashik (2008), Sea ice drift in the Arctic since the 1950s, Geophys. Res. Lett., 35, L19704, doi:10.1029/2008GL034791. Spreen, G., R. Kwok, and D. Menemenlis (2011), Trends in Arctic sea ice drift and role of wind forcing: 1992–2009, Geophys. Res. Lett., 38, L19501, doi:10.1029/2011GL048970. Vihma, T., P. Tisler, and P. Uotila (2012), Atmospheric forcing on the drift of Arctic sea ice in 1989–2009, Geophys. Res. Lett., 39, L02501, doi:10.1029/2011GL050118. [Timo Vihma, Finland]	Accepted/noted. We included the additional references suggested. The statement is kept short, since the chapter is not focussing on attribution of changes.
38400	59	22	59	24	Is there a reference for this statement? [Dirk Notz, Germany]	Noted/obsolete. The sentence was removed.
18186	59	38	59	38	I believe it should be 'where less data is available' not 'where fewer data are available'. [Gwenaëlle GREMIION, Canada]	Accepted, obsolete. The text is modified and the formulation not anymore included here.
7422	59	38	59	39	Reframe the sentence as 'In the Atlantic sector, where fewer data are available, much higher snowfall resulting in snow-ice formation has been observed than in the Western Arctic'. [Ashit Kumar Swain, India]	Accepted, obsolete. The text is modified and the formulation not anymore included here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6657	59	38	60	2	This section about snow on sea-ice could benefit from a rewrite and reshuffling, insisting on the main message (thinning of the snow cover, with regional variations), and linking the medium confidence to the scarcity of snow-depth observations and the uncertainty of recent satellite estimates. The overloading and snow-ice formation (mentioned at the start and end of the paragraph) seems a rather non-significant mechanism that might indeed happen but not to a scale warranting impact on the time and spatial scales the AR6 report is concerned. An advice to sharpen the snow-on-sea-ice message here. [Thomas Lavergne, Norway]	Accepted. We wrote the snow on sea ice part more concisely. Parts of the points previously included here are now removed and included in the Atlas chapter.
44632	59	39	59	39	"more" instead of "higher" may be better. [Liang Zhao, China]	Accepted. "more snow on sea ice" is used now. The text was also changed else and reduced in length.
35686	59	39	59	41	Why do we need to compare recent observations with data from the 1970s, 80s and 90s to establish that snow on sea ice data are available exclusively from in situ observations? Isn't this known a priori? [Nathan Gillett, Canada]	Accepted, obsolete. The text is modified and the formulation not anymore included here.
6655	59	39	59	43	This sentence could be rewritten to make it clear that the topic is the scarcity of in-situ snow-depth observation and not a thinning of the snow layer. [Thomas Lavergne, Norway]	Accepted, obsolete. The text is modified and the formulation not anymore included here.
38402	59	40	59	40	There is a word missing here, I think this should be „shows that data/information/measurements of snow on sea ice...“ [Dirk Notz, Germany]	Accepted/obsolete. The paragraph was changed and the wording does not exist anymore.
23880	59	40	59	40	Insert 'measurements', or 'observations' or 'records' after 'ice' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph was changed and the wording does not exist anymore.
7424	59	43	59	43	Reduce the words in the sentence as 'snow in the Canadian Archipelago from the 1950s (Howell et al., 2016).' [Ashit Kumar Swain, India]	Accepted/obsolete. The paragraph was changed and the wording does not exist anymore.
7426	59	43	59	44	Reorient the sentence as 'The climatology studies by Warren et al. (1999) indicates weak trends toward declining snow depth'. [Ashit Kumar Swain, India]	Noted. The sentence was reworded along with other changes in this paragraph.
7428	59	44	59	44	Howell et al. (2016) [Ashit Kumar Swain, India]	Accepted/obsolete. The text was changed and the reference not anymore included here.
23882	59	44	59	44	Delete, after al. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph was changed and the wording does not exist anymore.
23884	59	45	59	45	Change 'for the Oct-May period' to 'for October-May' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph was changed and the wording does not exist anymore.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31390	59	45	60	2	This paragraph goes into very much detail. Maybe a bit too much for a "global" chapter? [Gerhard Krinner, France]	Accepted. We wrote the snow on sea ice part more concisely. Parts of the points previously included here are now removed and included in the Atlas chapter.
7430	59	47	59	47	Reframe the sentence as '(Cavaliere et al., 2012; Brucker and Markus, 2013)', [Ashit Kumar Swain, India]	Accepted. In-text citations are now consistently formatted throughout the report.
35688	59	51			The word 'however' here indicates a false dichotomy - the implication is that we would expect reduced spring snowpack to be driven by reduced snowfall rate, rather than increased melting. [Nathan Gillett, Canada]	Noted/obsolete. The paragraph is changed and commented wording not included anymore here.
38404	59	54	59	54	There is a word missing here, I think this should be „). Snow thickness of around 0.4 m...“ [Dirk Notz, Germany]	Accepted. "Snow" is added here. Note that the sentence was slightly changed.
7432	59	54	59	54	Reframe the sentence as '2015; Kurtz and Farrell, 2011) thicknesses around 0.4 m or more'. [Ashit Kumar Swain, India]	Not applicable. The respective sentence is not anymore included in the text.
49944	59	54			What is meant by "earlier"? Does this mean years earlier? Or earlier in the season? [Owen Cooper, United States of America]	Accepted. This means in earlier years than recent years. The sentence is now reworded and times are specified. The text was also changed else and reduced in length.
23886	60	4	60	4	Change 'sea ice' to 'sea-ice' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/noted. Arctic SIA is used here now.
6659	60	4	60	8	In the summary, state "very high confidence" for the SIE decrease. [Thomas Lavergne, Norway]	Accepted. The confidence level is added. We note that SIA is used instead of SIE.
27176	60	4	60	8	Please mention that the Danish Meteorological Bureau reports that, neglecting fluctuations, the sea Arctic volume has not shown a decrease since 2007, contrary to the previous period. [François GERVAIS, France]	Rejected/Noted. This part of the report focuses on observations. Observational data on Arctic sea ice volume are sparse, and we probably have no sufficient literature to support that statement. DMI published modelled Arctic sea ice volume data.
23888	60	5	60	5	Delete , after 'become' and after 'thinner' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the commas are removed now.
42412	60	7	60	8	Instead of Proxy indicators, you could state "Paleoproxy indicators show that...". Additionally, I'm not sure what the last sentence is trying to say? The syntax is confusing "Arctic Sea-ice area was as likely as not last as low.."? [Elizabeth Fard, United States of America]	Accepted/noted: We keep the proxy indicator wording, since we see this as a suitable term. We changed the sentence after.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7434	60	8	60	8	Reference required for the sentence Arctic Sea-ice area was as likely as not last as low as today in the mid-holocene. [Ashit Kumar Swain, India]	Noted. References connected to summary statements are given earlier in the text. We note that the respective sentence was changed.
23890	60	8	60	8	change 'Sea' to 'sea' and 'holocene' to 'Holocene' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete. The text was changed and the sentence not anymore included here.
23892	60	12	60	12	Change 'sea ice' to 'sea-ice' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete. The wording was changed, the actual part is removed.
23894	60	17	60	17	Delete 'too' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete. The wording was changed, the actual part is removed.
37508	60	17			The "too" at the end of the line can be deleted. The following word "inadequate" does not need qualification. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete. The wording was changed, the actual part is removed.
56110	60	18	60	18	SROCC: unclear reference [Rolf Müller, Germany]	Noted. SROCC (IPCC special report) is commonly cited as SROCC throughout the chapter.
31392	60	18	60	18	"inadequate": in what sense? [Gerhard Krinner, France]	Accepted, obsolete. The sentence is removed.
50298	60	22	60	22	SIE not defined yet [Sophie SZOPA, France]	Rejected. SIE was introduced on page 58, line 37.
7436	60	27	60	27	Specify the methods in the line amongst other methods (King and Harangozo, 1998; Murphy et al., 1995, 2014), [Ashit Kumar Swain, India]	Rejected. The references give more information for interested readers. Due to space limits we did not include more details on the methods here.
7438	60	28	60	28	References may be rewritten as (Hobbs et al., 2016a, 2016b; Holland, 2014). [Ashit Kumar Swain, India]	Accepted. The references by Hobbs et al. are now written with a first and b second.
7440	60	30	60	30	References may be rewritten as (Hobbs et al., 2016a, 2016b). [Ashit Kumar Swain, India]	Accepted. The references by Hobbs et al. are now written with a first and b second.
23896	60	31	60	31	Change 'Twentieth century' to '20th Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. 20th century is now written consistently.
23898	60	33	60	33	Change 'seas' to 'Seas' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Seas" is written with capital S now.
31394	60	34	60	34	Is the interpretation of these ice-core data sure enough to be called "evidence"? [Gerhard Krinner, France]	Accepted. The wording is changed, and the respective part is now included in the beginning of the same paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43190	60	44	60	44	Appropriate here to add reference to Simmonds, I., 2015: Comparing and contrasting the behaviour of Arctic and Antarctic sea ice over the 35-year period 1979-2013. Annals of Glaciology, 56(69), 18-28, doi: 10.3189/2015AoG69A909. [Ian Simmonds, Australia]	Rejected/noted. There is already a number of recent references cited here.
23900	60	49	60	49	Change 'Lower' to 'lower' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Lower" is now written as "lower".
43192	60	49	60	49	Change 'Lower' to 'lower' [Ian Simmonds, Australia]	Accepted. "Lower" is now written as "lower".
7442	60	50	60	50	Change the sentence to 'string of record maxima with the second-lowest maximum extent value in the modern days occurring in'. [Ashit Kumar Swain, India]	Noted. The sentence was reworded, as also other parts of this paragraph.
37980	60	50	60	50	replace "maximum extent" by "annual maximum" or "winter extent" [Jean baptiste SALLEE, France]	Accepted, is replaced by annual maximum.
23902	60	51	60	51	Insert , after 'minimum' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete: The wording was changed and the change is not actual anymore.
31396	60	53	60	53	"serve to reduce...": why "serve"? Sounds like there's an intention behind. Simply "reduce" might be sufficient [Gerhard Krinner, France]	Accepted, changed to "reduce".
18188	61	11	61	17	Similar to as with the section on Arctic sea ice - could expand on methodological issues, as to why its hard to create long term trends for sea ice change. [Gwenaelle GREMION, Canada]	Rejected. Space limits and the approach used in AR6 (as well as in the other assessment reports) do not permit these additions. However, in most cases information on the use of methods and their limitations can be found in the literature cited.
23904	61	14	61	14	Edit reference to Xie et al. (2013) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete: The paragraph was shortened and respective part removed.
23906	61	16	61	16	Give rate as exponential [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete: The paragraph was shortened and respective part removed.
23908	61	17	61	17	Poor scientific expression: quantify 'extremely sparse' and provide reference(s) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/noted/obsolete. The paragraph was substantially reduced in length and replaced by a short statement. "extremely" is removed.
6465	61	19	61	19	In the last paragraph state if the 2016 decline exceeded the earlier increase. What is the net change? [Hugh Lefcort, United States of America]	Noted. The connected figure (2.18b) shows that levels recently were similar or lower than at the beginning of the time series. In the text, we discuss the changes and their effect on trends.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54986	61	19	61	20	The phrase "significantly lower levels since 2016" can be strengthened if possible especially to provide contrast to the previous percentage values when communicating the observations to policy-makers. [Kilkis Siir, Turkey]	Accepted/noted. More detail is included now about the recent changes.
6661	61	19	61	22	There should be the same confidence (very high) for Arctic and Antarctic sea-ice extent (the passive microwave data records are not significantly less or more accurate in the Antarctic than in the Arctic). Thus state "very high confidence" twice: once for the slight increase until 2015, and second for the lower values since 2016. [Thomas Lavergne, Norway]	Accepted, confidence now the same level for passive microwave-based statements (note that sea ice area is used instead of sea ice extent).
56112	61	19	61	22	The conclusion in WMO/UNEP (ozone assessment, 2018) is that models cannot reproduce the observed sea ice trend. It would be important to have a statement in this report on this issue. [Rolf Müller, Germany]	Rejected. This part of the report focuses on observations, not modelling.
41150	61	22	62	19	Although mention previous assessment for the Southern Hemisphere (AR5). This section is exclusively based on data for the Northern Hemisphere. Both SROCC and Chapter 9 (section 9.5.4.1) include new references for the Southern Hemisphere that are worth to mention in this section. For example, although the number of evidence is still quite low, between 2000 and 2015 a significant loss of Snow Persistence in the Andes south of 25°S, particularly on the east side of the Andes has been founded by (Saavedra et al., 2018). The decreasing trend in SP coincided with the megadrought describe for the Central Andes region (31°S'35°S) (Garreaud et al., 2017). Garreaud, R. D., Alvarez-Garretón, C., Barichivich, J., Boisier, J. P., Christie, D., Galleguillos, M., et al. (2017). The 2010–2015 megadrought in central Chile: impacts on regional hydroclimate and vegetation. <i>Hydrol. Earth Syst. Sci.</i> 21, 6307–6327. doi:10.5194/hess-21-6307-2017. Saavedra, F. A., Kampf, S. K., Fassnacht, S. R., and Sibold, J. S. (2018). A snow climatology of the Andes Mountains from MODIS snow cover data. <i>Int. J. Climatol.</i> 37, 1526–1539. doi:10.1002/joc.4795. [Lucas Ruiz, Argentina]	Rejected. Chapter 2 does not claim completeness in terms of all changes, the components presented are a selection. Southern hemisphere was not included in that selection for the terrestrial snow subsection.
46638	61	25	61	25	No mention of Southern Hemisphere snow cover in 2.3.2.2 [WGI TSU, France]	Noted - Chapter covers hemispheric scale and snow cover is more regional for Southern Hemisphere.
35690	61	27			This does not seem to line up exactly with AR5 WGI Chapter 4 ES. That gives 'very high confidence' that 'snow cover extent has decreased in the NH especially in spring' based on the satellite record. And medium confidence that station observations generally indicate decreases in spring. [Nathan Gillett, Canada]	Accept - There was an error in the AR5 statement and revisions made to correct this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37510	61	33			The reference to "data from NOAA" is a bit vague [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accept - correct citations for data set used included in SOD
18190	62	4	62	19	Talking about high latitude snow cover decline could cause some confusion, given the large number of reports already out which predict higher precipitation levels into the future (that readers may inherently assume means snow). It may be good to briefly explain that in the Arctic, while precipitation may be increasing, its falling increasingly as rain, now snow - (I think this is well put in the abstract of 'Towards a rain-dominated Arctic' by Bintanja and Andry 2017. Upon hearing Arctic precipitation is increasing, it can be assumed its falling as snow. If the audience are aware of the increasing precipitation trends, but have not made the distinction that its falling as rain, they could be confused when reading a chapter that states high latitude snow cover is decreasing). [Gwenaelle GREMION, Canada]	Noted / partly rejected - This section deals specifically with snow on ground and considers changes in key indicators. Precipitation is covered elsewhere in the chapter and attribution of changes in snow cover are considered in Ch. 9. Explicit pointing to precipitation in conjunction with changes in SCE and SWE is essentially the analysis of mechanisms and, thus, relevant to Ch.8 scope.
29882	62	12	62	15	The decline of SWE is either demonstrated by remote sensing or reanalysis data. Correspondingly the length of the analyzed time series is mostly relatively short. I miss at least one reference, which shows the SWE decline based on in-situ data (e.g:DOI: 10.1175/JHM-D-16-0188.1). Moreover, in my opinion, there is a too strong focus on the arctic and some of literature is already referenced in the former paragraphs. The last reference (Thompson et la. 2017a) seems to be wrong, since it is about "tropical Pacific climate variability over the last 6000 years"! [Christoph Marty, Switzerland]	Accept comment regarding Thompson et al. - this was an error and reference removed in revised text. The other comment is noted and additional and/or new publications were considered in preparation of SOD
41152	62	17	62	19	The summary confidence assessment made for snow decrease in Northern Hemisphere since 1978 agrees with section 9.5.4.1. [Lucas Ruiz, Argentina]	Noted.
31398	62	19	62	19	medium confidence here is seemingly in contrast to the AR5 assessment given at the beginning of the subsection. Can you elaborate on this? [Gerhard Krinner, France]	Take into account - Note that High Confidence was assigned to reductions in SCE since 1978 while medium confidence referred to longer time period. Confidence levels reviewed for SOD and text revised to be clearer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45404	62	22	63	45	All of the papers discussed in this section are within the SROCC submission window, yet the AR5 is discussed rather than the SROCC. More recent work discussed in Chapter 9 should also be linked from this section. [Baylor Fox-Kemper, United States of America]	Noted. Both AR5 and SROCC are discussed in the beginning of section 2.3.2.3. More updates connected to SROCC are now included. Chapter 9 has to some extent a different approach than chapter 2, but it is tried to minimize unnecessary overlap, and to have the content consistent across these two chapters.
31400	62	24	62	26	This sentence is confusing. Most glaciers are shrinking - since when? "At times" is a bit vague [Gerhard Krinner, France]	Accepted. Sentence is reworded and information added.
41154	62	24	62	39	The resume of previous assessment is a well-done introduction to the different components of the cryosphere. Nevertheless, it is not clear why you highlight the mass loss during the last 70 years in the Arctic and the possibility that the rate mass loss is larger than in the previous 4000 yrs when the confidence is medium, and the evidence is limited. This could bring the wrong impression that we have greater confidence in this assessment and deviate the focus to what is relevant, the mass loss in the Arctic is very high. Check consistency with last version of SROCC chapter 3. [Lucas Ruiz, Argentina]	Noted. As for other subsections, the text anticipates to give an overview of the main conclusions relevant from AR5 and SROCC. There were made some modifications to the text relative to the FOD version.
7444	62	25	62	25	Rewrite the sentence as '(collectively, "glaciers") were at times smaller than that at the end of the 20th century, and with very high' [Ashit Kumar Swain, India]	Noted. The sentence was slightly reworded, and we see find it understandable in the form it is now.
9466	62	27			SROCC is not defined anywhere in this chapter [Jason Briner, United States of America]	Rejected. SROCC (IPCC special report) is commonly cited as SROCC throughout the chapter.
7446	62	32	62	32	Rewrite the sentence as 'further that very likely Arctic glaciers have lost significant mass between 1961 and 2016,' [Ashit Kumar Swain, India]	Accepted. The sentence is changed to a wording similar to the suggestion.
53324	62	32	62	32	the word significant should not be used this way. Please reword. [Jan Fuglestedt, Norway]	Accepted. "significant" is now removed.
40608	62	33	62	34	".. The rate and magnitude of loss is larger than any time during the past 4000 years" - I think this is not true. Please look at the Fig. 3. "Selected time series for glacier size spanning the past two millennia" in Solomina et al., 2016 "Glacier fluctuations during the past 2000 years" in QSR. For instance for Spitsbergen this is clearly not the case. Moreover, the records of glacier retreats in general are still rather poorly constrained for such a strong statement. [Olga Solomina, Russian Federation]	Noted. The statement is changed and now only addressing glaciers in the Canadian Arctic.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7448	62	40	62	40	Rewrite the sentence as '275 (respectively) local glacier chronologies, based on total 376 peer reviewed studies, and they'. [Ashit Kumar Swain, India]	Noted/obsolete: The text was changed and reduced in length.
23910	62	41	62	41	Change 'geologic' to 'geological' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete: The paragraph was changed and respective wording is not included anymore.
40612	62	41	62	43	I guess that the conclusion that "glaciers generally retracted to their minimum extent sometime between 9000 and 6000 years ago" is mainly based on the curves from the figure 2.25. In many regions the smallest glaciers were earlier, in the beginning of the Holocene, so this statement is not quite correct. Please compare to Solomina et al., 2015: "In both hemispheres, glacier advances in the mid Holocene (between ca 8 and 5 ka) were generally small in comparison to their LIA magnitudes". I would recommend to keep the boundary for small (but not minimum) glaciers as 6000 years ago, but move the beginning of the period to 8000 years ago because the advance ca 8,2 ka was quite large in some regions. I also noticed that in the Chapter 9 the authors refer to the chapter 2 for the orbital forcing of glacier variations, but I did not find it here. I guess this topic rather belongs to the ch. 9 and would keep it there. [Olga Solomina, Russian Federation]	Noted. We note that the text and the figure were changed. It is communicated with chapter 9 that chapter 2 is not discussing orbital forcing.
23912	62	43	62	43	Insert , after 'thereafter' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, the comma is included now.
49338	62	43	62	44	This section has a nice summary of Holocene patterns for comparison with the 20th-21st centuries. (Same for this chapter as a whole.) A minor point here: The significance of these observations re mtn glaciers could be clarified by editing to "Glaciers worldwide expanded thereafter, in response to the long-term multimillennial cooling throughout the middle to late Holocene; they reached their maximum..." In other words, it should be made clear here that these fluctuations are closely linked to climate, and that major, long-term retreat in the 20th-21st century represents a break from the overall late Holocene trend of glacier growth. [Yarrow Axford, United States of America]	Rejected. Chapter 2 is not supposed to use space for attribution aspects.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41156	62	43	62	45	This sentence, as it is expressed, gives the wrong impression that after 6000 years ago, glaciers all around the globe reach their maximum post-glacial (Holocene) during the LIA. Nevertheless, evidence in the Southern Andes and New Zealand (which are treated in Solomina et al., 2015) shown that glaciers were more extensive in the early to middle Holocene than in the late Holocene or LIA. I think this discrepancy, which expresses the difference in the climatic settings or teleconnections are worth to mention, similar to what you are doing for the particular case of the northern Canadian Arctic glaciers. [Lucas Ruiz, Argentina]	Noted. In chapter 2, we focus on the large scale changes, while chapter 9 also details more regional aspects. We note that some changes to the text in the same subsection beyond the commented lines where made.
18192	62	44	62	45	Terminology - Little Ice Age. A bit of a subjective term for dating, which is debated as a concept. It could be seen as Northern hemisphere centric (at least in the way it is normally read) - during periods of Southern hemisphere glacial advance, there was warming in the Northern Hemisphere and vice versa (Brook et al, 2011), so the Southern hemisphere does not perfectly fit the LIA picture. Maybe a tentative date range for the LIA would be good - especially for people outside of the field of geosciences. [Gwenaëlle GREMION, Canada]	Noted. The LIA is listed in the table of cross-chapter box 2.1, table 1 as one of the paleo reference periods.
40614	62	44	62	45	"Glaciers worldwide expanded thereafter" is true, but "reaching their maximum post-glacial position during the LIA" is not correct. In many regions the earlier Neoglacial advances were larger. What is certain is that during the LIA the glaciers advanced worldwide, but the extent and the timing of advances varied from region to region. [Olga Solomina, Russian Federation]	Accepted/noted. Parts of the respective text were changed, and regional and temporal variability is mentioned.
15642	62	45	62	47	The comparison between Holocene and present glacier extents needs to come with a statement about glacier response times (Johannesson et al. 1989) to make clear that present day's glaciers are out of balance with respect to current climatic conditions and, hence, committed to further ice loss. Jóhannesson, T., Raymond, C. F., & Waddington, E. D. (1989). A simple method for determining the response time of glaciers. In Glacier fluctuations and climatic change (pp. 343-352). Springer, Dordrecht. [Michael Zemp, Switzerland]	Accepted. Additional information is added, and the reference to johannesson et al. Included.
49340	62	49	62	52	A possible final point to add to this paragraph: It is projected that under continued warming in the 21st century some/many mountain glaciers will retreat to their Holocene minima and/or disappear (e.g., Larsen et al. 2017 http://dx.doi.org/10.1016/j.quascirev.2017.05.008 for mtn glaciers in Greenland; various studies for tropical mtn glaciers eg Yarleque et al. 2018 https://doi.org/10.1038/s41598-018-33698-z) [Yarrow Axford, United States of America]	Rejected. Future projections are not supposed to be included in this part of chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23914	63	13	63	13	Give rate as exponential [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/obsolete: The unit is now written in the text in words.
41158	63	21	63	21	"New global compilations" is a meaningless sentence, what Pfeffer et al., 2014 and RGI Consortium, 2017 present is a "new global compilation of glacier inventories." [Lucas Ruiz, Argentina]	Noted. Parts of the sentence are kept, but more references are included so that in our opinion the wording fits.
18194	63	25	63	27	Where was this mass loss experienced? Was it a regional or global trend? Or is this an average trend? [Gwenaelle GREMION, Canada]	Accepted/noted. The sentence was reworded, and it should become clear out of the context that it is a global trend, and for which time it is given.
41160	63	25	63	30	In section 9.5.1 we will also update the numbers. [Lucas Ruiz, Argentina]	Noted.
23916	63	27	63	27	Define/explain w.e. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The explanation "water equivalent" is now added to the text.
30226	63	30	63	30	there is only one paper from Brun et al, 2017, and the period is 2000-2016; not 2000-2017 [patrick Wagnon, France]	Point a) Rejected. There is a Brun et al. 2018 paper, which is a correction to the 2017 paper. Point b) Accepted/noted. The time span is now updated either to 2000-2016.
15644	63	30			there are a few new global estimates: - Zemp, M., Huss, M., Thibert, E., Eckert, N., McNabb, R., Huber, J., Barandun, M., Machguth, H., Nussbaumer, S.U., Gärtner-Roer, I., Thomson, L., Paul, F., Maussion, F., Kutuzov, S., and Cogley, J.G. (2019): Global glacier mass changes and their contributions to sea-level rise from 1961 to 2016. Nature, https://doi.org/10.1038/s41586-019-1071-0 - Wouters et al. 2019: Front. Earth Sci., https://doi.org/10.3389/feart.2019.00096 And many new regional studies: - SROCC and references therein - Frontiers Research Topic (with paper submission deadline 31 Dec 2019): https://www.frontiersin.org/research-topics/9957/observational-assessments-of-glacier-mass-changes-at-regional-and-global-level [Michael Zemp, Switzerland]	Accepted/noted. New publications suggested are cited now.
23918	63	34	63	34	I think this should be 'retreated' rather than 'retracted' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted/obsolete. The text was changed, the wording is not included anymore.
23920	63	34	63	34	Insert 'recent' after 'but' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted/obsolete. The sentence is not anymore included here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9468	63	34		35	regarding "but glaciers have overridden the evidence of previous retreat" I'm not sure this is accurate. In most areas, glaciers have not overridden their deposits made as they retreated during the last deglaciation. The latter argument (lack of resolution in glacial geologic dating) is certainly spot on. [Jason Briner, United States of America]	Noted/obsolete. This point is not anymore included here.
40616	63	35	63	35	Probably not only resolution but also a completeness of the records is not enough to make these comparisons. [Olga Solomina, Russian Federation]	Noted/obsolete. The text was changed, the wording is not included anymore.
27178	63	42	63	44	Please mention among the exception the glacier Perito Moreno that is iconic [François GERVAIS, France]	Rejected. The summary statements need to be very brief. Regional aspects are dealt with in more detail in chapter 9.
41162	63	42	63	45	In Chapter 9 we agree with the confidence assessments. [Lucas Ruiz, Argentina]	Noted. In SOD the second part of the statement was modified relative to the FOD.
31402	63	43	63	43	Again: glaciers are shrinking. OK - but since when? [Gerhard Krinner, France]	Accepted. Sentence is reworded and information added.
40618	63	43	63	43	I would add "still" currently more extensive... to underline the on-going and continuing processes (see also 8-46) [Olga Solomina, Russian Federation]	Accepted/noted. The summary statements was reworded, and the fact that the glaciers are not in balance with respect to climate conditions is mentioned explicitly.
16010	63	43	63	44	The inclusion of the statement "In most places, mountain glaciers are currently more extensive than during the early-middle Holocene" in the Executive Summary because it may convey the wrong message to the audience that the current warming level is not significant. In face, the accelerated shrinking and retreat of glaciers in recent decades are very significant: e.g. P.62, Lines 28-30, Lines 33-34, Lines 50-51; P.63, Lines 25-26. Suggest to highlight them in the Executive Summary as appropriate. [SAI MING LEE, China]	Accepted/noted. We reworded text and summary statement, where we also mention that the rate of glacier retreat observed recently appears unusual in a context of the Holocene.
40620	63	44	63	44	I think it is necessary to add here a sentence about the non-equilibrium state of the modern glaciers. The delayed reaction of the glacier front positions might be the reason why they are still larger than they were in the early-mid Holocene. The glaciers are retreating and will continue to retreat in response to current warming. [Olga Solomina, Russian Federation]	Accepted. Related statements and a reference are included now in the text.
45406	63	50	65	43	Sea level rise and ice sheet mass observations from GRACE offer considerably more precise estimates of loss in GrIS and AIS. These along with their uncertainties are discussed in Sections 9.4-9.6 and should be linked from here. For this reason, I am unsure whether the (medium confidence) on loss, etc., belong in this section. [Baylor Fox-Kemper, United States of America]	Noted (although unclear what the issue is). Large scale changes in Ice Sheets is within scope of chapter 9. Conclusions are based on longer record rather than just GRACE period. In the FGD, we will implement better links to chapter 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9470	63	55			<p>regarding "virtually certain that the ice sheet lost mass (high confidence)". I completely understand IPCC jargon, I find the IPCC jargon a little confusing. This says that SROCC (ie, the update on this since AR5) concluded that since 2000 it is >99% that the ice sheet total balance is negative, but then it says "(high confidence" = 80% chance), which seemingly contradicts the virtually certain claim.</p> <p>To me, one who studies this stuff, it seems there is no chance that the ice sheet hasn't lost mass since 2000. I'm not aware of a single reliable paper that would claim such. Thus I'm not sure how the "high confidence" is justified. [Jason Briner, United States of America]</p>	<p>Noted - Statement was from SOD of SROCC (i.e. summarizing conclusions of that report). Revised draft text includes statements from final version of SROCC.</p>
9472	64	2	64	3	<p>I think the word "interglacial" on these lines should be replaced with "deglaciation" I believe that is what is meant. We don't know anything about the history of fluctuations during the last interglaciations, just some model and a very few data estimates of where the ice sheet even was (its size). [Jason Briner, United States of America]</p>	<p>Take into account - correct terminology in revised draft</p>
8758	64	2	64	22	<p>Perhaps it would be worth mentioning the study by Yau et al https://www.pnas.org/content/pnas/113/35/9710.full.pdf indicating that Greenland contributed significantly to global sea level rise toward the end of the Eemian, but did not contribute significantly to sea level rises at 127 or 120 ka. [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Noted - Additional references considered in SOD preparation.</p>
9474	64	5	64	6	<p>something perhaps lost in translation here. These studies regard the last deglaciation not the last interglaciation - although Vasskog includes the last interglaciation.</p> <p>There are a variety of modeling studies and sea level and sea level finger printing studies (summarized in Dutton et al. (2015) that contain support for a smaller ice sheet during the last interglaciation than the Holocene. [Jason Briner, United States of America]</p>	<p>Take into account - correct terminology in revised draft; additional references considered in preparation of SOD</p>
31404	64	6	64	10	<p>Might be good to give some information about coincident temperature variations, if possible [Gerhard Krinner, France]</p>	<p>Rejected. Beyond mandate/scope (attribution in Ch 9) - air temperature variations covered in 2.3.1</p>
27700	64	10	64	10	<p>replace with published article [Poot Delgado Carlos Antonio, Mexico]</p>	<p>Noted - References updated in revised draft</p>
33252	64	10	64	13	<p>Larsen et al, 2015 (doi:10.1130/G36476.1), supports the spatial variability, and estimates a reduced ice sheet equivalent to 0.16 m sea level rise. [Kristian Kjelden, Denmark]</p>	<p>Noted. Additional references considered in preparation of revised draft</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49350	64	11	64	11	Again, suggest being explicit that this ice sheet change was a response to climate/temperature change. Could reword to "... behind its present-day margin during the middle Holocene, in response to elevated temperatures over much of Greenland through the early to middle Holocene. Maximum retreat occurred at different times and to different extents in different sectors, with the ice sheet overall reaching its minimum Holocene extent between 5 and 2 ka." [Yarrow Axford, United States of America]	Rejected - Beyond scope - only considering ice sheet changes rather than attribution which is covered in Ch 9)
23922	64	14	64	14	Change 're-advanced beginning' to 're-advance began' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
49348	64	14	64	14	I think 4 ka may be a bit too precise, given variability around Greenland. I also think it's worth linking the readvance explicitly to cooling temperatures. Suggest rewording to "In response to progressively cooling climate, the ice sheet readvanced in the late Holocene, as indicated by..." [Yarrow Axford, United States of America]	Noted - text says "around 4K" and that timing differed by sector. Regarding 2nd comment - attribution is beyond scope and covered in Ch. 9.
9476	64	15	64	16	Little Ice Age should be LIA [Jason Briner, United States of America]	Editorial
11592	64	18	64	22	Authors write: "A new study integrating proxy evidence of climate forcing and ice-sheet size with modelling indicates that the last time the rate of mass change was similar to 20th Century rates occurred during the early Holocene..." This statement is somewhat misleading. Of even greater importance should be to the readers when the Greenland ice sheet was SMALLER than today. This was the case over several millennia of the Holocene. See Briner et al. 2016, doi /10.1016/j.quascirev.2016.02.010. The question of when the ice sheet had a similar size than today is of secondary importance and distracts the reader from the even bigger question. [Sebastian Luening, Portugal]	Taken into account in revision. The reviewer raises a good point. The preceding text does mention that ice sheet retreated behind present margin in mid Holocene but reference to ice sheet size was not included in summary statement. Text revised to be clearer and take the reviewer's comment into account.
7450	64	21	64	21	Rewrite the sentence as 'occurred during the early Holocene (Briner et al., in prep), with generally consistency (Brook and' [Ashit Kumar Swain, India]	Editorial
27702	64	21	64	21	replace with published article [Poot Delgado Carlos Antonio, Mexico]	Noted - References updated in revised draft
31406	64	21	64	21	early Holocene: any informations about warming rates at that time? [Gerhard Krinner, France]	Rejected. Beyond mandate/scope (attribution in Ch 9) - air temperature variations covered in 2.3.1
9478	64	21	64	22	Is Brook and Buizert the correct reference here? There is a "Buizert et al., 2018" paper in GRL that models total balance of the ice sheet, but they do not compare this to contemporary values. [Jason Briner, United States of America]	Accepted - Citation was incorrect. Revised text includes updated references and correct citations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23924	64	21	64	22	Edit reference to Brook and Buizert (2018) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23926	64	24	64	24	Change 'have' to 'has' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23928	64	28	64	28	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23930	64	31	64	31	Delete second , [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23932	64	33	64	33	Delete negative sign, it is already noted as a loss [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
49946	64	33			Here it is stated with certainty that, "The total annual mass loss from Greenland reached a historical record value during summer 2012 (-627±89 Gt), which was the greatest one-season loss since the end of the LIA". But in the summary statement on page 65 line 5, this exact same event is described as being "likely" the greatest one season loss since 1840. This summary statement is not consistent with the main text in terms of IPCC uncertainty language (was it likely or not?) and time scale (1840 vs. the end of the LIA, which is defined as 1850 in Table 2.1) [Owen Cooper, United States of America]	Take into account - Revisions to text to ensure consistency throughout section.
9480	64	34	64	35	Regarding the sentence "The short-term mass loss trend..." While true, I think this doesn't represent the current state of knowledge very well. Latest GRACE data that I'm aware of from Goddard (AGU 2018) for example shows an increase in mass loss from ~ -255 Gt/yr in the 203-2009 interval to ~ -307 Gt/yr in the 2009-2016 interval. I think data like these are more important to focus on that using such precious words to discuss year to year variability. [Jason Briner, United States of America]	Taken into account in revision to section to ensure better balance re time scales.
15646	64	40	64	46	GRACE signal includes the mass changes of both the Greenland Ice Sheet and of the peripheral glaciers. In Greenland, the glacier signal makes 14 or 20% of the total contribution from Greenland (cf. Bolch et al. 2013, https://doi.org/10.1002/grl.50270). Pay attention to avoid double-counting. [Michael Zemp, Switzerland]	Comment taken into account in preparation of final figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33254	64	40	64	46	Reg. figure 2.26: Kjeldsen et al, 2015, (doi:10.1038/nature16183) also provides total mass balance for the entire ice sheet based on a similar approach to Box and Colgan 2013, but using an updated version of the Box SMB model and more discharge data, effectively producing an updated data set. Also, the presented dataset from Box&Colgan2013, starts in 1840, but is not the end of the Little Ice Age. There is a rather large spread of when the ice started to retreat from its LIA max extent. Recognizing the latter, Kjeldsen et al, 2015, used 1900AD as a Greenland-wide year as the end of the LIA. [Kristian Kjeldsen, Denmark]	Noted - Kjeldsen et al. 2015 is cited in the text with respect to mass loss since 1900. Time series updated for SOD. Also, revisions made to text to ensure consistency with respect to terminology.
9482	64	43			<p>this is cited differently in the caption associated with the figure. I believe it is the same paper.</p> <p>I tried to track down the data used in this figure and was unable to. I did find some sle data from the WCRP publication, but I didn't see where that publication/working group partitioned sle for Greenland in the years shown on this plot. I did find a data file for Greenland (use link below) that showed sle data from 2005.5 to 2015.5, but these values do not correspond to the red dots shown in Figure 2.26.</p> <p>https://www.seanoe.org/data/00437/54854/ [Jason Briner, United States of America]</p>	Editorial (1st comment). 2nd comment: Noted - Combined data sets were used for figure. Correct data source/references included with final figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33258	64	50	64	52	The ice-thickness measurements has not revealed this , but they are used to generate ice discharge measurements. A combination of air- and satellite-borne laser- and radar- altimetry, Gravity data from GRACE, and ice input-Output (SMB minus ice discharge (from ice velocity in combination with ice thickness data) have all revealed widespread thinning and accelerated mass loss of the ice sheet. [Kristian Kjelden, Denmark]	Noted and taken into account. The sentence does say that the ice discharge measurements have revealed widespread acceleration (also indicates that ice discharge measurements were derived from ice thickness measurements). This would appear to be in agreement with reviewer comment. Some information in the section is provided about methodology utilized. The preceding paragraph does mention that an analysis of a combination of information (air photo, geomorphic information) has indicated mass loss. Figure 2.26 also documents results from GRACE. Revisions to text made to be clearer with respect to methodology.
35692	64	51			Clarify that this is referring to marine terminating outlet glaciers around Greenland (rather than in the Antarctic). [Nathan Gillett, Canada]	Noted - These results are for Greenland - This section is only concerned with Greenland not Antarctic which is covered in 2.3.2.4.2
33256	64	52	65	1	I would encourage the authors to include Mougnot et al, 2019, PNAS (https://doi.org/10.1073/pnas.1904242116), and Mankoff et al, ESSD2019 (https://doi.org/10.5194/essd-11-769-2019) as these provide updated time series from 1970s/80s through 2017, providing a more comprehensive view on recent changes. [Kristian Kjelden, Denmark]	Noted. New references considered in preparation of revised draft, including updates to time series.
27704	65	1	65	1	check bibliographic citation [Poot Delgado Carlos Antonio, Mexico]	Editorial (citations checked and corrected in revised draft)
23934	65	1	65	1	Delete (before 'van' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
16012	65	3	65	7	According to P.64, Lines 28-31, the ice mass loss of Greenland increased fourfold from the 20th century (~70 Gt/year) to early 21st century (~270 Gt/year) which is a very significant change and should be reflected in the summary. [SAI MING LEE, China]	Taken into account in revisions to text
9484	65	3			Little Ice Age should be LIA [Jason Briner, United States of America]	Editorial
23936	65	4	65	4	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23938	65	5	65	5	Delete negative sign, it is already noted as a loss [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37512	65	5			See comment 73. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Comment not specific and is thus unactionable.
9486	65	6			"smaller than present" of course this depends on how "present" is defined. But I don't have a better suggestion. [Jason Briner, United States of America]	Noted - taken into account in revised text
15648	65	14	65	15	Note that RGI 6.0 (and hence most glacier studies) currently does not include the peripheral glacier on the Antarctic mainland (located on the Antarctic Peninsula (Huber et al. 2017) and in the Dry Valleys (Fountain et al. 2016). Huber, J., Cook, A.J., Paul, F. and Zemp, M. (2017): A complete glacier inventory of the Antarctic Peninsula based on Landsat7 images from 2000–2002 and other pre-existing datasets. Earth Syst. Sci. Data, 9: p. 115-131. doi:10.5194/essd-9-115-2017 Fountain, Andrew G., Hassan J. Basagic IV, and Spencer Niebuhr. 2016. "Glaciers in Equilibrium, McMurdo Dry Valleys, Antarctica." Journal of Glaciology 62 (235): 976–89. https://doi.org/10.1017/jog.2016.86. [Michael Zemp, Switzerland]	Noted/rejected. Within the limitations of the chapter, we did not detail Antarctic peripheral glaciers. The chapter is assessing selected large-scale indicators of climate change.
23940	65	18	65	18	Delete 'The' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "The" is removed now. We note that the sentence was also modified else.
48712	65	18	88	19	The claim that West and East Antarctica experienced rapid retreat about 14,600 is not supported by the subsequent citations and is further disputed by the findings of RAISED consortium of glacial geologists: "Even after taking dating uncertainties into account this is consistent with only a minor contribution of Antarctica to this melwater pulse", Bentley et al, A community-based geological reconstruction of Antarctic Ice Sheet deglaciation since the Last Glacial Maximum, Quat. Sci Reviews 100, 2014 1-9. The possibility of such a retreat can't yet be ruled out, but the geological support is not there. [Lev Tarasov, Canada]	Noted/Taken into account. This paragraph was significantly revised in the SOD, including the mentioned statement.
23942	65	21	65	21	Insert 'the' before 'Ross' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "the" is added now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31408	65	27	65	27	There are several other publications besides Frezotti et al - might add recent work by Thomas et al, for example [Gerhard Krinner, France]	Rejected. Thomas et al. 2017 is cited in the sentence after the one commented here, along with another reference.
7452	65	28	65	28	Rewrite the sentence as 'growth rate in Antarctica is estimated to be 7 ± 1.3 Gt between 1800 and 2010 CE and 14 ± 1.8 Gt' [Ashit Kumar Swain, India]	Noted. The original file obviously contained an open change (from track changes). To be followed up. Otherwise, the proposed change does not differ from what is written.
7454	65	31	65	31	Line 31: 2015; Rewrite the sentence as 'Swain et al., 2015). ' Reference: A. K. SWAIN, P. K. SHRIVASTAVA, R. ASTHANA, A. CHATURVEDI, S. K. ROY, A. DHARWADKAR, RAGHURAM, P. KUMAR, R. K. MALLIK and M. J. BEG (2015). Fluctuation of snow accumulation and ablation pattern near Schirmacher Oasis, East Antarctica. Abstract volume of the International symposium on Antarctic Earth Sciences held at Goa during 13-17 July, 2015, pp. 183. [Ashit Kumar Swain, India]	Rejected. The reference is a conference abstract, not a peer-reviewed article.
31410	65	33	65	33	Mass balance of East Antarctica not significantly different from zero... need an assessment here, more refs, or (better?) refer to Ch 9 assessment [Gerhard Krinner, France]	Accepted. The mass balance of East Antarctica is now more detailed incl. citations in the text.
31412	65	36	65	36	Same for the peninsula: many more recent refs available, see Ch 9; might again refer to Ch 9 here. [Gerhard Krinner, France]	Accepted. The mass balance at the Antarctic peninsula is now more detailed incl. citations in the text.
35694	65	36	65	38	Is this for E Antarctica or the whole of Antarctica? [Nathan Gillett, Canada]	Accepted. Reference to new work (Rignot et al., 2019) about the AIS is included at the end of the paragraph before this one, and AIS is specified.
7456	65	37	65	37	Rewrite the sentence as 'mass balance decrease from -55.6 ± 5 Gt per year in 1999-2009 to -158.7 ± 8 Gt/yr in 2009-2017' [Ashit Kumar Swain, India]	Not applicable. The sentence is updated and rewritten, no at the end of the previous paragraph.
23944	65	37	65	37	Delete negative signs, it is already noted as a loss [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected/noted. The negative signs indicate the negative mass balance level, not the change between the two levels. But we updated and changed the wording and content in the text to "loss", then the negative signs are replaced by positive (no) signs.
31414	65	37	65	37	per year, not per ear [Gerhard Krinner, France]	Noted. Units are now written consistently with exponents.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27180	65	40	65	43	Please mention that the overall mass balance of Antarctica is of the order of 0.01 % since the beginning of measurements [François GERVAIS, France]	Noted/taken into account. In the text prior to the summary statements, new findings on AIS mass balance are given. The summary statement is limited in detail in order to be concise.
57828	66	4		55	Permafrost temperature increased and released of CH4 and CO2 in large quantities has been published on the 23rd of April 2019 by (John Watts). Permafrost melt is the main concern. greenhouses gases which are released when organic matter that had been frozen below the soil for centuries thaws and rots have already begun to escape at the current level of 1°C of Global heating. So far the impacts is small.Ten gigatonnes of carbon have been released from the permafrost but this ource of emissions will grow rapidly once temperature rise beyond 1.5°C. Therefore on thr current trajectory atleastv3°C of warming by the end of the century, melting permafrost is expected to discharge uptob28p gigatonnes of carbon dioxide and 3 gigatonnes of methane, which has a Climate effects that is 10 to 20 times stronger than CO2.Even at 1.5°C to 2°C there are impacts cost due to thawing permafrost. It would also add to global inequality because most of the economic burden equivalent to almost the entire worlds current annual GDP is likely to be borne by countries n warmer poorer regions such as; India and Africa, which are most Vulnerable to rise in temperature. we have the technology and policy instruments to limit warming but we are not moving fast enough. i strongly recommend point based solution policy making to be integrated along with the instruments available for combating and reducing warming. [Abiodun Adegoke, Nigeria]	Rejected. Beyond mandate/scope of the chapter - implications regarding carbon covered in Ch 5.
8760	66	13	66	19	The study by Vaks et al 2013 (https://science.sciencemag.org/content/340/6129/183.full) indicates that under climates only slightly warmer than today (as experienced during marine isotope stage 11) would cause widespread thawing of continuous permafrost in Siberia. [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Additional references considered in preparation of revised draft.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39346	66	14			<p>Section 1.5.1.2 P1-66 please insert in line 14:</p> <p>To understand this signal to noise problem it is helpful to recall that for scales longer than weather scales (about 10 days), instrumental and paleo temperature analyses display temperature fluctuations that systematically decrease with scale as the averaging period increases: the fluctuations largely cancel each other out, the temperature appears to be stable. Without external forcings (e.g. in GCM control runs), this continues to arbitrarily long time scales and characterizes the approach of the model to its long term climate (Lovejoy, Schertzer et al. 2013), (Lovejoy 2019), (Lovejoy, Varotsos et al. 2018). However, due to external forcings and/or very slow internal processes, at some critical time scale τ_c, the variability stops decreasing and starts to increase. τ_c punctuates the end of the high frequency macroweather regime. At lower frequencies - in the climate regime - temperature fluctuations grow with increasing time scale so that the temperature appears to “wander”, to be unstable (Lovejoy and Schertzer 2013), (Lovejoy 2013). At τ_c, the internal variability becomes dominated by the responses to external forcings (and in the pre-industrial epoch, possibly to new slow internal processes). In the last decades, τ_c is about 16-18 years (Lovejoy 2014). At shorter time scales, the internal variability is the dominant source of variability, at longer scales, the forced response is dominant. Over the late Pleistocene, the average τ_c was \approx 300 years although it varies at different phases of the glacial-interglacial cycle, and may be as long as several millennia in</p>	Wrong Chapter - Section number within comment implies Ch. 1 (although comment does appear relevant to section 2.3.2.5).
55912	66	16	66	17	Very misleading formulation. Better to say "Thawing started approximately 250 years ago..." [Martin Stendel, Denmark]	Editorial - Taken into account in revised text
7458	66	18	66	18	Line 18: Rewrite the sentence as ‘permafrost persists in peatlands at the southern extent of the permafrost, zone there has been thawing’ [Ashit Kumar Swain, India]	Accepted - Text revised
23946	66	30	66	30	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
8762	67	0	67	0	Section 2.3.3: You should mention the global SST reconstruction of McGregor et al 2015 (DOI: 10.1038/NGEO2510) which documents a cooling trend for the Common Era, with no clear expression of the Medieval Climate Anomaly. [Vasile Ersek, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - McGregor et al.'s reconstruction now cited. However, the resolution is too coarse for detecting the MCA.
49948	67	2			Climate is typically defined over a 30-year period, so the expression "interannual variability in climate" doesn't really work. Would be better to just say "interannual variability in temperature" [Owen Cooper, United States of America]	Accept and Taken into account in revision

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23948	67	7	67	7	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
55914	67	10	67	10	...Antarctica... [Martin Stendel, Denmark]	Editorial
31416	67	14	67	26	Rather detailed material that might better fit into Ch 9? [Gerhard Krinner, France]	Taken into account in revised text - Reduced to focus on evidence of thaw (within Ch 2. mandate) rather than process.
23950	67	17	67	17	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35290	67	17	67	17	definitely need to define lithalsa here, and probably palsa as well. [Charles Koven, United States of America]	Noted. Section has been revised significantly. Note that glossary includes definitions for terms like these that appear in the report.
41164	67	28	67	29	In Chapter 9 we agree with the confidence assessments. [Lucas Ruiz, Argentina]	Noted.
27182	67	34	69	34	Please cite and discuss Wunsch, C., Heimbach, P., 2014, Bidecadal thermal changes in the abyssal ocean. J. Phys. Oceanogr. 44, 2013, who estimate the heat content down to abyssal depths: "a total change in heat content, top to bottom, is found of approximately 4E22 J in 19 years, for a net heating of 0.2 W/m2, smaller than some published values." They conclude: "the uncertainties in all the fields remain too large to rationalize, for example, the apparent pause in warming". [François GERVAIS, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
6467	67	36	67	36	Expand the discussion for below 2000 meters and discuss if this accounts for the missing atmospheric heat. [Hugh Lefcort, United States of America]	Noted. Text and figures for Section 2.3.1.1 have been restructured
46122	67	41	67	50	Might be worth also adding hard tissue accretion in the list of reconstructions, it's a rapidly growing field with sub-annual recordings of both SST and deep ocean spanning up to 1400 years. [Amy Featherstone, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Agreed that Sclerochronology has advanced significantly since AR5. Author not aware, however, of any ocean basin-scale (or larger) reconstruction that has emerged to address the large-scale remit of CH2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37982	67	41	67	50	It would help the reading to keep some consistencies between the paleo paragraphs and the contemporary paragraph in terms of quantification of change. Paleo is quantified in MOT change or MOT change per century, or OHC change per century in ZJ. It would be usefull to provide a comparison standpoint, i.e. what is the "current" (past decades) rate of MOT change or OHC change in ZJ per century. I like the fact to have paleo and contemporary next to each other, but if we can learn something from it, and here the message is hard to interpret. [Jean baptiste SALLEE, France]	Accepted. Text and figures restructured
23952	67	46	67	47	Don't split number and units across a line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37984	67	47	67	47	I am not sure about "OHC uptake". Please use "Ocean heat content increase" or "ocean heat uptake" [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
23954	67	47	67	47	Give rate as exponential [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. ZJ represents 10 ²¹ J. Consistent use across AR5, SROCC and AR6.
27706	67	52	67	52	check temperature data [Poot Delgado Carlos Antonio, Mexico]	Rejected. Unspecific comment and thus unactionable.
23956	67	54	68	1	Insert space after 2nd 'the' and change to Pre-Industrial era [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37986	68	1	68	5	I find hard to understand what we learn by discussing Pacific IWT. What do we learn from that? Is that giving us more/less confidence in the previous quatifications/assessment? It looks like it could be removed altogether. [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
42414	68	1	68	5	Add "especially in the Southern Hemisphere, and to small climate perturbations..." [Elizabeth Fard, United States of America]	Editorial.
54988	68	4	68	4	The reference of "(Chambers et al., 2016)" on ocean heat content is missing from the list of references. Only "Chambers et al., 2014)" is included on a different topic. The year of publication of the missing reference is also unclear and may be another "(Chambers et al., 2014b)" that involves results for the "the complex upper ocean heat content structure." [Kilkis Siir, Turkey]	Noted. Text and figures for Section 2.3.1.1 have been restructured
18196	68	7	67	7	It's unclear what a "Sediments records of ocean heat content" is. I'd suggest rephrasing as: "Estimates of ocean heat content from proxy temperatures recorded in sediments". Besides, a reference might be needed. [Gwenaelle GREMION, Canada]	Accepted. Reworded as suggested.
23958	68	7	68	7	Chnge to metres, or better, m [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35696	68	7	68	9	Is this the local change in IWT, or an estimate of the global mean? This seems like a lot of warming. [Nathan Gillett, Canada]	Taken into account. It is local but apparently indicative of change in the world ocean.
37988	68	11	68	12	Which period compared to which period. MCA vs LIA. Would be good to clarify [Jean baptiste SALLEE, France]	Accepted. Clarified in the text.
23960	68	14	68	14	Space required between number and unit (700 m) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37990	68	14	68	15	Figure 2.29 is from 1993 to present inconsistent with the text. I think the 0-700 m change should be described (and shown consistently) from 1970 to present, consistently with Chap 7 and 9. If you think otherwise, please liase with chap 7 and 9. [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
45224	68	14	68	15	The text here mentions "direct observations" and then cites Figure 2.29, based on an ensemble that is dominated by ocean reanalyses (i.e. 3 out of 5). I don't think we can refer to these ocean data assimilation products as "direct observations". The text also cites warming relative to the 1970s and yet the figure only shows warming patterns since 1993 (presumably because some or all the ocean data assimilation systems make use of satellite alimeter data). This inconsistency needs to be addressed. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text and figures for Section 2.3.1.1 have been restructured
52342	68	15	68	15	Parenthetical references need clean-up - there are parentheses nested in parenthesis or alongside others where they could be combined; some incomplete pairs. [Katherine Glover, United States of America]	Accepted. Text and figures restructured
23962	68	17	68	17	Space required between number and unit (2000 m) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23964	68	19	68	19	Space required between number and unit [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37992	68	20	68	20	A confidence level should be provided here [Jean baptiste SALLEE, France]	Noted
23966	68	20	68	20	Insert space before ([Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35698	68	20			Replace 'an acceleration in the global ocean warming signal has been proposed recently' with 'an increase in the rate of global ocean warming has recently been identified'. [Nathan Gillett, Canada]	Noted. Text and figures for Section 2.3.1.1 have been restructured
23968	68	21	68	21	Quantify 'recent decades' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text and figures restructured
37994	68	21	68	23	Again confidence level is missing, as well as period over which those trends apply. I recommend to liase with Chapter 9 as there is overlap and those are provided there. [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38550	68	22	68	22	Though it is difficult to detect regional OHC trends associated with global warming as pointed out here, specific water mass warming were relatively large and could be detected Sugimoto et al. 2017, which may be cited here. Sugimoto et al., 2017, Enhanced warming of the subtropical mode water in the North Pacific and North Atlantic, doi:10.1038/nclimate3371 [Shinya Kouketsu, Japan]	Noted. Text and figures for Section 2.3.1.1 have been restructured
23970	68	34	68	34	Space required between number and unit (700 m) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
37996	68	45	68	46	Over which period of time? Note that for 1971-2014 your assessment is inconsistent with Chap 7 (high confidence in their chapter). Please liase with them. More generally, there are many inconsistencies with other chapters in this section. I highly recommend that your chapter better coordinate with the rest of the chapters. [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
35700	68	45	68	46	The global ocean is not a sink of heat in the earth system. It could be described as a sink of heat for the atmosphere. But I would suggest replacing 'sink' with 'store'. [Nathan Gillett, Canada]	Noted. Figure and caption revised
35702	68	50			The phrase 'reaching back in time' suggests paleoclimate context or at least the early instrumental period, but the figure referred to here only goes back to 1993. [Nathan Gillett, Canada]	Noted. Text and figures for Section 2.3.1.1 have been restructured
37998	68	52	68	52	Figure 2.30: Y-axis in this figure b/c is misleading. Please consider starting from the axis at 0, and with consistent axis for b/c [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
50300	68	53	68	53	remove the second "used" [Sophie SZOPA, France]	Editorial.
45218	69	2	69	3	It is not obvious from Figure 2.30c that the uncertainty is reduced during the Argo period. I think it would help to show a consistent Y-axis (preferably including the Y=0 line) and the same set of analyses between panel b) and c). This would greatly aid the comparison for the reader. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text and figures for Section 2.3.1.1 have been restructured
52346	69	5	69	5	(Lowell et al., 2014) is not in the bibliography on p. 123 [Katherine Glover, United States of America]	Noted. All references should now be included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38000	69	5	69	8	Periods and layer should be clarified. What is "upper", "deep" in this context. I guess 0-700 and 700-2000... Please clarify. Also please note Chap 9 assessment: "It is likely (medium confidence) that the 0-700m layer warmed from the 1870s to 1971 and virtually certain that this layer warmed over the periods 1971-present and 2005-present". I understand this is a slightly different statement as Chap 9 discuss warming while you discuss OHC, but, there is a need that you contact chap 9 for coordination. [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
23972	69	7	69	7	Delete , after 'both' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
45220	69	11	69	11	Figure 2.30: I think it would be helpful to include a secondary Y-axis on all panels that expresses the heating rates in $W m^{-2}$, relative to Earth's surface area. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text and figures for Section 2.3.1.1 have been restructured
45222	69	11	69	11	Figure 2.30: panel a). I wonder if it makes more sense to show discrete (rather than overlapping) warming layers, i.e. 0-300 m, 300-700 m, 700-2000 m, and 0-2000 m ? I would also suggest including an estimate for the layer > 2000 m, even if just assessed as a linear trend. I wonder if we really need a 0-300 m layer? Perhaps 0-700, 700-2000 and >2000 m is sufficient (this also tends to be what is shown elsewhere in the report). [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text and figures for Section 2.3.1.1 have been restructured
23974	69	13	69	15	Spaces required between numbers and units [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
29544	69	27	69	27	I suggest to not only mention global ocean, but also link to regionally varying patterns, probably explained in more detail in chapter 10? [Katja Matthes, Germany]	Noted. This is the domain of other chapters
38002	69	29	69	29	Virtually certain: Inconsistent with Chap 7 (but consistent with chap 9). Chap7 says very high confidence. Coordination needed. [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
32934	69	29	69	34	Need to check that numbers and confidence assessments are agreed upon with Ch9 (Kopp/Slangen), to ensure cross-report consistency [Aimee Slangen, Netherlands]	Noted. Text and figures for Section 2.3.1.1 have been restructured
38004	69	30	69	31	Contribution to warming or OHC change? I guess OHC, but your sentence before is about warming. Also, over which period, I don't we have high confidence in this layer splitup from 1970s onward. So I guess the period is different? [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
23976	69	30	69	31	Spaces required between numbers and units [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
38006	69	32	69	34	This sentence is misplaced. We didn't gain confidence in the "last deglaciation [...]" assessment. [Jean baptiste SALLEE, France]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46642	69	37	69	37	Suggest reference 9.2.3.3 on stratification [WGI TSU, France]	Noted.
38008	69	41	69	43	This sentence could be removed. If you want to keep it, I suggest rewording: " the regional high latitude ocean *surface* freshening" [...] "combined with *surface-intensified global* ocean warming, has lead ..." [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.3.2 have been restructured for SOD.
42416	69	45	69	47	Although this part chapter of the report is meant to provide limited information, with expanded explanations in different chapters, I still think it would be beneficial for readers to have a sentence showing how changing salinities in ocean water will affect climate change. Here, you could add something like, "Global-scale analyses show the "fresh get fresher and salty getting saltier" ... thus, affecting ocean water density and consequently changing seawater flow". This will provide just a bit of context that will aid user readability and understanding. [Elizabeth Fard, United States of America]	Noted. Thank you for suggestion.
38010	69	52	69	52	Should be rephrased: "An assessment over a particularly long period of time has reported coherent changes in the Atlantic Ocean across" [Jean baptiste SALLEE, France]	Noted. Thank you for suggestion.
23978	69	55	69	55	Insert) at end of line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
31918	70	2	70	6	In the paragraph says that the multidecadal changes in salinity are similar to climatological distribution find with Argos, and that they are shown in figure 2.31 (a,b), but in those figures the climatological distribution is not clear. [Leonor Vera, Ecuador]	Rejected. Climatological distribution in Figure 2.31 is represented by black lines whereas colors indicated salinity changes, as explained in figure caption.
38012	70	8	70	17	I think those regional, WM description, should be removed from chapter 2. It just does not flow with the rest of the chapter. If you prefer keeping, you should probably point toward Chap 9 for more detailed (and assessed) WM changes. [Jean baptiste SALLEE, France]	Noted. Text and figures for Section 2.3.3.2 have been restructured
31920	70	13	70	17	I suggest to include other figure where it was showed how has been the evolution of freshening and salting surface and depth ocean in time. [Leonor Vera, Ecuador]	Noted. Thank you for suggestion.
50302	70	19	70	19	"summarizing" [Sophie SZOPA, France]	Editorial
23980	70	30	70	30	change 'amd(d)' to 'and (d)' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
6469	70	41	70	41	Discuss sea level changes during a wider time period to give context. Extend back to the last glacial maximum. [Hugh Lefcort, United States of America]	Accepted. Discussion extended.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31922	70	43	70	45	In the sentence: "(with very high confidence in SROCC)". The high confidence is an statistical value? How much confidence? Or there is major confidence in SROCC results than in AR5 ones?. And in the phrase : "AR5 concluded with high confidence", which kind of confidence. It is not clear. [Leonor Vera, Ecuador]	Rejected. Confidence language follows IPCC guidelines.
23982	70	49	70	49	Capital C for century (for consistency elsewhere in Chapter [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)])	Editorial
50304	70	50	70	50	"but also revealing", here the causal link between previous overestimation of GMSL rise and larger acceleration is not obvious, maybe consider to separate in 2 sentences. [Sophie SZOPA, France]	Noted. Thank you for suggestion.
26130	70	51	70	56	Exaggeration of acceleration Compare with the paper " Wahl, T., Haigh, I., Albrecht, F., Dillingh, D., Jensen, J., Nicholls, R., Weisse, R., Woodworth, P.L., Wöppelmann, G. (2013): Observed mean sea level changes around the North Sea coastline from 1800 to present, Earth Science Reviews, 124, 51–67, which states " In general these results are consistent with those presented by Woodworth et al. (2009a), who estimated the geocentric component of sea level rise to be 1.4 ± 0.2 mm/yr for the 20th century from a few long UK tide gauge records. Periods of sea level rise acceleration (i.e. at the end of the 19th century and in the 1970's) and deceleration (i.e. in the 1950's/1960's) were evident in the tide gauge time series. The recent rates of sea level rise are considerably higher, with the highest rates at the end of the 20th century, but are still of the same order of magnitude to those which have been observed at earlier times in the 19th and 20th century. " Are you sure you are not guilty of exaggeration. The slope from 1940 to 1970 looks the same as the slope from 1980 to 2010 to me! [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Assessment in line with SROCC Chapter 4, including updated literature on acceleration and global mean sea level estimates.
31924	71	7	71	7	Indicate to wich figure is rerering: top o bottom [Leonor Vera, Ecuador]	Editorial.
35704	71	7	71	10	As written this text says that the rate of GMSL rise consistently increased over the past two millenia. This does not agree with Figure 2.32. Probably this is an issue with a mis-placed comma - the comma should be before 'but consistently increased', not after it. [Nathan Gillett, Canada]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8544	71	7	71	12	Is more restrictive than the assessment of either the cited source (Kemp et al 2018: thr 20th century rise "is the fastest centennial- scale rate of rise in the past 3000 years (P>0.999)" or of chapter 9 (9.6.2.1.1) (medium confidence that the 20th century rise was faster than any century since at least 1000 BCE). Reconcile. [Robert Kopp, United States of America]	Rejected. IPCC reports are supposed to assess papers and not just accept results/papers' own assessments.
8546	71	7	71	12	Why no uncertainties on the numbers in this paragraph? Also, converge with chapter 9 (9.6.2.1.1) on specific numbers. [Robert Kopp, United States of America]	Noted. Text revised
52350	71	7	71	12	Very wordy sentence - can this be split into two for readability? [Katherine Glover, United States of America]	Editorial.
27708	71	8	71	8	double parenthesis [Poot Delgado Carlos Antonio, Mexico]	Editorial
23984	71	8	71	8	remove one set of () [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23986	71	9	71	9	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23988	71	10	71	10	Delete negative sign [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
23990	71	10	71	11	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
45226	71	15	71	28	Nice figure! It would be good to ensure consistency of tide-gauge reconstructions with cross-chapter box 9.2. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Collaboration improved.
32936	71	17			fig 2.32 & associated text: please make sure that SL time series are agreed upon with CH9 (Kopp/Slangen) for cross-report consistency [Aimee Slangen, Netherlands]	Noted. Collaboration improved.
23992	71	21	71	21	Change to Pre-Industrial and Early-Industrial Period [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23994	71	24	71	24	Change 'errorbars' to 'error bars' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
23996	71	26	71	26	Change 'is' to 'are' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Although 'data' is plural in this context (so 'are' is correct), in other instances 'is' is used (treating 'data' as a collective singular; see previous lines 24 and 25 for e.g.). However, IPCC style guide shall be followed throughout to ensure consistency.
23998	71	31	71	31	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52352	71	31	71	32	suggested rephrasing for consistency with rest of chapter: "...tide gauge reconstruction (Dangendorf et al., submitted) is available..." [Katherine Glover, United States of America]	Noted.
8548	71	31	71	37	This mischaracterizes Dangendorf et al 2019, which is a tide-gauge based reconstruction. Altimeter products should be tied to an appropriate source, e.g. WCRP 2018 [Robert Kopp, United States of America]	Accepted. Text modified
32938	71	31	71	49	This part leans very much on 1 publication, sounds more like a review than an assessment [Aimee Slangen, Netherlands]	Noted. There is only one new long-term reconstruction available post-AR5 and SROCC.
8550	71	31	72	18	Reconcile with assessment of chapter 9 (9.6.2.1.2) [Robert Kopp, United States of America]	Noted. Collaboration improved.
45228	71	39	71	49	I don't think it appropriate to focus so much discussion here on Dangendorf et al (spelling?). The text here should focus on summarising the literature/knowledge as a whole and stating the present understanding of the rates and uncertainties. I would expect more detailed discussion to appear in Chapter 9. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There is only one new long-term reconstruction available post-AR5. Discussion transferred to Chapter 2. This paragraph was edited accordingly, with more focus on synthetic assessment. The text will have to be shortened.
35706	71	46	71	49	The text notes that the altimeter GMSL rate of increase is outside the rates of increase from tide gauges. But it doesn't say whether it is higher or lower. And given that the text highlights a discrepancy between two datasets, it should also assess which record is more reliable (or if the overall uncertainty in GMSL rise is so large that it encompasses both records). [Nathan Gillett, Canada]	Accepted. Text revised.
52354	71	47	71	48	update the placeholder [Katherine Glover, United States of America]	Noted.
31926	71	49	71	49	Indicate to which figure is referring: top or bottom [Leonor Vera, Ecuador]	Editorial.
32940	71	54			fig 2.33 & associated text: please make sure that SL time series are agreed upon with CH9 (Kopp/Slangen) for cross-report consistency [Aimee Slangen, Netherlands]	Noted. Collaboration improved.
24000	71	55	71	55	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
24002	72	6	72	6	Give rate in exponential form [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
32942	72	12	72	18	Need to check that numbers and confidence assessments are agreed upon with Ch9 (Kopp/Slangen), to ensure cross-report consistency [Aimee Slangen, Netherlands]	Noted. Collaboration improved.
26132	72	12	72	19	The slope from 1940 to 1970 looks much the same as the slope from 1980 to 2010 to me! [Stephen Taylor, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Differences in distance between years along the X-axis in Figure 2.32 (left panel) exist. There are four divisions: 500BP to 1750; 1750-1850; 1850-1970; 1970-2018.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29546	72	13	72	13	I suggest to link/refer to regional sea level change in chapter 10? [Katja Matthes, Germany]	Noted. Relevant cross-chapter links included
24004	72	14	72	14	Capital C for century (for consistency elsewhere in Chapter) x2 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
17932	72	16	72	16	See the same comment to the Chapter 1 about acceleration and deceleration. [Branko Grisogono, Croatia]	Rejected. Unspecific and unsubstantiated comment so unactionable.
49950	72	17			The section concludes with a summary, but the final trend value in the summary does not appear anywhere else in this section: 4.0 (3.7-4.4) mm per year [Owen Cooper, United States of America]	Accepted. Text revised.
12968	72	21	74	28	OOCC is not only AMOC, they should explain global ocean and link to Chapter 9, and also see SROCC chapter 6. Should explain global ocean circulation which include wind and thermohaline circulation [RADEN DWI SUSANTO, United States of America]	Taken into account. This section underwent major revision according to this and other reviewers' comments. The section is focussed on global and large scale changes, and coordination with other chapters (particularly Chapter 9) was established.
45408	72	21	74	30	Considerably more discussion of the observed changes in ocean circulation, MOC, including aspects not discussed here such as upwelling and tropical changes in found in Section 9.2 which should be linked from here. [Baylor Fox-Kemper, United States of America]	Taken into account. The section underwent major revision, and continued cross-chapter exchanges supported this revision.
38014	72	23	72	23	This section is weirdly only about AMOC while the title is "ocean overturning circulation", clearly missing large part of the Earth. Southern Ocean is mentioned in the AR5 paragraph, but not further mentioned. The section should be restructured or the title changed, though I would encourage to keep the title and to change the text so it reflects the title. Note that Chap 3 treats Southern Ocean overturning so it would make sense to have it here as well. [Jean baptiste SALLEE, France]	Taken into account. The section underwent major revision, and continued cross-chapter exchanges supported this revision, as well as the combination of other reviewers' comments (e.g. ID 45408).
31928	72	23	73	48	In this numeral would have include that happens in the Pacific with overturning currents. Por consultation: Wang, D. & Cane, M. 2011. Pacific Shallow Meridional Overturning Circulation in a Warming Climate; Burls, N., Fedorov, A., Sigman, D., Jaccard, S., Tiedemann R., and Haug, G. 2017. Active Pacific meridional overturning circulation (PMOC) during the warm Pliocene; Su, B., Jiang, D., Zhang, R., Sepulchre, P., and Ramstein, G.: Difference between the North Atlantic and Pacific meridional overturning circulation in response to the uplift of the Tibetan Plateau, Clim. Past, 14, 751-762, https://doi.org/10.5194/cp-14-751-2018 [Leonor Vera, Ecuador]	Taken into account / rejected partly. The assessment for this part underwent major revision according to this and other reviewers' comments, and in coordination with other chapters. However, some suggested references deal with modelling of future changes, which are out of scope of chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19186	72	23	73	48	this section may also want to consider the growing evidence for AMOC weakening at the MPT, as evidenced by data from Pena and Goldstein 2014, Farmer et al. 2019 and Yehudai et al. subm. [Baerbel Hoenisch, United States of America]	Taken into account. According to this and other reviewers' comments, the assessment for this part underwent major revision, in coordination with other chapters.
49430	72	26	72	26	"entrainment of these overflow waters" should be changed to "entrainment of overlying water into these overflows". It is the entrainment of N. Atl. Water by the overflows that contributes to the AMOC. [Sonya Legg, United States of America]	Accepted. The wording is revised in combination with other comments.
26452	72	34	72	34	The use of word "driven" requires care as discussed in Appendix C (P.436) of Wunsch (2015) textbook ISBN 978-0-691-15882-2. Two everyday usages of the word are "controller" and "power source". It is not yet established that dense water production controls the global scale ocean circulation (possibly, e.g. doi:10.1016/j.ocemod.2005.04.001). It is still arguable if the polar cooling is the one of the main sources of the global circulation (e.g. doi:10.1175/2009JPO4162.1). Contribution from wind is crucial both in controlling and providing power to the circulation. Better to say "Antarctic-origin" [Katsuro Katsumata, Japan]	Accepted and taken into account. The wording is revised in combination with other comments.
26454	72	35	72	35	If this "likely than not" is used in the AR-specific technical sense, please italicize. [Katsuro Katsumata, Japan]	Accepted.
26456	72	35	72	36	I am not aware of any publication that demonstrates the decline of AABW export from the Southern Ocean *in the Indian/Pacific sectors* with high confidence. Maybe https://doi.org/10.1029/2008JC004778 and/or https://doi.org/10.1175/JPO-D-12-0209.1 , but both with large uncertainties. https://doi.org/10.1016/j.pocan.2016.06.005 shows a different conclusion. I agree with the volume reduction of AABW but it seems not yet clear how this is related to *production* or *export* of AABW. [Katsuro Katsumata, Japan]	Accepted and taken into account. The wording is revised in combination with other comments, and this section has undergone major revision. For the SO lower cell more details are given also in chapter 9.
29116	72	40	72	52	This text of paragraph could do with being tightened up. The language seems a bit too sensationalist (e.g. dramatic, remarkable). Shorter sentences would help. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text rewritten

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51688	72	40	73	12	It is maybe worth mentioning that (emerging) evidence both from the North Atlantic (Galaasen et al., 2014 (Science)) and the Southern Ocean (Hayes et al., 2014 (Science)) supports transient stallings of deep overturning as a result of freshwater forcing during peak interglacial conditions of MISS. These studies suggest that under conditions that were warmer than pre-industrial, transient meltwater perturbations were substantial enough to affect the global thermohaline circulation [Samuel Jaccard, Switzerland]	Taken into account. Revised
52356	72	41	72	41	spell out "W.D.P." in the citation [Katherine Glover, United States of America]	Accepted.
19184	72	41	72	41	what does W. D. P., 2015 stand for? This is not included in the reference list [Baerbel Hoenisch, United States of America]	Accepted. See also comment ID 52356.
33102	72	44	72	44	While there is broad agreement of a shallower LGM AMOC, there are still conflicting interpretations of the strength. Some proxies do suggest a weakening (oxygen isotopes-- Lynch-Stieglitz et al., 1999 and Lynch-Stieglitz et al 2006) whereas others have been interpreted in terms of a strong but shallow AMOC (Pa/Th - Bradtmiller et al., 2014, Gherardi et al, 2009, Lippold et al., 2012). Modelling studies have found the water mass tracers to be consistent with both stronger (Kurahashi-Nakamura et al., 2017) and weaker (Menviel et al., 2017; Muglia et al., 2018) overturning cell. [Jean Lynch-Stieglitz, United States of America]	Taken into account. This paragraph went through major revision, taking reviewers comments into account, as well as assessments from previous IPCC assessments - particularly the AR6 special reports.
29118	72	44	72	44	There is a value of the AMOC for the LGM given in the paleo annex. Should it not be referred to here? [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
29120	72	46	72	48	I do not feel that Hoffman et al (2018) supports this statement. This uses a single core at a low resolution. It therefore cannot detect centennial variability in AMOC during the Holocene. The existence of this kind of variability is indicated by other work such as Oppo et al (2003, Nature, "Deepwater variability in the Holocene epoch") [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. According to this and other reviewers' comments, the assessment for this part has major revision, in coordination with other chapters.
24006	72	47	72	47	Insert space between number and unit (8 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18198	72	48	72	50	Lund et al. [2006]'s reconstruction only allows assessments about the surface AMOC and the Gulf Stream. A more accurate statement would be: "Geostrophic calculations based on temperature and salinity reconstructions in the Florida Strait suggest a decrease in the Gulf Stream of about 10% during the LIA" [Gwenaëlle GREMIION, Canada]	Taken into account. According to this and other reviewers' comments, the assessment for this part has major revision, in coordination with other chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29122	72	50	72	52	I recommend splitting this sentence and it places together two separate facts at the moment. It implies that the decline since the LIA is associated with similar mechanisms to the initiation in the early Holocene. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, and the sentence has been revised, also merged with other comments.
18200	72	54	72	54	Consider replacing the first word of the sentence from "proxy" to "indicator", or something similar. The paragraph beginning on line 54 explains that SST is used as a proxy for AMOC strength, which is an undisputed correct use of the word proxy. However, this sentence immediately follows the paragraph that focuses on AMOC changes during the LGM and Holocene, which is obtained using paleoclimate proxies, such as geochemical measurements on fossilized organisms or marine sediment. Although these are both proxy measurements, it initially raises some confusion using the same term for measurements of modern processes as well as paleo-processes. [Gwenaelle GREMION, Canada]	Accepted, the sentence has been changed accordingly.
49432	73	9	73	12	See recent publication which indicates that N.W Atl sealevel is related to AMOC via wind-stress changes, not through the buoyancy-driven geostrophic component of AMOC. Christopher G. Piecuch et al, How is New England Coastal Sea Level Related to the Atlantic Meridional Overturning Circulation at 26° N?, Geophysical Research Letters (2019). DOI: 10.1029/2019GL083073 [Sonya Legg, United States of America]	Taken into account. We have revised the text, and referenced the proposed paper, thank you. Piecuch et al. largely extends McCarthy et al. (2015) results referenced in the paper.
27710	73	23	73	23	check bibliographic citation [Poot Delgado Carlos Antonio, Mexico]	Accepted and taken into account, thank you.
48476	73	23	73	23	"methodological difference due to the unconstrained barotropic circulation variability at 16N" (Frajka-Williams et al., 2018) [Eleanor Frajka-Williams, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and taken into account.
18202	73	30	73	32	For consistency, consider reversing the order of these source waters of AMOC so that they are presented in the same order as in lines 25-27 of p 72. [Gwenaelle GREMION, Canada]	Accepted and taken into account, thank you.
27006	73	41	73	41	The word 'rapid' when associated with the AMOC has connotations of large, fast collapse etc. Also rapid with respect to what? I suggest the word is removed [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and taken into account, thank you.
27008	73	41	73	48	This section is a nice review of past AMOC changes. These are also discussed in chapters 3 and 9 so please coordinate to make sure there are no contradictions and reduce overlap [Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. According to this comment, and other comments, continued cross-chapter exchange has supported the text revision.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38016	73	41	73	48	These assessments are inconsistent with Chap 9. Please liase with Chap 9. [Jean baptiste SALLEE, France]	Taken into account, and in coherence with other comments, text has been revised in exchange with Chapter 9.
24008	73	44	73	45	Insert full stop after 'AMOC' and start new sentence with 'However' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
38018	73	51	73	51	I think this section should be entirely reconsidered (and coordinated with chap9). I find it difficult to understand that you describe individual current system as local as "East Australian Current", or current "near new Zealand", etc. in the context of large-scale climate indicator. I would have thought this section would be about global consistent gyres changes or consistent global WBC change, without entering in the describtion of individual current. Gyres are not mentioned here, which I find surprising. [Jean baptiste SALLEE, France]	Taken into account. According to this comment, and other comments, this section has been substantially revised.
18204	73	51	74	28	This discussion does not contain any direct mention to the North Atlantic subpolar gyre, which might be of interest to understand or because it is directly impacted by the changes in convection in the subpolar North Atlantic and in the ocean heat content described above in this section. Paleoceanographic evidence suggests that the subpolar gyre could have shifted from weak and strong phases over the Holocene [Thornalley et al., 2009; Nature], with weak phases during the 4.2 ka event [Jalali et al., 2019; Climate of the Past] and the Little Ice Age [Miettinen et al., 2012, Journal of Climate; Copard et al., 2012, Earth and Planetary Science Letters]. Estimates based on sea-surface height show some multidecadal variability [Hátún et al., 2016, Progress in Oceanography] or a weakening of the gyre circulation [Häkkinen and Rhines, 2004, Science; Häkkinen et al., 2013, J. Geophys. Res. Oceans]. Moreover, model simulations have connected the reconstructed ice mltting and SST warming in the Arctic [Kaufman et al., 2009, Science] with a strengthening of the subpolar gyre in the 20th century [Jungclaus et al., 2014, Climate of the Past] I'm sure this short review is not as comprehensive as it might be, but I hope it helps highlight the relevance of the gyre in this assessment. [Gwenaelle GREMION, Canada]	Taken into account. We have revised the text, and referenced the proposed papers, thank you. However, the whole section is about wind-driven circulation. In this respect responses of circumpolar gyre in the NA to wind stress curl and resulting changes (as presented in some of the references provided) might be useful.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38020	74	1	74	2	About Southward ACC shift: Chap 9 revises that assessment and says that there is no shift. Please do not mention this AR5 conclusion without the revised assessment or it will make things very confusing. General comment: coordinate with chap 9. (Note that SROCC also revised AR5 on that point, consistent with chap 9) [Jean baptiste SALLEE, France]	Accepted, thank you very much. We have now aligned the statements with Chapter 9 assessments, and the special reports, and text is revised accordingly.
24010	74	5	74	5	Capitalise Southern Hemisphere [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
48428	74	5	74	6	Section 2.4.5.2 does not present evidence for or mention the strength of the westerlies, rephrase [Julie Arblaster, Australia]	Not applicable. 'Strength of the westerlies' discussion removed in SOD text,
7852	74	5	74	19	the references not included in the references list [zhiyan zuo, China]	Accepted, and the references are now added in the list. Thank you.
35708	74	12	74	15	AR5 assessed (ES of Ch 3) that there is no evidence for trends in the transport of the ACC, but medium confidence that the ACC shifted south between 1950 and 2010 at a rate equivalent to about 1 degree of latitude in 40 years. This text here only assess the mean strength of the ACC, but does not assess trends in strength or position. Does the AR5 assessment on this still stand? Or is there now lower confidence in any trends in ACC latitude, as suggested by the over assessment that there is low confidence in changes in wind-driven circulation? [Nathan Gillett, Canada]	Taken into account. Thank you very much for this comment. The assessment of the ACC is in-line with SROCC, and this is given in more detail in chapter 9. We have however included main ACC assessment outcomes.
38022	74	12	74	15	Is that really chap 2 content? Chap 2 content is maybe more along the line (if you want to talk about ACC) "in contrast to SH winds, ACC has not strengthened", you can even add period and confidence level to that by liasing with Chap 9 (so we can update you in case of change): from 1990 (medium confidence) [Jean baptiste SALLEE, France]	Taken into account. The section underwent major revision and alignment with other chapters, particularly Chapter 9. See also reply to comment ID 35708
24012	74	20	74	21	Edit referce to Hsin (2015) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
35710	74	25	74	26	The meaning of the this sentence is unclear. I suggest something like 'While the atmospheric extratropical jets have likely shifted poleward since the 1980s, no consistent response of the wind-driven ocean circulation has been observed' or similar. [Nathan Gillett, Canada]	Accepted/noted. This part of the text in CH2 has been considerably revised.
50306	74	25	74	28	Isn't it rather low to medium confidence in the fact that there is no uniform changes in wind pattern? [Sophie SZOPA, France]	Taken into account. The section is considerably revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49434	74	27	74	28	The evidence in support of this statement "There is some evidence that increased wind forcing is leading to increased eddying of WBCs" is thin. The previous paragraph mentions that the gulf stream has become more eddying, but the connection to increased wind-forcing is not made. [Sonya Legg, United States of America]	Taken into account. This subsection - including the assessment of WBCs - has been considerably revised. Note that chapter 2 is focussed on observed changes only, and attribution / mechanisms are assessed on other chapters (e.g.; chapter 9)
32202	74	31	82	26	I welcome the changes made to the section on the Biosphere, which now focus much more than terrestrial vegetation and related issues (growing season, area extent). [Isabel Trigo, Portugal]	Noted with thanks.
15354	74	33	75	2	It is necessary to add information about changes in photosynthesis in terrestrial ecosystems and the ocean. For example: Crous, K. Y., Drake, J. E., Aspinwall, M. J., Sharwood, R. E., Tjoelker, M. G., & Ghannoum, O. (2018). Photosynthetic capacity and leaf nitrogen decline along a controlled climate gradient in provenances of two widely distributed Eucalyptus species. <i>Global change biology</i> , 24(10), 4626-4644. Guan, K., Pan, M., Li, H., Wolf, A., Wu, J., Medvigy, D., ... & Liang, M. (2015). Photosynthetic seasonality of global tropical forests constrained by hydroclimate. <i>Nature Geoscience</i> , 8(4), 284. Haworth, M., Belcher, C. M., Killi, D., Dewhirst, R. A., Materassi, A., Raschi, A., & Centritto, M. (2018). Impaired photosynthesis and increased leaf construction costs may induce floral stress during episodes of global warming over macroevolutionary timescales. <i>Scientific reports</i> , 8(1), 6206. Hutchins, D. A., & Fu, F. (2017). Microorganisms and ocean global change. <i>Nature microbiology</i> , 2(6), 17058. Drake, J. E., Tjoelker, M. G., Aspinwall, M. J., Reich, P. B., Barton, C. V., Medlyn, B. E., & Duursma, R. A. (2016). Does physiological acclimation to climate warming stabilize the ratio of canopy respiration to photosynthesis?. <i>New Phytologist</i> , 211(3), 850-863. [Oksana Lipka, Russian Federation]	Rejected. Key references are already included.
24576	74	33			Section 2.3.4.1: The observed seasonal cycle of CO ₂ should be moved to go with the CO ₂ observations in section 2.2.4.2, and then this section can refer back to it. [William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - section 2.2 focuses on changes in historical drivers whereas section 2.3 focuses on biospheric indicators; changes in the seasonal cycle of CO ₂ are considered the latter in this assessment.
32204	74	35			Replace "SRCCL (2019)" by "IPCC (2019)" to be consistent with references. Or alternatively change the reference to this report in the reference list. [Isabel Trigo, Portugal]	Noted. The reference was amended accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18206	74	37	74	38	"[...] in the biosphere; [...] imply changes in terrestrial ecosystem exchanges" approximately 1/4 of the anthropogenic CO2 is absorbed by the ocean via the physical and biological carbon pumps. So the marine biosphere has also a significant impact on the seasonal cycle of CO2. [Gwenaelle GREMION, Canada]	Taken into account - text revised (discussion now indicates the terrestrial biosphere is the primary driver and the marine biosphere has a smaller effect).
27356	74	37			it is unclear whether biosphere here refers to ocean and land biosphere or land only. [Sönke Zaehle, Germany]	Taken into account - text revised (discussion now indicates the terrestrial biosphere is the primary driver and the marine biosphere has a smaller effect).
18208	74	40	74	53	Same thing as above, that paragraph is really focused on the impact of change in terrestrial biosphere on climate change, but what about the ocean biological activity. Is it changing the same way as the terrestrial biosphere? We know that in some regions it decreases and in others it increases; but how the global marine biosphere activity is changing? and what is the contribution of the resulting trend to climate change? [Gwenaelle GREMION, Canada]	Taken into account - text revised (Second Order Draft includes a discussion of ocean colour; in addition, other aspects of the marine biosphere, such as ocean pH, deoxygenation, and phenology, are already discussed in subsequent sections in the chapter).
37514	74	43			"airborne data" could be reworded. The data are hopefully no longer airborne but stored away in an archive somewhere on land. I assume the writer means "data from aircraft". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
27354	74	51	74	53	This statement requires a reference [Sönke Zaehle, Germany]	Taken into account - text revised (sentence deleted).
51808	75	15	76	27	I believe there's been some major developments in using Born isotopes and B/Ca in corals to reconstruct past pH variability, see e.g.: McCulloch et al. (2018) https://doi.org/10.1007/978-3-319-64666-4_6 ; Chen et al. 2019 Paleo3: https://doi.org/10.1016/j.palaeo.2019.04.033 [Anson Cheung, United States of America]	This comment has been deferred to the FGD
57230	75	15	76	50	This section seems to belong in the ocean state section 2.3.3. It feels a bit odd to have this summary within the biosphere, given most of the drivers of pH and O2 changes are physical in nature (carbon intrusion and weakened ventilation and warming). [Yassir Eddebbbar, United States of America]	Taken into account. We have reorganized the information, and linked/referenced
19188	75	28	75	29	please also include Penman et al. 2014 as the first assessment of PETM rate of acidification [Baerbel Hoenisch, United States of America]	Taken into account. Reference has been added.
43732	75	29	75	31	I would rewrite the sentence as '...development of the benthic foraminifera d13C gradient method....' [Carles Pelejero, Spain]	Taken into account, that section of the chapter has been modified extensively.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33458	75	33	75	34	"Acidic" waters are considered those with pH of < 7; except for some extreme environments, the oceans are alkaline. I recommend this be worded as "when surface waters have transiently increased in acidity" as called for by Gattuso et al. here: https://news-oceanacidification-icc.org/2015/08/26/aplea-to-ocean-acidification-scientists/ [Adrienne Sutton, United States of America]	Accepted, thank you, and text revised accordingly.
31656	75	33	75	51	In the first paragraph of this block, the authors introduce PETM as a best-available natural analogue of present ocean acidification. In the next paragraph, however, they describe ocean deoxygenation event in PETM without any introduction. Moreover, they attribute the observed deoxygenation to warming and ocean stratification without, again, any introduction. The authors may do not feel strangeness about this because they already know all three events (i.e., warming, deoxygenation and acidification) had occurred simultaneously in PETM, but most of the readers do not know that. The authors should introduce in the beginning of this block that PETM is the precious geological period when warming, deoxygenation and acidification had simultaneously recorded with sufficient chronological control. [Tsuneo Ono, Japan]	Thank you for the suggestion. Text modified for clarity. Furthermore, coordination with Glossary for consistency.
24014	75	40	75	41	Don't split number and units across a line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
6471	75	53	75	53	Expand on why "Surface ocean pH has gradually declined over the last 50 million years (Anagnostou et al., 2016)". Also add something about warmer water holding less CO2. Will this result in even higher atm levels? [Hugh Lefcort, United States of America]	Taken into account. According to this and other comments the text has been revised, taking the word limit constraint into account.
19190	75	54	76	1	since this statement refers to the last million years, Martinez-Boti et al. 2015 should be removed rom this reference list, Seki et al. 2011 provide only 3 data points for this interval, whereas many more are included in Hönisch et al. 2009 [Baerbel Hoenisch, United States of America]	Accepted. The suggested references are considered in the revisions.
43740	76	1	76	3	It would be good to cite Sosdian et al., 2018 at the end of this sentence, together with the call to Fig. 2.35 [Carles Pelejero, Spain]	Accepted. Thank you, reference taken into account in the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57232	76	1	76	50	The O2 summary is very informative but feels unbalanced towards descriptions of earlier eras. This section merits further description of the recently observed O2 trends and their drivers, especially as they relate to the expansion of the tropical oxygen minimum zones. Perhaps, most importantly is the need to add a figure showcasing O2 losses in space, as well as timeseries of global O2 loss in the upper ocean. Figure 1 of Schmidtko et al. 2017, figure 1 of Levin 2018, and/or figure 1 of Ito et al 2017 provide very clear demonstration of these trends. Nearly every variables in this chapter received a visual figure, so it only seems adequate to present the O2 changes as well, especially given their implications for marine life and global fisheries. [Yassir Eddebbar, United States of America]	Taken into account. This paragraph has been revised according this and other comments, and better aligned with assessments from the AR6 special reports. Note, however, that we have to respect the page limitation.
57234	76	1	76	50	The drivers of the reported O2 changes in the tropics as they relate to the OMZ expansion are still not well understood. This section should at minimum describe the role of natural variability in masking or amplifying the O2 signal expected from anthropogenic warming, with proper reference to recent advances in this region, including those associated with the PDO (Deutsch et al 2011; 2014, Duteil et al 2014, 2018),and ENSO (Ito and Deutsch 2013, Eddebbar 2017, Leung et al 2019). This section could also briefly describe that attribution of the anthropogenic trends in the presence of internal variability may not be achieved until 2030-2050 for major regions of the world's oceans (Rodgers et al 2015; Long et al 2016; Henson et al 2017), and that models are able to account for about 50% of the observed changes (Schmidtko et al 2017; Oschlies et al 2018). [Yassir Eddebbar, United States of America]	Taken into account. This paragraph has been revised according this and other comments, and better aligned with assessments from the AR6 special reports.
57924	76	2	76	2	Replace Pliocene with Piacenzian in Table 1 [Bas de Boer, Netherlands]	rejected Pliocene is the term used and defined in the new cross-chapter box 2.1
19192	76	2	76	2	please include Hönisch et al. 2009 along with Chalk et al. 2017 [Baerbel Hoenisch, United States of America]	Noted. Additional reference considered in revision.
52358	76	3	76	3	remove comma after "Cenozoic" [Katherine Glover, United States of America]	Accepted, sentence has been modified.
52360	76	7	76	7	parentheses for values and citation need clean-up [Katherine Glover, United States of America]	Editorial.
52362	76	9	76	9	add comma after "pCO2 rise" [Katherine Glover, United States of America]	Editorial. Sentence has been modified.
24016	76	12	76	12	Subscript for 2 in 'CO2' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19194	76	12	76	12	Martinez-Boti et al. 2015 do not include any Pleistocene data. If this study refers to Martinez-Boti et al. 2015 (Pleistocene carbon leakage), then that is not the correct reference. However, the leakage paper does not look at whole ocean changes but only regional changes. I do not think this is appropriate in the given context [Baerbel Hoenisch, United States of America]	Taken into account. The chapter is being revised to emphasise large-scale observations. This comment is being taken into account in this process. Attribution of Martinez-Boti et al. is revised.
18210	76	20	76	22	"Present-day rates of ocean acidification, [...] are now much higher" How much higher? In the next paragraph about deoxygenation you quantified that change. That would be great to do it here too, or at least give a range. [Gwenaelle GREMION, Canada]	Taken into account. Now noted that the rates are variable across ocean basins.
43730	76	20	76	27	None of the papers listed in this paragraph is listed in the reference list [Carles Pelejero, Spain]	Editorial.
24018	76	21	76	21	Change 'timeseries' to 'time series' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24020	76	30	76	30	Delete negative sign/hyphen, it is confusing [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
43742	76	30	76	30	(Schmidtke et al., 2017; Levin 2018) [Carles Pelejero, Spain]	Noted. Literature cited was reviewed.
52364	76	30	76	32	clean up parenthesis and citations, also Levin (2018) is cited but full reference does not occur in bibliography [Katherine Glover, United States of America]	Editorial.
31658	76	30	76	34	In line 32-33, the authors say that oxygen decrease is strongly tied with multidecadal increase of heat content. However, in the next sentence, they say that solubility-induced decline of oxygen explains only 15 % of observed oxygen decrease. I think most of the readers will be confused by this apparently-contradicted description. This is because most readers don't easily notice that increase of heat content is not the cause of deoxygenation but both heat content increase and deoxygenation are the results of same process. Chapter3 (p.49 127-35) skillfully succeed to explain this complicated situation. I hope the authors to refer description in Chapter3 and improve this paragraph to avoid reader's confusion. [Tsuneo Ono, Japan]	Taken into account, text revised.
24022	76	32	76	32	Insert second) after '2017' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
43744	76	32	76	32	delete comma after mol [Carles Pelejero, Spain]	Editorial.
24024	76	33	76	33	Insert 'the' after 'after' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
52366	76	34	76	37	wordy, can this be rephrased to be less passive? [Katherine Glover, United States of America]	Taken into account, thank you.
43746	76	36	76	36	; after (Breitburg et al., 2018) [Carles Pelejero, Spain]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37516	76	39	76	40	I think "throughout the water column and across the ocean basins" would be better than "across the water column and throughout the ocean basins". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you.
24026	76	47	76	47	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
37518	76	47			I think a word such as "experiencing" is needed before "declining". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
43748	76	48	76	48	better rephrase to '... commensurate expansion of the oxygen minimum zones' [Carles Pelejero, Spain]	Accepted, thank you.
24028	76	49	76	49	No capital O for 'Ocean' x 2 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
8196	77	14	77	55	Would it not be good to broaden this section to something similar to 2.3.4.4 and call it marine ecosystem changes (ocean phenology is also strange wording, it is not the phenology of the oceans, but of organisms living in the oceans). The Poloczanska dataset (updated here) documents much wider changes than phenology alone: range shifts, changes in abundance, demography, community/biodiversity and calcification that are worth mentioning. Undoubtedly, this reflects my personal preferences, but I think it is important to highlight studies that show that marine ecosystems are now different from before marked human influence, e.g: Moy, A. D., Howard, W. R., Bray, S. G. & Trull, T. W. Reduced calcification in modern Southern Ocean planktonic foraminifera. Nature Geoscience 2, 276, doi:10.1038/ngeo460 (2009); Jonkers, L., Hillebrand, H. & Kucera, M. Global change drives modern plankton communities away from the pre-industrial state. Nature, doi:10.1038/s41586-019-1230-3 (2019). The terrestrial biosphere seems to follow take that long-term perspective.. [Lukas Jonkers, Germany]	Taken into account. Section has been modified, taking into account the word limit constraint. References suggested were considered, but note that we are not dealing with attribution issues in Chapter 2.
13172	77	14	77	55	Have earlier life cycle events lead to negative impacts on these species and their survivability? Are certain groups declining because of this? What about the timing of phytoplankton blooms, are these changing and are these having impacts on other species? [Nora Richter, United States of America]	Noted. Text dealing with phenology of marine organisms has been revised extensively.
52372	77	14	77	55	The "Ocean Phenology" section frames the findings in context of a null hypothesis of no net change due to climate. Should this hypothesis-based framing be done consistently throughout the chapter/report? [Katherine Glover, United States of America]	Rejected. Thank you for the comment, but the assessment relies on specific baseline periods for different parameters according to availability of data. Use of a common framework for all would not be feasible.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15356	77	14	77	55	The shift is not only in the time, it is also in the space. Large areas are temporary or permanently more ice-free. So, the breeding areas has shifted. Please add changes in the sea ice to show new spaces for species distributions and breeding. Haug, T., Bogstad, B., Chierici, M., Gjøsæter, H., Hallfredsson, E. H., Høines, Å. S., ... & Loeng, H. (2017). Future harvest of living resources in the Arctic Ocean north of the Nordic and Barents Seas: a review of possibilities and constraints. Fisheries Research, 188, 38-57. [Oksana Lipka, Russian Federation]	Taken into account. The reference is added, and the text is modified.
50308	77	14	80	3	These sections should rely on the last IPBES report and the details should probably appear in WG2 report only [Sophie SZOPA, France]	this comment has been deferred to the FGD
45770	77	14			suggest focusing this on satellite observations of ocean colour and land surface greening only, with reference to AR5 and AR6 Special reports for other evidence. The assessment of ecological responses will be in WGII [Katja Mintenbeck, Germany]	Noted. Section has been revised extensively to include ocean colour.
52368	77	16	77	19	Wordy - makes little sense, esp. sentence opener at lines 16-17 [Katherine Glover, United States of America]	Taken into account, text revised.
31660	77	16	77	19	Here the authors referred the number of seasonal advance (4.4 days/decade) estimated by Poloczanska et al. (2013), but this number actually depends on the observed number of no phenological change (yellow circles of Figure1 in Poloczanska et al., 2013). In general, scientist tend to publish no report when their results show no change. Poloczanska et al. (2013) mention about this potential publication bias and stated their results are “relatively robust” to this risk because their data include only 11 % of single-species assessment, that are especially fragile to this kind of publication bias. However, multi-species studies are still subject of this bias, as scientist may cancel publication if all observed species do not indicate any phenological changes. The numbers estimated by Poloczanska et al. (2013) and presented here should be treated as “maximum estimate”, as there are little grounds to believe that number of no-change counted by Poloczanska et al. (2013) is good enough. [Tsuneo Ono, Japan]	Taken into account. This section has been revised extensively, and many details have been condensed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57830	77	16		52	In the Atlantic ocean at the Gulf of Guinea, there have been a series of changes in the shift of species, migrating from onr regions per several kilometres due to anthropogenic changes in marine ecosystem. Fisheries and marine mammals are drastically changing their phenology as of the case of thevUS Atlantic coast areas. some black tiger shrimp are changing their phenology rapidly as the location is not conducive for the marine ecosystem. An integrated approach to combat phenology of marine animals must be implemented. Monitoring, response and marine ecosystem adaptation mustvbe stated and implemented. [Abiodun Adegoke, Nigeria]	Noted. This section of the report has been revised extensively.
24030	77	17	77	17	Change 'Spring' to 'spring' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
31662	77	21	77	24	In this sentence the authors mention about percentage of phenological advance among recently observed phenological changes. The authors, however, do not mention about ratio of occurrence/non-occurrence of phenological changes among all observations, although original reference of Poloczanska et al. (2013) provided that figure. If the authors feel that Poloczanska's occurrence/non-occurrence estimation is reliable, that should be described here as this is essential information about climatological phenology shift similarly to advance/prolong ratio. Actually the authors must feel that Poloczanska's estimation is reliable, as this is the implicit basis of the estimated number of seasonal advance presented in this chapter (lines 17, 42, 48, and 49). [Tsuneo Ono, Japan]	Taken into account. The section has undergone major revision.
52370	77	22	77	24	Sentence needs editing - seems like it's missing "and" [Katherine Glover, United States of America]	Accepted, thank you, and text revised .Please note that the entire section has been revised extensively.
37520	77	22			See comment 74. This sentence would read more easily if a word such as "seasonally dependent" were to appear before "lifecycle". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section has undergone major revision.
18212	77	26	77	27	"There is high confidence that fish[...]" Life-cycle? Recruitment? Development? [Gwenaelle GREMION, Canada]	Taken into account. Section has been re-written.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24032	77	26	77	30	This is confusing. Do you mean fish in all oceans, or just some parts of the ocean? The phrase 'fish are occurring earlier' at the best is poor English and to some extent does not make sense, although it is to some degree clarified later on. but I suggest editing to 'that fish are occurring earlier in all parts of the oceans' (if this is indeed the sense of what you are trying to say. I would consider defining anadromous, I am not sure all readers will be familiar with the term (and demersal is defined, which is good), and please give genus/species details for salmon, cod and sole. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, thank you. The whole section is revised extensively.
18214	77	31	77	31	"There is high confidence in the timing[...]" should be earlier timing? [Gwenaelle GREMION, Canada]	Accepted, and taken into account. Thank you. Please note that the entire section has been revised extensively.
24034	77	34	77	35	Change 'seabirds' to 'sea birds' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24036	77	39	77	39	Change 'seabird' to 'sea bird' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24038	77	47	77	47	Change 'seabirds' to 'sea birds' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
35712	77	49	77	52	This formulation is unclear. Is it saying that seabirds and marine mammals are assessed to have a net rate of movement close to zero, but we have low confidence in this? Or is it saying that sea birds and marine mammals probably have a net rate of movement which is quite different from zero? Also, as written the second sentence seems to be an example of why the confidence in the first sentence is low - but of course the first sentence just says that the average change in phenology is close to zero - the fact that polar bears are an exception does not invalidate the first sentence. I suggest something like 'On average seabirds and marine mammals have exhibited close to zero net shift in phenology (low confidence). However, polar bears are delaying....'. [Nathan Gillett, Canada]	Taken into account. This section has been revised extensively.
24040	77	50	77	51	I would not class the polar bear as a marine mammal. Saying 'net rate of movement' is poor, it implies they are moving slowly! I suggest changing this to 'a shift close to zero' (the sense is then clearer from the previous sentence. Give the genus/species for polar bears. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, and text revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37522	77	50	77	52	It can be questioned whether this sentence refers to terrestrial phenology not oceanic phenology. Most polar bears make dens on land, albeit near the sea, although some make dens in snow drifts on sea ice. Is it terrestrial or oceanic conditions that determine when the terrestrial-denning polar bears choose to enter their dens? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference to polar bears no longer appears in the revised text.
8198	78	5	78	6	From the figure caption it is unclear that these responses to climate change only pertain to phenology. Poloczanska et al. synthesised other changes too (including shifts in abundance, distribution, demography, community change and calcification). Please clarify [Lukas Jonkers, Germany]	Taken into account - text revised (caption now specifies the types responses to climate change included in the figure).
53328	78	16	80	3	You may consider involving a WGII authors as CA (if you havent already done so) [Jan Fuglestad, Norway]	Noted.
54632	78	17	82	26	are there not several findings regarding the contents of those section in the Special Report 1.5? [Ruth Cerezo, Mexico]	Rejected - the IPCC Special Report focuses on the impacts of global warming of 1.5 °C rather than observed changes in the biosphere.
24042	78	18	78	18	Change 'geographic' to 'geographical' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
18216	78	21	78	25	The term "turnover" should be defined a little bit more. Are you referring to the rate of apperance and extinction of species within an ecosystem? How is it defined in figure 2.37? Is that only terrestrial plant turnover in figure 2.37? [Gwenaelle GREMION, Canada]	Taken into account - combined with comment 35714.
7476	78	24	78	35	Define ecosystem turnover [Rose Abramoff, France]	Taken into account - combined with comment 35714.
31300	78	27	78	37	These observations about turnover rates are of little use without numbers – "low" and "high" are insufficient. More crucially, when this work is cited it should be made clear what is the maximum possible turnover rate that could be reconstructed using this methodology. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.
31996	78	27	78	37	This section on terrestrial ecosystems since the LGM is rather poor. The content is not very informative. The issue of natural versus anthropogenic changes is not mentioned. The section is missing references. [Marie-France Loutre, Switzerland]	Taken into account - text revised.
45772	78	27	78	46	No attribution of recent patterns to climate change, suggest leave this to WGII [Katja Mintenbeck, Germany]	Taken into account - text revised.
35714	78	27			Explain what 'changes in turnover' means. [Nathan Gillett, Canada]	Taken into account - text revised accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
48226	78	31	78	37	I am surprised by the description of the changes in ecosystem (vegetation) turnover during the Holocene in Europe. Marquer et al. (2017) in QRS shows a decrease of turnover through the Holocene shown by both pollen percentages and pollen-based REVEALS-estimates of plant cover. The only increase of turnover is seen for the last 2 centuries. There are, however, increases in vegetation (taxa compositional) rate-of-change earlier, around 5k (Neolithic) and 2k, but the most spectacular increase in rate-of-change is seen over the 2 last centuries as well. I would advise to include the results of Marquer et al. (2017) here because they are based on changes in both pollen percentages AND pollen-based PLANT COVER. The study of Fiesinger et al. (2018) should stay as an important reference, but it studies the "emergence of novelty" and its relationship to "rate-of-change" in pollen data. This is not the same as "turnover" (as defined by Vellend 2001). The text should be adjusted to avoid misunderstandings. [Marie-Jose Gaillard, Sweden]	Taken into account - text revised.
24044	78	43	78	43	I had to look up 'Neotoma' and found it was a genus of packrat! Further digging revealed a reference to the 'Neotoma Palaeoecology Database' which I assume is what is referred to here. For clarity and less confusion I suggest defining the name in full. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.
24046	78	49	78	49	Delete , after 'invertebrates' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the paper discussed in this sentence considers mammals, birds, fish, invertebrates, and plants.
45774	78	49			suggest focusing this on evidence in AR5 and AR6 Special reports . The assessment of ecological responses will be in WGII [Katja Mintenbeck, Germany]	Rejected - Chapter 2 is charged with considering all observational evidence on the changing state of the climate system.
7478	78	51	78	53	Delete first clause of sentence as it contradicts the last clause. I suggest, "Most local assemblages..." [Rose Abramoff, France]	Taken into account - text revised.
13174	79	4	79	5	Specify how ecosystems are changing and are different from the past 200,000 years. In other words, state that certain species are expanding or are observed that were previously not seem with the same levels of abundance and/or not present in Arctic lake ecosystems. [Nora Richter, United States of America]	Taken into account - text revised (sentence deleted).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15358	79	7	79	20	Please add also information about Russia: Константинов, А. В., & Сергиенко, В. Г. (2016). Влияние изменений климата в голоцене на формирование разнообразия современных лесов и их трансформация к концу XXI века в Европейской России. Лесотехнический журнал, 6(3 (23)). [Oksana Lipka, Russian Federation]	Rejected - suggested reference is not readable (appears as a series of question marks). The author of the comment was contacted but did not reply.
27358	79	7			Unclear what ecosystem turnover means: species turnover, or biogeochemical turnover? [Sönke Zaehle, Germany]	Taken into account - combined with comment 35714.
45636	79	8			This is the single mention of methane in the entire chapter. Is methane really so unimportant, so trivial? To date, it's had roughly half the impact of CO2 - isn't that serious? Are methane impacts not of interest? - e.g. increased fluxes from wetlands and cattle as the tropics warm and expand, or larger fluxes from boreal wetlands? [Euan Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
52374	79	11	79	11	Capitalize "Last Glacial Maximum" or use LGM to stay consistent with section 2.3.4.4 and Table 2.1 [Katherine Glover, United States of America]	Editorial.
41414	79	11	79	13	Review the wording [Lucas Bianchi, Argentina]	Editorial.
46812	79	13	79	13	It ought to be clearer that this is referring to the very northernmost location of the tree-line in the world. If so, however, pockets of woodland seem to have occurred even further north than 62°N during the Last Glacial Maximum in e.g. interior Alaska. [Charpentier Ljungqvist Fredrik, Sweden]	Taken into account - text revised
24048	79	22	79	22	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
30468	79	22	79	49	What could be mentioned here too is that species living in the shade of trees (in forest understoreys) are often thermally buffered from changes in the macroclimate and can show lagged responses. The evidence for this is now increasing: see Bertrand et al. 2011 Nature, De Frenne et al. 2013 PNAS, De Frenne et al. 2015 Nature Plants, Scheffers et al. 2013 Global Change Biology, De Frenne et al. 2019 Nature Ecology & Evolution, ... [Pieter De Frenne, Belgium]	Rejected. Key references are already included.
50310	79	29	79	30	no uncertainties accompanying these rates? [Sophie SZOPA, France]	Accepted - text revised.
24050	79	29	79	30	Change to 'kilometres' or km, and metres or m. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24052	79	30	79	30	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46814	79	40	79	42	This statement is not correct. The northernmost tree-line in the world, in Taimyr region of Siberia, at c. 72.5°N has been more or less been stable for a long time. This woodland area of the Khatanga River valley has been well-known since at least the early 20th century and trees in the region at this high latitude in not something new related to recent warming (as wrongly stated). The reference given is, moreover, not referring any modern tree-line change but to the more northern extension of the tree-line earlier in the Holocene. [Charpentier Ljungqvist Fredrik, Sweden]	Taken into account - text revised (sentence now clarifies location of present-day treeline relative to Mid-Holocene.).
24054	80	2	80	2	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
6473	80	6	80	6	Discuss F.A.C.E. experiments. How much CO2 uptake results from the greening? [Hugh Lefcort, United States of America]	Rejected - the chapter assesses large scale (i.e., global and hemispheric) changes rather than results from field experiments.
15360	80	6	80	26	Please add any analysis of the reasons of the greening. It can be not only productivity rising, but, for example, overgrazing in Arctic. Campeau, A. (2016). Remotely-sensed changes in the primary productivity of migratory caribou calving grounds and summer pasture: the mixed influences of climate change and caribou herbivory. [Oksana Lipka, Russian Federation]	Noted/rejected. Key references are already included.
18220	80	6	81	9	That 2.3.4.5 section is really terrestrial oriented. I understand that it might be more complex to analyse "greening" in the oceanic zone but a paragraph discussing the equivalent of that greening in the ocean will be very interesting. Is the ocean is becoming greener or blueer? With all the satellite products we have now it should be possible to have an idea (at least qualitatively) of the marine photosynthetic activity trend. [Gwenaelle GREMION, Canada]	Taken into account - text revised (an assessment of changes in ocean colour has been added to the Biosphere section of the chapter).
35716	80	6	81	9	This section describes global greening, and section 2.2.7 describes changes in global albedo due to land use and land cover i.e. they describe changes in the greenness and the brightness of the land surface respectively. But there are no cross-references between the sections - these must be closely linked. [Nathan Gillett, Canada]	Taken into account - text revised (cross-references added).
18218	80	12	80	19	The indices NDVI, LAI and FAPAR might need to be a little bit more defined since they are not trivial for people that are not specialists in that field. [Gwenaelle GREMION, Canada]	Rejected - space limitations preclude the addition of background definitions such as these.
24056	80	14	80	14	Change 'earth's' to 'Earth's' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24058	80	16	80	16	likely' should be in italics [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13946	80	24	80	26	"consistent with CO2 fertilization, nitrogen deposition, land-cover change, and temperature increases (Chen et al., 2019a; Zhu et al., 2016), the latter being particularly important at northern high latitudes such as the Loess Plateau (Kang et al., 2016; Keenan and Riley, 2018; Kong et al., 2017; Wang et al., 2018)." [Jun Wen, China]	Accepted - text revised.
15362	80	38	81	9	The changes in vegetation are so different over the world and depend of so many reasons, that the uncertainty will be too large anyway. It is better to speak about changes in albedo here. For example: Williamson, S. N., Barrio, I. C., Hik, D. S., & Gamon, J. A. (2016). Phenology and species determine growing-season albedo increase at the altitudinal limit of shrub growth in the sub-Arctic. <i>Global change biology</i> , 22(11), 3621-3631. Duveiller, G., Hooker, J., & Cescatti, A. (2018). The mark of vegetation change on Earth's surface energy balance. <i>Nature communications</i> , 9(1), 679. Stark, S. C., Breshears, D. D., Garcia, E. S., Law, D. J., Minor, D. M., Saleska, S. R., ... & Borma, L. S. (2016). Toward accounting for ecoclimate teleconnections: intra-and inter-continental consequences of altered energy balance after vegetation change. <i>Landscape ecology</i> , 31(1), 181-194. [Oksana Lipka, Russian Federation]	Rejected - the purpose of the section is to assess changes in vegetation, not albedo.
17934	80	38	81	9	First, it is said that browning increases, etc. Then, the summary says there is more greening, i.e., vegetation. This seems inconsistent. Moreover, how does that inconsistency fits into a Chap. 1 statement [Branko Grisogono, Croatia]	Taken into account - text revised (discussion of greening and browning has been rewritten based on newly published papers that resolve the inconsistencies and strengthen confidence).
7480	80	41	80	43	Suggest, "A number of factors...such as regions that are generally associated with drought and wildfire (REFS). Land use changes...", for flow and because "over smaller areas" suggests that drought and wildfire are large areas and agricultural activity and land abandonment small areas, which is not necessarily true. [Rose Abramoff, France]	Not applicable, this paragraph no longer included in the chapter.
13176	80	47	81	6	Are there any estimates for how diverse the species are that make up this "global greening" and how it compares to prior biodiversity in the ecosystems? [Nora Richter, United States of America]	Rejected - not supported by the peer-reviewed literature.
24060	80	52	80	52	Insert 'is' after 'factors' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
56282	80		81		Need to highlight impacts on dune fringed coasts of greening - this will retain higher dune volumes and heighten resilience to future storms [Derek Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - beyond the mandate of WGI (refers to impacts).
50312	81	6	81	6	What do find these models? [Sophie SZOPA, France]	Rejected - comment is ambiguous.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50314	81	8	81	9	the medium confidence is not obvious considering the previous paragraphs [Sophie SZOPA, France]	Taken into account - combined with comment 17934.
27944	81	12	81	12	After the first alinea, this section sums up different studies from the mid1990s to mid2000s concluding either greening or browning, after this, no conclusions or comment is being made, and at the end of the section medium confidence is given that terrestrial vegetation across the globe has increased since the early 1980s [roderik van de wal, Netherlands]	Taken into account - combined with comment 17934.
18222	81	12	82	26	Same comment as above for the section 2.3.4.6. [Gwenaelle GREMION, Canada]	Rejected - comment is ambiguous.
45776	81	12	82	26	suggest focusing on satellite observations of greening and drawing other evidence from AR5 and AR6 special reports, assessment of eg advances of cherry blossoms, will be in WGII [Katja Mintenbeck, Germany]	Rejected - Chapter 2 is charged with assessing a broad range of observational evidence on the changing state of the climate system.
24062	81	18	81	18	Change 'geographic' to 'geographical' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24064	81	22	81	22	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24066	81	22	81	22	Edit referenc to Donat et al. (2013a) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted/accepted. The reference was amended accordingly.
24068	81	27	81	27	Edit reference to McCabe et al. (2015) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted/accepted. The reference was amended accordingly.
24070	81	29	81	29	Edit reference to Xia et al. (2018) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted/accepted. The reference was amended accordingly.
24072	81	30	81	30	Edit reference to Jung et al. (2015) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted/accepted. The reference was amended accordingly.
24074	81	51	81	51	Change 'centered' to 'centred' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
27086	81	51	81	52	In as case study in Chin State, Myanmar, it was observed that the fallow vegetation increased in areal extent and biomass due to alternative livelihood changes and development of alternative livehood activities. It also contributed to the sentence 51 on page 81. (Ref: https://doi.org/10.1659/MRD-JOURNAL-D-14-00083.1) [Nyein Chan NIL, Myanmar]	Rejected. Key references are already included.
24076	82	1	82	1	Edit reference to Barichivich et al. (2013) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference was amended accordingly.
24078	82	5	82	5	Edit text to '2000s, and Zhao et al. (2015) pointed out' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24080	82	7	82	7	Edit text to Hogda et al. (2013) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42908	82	12	82	21	Be more specific here: can you say the uncertainties are independent? Or semi-independent (if you think the uncertainties in the definition of growing season between in situ and remote retrievals are related?) This is an important theme in this chapter - making statements about likelihood of observed changes given multiple (independent) lines of evidence that show common features. [Michael Evans, United States of America]	Accepted - text revised.
31154	82	16	82	16	"... whereas others use the span between the date of the last spring freeze and the date of the first autumn frost" ... add also or the span between the dates when fixed thresholds on vegetation indexes (e.g. NDVI, EVI, ...) seasonal cycles are met [Edoardo Cremonese, Italy]	Rejected - the sentence discusses in situ analyses whereas NDVI refers to satellite analyses.
50316	82	23	82	26	what about other regions? Maybe conclude on the lack of information [Sophie SZOPA, France]	Taken into account - text revised
24082	82	25	82	25	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
27946	82	29	82	29	I think the take home message of this section is said in the last alinea. The rest of the section is of less importance and can be left away or be shortenend. The figure refered to from this section is good and illustrative, this needs to be kept, it shows the climate change for different time periods nicely. [roderik van de wal, Netherlands]	Taken into account. We believe a summary needs to be given but have tried to shorten.
42910	82	29	83	13	Add AR6 likelihood descriptors to this synthesis. [Michael Evans, United States of America]	Taken into account. This section is to be a synthesis and we have tried to attain a better balance in the SOD while still differentiating it from the prior sections.
37524	82	31	82	32	"land" should be added to "atmosphere, cryosphere, oceans and biopsphere". See earlier comments on soil moisture and runoff. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. See response to comment 37430.
30560	82	39	82	42	this sentence risks to be in contradiction with lines below, also with the summary of the section [Annalisa Cherchi, Italy]	Taken into account. We have edited accordingly.
24084	82	44	82	44	Insert space between number and unit (800 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
7854	82	44	82	44	wrong words [zhiyan zuo, China]	Comment is not sufficiently specific to be actionable.
24086	82	46	82	47	Edit to ' Direct observations are available solely for the last 150 years or so and'. Quantify last few decades [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Edited for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24088	82	49	82	49	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
35718	82	54	83	2	This sentence says 'Some biogeochemical indicators... are in states unseen in... virtually certain millions of years'. Since a quantified likelihood is given here (P>99%) the sentence needs to say to which biogeochemical indicators the statement applies. [Nathan Gillett, Canada]	Taken into account. The new biospheric assessment aided by a new LA has informed a new version of this phrasing.
24090	83	1	83	1	Insert space between number and unit (800 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
35274	83	2	83	4	The sentence starting with "several relatively..." is too hard to read The overall idea is ok, but because of its redaction, it seems hard to follow. [eugenia gayo, Chile]	Taken into account. Edits have been made to clarify.
7222	83	3	83	6	This summary fails to mention trends in hydroclimate [Hillman Aubrey, United States of America]	Rejected. The summary is not intended to call out all the components assessed in the prior segments.
24092	83	4	83	4	Insert space between number and unit (125 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24094	83	6	83	6	Change 'Centuries' to 'centuries' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24096	83	6	83	6	Change to 'Pre-Industrial times' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
52376	83	6	83	6	lowercase for "Centuries" [Katherine Glover, United States of America]	Editorial.
35720	83	6	83	8	I recommend listing components here, rather than saying 'almost all components'. There are some key indicators (e.g. global mean precipitation) for which current rates of changes may not be highly unusual. [Nathan Gillett, Canada]	Taken into account. We wish to avoid a laundry list approach but have tried to be clearer in redrafting. Moreover, the list of components was given in Chapter 2 in several instances before; e.g. Box 2.1
35722	83	10	83	11	The authors have tried to strength the AR4 assessment that warming is unequivocal by broadening focus to indicate that 'directly observed changes in key large-scale atmospheric, oceanic, cryospheric and biospheric indicators of climate' are unequivocal'. But this is actually a much weaker statement, since all it is saying is that changes have been observed in these indicators, without saying what these changes are or whether they are consistent with warming. I sugest re-framing in terms of observed changes which are consistent with warming. Finally, are the authors convinced that observed changes in key large-scale biospheric indicators are unequivocal? [Nathan Gillett, Canada]	Taken into account. The authors spent considerable effort to finesse this statement based upon this feedback as well as feedback to the statement in the ES.
44864	83	13	83	13	Add "." in the end. [Kaoru Kubota, Japan]	Editorial.
53330	83	17	83	19	figure 2.40 will be very useful [Jan Fuglestedt, Norway]	Noted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46664	83	24	93	11	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, but it is unclear whether any suggestions are being made here.
46666	83	24	93	11	It may be appropriate to provide a brief explanation of each MoV for the first time it shows up in this report, rather than repeatedly introduce these terms every time they occur like chapter 3 and 4. For example, Section 3.7.1 introduced NAM once and Section 4.4.3.1 introduced it again.Thus, put introduction of MoVs in Chapter 2 may be better. [WGI TSU, France]	Taken into account. A Technical Annex which contains brief explanations of all modes of variability is now included in the report.
35724	83	24	93	11	This section is the point in the assessment where the modes of variability are first introduced. At present the sections are written assuming the reader is already familiar with each of the modes. I recommend that each section starts with a brief description of the mode, and the index or indices used to define it. The authors could also consider including a table of the principal index definitions used for the modes - I think this was a topic of cross-chapter discussion for the modes tiger team. [Nathan Gillett, Canada]	Accepted/noted. SOD includes Technical Annex on modes of variability, where the reader can find condensed definitions and explanations of all modes. This section in CH2 provided x-ref to the Annex.
30562	83	32	83	32	remove "latter", change "process based" with "process-based" and add "that follow"at the end of the line [Annalisa Cherchi, Italy]	Editorial. Noted.
40454	83	35	84	5	The examples given focus on the Holocene and Pliocene whereas there are numerous examples of ENSO reconstructions for the LGM, which is also part fo the modeling comparison. These citations include Ieduc et al, 2009, Koutavas and Joanides 2012, Sadekov et al., 2013, Ford et al., 2016, and 2017. [Heather Ford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. LGM reconstructions were included in SOD.
38954	83	37			Please add a few sentences to describe what's ENSO. Please do alike for other modes of variability. [Masahide Kimoto, Japan]	Taken into account. This is covered by a separate annex on modes of variability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18224	83	38	83	39	The two sentences that follow each other starts with "It was", the second one can be revised to "AR% concluded that large variability" [Gwenaelle GREMION, Canada]	Accepted. Section reworded.
35726	83	39			Is orbital modulation of ENSO expected over the last 7 kyr? [Nathan Gillett, Canada]	Noted. This is a direct reporting of AR5 assessment findings. (The implication in the comment is that orbital modulation on that timeframe is so unlikely that this finding is not worth reporting, but as AR5 did so, we consider it appropriate to follow their lead in reporting on their findings.)
47432	83	50	83	51	Although there is not a commonly accepted list for the existed ENSO events so far, it will be a helpful try to recommend one from now on. Recently, a standard for objectively identifying the El Nino/La Nina events has been published by Ren et al. (2018) for the reference of community in focusing on the relevant research regarding ENSO events. Ren, Hong-Li, Bo Lu, Jianghua Wan, Ben Tian, and Peiqun Zhang, 2018: Identification standard for ENSO events and its application to climate monitoring and prediction. Journal of Meteorological Research, 32(6), 923–936, doi: 10.1007/s13351-018-8078-6. [Hong-Li Ren, China]	Noted. Making recommendations to operational agencies on ENSO event definitions is outside the brief of IPCC. Should the Ren et al. (2018) methodology or another definition gain widespread acceptance within the operational and scientific community in future, this will be of value to future Assessment Reports.
24098	83	52	83	52	Define TAO [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The whole report text has been edited to be in a more consistent style at the SOD stage.
18226	84	1	84	5	With the changes that has been documented on the version of SST data, a statement need to come clear on which data set is still recommended to be used by IPCC when it comes to SST analysis. [Gwenaelle GREMION, Canada]	Taken into account. Major updates to some data sets have been released since FOD and an assessment has been made of the latest state of foundational datasets before SOD submission.
35728	84	4	84	5	The implication here is that the previous versions of the datasets need to be treated with caution, but the latest versions are reliable. Some assessment of possible remaining biases in updated datasets should be included in the next draft. [Nathan Gillett, Canada]	Taken into account. Major updates to some data sets have been released since FOD and an assessment has been made of the latest state of foundational datasets before SOD submission.
18230	84	13	84	13	Watanabe et al. (2011) is missing in the references list [Gwenaelle GREMION, Canada]	Accepted. References were fully reconciled for SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18236	84	19	84	20	The way this sentence is written it suggests that this is the first paper that looked at ENSO activity in the mid-Holocene. I suggest rewording this sentence, perhaps "Building on previous ENSO work, Cobb et al (2013)..." [Gwenaelle GREMION, Canada]	Rejected. IPCC reports generally assume knowledge of citations which were available at the time of earlier Assessment Reports, unless they provide the primary evidence for assessment findings which are not the subject of more recent publications. They are not intended to be a comprehensive review of the literature over time.
35276	84	19	84	32	Here, evidences for late Pleistocene changes in ENSO activity are ignored. This information is quite relevant. For instance, see Palmer & Pearson (2003) and/or Rein et al (2005). [eugenia gayo, Chile]	Taken into account. Discussion of ENSO at the LGM is included in SOD.
18232	84	21	84	21	Thompson et al., 2017; is not in the reference list. The only once available are Thompson et al., 2017a and b in the list. [Gwenaelle GREMION, Canada]	Taken into account. Thompson et al 2017a and 2017b are duplicates in the reference list. References are fully reconciled with SOD submission.
30564	84	27	84	29	this sentence about Early Holocene should precede the discussion of mid-Holocene. Keeping a consistency in time helps readability of the text [Annalisa Cherchi, Italy]	Rejected. We agree that there are a number of ways in which this paragraph could reasonably be structured. However, it is the norm in this type of paragraph to lead with key AR5 findings if they exist, so we started with the main paper cited in AR5 (Cobb et al 2013), which focused on the mid-Holocene, then moved to how other papers built on that work. The few (non)-results on the early Holocene do not warrant a paragraph of their own.
18228	84	34	84	43	Several studies have given evidence in variability of ENSO at different time scale. However, there is no mention of any attribution or anticipatory attribution to the observed variability. [Gwenaelle GREMION, Canada]	Rejected. Attribution is outside the domain of this chapter. Depending on the context it may fall within the domain of Chapters 3 or 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18234	84	37	84	37	(Dätwyler et al., 2019) is wrongly referenced in the reference list. Should be referenced as = Dätwyler, C., Abram, N. J., Grosjean, M., Wahl, E. R., & Neukom, R. (2019). El Niño–Southern Oscillation variability, teleconnection changes and responses to large volcanic eruptions since AD 1000. <i>International journal of climatology</i> , 39(5), 2711-2724. [Gwenaëlle GREMION, Canada]	Accepted. At the time of FOD submission deadline this paper was accepted but did not yet have a final citation.
24100	84	43	84	43	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
27712	84	53	84	53	replace with and [Poot Delgado Carlos Antonio, Mexico]	Rejected. The sentence reports as intended, that different papers in the literature using different methods report opposite results.
35730	85	1			Explain what 'pressure velocity' is. [Nathan Gillett, Canada]	Taken into account. 'Pressure velocity' is the term used by the authors cited here but it can also be described as vertical velocity in pressure coordinates.
37526	85	1			"pressure velocity" is an unusual term. Could "geostrophic wind" be used instead, or does it refer to something else? [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. 'Pressure velocity' is the term used by the authors cited here but it can also be described as vertical velocity in pressure coordinates.
54220	85	6	85	8	This paper could be useful: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2018GL079764 [Nicola Maher, Germany]	Noted. Maher et al 2018 is more relevant for chapter 3 and is assessed there.
27714	85	7	85	41	replace with and [Poot Delgado Carlos Antonio, Mexico]	Unanswerable - could not identify where the comment refers to in the text.
24102	85	10	85	10	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37528	85	30	85	33	The 1982-83 El Nino may have been extreme in some respects, but the imprint of the 1982 eruption of El Chichon complicates the picture. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. As this text refers specifically to the Cai et al. definition (which 1982-83 clearly met), the El Chichon eruption is not relevant here. El Chichon is clearly relevant to the 1982-83 event's effect on global temperatures, and there is also ongoing discussion within the science community on the potential role of major volcanic eruptions in forcing El Nino events, but neither of these are within the scope of this section.
46824	85	31	85	31	Is the anthropogenic forcing referred to land use changes? Otherwise the influence of anthropogenic forcing, compared to natural forcing, is very small prior to the 20th century. [Charpentier Ljungqvist Fredrik, Sweden]	Unanswerable - could not identify where the comment refers to in the text. It may be attributed to the wrong chapter given the nature of the comment.
46826	85	31	85	33	Internal variability also appears to be a major driving force at multi-decadal time-scales. [Charpentier Ljungqvist Fredrik, Sweden]	Unanswerable - could not identify where the comment refers to in the text. It may be attributed to the wrong chapter given the nature of the comment.
38956	85	37			Please define ONI. [Masahide Kimoto, Japan]	Editorial.
24104	85	39	85	39	Edit to Yu and Kim's (2013) implimentation [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text to be amended.
18238	85	46	85	49	This is an important sentence as it is summarizing the past three paragraphs, however I found it confusing and I think the conclusions can be more clearly stated. [Gwenaelle GREMION, Canada]	Taken into account. Sentence reworded.
24106	85	47	85	47	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
46828	85	48	85	48	Could be clarified that "those observed" refer to in instrumental data. [Charpentier Ljungqvist Fredrik, Sweden]	Unanswerable - could not identify where the comment refers to in the text. It may be attributed to the wrong chapter given the nature of other comments by this reviewer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42912	85	54	86	4	add citation and reference to work of Fedorov and Philander (2000, 2001) describing hypothesized physics of remote and local mode ENSO. [Michael Evans, United States of America]	Rejected. These are references which have been available to several previous Assessment Reports and in any case are not necessary to define the event types described in this paragraph.
36690	86	1	86	8	Look at paper "A model for super El Ninos" by Hameed et al. [William Lorenz, Australia]	Noted. The causes of extreme/super El Ninos are outside the scope of this chapter.
35308	86	1	86	19	A relevant paper that shows the important role of the semiannual oscillation in the seasonal cycle of the Southern Hemisphere midlatitudes in contributing to the development of either a central Pacific or eastern Pacific El Nino: Meehl, G.A., H. van Loon, and Julie M. Arblaster, 2017: The role of the Southern Hemisphere semiannual oscillation in the development of a precursor to central and eastern Pacific Southern Oscillation warm events, Geophys. Res. Lett., 44, doi:10.1002/2017GL073832. [Gerald Meehl, United States of America]	Noted. The causes of particular types of ENSO events are outside the scope of this chapter.
27716	86	4	85	12	replace with and [Poot Delgado Carlos Antonio, Mexico]	Unanswerable - it could not be found what this relates to in the text.
47434	86	10	86	28	Here, regarding the diversity of ENSO, viz. the EP/CP types, more studies would need to be reviewed because this issue has been one of the most active and productive topics in ENSO community during the past of decade. Particularly, the ENSO regime changes need to be introduced more, which occurred in the late 1970s besides ~2000. E.g., ref. to: (1) Ren, Hong-Li and Fei-Fei Jin. 2011: Niño indices for two types of ENSO, Geophys. Res. Lett., 38, L04704, doi:10.1029/2010GL046031. (2) Ren, Hong-Li*, Fei-Fei Jin, Malte F. Stuecker, and Ruihuang Xie. 2013: ENSO regime change since the late 1970s as manifested by two types of ENSO. Journal of the Meteorological Society of Japan, 91(6), 835-842. doi:10.2151/jmsj.2013-608. [Hong-Li Ren, China]	Rejected. In our view the number of references cited is sufficient to support the text. It is also normal in IPCC reports not to cite references which were available at the time of the previous Assessment Report where more recent papers exist.
7858	86	14	86	15	chapter 2 and chapter 10 should discuss the part. It seems chapter 10 do not specially discuss the mechanism of NAO links to regional climate [zhiyan zuo, China]	Rejected, as unanswerable - it cannot be determined which part of the Chapter 2 text this refers to. (It may, however, identify an issue with our NAO section which needs to be reconciled with Chapter 10?).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7224	86	35	86	37	Which Asian monsoon system? Does this refer to the East Asian, the Indian, or both? [Hillman Aubrey, United States of America]	Taken into account. This refers to the broad Asian monsoon circulation in general, of which the East Asian and Indian monsoons are subsets. Wording has been added to make this clearer.
40348	86	35		37	during the developing phase of the ENSO [Chenxi Xu, China]	Rejected. Assuming that the reviewer is saying that 'ENSO' should replace 'monsoon', this sentence is worded as intended.
6782	86	37	86	37	Maybe add: "Daetwyler et al. (2019) identified multi-decadal periods with changed teleconnections over the past 400 years." [Raphael Neukom, Switzerland]	Accepted. Wording to be changed.
13910	86	39	86	48	As well as the shift to more central Pacific Ninos, there is evidence that nonstationarity in ENSO teleconnections can result from natural variability and from nonlinearities in the precipitation response to SST anomalies (O'Reilly 2018, https://doi.org/10.1007/s00382-018-4081-y) [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The purpose of this text is not to provide an exhaustive attribution of different factors which can influence changes in teleconnections, rather to report that some changes in teleconnections which had been found were consistent with those which would be expected with the shift to more CP events.
24108	86	46	86	46	Date missing from reference [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
18240	86	51	87	12	Section 2.4.1.5 talks of Recent events and their implications for longer term trends. The section on recent events is well captured, however, there is no mention their implications for longer-term trends which makes this section incomplete. [Gwenaelle GREMION, Canada]	Rejected. This is dealt with in the final sentence of the paragraph.
7856	86	53	86	55	do we need to say something about the winter or summer NAO/AO/NAM here since we have said lots about winter or summer NAO and the NAO exhibits different feature in different season and different from the annual NAO. [zhiyan zuo, China]	Rejected. Unanswerable - this appears to refer to the wrong section.
24110	87	4	87	4	Insert 'a' before each Niño [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Inserting 'values' after 'Nino 3' makes this consistent with standard usage in the ENSO community.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47436	87	15	87	24	This summary, accurately speaking, is not for ENSO understanding, but for ENSO changes. More importantly, Walker Circulation (WC) is not the associated thing of ENSO but a climatic phenomenon with distinct property. The WC could be influenced by ENSO variation or change, but which doesn't mean more. Just a suggestion, why not mention more sea level pressure associated with Southern Oscillation? It is closely related with WC and El Nino, and have a long-term record, which should not be low confidence only before the lastest 20-30yrs. [Hong-Li Ren, China]	Rejected. This sentence refers specifically to the mix of CP and EP events, not to the whole ENSO phenomenon as the reviewer comment implies.
46674	87	17	87	24	Assessment on ENSO variability is inconsistent with assessment in Section 12.5.2.3 [WGI TSU, France]	Rejected. There is no inconsistency here - the context of the Chapter 12 assessment is the projections findings in Chapter 4, not the observed changes in Chapter 2.
24112	87	20	87	21	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
43344	87	23	87	23	Change "centred" to "based" [James Renwick, New Zealand]	Accepted. Text to be amended.
13250	87	35	87	36	2.4.2 Indian Ocean Basin and dipole modes "The tropical Indian Ocean experiences uniform warming (cooling) during the decay phase of El Niño (La Niña) events, most pronounced during austral summer into autumn, giving rise to the IOB mode." This sentence can be enhanced by citing the previous studies such as following papers. Ohba, M., and H. Ueda, 2005: Basin-wide warming in the equatorial Indian Ocean associated with El Nino. SOLA, 1, 89-92, doi:10.2151/sola.2005-024. Ohba, M., and H. Ueda, 2009: Seasonally Different Response of the Indian Ocean to the Remote Forcing of El Nino: Linking the Dynamics and Thermodynamics. SOLA, 5, 176-179, doi:10.2151/sola.2009-045. [Masamichi Ohba, Japan]	Noted. Definitions/explanations of modes of variability are no longer contained in Chapter 2, they are now moved into technical annex. With respect to the suggested references, in AR6 the papers published after AR5 are prioritized.
35732	87	36			The meaning of 'giving rise to the IOB mode' is not clear. Do the authors mean that this uniform warming/cooling of the Indian Ocean in the decay phase of El Nino/La Nina is defined as the IOB mode? Or that the IOB mode is an independently-existing phenomenon which is excited in the decaying phase of El Nino/La Nina? [Nathan Gillett, Canada]	Definition/explanations of the modes are now moved to the Annex on modes of variability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24114	87	44	87	44	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
24116	87	46	87	51	Insert space between number and units (5.2 ka, 2 ka, 4.6 ka, 51 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
35734	87	52			What does 'east-west dipole in hydroclimate' mean here? Could 'hydroclimate' be replaced by the name of one or more variables, such as precipitation, evaporation or others? [Nathan Gillett, Canada]	Rainfall is now used instead of hydroclimate.
24118	88	1	88	5	Insert space between number and units (8 ka, 17 ka, 17 ka) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
24120	88	27	88	27	Edit reference to Du et al. (2014) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
13252	88	27	88	55	2.4.2 Indian Ocean Basin and dipole modes In this page, it would be good to discuss about the interannual and interdecadal coupling between the IOB and ENSO. [Masamichi Ohba, Japan]	Rejected/noted. The focus of Ch. 2 is on the changes in large-scale indicators and modes, analysis of underlying processes is out of scope of Ch.2
24122	88	33	88	33	Reference required [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. References are included in SOD.
24124	88	45	88	45	Insert 'a' before 'thermocline' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
47438	88	47	88	51	From the second half of the 20th century into the 21th, the positive IOD became more frequent. This statement is based on the paper of Cai et al. (2009) and not true from that time. In 2016, there was an extreme negative IOD event that was the strongest since 1950s and induced a big disaster of drought in East African region and more rainfall in Maritime Continent. This negative-phase event of IOD and its cause as well as its potential contribution to the long-term trend of IOD, need to be mentioned here. Will there be more negative event of IOD occur in the new future? Ref to Lu, Bo, Hong-Li Ren, Adam Scaife, Jie Wu, Nick Dunstone, Doug Smith, Jianghua Wan, Rosemary Eade, Craig MacLachlan, Margaret Gordon, 2018: An extreme negative Indian Ocean Dipole event in 2016: dynamics and predictability. Climate Dynamics, 51(1-2), 89-100. doi: 10.1007/s00382-017-3908-2. [Hong-Li Ren, China]	Noted. The discussions of the decadal fluctuations of the IOD with more frequent positive phase in recent years have been improved in the SOD.
24126	88	48	88	48	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46816	89	0	89	0	For a long-term perspective of the AMV/AMO it would be advisable to refer to Wang et al. (2017): Wang, J., Yang, B., Ljungqvist, F.C., Luterbacher, J., Osborn, T.J., Briffa, K.R., and Zorita, E. 2017: Internal and external forcing of multidecadal Atlantic climate variability over the past 1,200 years. Nature Geoscience, 10, 512–517. [Charpentier Ljungqvist Fredrik, Sweden]	Noted.
18244	89	1	89	1	Is this "low confidence" categorization a reflection of the lack of paleodata? Or observations? I suggest some additional information is included. [Gwenaelle GREMION, Canada]	Justification of the confidence statement has been improved using additional information on instrumental/paleo data availability/quality.
18248	89	2	89	3	Another attribution that can be added is the changes in instruments or data used in analysis. [Gwenaelle GREMION, Canada]	Data uncertainty is noted in the SOD, so comment has been addressed.
13178	89	6	89	45	Include the following references in this summary: Yeager & Robson 2017 (a summary of the current knowledge of ocean circulation and it's relation to the AMO/AMV) and Wang et al. 2017 (a recent multi-site paleoclimate reconstruction that attempts to separate out the AMO and AMV). [Nora Richter, United States of America]	Accepted, reference is included.
55984	89	6	89	45	I would mention there that there is not a clear consensus whether a part of the AMV is externally forced or not. this question the definition of the AMO/V itself. In particular, the cold phase of the AMV over the period 1950-1980 is likely related to the aerosol forcing that started to decrease after this period. Among other articles, this is discussed in the following reference: Haustein, K., Otto, F.E., Venema, V., Jacobs, P., Cowtan, K., Hausfather, Z., Way, R.G., White, B., Subramanian, A. and Schurer, A.P., 2019. A limited role for unforced internal variability in 20 th century warming. Journal of Climate, (2019). [Martin Ménégoz, France]	Rejected. Discussion on the mechanisms and attribution of changes is outside the scope of Chapter 2, this part of the text has now been removed.
18242	89	6	89	46	My understanding is that there is a debate in the literature about the drivers of the AMV. For example the role of oceanic circulation on the AMV and the potential link to atmospheric processes are debated topics. This debate is nicely summarized in Bellomo 2018 Clim Dynamics and perhaps should be referenced here. [Gwenaelle GREMION, Canada]	Discussion of mechanisms is outside the scope of Chapter 2 and has now been removed.
17938	89	9	89	9	Correlations [a typo]. [Branko Grisogono, Croatia]	Editorial. Noted.
24128	89	9	89	9	Correct spelling to 'correlations' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
30568	89	9	89	9	correlations is wrong spelled [Annalisa Cherchi, Italy]	Editorial. Noted.
54990	89	9	89	9	Noticed examples that require proof-reading are not commented while it may be useful to share the need to correct "correlations" in this line. [Kilkis Siir, Turkey]	Editorial. Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18246	89	14	89	18	I suggest including the Wang et al., 2017 Nature Geosciences reference here. The authors found multidecadal variability from 800-2010 CE. [Gwenaelle GREMION, Canada]	Noted.
24130	89	15	89	15	Define 'last years' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The first para after recap was significantly modified
51810	89	15	89	18	there's a new AMO/AMV multi-proxy record that might be worth discussing also -- Wang et al. 2017 Nat. Geosci, doi: 10.1038/NGEO2962 [Anson Cheung, United States of America]	Noted.
24132	89	17	89	17	Remove] [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
18250	89	17	89	17	Gray et al., 2004; and Kilbourne et al., 2014 are missing in the reference list. [Gwenaelle GREMION, Canada]	Editorial. Noted.
18252	89	18	89	18	Svendsen et al., 2014 is missing in the reference list. [Gwenaelle GREMION, Canada]	Editorial. Noted.
18254	89	24	89	30	Enfield et al., 2001; Sutton and Hodson, 2005; Ting et al., 2009; Svendsen et al., 2014; are all missing in the list of reference [Gwenaelle GREMION, Canada]	Editorial. Noted.
24134	89	29	89	29	change to 1990s [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
18256	89	31	89	31	(Robson et al., 2016) Spelling error should be (Robertson et al., 2016) [Gwenaelle GREMION, Canada]	Editorial. Noted.
24136	89	34	89	34	change 'fluxes' to 'flux' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
24138	89	36	89	36	Insert space between number and unit (300 m) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
43194	89	38	89	40	Reference to recent paper of Luo, D. et al., 2017: Winter Eurasian cooling linked with the Atlantic Multidecadal Oscillation. Environmental Research Letters, 12, 125002, doi: 10.1088/1748-9326/aa8de8 would be valuable to shed light on the AMO-blocking relationship [Ian Simmonds, Australia]	Rejected. This reference is relevant for the analysis of underlying processes driving the regional (while large and important) signal. CH2 is focussed only on the large-scale (global/continental) changes in key climate indicators.
57228	89	40	89	40	For the response of blocking to AMV you could add also Davini et al. 2015 ERL 7. Davini P., J. von Hardenberg and S. Corti, 2015: "Tropical origin for the impacts of the Atlantic Multidecadal Variability on the Euro-Atlantic climate." Env. Res. Let. doi:10.1088/1748-9326/10/9/094010 [Corti Susanna, Italy]	Noted.
13904	89	42	89	43	It is not clear what the 'instrumental period' is here. [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. A common definition of the instrumental period may eventually be adopted throughout the whole AR6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
47440	89	48	90	33	The title of 2.4.4 should be modified in the similar way to 2.4.3, that is, PD Oscillation / Variability. It is because the PDV has only been used once whereas the PDO much more. Compared with AMO, it is quite strange and needs to be consistent with. [Hong-Li Ren, China]	Taken into account. To be consistent with the rest of the report, the title will remain as PDV.
24140	89	51	89	51	Change 'form' to 'from' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
51812	90	2	90	7	I believe there are a lot more recent PDO/PDV/IPO records and proxies used than the ones mentioned in here -- e.g. Tree rings: Buckley et al. 2019 Clim. Dyn.: https://doi.org/10.1007/s00382-019-04694-4 ; Coraline Algae: Williams et al. 2017 GRL : https://doi.org/10.1002/2017GL073138 ; Geoduck + tree rings: Black et al. 2009 Paleo3: https://doi.org/10.1016/j.palaeo.2009.04.010 [Anson Cheung, United States of America]	Noted. The suggested literature was reviewed and included in the assessment of Pacific Decadal Variability.
24142	90	6	90	6	Date missing from Felis et al. reference [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
44624	90	13	90	13	"the global mean temperature signal removed" corrected to "the global mean temperature signal was removed". [Liang Zhao, China]	Rejected. In the context of the sentence, there is no need to add "was".
35310	90	15			A relevant paper that quantifies the high correlation between IPO and PDO indices in their Fig. 2c: 226. Han, W., G.A. Meehl, A. Hu, M.A. Alexander, T. Yamagata, D. Yuan, M. Ishii, P. Pegion, J. Zheng, B.D. Hamlington, X.-W. Quan, and R.R. Leben, 2014: Intensification of decadal and multi-decadal sea level variability in the western tropical Pacific during recent decades. Clim. Dyn., 43, 1357-1379, doi:10.1007/s00382-013-1951-1. [Gerald Meehl, United States of America]	Noted. The suggested literature was reviewed and included in the assessment of Pacific Decadal Variability.
24144	90	16	90	17	Remove ' from all dates [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
44626	90	18	90	18	"quite similar" corrected to "quietly similar". [Liang Zhao, China]	Rejected. The appropriate word is 'quite'
18258	90	26	90	28	This sentence lacks citations to support this statement. [Gwenaelle GREMION, Canada]	Accepted. References were included to support the statement.
18260	90	30	90	33	Could there be any attribution to this observed large decadal variability to be include in the summary? This can also be included in other summary sections. [Gwenaelle GREMION, Canada]	Rejected. Assessment of attribution is the purview of chapter 3 and is not covered here.
30566	90	30	90	33	here in the summary PDO but in the title PDV. Definitions need to be harmonized once for all (for all chapters) [Annalisa Cherchi, Italy]	Taken into account. PDV was used instead of PDO.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16014	90	31	90	32	Re: "Since AR5 a shift from positive to negative phase of the PDO has occurred". The timing of phase change of PDO depends on the period (e.g. 11 years) and method (e.g. centre average, trailing average) of averaging of the PDO index. Using monthly PDO index data from JISAO (http://research.jisao.washington.edu/pdo/), the centre-average of 11 years data shows an increasing trend from negative values to about zero at 2013 (i.e. the year when AR5 WGI report was published). In addition, the statement seems inconsistent with a statement in Ch.3, P.72, Lines 13-14: " This was accompanied by a PDV shift toward it positive phase". Please consider revision and alignment of the statements in Ch.2 and Ch.3. [SAI MING LEE, China]	Taken into account. We ensured consistency between chapters regarding the PDO.
39774	90	36	91	31	Notice that the human influence on both SAM and NAM changes is assessed in CH3 (sections 3.7.1 and 3.7.2) while the projections are assessed in CH4 (section 4.3.3.1).. Check consistency and coherency across chapters regarding definitions and associated references. [Carolina Vera, Argentina]	Noted. Consistency was checked among chapters 3 and 4.
47442	90	36	91	40	The conception of annular modes has been confusing to me. NAO is absolutely not an annular mode though it looks similar to AO only during boreal winter. NAO is a definitely regional climate variability mode and different from Arctic Oscillation (AO). I suggest that the title should not be annular modes but mid-high-latitude primary modes. NAM (SAM) is also quite different from AO (AAO) because the mechanisms of them are fairly different from each other. Moreover, the interdecadal variations of NAO is not only featured by its intensity but also its zonal position. Zuo et al. (2016) revealed that since 1950s the NAO has zonally migrate from the westward to the eastward but back westward again after 2000, which influenced the surrounding and downstream climate variations with large differences. Ref to Zuo, Jinqing, Hong-Li Ren, Weijing Li, and Lei Wang. 2016: Interdecadal variations in the relationship between the winter North Atlantic Oscillation and temperature in south-central China. Journal of Climate, 29(20), 7477-7493. doi: 10.1175/JCLI-D-15-0873.1. [Hong-Li Ren, China]	Taken into account. To ensure consistency across chapters 2, 3 and 4, the name of the section is "annular modes". The suggested paper provided regional information, while we are evaluating large-scale changes.
24146	90	43	90	43	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
44630	90	43	90	43	"unusual" corrected to unusually". [Liang Zhao, China]	Rejected (editorial). The appropriate word is 'unusual' in this context.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7172	90	46	91	8	The NAO is mainly a winter phenomenon in the North Atlantic/European sector. The reconstructions in this section use lake and marine sediment and tree-ring information that responds mainly to summer conditions. Reconstructions should be making use of winter-responding proxies (which I know are hard to find, but a few do exist). Why not look at a paper that does refer to these (Jones, P.D., Harpham, C. and Vinther, B.M., 2014: Winter responding proxy temperature reconstructions and the North Atlantic Oscillation. J. Geophys. Res. 119, 6497-6502, doi: 10.1002/2014JD021561). The Winter NAO is know to lead to an out-of-phase relationship between NW Europe and Western Greenland, which was noticed and commented upon by the Norse in Greenland in the 1100s and 1200s. You though refer to Trouet et al (2009) which produces an and NAO reconstruction which gives the while period from 1000 to 1300 a strongly positive NAO, which is ridiculous. If this did happen, NW Europe would have mild winters (which it did), but western Greenland would have had very cold ones (which it didn't). [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The suggested literature was included in the assessment of NAO variability. We also re-evaluated the findings of Trouet et al. (2009) in contrast with the suggested paper.
13906	90	48	90	50	Some quite precise paleo statements such as this one are made with only one reference. How is our confidence in these? [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. New references were incorporated to the assessment.
18262	90	51	90	51	Trouet et al., 2009 is missing in the reference list. [Gwenaelle GREMION, Canada]	Accepted, the reference is added.
24148	90	52	90	52	Change to 'Industrial and 'Pre-Industrial eras' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33312	90				<p>I am sure the authors do not want to include every reconstruction of PDV ever completed, but the list here (1 tree ring, 3 coral, and one lacustrine sediment) seems like an unusual selection. Frequently used/referenced reconstructions include:</p> <p>1) Documentary data Shen, C. M., Wang, W. C., Gong, W., & Hao, Z. X. (2006). A Pacific Decadal Oscillation record since 1470 AD reconstructed from proxy data of summer rainfall over eastern China. <i>Geophysical Research Letters</i>, 33(3), L03702–L03702. https://doi.org/10.1029/2005GL024804</p> <p>2) Tree rings Biondi, F., Gershunov, A., & Cayan, D. R. (2001). North Pacific decadal climate variability since 1661. <i>Journal of Climate</i>, 14(1), 5–10. D'Arrigo, R., Villalba, R., & Wiles, G. (2001). Tree-ring estimates of Pacific decadal climate variability. <i>Climate Dynamics</i>, 18(3–4), 219–224. Gedalof, Z., & Smith, D. J. (2001). Interdecadal climate variability and regime-scale shifts in Pacific North America. <i>Geophysical Research Letters</i>, 28(8), 1515–1518. MacDonald, G. M., & Case, R. A. (2005). Variations in the Pacific Decadal Oscillation over the past millennium. <i>Geophysical Research Letters</i>, 32(8), L08703–L08703. https://doi.org/10.1029/2005GL022478</p> <p>3) Ice cores</p>	<p>Taken into account. Several of the suggested papers were included in the assessment of Pacific Decadal Variability.</p>
35736	91	10	91	15	<p>The title of the section is 'Northern Annular Mode', but this paragraph refers to the mode as the 'Arctic Oscillation'. NAM and AO are two different terms for the same thing, so the authors should pick one term and use that throughout, rather than switching between the two. Cross-chapter box 2.1, Table 2 uses NAM only. AR5 Chapter 2 used NAM only. I recommend using 'NAM'. [Nathan Gillett, Canada]</p>	<p>Noted. To ensure consistency, we used 'NAM'.</p>
35738	91	17	91	40	<p>Replace 'AO' with 'NAM'. [Nathan Gillett, Canada]</p>	<p>Noted. Suggested correction implemented.</p>
24150	91	18	91	18	<p>insert space between number and unit (1000 hPa) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]</p>	<p>Editorial.</p>
29548	91	24	91	25	<p>The period from 2000-today is rather short to call it a trend, could be the upward phase of multi-decadal variability! [Katja Matthes, Germany]</p>	<p>Accepted. We removed the sentence.</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13908	91	27	91	31	This paragraph focuses on a very short period and highlights one recent event which may not be representative. There is also evidence that the level of variability has itself varied on multidecadal timescales, so these may not reflect long term trends (Woollings et al 2018, JCLim, https://doi.org/10.1175/JCLI-D-17-0286.1) [Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The paragraph was modified accordingly.
18264	91	43	92	31	There are teleconnections to the SAM which are not described here that might be worth considering. For example the associated changes in the Southern Westerly winds and connections to the Southern Ocean upwelling. In addition, there are longer paleo-reconstructions of SAM-like variations which are not described here, which might be worth considering particularly in the context of these teleconnections. [Gwenaelle GREMION, Canada]	Taken into account. Results based on longer paleo-reconstructions of SAM were reviewed and included in the assessment. Links with changes in other variables of the Southern Hemisphere were considered in a Technical Annex.
6790	91	52	91	52	Dätwyler et al. year is 2018 [Raphael Neukom, Switzerland]	Editorial.
18266	91	53	91	54	Dätwyler et al. (2018) is not referenced [Gwenaelle GREMION, Canada]	Accepted, the reference is added.
35740	91	53	91	54	Datwyler et al. (2018) should be Datwyler et al. (2017). [Nathan Gillett, Canada]	Rejected. The reference is: Dätwyler, C., Neukom, R., Abram, N.J. et al. Clim Dyn (2018) 51: 2321. https://doi.org/10.1007/s00382-017-4015-0
24152	92	1	92	1	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24154	92	5	92	5	Insert 'the' before 'Campbell' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24156	92	7	92	7	Does this mean the past 100 years or the whole of the 20th Century? Please give dates/quantification. [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was modified.
24158	92	12	92	13	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
48432	92	13	92	14	repetitive with paleo paragraph above [Julie Arblaster, Australia]	Noted. The sentence was modified according to your suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43196	92	17	92	19	For variations in SAM over this period also reference the analysis of Screen, J. A. et al., 2018: Polar climate change as manifest in atmospheric circulation. Current Climate Change Reports, 4, 383-395, doi: 10.1007/s40641-018-0111-4. Also to another very relevant Cerrone paper: Cerrone, D. and co-authors, 2017: Dominant covarying climate signals over the Southern Ocean and Antarctic sea ice influence over the last three decades. Journal of Climate, 30, 3055-3072, doi: 10.1175/JCLI-D-16-0439.1. [Ian Simmonds, Australia]	Noted. The literature was reviewed and the paper of Screen et al. (2018) was included in the assessment.
48426	92	17	92	26	This paragraph mixes SAM changes with SAM impacts/teleconnections, suggest focusing only on the former. Ensure cross-reference to jet section in 2.3.1.3.3. Add Ivy et al 2017 https://doi.org/10.1175/JCLI-D-16-0394.1 [Julie Arblaster, Australia]	Taken into account. The suggested literature was reviewed and included in the assessment of SAM. We included a cross-reference to jet section.
35742	92	19			The change in the relationship between the SAM and East Antarctic temperatures in the 21st century does not in any way contradict the findings regarding the trends in the SAM from instrumental records over the last 40 to 60 years. Delete 'However'. It is more relevant for interpretation of paleo records of the SAM. [Nathan Gillett, Canada]	Noted. Suggested correction implemented.
24160	92	21	92	21	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
39504	92	22	92	24	Vera and Osman (2018) show that before the middle 80's the relationship between SAM and ENSO during austral summer was positive and significant, but after that until 2015 it was negative and significant. Vera, C.S., and M. Osman, 2018: Activity of the Southern Annular Mode during 2015–2016 El Niño event and its impact on Southern Hemisphere climate anomalies. Int. J. Climatol., 38, S1, e1288-e1295, https://doi.org/10.1002/joc.5419 . [Carolina Vera, Argentina]	Noted. The suggested literature was reviewed and included in the assessment of SAM.
48430	92	25	92	26	This sentence seems out of context, is it referring to a figure? A reference should be provided if not [Julie Arblaster, Australia]	Noted. The sentence was removed.
48434	92	28	92	29	Ensure time period is consistent with previous statements (which do not include 1970) . Also check consistency with SROCC and WMO/UNEP 2018 ozone assessment [Julie Arblaster, Australia]	Taken into account. Consistency with previous statements was checked, as well as the recommended reports.
24162	92	37	92	37	Change 'twentieth century' to '20th Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
24164	92	37	92	37	Insert 'to' after 'referred' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24166	93	3	93	3	Change 'twentieth century' to '20th Century' for consistency [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Noted.
29124	93	5	93	7	This discussion of the Atl3 index does not fit well with the discussion of ENSO in 2.4.1 nor the IOB in 2.4.2. Both of these, like Atl3, are defined as area averaged SST anomalies (rather than the difference between 2 area). In the ENSO section it is clear that the mode of variability is treated separately to the basin wide warming. The summary in 2.4.2 is explicit that understanding what the IOB is doing is hard because of the warming trend. Yet the conclusion for the Atl3 conflates the warming trend with the zonal mode variability on top of it. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Linear trends are no longer retained in the discussions of the modes of variability. Also definitions/explanations of the modes are now done in a technical annex for that purpose. Efforts have been made to have consistency in the discussion of the different modes of variability.
40984	93	14	93	14	Three substantial gaps in knowledge appear to be missing: a) Water vapour is a key short-lived GHG and its current trends in all parts of the atmosphere need to be quantified better, b) aerosol radiative forcing uncertainty still represents the largest contributor to the overall radiative forcing uncertainty since pre-industrial times and c) limited understanding of stratospheric circulation changes is prohibiting a better understanding of trends in that region. [Johannes Laube, Germany]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
27948	93	14	93	14	A lot of these gaps in knowledge can be summarized into - Significant uncertainties remain in construction - Lack of knowledge - Short and sparse timeseries - and then ofcourse other gaps I think the section would be more clear if similar bulletpoints (for example significant uncertainties remain in construction) or lack of knowledge) were grouped [roderik van de wal, Netherlands]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
6475	93	14	93	14	Well done. [Hugh Lefcort, United States of America]	Noted with thanks
7226	93	14	94	47	This gaps in knowledge section fails to mention hydroclimate. The Southern Hemisphere in particular lacks sufficient temporal and spatial hydroclimate reconstructions to lead to substantive conclusion [Hillman Aubrey, United States of America]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
29550	93	16	93	16	I agree with this statement in the first sentence. However, I would like to see a statement added that the natural drivers so far do not include any uncertainty estimates, which would be needed in order to estimate with more confidence in particular the regional responses to those drivers. [Katja Matthes, Germany]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7174	93	16	94	47	Amongst these gaps in knowledge (maybe after the one that refers to Allan et al 1991 about undigitized instrumental data before 1950) there is the lack of winter-responding proxies. Ice layers can be separated into winter and summer responding snowfall, and more could be made of snow and lake freezing periods in northern Europe. Also, more could be made of winter temperature reconstructions from historical sources. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
24168	93	23	93	23	Poor English, replace 'like' with 'such as' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
37530	93	25	93	33	Long-lived greenhouse gases (LLGHGs) make an appearance here - though methane is call short-lived in chapter 6. LLGHG could be replaced by WMGHG. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
24170	93	30	93	30	Don't split numbers and units across a line [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
35744	93	32	93	36	Such a discussion on constraints on sources, sinks and budgets of the LLGHGs would appear to be out of scope for Chapter 2, but rather to belong in Chapter 5. [Nathan Gillett, Canada]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
24172	93	39	93	39	Change to Pre-Industrial for consistency elsewhere in the text [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
8254	93	49	94	5	It should mention the lack of Arctic surface temperature data. See reference: Huang J.B. et al., Recently amplified arctic warming has contributed to a continual global warming trend. Nature Climate Change, 10.1038/s41558-017-0009-5h [Zong Ci Zhao, China]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
37532	93	49			The earlier discussion of GMST/GSAT differences failed to cite work using reanalyses, in which GMST and GSAT differences are observationally constrained by combining boundary-layer modelling subject to observational constraints on variables such as surface wind. Earlier discussion also failed to recognise that reanalysis provides data on GSAT. So the word "paucity" is a bit strong. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The new cross-chapter box addresses these points and the reviewer is thanked for raising them so constructively.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14512	94	1	94	15	Our best instrumental data are from the land surface air observations. However, these data have uncertainties or biases when they are used to estimate the long-term trends. An issue is related to the effect of urbanization in the surface air variables trends, including those of temperature and wind speed. Studies of the last two decades from China (hundreds of peer-reviewed publications in Chinese and English) have confirmed the large and significant effect of urbanization on the trends of surface air temperature estimated based on the data of national stations over the past five to six decades, but we have not known whether or in what extent the urbanization effect exist for global land surface air temperature series for varied time periods. This could be added somewhere in the subsection. (CUG, Guoyu Ren) [Guoyu Ren, China]	Rejected. We discuss urbanisation impacts in 2.3. This section is on key gaps in knowledge on the global scale in climate change and given the assessment in 2.3 it does not arise to such a status here.
8258	94	5	94	6	suggestion is to add the effects of urbanization on both surface air temperature and surface wind speed. [Zong Ci Zhao, China]	See comment 14512; urbanization impact on time series was discussed.
24174	94	6	94	6	Change to 'Post-Industrial' and insert 'the' after 'in' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
49952	94	6			What is the definition of post-industrial? This statement seems to imply that we are in an age beyond industrial activity. [Owen Cooper, United States of America]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
35746	94	6			Define 'post-industrial warming'. Usually 'post-industrial' refers to a society or economy, rather than a period in history. [Nathan Gillett, Canada]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
24176	94	12	94	12	Change to 'Pre-Industrial' and 'Early Industrial' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
50318	94	12	94	12	pre-industrial (typo) [Sophie SZOPA, France]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
17940	94	15	94	15	Similar to that in Chapt. 1, it is unclear which boundary layer is concerned (atmospheric, those in oceans, or?)/ [Branko Grisogono, Croatia]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
49954	94	16			This sentence is poorly phrased: "There remains uncertainty as to whether there has been long-term amplification of boundary layer temperature changes throughout the tropical troposphere." It seems to imply that the boundary layer extends throughout the depth of the troposphere. Does the following statement work? "There remains uncertainty as to whether there has been long-term amplification of boundary layer temperature changes across the entire tropical region." [Owen Cooper, United States of America]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37534	94	18	94	19	"and biases in reanalysis datasets due to assimilation of aircraft observations" should be deleted, as this issue has been addressed in the three latest comprehensive global reanalyses, as explained in earlier comments. See also comment 8 on the entire report regarding the use of different generations of reanalyses. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
35748	94	20	94	23	The examples given of paucity of cryospheric data are all for the Arctic. Is there more cryospheric data available in the Antarctic than in the Arctic? [Nathan Gillett, Canada]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
24178	94	22	94	22	Delete 'season' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
24180	94	31	94	31	Delete , after 'Greenland' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
24182	94	37	94	37	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Following the LAM3 discussions section 2.5 has been rewritten in entirety.
13180	95	1	96	1	Include an explanation about what is causing the current warming in our climate systems and how humans have contributed to it. [Nora Richter, United States of America]	Rejected. This FAQ focuses on the observational evidence for temperatures through time. The causes of recent warming is covered in other FAQs.
7262	95	1	97	52	I have focussed my comments on these summarizing frequently asked questions. In almost every case it is very difficult to easily confirm the conclusions by looking at a chapter figure or text. This is a big mistake for a section to which most people will turn at the beginning of their read. Every single statement here should be clearly evident in the text and figures. I will comment on several specifics below. [Bryan Weare, United States of America]	Rejected, thank you. The content of AR6 FAQs does not necessarily directly relate to material covered within the hosting chapter, thus our focus was to draft the FAQs as 'stand-alone' pieces. But we agree that statements need to be consistent with content of the chapter and across the AR.
32944	95	1			This FAQ seems to follow on to FAQ 1.3, check that they are consistent with each other [Aimee Slangen, Netherlands]	Noted - The overlap between FAQs 1.3 and 2.1 were discussed during LAM3 and a plan was developed to eliminate redundancy and maintain consistency.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11594	95	3			The current rate of climatic change is not unprecedented, as many palaeoclimate case studies have demonstrated. It is wrong to just look at regional and global composites because any fast-paced change will be blurred, simply due to remaining uncertainties in age models and proxy limitations. It is very hard to compare modern observational data with palaeoreconstructions, therefore any statement must be very cautious and of rather low confidence until better regional and global temperature composites are available. [Sebastian Luening, Portugal]	Accepted - Changed "unprecedented in geologic history" to "unusual" ... over "millennia".
7264	95	6	95	7	No figure or table directly confirms the statement that the "synchronicity of recent warming appears to be without precedent in geological history." No figure or figures illustrates geological temperatures and forcing factors together in a way that can be compared to similar terms for the recent past. In fact figures 9 and 12 do a poor job for the current era. This lead statement must be clearly backed with data or greatly modified. [Bryan Weare, United States of America]	Accepted and taken into account. Changed "unprecedented in geologic history" to "unusual" ... over "millennia". No discussion of forcing factors is included in this FAQ.
31302	95	6	95	10	This Summary states that the rate of contemporary climate change is unprecedented. This is false. Similar rates of change during the transition from the Younger Dryas to the Holocene are well documented, as they are also during repeated Dansgaard-Oeschger cycles during the last glacial period, as was described in the Palaeoclimate chapter of AR5 (although not in the summary of that chapter, which unfortunately quoted rates per millenium for changes that took place over decades). I noticed that in other parts of the chapter the authors have carefully stated that contemporary rates are very rapid compared to other climate changes during the Holocene (or postglacial period), which is true. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and taken into account - Changed "unprecedented in geologic history" to "unusual" ... over "millennia". In addition, this FAQ concerns large-scale (mainly global) temperatures whereas the high rates of change observed during the end of the YD and DO events are generally based on site-level studies, or regional compilations at the largest scale. The rate of change for GMST (the focus of this FAQ) is probably much slower than local changes and in this respect the rate of change in global estimates is fast compared to those periods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32646	95	6	95	10	To repeat an earlier point, it is important to be saying that earlier changes were not just random events, but very much look to be due to, or consistent with, changes in various factors that would affect the radiative forcing, etc. As such, these earlier events are sort of analogues for the present, indicating that if a forcing factor changes, the climate responds--and so whether there is a natural forcing or a human forcing, the calibrated response would be expected to be similar. It the contrarians really wanted support for GHGs not affecting climate, they would want the past climate to be highly stable despite changes in forcing, and the paleo record makes clear that this is not the case--indeed, the climate changed in the past and that is why there is so much concern about the potential influences of human activities. So, past variations in climate, and not just in temperature, but also in sea level (and this needs to be mentioned--very few have any idea how much sea level has changed in the past--and due to orbital forcing that has no top-of-the-atmosphere annua- and global- average forcing at all). Given that SL was down 120 meters at Last Glacial Maximum and this was followed by 120 centuries when SL rose an average of 1 m/century while the global average temperature was rising, on average, 1 C /2000 years (I see later in chapter, the rate was suggested to be twice this--but the conclusion still applies), that the global average temperature went up 1 C in the last century and may go up 2 C this century, makes the IPCC 1.5 projection of less than 1 m SL rise by 2100 seem likely to be very much less than what would seem plausible based on paleo data. [Michael	Reject - This FAQ focusses on the observational evidence for temperatures through time. The causes of recent warming is covered in other FAQs. Other evidence for climate change is outside the scope of this brief FAQ
11596	95	9	95	10	Authors write: "The recent warming has reversed the long-term natural cooling trend and global temperature is likely higher now than it has been for millennia". This is not entirely true. Several millennial-scale temperature cycles occurred (Bond cycles, Bond et al 2001 in Science) which brought already previous brief warm phases of a few centuries which include e.g. the Medieval Climate Anomaly and the Roman Warm Period. Global Holocene long-term temperature reconstructions such as the ones by Marcott et al 2013 are not able to resolve these because data points are too widely spaced and age models too uncertain. A monotonous long-term cooling as is suggested in this chapter 2 does not represent current knowledge of the palaeoclimate community. [Sebastian Luening, Portugal]	Taken into account - similar to previous comment/responses, see response #32646

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16322	95	12	95	22	The first two paragraphs of this FAQ overlap with FAQ1.3 and FAQ1.4 and so they could perhaps be shortened (or even removed) to create a more succinct FAQ. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Discussions during LAM3 led to a plan to reduce redundancy with FAQ 1.3. Note the responses to disagree that FAQ 1.4 has overlap with FAQ 2.1. Note the responses to #28862, 32944.
50320	95	16	95	16	please consider to replace "understanding" by "characterising" [Sophie SZOPA, France]	Accepted - changed to "characterizing and understanding".
31306	95	19	95	20	This text gives the impression that tree rings are a uniquely reliable source of information on palaeoclimate, whereas in fact they are one of the most controversial – hence the large discrepancies among different reconstructions of climate during the past 1000-2000 years. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Omitted "calculated from tree rings"
51814	95	20	95	20	It's not just the number of tree ring records decline but instead the number of *all* high resolution (<=annual) proxies decline as we go further back in time [Anson Cheung, United States of America]	Accepted - Omitted "calculated from tree rings"
11598	95	24	95	24	Authors write: "Firstly, it's currently warming when natural drivers of climate are acting to cool the Earth." This is wrong. The second half of the 20th century was characterized by exceptionally strong solar activity, some of the strongest of the Holocene context. When solar activity finally left its high plateau around 2000, global warming slowed down, which is a remarkable coincidence. Both AMO and PDO have helped to boost the strong waring that occurred between 1980-2000. It is wrong to say, that potential natural drivers were in cooling mode in the late 20th century when much of the modern warming happened. [Sebastian Luening, Portugal]	Accepted - Omitted references to natural drivers of climate change
7266	95	24	95	29	What figures or data support the assertion that "natural drivers of climate are acting to cool the earth." On the contrary figure 1.b suggests that recent solar changes are acting to warm. What other natural drivers are being referred to? Yes, volcanoes cool, but figure 1c suggests that they are less prevalent in the current period. If the authors are referring to the cooling over millions of years, this statement is misleading at best. [Bryan Weare, United States of America]	Accepted - Omitted references to natural drivers of climate change
9488	95	25			Possibility should be possibly [Jason Briner, United States of America]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37536	95	26			Why does this say "by the end of the 19th century". Figure 5.7c of the AR5 WG1 report shows global temperature at a minimum around 1700. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted - Although 1700 was cold, it remained relatively cold until the marked uptick following the volcanically active early 19th century.
46818	95	31	95	35	Is the 1400-year time-scale taken from AR5? [Charpentier Ljungqvist Fredrik, Sweden]	Taken into account - Omitted reference to "1400 years" and replaced with more general and conservative, "past 1000 years"
11600	95	31	95	36	Authors write: "It's been a very long time since global temperature has been this high. The past 50 years are probably the warmest over the last 1400 years, and possibly much longer." It is hard to justify this statement, given that our understanding of the palaeotemperatures of the last millennia is still evolving. A high confidence statement is being made in the text about the MCA, even though the opposite is being claimed in Chapter 1 (page 69, lines 35-37: "Although Lüning and Vahrenholt (2017) suggest a much longer context for defining pre-industrial, estimates of natural radiative forcings and global temperature are too uncertain to allow a reliable estimate for longer periods." How can a high confidence statement being made under those circumstances. In fact, in reality global and hemispheric temperature reconstructions are still unstable and change significantly from version to version. New evidence now indicates that the MCA was in fact more synchronous than thought. [Sebastian Luening, Portugal]	Taken into account - similar to previous comment
11602	95	32	95	36	Again, very little evidence to back up this statement. The statement mostly refers to a paper by Marcott et al. 2013 which is predominantly based on sea surface temperatures. Only about 10% of the proxies used in the paper originate from land sites. The warming of the HTM in this paper appears significantly underestimated because (1) the oceans warm slower and less intense than land, and (2) switch of currents leading to a colder HTM were misinterpreted as a cooling. The results of Marcott et al. 2013 therefore have to be treated with caution. It is very likely that the HTM on a global scale was much warmer, when reconstructed using a more balanced mix of land and oceanic sites. In many parts of the Arctic, summer temperatures were up to 4°C warmer than today. The Greenland ice sheet was smaller than today and many glaciers in the Alps were smaller than today or have disappeared altogether. [Sebastian Luening, Portugal]	Taken into account - similar to previous comment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40634	95	34	95	36	I do not think this is a solid statement. The mountain glaciers in the Early Holocene were smaller than today and the tree line was higher, at least in the Northern Hemisphere (Solomina et al., 2015). Although both proxies have a delayed reaction to temperature changes (from a few decades to a century) it is not yet proved that the modern climate is warmer than the one in the early-mid Holocene. [Olga Solomina, Russian Federation]	Taken into account - Unclear which statement the reviewer is referring to as "not solid" because the text agrees with the reviewer's comment, i.e., "there is ongoing discussion on whether the world was warmer before the cooling trend that began thousands of years ago." Agreed that the NH was warmer during the early Holocene than now (as was insolation), but this FAQ focuses on the global mean temperature, not on the NH alone.
48714	95	36	95	36	5 m is inconsistent with current constraints and subsequent statements in the draft report -> 6-10 m [Lev Tarasov, Canada]	Accepted - value should now be consistent with CH9 value
37538	95	36			Change "present warming", which makes no sense, to something like "their present level". [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Omitted "warming"
16324	95	38	95	39	It would be helpful to define 'interglacial period'. It would also be clearer to state when the Holocene Epoch began. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Defined "interglacial" as the interval separating major ice ages and Holocene as beginning around 12,000 years ago
11604	95	38	95	44	Authors are ignoring millennial scale cycles with significant amplitudes that have been found on all seven continents. See Lüning & Vahrenholt 2016: "The Sun's Role in Climate". doi: 10.1016/B978-0-12-804588-6.00016-1. Global Holocene long-term temperature reconstructions such as the ones by Marcott et al 2013 are not able to resolve these because data points are too widely spaced and age models too uncertain. A monotonous long-term cooling as is suggested in this chapter 2 does not represent current knowledge of the palaeoclimate community. [Sebastian Luening, Portugal]	Taken into account - similar to previous comment #48714
40636	95	38	95	44	The paragraph is quite impressive, but I think that the direct comparisons of low resolution data (temperature change of about 5°C took about 5000 years) and short-term trends ("Compare this to the present temperature increase of 1°C in just 170 years") should be complemented by a comment that the resolution of our proxies in many cases is not enough for the direct comparisons of the high frequency variations of modern and ancient temperatures. [Olga Solomina, Russian Federation]	Accepted - Qualified this statement, "The resolution of most paleoclimate records during the last deglacial period is too low to reconstruct century-scale global average temperature for direct comparison with the industrial era."

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46820	95	38	95	45	The actual speed of past episodes of warming is hard to detect from available evidence – thus precluding a good comparison with recent warming. The warming at the deglaciation and during the early Holocene did most likely not occur gradually but rather step-wise during a number of periods. From ice-core data some episodes of warming seem to have occurred very rapidly in a short time-span and other proxies have a too low resolution to resolve the issue of the speed of the warming. [Charpentier Ljungqvist Fredrik, Sweden]	Accepted - Qualified this statement as, "The resolution of most paleoclimate records during the last deglacial period is too low to reconstruct century-scale global average temperature for direct comparison with the industrial era."
24184	95	39	95	39	Poor English, replace 'like' with 'such as' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
31304	95	39	95	41	The transition from the last glacial to the present interglacial period is said to have taken 5000 years. This is misleading because it ignores the extremely "bumpy" nature of the transition. The most recent rapid transition took place at the end of the Younger Dryas period, which was a temporary (millennial-scale) freeze when temperatures over much of the Earth were comparable to those during the LGM. Over this transition, multiple indicators of regional and global environment changed from glacial to interglacial states during a period of a few decades. See Steffenson et al. (2008) Science for details. [Iain Colin Prentice, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Qualified this statement by stating that the rates of change were variable during this interval.
37540	95	42			"170 years" is too long. The early-industrial baseline of 1850-1900 is taken as an approximation to the pre-industrial level. The atmosphere in recent years has been at about 1°C above this baseline level. But the baseline level is not the 1850 value. Fig 2.11 shows temperatures to have been rather uniform on average over the period 1850-1900, so one could say there has been 1°C of warming since 1875 (the mid-point of the reference period; i.e. over 145 rather than 170 years) or since 1900 (i.e. 120 years rather than 170 years). [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - used 1850-1900 as the baseline (as in other sections of AR6) and Omitted reference to "170 years".
16326	95	46	95	48	It might be helpful to state when the Medieval Climate Anomaly was. [Renee van Diemen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - stated the timing of the MCA (950-1250 as defined in AR5).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11606	95	46	95	50	It is simply not true that the MCA was restricted to the North Atlantic region. This claim stems from a time when the database outside the North Atlantic was still fragmentary. Meanwhile much more data from e.g. the Southern Hemisphere are available which document that the MCA was warm in most parts of the world. See Lüning et al. (2019a): The Medieval Climate Anomaly in South America. Quaternary International, 508: 70-87. doi: 10.1016/j.quaint.2018.10.041; Lüning et al. (2017): Warming and cooling: The Medieval Climate Anomaly in Africa and Arabia. Paleoceanography 32 (11): 1219-1235, doi: 10.1002/2017PA003237. For Australasia see Gergis et al. 2016, DOI: 10.1175/JCLI-D-13-00781.1 and Lüning et al. (2019b): The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012. For Antarctica, for ice core data see Stenni et al 2017, doi 10.5194/cp-13-1609-2017, for non-ice core data see e.g. van der Bilt et al. 2017, DOI: 10.1002/jqs.2937. For further references click on sites colour-coded in red on this map: http://t1p.de/mwp . [Sebastian Luening, Portugal]	Taken into account - Agreed that warming during the MCA is known from outside of the North Atlantic. Statement is now qualified to say that the North Atlantic region warmed more than "MANY" other regions. Elsewhere, have now added the statement that temperature fluctuations during these loosely defined periods varied regionally
46822	95	47	95	48	Actually, while it might be entirely true that the North Atlantic warmed comparatively more during the MCA the evidence for supporting this are rather weak given the sparse proxy coverage. Some proxies from the interior of Asia also show an amplified warming. [Charpentier Ljungqvist Fredrik, Sweden]	Taken into account - Agreed that warming during the MCA is known from outside of the North Atlantic. Statement is now qualified to say that the North Atlantic region warmed more than "MANY" other regions. Elsewhere, have now added the statement that temperature fluctuations during these loosely defined periods varied regionally
24186	95	49	95	49	Repl;ace hyphewns with , x2 [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
24188	95	52	95	52	Edit to 'Not only is the current warming ..' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial
11608	95	52	95	54	Delete this. The term "Anthropocene" was formally rejected by the Stratigraphic Commission. [Sebastian Luening, Portugal]	Accepted - Omitted sentence referring to Anthropocene.
24190	95	54	95	54	There is debate about the timing of the Anthropocene: it would be helpful to know the context of that definition here [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - Omitted sentence referring to Anthropocene.
31998	95	54	95	54	Is the FAQs the place to start the debate about defining a new geological period (or is it an epoch? Or an age? Or an era? [Marie-France Loutre, Switzerland]	Accepted - Omitted sentence referring to Anthropocene.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33314	95				There are a few statements mentioning that recent (post-industrial) warming reduced the "long term natural cooling trend" (p.95, lines 9-10) in this chapter. In this FAQ 2.1, line 24 mentions that "natural drivers of climate are acting to cool the Earth." Suggest including information somewhere on what those natural drivers of cooling are, when and why this trend began, etc. [Erika Wise, United States of America]	Taken into account - Omitted statements about natural drivers that are acting to cool the Earth. The strongest negative forcing agents are aerosols, which are only partly natural. Orbital changes are too slow to be significant over short time scales.
15102	97	1	97	52	While it's undeniable that the climate changes, what isn't established is the presumed connection to CO2 emissions, other than the fact that CO2 is a GHG, that CO2 concentrations are increasing combined with a significantly over-estimated ECS based on the policy requirements for a large effect combined with attributing all change to the activities of man. Equally coincidental is that the start of the Industrial Revolution coincided with the end of the LIA, after which natural warming and the corresponding CO2 increases should be expected. This is just one example of many where the null hypothesis is proactively denied. There's also no evidence suggesting that the contemporary rate of change is unusual based on prior change. In fact, the ice core data tells us that even longer term averages typically vary at rates comparable to and often larger than the current rate of change in much shorter term averages. [George White, United States of America]	Noted. No specific changes requested or actioned. Comment is at odds with the evidence presented throughout the report.
56220	97	1	98	8	This FAQ could also mention global changes in extremes addressed in chapter 11 as further evidence. [Sonia Seneviratne, Switzerland]	Taken into account - text revised (added new sentence on extremes and reference to Chapter 11).
35750	97	6	97	8	This sentence describes warming from 'the depths of the ocean to high in the atmosphere'. The meaning of the 'high in the atmosphere' is not specific, but it could be interpreted as above the tropopause, in which case of course this is not correct, because the stratosphere has cooled. I suggest deleting 'high in the'. [Nathan Gillett, Canada]	Taken into account - combined with comment 37544.
24192	97	8	97	8	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
50322	97	8	97	8	"inbetween" => "inbetween" [Sophie SZOPA, France]	Editorial.
24194	97	12	97	12	Capital E for 'earth' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - sentence no longer included in the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37542	97	12			Yes, relative to what we had in the past we are in a golden age of earth observation. But those who have to argue for resources to improve further (or even sustain) the present observing system, which is still not all it could be, might not be helped by reference to a golden age. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised (sentence deleted).
24196	97	18	97	18	Change to Industrial Revolution [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24198	97	19	97	19	Capital C for century and superscript 'th' (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
24200	97	21	97	21	change 'kilometers' to 'kilometres' [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.
7268	97	21	97	22	There is little support for the statement "with associated increases in ... rainfall." The text referring to precipitation trends on page 46, lines 46-49 indicates low confidence before 1950 and "medium confidence afterwards with mixed and non-significant global trends." Only recent mid-latitude trends are designated with high confidence. The noisy data in figure 15 and the highly variable results in figures 16 and 17 do not contribute to a more optimistic assessment. The text and table on pages 47 and 48 also suggest low overall confidence. [Bryan Weare, United States of America]	Taken into account - text revised (sentence now states there is only suggestive evidence of rising precipitation).
37544	97	21			It's more than a few km, it's the depth of the troposphere. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
7270	97	24	97	26	I see nothing in the text stating "more than 90% of the excess energy gained in the climate system being stored in the oceans." Also figures 29 and 30 say nothing about this. [Bryan Weare, United States of America]	Rejected. This issue is outside the scope of the chapter (topic is covered in Chapters 7 and 9).
7272	97	36	97	38	I see no budget suggesting that sea level rises are balanced by glacial melt and thermal expansion. The potentially relevant figures 26,27 30, and 32 require considerable manipulation to show such a thing. This indeed would be a worthwhile budget analysis. [Bryan Weare, United States of America]	Noted. The assessment of key-processes driving sea level change is performed in x-chapter Box 9.1 to which chapter 2 is contributing.
24202	97	45	97	45	Capital C for century (for consistency elsewhere in Chapter) [Peter Burt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33460	97	45	97	46	When discussing ocean acidification, the IPCC report should not use the words "acid" or "acidic." "Acidic" waters are considered those with pH of < 7; except for some extreme environments, the oceans are alkaline. I recommend this be worded as "the global ocean has increased in acidity" as called for by Gattuso et al. here: https://news-oceanacidification-icc.org/2015/08/26/a-plea-to-ocean-acidification-scientists/ [Adrienne Sutton, United States of America]	Accepted - text revised.
49956	97	49			The definition of "ubiquitous" is: existing or being everywhere at the same time, or constantly encountered. Therefore this word is far too strong for describing changes of the climate system, because some components have not changed. Take for example the summary of the global hydrological cycle discussed in Section 2.3.1.2. Some aspects of the hydrological cycle have changed, while others have not. [Owen Cooper, United States of America]	Taken into account - text revised.
55874	99	35	99	40	Double reference Albani et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55910	100	31	100	36	Double reference Angerer et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
30138	103	43	103	45	Brun et al (2018) listed here is wrong and is the same as Brun et al 2017, listed 3 lines below [patrick Wagnon, France]	Noted. The reference was amended accordingly.
55876	104	4	104	6	Double reference Cai et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
25548	105	44	105	45	Please enclose the following reference: Chowdhry Beeman, J., et al. (2019). "Antarctic temperature and CO2: near-synchrony yet variable phasing during the last deglaciation." Clim. Past 15: 913-926. [Dominique Raynaud, France]	Reference was deleted from text. No longer in SOD.
6792	107	1	107	1	Dätwyler et al. year is 2018 [Raphael Neukom, Switzerland]	Noted. The reference was amended accordingly.
55878	108	55	108	58	Double reference Durack and Wijffels [Martin Stendel, Denmark]	Noted. Was amended accordingly.
55880	110	25	110	25	Typo: Cambridge [Martin Stendel, Denmark]	Noted. Was amended accordingly.
55882	110	47	110	50	Double reference Foster and Rae [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
48478	110	53	110	54	"Frajka-Williams, E., Lankhorst, M., Koelling, J, and Send, U. (2018). Coherent circulation changes in the deep North Atlantic from 16N and 26N transport arrays. J. Geophys. Res.-Oceans. 123, 3427-3443. doi: 10.1029/2018JC013949." [Eleanor Frajka-Williams, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference was amended accordingly.
43734	111	47	111	50	Foster and Rae, same reference listed twice, in different ways [Carles Pelejero, Spain]	Noted. The reference was amended accordingly.
55884	113	49	113	57	Triple reference Gutjahr et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43736	113	49	113	57	Gutjahr et al, same reference listed three times [Carles Pelejero, Spain]	Noted. The reference was amended accordingly.
55886	115	33	113	36	Double reference Hirahara and Fukuda [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55888	116	11	116	18	Double reference Hollis et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
19196	116	22	116	22	The Hönisch et al. 2012 reference cited in the text (page 76, line 23) is missing from the reference list [Baerbel Hoenisch, United States of America]	Noted. The reference was amended accordingly.
13948	118	50	118	54	"Kamae, Y., Li, X., Xie, S. P., and Ueda, H. (2017). Atlantic effects on recent decadal trends in global monsoon. <i>Clim. Dyn.</i> 49, 3443–3455. doi:10.1007/s00382-017-3522-3. Kang, Y., Wen, J., Zhang, T.T., Tian, H., Chen, H. (2014). Assessment of the land surface wetness by using satellite remote sensing data over the Loess Plateau. <i>Chinese J. Geophys.-Ch.</i> 57(8), 2473–2483. doi: 10.6038/cjg20140809 Kaplan, A., Cane, M. A., Kushnir, Y., Clement, A. C., Blumenthal, M. B., and Rajagopalan, B. (1998). Analyses of global sea surface temperature 1856–1991. <i>J. Geophys. Res. Ocean.</i> 103, 18567–18589. doi:10.1029/97JC01736. " [Jun Wen, China]	Noted. The references were amended accordingly.
55890	120	47	120	50	Double reference Koutavas et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55892	122	12	122	15	Double reference Lean [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55894	123	55	123	55	Incomplete reference: Should read Luterbacher, J., Schmutz, C., Gyalistras, D., Xoplaki, E., Wanner, H., 1999: Reconstruction of monthly NAO and EU indices back to AD 1675. <i>Geoph. Res. Lett.</i> 26, 2745-2748. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
52348	123		123		Lowell et al., 2014 (cited on p. 69) is missing [Katherine Glover, United States of America]	Noted. The reference was amended accordingly.
52380	132		132		"Routson et al. in press" citation appears 2x in bibliography [Katherine Glover, United States of America]	Noted. The reference was amended accordingly.
55896	133	2	133	5	Double reference Ryu et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55898	133	25	133	28	Double reference Santoso et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55900	139	35	139	38	Double reference Vinogradova and Ponte [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55902	140	30	140	35	Double reference Wang et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
13950	140	36	140	40	"Wang, W., and Zou, C.-Z. (2014). AMSU-A-Only Atmospheric Temperature Data Records from the Lower Troposphere to the Top of the Stratosphere. J. Atmos. Ocean. Technol. 31, 808–825. doi:10.1175/JTECH-D-13-00134.1. Wang, X.H., Wang, B.T., Xu, X.Y., Liu, T., Duan, Y.J., Zhao, Y. (2018). Spatial and temporal variations in surface soil moisture and vegetation cover in the Loess Plateau from 2000 to 2015. Ecol. Indic. 95, 320–330. doi: 10.1016/j.ecolind.2018.07.058. Wang, X. L., Feng, Y., Chan, R., and Isaac, V. (2016d). Inter-comparison of extra-tropical cyclone activity in nine reanalysis datasets. Atmos. Res. 181, 133–153. doi:10.1016/j.atmosres.2016.06.010. " [Jun Wen, China]	Noted. The references were amended accordingly.
55904	141	13	141	18	Double reference White et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55906	143	9	143	9	Triple reference Zeebe et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
55908	143	41	143	46	Double reference Zhang et al. [Martin Stendel, Denmark]	Noted. The reference was amended accordingly.
40346	143	55	143	57	The citation should be "An, Z., Wu, G, Li, J., Sun, Y., Liu, Y., Zhou, W., et al. (2015). Global Monsoon Dynamics and Climate Change. Annu. Rev. Earth Planet. Sci. 43, 29–77. doi:10.1146/annurev-earth-060313-054623." [Chenxi Xu, China]	Noted. The reference was amended accordingly.
28030	145	1	145	1	This figure does not seem to support the conclusions made in the chapter that there is a "slight strengthening observed in the southern hemisphere Hadley Cell" and that "there has been a very likely widening of the Hadley Circulation since the 1980s". In the figure, it looks like the southern hemisphere HC is slightly weakening and that its extend shows no trend. [roderik van de wal, Netherlands]	Noted/taken into account. This comment seems to be referring to Fig. 2.18. The concern comprises the expansion of both NH and SH HC. This figure only shows one of the most commonly used metrics to define the position of the HC edge based on the three reanalyses, while the assessment includes all the available metrics from the recent literature. Trends for the SH HC strength are positive and significant, based on ERA-Interim and MERRA-2. This is now better articulated in the report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11610	145	1			Solar activity development in Fig. 2.1a is not in line with previous longer-term reconstructions. In reality the second half of the 20th century was one of the most active phases of the entire Holocene. See Steinhilber et al. 2012 (doi 10.1073/pnas.1118965109) and Solanki et al. 2004, https://www.nature.com/articles/nature02995 . In contrast to sun spots, the solar magnetic field reached its highest values in the late 20th Century. I suggest that you show the solar magnetic field as well. [Sebastian Luening, Portugal]	Reject - Fig. 2.1a does show that the TSI of the second half of the 20th century was relatively high, especially the minima of the 11-year cycles. Comparison with the entire Holocene isn't possible in this figure because the time scale extends back only 2500 years. The reconstruction of the magnetic field largely parallels the reconstruction of TSI.
50324	145	4	145	4	Lower panels "volcanic" should appear on the vertical axis [Sophie SZOPA, France]	Accepted - Add "volcanic" to y-axis title.
9126	145	4	145	18	The Figures 2.1 (a) and (b) provide good evidence of prior warm and cool periods (eg Roman, Medieval and current warming, versus the Little Ice Age). [Jim O'Brien, Ireland]	Noted - no specific revision suggested.
47530	145	16	145	16	reference should be to 7.3.4.5 [Matthew Toohey, Germany]	Accepted - corrected cross-chapter reference.
7274	146	0	146	0	I believe that discussions of CO2 over millions of years confuses much more than it edifies. Fig. 3 gives all of the background that is necessary. [Bryan Weare, United States of America]	Rejected - Figure 2.3 gives information on only the last 800 ka, whose time span may not be sufficient to evaluate current stage of GHG concentration levels.
11612	146	1			Position of the time unit label is confusing as it is not clear as it refers to the graph below or above. [Sebastian Luening, Portugal]	Accepted - the positions of "Age (Myr bp)" placed closer to the axis.
32000	146				The main text uses BP (capital letters) while this figure uses bp (lower case). [Marie-France Loutre, Switzerland]	Accepted - editorial
32002	147	1	147	16	I have some problem with the colors, for example, blue cross, red band. What is the grey shade in panel d? Is it uncertainty? [Marie-France Loutre, Switzerland]	Accepted - editorial
42914	147	1	148	11	Are the different data presented on the same (or even similar) time resolution in these figures? This would enable an apples-to-apples comparison. If not, the difference in resolution should be noted in the caption, especially for comparison of direct modern CO2 estimates to indirect paleoCO2 estimates [Michael Evans, United States of America]	Taken into account - we add words in the figure caption to notify the multiple timescales.
35752	147	4			Avoid using 'references therein'. Cite the original sources. [Nathan Gillett, Canada]	Taken into account - several references were added.
41416	148	0	148	10	Add indexes a), b) and c) to the figures and remove the lines joining the periods without N2O data in figure a). [Lucas Bianchi, Argentina]	Accepted, thanks. Indices are added and the line removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40986	148	1	148	1	Figure 2.3: It is not clear why there is a temporal gap between the end of the left hand panels and the start of the right hand ones. [Johannes Laube, Germany]	Accepted/noted. In the SOD panels (b) and (c) are presented to show recently-published high-resolution records.
28032	148	1	148	1	Useful to show untill present day to see extream of nowadays GHG-levels. Why is in c) cutt-off at 300 ppm and in a) cutt-off at 350 ppm? [roderik van de wal, Netherlands]	Taken into account - we include modern values in (a), but keep the cut-off in (c) so that we better show the change in the early part of the anthropogenic increases.
28034	148	1	148	1	It would be nice to add the time periods to panels b) and c), instead of just mentioning them in the caption. [roderik van de wal, Netherlands]	In figure, we graphically show which parts in (a) correspond to age intervals of (b) and (c).
28036	148	1	148	1	The bottom panels are too crowded and contain complex information which does not make their purpose very clear. These need further explanation and a conclusion. [roderik van de wal, Netherlands]	Taken into account - In (c), we delete the error bars so that better show individual data.
28038	148	1	148	1	The caption and the figure do not show the information nicely. When I just look at the figure, I see consistency/shift versus group, this is not clear because it is not shown what consistency/shift is refering to. For example, in the caption 'shift' is only shown as 'shift in time (days per decade)'. you will need to read the text of the section to understand what the figure is about, it needs more information to be self explanatory. [roderik van de wal, Netherlands]	Rejected. Not clear - could not find 'shift' in the figure caption
28040	148	1	148	1	It is not clear to me what the Squared chord distance (SCD) is, I am not sure if this is due to my background in physics or that it needs more explanation. The same goes for compositional turnover rate, I do not know what this is or how to explain this. Due to this, I am not able to get a take home message from this figure, so I think it need more basic information at least about the two points above. [roderik van de wal, Netherlands]	Rejected, Not clear - could not find 'SCD' in the figure caption
42726	148		148		Figure 2.3 - Consider color-coding gas axes to data in top graph, otherwise methane axis looks somehow related to the blue box for the lower graphs. Then you'd perhaps be able to make that box black too instead of the same color as some of the data? For X-axes, I think labeling the axis "thousands of years" is more clear than only one of the axis values having 10 ^{^3} , it's unclear that the unit extends to the other values as well. [Stephanie Courtney, United States of America]	Accepted - editorial - we changed the label text colors so that a specific colour indicates a specific gas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35754	148				Figure 2.3. There is a gap between 10kyr and 2kyr before present - this period is not covered in any of the graphs. [Nathan Gillett, Canada]	(b) and (c) are used only for the time intervals where we have high-resolution records and significant improvements since AR5.
57326	149	2	149	15	Can you scale the vertical axes of the three figures to show approximately the same impact on radiative forcing? [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This information is partly conveyed in revised ERF figure -Figure 2.9 and in much greater detail in Chapter 7.
51816	149	14	149	14	The statement, "...prevent calculation of growth rates..." is specifically related to N2O right? [Anson Cheung, United States of America]	Accepted. Indeed, and this is changed
40988	150	1	150	1	Figure 2.4: The selection of gases, in particular for the lower two panels, is inconsistent with both the global abundance and the radiative importance of halogenated gases. As mentioned before, other compounds such as HFC-32, HFC-245a, c-C4F8, or HFC-227ea have comparable or even larger radiative impacts than HFC-152a, C3F8, NF3 or SO2F2. [Johannes Laube, Germany]	Accepted. Figure 2.4 follows now according to a rationalized approach.
50326	150	3	150	3	"select" should be "selected. There is no lifetime in table 2.2. [Sophie SZOPA, France]	Noted.
18268	151	6	151	10	The time period 1970 - 2017 is shown in the figure, but as the confidence interval uses the period 1964-1980 it may be beneficial to extend this axis (i.e. back to 1964) for continuity [Gwenaelle GREMION, Canada]	Accept. The Figure now goes back to 1964
7276	152	0	152	0	This figure is very confusing and adds little to Figure 6. [Bryan Weare, United States of America]	Rejected. The reviewer seems to mix up ozone (Fig. 2.6) and aerosols (Fig.2.8).
40990	152	1	152	1	Figure 2.7: Triangles are asymmetric and therefore should not be used as symbols conveying quantitative messages. Also, axis descriptions are missing for the left-hand panel. [Johannes Laube, Germany]	Accepted/noted. Axis descriptions were present in the original figure, and unfortunately disappeared in the creation of the pdf. Triangles are equilateral triangles, with 3 lines of symmetry: see https://www.mathsisfun.com/geometry/symmetry-line-plane-shapes.html
40350	152	3		12	Reference of rBC from South Asia should be added. [Chenxi Xu, China]	Taken into account. The South Asia curve was shown by mistake only and should not appear.
35758	152	6			Over what period are the trends shown calculated? [Nathan Gillett, Canada]	Accepted. The periods are stated in the Caption, but were added to the Figure itself, too.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35760	152	6			Given that trends are shown, the base period for the calculation of anomalies does not matter. [Nathan Gillett, Canada]	Rejected. No base period is mentioned, nor are anomalies.
42728	152		152		Figure 2.7 - Key needs to be emphasized and maybe contextualized more, consider separating it out below to do that. Explanation and labels of left-side graph needed. Left graph could also be separated out below, it's nice to have contiguous with the latitude of the map, but the map is difficult to see so separation might be worth it to make it larger and more readable. [Stephanie Courtney, United States of America]	Taken into account. The Suggested modifications are considered along with other requests.
35756	152				Figure 2.7. The panel on the left is missing titles and units on the x and y axis. [Nathan Gillett, Canada]	Accepted. Original figure had the labels, but they were cropped off during generation of the pdf. The Figure is completely revised.
28042	153	1	153	1	In c) and d) there are areas with large positive or negative trends, but these are not significant (south-Asia). This seems misleading. [roderik van de wal, Netherlands]	Taken into account. However, it seems better to report the trends even if variability in these regions is so large that the trends are not significant.
35762	153	3			Specify the location in Europe. Note that Svalbard, and Russia are mentioned separately on the following lines, and these are both (at least in part) in Europe. [Nathan Gillett, Canada]	Taken into account. The exact locations are now provided in the Figure caption.
49958	153	17			In Figure 2.8 it's very difficult to see the superimposed trends of AOD from the AERONET network. [Owen Cooper, United States of America]	Taken into account. We are sorry about the poor quality of the plot, which occurred during the compilation of the FOD. In reality the figure have a much better quality
35764	153	20			AODf is not defined. [Nathan Gillett, Canada]	Editorial
7278	154	0	154	0	This important figure is very difficult to read and interpret. It would be far more useful if it had a panel like Fig. 12 with decadal means. This would allow clearer separation of the various components and a more direct comparison with temperature changes. [Bryan Weare, United States of America]	Taken into account. A more readable distinction between the species was made. For more detail (e.g. comparison with temperature changes, decadal averages), the reader is referred to Chapter 7.
25612	154	1	154	1	Give uncertainties by bars and whiskers at the right of the time series as in Figure 8.18 of AR5. [Stephen E Schwartz, United States of America]	Taken into account. Uncertainties are shown more clearly. However, bars is rather something for Chapter 7.
40992	154	1	154	1	Figure 2.9: The term "Halogens" is very misleading as chemically that would refer to fluorine, chlorine, bromine and iodine only, not the halogenated organic compounds that are actually causing the radiative effects here. Also, shouldn't it be "1750" in line 5? [Johannes Laube, Germany]	Accepted. Instead, the term "Synthetic greenhouse gases" is now used.1750 is correct, text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42730	154		154		Figure 2.9 - Potentially a widely useful and distributed graphic so the simplicity is excellent. A few things could help comprehension though, like fully spelling out ERF for the axis label, maybe color-coding uncertainties to the forcings (may require some color changes), decreasing saturation of forcings so composite trend is more prominent, using less pastels so overlapping lines are more distinguishable, placing forcings legend on the right-hand side outside the graph so they line up with the corresponding data, etc. [Stephanie Courtney, United States of America]	Taken into account. ERF is now spelled out. Colours have been modified
11614	155	1			Figure 2.10d: The combination of reconstructed palaeoclimate data with modern observational data in one diagram should be avoided. The two datasets refer to very different scales. The smoothed palaeodata does not show the full range of amplitudes. Delete the red line. Furthermore the new PAGES 2k temperature series should not be used until it has been approved by per review. The paper is still in review and it is unclear if it will pass. One gets the idea that the new paper was just written for the IPCC AR6. The new temperature has much less pre-industrial amplitude and reminds very much of the early Hockey stick graphs of Mann et al. Especially the MCA appears to be misrepresented, whilst the LIA is much too warm. Meanwhile much more data from e.g. the Southern Hemisphere are available which document that the MCA was warm in most parts of the world. See Lüning et al. (2019a): The Medieval Climate Anomaly in South America. Quaternary International, 508: 70-87. doi: 10.1016/j.quaint.2018.10.041; Lüning et al. (2017): Warming and cooling: The Medieval Climate Anomaly in Africa and Arabia. Paleoclimatology 32 (11): 1219-1235, doi: 10.1002/2017PA003237. For Australasia see Gergis et al. 2016, DOI: 10.1175/JCLI-D-13-00781.1 and Lüning et al. (2019b): The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012. For Antarctica, for ice core data see Stenni et al 2017, doi 10.5194/cp-13-1609-2017, for non-ice core data see e.g. van der Bilt et al. 2017, DOI: 10.1002/jqs.2937. For further references click on sites colour-coded in red on this map: http://t1p.de/mwp . [Sebastian Luening, Portugal]	Rejected - The reconstruction shown in Fig. 2.10D is now published. Both the instrumental data (red line) and the reconstruction are based on annual data and both have been smoothed equally.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11616	155	1			Fig. 2.10c: The HTM was most likely much warmer than shown in this graph by Marcott et al. 2013 which is predominantly based on sea surface temperatures. Only about 10% of the proxies used in the paper originate from land sites. The warming of the HTM in this paper appears significantly underestimated because (1) the oceans warm slower and less intense than land, and (2) switch of currents leading to a colder HTM were misinterpreted as a cooling. The results of Marcott et al. 2013 therefore have to be treated with caution. It is very likely that the HTM on a global scale was much warmer, when reconstructed using a more balanced mix of land and oceanic sites. In many parts of the Arctic, summer temperatures were up to 4°C warmer than today. The Greenland ice sheet was smaller than today and many glaciers in the Alps were smaller than today or have disappeared altogether. Using the graph from Marcott et al. 2013 here and compare it to other data from the last 200 years is like comparing apples to pears. The CE data of PAGES 2k is for land-based sites, only, whilst the Holocene data from Marcott et al. is mostly from ocean locations. [Sebastian Luening, Portugal]	Taken into account - similar to previous comment.
18270	155	18	155	18	Unclear: Instrumental data shown in the red line? [Gwenaelle GREMION, Canada]	Accepted - changed "instrumental data" to "instrumental-based temperature".
6784	155	18	155	18	Figure seems to end at the year 2000 (sharp), so either change period of instrumental data in caption to 1850-2000 or make the last two decades visible. Suggest to apply the latter to allow putting most recent warming into long term context. [Raphael Neukom, Switzerland]	Accepted - revised figure
33108	155				Figure 2.10: The Snyder (2016) record seems inconsistent with the best LGM temperature estimates, which in my mind reduces the confidence in the reconstruction. This study should be examined for consistency with the LGM estimates (based on a more comprehensive data set) and other approaches for global temperature time series over the 1-2 my time scale. [Jean Lynch-Stieglitz, United States of America]	Noted - Expanded search of LGM reconstructions included in revised text.
35766	155				Figure 2.10. The scaling between the time-axes on the panels is irregular, and there is no panel which clearly shows temperature variations over recent glacial cycles. The ratio of the periods covered in b) to a) is 15 and from d) to c) it is 10, but from c) to b) it is 200. I suggest adding an extra panel showing variations over the past 200 ka. [Nathan Gillett, Canada]	Taken into account - Figure no longer used.
35768	155				Fig 2.10. Add uncertainty estimates to panels a and b. [Nathan Gillett, Canada]	Taken into account - Figure no longer used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18272	156	0	156	0	Diverging colorbar not centred on 0 is misleading at first glance. Would assume all red is positive/warming. Or is colorbar mislabeled? [Gwenaelle GREMION, Canada]	Accepted. Colour scale replaced.
18274	156	6	156	6	Time series are not shown [Gwenaelle GREMION, Canada]	Taken into account. This figure is no longer in SOD in its current form.
18276	157	1	157	1	Please add additional x-axis hatch marks to better distinguish years or decades [Gwenaelle GREMION, Canada]	Taken into account. This figure has been redesigned.
32004	157	1	157	9	The title in the figure seems to suggest that the reference period is 1901-2000, while the caption indicate 1961-90 [Marie-France Loutre, Switzerland]	Accepted. Resolved
26940	157	1	162	11	Please unify wording for the explanation how "significance" of trends etc. is shown. For example, the texts of figure 17 and 19 are different, but describe the same fact. [Joachim Rock, Germany]	Taken into account. Efforts have been made to make text more consistent throughout, including the figure captions.
50328	157	5	157	5	what means "conventionnal" products? [Sophie SZOPA, France]	Noted. In this context, "conventional" means the non-reanalysis data sets. This is more evident from the associated text.
52378	157		185		General comment on figures - some were at very low resolution, and thus highly pixelated - to the point I could not read axes numbers, titles, or labels within the graphic. These include Figures 2.12, 2.13, 2.37, 2.40 [Katherine Glover, United States of America]	Noted. Higher-resolution versions are used in SOD for those figures which remain.
35770	157				Fig 2.12. I suggest using 1850-1900 base period to be consistent with quoted headline warming figures, and to be most relevant to Global Stocktake. [Nathan Gillett, Canada]	Accepted. Figure redesigned.
40994	158	1	158	1	Figure 2-13: Why are other reanalysis data sets such as JRA-5 and MERRA-2 excluded here as well as in section 2.3.1.1.4? [Johannes Laube, Germany]	Taken into account. The intention was to use reanalyses in Figure 2.13 (now 2.12) for illustrative purposes, and play a limited role in the text, noting the identified issues with reanalyses around the 300 hPa level. We anticipate that ERA5 will have significant improvements in this respect, taking it beyond the most recent generations of MERRA and JRA, but until the issues in the lower stratosphere with ERA5 are fully resolved we are omitting reanalysis data from this figure altogether in SOD (with the intention of reintroducing later).
50330	158	4	158	4	add "atmospheric" heights [Sophie SZOPA, France]	Accepted. Text amended.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35772	158				Fig 2.13. There are too many panels in this figure, and it is hard to read the individual panels. I suggest dividing into two or three separate figures. [Nathan Gillett, Canada]	Accepted. This figure has been redesigned
18278	161	0	161	0	Colours are really hard to distinguish in top plot and hatching makes them even harder to see. Also would be more intuitive to reverse color bar so blue is wetter. [Gwenaelle GREMION, Canada]	Taken into account. We homogenised several features of the figures, like axis labels, colorbars and extra-information. We improved the quality of the figures and its description.
18280	161	1	161	1	Would it not be more intuitive to plots graphs as 2016-1980 as opposed to 1980-2016, so wetter regions (blue) = increasing mm/decade (thus more consistent with figure 2.17) [Gwenaelle GREMION, Canada]	Taken into account. We homogenised several features of the figures, like axis labels, colorbars and extra-information. We improved the quality of the figures and its description.
26938	161	1	161	9	Figure 2.16: It is not explained what the crosses should show. Please add explanation for this or remove crosses from figure. If crosses were meant to show significance as in figure 2.17, please improve quality of the figure so that it can be seen what is "behind" the stippling. [Joachim Rock, Germany]	Taken into account. We homogenised several features of the figures, like axis labels, colorbars and extra-information. We improved the quality of the figures and its description.
35774	161	8			Fig 2.16. No confidence intervals are shown. [Nathan Gillett, Canada]	Taken into account. We homogenised several features of the figures, like axis labels, colorbars and extra-information. We improved the quality of the figures and its description.
42916	162	1	162	11	Fig 2.17 shows an ENSO-cold phase like pattern in significant spatial pattern of trend. That's an important synthesizing statement to make with regard to these results. [Michael Evans, United States of America]	Taken into account. We homogenised several features of the figures, like axis labels, colorbars and extra-information. We improved the quality of the figures and its description.
42734	162		162		Figure 2.17 (and many other graphs in the report) have too little information in the graphic itself. I suggest adding more information to axis labels and including descriptive or declarative titles. Especially true in graphics like 2.17 where they're simple enough that extra text wouldn't be too cluttering. [Stephanie Courtney, United States of America]	Taken into account. We homogenised several features of the figures, like axis labels, colorbars and extra-information. We improved the quality of the figures and its description.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29576	163	1	163	9	Will this figure be extended by the newest reanalysis product ERA5 which has a considerably higher vertical resolution than the other reanalyses data sets? [Katja Matthes, Germany]	Noted. ERA5 was considered for the calculation of the figure.
35780	165	4	165	6	Show trends everywhere, not only where they are outside of the range of internal variability, based on the AR(1) model. Moreover, as indicated in my general comment, I suggest not testing significance of trends against internal variability, since this is out of scope of the chapter, but instead adding more information on observational uncertainty. For example, the figure could be replaced with one based on multiple datasets, for example showing the multi-dataset mean, and hatching where trends are of inconsistent sign across datasets or similar. [Nathan Gillett, Canada]	Not applicable. This figure is no longer included in Chapter 2. Observed SLP changes can be found in the text and in Section 2.4 'Changes in modes of variability'
35776	165	4			I suggest showing results instead from in situ data (HadSLP) and perhaps other reanalyses, rather than from a single reanalysis, since reanalyses undoubtedly have biases/differences, and showing just one gives no idea of the uncertainties. [Nathan Gillett, Canada]	Not applicable. This figure is no longer included in Chapter 2. Observed SLP changes can be found in the text and in Section 2.4 'Changes in modes of variability'
35778	165	7			Decadal mean SLP for which decade? [Nathan Gillett, Canada]	Not applicable. This figure is no longer included in Chapter 2. Observed SLP changes can be found in the text and in Section 2.4 'Changes in modes of variability'
6663	166	6	166	8	Since the NasaTeam and Bootstrap in the NOAA CDR v3 are actually "verbatim" from Nasa Goddard (GSFC) datasets, it would be more clear to trace them back to the GSFC source (NSIDC-??? and NSIDC-???) than to the NOAA CDR (that only starts as a CDR in 1987). Second, consider showing Walsh only prior to 1979 (it relies heavily on one of the passive microwave datasets) after that date. Finally, consider showing the trend line for Bootstrap and/or OSISAF/CCI as NasaTeam is known to underperform in Antarctic due to snow layering effects (Fig 2.23). The OSISAF/CCI curve should be extended after 2015 for SOD. [Thomas Lavergne, Norway]	Noted. Some of the suggestions are followed, others not. Data are updated relative to the FOD. The Walsh dataset is shown also beyond 1979 for consistency. The trend for NASA team is just to guide the reader's eye, not to value that series relative to the others.
35782	166				Fig 2.21. The vertical scale used makes it very difficult to see the trends. I suggest using separate scales for March and September, each with a much smaller range than currently used. [Nathan Gillett, Canada]	Noted/rejected. For showing the seasonal differences and because of space limitations, the graphs are plotted in one panel each. In addition, for the SOD, the plot for the Arctic and Antarctic are combined.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35784	166				Fig 2.21. There are multiple studies and datasets examining sea ice extent changes before the satellite record beyond the single Walsh et al. (2017) study shown here. Data from additional studies should also be shown. See e.g. Gagne et al. (2017) https://doi.org/10.1002/2016GL071941 , Titchner and Rayner (2014) https://doi.org/10.1002/2013JD020316 , Mueller et al. (2017) 10.1175/JCLI-D-17-0552.1; Pirón, M. Á. C., and J. A. C. Pasalodos, 2016: Nueva serie de extensión del hielo marino ártico en septiembre entre 1935 y 2014 [A new time series of September Arctic sea ice extent: 1935–2014]. Rev. Climatol., 16, 1–19. These studies generally show an increase in Arctic SIE prior to 1975. Further these datasets generally show that the decrease in September SIE began before the 1990s. [Nathan Gillett, Canada]	The same comment was already made earlier (page 58), there the response was: Noted. The Walsh dataset is seen as a representative dataset for the assessment.
35786	167				Fig 2.22. Are all the thickness measurements shown on the graph an average over the blue area shown on the map, including open water? If the coverage area changes over time (or if it is sampled only where ice is present) this could introduce biases into the thickness shown. Clarify the approach used in the caption. [Nathan Gillett, Canada]	Noted/rejected. To our knowledge, open water is included in the data. More details on how data are collected can be found in the literature cited in the subsection.
6665	168	6	168	8	all my comments on Fig 2.21 apply for Fig 2.23 as well (to the exception of Walsh). [Thomas Lavergne, Norway]	Noted. Some of the suggestions are followed, others not. Data are updated relative to the FOD. The trend for NASA team is just to guide the reader's eye, not to value that series relative to the others.
35788	168				Fig 2.23. The vertical scale used makes it very difficult to see the trends. I suggest using separate scales for February and September, each with a much smaller range than currently used. [Nathan Gillett, Canada]	Noted/rejected. For showing the seasonal differences and because of space limitations, the graphs are plotted in one panel each. In addition, for the SOD, the plot for the Arctic and Antarctic are combined.
57442	169	1	169	10	Please add trend estimates and uncertainties for the regression line. The information content can potentially be enhanced when satellite products would be added. [Marc Schröder, Germany]	Rejected. Figure and caption revised to include additional information for regression line (note trend estimate is provided in section 2.3.2.2 text). Note figure shows NOAA CDR and reference to results from other satellite products are included in section 2.3.2.2 text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11618	169	1			Fig. 2.24 is a good example for cherry picking. If you check the snow trends on Rutgers Global Snow Lab, snow cover has gone up in the Northern Hemisphere both during fall and winter. Authors decided to cherry pick the April because it shows a more dramatic picture. The IPCC should avoid this kind of alarmism and attempt a balanced description of the state of climate. Add fall and winter or delete this figure. [Sebastian Luening, Portugal]	Rejected- Numerous studies, as cited in the text, document decreases in spring SCE which is a key indicator of change. The text does refer to other seasons and cites results of a number of studies and comments on weaker trends in autumn. Note that time series prior to satellite record was not available for entire Northern Hemisphere for the fall period to show SCE change over the last century
35790	169				Fig 2.24. There are big differences between SCE trends in different observational datasets. Show multiple datasets. See for example Mudryk et al. (2015) doi: 10.1175/JCLI-D-15-0229.1, Najafi et al. (2016) doi: 10.1007/s10584-016-1632-2. [Nathan Gillett, Canada]	Noted - Text does present trends based on other observational data sets (including Mudryk et al.).
32008	170	3	170	3	The unit in the figure (tons per m-2) is most likely wrong (probably tons per m2 or tons m-2) [Marie-France Loutre, Switzerland]	Accepted. The unit in axis label is now corrected.
9128	170	4	170	19	As Fig 2.25 (a) shows that glaciers have come and gone since time immemorial, some melts revealing previous vegetation and inhabitation in those areas. [Jim O'Brien, Ireland]	Noted. The respective figure is changed, however It is unclear what was the point and which changes are suggested by the reviewer.
15650	170	5	170	13	You might consider to complement this figure with data from the following study: Zemp et al. (2015, J. Glaciol., Fig 6; for ref see above) provides a figure of the relative length changes since 1600 that might be included as inset. [Michael Zemp, Switzerland]	Noted/rejected. The respective figure was modified relative to the FOD version. Following the chapter outline, we focus on large scale changes.
15652	170	13	170	18	The WGMS reference glaciers are indeed the backbone of the glaciological observation network but - most probably - not representativ for the global changes of all glaciers. For the present figure, you might consider using the annual estimates 1961-2016 by Zemp et al. (2019, Nature; for ref see above). [Michael Zemp, Switzerland]	Noted. The figure (a) was substantially changed. The reference by Zemp et al. 2019 is now cited in the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40610	170		170		Figure 2.25. (a) Mountain glacier relative extent over the Holocene. I believe that the figure is premature. In my opinion it is impossible to produce such curves from the information provided in Solomina et al., 2015. We do not know the size of former glaciers in most cases. The state of knowledge is illustrated at the fig. 4. "Glacier front fluctuations and (or) ELA variations in comparison with the regional orbital signal" in Solomina et al., 2015. It is clear from this figure that there are so many gaps and uncertainties even for the best records that it is too early to construct the curves even for the relative glacier length changes. [Olga Solomina, Russian Federation]	Noted/obsolete. The figure is changed towards a shorter time span and global data.
15654	171	3			Attention to double counting of mass changes from peripheral glaciers in Greenland and Antarctica. See comment above. [Michael Zemp, Switzerland]	Noted. The figure was changed, and the regional data are not anymore shown.
35792	171		172		Fig 2.26 and Fig 2.27. Changes in Greenland ice mass and Antarctic ice mass are shown in different units and separate graphs over different periods. But both contribute to changes in GMSL, and it would be helpful to readers to be able to compare their changes side-by-side. I suggest showing mass balance in Gt/yr for both Greenland and Antarctica, with right hand scales of sea level rise equivalent mm/yr, and Greenland in panel a) and Antarctica in panel b). Greenland mass balance estimates are shown since 1840, whereas Antarctica is only shown since 1992. If there are longer-term estimates of Antarctic mass balance changes then add these to the figure, or if not, then don't shown them but keep horizontal axes the same - this would show lack of knowledge of long-term changes in Antarctic ice sheet. [Nathan Gillett, Canada]	Accepted/noted. The ice mass balance time series for Greenland and Antarctica are now merged into one graph. Several of the suggestions made are taken into account.
9130	172	7	172	10	The West Antarctic ice mass loss shown in the graph represents only 0.1% of the total Antarctic ice mass, and so is within the bounds of natural variability. [Jim O'Brien, Ireland]	Rejected. It remains unclear how the natural variability would be connected to a ratio of change vs. total ice mass, and no reference is given for this statement.
18282	174	1	174	14	Figure 2.29: Consider replacing this figure with a time series showing how ocean temperatures have increased since the 1970s, as mentioned in the text (p. 68, lines 14-15), as opposed to just from 1993. [Gwenaëlle GREMION, Canada]	Noted. Text and figures for Section 2.3.1.1 have been restructured
35794	174	10	174	12	Fig 2.29. Generally dots are shown over regions with weak changes, but the caption indicates that the dots show area of 'most robust signatures'. Is there are mistake? [Nathan Gillett, Canada]	Noted. Text and figures for Section 2.3.1.1 have been restructured
7280	174	10	174	13	This final phrase is confusing. The stippled areas are insignificant. Does this also apply to the difficult-to-read hatched areas in b)? [Bryan Weare, United States of America]	Noted. Text and figures for Section 2.3.1.1 have been restructured

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
26458	175	0	175	0	Fig.2.30. Some of the references cited in the caption are missing in the reference list. [Katsuro Katsumata, Japan]	Noted. Thank you. Corrected
42736	175		175		Fig 2.30 - Since a) has negative values I'm guessing there's a baseline period, please add in caption. Also add units to b and c [Stephanie Courtney, United States of America]	Noted. Text and figures for Section 2.3.1.1 have been restructured
35796	175				Fig 2.30. What do the uncertainty ranges on the ensemble mean show? According to the intro, uncertainty ranges shown are 90% or 95%, but it does not look like these ranges encompass that percentage of individual datasets. Describe how these uncertainties are calculated. I would recommend showing the sample uncertainty ranges, rather than the uncertainty in the mean. The latter would only be valid if errors in individual datasets were independent (i.e. if there were not systematic biases present across datasets). [Nathan Gillett, Canada]	Noted. Text and figures for Section 2.3.1.1 have been restructured
35798	175				Fig 2.30. The spread across individual datasets is large. Does this reflect real observational uncertainty, or are some datasets included which are not reliable? Are there some that could be ruled out from the analysis? [Nathan Gillett, Canada]	Noted. Text and figures for Section 2.3.1.1 have been restructured
42738	176		176		Fig 2.31 - Please add units for salinity change in caption and/or in figure [Stephanie Courtney, United States of America]	Editorial.
8552	177	1	178	10	Reconcile with corresponding figure in chapter 9 (Fig 9.35) [Robert Kopp, United States of America]	Noted. Reconciled
11620	177	1			Fig. 2.32: Which lines represents the global sea level of Kopp et al. 2016? You should be using this, not the local Newfoundland sea level of Kemp et al. 2018. [Sebastian Luening, Portugal]	Rejected. Using global mean sea level time series from Kemp et al. 2018 (see their Figure 10A).
49960	177	2			In Figure 2.32 the expression "Before Present" is used. This expression does not appear anywhere else in Chapter 2 and it needs to be defined. [Owen Cooper, United States of America]	Editorial
17942	178	1	178	1	Quality of some figures is low, e.g., Fig. 2.33, pg. 178, etc. [Branko Grisogono, Croatia]	Noted. Original resolution is higher than made available for FOD review.
11622	178	1			Fig. 2.33: You should be using the global sea level of Kopp et al. 2016, not the local Newfoundland sea level of Kemp et al. 2018. [Sebastian Luening, Portugal]	Rejected. Using global mean sea level time series from Kemp et al. 2018 (see their Figure 10A).
18284	179	1	179	16	Comments on figure 2.34: Incomplete figure captions. Reference of the figure should be added to the figure caption, especially since this figure is exactly the same as in Graven et al., 2013). [Gwenaelle GREMION, Canada]	Taken into account. The figure has been deleted in the SOD.
18286	179	1	179	16	You should specify how the amplitude of the seasonal cycle is calculated: Here it seems that it's the maximum of the detrended CO2atm minus the minimum of that same parameter. [Gwenaelle GREMION, Canada]	Taken into account. The figure has been deleted in the SOD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18288	179	1	179	16	What are the 700mb and 500mb for? It's not obvious that these correspond to different aircraft altitudes. In their paper, Graven et al. (2013) specify that those aircraft measurements were made at 500mb and 700mb pressure surface (which correspond to approximately 6 and 3km altitude). In your figure, we don't know what the empty dots are... [Gwenaelle GREMION, Canada]	Taken into account. The figure has been deleted in the SOD.
18290	180	1	180	17	Both panels represent low-latitude surface ocean pH. It would be great to specify which latitude or range of latitudes. [Gwenaelle GREMION, Canada]	Taken into account. Figure and legend have been modified.
19198	180	5	180	13	Sosdian et al. 2018 have generated only 8 new data points for the interval 0-3.5 Ma. The data included in this study should be cited by who generated them: Chalk et al. 2017, Hönsch et al. 2009, Dyez et al. 2018/Bartoli et al. 2011. Similarly, Gutjahr et al. 2017 followed Penman et al. 2014; that study should also be cited. In addition, Fig.2.2 shows d11B data from Anagnostou et al. 2019, those should also be included in this figure [Baerbel Hoenisch, United States of America]	Taken into account. Figure and legend have been modified.
43738	180	8	180	8	shown, not show [Carles Pelejero, Spain]	Editorial
35800	180				Fig 2.35. Could you add a panel showing recent observed changes in pH to this figure, in order to allow readers to compare paleo variations with recently observed changes? [Nathan Gillett, Canada]	Taken into account. Figure and legend have been modified.
18292	181	1	181	14	In the text, it was said that the mean shift in timing across all marine groups is earlier by 3.22 days/decade. It would be useful to have that information (as la line for example) on the figure 2.36b; especially since the text refers to that figure. [Gwenaelle GREMION, Canada]	Taken into account - figure revised.
41418	182	1	182	12	The scales of the x-axis and y-axis differ among Figures, if they were all the same it would be easier to compare among regions. For example, as it is now, Squared chord distance in Western North America seems to be more variable and even with a higher increase at the end than the two others. However if they were all the axes on the same scale, it probably will not be seen in the same way. [Lucas Bianchi, Argentina]	Taken into account - figure revised.
35802	182				Fig 2.37. Could you show recent observed changes in turnover on this figure? [Nathan Gillett, Canada]	Rejected - the underlying scaling of the paleoclimate data is different from those gathered in contemporary ecological surveys, so the inclusion of both on the same figure would create an inappropriate "apples to oranges" comparison.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7796	183		183		Please reconsider Figure 2.38. It shows negative trends over Northern Scandinavia and positive trends over Indonesia. But the trends are actually reversed that Northern Scandinavia is greening due to temperature increases and Indonesia should have a negative trend due to intense deforestation. The dark green blob over East China is also questionable. [Merja Tölle, Germany]	Taken into account - combined with comment 56802.
18294	184	3	184	11	Figure 2.3. For the diagram on the bottom right (figure c -ice-core records) can the data series be offset a little on separate axes to make the data series trends clearer to see? Labelling of a),b) and c) in the caption are missing from the figure. [Gwenaelle GREMION, Canada]	Taken into account - figure revised.
7282	185	0	185	0	This summary figure is impossible to read and thus evaluate, even when magnified. [Bryan Weare, United States of America]	Noted. Figure resolution occurred during the compilation of the FOD, the actual figures have a better resolution/quality
7284	185	0	185	0	What does this say about variations in ENSO frequency, strength or character? Useless. [Bryan Weare, United States of America]	Noted. The figure is not intended to show ENSO or any other mode of variability so no action required.
15326	185	0	185	0	This type of chart is excellent for putting changes into context and communicating changes at scales that are difficult for people to comprehend. Make sure colors are meaningful and make sense. A key for colors could be helpful here. [Lia Cairone, United States of America]	Taken into account. The figure has been substantively revised following discussions at LAM3
26942	185	1	185	8	Figure 2.40: The quality of the print is too poor to evaluate the figure. [Joachim Rock, Germany]	See comment 7282
42918	185	1	185	8	I had a hard time reading this figure - resolution too low. [Michael Evans, United States of America]	See comment 7282
18296	185	1	185	10	This figure has a really poor resolution and is really hard to read [Gwenaelle GREMION, Canada]	See comment 7282
56240	185	1			Excellent figure! Note that resolution is not so good in provided pdf. Suggest to indicate temperature anomalies with + and - signs (so that it is clear that these are not absolute temperatures, e.g. for Paleocene-Eocene Thermal Maximum) [Sonia Seneviratne, Switzerland]	See response to 15326
29128	185	6	185	8	I really like this figure. It is especially nice that the you provide images to give lay readers an intuition of the time periods. I do not understand what the circles are meant to portray for the midHolocene and last deglacial. May I recommend a either Stonehenge or a pyramid for the mid-Holocene (both of these are a little later, but easily recognisable). I don't know what to recommend for the last deglaciation - perhaps a sickle to indicate the domestication of wheat (again actually happened a little later). The mid-Piacenzian could be represented by a sketch of "Lucy" i.e. an Australopithecus afarensis. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	See response to 15326

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42740	185		185		Fig 2.40 - Love the graphic overall, might adjust some of the coloring - for example, the color green for modern co2 is very similar to the 1400ppm color (even at lowest estimate 1000ppm) which could be read as misleading. Some context may also help in caption, like what the zig-zag lines in last deglaciation (? hard to read) mean. [Stephanie Courtney, United States of America]	See response to 15326
26944	186	1	186	8	Figure 2.41: The quality of the print is too poor to evaluate the figure. [Joachim Rock, Germany]	Noted. This figure has been redesigned for SOD.
42920	186	1	186	9	some references (for example, Wilson et al 2010, Evans et al 2001 2002; Tierney et al 2015) missing - is this a placeholder figure? Add median and error bars for synthesis statement support from this figure? [Michael Evans, United States of America]	Taken into account. References are fully reconciled for SOD. The figure has also been further refined.
29126	187	3	187	13	You may want to show a linear trend through the Atl3 and IOB indices to give an indication of the warming trend. See for example, the Alt3 images produced by the Climate Variability Diagnostics package that is part of the ESMValTool: http://webext.cgd.ucar.edu/Multi-Case/CVDP_repository/cmip5.historical/atl3.timeseries.png [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Chapter 2 does not show trends for the indices of modes of variability, the indices shown are only indicative of the modes of variability and have been computed from the de-trended data or de-trended prior presenting, because the focus here is on the internal variability as seen in observations and how much they have changed, based on peer-reviewed literature.
7286	188	0	188	0	This adds nothing to Figure 10 and, if anything, is far more confusing. More importantly it does not help elucidate the rate or global synchronicity of climate change as discussed on page 95. [Bryan Weare, United States of America]	Taken into account. This figure is no longer used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11624	188	1			FAQ 2.1, Figure 1: No references give for the origin of the various segments of this graph. It is assumed that the Holocene is represented by Marcott et al. 2013 which is predominantly based on sea surface temperatures. Only about 10% of the proxies used in the paper originate from land sites. The warming of the HTM in this paper appears significantly underestimated because (1) the oceans warm slower and less intense than land, and (2) switch of currents leading to a colder HTM were misinterpreted as a cooling. The results of Marcott et al. 2013 therefore have to be treated with caution. It is very likely that the HTM on a global scale was much warmer, when reconstructed using a more balanced mix of land and oceanic sites. In many parts of the Arctic, summer temperatures were up to 4°C warmer than today. The Greenland ice sheet was smaller than today and many glaciers in the Alps were smaller than today or have disappeared altogether. Using the graph from Marcott et al. 2013 here and compare it to other data from the last 200 years is like comparing apples to pears. The CE data of PAGES 2k is for land-based sites, only, whilst the Holocene data from Marcott et al. is mostly from ocean locations. [Sebastian Luening, Portugal]	Noted - IPCC style for FAQs does not allow for references. Comment about HTM is same as for comment #11578 s by this reviewer.
7176	188	7	188	13	FAQ2.1 Figure 1 needs a lot of work. You could go back to the 5 panels one above each other in the First IPCC Report. The issue is that you have smoothed the last two (last 2000 years and last 200 years) and the others. The plots miss periods out, so you show 2000 to 300 years ago and then from 1800 to 2015. If you plotted these in panels, you could show annual variations from 1800-2015 with a greater clarity than the few cm it currently has. Another issue is that you're missing out the years from 1700 to 1800 and earlier periods. Also the LIA seems longer than the other named periods, mostly due to the scale issue, but you've combined the LIA from the 2000-300 period with that from the 1800-2015 period. [Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Figure no longer used for FAQ
9132	188	7	188	18	In FAQ 2.1, Fig 1, the Mid-Holocene (Minoan Warming) and Medieval Warming Period temperatures were greater than now, as in comments 1 and 22 above. [Jim O'Brien, Ireland]	Noted - Agreed that temperatures in some regions were higher than now during the mid-Holocene and medieval warm period. The focus of the FAQ is on global mean surface temperature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6786	188	11	188	13	I find the most recent part of this figure confusing. It suggests there is an observational gap between instrumental records and reconstructions, which is not the case. People may understand that the period 1800-1850 remains unquantified.... Suggest using un-interrupted horizontal axis for the past 2000 years, separating reconstructed and instrumental temperatures using different colours or a vertical bar. [Raphael Neukom, Switzerland]	Taken into account - Figure no longer used for FAQ
42804	189	1	189	2	The shape of ice sheet need to be shown in a more visible way at the left low corner [Xiao Cunde, China]	Taken into account - figure revised.
8256	189	1	189	9	suggestion is to add the observed changes of various variables for the last 50 (or 100) years in this figure and summarized it. [Zong Ci Zhao, China]	Taken into account - combined with comment 8256.
42922	189	1	189	9	The caption of this figure presupposes the global warming hypothesis, but this is explicitly not presented in Chapter 2. Either change caption to be in line with the chapter development (e.g. as described in section 2.1. p 12, l. 3-16: "synthesis of significant changes observed in the climate system", possibly keyed to AR6 standard likelihood descriptors: virtually certain --> more likely than not?), or else introduce the gross predicted changes under the warming hypothesis, and the null hypothesis of unforced variation explicitly within the introduction (with reference to Chs 1 and 3 for full presentation). [Michael Evans, United States of America]	Taken into account - text revised (caption now specifies that the figure depicts significant observed changes in the climate system since the late 19th Century).
56242	189	1			Indicate that, over land, near-surface humidity and soil moisture are projected to increase in some regions and decrease in others. Else, indicated increase in surface humidity to be assumed by the reader to be pervasive in all world's regions. [Sonia Seneviratne, Switzerland]	Rejected - outside the scope of Chapter 2, which focuses on observed (vs. projected) changes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42424	2-31	6			<p>Box 2.1, page 2-31 , line 6</p> <p>Chapter 2 should include a section on Ground Surface Temperatures. It seems like all references to past ground temperature reconstructions from geothermal data have been ignored. There is a n extensive body of literature in this area. This is rather unwise as ground surface temperature histories are one of the few (if not the only) indicators of past climatic change that is NOT a proxy, but borehole temperature-depth profiles are collected as direct measurement of subsurface temperature and the ground surface temperature histories are reconstructed from either inversion, or direct extrapolation of the temperature gradient as a function of depth to yield low-resolution surface temperature histories at each site where data exist. There are approximately 1000 climate-useful borehole temperature profiles in the world distributed in all continents except Antarctica. There are a number of borehole temperature based reconstructions since the late 1980s. Most recent publication using both, the inversion and the direct extrapolation is: Pickler*, C., H. Beltrami, and J.C. Mareschal (2016) Laurentide Ice Sheet basal temperatures during the last glacial cycle as inferred from borehole data, <i>Climate of the Past</i>, 12, 1-13, 2016, doi:10.5194/cp-12-1-2016.</p> <p>Pikler et al., 2016 reconstruct Ground surface temperature for the last 100Ka from deep boreholes temperature profiles so that they provide low-frequency, integrated-heat, Deep past</p>	<p>Rejected. Ground surface temperature is not included in the list of key climate variables listed in X-Chapter Box 2.1 (Table 1). Borehole temperatures have been assessed in 2.3.1.1</p>
15540	2-52	48	53	4	<p>2.3.1.3.3: Extratropical jets storm tracks and blocking: The authors should refer to a previous literature which showed that the extratropical jet in Asia-North Pacific sector is shifted to the north before and after the late-1990s. It is necessary to review more details on the change in the jet stream in Asia-North Pacific sector on the low-frequency timescales. Current version too much emphasized the changes in the jet stream in paleo-climate period. [SANG-WOOK YEH, Republic of Korea]</p>	<p>Taken into account. Chapter 2 aims to assess both recent and paleo changes of the jet streams where only global/largest scale features should be considered. A stipulated page limit precludes significant expansion of this paragraph to include more details. Additional regional details can be found in Chapters 8, 10, and 11. More recent literatures have been reviewed and included.</p>
15544	2-84	46	86		<p>The content in the section of 2.4.1.3 (in particular line 21-28 in page 2-86) should be reflected in the section of 2.4.1.2. The increase of CP El Nino event can be considered as the most striking feature of ENSO variability during the instrumental period. [SANG-WOOK YEH, Republic of Korea]</p>	<p>Taken into account. The section has been substantively reordered and shortened with much of the text going to the new technical annex</p>

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15542	2-84	48	84	51	There remains no clear evidence of any significant long-term trend in the east-west SST gradient across the equatorial Pacific Ocean during the instrumental period, with periods of gradients both above and below the long-term average on decadal timescales, associated with a predominance of La Nina or El Nino events respectively ->>There are a number of literature which showed that there is significant long-term trend in the east-west SST gradient across the equatorial Pacific Ocean. Therefore, it is necessary to revise this sentence in more details. [SANG-WOOK YEH, Republic of Korea]	Rejected. Long-term in this context refers to century-scale (rewording has occurred to make this clearer). Studies finding trends are typically assessing shorter time periods, which can be considered as part of decadal variability as discussed at lines 49-51.
15546	2-86	4	86	5	Banholzer & Donner (2014) found that CP events had a weaker influence on global mean surface temperature than did EP events ->>It is ambiguous for CP events to weakly influence on global mean surface temperature compared to EP events. It is more correct to say that the influence of CP and EP events on global mean surface temperature is different. [SANG-WOOK YEH, Republic of Korea]	Rejected. In this context GMST is a single value of a global average, so there is no ambiguity to this statement. Whether CP and EP events have different regional impacts is a separate issue not discussed here.
15548	2-86	51	87	12	2.4.1.5 (Recent events and their implications for longer-term trends) should be properly added in the content of the section of 2.4.1.2. Overall, it is necessary to re-organize the section of 2.4.1 of ENSO. [SANG-WOOK YEH, Republic of Korea]	Taken into account. The section has been substantively restructured
15550	2-90	1	90	34	2.4.4: Pacific Decadal variability : The section of 2.4.4 simply described the definition of PDO and its difference with IPO along with its trend. I think more relevant arguments associated with the PDV (PDO/IPO) (for example, its origin, dynamics and impact) should be discussed. This section is too weak to emphasize the importance of PDV which significantly influences climate variability in the globe. [SANG-WOOK YEH, Republic of Korea]	Rejected. Assessment of PDV dynamics is the purview of chapters 3 and 9, and is not covered here. Our focus is solely on documentation of changes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42426	2-97	20			<p>“change is not limited to the surface”..</p> <p>By this I assume that you mean SAT, but also the surface temperature of the ground has increased for a couple of centuries, depending on the location. The surface temperature of the ground generally follows the changes in SAT (in fact the changes in ground surface energy balance determine the change of the thermal state of the shallow (~1km) ground at short-time scales <~10^5 years). These changes are recorded below the surface of the ground; in most cases these changes appear as increases in temperature of the upper layers of the subsurface as the ground has gained energy in the last few centuries. [We should have a paper out in a few months where we show, that the heat-flux into the ground in the last four decades is almost the same as the heat-flux into the ocean.] [Hugo Beltrami, Canada]</p>	Noted. This detail is too great for a FAQ.
38390	¼4	50	15	2	<p>References for the above: Svensmark 2016: J. Svensmark, M.B. Enghoff, N.J. Shaviv, H. Svensmark: The response of clouds and aerosols to cosmic ray decreases, J. Geophys. Res. : Space Physics 121, 8152-8181, 2018, doi:10.1002/2016JA022689</p> <p>Howard 2014: D. Howard, N. J. Shaviv, H. Svensmark, The solar and Southern Oscillation components in the satellite altimetry data, J. Geophys. Res: Space Physics 120, 2015, doi:10.1002/2014JA020732</p> <p>Shaviv 2008: N.J. Shaviv, Using the oceans as a calorimeter to quantify the solar radiative forcing, J. Geophys. Res 113, A11101, 2008, doi:10.1029/2007JA012989</p> <p>Gordon 2017: H. Gordon et al, Causes and importance of new particle formation in the present-day and preindustrial atmospheres, J. Geophys. Res. Atmospheres 122, 8739-8760, 2017, doi:10.1002/2017JD026844</p> <p>Tomicic 2018: M. Tomicic, M. B. Enghoff, H. Svensmark, Experimental study of H2SO4 aerosol nucleation at high ionization levels, Atmos. Chem. Phys. 18, 5921-5930, 2018</p> <p>Svensmark 2013: H. Svensmark, M.B. Enghoff, J.O.P. Pedersen, Response of cloud condensation nuclei (>50 nm) to changes in ion-nucleation, Physics Letters A 377, 2343-2347, 2013</p> <p>Svensmark 2017: H. Svensmark, M.B. Enghoff, N.J. Shaviv, J. Svensmark, Increased ionization supports growth of aerosols into cloud condensation nuclei, Nature Communications 8:2199, 2017, DOI: 10.1038/s41467-017-02082-2</p> <p>Pierce 2009: J.R. Pierce and P. Adams, Can cosmic rays affect</p>	Reject - part 3 of 3 part comment (38386, 38388 and 38390). See response in Part 1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54768	ES				I was trying to find out which years have been warmest on record recently and couldn't find it. While I agree that ranking individual years is not that robust, statistics like the warmest 5 on record were the recent 5 (climate central - is that correct for the met office?) are quite powerful and i think would be useful to show. if i've missed it, i apologize. [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Following discussion at LAM3 and bearing in mind space constraints we decided that we could not accommodate this request which is information that is readily available from several other sources.
54770	Figure				The rainfall figure 2.16: I am slightly concerned about having large coverage of that figure - I seriously doubt that you can do a reliable trend over high northern latitudes since 1901, for example, When we worked on causes of rainfall change my colleagues determined that prior to 1920 the observed data density is not high enough. I am generally slightly concerned about using infilled datasets here. i would rather use gridded data taht do not infill beyond individual boxes [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. New maps and time series for the assessment of global precipitation were developed in SOD.
54772	Figure				Figure 2.11 is nice. I am less sure about the preceding figure of global temperature from PAGES2k - while its good to show it, we understand NH mean temperature so much better and the reconstructions are more skillful, so I would also show multiple reconstructions of NH mean tepeatures [Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Reject - While NH paleo temperature is known much better than global temperature, the focus of the paleo temperature discussion, and the purpose of Fig. 2.10, is to display global mean annual temperature for all of the periods of interest using the best available reconstruction, especially new information since AR5. Rather than featuring the better-constrained regional reconstructions, the target is global mean temperature because it is fundamental for understanding the energy balance of Earth, even if it's less well constrained than hemispheric temperature. In addition, in CH2 the goal is to balance attention across multiple states of the climate system, not only the most recent.
49498		6	7	7	Tropopause height risen. Could it be more appropriately stated as shrinking of tropopause ? [Anuj Parihar, India]	Rejected. In this context the tropopause is a level, not a layer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
49500		19	9	20	Salty regions becoming more saltier is understood, However, Fresh regions becoming fresher doesnot carry clarity on the fact/ statement. [Anuj Parihar, India]	Rejected. Ocean salinities can increase (become saltier) or decrease (become fresher). Please see Durack et al. 2010, 2012 for detailed explanation.
49496		36		49	In case of GMST, estimated warming from 1850-1900 to 1995-2014, from 1850-1900 to 2009-2018, from 1850-1900 to 2002-2018 and a linear trend fit over the period 1980-2018 is well mentioned while in case of GAST estimated changes for all timescales are given except for 1850-1900 to 2002-2018. Kindly check. [Anuj Parihar, India]	Noted. Some of the periods chosen are for connections with other chapters, as is the provision of GSAT estimates (since GSAT is used by the modelling chapters). As no other chapter has a requirement for a 2002-2018 GSAT estimate one is not provided in this chapter.
40604		51	8	53	When you say "historical records" do you mean "since 1840"? It is not quite clear from the text. Please specify the reference period. [Olga Solomina, Russian Federation]	Accepted. The sentence was reworded, and the commented wording is not anymore included like that.
17936					There was my typo above... how does that fit into Chapter 1 statement about more vegetation and a little bit more albedo now than before? [Branko Grisogono, Croatia]	Context of comment unable to be sufficiently well ascertained to be able to action.
43302					Most of the figures are highly obscured and poorly respresented in chapter. Check: Fig. 2.13 page 158; Fig. 2.32 page 177; Fig. 2.33 page 178; Fig. 2.36 page 181; Fig.2. 37 page 182; Fig. 2.40 page 185; Fig. 2.42 page 187 [Onema Adojoh, United States of America]	This problem of obscuration occurred during the compilation of the FOD, to keep the size of the chapter file to an acceptable level. The actual figures have a better resolution.
27454					The topic addressed by FA2 is important but It would be good if the title is changed to be more attractive [Fatima Driouech, Morocco]	Comment not sufficiently clear to be actionable
49502					All the sections of the chapter are well organized with all necessary detailed description and are well supported by data and graphs. The draft compilaion provides detailed findings of AR5 with respect to the sections covered in this chapter that helps in establishing link with the previous and current presentations of the facts.Overall presentation of the facts in the chapter is excellent. [Anuj Parihar, India]	Noted with thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56744					Figure General chapter 1: ideally, the figure should be a bit more independent from the caption => some crucial information in the caption should be included directly in the figure! // 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 // make the visualisation of the significance of your results consistent (either hatching or stippling (points) or stippling (crosses) // figures should be uncluttered for SOD (e.g. remove unnecessary grids and frames) // some unites are missing in the axis labels [WGI TSU, France]	Noted. Aspects discussed at LAM3 and implemented in the SOD.
56746					Figure 2.2: There is no red band in the figure (a) as indicated in the caption // add all the legend in the plot if enough space (for (a) , $\delta^{13}C$ of phytane, non-parametric LOESS fit are missing) [WGI TSU, France]	Accepted - editorial
56748					Figure 2.3: which plots are (a), (b), (c) ? // low-right plot: what is the black line for CH4? [WGI TSU, France]	Accepted - editorial. (a), (b) and (c) are added and the color of the black line is changed
56750					Figure 2.4: it would be easier to have the legend of colors right in the plots (like in fig. 2.3) // in plot (a), the color is red not orange // red-green is not distinguishable with colorblind vision [WGI TSU, France]	Accepted. Graph is improved.
56752					Figure 2.5: I suggest to use only full colored lines (instead of dotted lines) // like done in previous figures, you can write the chemical name in the corresponding colour and close to the line (here it seems that there is some space on the right side of the plot) // it would be nice to have a bigger time resolution in the x axis from 2000 to 2020, to allow the reader to match better the data trends with recent years. [WGI TSU, France]	Accepted. Figure has full coloured lines, and added ticks on the axis to aid interpretability.
56754					Figure 2.6: adding a schematic of a globe next to each plot with the corresponding zonal bands highlighted would be a big cognitive help for the reader. For more guidance, contact the TSU graphics officer. [WGI TSU, France]	Accepted. Done.
56756					Figure 2.6: The definition of the dotted grey line should be stated at least in plot (a) if not in all of them // adding a schematic of a globe next to each plot with the corresponding zonal bands highlighted would be a big cognitive help for the reader. For more guidance, contact the TSU graphics officer. [WGI TSU, France]	Accepted. The Figure is completely revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56758					Figure 2.7: Colors are not accessible for color blind // x and Y axis on the left panel are missing (latitudes for the Y? just add "" to the numbers and it will become more obvious) // switch downward triangle with circle symbols as it is more intuitive to have sites below 1500m represented by opposite symbol to the one used for >1500 m sites. [WGI TSU, France]	Accepted. Updated figure.
56760					Figure 2.7: Information seems to be similar in both pannels, just visualised in different ways. Information redundancy could be avoided - but this depends on the message(s) covered in the main text for that figure. For more guidance, contact the TSU graphics officer. [WGI TSU, France]	Taken into account. The figure is modified.
56762					Figure 2.8: It is unclear what NSS and "r" in rBC stand for (spell out acronyms in caption at least). Ideally use Black Carbon instead of BC in axis title. [WGI TSU, France]	Accepted. Explanations added.
56764					Figure 2.9: Spell out ERF in axis title, if enough space. // shouldn't the Y axis go beyond -3 to show the full range of data (seems like some bits are cut off)? // legend should be revised to be more comprehensive ("other anthropogenic" forcing?) one option could be to list "type of radiative forcing: CO2, CH4, N2O...Volcanic, Solar etc...) [WGI TSU, France]	Accepted. This was changed. Since the "other anthropogenic" is clarified in the caption, we kept the simpler version in the Figure.
56766					Figure 2.10: legends directly in the plots are missing. We should be able to understand the main components of the figure without checking the caption. Add what the blue line is (b), black and orange lines (c), red lines (d) // "bands" in the caption but not in (d). // unclear what CE stands for in the x axis [WGI TSU, France]	Accepted. Revised as suggested.
56768					Figure 2.12: the title and subtitle of the figure are a great way to understand straight away the topic of the figure - something to think for the other set of figures // make it clearer that the annual average is related to the 1st plot and the decadal average is related to the second plot (split secondary title might be an option). [WGI TSU, France]	Taken into account. The figure has been redesigned and is now incorporated into a larger figure.
56770					Figure 2.13: remove grids it not necessary // Note that for a non-expert (top right panel), it is more intuitive to have the latitudes as Y axis as this is mainly how they are visually represented in maps. // the figure needs a better flow in terms of how the information is being displayed. For more guidance, contact the TSU graphics officer. [WGI TSU, France]	Taken into account. Figure redesigned
56772					Figure 2.14: Ideally spell out % Relative Humidity [WGI TSU, France]	Accepted. The figure caption was corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56774					Figure 2.16: not stated what the stippling are representing // a.b.c not stated in the figure // unsure what the variable is: either write a general title on top first map (e.g. observed precipitation change over decades) or below the color bar [WGI TSU, France]	Taken into account. We homogenised several features of the figures, like axis labels, colorbars and extra-information. We improved the quality of the figures and its description.
25544					This is a general comment concerning interglacial periods during the last million of years, a subject which seems to be quite absent as such in the FOD of this chapter. A deep review paper on the subject is post AR5 and certainly would merit to be mentioned in this chapter: Past Interglacials Working Group of PAGES (2016), Interglacials of the last 800,000 years, Rev. Geophys., 54, 162–219, doi:10.1002/2015RG000482. It is the best synthesis you can get about all the interglacial periods during the last 800 kyr. On the whole, all these interglacials are different from each other, and you can find in this review a state of the art concerning the relative role of insolation, CO2, ice extent on the strength or the duration of these interglacials. [Dominique Raynaud, France]	Taken into account. Given the space constraints we are under we need to concentrate upon the standard PMIP windows in support of other chapters of the assessment.
56776					Figure 2.17: It would be nice to add the information in the caption concerning towards blue=moisture and towards red=dryness directly in the figure as well. // a title in the figure would help understanding what it is all about (similar to fig. 2.12) [WGI TSU, France]	Taken into account. A way to homogenise additional features for the figures, like axis labels, colourbars and extra-information has been implemented.
56778					Figure 2.18: if possible, spell out NH and SH. [WGI TSU, France]	Editorial
56780					Figure 2.19: a title in the figure would help understanding what it is all about (similar to fig. 2.12) [WGI TSU, France]	Accepted. Figure is re-plotted and the title has been modified.
32206					Hoogakker et al., 2018b the same as Hoogakker et al., 2018a: change section 2.3.4.2 and references. [Isabel Trigo, Portugal]	Noted. The reference was amended accordingly.
56782					Figure 2.20: in the figure indicate that the legend concerning 1004/1020.5 hPa is about the location of the decadal mean and the color bar is changes in SLP. // spell out SLP in figure/caption (sea level pressure) [WGI TSU, France]	Not applicable. This figure is no longer included in Chapter 2.
32208					General comment: I recognize the great improvement made in the current version of this chapter, with somehow more homogeneous sections and subsections. It is also clear the effort made towards completeness. The achievement towards a more coherent chapter is more difficult, given the large number of datasets and of topics covered. I would insit on this point as a goal for the next version of the chapter, particularly for the introduction and executive summary. [Isabel Trigo, Portugal]	Taken into account. We agree with this assessment and have made further efforts to this end in the SOD preparation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56784					Figure 2.25: Year CE is not a familiar term, spelling out CE might help understand the concept // what are the units of the advances and length? [WGI TSU, France]	Accepted. The figure is modified and "Year" is used as axis label.
56786					Figure 2.26: legend of color/symbol is missing in the figure [WGI TSU, France]	Editorial (Figure revised).
56788					Figure 2.29: it is not clear what are the variables of each plot (no title on the color bar), and therefore what is the figure about. // color bar is not consistent with the temperature palette in the IPCC Visual Style Guide [WGI TSU, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
56790					Figure 2.30: figure should be more independent from caption => spell out acronyms (OHC) // year labels should appear on top of plot not bottom. [WGI TSU, France]	Noted. Text and figures for Section 2.3.1.1 have been restructured
56792					Figure 2.31: "In all panels blue denotes freshening regions and red denotes regions with enhanced salinities." such information could appear as short annotation in the figure // What does the white stand for? [WGI TSU, France]	Noted. Figure and caption revised
56794					Figure 2.32: the right hand panel should indicate visually which part of the left panel it is a zoom of [WGI TSU, France]	Editorial.
35804					Is calibrated uncertainty language applied consistently across paleo observations and instrumental observations? For example Pg 6, In 26 'During the last inter-glacial, GMST averaged over the warmest millenium was 1 +/- 0.5 C warmer than pre-industrial (medium confidence), and very likely did not exceed 2C higher.', or pg 6, In 30-32, 'the Little Ice Age, which was globally the coldest multi-century interval of the Holocene (high confidence).', or pg 58, I19, which indicates there is high confidence 'in millenial-scale estimates of relative changes of pan-Arctic sea-ice coverage through the past 13000 years'. Compare for example with confidence in instrumental observations such as 'Global land precipitation has likely increased since the beginning of the 20th century (medium confidence)' or 'Snow thickness on sea ice has also decreased in the western and central Arctic (medium confidence).' I am not suggesting these assessments are necessarily wrong, but just encouraging the authors to ensure there is consistency across assessments of paleo and instrumental changes. [Nathan Gillett, Canada]	Taken into account. There were inevitable heterogeneities in use of confidence and likelihood language which we have tried to equalise in the SOD. These were not just between paleo and instrumental but also across sections.
56796					Figure 2.33: this figure should be uncluttered - many visual elements that are not necessary // why is the Y-axis on the right hand? [WGI TSU, France]	Accepted. Figure redrafted
56798					Figure 2.35: legend of colors is missing in the figure [WGI TSU, France]	Accepted, missing information was added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
56800					Figure 2.37: unclear what grey/red color represents (not mentioned in caption neither) // this figure cannot be understood without referring back to the main text. Adding some more detail in the caption could help (e.g. plant ecosystem)? [WGI TSU, France]	Take into account - text modified.
56802					Figure 2.38: white might not be the best color to represent not statistically significance (white is often being used for "lack of data" as in figure 2.19 [WGI TSU, France]	Taken into account - figure revised.
56804					Figure 2.40: the information and how the elements relate to each other is not very clear. For more guidance, contact the TSU graphics officer. [WGI TSU, France]	We discussed this issue with TSU at LAM3 to clarify the issues and the feedback has helped to guide the revisions undertaken to this figure.
56806					Figure General comments Chapter 2: ideally, the figure should be a bit more independent from the caption => some crucial information in the caption should be included directly in the figure! // 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 // make the visualisation of the significance of your results consistent (either hatching or stippling (points) or stippling (crosses) // figures should be uncluttered for SOD (e.g. remove unnecessary grids and frames) // some units are missing in the axis labels. 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. Further nuancing of figures has occurred which has both reduced the number of figures and improved their consistency with the style guide.
35582					The Datwyler et al. (2017) paper included in the references list is a SAM reconstruction paper which doesn't seem relevant here. [Nathan Gillett, Canada]	Context of comment unable to be sufficiently well ascertained to be able to action.