

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
130471	0	0	0	0	The uncertainties of the linear trends used in the past are always at 10-90% level, but it is changed to the 5-95% significant level in some chapters (some are still at 10-90% level) in AR6. Can we unify the usage in AR6? [ Panmao Zhai, China]	Accepted. We now follow guidance from TSU in this respect which was clarified after SOD submission. We now consistently use the very likely range and calculate it correctly as 1.645* sigma.
57779	0	0	0	0	Since this chapter reviews the numerous observational records, I wonder if it may also be useful to have a section near the beginning of this chapter dedicated to describing some of these techniques e.g. satellite remote sensing, sediment coring etc. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. As charged this is the scope of chapter 1 and not chapter 2. We have worked with chapter 1 to improve this aspect and better cross-link in the FGD.
67659	0	1	200	1	The impact of large fires on the stratosphere appears to be missing. Again, in recent years there have been two large events, on in 2017 (Pacific NW fires where smoke persisted for 9 months) and the recent Dec 2019 Australia pyro Cb, whose smoke is still making the rounds of the SH stratosphere. Papers are just starting to come out about the 2019/2020 event. There are possible radiative impacts, and also ozone chemistry impacts that are unknown at this time. [ Karen Rosenlof, United States of America]	Taken into account. The topic is Taken up in Chapter 5, though, not in Ch. 2.
17367	0		192		Arid and semi-arid area in West Asia and Central Asia are important locations as global view on climate change issue as well as drought, desertification and dust storm phenomena, evaluation of its changes could be helpful in sense of climate system, but there is no any citation for West Asia and few references for Central Asia, which great changes in recent years has taken place. [ Mostafa Jafari, Iran]	Rejected. This is the focus of regional chapters and consideration of regional changes was avoided to avoid overlap with them.
107023	0				Please use consistent acronyms for the modes, for instance AMV and not AMV/O, etc., as defined in the Technical Annex and used later in Chap3 and Chap 4. [ Christophe CASSOU, France]	Noted. The final report will include an annex with acronyms, to aid readability.
132125	0				I have a major concern with the choice of observations realms covered by Chapter 2, which do not include "Land". This is inconsistent with the recent publication of an IPCC report on "Climate Change and Land". In addition, chapter 1 clearly outlines that the Earth System can be subdivided in 5 realms: Atmosphere, Ocean, Cryosphere, Biosphere and Land (see from page 10, line 55 to page 11, line 1, and Section 1.5.1.1). The Biosphere cannot be considered equivalent to Land. A substantial fraction of the land area is not covered by vegetation, and there is also biosphere in the ocean. Variables that could be covered under "Land" include soil moisture, runoff, lakes, soil heat storage, land surface temperatures, and albedo. All of these variables are observed. [ Sonia Seneviratne, Switzerland]	Taken into account. The chapter in FGD better stresses terrestrial aspects, explicitly pointing to 'terrestrial' in e.g. 'permafrost' and 'ecosystems' sections, but without calling out land as its own over-arching theme.
547	0				General comments on Chapter 2: The basic structure of giving the AR5 and other earlier results at the start of the individual sections and then proceeding with the new results is excellent. The text overall is quite dense with valuable information but certainly not an easy read and not appropriate for general readership. There is too much use of acronyms, making for tough reading. In sharp contrast, the two FAQ pages (pp. 2-95 and 2-96) are overwhelmingly more readable than the rest of the chapter. The FAQ pages are very nicely done. [ Claire Parkinson, United States of America]	Noted. This is largely inevitable given the differing target audiences of these distinct sessions.
549	0				The above review comments are from Claire Parkinson/NASA Goddard Space Flight Center. [The emailed instructions said that anonymous comments will not be accepted, but I didn't see where I was supposed to put my name. Perhaps in column A, but I'm not sure.] [ Claire Parkinson, United States of America]	See response to 107023
132141	0				It is essential that Chapter 2 addresses clearly the question of the "global average temperature" definition and how this affects assessments. The ES should mention that different estimates were used in the past, and I suggest to introduce a new acronym distinct from either GMST and GSAT to refer to the concept "global average temperature" which is the term used in the Paris Agreement (e.g. Tglob or global warming (GW)). [ Sonia Seneviratne, Switzerland]	Taken into account in revisions applied to CCB2.3

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36929	0				For ALL climate factors discussed in this chapter, what are the error margins and what is the coverage? Are they all as uncertain as the HadCRUT4 historical temperature record that (a) did not exceed 50% global coverage until 1904, (b) did not consistently exceed 50% coverage of the southern hemisphere until 1949, (c) relied on a single weather station for Southern Hemisphere data from January 1850 to June 1852 and (d) even today (Apr 2020) has a Caribbean weather station (Golden Rock Airport) reporting a mean monthly temperature of 0.0C in December 1981 and December 1984? [ John McLean, Australia]	Taken into account. Issues of coverage and uncertainty estimation are evaluated throughout the chapter. Chapter 1 provides the primer on evolving observational capabilities.
132163	0				Given the issues with the definition of global warming (depending on definitional choices), and the fact that this depends on the considered warming over the different components (ocean, land, sea ice), it is essential that Chapter 2 gives a key priority to the separate assessment of: 1) global warming over the oceans (without sea ice points), 2) global warming over the oceans in areas covered by sea ice at some point in pre-industrial time), 3) global warming over land points (without snow or ice-covered areas), 4) global warming over land points that had snow or ice at some point in the pre-industrial period) and 5) the combination of these statistics in different forms: a) Global warming total, b) global warming over land, c) global warming over the oceans. Being more explicit about the contributions of different Earth Systems realms to the overall warming will help clarify many misunderstandings. [ Sonia Seneviratne, Switzerland]	Taken into account. The impact of changes in sea ice coverage on global temperature assessments is covered in the revised Cross-Chapter Box 2.3. Land and ocean changes are reported separately. The areas affected by retreat of the cryosphere are (to date) small in a global context and hence unlikely to be a significant contributor to GMST changes.
36933	0				IPCC authors don't seem to know the difference between "global mean surface temperature" (GMST) and "global mean surface temperature anomaly" nor the difference between "global surface air temperature" (GSAT) and "global surface air temperature anomaly". There is no such things as GMST or GSAT because temperature is not measured everywhere on earth. The averages are of anomalies, which in the case of GISS and HadCRUT4 data are first calculated as average anomalies of each grid cell. [ John McLean, Australia]	Noted. No specific action requested. However, the CCB2.3 has been completely revised which may allay the reviewer concerns or at the very least significantly clarifies the assessment.
36955	0				<p>The first IPCC climate assessment report introduced the concept of Global Warming Potential (GWP) on pages 54 to 56. Among the 'particular problems associated with evaluating the GWP' was '[t]he dependence of the radiative forcing of a gas on its concentration and the concentration of other gases with spectrally overlapping absorption bands'.</p> <p>Every IPCC report since the first has largely ignored that very important qualification and pretended - there is no other appropriate word - that water vapour did not exist, was not 15000ppm compared to the minute quantities of other GHGs and that the bandwidths over which water vapour absorbs and scatters largely overlaps with carbon dioxide, methane and nitrous oxide (CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O) to name just a few.</p> <p>IPCC 2AR (1995) says (pg 60) 'the carbon dioxide absorption is saturated over part of the spectral region where it absorbs', which seems to indicate that CO<sub>2</sub> has negligible effect. Also, IPCC TAR (2001) says on page 145, in a discussion of the radiative forcing of N<sub>2</sub>O, '[t]his RF is affected by atmospheric CH<sub>4</sub> levels due to overlapping absorptions.' These were the only instances that I could find that even slightly talk about bandwidths and overlapping absorption.</p> <p>Not only does this mean that the GWP of GHG's other than water vapour are determined using fantasy situations that simply do not occur in the atmosphere, it also means that the warming these other GHGs cause is negligible and there is no good reason why any gas other than CO<sub>2</sub>, which has a small bandwidth that does not overlaps with H<sub>2</sub>O, should ever be mentioned in an IPCC report.</p> <p>A reference that shows this simply is  <a href="https://commons.wikimedia.org/wiki/File:Atmospheric_Transmission.png">https://commons.wikimedia.org/wiki/File:Atmospheric_Transmission.png</a> Why has this figure or one like it not appeared in IPCC reports? It should be presented and discussed honestly in at least one chapter and cross-referenced from other chapters.</p> <p>Also, remove all discussion of GHGs whose action is negligible either by being present in such small</p>	Rejected. There is no robust basis provided to support the requested actions here. The assessment text makes numerous references to relevant literature to support the assessment undertaken.

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93045	0				Great to see that records of past change are included throughout this chapter. [ Bette Otto-Bliesner, United States of America]	Noted.
65655	0				Suggest presenting the information from multiple time scales in a more coherent way. In this chapter the 'narrative' switches between different time scales too often. Suggest presenting historical data (temperature history, Mauna Loa GHGs, etc.) first, and then 'zooming out' to the longer paleoclimate time scales out to hundreds of millions of years. [ Kushla Munro, Australia]	Rejected. We prefer to retain the structure whereby per indicator the evidence is assessed from as deep past as is possible forwards. This minimises the potential repetition and enables a consistent assessment. This structure was implemented starting from the beginning of the process in AR6.
93051	0				Further assessment of the LIG is needed. See additional comments in specific sections. [ Bette Otto-Bliesner, United States of America]	Noted.
36999	0				This chapter cites various reconstructions and reanalyses but gives no reason why any of them should be regarded as more accurate than the data they replace or process. [ John McLean, Australia]	Rejected. Reconstructions and reanalyses are used where and when appropriate and are never used to replace observed data.
42901	0				There's a lot of weird capitalisation throughout Cryosphere, Northern Hemisphere are two examples. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Addressed in copy edits.
130469	0				When assesing observed key large scale changes in the climate system, please not forget to sufficiently cover atmospheric changes. [ Panmao Zhai, China]	Noted. Atmospheric changes constitute the largest single segment of the chapter.
132321	0				On the Earth System's realms considered in chapter 2, and the fact that they do not include Land: Note that the GCOS Essential climate variables (ECVs) clearly highlight a list of Land variables, along with ocean and atmosphere variables ( <a href="https://www.ncdc.noaa.gov/gosic/gcos-essential-climate-variable-ecv-data-access-matrix">https://www.ncdc.noaa.gov/gosic/gcos-essential-climate-variable-ecv-data-access-matrix</a> ). It would seem very strange for the IPCC WG1 report to be inconsistent with the structure followed by the GCOS ECVs. [ Sonia Seneviratne, Switzerland]	See response to 132125
81287	1	1	1	1	Firstly, I congratulate the author team on the work they have carried out since the last draft, which has improved many parts of the chapter considerably. As a general comment, I find that more coordination is needed with other chapters to ensure a more consistent approach and message. Secondly, for synthetic GHGs a clear ERF criterion for including a species needs to be defined, as currently some of the species included have lower ERFs than some that are not (see specific comments). [ Johannes Laube, Germany]	Accepted. We have introduced all halogenated components with a ERF>0.001 W m <sup>-2</sup> in the table now, while a more complete overview of all observed components is given in Annex V. A few additional components are mentioned in the text, which warrant discussions, as their abundance is growing rapidly.
81289	1	1	1	1	Thirdly, I do embrace the idea that the chapter is meant to “perform an assessment of the observational evidence for large scale changes in the climate system across all of its components” – but the sections on the BDC and the QBO have been removed completely. I find this very strange, especially since this the stratosphere entails 15 – 20 % of the mass of the atmosphere, and substantially influences its radiative budget. In addition, much recent research has focused on both BDC and QBO. Is the global stratospheric overturning circulation no longer considered a “driver of climate change”? Is the QBO less important than SSWs? Many scientists would strongly disagree. [ Johannes Laube, Germany]	Rejected. QBO is no longer assessed in the chapter. We did undertake substantive discussions within and across chapters and decided to stick with the decision to omit the QBO.
113087	1	1	1	1	Nothing in this title implies 'observations' despite the multitude of references to this chapter as being about 'observations'. I think the general structure of the report is already not very intuitive, so try to help the reader by making these titles more explicit. The current title does not even indicate the chapter refers to past-to-present time. [ Diego Miralles, Belgium]	Rejected. Title cannot be changed from that scoped by the parties
4003	1	1	93	34	A major concern of mine for this chapter would be the possible overlap of the contents with those of other chapters of WG1 especially WG2, and the 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. There is an 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. [ Guoyu Ren, China]	Noted. The chapters and reports were scoped and agreed by the parties. It is not in the gift of the authors to change this.
77179	1	1	192	1	This is a well constructed and clearly written chapter. Some uses of scientific terms could be reduced to assist the non-specialist reader [ Emer Griffin, Ireland]	Noted with thanks.

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1221	1	1	192	1	The general text of this report is convoluted and littered with a number of acronyms that are certain to be unfamiliar for most readers. Those who already know them probably also knows the message that the report tries to convey. I think the report can be improved greatly by letting external communication experts read it and rephrase awkward sentences. The American Fourth National Climate Assessment is an example to follow. A report full of cryptic sentences is not much value and can have the unfortunate effect in making the division between climate scientists and the general public wider because the two groups use very different languages. This is a great concern these days with the emergence of political populism and an apparent anti-science movement. Also, there are relevant studies that this draft has neglected (I have several publications that are relevant and should be cited, and it is likely that here also are many others) - without these, it would give an incomplete picture of the status of our knowledge. [ Rasmus Benestad, Norway]	Editorial. The final report will include an annex with acronyms, to aid readability.
69793	1	1	192	8	SLCF is a new chapter in AR6 and would like to point out that in Chapter 2 the following usage is encountered. Word (usage) - SLCF (3); Short lived climate forcers (3) aerosols (90). [ Bhupesh Adhikary, Nepal]	Noted, unclear what is being requested so no changes made.
10401	1	1			I strongly discourage the use of "Medieval Warm Period" or "MWP" throughout this chapter. It is an inaccurate term, and puts in the readers mind that it was warm uniformly over some ill defined period. The previous IPCC report used "Medieval climate anomaly", so it is a retrograde step to go back to using MWP. The word "Medieval" refers to a historical period, 5th to 15th century, in mostly western Europe. To use the word in a term to associate with climate in other parts of the world is a rather old fashioned way of doing things, not fit for the 21st century. The "MWP" term is ambiguous, Chapter 9 uses it to refer to "Meltwater pulse". The references that point to some climate change during the "MWP" are not all referring to the same period that is defined by the IPCC. It worries me that disparate sources of information are being used to support the idea of a coherent warm period, when they actually refer to different periods within the same 400 or so year period. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The MWP is no longer referred to within the chapter or the report.
10405	1	1			Strongly recommend not using term "Little Ice Age". It was far from being as cool as an "Ice Age", the period had some warm summers (e.g. in CET), and originally was a term describing Holocene expansion of glaciers in West north America (Lockwood et al, "Frost fairs, sunspots and the Little Ice Age", Astronomy and Geophysics, 2017). The period covers the period 1750, which is being used elsewhere in this report as "pre-industrial", which means there is a conflict in definitions that needs resolving. Climate may not have been coherently cool in this period (Neukom et al., "No evidence for globally coherent warm and cold periods over the preindustrial Common Era", Nature 2019) [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The term LIA is no longer referred to within the chapter or report.
41541	1	16	1	16	Michael Byrne (UK / Ireland) [ Michael Byrne, United Kingdom (of Great Britain and Northern Ireland)]	Comment not present.
96207	1	192			Please check for double names for unit 'Myr' and 'Ma' standing for 'million years' and decide for one unit. Since 'Myr' and 'Ma' are quite unusual units in non-climate science it may be worth to write the full name 'million years' instead of abbreviation. [ Nicole Wilke, Germany]	Editorial. Consistent with style guide.
81189	2	6	2	6	"latter" may be replaced by 'later' [ Supriyo Chakraborty, India]	Editorial. Addressed in copy edits.
18387	2	6	2	7	"...since the cessation of the Little Ice Age (late 1700s to mid 1800s) - the dates are not consistent with other chapters (Cross-Chapter Box 1.3: Paleo standard / reference) [ Olga Solomina, Russian Federation]	Accepted. Little Ice Age is no longer used within the chapter.
18389	2	7	2	8	"The number retreating is highly anomalous in the context of the last 2000 years (high confidence)" - "glaciers"are missing [ Olga Solomina, Russian Federation]	Editorial, context is set clearly in the previous sentence.
90899	2	9	2	9	Inconsistent use of capital letters Greenhouse gases cf line 19 [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.

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90901	2	10	2	10	Inconsistent use of capital letters ka Ma [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Consistent with style guide.
112381	2	10	2	10	Deep past means different time span for different people. How about the word "Sedimentary Archive" instead? - CO2 in the Sedimentary Archives [ Feng Ran, United States of America]	Rejected; the time period of "deep past" is specified (500 Ma to 800 ka). The term "sedimentary archives" is not well understood by non specialists
112383	2	11	2	11	Glacial-interglacial cycle shows in many past periods from Late Paleozoic to Quaternary. The uniqueness of 800 ka onward is the availability of ice core record. Maybe change to "Glacial-interglacial WMGHG fluctuation from ice core record". [ Feng Ran, United States of America]	Rejected; the time period is stated (800 ka). This section is mainly evidence from ice cores, but does include some non-ice-core information
90903	2	16	2	16	dioxide - lower case [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30065	2	20			'chlorofluorocarbons' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
81291	2	21	2	22	"HCFCs" is not defined yet. In addition, a list of acronyms would help, especially since other chapters already have one. The 6 in SF6 should be subscript. [ Johannes Laube, Germany]	Editorial. The final report will include an annex with acronyms, to aid readability.
130473	2	23	2	23	The Section D title is "Climate information to support mitigation and adaptation action" but key messages are mainly for mitigation. Please consider to include more adaptation points. [ Panmao Zhai, China]	Not applicable. Misplaced comment. This is for SPM-38 line 21. The structure of the SPM has been revised. The 3rd section now focuses on information for adaptation, while the 4th section is about mitigation.
58103	2	23	6	23	It is stated in the document that "GMSL is now higher than at least the last 6000 years and likely since the last interglacial". But many studies has shown that the GMSL during last interglacial was much higher than now. For example: Studies by O'Leary et al., 2013 and Dutton et al., 2015 shows that Global sea level reached up to a maximum of 6 to 9 meters when compared with the present and mentions that it would have resulted from Greenland Ice sheet melting and partly from the west Antarctic ice sheet melt. 1. O'Leary, M.J., P. J. Hearty, W.G. Thompson, M.E. Raymo, J.X. Mitrovica and J.M. Webster (2013) Ice sheet collapse following a prolonged period of stable sea level during the last interglacial. Nature Geoscience, Ol: 10.1038/NGEO1890 2. A. Dutton, A. E. Carlson, A. J. Long, G. A. Milne, P. U. Clark, R. De Conto, B. P. Horton, S. Rahmstorf, and M. E. Raymo (2015) Sea-level rise due to polar ice-sheet mass loss during past warm periods, Science, DOI: 10.1126/science.aaa4019. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; statement about GMSL during the mid-Holocene has been extensively revised.
112385	2	34	2	34	Here deep past is used to refer to Cenozoic. Whereas in line 10, the word is used to refer to 500 Ma to 800 ka. Maybe avoid the word "deep past" completely? [ Feng Ran, United States of America]	Taken into account; changed "Cenozoic" to "65 Ma to 8 ka" to be specific.
112387	2	35	2	35	"Post Glacial" is a bit too broad for this section. Readers might appreciate a bit more specifics for the title. [ Feng Ran, United States of America]	Taken into account; added "past 8000 years" to be specific.
109277	2	47	2	47	"Changes in both atmospheric composition from atmospheric chemistry, and land-use change..." is grammatically confusing. Do you mean: (a) "Changes in atmospheric composition, caused by both atmospheric chemistry and land-use change," or do you mean (b) "Changes in both atmospheric composition and atmospheric chemistry, as well as land-use change," or do you mean (c) "Changes in both atmospheric composition [or atmospheric chemistry] and land use"? [ Paul Edwards, United States of America]	Taken into account. The text has been revised to now split SLCF and land-use into two separate ES statements.
90905	2	48	2	48	Evaporation lower case [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
81293	2	48	2	48	Why is evaporation starting with a capital letter? If it is being used as a name here, then giving the "P-E" abbreviation would be recommendable. [ Johannes Laube, Germany]	Editorial; copyedit to be completed prior to publication.
7321	3	1	3	9	Hindukus Himalayan ice sheet information should be included under 2.3.2. [ SAN WIN, Myanmar]	Rejected. This is not included since this is a regional aspect and Chapter 2 is global in scope.
90907	3	3	3	4	Sea ice - inconsistent use of capital letters [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
407	3	20	3	20	I guess the whole WGI report is going to use de-oxygenation, with hifen. [ Leticia Cotrim da Cunha, Brazil]	Editorial; copyedit to be completed prior to publication.
90909	3	33	3	33	Case modes [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.

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100529	4	1	4	1	I hope that most of the data in this chapter will be updated to 2020 values, once the report is finished. [ Peter Lemke, Germany]	Taken into account. Most data is updated through 2019. For the specific case of GMST/GSAT the update is through 2020. For most other parameters data latencies and/or implications for completion of downstream chapters preclude an update beyond 2019.
86687	4	1	7	2	Findings from paragraph 2.2.7 deserve mention, namely that whereas land use and land use change has contributed significantly to overall GHG increase, it has also led to increase in albedo cooling, and due to these two opposing effects the overall effects of historical land use/land cover change on global warming are not clear (according to para 2.2.7.). This indicates that land use/land cover tend to have a equivocal effect on climate (also in the future), and that we shouldnt rely too much on land use/land cover change in a mitigation strategy. [ Oyvind Christophersen, Norway]	Taken into account - text revised.
86703	4	1	7	2	General comment on the Executive Summary. This text appears not to be written with the reader in mind. Many sentences are long, complicated and needs to be read many times to be properly understood. Please consider to use shorter sentences and less information in each sentence. Also please use simpler and less abstract words, whenever possible. Decide what the message of the statement is, and then present it in a clear and direct way. [ Oyvind Christophersen, Norway]	Taken into account. Efforts have been made to simplify the text wherever possible.
105075	4	1	7	3	In this executive summary, the reference periods are not homogeneous for the different components of the climate system. The statements refer to as little as a century for GSAT/GMST to six millenia for the sea level. As a result, the word "proxy" is not used very clearly (see my previous comment). It would be good to give a little more context to the statements by including the discussion of possible states, as described by paleodata. This is all the more surprising that in the core of the chapter, there are a lot of long-term paleoclimatic references. [ Masa KAGEYAMA, France]	Taken into account. Periods are driven by data availability so are only possible to be homogenised to a limited extent.
1211	4	1	7	3	The English of the executive summary is not very elegant and the way the executive is written makes it unaccessible to most readers. It's written in a "IPCC speak" dialect that makes it harder when it comes to explaining to the public and decision-makers what the message actually is. The sentences are so convoluted that the reader is likely to have forgotten the first words of a sentence by the time they read its end. My impression can be tested by letting people who wrote the other chapters (or non-experts) read the executive summary of other chapters and then quiz them on the findings. It is a bad tradition within the IPCC writing in such a convoluted style and makes the report less influential. I suggest getting help from communications experts writing the executive summary. Please avoid uncommon abbreviations if you want to reach a broader readership. It could help using some infographics. It is also difficult to say which message is the most important one, given so many key messages. Perhaps drop some of them? Are there some points that can be dropped in the executive summary? Are some of them repetitions of messages from previous reports? Perhaps drop the points where there is low confidence or even medium confidence? [ Rasmus Benestad, Norway]	See response to 86703
114721	4	3	4	12	This intro is useful and works well [ Jan Fuglestad, Norway]	Noted with thanks

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17069	4	3	4	12	I suggest these changes: This chapter assesses observed large-scale changes in climate system drivers, key climate indicators, and principal modes of climate variability. Chapters 3 and 4 respectively consider model performance / attribution of current conditions and future projections in a subset of these same various indicators and modes of variability. Collectively these chapters provide the basis for latter chapters which focus upon on global and regional climate processes and changes regional change. Within Chapter 2, changes are assessed from in-situ and remotely sensed data, its derivative data, and products derived therefrom; and indirect evidence of longer-term changes evidences based on upon a diverse range of climate proxies. Assessable periods Periods able to be assessed are directly dictated by the time-evolving availability of observations and proxy information (cross-chapter boxes 1.2 and 2.1). Wherever possible, recent changes' significances are assessed for significance in a longer-term context, both in terms of mean state and change rates of change. Where stated, ranges represent the assessed very most likely range (5-95%) range. Unless otherwise specified, conclusions support relevant AR5, SR1.5, SROCC and SRCL findings. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Addressed in ES edits.
73293	4	3	96	55	Overall, this Chapter is well written and flows well. My major concern is the citation of material 'submitted'. Normally, journals will not accept such citations, although those accepted may be cited as such ('accepted' or 'in press'). Presumably there will be a check on whether or not these papers have been accepted There are several other persistent editorial issues. 'Century' should be capitalised when it is used as a proper noun (e.g. '20th Century'). This is done inconsistently: I have not flagged all the instances of this in the text. Throughout the Chapter, by and large, British spellings are used. The exception to this is the use of 'paleo' as a single word or a suffix. This is incongruous in the context of other spellings, and ideally should be changed to 'palaeo'. Again, I have not flagged these instances, but a global search/replace could be applied. It would be helpful to have an acronym/abbreviation list so all the terms used are available in one place. I found keeping track of acronyms (many) pages after they were first introduced hard and, in some cases, I am not sure a definition was ever given. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Use of submitted literature is per guidelines in SOD and the FGD only uses papers accepted for publication by the cut-off. Spelling of paleo is consistent with prior ARs. Capitalisation follows the style guide. Acronym use has been reviewed and where not necessary replaced. An appendix with acronyms has been added.
71857	4	4	4	6	Delete these two sentences - not relevant to this chapter. [ John Church, Australia]	Rejected. This is necessary to orient the reader to the context of the chapter and where to search for additional information.
19681	4	4	4	6	Please remember this is a summary of chapter 2. Refrain from summarizing other chapters and save room! [ philippe waldteufel, France]	See response to comment ID 71857
73295	4	6	4	6	Replace 'latter' with 'later' (better English). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Addressed in ES edits.
78827	4	6	4	6	Odd formulation, or a verb is missing, so that the mening of the sentence os not clear. [ MONICA TOLOTTI, Italy]	Editorial. Addressed in ES edits.
57531	4	6	4	8	Changes are also assessed from Earth System Model datasets and are not mentioned at the offset. The terminology 'changes are assessed from in-situ and remotely-sensed data and products derived therefrom' doesn't necessarily refer to modelled output, and should be explicitly included. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The chapter is not concerned with ESMs or their assessment. This is instead performed in chapter 3
24341	4	8	4	8	The following is phrased very awkwardly: "Periods able to be assessed are directly dictated..." A better choice would be: "Periods available for assessment are directly dictated..." [ Owen Cooper, United States of America]	Editorial. Addressed in ES edits.
90911	4	8	4	8	able to be assessed - rephrase? [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Addressed in ES edits.
4605	4	11	1	11	Is 5-95% correct? Not maybe >95% likelihood that the observation is not by chance? [ Andries Kruger, South Africa]	See response to 112389
112389	4	11	4	11	I found the "very likely range" a bit confusing. It makes more sense to use the word confidence interval with the explanation of what it is. [ Feng Ran, United States of America]	Taken into account. Sentence removed.
9911	4	11	4	11	(5-95%) not clear [ Olga Zolina, France]	See response to 112389

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4603	4	11	4	11	Definition for "very likely" is provided here, what about "likely" and other definitions of certainty? [ Andries Kruger, South Africa]	See response to 112389
90913	4	11	4	12	where stated -meaning is unclear [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	See response to 112389
86689	4	11	4	12	The sentence, starting with "Where stated ..." can perhaps be misunderstood. The word "range" is used twice but with two different meanings. [ Oyvind Christophersen, Norway]	See response to 112389
455	4	11	4	12	"ranges represent the assessed very likely (5-95%) range" reads as though anything from 5% to 95% is considered "very likely", which seems quite unlikely. The sentence should be rewritten to clarify what is meant. [ Claire Parkinson, United States of America]	See response to 112389
102715	4	12	4	12	The sentence 'unless otherwise specified, conclusions support relevant AR5, SR1.5, SROCC; SRCCL findings.' should be lifted to the SPM. Ideally a reference to IPBES GA would be added [ Philippe Tulkens, Belgium]	Noted. This is not in the gift of the chapter team to action.
7405	4	14	4	55	ERF is separated in different components (well-mixed GHG, aerosol, solar ...) and a value is given for each component. I don't see a global estimate of the changes in ERF since the pre-industrial era including the different components. I suggest to add such estimate ? [ Jeremy PANTHOU, France]	Rejected. The overall ERF is presented in section 2.8.
24343	4	16	4	16	The following is phrased awkwardly: "Climate system drivers act to modify accumulated shortwave radiation..." A better choice would be: "Climate system drivers modify accumulated shortwave radiation..." [ Owen Cooper, United States of America]	Accepted. This sentence is reformulated.
86691	4	16	4	17	Please consider to rephrase the sentence starting with "Climate system..." It can be difficult to comprehend an accumulation of radiation emitted to space. Perhaps "total" is a better word than "accumulated". Or perhaps you can write about the balance between shortwave and longwave radiation being modified. [ Oyvind Christophersen, Norway]	Accepted. This is now shortened and reformulated.
77181	4	16	4	18	This is very technical. It could be expressed as changes to the Energy balance which are termed radiative forcing, e.g. use text similar to text in chapter 1 section 1.3.3. In essence the Earth's energy balance has remained relatively stable for millennia but this has been changed by accumulation of GHGs in the atmosphere. [ Emer Griffin, Ireland]	Accepted. This sentence is now shortened and reformulated.
19683	4	16	4	18	what is accumulated is energy rather than radiation. The "s" in "leads" is not necessary [ philippe waldteufel, France]	Accepted. This is now shortened and reformulated.
114723	4	17	4	17	I don't think you need to say "introduced in AR5". Not relevant at ES level. [ Jan Fuglestad, Norway]	Accepted. This is now omitted.
132101	4	17	4	18	It is a bit abrupt to start with Effective Radiative Forcing and have so many assessments on it without briefly introducing this term. Even if it is introduced in the AR5, it cannot be expected that all readers will exactly know what is meant by it. Would be useful to add a sentence to introduce this term before using it in the ES. [ Sonia Seneviratne, Switzerland]	Rejected. The definition and derivation of ERF is the task of Chapter 7 (and Chapter 1).
86693	4	17	4	18	ERF is described under Radiative forcing in the glossary, please consider to point the reader to the glossary for explanation. [ Oyvind Christophersen, Norway]	Accepted. Reference to the glossary inserted.
24345	4	18	4	18	Is there any reason to say "climate changes"? This sounds very awkward. Why not just "climate change"? [ Owen Cooper, United States of America]	Accepted. Corrected.
90915	4	18	4	18	lead not leads [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected accordingly.
18741	4	18	4	18	Fast adjustment (which is not part of ERF) also causes climate change. Therefore, one can say climate change is proportional to ERF or ERF is an excellent metric for projecting climate change [ Govindasamy Bala, India]	Rejected. Fast adjustments are in fact part of ERF (ERF = RF + adjustments).
30067	4	18			'lead' [ Gilles Delaygue, France]	Accepted. Corrected accordingly.
132121	4	20	4	21	"Changes in ERF since the late 19th century are dominated by increases in concentrations of greenhouse gases and variations in aerosols". "Dominated" is vague, just means >50%. Can you provide a more quantitative estimate? What is the contribution of greenhouse gases and aerosols, more than 90%, about 100%? [ Sonia Seneviratne, Switzerland]	Rejected. The issue is that aerosol and greenhouse gas forcings are of different sign. So what the reviewer suggests does not work. If one would go for absolute numbers, a complicated statement would emerge.
77183	4	20	4	23	Similar to above some use of simpler statements could make it clear that changes to the energy balance are the key problem. These are driving climate change. [ Emer Griffin, Ireland]	Accepted. The concept of ERF is now introduced very clearly in the opening paragraph of this section.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17071	4	20	4	23	I suggest these changes: Changes in ERF since the late 19th century are dominated by increases in concentrations of greenhouse gases and variations in other various aerosols; the net ERF is positive and likely accelerating. Present-day global concentrations of atmospheric CO <sub>2</sub> are at elevated levels and were not experienced in at least the past two million years (high confidence). {2.2, 7.3} [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The edit suggested by the reviewer is to add "other various" before "aerosol", but the assessment indeed relies to quite some extent on AOD, which is related to the sum of all aerosol.
7507	4	20	4	24	The expressing of uncertainty by reporting evidence and agreement (i.e., robust evidence, low agreement) is very confusing. I see why you use it but as a scientist I find it baffling. Please just report your stat test and P value. If you don't have a stat test then do not use terms normally used in statistics. Instead of saying medium confidence just say nothing. [ Hugh Lefcort, United States of America]	Rejected. It is unclear to which statement the reviewer refers. There is no "medium confidence" statement at the instance cited. As a general remark, the reviewer is referred to the explanation of the IPCC calibrated language.
132109	4	20	4	29	My understanding from Fig. TS.10 of the AR5 is that the best estimate of the total human influence on observed warming since 1950 (and maybe earlier?) is that ca 100% of the observed warming is due to human influence. Could this ES or that of chapter 3 provide such a quantitative estimate of what is the most likely total influence of human forcing on the observed warming, possibly with a confidence interval (e.g. 95%-105%)? Compared to the AR5, it seems we could now provide more quantitative estimates. [ Sonia Seneviratne, Switzerland]	Rejected. This is indeed a very important conclusion of WG I AR6. However, it belongs to Chapters 3 and 7, but not to Chapter 2 that explicitly is not about attribution.
93479	4	20			dominated by increases in concentrations of 21 greenhouse gases and variations in aerosols; [ Rahab KINYANJUI, Kenya]	Rejected. This comment is difficult to understand. There are not 21, but really four species of greenhouse gases that are key.
93481	4	20			dominated by increased concentration of 21 greenhouse gases and variations in aerosols; [ Rahab KINYANJUI, Kenya]	Rejected. This comment is difficult to understand. There are not 21, but really four species of greenhouse gases that are key.
132103	4	21	4	21	Please state a quick explanation why ERF is "likely accelerating". Is this because of an acceleration in the rates of emissions? [ Sonia Seneviratne, Switzerland]	Taken into account. The wording is revised, and the causes are explained in the concluding statement of the supporting Chapter section.
36935	4	21	4	21	GSAT change (a fantasy enough) cannot be discussed in percentage terms without specifying the units. Units in C, F, R or K will all produce different percentage changes, as will calculating the percentages from anomalies. [ John McLean, Australia]	Not applicable. There is seemingly a mistake as the statement the reviewer refers to does not talk about GSAT.
1961	4	21	4	21	I don't think an ERF can "accelerate" (sounds like it is a moving object!). I know what you mean, but maybe "the rate of change of ERF is increasing" might be more precise? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We are happy that the meaning is at least clearly conveyed. But also in light of other reviewer comments, we now avoid the word "accelerate".
99173	4	22	4	23	what is the justification of the use of 2M. The scientific literature does not support that. This is longer than ice core records i.e. the instrumental measurements are not the cause. The proxy records support a statement 3Ma. [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We have high confidence that CO <sub>2</sub> as high as today was not experienced in the last 2 million years. The sediment record (from which all >1Ma estimates of CO <sub>2</sub> are derived) is not continuous and represents an average signal (due to sediment mixing processes). Because of this, and because there are intervals where the upper error envelope of CO <sub>2</sub> reaches or exceeds 400 ppm at ~2.3 Ma, in order to have a statement with "high confidence" we chose a conservative temporal limit.
132105	4	25	4	25	Maybe clarify that this sentence is about natural forcings or non-human forcings, e.g. "Changes in ERF due to non-human causes, i.e. solar and volcanic eruptions, are likely to be small", or alternatively "Changes in ERF due to solar and volcanic eruptions are likely to be small in comparison with the total human-induced changes in ERF". [ Sonia Seneviratne, Switzerland]	Accepted. A formulation similar to that proposed by the reviewer is adopted.
30069	4	25	4	26	i think this sentence is not correct because it describes 'changes' of ERF (at odd with the concept of ERF): for instance the Pinatubo eruption in 1991 had an ERF of about -2 W/m <sup>2</sup> (Fig2.2). It contradicts the sentence p.13 L3-4. Suggestion: remove "Changes in" and start the sentence with "ERF due to..." (i.e., the ERF over the period of instrumental observations). Note that the previous sentence correctly uses "changes in ERF" to relate them to increases in GHG concentrations. [ Gilles Delaygue, France]	Accepted. The reviewer is right that this statement was a bit confusingly formulated and we have made edits accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4499	4	25	4	27	Authors write “Changes in ERF due to solar and volcanic forcings over the period of instrumental observations are small in comparison to other drivers, and not unusual in the long-term context (high confidence). Solar activity since 1900 was high but not exceptional compared to the past 9000 years (high confidence).” This statement is misleading and sends out the wrong message. It 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, <a href="https://www.nature.com/articles/nature02995">https://www.nature.com/articles/nature02995</a> . 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, Switzerland]	Rejected. The new references cited in the relevant section revised the former results quoted by the reviewer.
54879	4	25	4	29	Recommend adding to this paragraph a value for solar forcing (e.g. since 1900) to be able to readily see what 'small' means in comparison to the forcing from WMGHGs (i.e. an order of magnitude smaller). This would also help ensure that the text on line 27 describing solar forcing as "high" is not misinterpreted. It would also be useful to include a key characteristic of volcanic forcing here - that it is generally short-lived and episodic. [ Nancy Hamzawi, Canada]	Accepted. We now open this ES statement with a clearer “negligible” statement.
132107	4	26	4	26	"in comparison with other drivers". Can you be more explicit? What are these other drivers? Wouldn't it suffice to state "Changes in ERF due to solar and volcanic eruptions are likely to be small in comparison with the total human-induced changes in ERF"? [ Sonia Seneviratne, Switzerland]	Accepted. It is clarified that this is in comparison to anthropogenic drivers.
17073	4	27	4	29	I suggest these changes: The average magnitude and variability of volcanic aerosol forcing since 1900 have not been usual unusual when compared to the past 2500 years (medium confidence). {2.2.1, 2.2.2} [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The reviewer suggests replacing “has” by “have” which is fine.
90917	4	29	4	29	Brackets should not be italicised [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
38309	4	31	4	32	Methane is expressed as different gas types in different chapters. Chapter 2 classifies methane as one of well-mixed greenhouses gases, chapter 5 as one of GHGs, chapter 7 as one of reactive well-mixed greenhouses gases, and chapter 6 as one of SLCFs, discussing its climatic effects in great details. It is suggested to coordinate the content concerning methane in different chapters, standardize the use of concepts, and modify the relevant content. [ Yaming LIU, China]	Noted. In Chapter 2 we exclusively focus on methane as a WMGHG, as observed by global networks, Chapter 6 further explains its role as SLCF.
57533	4	31	4	32	The statement lacks an immediate reference though 2.2.3 has results from AR5. The NOAA annual greenhouse gas index can be one potential reference. [Hofmann, David & Butler, James & Conway, Thomas & Dlugokencky, Edward & Elkins, James & Masarie, Kenneth & Montzka, Stephen & Schnell, Russell & Tans, Pieter. (2011). The NOAA annual greenhouse gas index (AGGI).] [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This is an assessment statement and details follow in the respective sections.
18743	4	31	4	38	The ERF of CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O in 2018 may be quoted for the sake of completeness [ Govindasamy Bala, India]	Rejected. For conciseness we decided not to do so, but refer to Annex 3 and section 2.8 for further detail.
77185	4	31	4	38	Not clear why some much detail on radiative forcing is included here as Chapter 7 deals with these issues. Perhaps overall changes in the Earth's energy balance and trends in this since the industrial revolution would be enough. [ Emer Griffin, Ireland]	Noted. According to the scoping CH2 was changed to present consistent view of the present state of climate system, thus the use of some estimates assessed in details in the other chapters is unavoidable. Thus, we have presented here some overall numbers.
89661	4	31	4	45	I was very surprised to find these ES bullets on various radiative forcings here, given that they are not assessed in chapter 2 at all. The GHG forcing numbers are at least included in Table 2.3, but the aerosol ERF number is actually not in the chapter at all (and is also out of date, see section 7.3). If you want to keep these ES bullets then at least reference the chapter/section where the forcings are assessed, and obviously don't include anything in your ES that is nowhere to be found in the actual chapter. [ Trude Storelvmo, Norway]	Noted. As it is in the remit of Chapter to also report on RF, we present some integrated numbers taken from Chapter 7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98897	4	32	4	32	Please change "unseen in the past 800 ka" to "unseen in at least the past 800ka, and very likely much longer" The 800 ka presumably comes from the lonest ice core, but unless this is specifically noted, one does not want to lead the reader to conclude that there were such changes just before this time. Readers need to know these changes are really very unprecedented--I don't know of any way that natural processes could have transferred anywhere near as much carbon to the atmosphere from its geological locations in anywhere near so short a time--save perhaps a very large asteroid or similar that would have left a clear indication in the records that we do have. [ Michael MacCracken, United States of America]	Taken into account. This statement has been revised in collaboration with chapter 5 to ensure consistency.
16467	4	32	4	32	The ERFs here come from section 7.3.5. This should be stated. I'm not convinced an ERF statement is needed here, since it is also an ES point from chapter 7. If it is needed upfront in the report. It would make sense to include halocarbons and show a WMGHG total for consistency with chapter 7. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ERF statements are traced to Ch. 7.
52099	4	32	4	32	New CO2 proxy evidence from delta13C in terrestrial C3 plants suggests in fact last time CO2 exceeded present day levels was in the mid-Miocene c. 14 million years ago: see Cui Y, Schubert BA, Jahren AH. A 23 my record of low atmospheric CO2. Geology. 2020 May 29. DOI: <a href="https://doi.org/10.1130/G47681.1">https://doi.org/10.1130/G47681.1</a> (Note: direct CO2 measurements can only be measured in the ice cores though, which is elaborated on in the main text section 2.2.3.1 but not here in the summary - not sure if that should be clarified given the significance and the fact that non-specialists would more likely read the summary) [ Kathryn Fitzsimmons, Germany]	Noted. The use of carbon isotopes is discussed in 2.2.3.1 and in general more weight is given to the evidence from Boron isotopes.
73865	4	32	4	32	undetected seems to me more appropriate than unseen [ Dominique Raynaud, France]	Taken into account. Changed to unprecedented to avoid conflation with charge of chapter 3.
126859	4	32	4	32	Since the 800 ka figure is driven by data availability, text should say "unseen in at least the past 800 ka". [ Trigg Talley, United States of America]	Accepted. Sentenced revised.
83943	4	32	4	32	it would be interesting to insert "(very high confidence)" after "past 800 ka" as it is stated at section 2.2.3.2.1 [ Marco Tulio Cabral, Brazil]	Accepted. Very high confidence is added.
57717	4	32	4	33	Concentrations have increased to 2018 is confusing phrasing. Perhaps "Between 1750 and 2018, concentrations increased ..." [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We have changed the sentence to: "By 2018, concentrations increased from ...to ..."
126857	4	32	4	34	How about "By 2018, concentrations increased from XX to XX"? This sentence is awkward otherwise. [ Trigg Talley, United States of America]	Accepted. Rephrased according to reviewers suggestion
86695	4	32	4	34	The sentence, starting with "Concentrations have..." is very complicated to read. Please consider to spilt into three sentences, one for each gas. Specify over what time period the changes occur. E.g. "Concentrations of CO2 was 407 ppm in 2018, 129 ppm higher than in 1750, representing a 46 % increase." [ Oyvind Christophersen, Norway]	Accepted. Sentenced revised.
57535	4	32	4	34	Sentence structure makes it hard to grasp the key message since both magnitude and percentage increase are stated - one is enough. Could be reframed as follows: 'From 1750 to 2018, concentrations of CO2 increased by 46% reaching levels of 407.4 ± 0.3 ppm in 2018, of CH4 by 157% reaching levels of 1858.6 ± 3 ppb in 2018, and of N2O by 23% reaching levels of 331.2 ± 0.3 ppb in 2018 (very high confidence).' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Sentenced revised.
93483	4	32			concentrations have increased to 2018..." to change to "concentrations have increased in 2018 [ Rahab KINYANJUI, Kenya]	Accepted. Sentence was modified accordingly
42871	4	33			This is written very awkwardly with both the percentage and the gas in the same parenthesis. Would be easier to understand as "129 ± 2 ppm (46%) for CO2," [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentenced revised.
15467	4	34	4	34	According to the WMO Greenhouse Gas Bulletin 2019 ( <a href="https://library.wmo.int/doc_num.php?explnum_id=10100">https://library.wmo.int/doc_num.php?explnum_id=10100</a> ), the atmospheric methane concentration in 2018 was 1869 ppb. Please double check. [ SAI MING LEE, China]	Rejected. The IPCC assessment is based on multiple sources of informing including the WMO GHG bulletin.
57775	4	34	4	34	I hope that the concentrations of these traces gases will be updated during the life cycle of the IPCC report review process. As of 13th April 2020, I have the concentration of CO2 down as 415.46 ppm. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. All concentrations have been updated to include 2019 as the latest year of assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21231	4	34	4	34	Chapter 1 states the uncertainty for the CO <sub>2</sub> concentration as $\pm 0.17$ ppm [ Michael Schmitt, Germany]	Accepted. The uncertainty was taken from a single network. However, in line with Chapter 1, we now take the standard error from X independent networks. Results: CO <sub>2</sub> = 0.17 ppm (3 networks); CH <sub>4</sub> = 3.4 ppb (4 networks); N <sub>2</sub> O = 0.3 ppb (3 networks) (90% C.L.)
126861	4	35	4	35	"present" should be changed to "2019". [ Trigg Talley, United States of America]	Accepted. Changed to 2019.
126863	4	35	4	36	Should state what time period this is (kyrs from present). [ Trigg Talley, United States of America]	Rejected. The reference to Holocene is no longer there.
4607	4	36	4	36	This section deals with increases in WMGHGs since 1750 and therefore this sentence can be omitted unless there estimations can be backdated to around the pre-industrial era. [ Andries Kruger, South Africa]	Rejected. The explicit comparison to changes before pre-industrial is made to demonstrate the unusualness of the changes after 1750.
57537	4	36	4	38	The additional ERF from synthetic GHGs has increased by 4% according to subsection 2.2.4.3 (p.22, line 8), not by 3% as stated in the Executive Summary. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The section information is the correct one.
131499	4	37	4	37	Abbreviations "CFCs, HCFCs, and HFCs" appear here without explanation what they stand for (it comes later in the chapter). I suggest to introduce the terms/abbreviations here, as the ES comes first [ Hans Poertner and WGII TSU, Germany]	Rejected. We refer to the annex with acronyms and the glossary for further clarification.
57777	4	38	4	38	The curly brackets should reference Figure 2.5 which sums this section up nicely. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Inconsistent with style guide, only references to paragraphs.
132123	4	40	4	41	Is the "medium confidence" referring to the overall sentence or only the 2nd part of the sentence? I would assess that the first half sentence ("Atmospheric aerosol concentrations across the Northern Hemisphere mid-latitudes increased since 1700") is at least "likely", or possibly "very likely". The decrease in aerosols in the last 20 years is also well documented, at least in Europe and North America, would seem to be at "likely" level as well. [ Sonia Seneviratne, Switzerland]	Accepted. We now are much more specific on what we can say and select a statement for which we have "high confidence".
52101	4	40	4	41	Is there scope for an addendum discussing the influence of Covid19 lockdowns on reducing atmospheric aerosols? Data are emerging to show this is the case, e.g. Venter ZS, Aunan K, Chowdhury S, Liljeveid J. COVID-19 lockdowns cause global air pollution declines with implications for public health risk. medRxiv preprint <a href="https://doi.org/10.1101/2020.04.10.20060673">https://doi.org/10.1101/2020.04.10.20060673</a> [ Kathryn Fitzsimmons, Germany]	Taken into account. The report now includes an extra box on the COVID-19 effects.
57539	4	40	4	42	According to subsection 2.2.6 there's 'high confidence' in atmospheric aerosol concentration changes in the northern mid-latitudes in the last quarter of the 20th century and decreases since then (p. 27, lines 17-19), but according to the Executive Summary there's only 'medium confidence'. Not sure which one is correct, or if the Executive summary has taken into account some other aspects in its confidence assessment, which explains this difference. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We now make a slightly revised statement for which we can state "high confidence".
30015	4	40	4	45	wording please add some words for the contribution to AOD by the global sandstorms, especially in Africa. [ Yihui Ding, China]	Rejected. The observations assessed in this Chapter do not allow to attribute the AOD to specific sources, and also it seems inappropriate to include such a statement in the ES.
67651	4	40	4	45	It should be emphasized that this applies to tropospheric aerosols [ Karen Rosenlof, United States of America]	Accepted. This is a good suggestion. We replaced at the beginning of the statement "Atmospheric" by "Tropospheric".
77187	4	40	4	45	The term aerosol is well known in atmospheric science but is used more broadly elsewhere, perhaps also used by PM here. [ Emer Griffin, Ireland]	Rejected. In fact, PM is used only in the narrow field of air quality research, whereas aerosol is the broader term used in climate science.
36931	4	41	4	41	It makes no sense to talk about a trend "prior to" a certain time? Any discussion of a trend needs to specify start and end points. [ John McLean, Australia]	Rejected. The comment is not relevant anymore, the half-sentence is omitted in the revised version.
16469	4	41	4	41	Why is 1700 used here? Everywhere else in the report (and for the chapter 7 ERFs) 1750 is used as the pre-industrial baseline. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reason is that the ice cores in their majority go back to 1700 (Fig. 2.9).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6471	4	41	4	42	The southern hemisphere trends are what they are. The confidence statement applies to our knowledge or estimates of the trends, not the trends themselves. The sentence should be reformulated. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	See response to 36931
86697	4	41	4	42	Please consider to refer to a year or a decade, rather than the "satellite era". Not all readers know immediately when that started and when they started tracking aerosol concentrations. [ Oyvind Christophersen, Norway]	Rejected. Since the details are given in the section that corroborates this ES statement we rather not dwell on this here.
114729	4	42	4	43	Please check this number for aerosol ERF with ch7 [ Jan Fuglestad, Norway]	Accepted. It is in fact from Ch. 7
73297	4	42	4	43	Please edit the end of the sentence to remove the hanging negative sign. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
54881	4	42	4	43	It would be useful to reiterate here a key message from AR5, which is still true in the AR6 based on Ch.2's assessment, that the net negative aerosol RF since 1750 has offset a significant amount of the positive forcing from GHGs. [ Nancy Hamzawi, Canada]	Rejected. On the one hand, this is implicitly already said, since we provide both numbers. On the other hand, the explicit discussion of this is better reported in Ch. 7.
86699	4	42	4	43	Please consider to include that negative ERF means a cooling effect. [ Oyvind Christophersen, Norway]	Rejected. We believe this should rather be discussed in Ch. 7.
114727	4	42	4	55	This ES gives much info on ERF. Obviously, close coordination with ch7 is needed. [ Jan Fuglestad, Norway]	Accepted. The ERF values are indeed Taken in close collaboration with Ch. 7, and the LA and CA responsible for ERF (Bill Collins and Chris Smith) accordingly are CA in Ch. 2.
9913	4	43	4	43	"Aerosol Optical Depth (AOD) has decreased since 2000" what is the level of confidence? [ Olga Zolina, France]	Taken into account. The confidence statement at the end of the sentence refers to the entire sentence, including "predominantly negative trends since 2000".
57541	4	43	4	45	The 'high confidence' on AOD changes is not stated in the subsection 2.2.6. (p. 27, lines 20-22). Not sure whether the confidence level should be added to the subsection 2.2.6 or whether the the Executive summary has taken into account some other aspects in its confidence assessment, which explains this difference. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The confidence statement is added to the summarizing paragraph in section 2.2.6.
77189	4	44	4	45	Fine mode AOD? What size range are being referred to? Also AOD is obscure for policy makers [ Emer Griffin, Ireland]	Accepted. We agree that AOD is a rather indirect measure, but that is what can be assessed. "Fine-mode AOD" is clarified by writing now "AOD from sub-micrometre particles"
77191	4	44	4	45	This could be expressed as these have "modified the Earth's energy balance measured as ERF" [ Emer Griffin, Ireland]	Accepted. The formulation proposed by the reviewer is adopted, except that we replace "measured" by "quantified" since ERF is not measured.
86701	4	44	4	45	Please consider to explain that "fine-mode AOD" is or use other words. [ Oyvind Christophersen, Norway]	Accepted. Clarified as "sub-micrometre"
24347	4	45	4	45	The word "yet" is not needed, and sounds awkward. [ Owen Cooper, United States of America]	Accepted. Changed as suggested.
90919	4	45	4	45	word order yet more? [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Modified accordingly.
35931	4	45	4	45	Could indicate that changes in fine-mode AOD suggest that trends are anthropogenic in nature, perhaps with medium confidence? [ Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Good suggestion, even if this is somewhat attribution it is taken up here.
73867	4	47	4	47	Why not to say Changes in stratospheric ozone, tropospheric ozone and stratospheric water vapour instead of atmospheric composition from atmospheric chemistry? [ Dominique Raynaud, France]	Accepted. The sentences have been modified.
126867	4	47	4	48	The statement "Changes...have additionally modified ERF" is so vague as to be meaningless. Consider the converse: It's almost impossible that such changes would have zero effect on ERF. Possible fix would be to add an order of magnitude statement: "...by tenths of W m-2". [ Trigg Talley, United States of America]	Accepted. The summary statement has been changed.
78829	4	47	4	48	In this title it is not clear what kind of change is referred to. [ MONICA TOLOTTI, Italy]	Accepted. Sentenced has been revised.
4609	4	47	4	55	Can something be included here about changes in tropospheric water vapour? This could also have a bearing on the contents of the previous section on changes in WMGHG as water vapour is a greenhouse gas. [ Andries Kruger, South Africa]	Rejected. This is a feedback rather than a driver and is assessed in the indicators section that follows accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16475	4	47	4	55	The timeperiods for the observations and ERFs are mixed up here. The ERFs are 1750 to 2018, and come from chapter 7, whereas the observations are for different time periods. I don't think the ozone ERFs should be in a chapter 2 ES point since they are based on observations rather than observations, but if they are needed upfront in the report they should at least reference section 7.3.5. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ERF and observational time periods are harmonized.
126865	4	47	4	55	Land-use change isn't mentioned here (except in the paragraph heading). How does that compare with ozone and albedo effects? Isn't it about 0.9 PgC/yr? There are likely estimates of cumulative effects in the literature. [ Trigg Talley, United States of America]	Accepted. A separate bullet is provided for albedo in land-use change.
67653	4	48	4	49	It should be emphasized that this is the total stratospheric column of ozone, and that the lower stratosphere may show a different trend (that of decrease) over the satellite record. The Ball et al. and Chipperfield et al. work is noted in the text (on the lower stratospheric ozone changes) [ Karen Rosenlof, United States of America]	Accepted. Columns are mentioned, while details on the height are in the section.
80247	4	48	4	49	It is inaccurate to state that stratospheric ozone declined from 1980 to 2018 by 2.2%. It declined from 1980 to about 2000 with different rates of decline depending on the regions (tropics, mid-latitudes, polar). Since 2000 the ozone layer has stabilized and in 2018, it was 2.2 % below 1980 levels. [ Sophie Godin-Beekmann, France]	Rejected. While we agree with the comment, for conciseness the details are presented in section 2.2.5.2
32499	4	48	4	49	"Stratospheric ozone has declined between 60°S - 60°N from 1980 to 2018 by 2.2%". This does not seem to be entirely consistent with the statement on page 23, lines 9-10, which says: "Near-global 2014-2017 mean total ozone was about 2.2% below the pre-ozone depletion 1964-1980 average". Clearer formulations are needed. [ Sophia Mylona, Kenya]	Accepted. The summary statement has been updated.
24349	4	49	4	49	I worked on this section and the way this summary statement was edited, the result is not quite correct. The original statement was "Since the mid-20th century", which is not quite the same as "Since the 1950s", because the data from the mid-20th century spans the 1930s to the early 1970s. Please use, "Since the mid-20th century". This correction will make this statement consistent with the summary at the end of Section 2.2.5.3 Tropospheric Ozone. [ Owen Cooper, United States of America]	Accepted. Statement harmonized with 2.2.5.3.
16471	4	49	4	49	The uncertainty in stratospheric ozone ERF is asymmetric (-0.15 to 0.0) in table 7.8. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Correct. Changed, in accordance with chapter 7.
24351	4	50	4	50	I worked on this section and the term "and Tropics" was supposed to be deleted, because "Tropics" refers to the northern and southern hemispheres, but we did not have reliable historical observations from the southern hemisphere tropics. Therefore this statement needs to be simply, "...across the Northern Hemisphere (medium confidence);" This correction will make this statement consistent with the summary at the end of Section 2.2.5.3 Tropospheric Ozone. [ Owen Cooper, United States of America]	Accepted the statement has been modified accordingly.
42873	4	50			Just a stylistic decision to make throughout, but I have no idea why Northern Hemisphere and Tropics are capitalised. In English they are not. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. IPCC is following harmonized spelling.
73299	4	52	4	52	Capital 'T' for 'tropics' (as a proper noun and for consistency elsewhere). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. IPCC follows harmonized spelling rules.
16473	4	52	4	52	The tropospheric ozone ERF is 0.35 in table 7.8 [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ERF is aligned with Chapter 7.
67655	4	54	4	54	remove parenthesis before ERF [ Karen Rosenlof, United States of America]	Editorial. Modified.
93485	4	54	4	55	is it possible to state the main historical proxies used to determine the increased albedo, in the text? [ Rahab KINYANJUI, Kenya]	Rejected. This level of detail is not appropriate for the ES.
12605	5	1	5	55	There is no ocean heat content and salinity change in ES, I suggest to add in because T/S are two most fundamental parameters in the ocean, and they are linked to both energy and hydrological cycle. [ Lijing Cheng, China]	Taken into account. OHC changes were in the ES and remain so. Salinity is now included.
130475	5	1	5	55	There is no ocean heat content and salinity change information in ES, I suggest to add this because T/S are two most fundamental indicators in the ocean, and they are linked to both energy and hydrological cycle. [ Panmao Zhai, China]	See response to comment id 12605.
19685	5	2	5	2	"most recent decade"? Keep in mind that this report is to be published in 2021. [ philippe waldteufel, France]	Taken into account. The analysis has been updated through 2020.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83159	5	3	5	3	Define cryosphere/cryospheric here, by listing the components - snow, sea ice, permafrost, ice sheets, glaciers, icebergs etc. [ Robert Massom, Australia]	Rejected. The ES is not the place to get into such granularity.
132129	5	3	5	4	"Directly observed atmospheric, oceanic, cryospheric and biospheric changes provide unequivocal evidence of a warming world". The choice of the chapter 2 authors not to have a "land" realms has for consequence that "land" is not mentioned in this sentence. To be honest, this seems really strange, especially after having a fully IPCC special report on "climate change and land". It would seem most logical to mention here "atmosphere", "oceans", "land", "cryosphere", and maybe the "biosphere". [ Sonia Seneviratne, Switzerland]	See response to 132125.
79025	5	3	5	4	They provide unequivocal evidence that the world has warmed not evidence of a warming world. For that one need models. [ John Kennedy, France]	Accepted. Text modified accordingly.
83161	5	3	5	4	Add that although the atmosphere, ocean, cryosphere and biosphere - and changes therein - are closely coupled compenents of the climate system, they are treated separately here. [ Robert Massom, Australia]	Rejected. This is implicit in the text.
99331	5	3	5	5	The reference to the paleo record, while correct, distracts from the message. Is it really needed here? If yes as it contradicts AR5 it needs to be expanded to be self explanatory [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; CH2 remit is to assess changing state of climate as far back as data enable. Long-term context is needed. No 'contradiction' is found with AR5
15915	5	3	5	6	The statement:  " Many key climate indicators are now in states not experienced for centuries to millennia or longer, and since 1900 several key indicators of the global climate system have changed at a rate unprecedented over at least the last two thousand years. {2.3.5}"  ignores paloclimate records, instead it is more correct to say:  "The change to the critical climate indicator of CO2 is already twice the magnitude and nearly 100 times faster than any equivalent change seen on the paloclimate records for the past 800,000 years (ref the Vostok Ice Core data)." [ Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The purpose of this statement is to be inclusive of multiple indicators of the change across the climate system.
17075	5	4	5	6	I suggest these changes: Current Many key climate indicators states are now in states not ever experienced for centuries to millennia. or longer. , and. Since 1900 several key indicators of the global climate system have changed at an a rate unprecedented rate over at least the last two thousand years. {2.3.5} [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Unclear. Comment appears to have been somehow corrupted as the suggestion appears incomplete?
225	5	5	50	50	It should be noted explicitly whether storm tracks have shifted poleward in a similar rate in both hemispheres. It is unclear if this sentence relates to both hemipsheres or just to the Northern Hemisphere. [ Sebastian Schemm, Switzerland]	Accepted. Text has been modified to clearly indicate poleward shift in both hemispheres, but not uniformly in seasons.
98899	5	6	5	6	Saying "at least the last two thousand years" here and on line 9 seems likely to be a serious understatement. How about adding a phrase to the effect that "; for much longer times there is no supporting evidence that this is not also the case"--to at least make clear that there is no contradictory evidence over much longer times even though the exact conclusion cannot be documented with high statistical confidence. [ Michael MacCracken, United States of America]	Accepted. A new statement has been added to address the long-term unusualness.
132139	5	8	5	11	Before mentioning GMST right away, it would be useful to introduce the concept of "global average temperature", which is the term used in the Paris Agreement. For instance: "In the context of the Paris agreement, global average temperature (Tglob) is used as metric of climate change. In past IPCC reports, observed Tglob changes were estimated based on the metric "Global mean surface temperature (GMST), which is based on 2-m air temperature over land, sea surface temperatures over the ocean, 2-m air temperature over sea ice." And then continue with other assessments based on GMST. Mention afterward "Another measure of Tglob is the so-called Global surface air temperature (GSAT), which is only based on 2-m air temperature on both land and ocean. GSAT-based metrics were used as estimates of Tglob in model-based analyses of the AR5." And then any assessments based on GSAT. [ Sonia Seneviratne, Switzerland]	Rejected. The Paris Agreement is no longer referred to explicitly in the revised cross-chapter box 2.3 although there is discussion of 1.5 and 2 C global warming levels.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7529	5	8	5	11	<p>The sensationalist parts of the sentence (i.e., unprecedented, in at least) do not match the confidence levels. In a normal science paper if the trend was statistically significant we would replace unprecedented with higher than the last two thousand years. If it was not statistically significant then we would just say not higher than the last two thousand years. Since you only have medium confidence we know it's not statistically significant.</p> <p>The second part of the sentence is equally bad. It's 50/50 so we would normally write we see no statistically significant evidence of surface warming. The way it is written, at first glance, looks like it is warming.</p> <p>I'm not trying to be difficult but take a look at the following two sentences. Both say the same thing but one is sensational and fear-inducing and one is boring.</p> <p>1. We are only 50% sure that your chances of getting Corona virus are lower today. 2. We see no evidence that your risk of getting Corona virus has changed.</p> <p>Isn't #1 more alarming? Why write like this? [ Hugh Lefcort, United States of America]</p>	Rejected; the stated confidence levels are not strictly equivalent to a quantitative probability. Instead, they are qualitative statements based on the type, amount, quality and consistency of evidence and the degree of agreement.
81515	5	8	5	11	<p>Recommend to revise '...and it is about as likely as not that no multi-centennial period...', as it is unclear and caused confusion. [ Ee Ling Lee, Malaysia]</p>	Accepted; statement was re-arranged and simplified
4501	5	8	5	11	<p>The statement "that no multi-centennial period since the last interglacial period (125 ka) was warmer globally than the most recent decade" is clearly wrong and misleading. Land temperatures during Holocene Thermal Maximum were 1-3°C warmer than today in many places of the world. There is currently no reliable global Holocene temperature composite. 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 Holocene Thermal Maximum (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. Likewise, the claim that sea level only varies by 10 cm is wrong. During the HTM, the sea level in many parts of the world was up to several metres higher than today. This makes sense because the Greenland ice sheet at the time was smaller than today. Your statements are misleading and suggest a steady state for the pre-industrial last few millennia that did not exist. [ Sebastian Luening, Switzerland]</p>	Taken into account; agreed that the Arctic summer was probably warmer, but the assessment refers to and focuses on "global mean" temperature, not regional summer. In addition it refers to a relatively narrow "multi-centennial period", not to the HTM, which was time-transgressive over many millennium and therefore if combined to represent on time slice would necessarily overestimate of the global temperature at any one moment. I was not able to locate any peer-reviewed articles with evidence that the reconstruction by Marcott et al. (2013) somehow underestimated the GMST during the Holocene. To the contrary, a recent multi-method analysis of a major proxy data compilation (with a mix of terrestrial and marine sites) produced a Holocene temperature reconstruction nearly identical to Marcott's (Kaufman et al., 2020). Published criticism of the Marcott reconstruction (e.g., Liu et al., 2014) and AR5-generation proxy data compilations (Harrison et al., 2015) suggests that the estimated temperatures are too high, not too low. Local relative sea level and global mean sea level can be very different. This information is presented in the report.
86705	5	8	5	11	<p>This is an important sentence, however it can for some readers be too long and complicated. It is simply not clear what the second part, starting with "and it is about as likely ..." means. Does it mean that it is a 50-50 chance that the last decade was warmer than any time period, lasting hundreds of years, over the last 125 000 years? And what does that actually tell us? [ Oyvind Christophersen, Norway]</p>	Taken into account; statement was re-arranged and simplified. Meaning of confidence levels is presented in CH1.
17077	5	8	5	11	<p>I suggest these changes: Over the last 50 years, global mean surface temperature (GMST) has increased at an unprecedented observed rate unprecedented in at least the last two thousand years (medium confidence). , and It is about as likely as not that No multi-centennial period since the last interglacial period (125 ka) was warmer globally than the most recent decade. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]</p>	Taken into account; statement was re-arranged and simplified



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34833	5	8	5	19	Detailed Comments by SOD Chapter – Chapter 2: During the Mid-Pliocene, GMST was 3°C warmer and during the last Interglacial GMST was 1.5°C warmer, with subsequent cooling since the mid-Holocene 6k years ago; in that context, there is nothing unprecedented about current GMST. [ Jim O'Brien, Ireland]	Rejected. The 'unprecedented' statement here clearly refers to the whole period of last 2000 years, so the examples quoted by the reviewer are not relevant to it.
7323	5	8	5	19	Weakness in GMST should be clarified if GSAT which has larger change of 4% (2-7%) with high confidence is recommended for analysis of climate model simulation in term of SR15 between line 21 - 25. [ SAN WIN, Myanmar]	Taken into account. Cross-Chapter Box 2.3 discusses the uses of GMST and GSAT. There is no longer a difference assessed in the amount of warming in GMST and GSAT.
35933	5	9	4	11	Suggested rewrite for clarity: "it is about as likely as not that the most recent decade was warmer globally than any other multi-centennial period since the last interglacial period (125 ka)" [ Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been reworded.
73869	5	9	5	10	Is that really helpfull to state that it is about as likely as not that no multi-centennial period since the last interglacial was warmer globally than the most recent period? Why not to simply say that we don't know. [ Dominique Raynaud, France]	Taken into account. The revised assessment enables a more certain likelihood.
105659	5	9	5	11	The "about as likely as not.." statement seems a very weak, at 50/50 should this be included as an exec summ bullet (and propagated to the SPM/TS)? [ Paul Durack, United States of America]	See response to 73869
36337	5	9	5	11	Saying that "it is about as likely as not that no multi-centennial period since the last interglacial period (125 ka) was warmer globally than the most recent decade" seems equivalent to saying "we have no idea whether or not the most recent decade was the warmest over the last 125 ka." This may be a true statement, but it's not notably informative. Does it really belong here in the Executive Summary (and in Figure 2.11)? [ Curt Covey, United States of America]	See response to 73869
8873	5	10	5	10	Comparison of levels averaged over vastly different periods of support (multi-centennial vs decadal) is hard to justify. [ Robert Kopp, United States of America]	Noted; while there are important assumptions, the "justification" is that there is no other alternative. Additional information is in the underlying text.
99175	5	10	5	11	the reference to the LIG makes the sentence very complicated and it is not clear how this information here is needed [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; replaced LIG with date range
58105	5	10	10	4	In Cross Chapter Box 2.1, the last interglacial is mentioned as the period between 129-116ka. But on page 5 line 10 the last interglacial period is 125ka. Please give justification. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; replaced "125 ka" with "129-116 ka"

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58107	5	10	72	20	In the executive summary of chapter 2 and later in other sections, it is mentioned that over the last 50 years, the global mean surface temperature (GMST) has increased at an unparalleled rate. To get a far insight into this phenomenon the current climate is compared with two interglacials from the past, specifically the last interglacial period (125ka) and the Mid Pliocene warm period (around 3.3-3.0 Ma). The main aim of this kind of comparison is to get a precise idea about how far the present climate has warmed when compared with the previous warm periods and how warming becomes a threat to living beings in the future. It will also help us to take measures to reduce the unprecedented warming if it is a completely man-made contribution. Comment #Compared to the present, the last interglacial period or MIS 5e period was followed by enhanced CO2 level, higher Sea-level, high Arctic amplification, and reduced ice sheets over Greenland and Antarctica. Interestingly, the archaeological and geological evidence shows that the last interglacial period witnessed the migration of human beings from Africa to Asia, and Europe (eg. Petraglia, 2007; Groucutt and Petraglia, 2014). This period also witnessed high humidity over Sahara and Arabia. Hence I feel that there is a need to understand the reasons for such a plight of human beings from Africa to other places on the globe or what might have made Africa a place not suitable for dwelling? Such studies will help us to find out whether any region on the globe at present is vulnerable or will be vulnerable to human dwelling shortly. Also, such studies will give the exact range of the danger of global warming if the current situation is completely due to anthropogenic forcings. I believe that such studies will tell more about future climate if the unprecedented warming persists at the current rate. Similarly, there is a need to understand the Mid-Pliocene Warm Period, around 3.3-3.0 Ma (million years ago), when the global mean surface temperature was $3 \pm 1^\circ\text{C}$ warmer than the present (the presence of human beings is uncertain). Hence, in summary, I would tell there is a great need to identify the hot spot climate regions during the two past interglacial periods, the last interglacial and the Mid-Pliocene warming, and compare it with the present. This will give a precise knowledge about whether there exists any place on earth which was fine before the industrial revolution, and currently not a suitable place for living beings to survive. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The impacts of climate change (past or future) on human and natural systems are the domain of other IPCC Working Groups.
83589	5	10			<p>“unprecedented in at least the last two thousand years (medium confidence), and it is about as likely as</p> <p>10 not that no multi-centennial period since the last interglacial period (125 ka) was warmer globally than</p> <p>11 the most recent decade.”</p> <p>This statement (two thousand years) seems unlikely to be correct for the northern hemisphere since historical, alpine glacier, Chinese agricultural, and deep sea sediments suggest otherwise. Whether it is true for both hemispheres I cannot argue, but the statement should be qualified in view of the strong N Hemisphere evidence.</p> <p>The second part of the statement (“since the last interglacial period”) is manifestly wrong. I have never seen a Holocene or longer temperature proxy which fails to show the mid-Holocene (~6ka) as being warmer than the recent decade. [ michael asten, Australia]</p>	Rejected. The statement follows from the substantive assessment.
98901	5	11	5	12	Is there not a need to indicate that these estimates are from a linear analysis, or was this done accounting for the time-varying roles of all factors as is normally done in the detection and attribution chapter--it would seem to me that the approach for getting these results needs to be stated. In any case, it should likely be stated that the changes were not linear due to the changing rates of emissions of various species and the roles of other factors--and so this is, as I understand it, the cumulative/net effect and not just the role of the GHGs countered by aerosols. [ Michael MacCracken, United States of America]	Rejected. This is in fact a change in means between two periods, as explained in the underlying chapter. It also falls within the scope of the expanded cross-chapter box 2.3.
126869	5	11	5	12	"to the most recent decade" needs to be "for the most recent decade". [ Trigg Talley, United States of America]	Rejected. "To" is correct as it is reporting a change with respect to 1850-1900.

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41545	5	11	5	15	you should simplify, keep same reference periods and homogenize: data is available, reading one wonders what big warming happened between 2014-2018 [ Laurent Labeyrie, France]	Rejected. The 1995-2014 value is to provide a linkage to model simulations which use this as their modern reference period. This is a key number for the report as a whole.
57543	5	12	5	13	Statement on the last 4 decades being warmer than any preceeding could be qualified by a confidence statement - presumably high confidence [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This is presented as a statement of fact as it is effectively certain.
83591	5	12			"Each of the last four decades has in turn been warmer than any decade that preceded it since 1850." This statement is true in a narrow literal sense but it is misleading in its emphasis on last 4 decades. HADCRUT data shows it is obvious that the 1960s were cooler than the 1950s. And the 1900s were cooler than the 1890s. If we want to make a summary statement about post 1850 temperature rises we should be prepared to admit that cycles of change both up and down have occurred. [ michael asten, Australia]	Rejected. Information about decadal variability of GMST is available through Figure 2.11b.
98903	5	13	5	15	Given the reason for this is so clear, namely a greater effective heat capacity over the ocean, I think giving the reason this is consistent with expectations would be helpful [ Michael MacCracken, United States of America]	Rejected. Attribution of changes is the domain of Chapter 3.
98731	5	13	5	15	Meaning of sentence is unclear. Are the two ranges for land versus sea? Could rephrase as follows: Temperatures have increased faster over land than over the oceans since 1850-1900. Temperatures over land have increased by 1.44 °C (1.32 – 1.60 °C) from the 1850-1900 mean to the 2009-2018 mean, compared to a 0.89 °C (0.80 – 0.96 °C) increase over oceans. [ Meredith Parish, United States of America]	Taken into account. Sentence has been reworded.
8875	5	15	5	15	1.44°C vs. 0.89°C, "respectively" [ Robert Kopp, United States of America]	Accepted; added "respectively"
100589	5	15	5	15	Before Pliocene, add: "During the Miocene climatic optimum between 16.9 and 14.7 Ma (millions of years ago), GMST was 8.7±2.3 °C warmer than pre-industrial" [ Matthew Kohn, United States of America]	Rejected; Miocene temperature is now assessed in CH2 text, but temperatures are not stated in the ES for every reference period -- only a sampling.
83411	5	15	5	16	I recommend to be more consistent when refering to ages for paleoclimate periods, i.e. the Mid-Pliocene Warm period is given as an interval (age range) whereas the LIG is described as one point in time (also in line 10 on same page). [ Antje H. L. Voelker, Portugal]	Accepted; all paleo reference periods ascribed to time ranges.
1963	5	15	5	17	In this paragraph, the uncertainites in the paleo estimates are presented in a different format (y ± dy) to the other uncertainty estimates y (y1 - y2). [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; replaced x ± y with a – b format to avoid implications of symmetrical uncertainties
83413	5	15	5	17	Following the examples for the MPWP and the mid-Holocene, I recommend to apply the phrasing of "years ago" also to the LIG to make the reading and understanding easier for non-expert readers. [ Antje H. L. Voelker, Portugal]	Accepted; as suggested
1541	5	15	5	19	The Miocene and last interglacial seem quite different from the period from 1850. Make a separate key comment on the distant paleo. The next few key indicators revert back to recent changes since the 1950s or the 1980s. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; assessments that are relegated to separate sections in the text are integrated in the Executive Summary in accord with CH2 theme of changing climate states across time.
58109	5	15	90	20	In Chapter 2, it is mentioned that the Mid Pliocene Warm period is an interglacial with similar atmospheric CO2 concentration, much warmer Arctic region, and higher Sea level when compared with the present. Currently, we face unprecedented warming with high CO2 concentration and anthropogenic forcings are considered as a reason. So it is very important to understand what might have caused similar CO2 concentration as the present-day during the Mid Pliocene warm period during which the presence of human beings has not yet confirmed. Hence, I feel that there is much urgency to understand the reasons for the CO2 increase during this period. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; the cause of CO2 changes is outside of the scope for CH2; however, some information about the climate state during the Pliocene warm period are presented in CCB2.1, Table 1 and in CCB2.4.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42875	5	16			LIG value of 1.5 +/- 0.5 of 1850-1900: I can almost see how you get it, but it's a weakness that you don't spell it out in the rather indiscriminate analysis on page 32-33. There you cite SROCC as showing 0.5-1 degree (page 32, line 16) and individual data that come in at 1.0, 0.5 and 1.6 degrees for SST. Assuming all those studies are of equal quality, and assuming you then scaled them all by 1.6 as in Fischer, then I could almost see your range, but you never explicitly state 1.5 as the answer on page 33, nor do you justify the uncertainty range, which seems too small to me. Fuller discussion under page 32 line 53. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; see reply to comment on page 32, line 53
15165	5	17	5	19	First clause implies high confidence in a decrease from mid-Holocene to today. The confidence assessment of relevance here is the mid-Holocene to before the recent period of warming. Recommended change: "GMST slowed decreased from the mid-Holocene (around 6000 years ago) until the early 19th century (high confidence)". Then reiterate the confidence in recent warming. [ Simon Donner, Canada]	Taken into account. Statement about warming since 1850 however is factual and not confidence based.
112583	5	21	5	21	4% is less than the majority of available estimates of this effect, 5% looks like the lowest defensible number. [ Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The expanded cross-chapter box 2.3 now includes the full range of evidence from models, reanalyses and observations, including that noted by the reviewer.
132145	5	21	5	22	These numbers need to be carefully checked. Based on model projections, a larger ratio is possible (up to at least 10%, see Beusch et al, GRL, in press: <a href="https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL086812">https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL086812</a> ). On the other hand, reanalyses suggest possibly lower numbers. [ Sonia Seneviratne, Switzerland]	Taken into account. The expanded cross-chapter box 2.3 now includes the full range of evidence from models, reanalyses and observations, including that noted by the reviewer.
57719	5	21	5	22	There is high confidence in the fact that GSAT change is larger, but relatively low confidence currently in the magnitude of this. Perhaps a portion could be added to this sentence to reflect that. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account in redrafts to cross-chapter box 2.3 and this statement.
79027	5	21	5	22	Given that the introduction notes that the evidence presented here is based on observations and measurements, it should be made clear here that this estimate is model based and that there is no direct evidence of this from observations. [ John Kennedy, France]	Rejected. This level of detail is discussed as part of the cross-chapter box.
7325	5	21	5	25	More clarification and comparison of GMST and GSAT is recommended if GSAT is better than GMST. [ SAN WIN, Myanmar]	Taken into account. Cross-Chapter Box 2.3 discusses the uses of GMST and GSAT. There is no longer a difference assessed in the amount of warming in GMST and GSAT.
112391	5	21	5	27	I wonder, this might be a bit confusing for the general public and policymakers without additional explanation - i.e., surface air over the ocean is warming faster than the water temperature itself... [ Feng Ran, United States of America]	Taken into account. The explanation of the GMST/GSAT difference is refined in the expanded cross-chapter box 2.3.
86707	5	21	5	27	This paragraph tells the difference between GMST and GSAT, but does not inform the reader which is "better" or more correct to use. And it does not clearly explain what it implies for carbon budgets and temperature targets other than it has "an impact". The paragraph can potentially bring more confusion than enlightenment for most readers. The difference between, and associated implications, of choosing either Global Mean surface temperatures or Global Mean Surface Air Temperatures as basis for the assessment report will be very difficult to understand, especially for policymakers. Also the potential implication of the fact that some policymakers may interpret this as if difference between current temperature levels and the temperature goals that they have agreed and discussed extensively are changing due to choice of methods from every other IPCC report should be very well thought through. We acknowledge and appreciate that science is evolving, but you have to be very precise and clear when explaining why GSAT are chosen instead of GMST. This links directly to the WGII and WGIII assessment reports that are coming, so it is crucial that there are extensive across WG cooperation and alignment on this issue. [ Oyvind Christophersen, Norway]	Taken into account. Cross-Chapter Box 2.3 discusses the uses of GMST and GSAT. There is no longer a difference assessed in the amount of warming in GMST and GSAT.
105661	5	21	5	27	The GMST vs GSAT sentence is very clear, with 4 (2-7)% very quantitative, but why not include the degC value in addition to the % difference? The degC value is the headline value, whereas % hides this in the main text, and 4% of 1.1C is mental arithmetic that readers should not have to undertake [ Paul Durack, United States of America]	Taken into account. Explicit GMST and GSAT assessments are now included in Cross-Chapter Box 2.3 Table 1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19687	5	21	5	27	Admittedly this is a significant methodological issue for the WGI community. In terms of the changing state of the climate system however, it appears negligible, as recognized in CCB 2.3; do you then want to keep this paragraph? [ philippe waldteufel, France]	Taken into account. The expanded cross-chapter box 2.3 places this into the broader context of changes from AR5 to AR6.
90921	5	22	5	23	The difference between these two terms is not clear [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. These terms are explained more fully in cross-chapter Box 2.3, and in the glossary.
6473	5	22	5	23	What happens over sea-ice? Chapter 3 (lines 53 and 54, page 3-83) defines GMST as a combination of SST over open ocean and air temperature over land and sea ice. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Issues with temperature analyses over sea ice are discussed in detail in cross-chapter box 2.3.
126871	5	22	5	23	"GMST is a combination of air temperature over land and sea surface temperature, whilst GSAT is air temperature over both domains." This sentence is incredibly confusing. The way it's written makes it read that both GMST and GSAT are the same thing. Reference to the Cross-Chapter Box (line 27) should be moved up or additional explanation from this box should be provided to give additional information/context. [ Trigg Talley, United States of America]	Taken into account. These terms are explained more fully in cross-chapter Box 2.3, and in the glossary.
9915	5	22	5	23	"GMST is a combination of air temperature over land and sea surface temperature, whilst GSAT is air temperature over both domains." This requires a clear statement on the relationship between GMST and GSAT. Box 2 requires more in depth consideration of the physical ground of differences between GMST and GSAT. [ Olga Zolina, France]	Taken into account. This forms part of the expanded scope of the cross-chapter box 2.3.
10431	5	22			Is air temperature over sea ice included in this definition of GMST? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Issues with temperature analyses over sea ice are discussed in detail in cross-chapter box 2.3.
6475	5	24	5	24	"observed GMST" is a phrasing that should be avoided. GMST is not an observable. It is a quantity deduced from observations, and its value depends on how information is spread from well-observed regions into sparsely observed regions. "Observationally-based estimates of GMST" would be better wording. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. In the context of this chapter, "observed" is taken to refer to any quantity derived from observations, regardless of the amount of post-analysis involved in the derivation.
132147	5	24	5	27	Excellent and very informative text! [ Sonia Seneviratne, Switzerland]	Noted. Thanks for the compliment.
78291	5	25	5	25	Missing decimal point when referring to SR 1.5 (SR15) [ Leonie Lee, Singapore]	Editorial. Copyedit to be completed prior to publication, which will include standardised terminology for references to the Special Reports.
89857	5	26	5	27	Why would it affect policy if, for example, the Paris agreement was based on GMST all along? I for one would argue that agreeing on the 2°C GMST target implicitly refers to - say - 2.1°C GSAT. While I more than welcome to use GSAT as another metric next to GMST (and it is neatly introduced here), I have a hard time believing that it is helpful in a policy context. It adds a whole new layer of complexity, which is likely to add more confusion. Again, no technical complaint, but I would leave GSAT out of policy relevant estimates such as the remaining carbon budget. The same comment is valid for Cross-Chapter Box 2.3 (page 2-39 lines 1-8). Please also note my next comment. [ Karsten Hausteine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The issue raised by the reviewer is no longer so significant now that GMST and GSAT are assessed as having the same change.
132149	5	29	5	30	"Troposphere" and "stratosphere" might be a bit technical for some readers. Maybe write: "The troposphere (i.e. the atmospheric layer from the Earth's surface up to ca. 10km) .... that the stratosphere (atmospheric layer above 10km) ...". [ Sonia Seneviratne, Switzerland]	Rejected. This is dealt with through the glossary.
77193	5	29	5	30	The troposphere is obscure could it be explained as warming the atmosphere to a height of or similar? [ Emer Griffin, Ireland]	Rejected. This is dealt with through the glossary.
126873	5	29	5	30	Why is there not a confidence/likelihood qualifier to go with the statement "The troposphere has warmed"? [ Trigg Talley, United States of America]	Rejected. This is assessed to be certain so no confidence/likelihood language is required.
8877	5	29	5	30	Why is assessment language used for the stratosphere but not the troposphere. [ Robert Kopp, United States of America]	Rejected. The very limited availability of stratospheric data in the early part of the record preclude an absolute finding of fact for changes since the mid-20th century.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98905	5	29	5	31	I don't understand why stratospheric cooling is only virtually certain. "virtually certain" means greater than 99% chance whereas not needing to include this, as for the case for tropospheric warming, would seem to mean that there are useful distinctions in probability between 99 out of 100 and 100 out of 100 and this can be determined with some reasonable level of confidence. Is this really the case? On line 31, why not say instead that there are several reasons to conclude that the tropopause height has gone up „and then say the "but there is low confidence in the magnitude" [ Michael MacCracken, United States of America]	Rejected. The very limited availability of stratospheric data in the early part of the record preclude an absolute finding of fact for changes since the mid-20th century.
132157	5	29	6	43	None of this text mentions any statistics over land. This seems a bit myopic when one considers that we are all living on land. It is critical that this is changed for the FGD. [ Sonia Seneviratne, Switzerland]	Rejected. There are multiple assessment findings over land in this section (although no quantitative ones). A quantitative assessment of observed temperature change over land is included in section 2.3.1.1.
6477	5	30	5	31	The sentence refers to one new type of satellite measurement available since about 2001 that has raised the degree of confidence that the atmosphere has warmed faster in the upper troposphere than near the ground. But there is evidence directly from observations, and from the reanalyses produced by processing them, that the upper troposphere was warming faster than the near-surface before 2001 also. Consideration could be given to formulating a statement (more similar to the summary given on page 43) that recognises that there is evidence from observations prior to 2001, albeit less strong than the evidence we have for the period from 2001 onwards. The wider evidence includes supporting information from observations and reanalyses of humidity in the upper troposphere. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The pre-2001 results are assessed in the main text of the chapter and contribute to a low confidence assessment for that period. With few exceptions, low confidence statements are not included in the Executive Summary.
86709	5	31	5	32	Has the tropopause height increased globally or only in the tropics? Please clarify. [ Oyvind Christophersen, Norway]	Taken into account. 'Globally' inserted to make this clearer.
132151	5	34	5	34	Clarify what is meant here by "strengthened". [ Sonia Seneviratne, Switzerland]	Noted. The executive summary is not the place for such clarifications.
9917	5	34	5	34	"The global hydrological cycle has strengthened" – meaning of "strengthened" is not clear with respect to hydrological cycle, accelerated would be a better word and probably can be used together with recycling [ Olga Zolina, France]	Taken into account. We decided not to use of the word "acceleration". Instead, the use of strengthening/intensification provides a better sense of the changes in the global hydrological cycle.
105073	5	34	5	36	Assertion to the paleoclimate evidence does not refer to any specific period. However, on the time scales of several millennia, the changes in precipitation (e.g. monsoons) are unequivocal, even though they are difficult to quantify. So I don't understand this comment. [ Masa KAGEYAMA, France]	Noted. A clarification of the recent changes in the global hydrological cycle was provided. However, it is difficult to put the observed recent changes in a paleo context.
45221	5	34	5	36	The global hydrological cycle has strengthened since at least 1980 (high confidence)...Consistency with Chapter.8 can be checked. [ Krishnan Raghavan, India]	Taken into account. We ensured consistency with the findings of Chapter 8, even when the summary statements from Chapters 2 and 8 relative to the global hydrological cycle were difficult to compare.
98907	5	34	5	42	I would think the point needs to be made (and has been made in earlier assessments based on data from all continents save Antarctica, as I recall) that a greater fraction of precipitation is tending to fall in what might be called drenching rains, with lesser occurrence in moderate rains. This is very important for several reasons and I would think merits mention. [ Michael MacCracken, United States of America]	Rejected - outside the scope of the chapter. Assessment of precipitation types is the purview of Chapter 8 and is not covered here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17079	5	34	5	42	I suggest these change: The global hydrological cycle has strengthened since at least 1980 (high confidence). , but There is low confidence in the paleo context given that limitations in proxy-based reconstructions at continental and global scales are limited. Global land precipitation has likely increased since 1950, with a faster increase since the 1990s (medium confidence). Near surface specific humidity over land has very likely increased since at least the 1970s, and likely increased over the oceans. Global total column water vapour content has likely increased since based on satellite observations data, which started to be available since it is commenced in the 1980s. Relative humidity has very likely decreased over land areas since 2000, particularly over mid-latitude regions of the Northern Hemisphere. Observational uncertainty leads to low confidence in global trends in precipitation minus evaporation, while large-scale trends exhibit a very likely “wet-get-wetter, dry-get-drier” pattern over the Tropical oceans. {2.3.1.3} [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The summary statement for the global hydrological cycle was modified according to the new evidence assessed in section 2.3.1.3.
105663	5	34	5	42	When referring to the “hydrological cycle” you are only referencing water cycle stores that reflect ~2% of Earth’s water. Both the ocean and cryosphere are omitted from this statement (which account for ~97% and ~2% respectively, mind your focus is on an important 2%) [ Paul Durack, United States of America]	Noted. The conclusions from sections 2.3.2 (Cryosphere) and 2.3.3 (Oceans) were included in the Executive Summary.
29607	5	35			“in the paleo context”. Would it be better to refer as geological context? Or multi-millennial context? [ Villaseñor Tania, Chile]	Noted. The sentence was modified considering your suggestion.
131501	5	38	5	38	“Total column water vapor content” won’t be understood by a non-expert, please explain this term [ Hans Poertner and WGII TSU, Germany]	Noted. The executive summary is not the place for such clarifications.
4611	5	38	5	40	This information on water vapor can be included in the section starting on Page 2-4 line 47 [ Andries Kruger, South Africa]	Noted. We prefer to put the summary statements in the same order as the sections in the chapter.
132153	5	40	5	42	These trends are of most relevance over land, please add a statement on this in the sentence. [ Sonia Seneviratne, Switzerland]	Noted. The summary statement was modified according to the evaluation performed in section 2.3.1.3.5
18745	5	41	5	42	“wet-get-wetter, dry-get-drier” pattern is true for zonal mean changes too. [ Govindasamy Bala, India]	Noted. The summary statement was modified according to the evaluation performed in section 2.3.1.3.5
77195	5	41	5	42	Dry getting drier for tropical oceans? [ Emer Griffin, Ireland]	Noted. The summary statement was modified according to the evaluation performed in section 2.3.1.3.5
112393	5	42	5	42	The wet-gets-wetter, dry-gets-drier applies to tropical, subtropical and mid-latitude ocean. [ Feng Ran, United States of America]	Noted. The summary statement was modified according to the evaluation performed in section 2.3.1.3.5
132155	5	42	5	42	The term “wet-gets-wetter, dry-gets-drier” does not make sense over the oceans, a water surface. In addition, “tropical oceans” should be mentioned at the beginning of the last subsentence, else one gets the impression that this text refers to global-scale tendencies. Change to “... while tendencies over the tropical oceans display very likely patterns towards increased P-E gradients”. [ Sonia Seneviratne, Switzerland]	Noted. The summary statement was modified according to the evaluation performed in section 2.3.1.3.5
70313	5	42	5	42	I question the use of “wet gets wetter, dry gets dryer pattern of the tropical oceans” as by retaining focus on the tropics only as the dry regions normally implied by this concept are extra-tropical. This paradigm is also often used to described projected future rainfall, and when focusing on the tropics the projected rainfall changes do not look a great deal like those observed over the recent past (compare figure 4.47 with Figure 2.15). It also seems that CH4 is moving away from this simplification, suggesting that it does not work in the tropics (lines 52-53 of page 58 of the SOD CH4). [ Shayne McGregor, Australia]	Noted. The summary statement was modified according to the evaluation performed in section 2.3.1.3.5
1965	5	44	5	44	“likely” should be in italics, or different word chosen? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication
98909	5	44	5	53	Is it not the case that the poleward shifts of the extratropical storm tracks are associated with a polar expansion of the subtropics (more air going up in the Hadley Cell requires more air to come down in the subtropics) and this is leading to a poleward tendency to aridification, particularly along the equatorward reach of important agricultural areas? It seems to me worth mentioning this trend, at least in terms of subtropical extent. [ Michael MacCracken, United States of America]	Noted. Direct implications of the trends in atmospheric circulation to the agricultural areas is dealing with attribution issues and outside the scope of the assessment performed in Chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77197	5	44	5	53	Why are low confidence statements so prominent? [ Emer Griffin, Ireland]	Noted. Confidence levels were revised according to the available scientific evidence. For some sections, we could improve our confidence in recent and past changes in the global hydrological cycle.
10959	5	45	2	46	I don't think the evidence given supports that Hadley Cell widening has mostly occurred in the Northern Hemisphere - see eg Grise et al (2019) [ Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Hadley circulation strengthening occurred particularly over the Northern Hemisphere. See Section 2.3.1.4.1.
57745	5	45	5	46	Is this HC widening simply a return from the previous contraction? Does this recent expansion make up for earlier contraction? Could be construed as a positive trend without the wider context. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Due to space limitations within the Executive Summary, the evaluation the recent changes in the HC compared to what was observed previously was performed in section 2.3.1.4.1
112405	5	46	5	46	To my knowledge, changing Hadley circulation strength is quite controversial partly due to drift in early Satellite observations. See the following study for example: Mitas, C.M. and Clement, A., 2006. Recent behavior of the Hadley cell and tropical thermodynamics in climate models and reanalyses. Geophysical Research Letters, 33(1). [ Feng Ran, United States of America]	Noted. To our knowledge what remains controversial is the inconsistency between reanalyses and model simulations in terms of HC strength. This was clarified in the Section 2.3.1.4.1.
15469	5	46	5	47	Normally, "monsoon" refers to circulation/winds. Since near-surface winds have opposite trends over land and ocean (P.5, Line 48-49), it is suggested removing "intensity and" here to avoid confusion. [ SAI MING LEE, China]	Noted. Global monsoon trends refer to changes in precipitation. This was clarified in the Executive Summary.
3491	5	46	5	48	The statements here are vague and misleading and need to be changed. "Global monsoon intensity has likely increased, being dominated by Northern Hemisphere summer trends (medium confidence)." Unfortunately not enough detail is provided, and the structure of the paragraph in which it sits may strongly mislead the reader. An accurate description of the science involved may be found in Chapter 3 of the SOD, which states, "In the instrumental records, global summer monsoon precipitation intensity (measured by summer precipitation averaged over the monsoon domain) decreased from the 1950s to 1980s, followed by an increase". However this ES statement is inaccurate. Is the time scale referred to for the "likely increase" meant to describe "the mid-20th century", "the 1980s", or even the "1970s" which are all mentioned in the paragraph? The wording needs to be tightened up to avoid any misconceptions being given. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We improved the summary statement for global monsoon changes, related to the recent trends in global monsoon precipitation.
4613	5	48	5	48	Evidence points to very likely [ Andries Kruger, South Africa]	Noted. The available scientific evidence evaluated in the section points to likely.
3493	5	48	5	48	It is stated here, "Since the 1970s near-surface winds have likely weakened over land." As it stands this sentence is devoid of any meaning. Does it refer to winds in the global monsoon region (following the previous sentence about the global monsoon), or winds in general? [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The summary statement of section 2.3.1.4.4 refers to global surface winds.
23303	5	48	5	49	Wind speed is increasing over land rapidly and globally. It is very important to leave this info to policymakers. Zeng et al., 2019 NCC: Zeng, Z., et al. (2019). "A reversal in global terrestrial stilling and its implications for wind energy production." Nature Climate Change. [ Zhenzhong Zeng, China]	Noted. The summary statement of section 2.3.1.4.4 included the recent recovery in surface winds.
1951	5	51	5	52	medium confidence' for the storm track changes during the Medieval Warm Period seems too optimistic to me. I would use 'low confidence' as for the mid-Holocene (see Chapter 2). [ Hugues Goosse, Belgium]	Accepted. Confidence level has changed to 'Low'.
131503	5	52	5	52	"Stratospheric vortex" - explain here and, if the term is used across different chapters, consider adding it to the Glossary. [ Hans Poertner and WGII TSU, Germany]	Noted. Added in Glossary
81295	5	52	5	53	Does the exceptional Arctic vortex of 2020 have any implications for this trend or its level of confidence? [ Johannes Laube, Germany]	Noted. The boreal winter as a whole there is long-term weakening trend during the reanalysis period. There are strong vortex events in March 2019 and in later winter of 2019/2020. In Section 2.3.1.4.5 we have reported that "the northern polar vortex has strengthened during later winter"



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73871	5	55	5	55	Here again i would suggest to use undetected instead of unseen [ Dominique Raynaud, France]	Rejected. The heading is now changed but the term "unseen" is kept, since we find it fits well.
9919	5	55	6	16	Should one sentence about terrestrial snow cover be added? [ Olga Zolina, France]	Rejected. There was already a sentence included a few lines later ("Reduction in spring snow cover ..."). The wording is kept as it was.
70315	5		7		Many of the executive summary points for climatic changes are lost in these huge paragraphs. i.e., there is currently one paragraph that encompasses all ocean changes and includes changes in ocean state, circulation, SLR... Each of these have their own subsection within the chapter so it would make sense to simply separate these summary points. Changes in ocean heat content are also not reported and really should be. [ Shayne McGregor, Australia]	Rejected. The ES has been drafted in this manner to meet the style guidance presented by the Bureau and TSU, recommending the appropriate number and size of paragraphs.
26123	5		52		The comment concerning the Hadley Circulation strengthening. The authors of chapter 2 wrote at the pp.2-51, 2-52: "In summary, there has been a very likely widening of the Hadley Circulation since the 1980s, although there is only medium confidence in the extent of the changes. This has been accompanied by a strengthening of the Hadley Circulation, particularly in the northern hemisphere (medium confidence)". This point is also can be read at the p.2-5. The question is: Why the Hadley Circulation intensifies? It seems, a global warming (the most significant in the high latitudes of the Northern Hemisphere) should lead to the Hadley Circulation weakening because of the decreasing of temperature contrast between the Equator and the Northern Pole. If its intensity increases since the 1980s it may be due to the multidecadal natural climate variability. If so, it should be definitely specified. In fact, the intense multidecadal variations of the zonal wind in the low latitudes (as manifestation of the Hadley Circulation changes) are described a lot of times (see for instance, our paper Polonsky A.B., Krasheninnikova S.B., Basharin D.V. Interdecadal Variability of the Meridional Ekman Heat and Mass Transport in the North Atlantic and its Relation to the Atlantic Multidecadal Oscillation. Russian Meteorology and Hydrology, 2017, v. 42, Is.10, pp. 653-660 and references in this paper). [ Alexander Polonsky, Russian Federation]	Rejected - outside the scope of the chapter. Assessment of attribution is the purview of Chapter 3 and is not covered here.
6479	6	1	6	1	This sentence is incorrectly worded, as the first part has the subject "sea ice area" whereas the "it" in the second part refers to sea ice not "sea ice area". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; This is now changed to "sea ice" instead of "it".
15471	6	1	6	2	The decreasing trend of September Arctic sea-ice area is very significant: -74,000 km <sup>2</sup> yr <sup>-1</sup> (-15% relative to the 1981-2010 mean) (P.57, lines 8-10). Also, multiyear ice has nearly disappeared in 2018 (P.57, line 34-35). It is suggested to reflect these significant changes in the Arctic in the Executive Summary. [ SAI MING LEE, China]	Taken into account; Numbers for Arctic sea ice area decadal mean changes are now added. More information can be found in the cryosphere section (2.3.2) of chapter 2.
98911	6	2	6	4	Here it is very important to indicate how the analysis was done. If one is looking for a linear trend across the whole record, there is no significant trend. But this is all due to how the analysis was done. What has happened is that changes due to stratospheric ozone depletion drove a circulation change that led to a trend to greater sea ice extent and now global warming has come to the fore and is pushing the other way. I thus view the statement here as very poorly based--as unfortunate as all that clamor over an early 21st century cooling by starting one's trend line in 1997 and the major El Nino. It is the analysis approach that is the problem here, and to given this sentence "very high confidence is really misleading, suggesting a naive thinking that the atmosphere is responding to only one forcing, something the denier community does--it is simply irresponsible. This statement needs reworking and not just a description of no linear trend. [ Michael MacCracken, United States of America]	Noted; The sentence was now reworded, a comparison of decadal means is included, and the confidence level was adjusted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83163	6	2	6	4	This statement about overall Antarctic sea-ice coverage does not adequately capture either (1) the strong and contrasting regional trends in Antarctic sea ice extent AND seasonality, or (2) the recent unanticipated and somewhat baffling shift to first record maximum overall extents in 2012-2014 then record minima from 2016. It is important to highlight these factors here, as discussion of the trend in overall/net Antarctic sea ice coverage masks the fact that this is made up of distinctive regional and seasonal components. In this way, Antarctic sea ice differs distinctly from Arctic sea ice. SUGGESTED CHANGE: "Total averaged Antarctic sea ice coverage has experienced interannual and decadal variability but no significant trend for the period of continuous satellite observations (1979-2018) (very high confidence). This overall pattern is made up of contrasting regional and seasonal contributions, with an unanticipated shift to first record positive overall coverage from 2012 to 2014 then record negative coverage from 2016 (very high confidence)." NB I have added the term "coverage" as thickness is not known. [ Robert Massom, Australia]	Accepted; In the final FGD new decadal means are included. For the reason of space limitation more extensive information on the changes since 1979 are given in section 2.3.2. Moreover, Chapter 2 focuses at large/global scales, therefore regional aspects are typically not considered.
69559	6	6	6	6	"that, with few exceptions, glaciers" What are the exceptions? On short timescales, there are certainly exceptions, but I've looked at all glacier length records in the WGMS database that go back to 1900, every single one had retreated. The reference for this is LeClerq and Oerlemans (The Cryosphere, 2013). The one exception (Chungurchatchiran on Mt. Elbrus in the Caucasus) is a clear error in the dataset. Perhaps there are other observations out there? [ Gerard Roe, United States of America]	Rejected. The length limits do not allow to go into detail about this. Chapter 9 discusses for example the Karakoram anomaly.
57715	6	6	6	8	I think it is important to add to the summary that the anomaly is not only in the number of glaciers retreating, which is not surprising since it is known that most glaciers are not in balance with current climate conditions, but also in the rate of retreat, which is extraordinary. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The sentence is now reworded.
78831	6	7	6	7	change: the number of retreating glaciers [ MONICA TOLOTTI, Italy]	Taken into account; The statement is now reworded. The number of glaciers is not addressed here anymore.
78397	6	7	6	8	Unclear what this sentence means: "The number retreating is highly anomalous 8 in the context of the last 2000 years". Retreating glaciers? [ Hans W Linderholm, Sweden]	Accepted; This part is now reworded.
41547	6	7	6	8	Why not write about possible inversion in ice dynamic after LIA, with ice expansion until LIA and recession since for the faster reacting elements, but not the slower ones?...What about fast reacting systems and low reacting systems which would indicate the whole ice mass is still within a disequilibrium stage? Ice mass time constants are in part on century- millennial time range? [ Laurent Labeyrie, France]	Rejected; Adding of more detail here is not possible due to length limitations. Further, chapter 2 is not supposed to discuss details about processes. More information can be found in subsection 2.3.2.3, and regarding processes in chapter 9.
57721	6	7	6	8	The number of glaciers retreating is more likely not anomalous (e.g. Solomina et al., 2016), however there is considerable uncertainty in many proxies and glacier retreat is often poorly recorded. I am not sure evidence is sufficient here for the 'high confidence' qualifier. What is more certain is that the rate of glacier retreat is anomalous, that we likely have enough evidence for 'high confidence' or even 'very high confidence'. Number of glaciers retreating is not necessarily a useful statistic, as it may in fact be higher towards the end of cold periods than in extreme warm periods (as more glaciers are present to retreat). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; The statement is reworded now. The number of glaciers is not anymore addressed here.
86711	6	7	6	8	The sentence starting with "The number retreating..." could perhaps be simplified. Many readers can have difficulties understanding the number of glaciers (not even mentioned in the sentence) as an anomaly in a context. And an anomaly does not really tell if it is more or less than normal, just a deviation for normal. Also the sentence says nothing about when this anomaly is. If you are trying to say that many more glaciers have retreated recently than normal during the last 2000 years, just write that. Please don't overcomplicate. [ Oyvind Christophersen, Norway]	Taken into account; The statement is now reworded. The number of glaciers is not addressed here anymore. A sentence is added to address recent changing rates of glacier retreat.
126875	6	8	6	8	Since the 2000-year figure is driven by data availability, text should say "in the context of at least the last 2000 years". [ Trigg Talley, United States of America]	Accepted - revision made in ES and main text

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15473	6	8	6	9	The ice loss of the Greenland Ice Sheet in recent decade (2005-2015) is very significant: -273 Gt yr <sup>-1</sup> . It is suggested to reflect such significant change in the Executive Summary. Re: the rate of loss has increased four-fold since the turn of the 21st century. A recent study has shown that the Greenland Ice Sheet is losing mass even faster. Ref.: The IMBIE Team, Mass balance of the Greenland Ice Sheet from 1992 to 2018. Nature, (2019). Please consider incorporating the latest research result. [ SAI MING LEE, China]	Taken into account. Allusion to 4-fold has been removed.
26601	6	9	6	9	Units of time differ from FAQ 1.3, Figure 1 [ Eric Brun, France]	Unclear. Wrong chapter, page, line reference?
89365	6	11	6	13	I think that this sentence ("Mass balance ... since the 1990s") could be re-worded/shortened to help clarify, perhaps "Mass balance in eastern Antarctica has not significantly changed since the early 1990s, while mass loss in western Antarctic has accelerated 3-fold over the same time period." [ Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Noted; the statement is now reworded, and due to space limitations and potential overlap with chapter 9, West and East Antarctic ice sheet is not discussed explicitly here. More details are given on that in section 2.3.2.4.2.
83165	6	13	6	15	This statement about permafrost is somewhat ambiguous compared to the preceding statements about trends in other cryospheric components/elements. What is the trend in the areal coverage of permafrost? [ Robert Massom, Australia]	Noted. Permafrost temperature is an ECV and accepted indicator of change. Observations of permafrost extent over time do not really exist as it is a subsurface phenomena that can't be observed like other cryospheric components with remote sensing. Estimates of permafrost extent are obtained through modelling rather than observation (see chapter 9).
71153	6	13	6	15	It is important to note that permafrost degradation is not simply change in temperature. As shown by many studies (e.g. Smith et al., 2016), the increase in permafrost temperature is faster in cold conditions than in warm, discontinuous conditions. In warm permafrost, the ground ice melt, i.e. latent heat effect, start to become dominant and as such, permafrost degradation is partially a temperature increase, but also a phase change effect. This process is not well represented and explained in the document, but critical, in particular when it comes to risks to infrastructure and mass movements / geohazards. [ Lukas Arenson, Canada]	Noted. Agree with reviewer. However, chapter focusses on large scale observed change (rather than process and impact) and permafrost temperature is an ECV and indicator of change. Note section 2.3.2.5 also discusses evidence of permafrost thaw.
66407	6	13	6	16	Why this particular observation in the executive summary rather than, e.g., observed changes to active layer thickness or other permafrost thaw processes as discussed on page 65? These are more relevant to feedback processes or permafrost impacts than permafrost temperatures per se, so suggest focusing on that rather than this statement. [ Charles Koven, United States of America]	Noted. Focus is on evidence of changing climate rather than impacts of changing permafrost conditions. Permafrost thermal state is an accepted ECV and has been used in previous IPCC reports and other assessments. Increase in permafrost temperature is required for permafrost to thaw. Evidence of permafrost thaw is mentioned in section 2.3.2.5.
73301	6	14	6	14	Capital 'O' for 'oceans'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copy edit to be completed prior to publication
73303	6	15	6	15	Insert space between 30 and m. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Correction made
9921	6	18	6	18	"The current ocean state is unprecedented for centuries to millennia for some indicators" this sentence is formulated in a much generalized and not entirely clear manner. Which indicators of 'state' are assessed by this sentence? For instance, unlikely the ocean density has changed [ Olga Zolina, France]	Editorial. Text revised.
15917	6	18	6	19	This statement is vague:  "The global ocean has warmed since at least 1971 when globally representative measures are available."  More correctly:  "The oceans are main repository for the absorbed heat with approximately 70% of the heat being confined to the upper layers of the ocean which is the most bio-sensitive regions and also the interface region with much of the world's unstable methane clathrates, see Glecker DOI: 10.1038/NCLIMATE2915." [ Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81517	6	18	6	19	Recommend to revise 'The current ocean state is unprecedented for centuries to millennia for some indicators (high confidence)....', as the actual ocean state is not reflected. [ Ee Ling Lee, Malaysia]	Editorial. Text revised.
77199	6	18	6	19	Consider stating what indicators are used here rather than "ocean state" as it's not clear. [ Emer Griffin, Ireland]	Editorial. Text revised.
34835	6	18	6	32	The SOD claims that GMSL has risen faster in the last century than in the last 6k years. Please see rebuttal comment #6 above. [ Jim O'Brien, Ireland]	Taken into account. Text revised for clarity.
98913	6	18	6	32	I was very surprised that no numbers (with ranges) were even mentioned about sea level rise so readers could have a sense of how important the trends are. I would urge making the statements quantitative. [ Michael MacCracken, United States of America]	Editorial. Text revised.
99177	6	18			the sentence does not have a clear message, it basically states that some things have changed over some time intervals [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
35033	6	19	6	20	would be more precise to say "1971, the date since which globally representative measures are available [ W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
102717	6	21	6	22	Suggest replacing "accelerating" by 'increasing'. Rationale: an accelerating rate seems strange. Alternatively one could say GMSL rise is accelerating. [ Philippe Tulkens, Belgium]	Editorial. Text revised.
99179	6	22	6	23	again no clear message, sea level was higher and lower, so what does this imply [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text removed.
58111	6	22	69	8	AR5 reported with very high confidence that global mean sea level (GMSL) during the last interglacials, over thousand years between 5 and 10m higher than 1985-2004 (medium confidence). But on page 2-6 line 23 it is written as "GMSL is now higher than at least the last 6000 years and likely since the last interglacial". Justification or proof for the difference between AR5 and AR6 is required. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; statement about global mean sea level during the mid-Holocene has been extensively revised.
90923	6	23	5	24	Sentence GMSL...interglacial. This sentence does not make sense, [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; statement about paleo GMSL has been extensively revised.
8879	6	23	6	23	The statement that GMSL is now higher than in the least 6000 years is not supported by the literature; indeed, we cannot make this statement confidently even for the last 3kyr (see Kemp et al 2018). By contrast, we can make a strong statement about the rate of GMSL rise since 1900 being faster than over any comparable period since at least 1000 BCE (Kopp et al 2016, Kemp et al 2018), which therefore seems more appropriate to highlight in the ES. [ Robert Kopp, United States of America]	Taken into account; statement about GMSL during the mid-Holocene has been extensively revised.
105481	6	23	6	24	Sentence GMSL...interglacial. This sentence does not make sense, [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; statement about paleo GMSL has been extensively revised.
4503	6	23	6	24	Statement „GMSL is now higher than at least the last 6000 years and likely since the last interglacial.“ Sea level was actually higher in all oceans during the Holocene Thermal Maximum (HTM, i.e. pre-6000 years), except in the North Atlantic Basin. The statement therefore needs to be changed to: „GMSL is now higher than the last 6000 years, whilst it is likely lower than during the period 9,000-6000 years ago.“ [ Sebastian Luening, Switzerland]	Taken into account; statement about GMSL during the mid-Holocene has been extensively revised. Also, the reviewer seems to be confusing relative sea level and global mean sea level.
81297	6	24	6	25	To be correct and consistent with earlier notation his should be "fresh-get-fresher" and "salty-get-saltier". [ Johannes Laube, Germany]	Editorial. Text revised.
113089	6	24	6	25	Is it 'virtually certain that fresh go fresher and salty got saltier' if not, restructure sentence. [ Diego Miralles, Belgium]	Editorial. Text revised.
113091	6	25	6	25	The first 'saltier' should be 'salty' I guess. [ Diego Miralles, Belgium]	Accepted, thank you.
32893	6	25	6	26	The statement that direct observation show that AMOC has weakened since at least the mid 2000s is a bit woolly and not entirely consistent with statements elsewhere in the AR6 draft. Smeed et al. (2014) observed a decline 2004-2012, and with a longer time series Smeed et al. (2018) concluded that that it was in a reduced state 2008-2017 as compared to the earlier observations 2004-2008. [ Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: We agree, and we undertook refinement of the wording, and we have liaised with the other chapters to obtain coherence in the assessment outcome

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7795	6	25	6	28	I agree with these statements, however chapter 3 says an AMOC weakening over the historical period is medium confidence, and Chapter 2 P70 L37 says the SROCC has medium confidence of weakening. Also (for the weakening since the mid 2000s, chapter 9 (P5 L37) also has high confidence that this is natural variability - maybe include? [ Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, and we interacted with the corresponding chapters to obtain coherence in the assessment outcome
10961	6	25	6	28	Multidecadal variability is suggested to weaken confidence in AMOC trends over the 20th century, but it is likely to be an even greater issue for the shorter, post-2000 period mentioned in the previous line. [ Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Comment also considered for the assessment in 2.3.3.4.
89303	6	25	6	28	<p>I strongly propose to change this statement from low confidence to (at least) medium confidence, given the variety of different proxy evidence for a weakening AMOC which provides a highly consistent picture using different methods and different geographic locations, some local, some large-scale regional.</p> <p>For example the following studies have reconstructed the AMOC for the last millennium or more: Rahmstorf S, Box J E, Feulner G, Mann M E, Robinson A, Rutherford S and Schaffernicht E J 2015 Exceptional twentieth-century slowdown in Atlantic Ocean overturning circulation Nature Climate Change 5 475-80  Sherwood O A, Lehmann M F, Schubert C J, Scott D B and McCarthy M D 2011 Nutrient regime shift in the western North Atlantic indicated by compound-specific delta15N of deep-sea gorgonian corals Proc Natl Acad Sci U S A 108 1011-5  Spoonier P T, Thornalley D J R, Oppo D W, Fox A D, Radionovskaya S, Rose N L, Mallett R, Cooper E and Roberts J M submitted Exceptional 20th century ocean circulation in the Northeast Atlantic Geophysical Research Letters  Thibodeau B, Not C, Zhu J, Schmittner A, Noone D, Tabor C, Zhang J and Liu Z 2018 Last Century Warming Over the Canadian Atlantic Shelves Linked to Weak Atlantic Meridional Overturning Circulation Geophysical Research Letters 45 12,376-12,85  Thornalley D J R, Oppo D W, Ortega P, Robson J I, Brierley C M, Davis R, Hall I R, Moffa-Sanchez P, Rose N L, Spoonier P T, Yashayaev I and Keigwin L D 2018 Anomalously weak Labrador Sea convection and Atlantic overturning during the past 150 years Nature 556 227-30</p> <p>The latter with two independent proxies, making together six different reconstructions all finding a 20th Century AMOC decline that is unprecedented in at least a millennium.</p> <p>For the 20th Century there is further relevant papers providing evidence:  Caesar L, Rahmstorf S, Robinson A, Feulner G and Saba V 2018 Observed fingerprint of a weakening Atlantic Ocean overturning circulation Nature 556 191-6</p>	Taken into account. The proposed references (both which had been not already cited before, and which fall into cut-off dates), and those based on observations, have been added. Moreover, the confidence level has been changed to 'medium confidence'.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89305	6	25	6	28	<p>(continued)</p> <p>A key piece of evidence is the marked cooling in the subpolar Atlantic, shown in previous IPCC reports to be the only region on Earth with significant cooling. This cooling has been predicted by climate models as a response to anthropogenic warming and shown to be linked to an AMOC slowdown, no credible alternative explanation for this northern Atlantic "warming hole" has been advanced, and it has been shown to be of anthropogenic origin:</p> <p>Chemke R, Zanna L and Polvani L M 2020 Identifying a human signal in the North Atlantic warming hole Nature Communications 11 1540</p> <p>The fact that this cooling is just what models predict in response to an AMOC slowdown, the associated observed strong warming along the US east coast as additional part of the "fingerprint" of an AMOC slowdown, and the lack of an alternative explanation for these observed features should alone give us at least medium confidence for the reality of an AMOC slowdown, even without the multitude of proxy evidence listed above.</p> <p>I think a reasonable discussion could be held whether the consistent results with a variety of independent proxy methods, models and attribution studies provide "high" or "medium" confidence about an unprecedented 20th Century AMOC weakening, but "low confidence" is not reasonable. Perhaps it is because several of these studies have apparently not been considered by the chapter authors.</p> <p>To do justice to its assessment role, the AR6 should include a graph with a compilation of these proxy series, all of which show a highly consistent picture. An example of such a graph was presented in August 2019 at the International Conference on Paleoclimatology and is found here: <a href="https://twitter.com/rahmstorf/status/1176873455473614848?s=20">https://twitter.com/rahmstorf/status/1176873455473614848?s=20</a></p> <p>A more comprehensive intercomparison is presented in a manuscript by Caesar et al., currently in review at Nature Geoscience. [ Stefan Rahmstorf, Germany]</p>	Taken into account. This section does not discuss the 'warming hole' in particular. In-line with the reply above: the confidence statement has been changed to 'medium confidence'.
39157	6	25	6	28	What are method uncertainties? Further, quoting 6.7 of the SROCC, ("The SROCC concluded that there is medium confidence that the AMOC has weakened over the historical era, but there is insufficient evidence to quantify a likely range of the magnitude of of the change, does this update negate this finding? [ Lourdes Tibig, Philippines]	Taken into account. We agree on the unclear wording and changed accordingly. Moreover, we revised after cross-chapter exchanges.
39159	6	25	6	28	Please see lines 42-44 in page 71. The statements say direct observations show a weakening of the AMOC beginning around 2005-2008 (high confidence), and that the records are too short to indicate whether there is a significant trend. Very ambiguous! [ Lourdes Tibig, Philippines]	Thank you for the comment, noted.
80443	6	26	6	26	calibrated language should be consistent with CH09 (e.g. p. 9-28/29). "Since 2004, the strength of the AMOC has been measured at 26.5°N by the RAPID Array (Smeed et al., 2018) (Section 2.3.3.4). The short length of the record and the surprisingly large variability compared to CMIP models (Roberts et al., 2014) gives low confidence in a meaningful long-term trend from the array even though Smeed et al. (2018) argue that between 2007 and 2011 the AMOC shifted to a state of reduced overturning." [ Samuel Jaccard, Switzerland]	Taken into account, and cross-chapter discussions performed to seek coherency.
18747	6	26	6	26	Since the weakening of AMOC has been observed over only the last 10-13 years, the cause of the weakening need not be climate change. It could be due to internal variability. [ Govindasamy Bala, India]	Noted. This has been specified in section 2.3.3.4, page 71, l. 43-45. However, ES statement has been revised to improve clarity.
99181	6	26			can the weakening be quantified to improve the statement, is it outside the normal variability? [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, this is provided in section 2.3.3.4, page 71, l. 43-45
90925	6	27	5	28	large ...variations. This part is unclear. Should it be superimposed? [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The wording superposed is correct.
105483	6	27	6	28	large ...variations. This part is unclear. Should it be superimposed? [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The wording superposed is correct.
69801	6	28	6	28	Confidence of pH decrease is "high"? Why not "very high"? Lack of observation in some regions? [ Kaoru Kubota, Japan]	Noted. There is more information available in section 2.3.4.1, and confidence statement build on the publications available.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99183	6	28	6	29	why is there very high confidence in pH but only high confidence in T when there is more data and proxy understanding for the latter? [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The confidence statement build on the publications available. However, reviewers comment is not clear as there is no mention on 'T' (temperature?) in text L28-29.
93363	6	28	6	30	I would say 'It is virtually certain that the global-mean surface ocean pH has declined since the early 20th century, and there is very high confidence that global-mean surface pH values as low as today have not been experienced in the last two million years. In fact, I like better the way this is summarized in page 73, line 56: "it is virtually certain that the global open ocean and all ocean basins individually have experienced a decline in surface pH since the early 20th century." [ Carles Pelejero, Spain]	Taken into account. The summary statement has been revised also due to other review comments.
102719	6	29	6	30	The statement 'there is very high confidence that surface pH values as low as today have not been experienced in the last 2 million years' is a key message and it should be lifted to the SPM. [ Philippe Tulkens, Belgium]	Taken into account, and the message is transferred to the SPM authors.
73305	6	31	6	31	Change 'kilometer' to 'kilometre'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, thank you.
57545	6	32	6	32	Subsection "2.3.4.1" on Ocean pH is missing in the references. It should be added since changes in ocean pH values are discussed in this paragraph. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, thank you. Note that numbering has changed through reorganisation, and has been adopted accordingly.
132159	6	34	6	43	This is a very useful paragraph. But I don't understand why chapter 2 is not including a paragraph of similar length describing specific changes in climate over land (temperature, lakes, soil moisture, heat storage), which are clearly distinct from those in the biosphere. [ Sonia Seneviratne, Switzerland]	Accepted. We now split the terrestrial and ocean biosphere into two distinct ES paragraphs.
99185	6	34	6	43	the impact of environmental change on biota is not in the remit of this working group. The messages are vague "indicators have changed" does not say much. Range changes are exceptional compared to what? Overall the message is repeating the 1.5 and the SROOC without novelty [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Chapter 2 is charged with assessing a broad range of observational evidence on the changing state of the climate system, including the biosphere.
19689	6	35	6	36	Possibly the evidence supporting this statement is present in section 2.3.4, but I could not find it! [ philippe waldteufel, France]	Taken into account - text revised (sentence deleted).
98915	6	38	6	39	Regarding the statement that greenness indicators have increased. Is this referring to global average greenness, or intensity of greenness where it is green (I don't suppose it is greener over the Sahara, etc.). This statement needs to be much more precisely stated as there are also areas of increasing aridification, changes in the timing and duration of greening, etc. This is just far too generally stated. [ Michael MacCracken, United States of America]	Taken into account - combined with comment 109565.
39161	6	38	6	39	To what finding does the "high confidence" apply? The increase of vegetation greenness indicators? [ Lourdes Tibig, Philippines]	Taken into account - text revised (confidence statement moved to the end of the sentence to reduce ambiguity).
109589	6	39	6	39	Odd phrasing. Seems more accurate to say that greening is the indicator of green leaf area and photosynthetic activity given that satellites actually measure spectra (i.e., colour/greenness). Greenness is what's actually being measured and isn't the only factor that affects photosynthesis. Suggest rephrasing to the simpler and more direct observation: "There is high confidence that since the onset of global observations in the 1980s, vegetation greenness has increased indicating greater leaf mass/area." [ Anthony Walker, United States of America]	Taken into account - combined with comment 109565.
81191	6	39	6	39	After the sentence "greenness indicators... activity have increased" the following statement may be added - 'as a result of carbon fertilization effect'. [ Supriyo Chakraborty, India]	Rejected - attribution is outside the scope of the chapter.
80445	6	40	6	41	Recent high-resolution paleoceanographic evidence suggests that the AMOC has remained remarkably stable throughout the Holocene even when affected by large-scale, transient freshwater perturbations (Lippold et al., 2019 (GRL)). [ Samuel Jaccard, Switzerland]	Taken into account. AMOC assessment has been completely revised.
102721	6	41	6	43	The key message (on phenological changes) is to be lifted to the SPM. [ Philippe Tulkens, Belgium]	Noted with thanks.
57547	6	43	6	43	Considering that both subsection 2.3.4.1 and 2.3.4.2 are covered in the above paragraph, it could be instructive to replace {2.3.4} with {2.3.4.3, 2.3.4.4, 2.3.4.5, 2.3.4.6}. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The structure of the sections has been reorganised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4505	6	45	6	45	Modes of variability have been reconstructed also for the past few millennia and exerted an important influence on pre-industrial climate. Even though not all reconstructions of ENSO, AMO, NAO, PDO, SAM agree, there is a consensus view on the development of these “modes of variability” beginning to form which helps to better understand the interaction and relationship with pre-industrial and in fact modern climate. The paragraph in the SOD downplays this important field which is probably key for better understanding pre-industrial climate change that is much more variable than suggested in this AR6 Chapter 1+2 SOD. Please keep in mind that it is just a matter of time that these significant pre-industrial climatic changes are fully documented and understood in terms of climate factors. It is not a good strategy to pretend there was hardly any pre-industrial changes in climate. This takes us back to the infamous era of the hockey stick discussion which was unrealistic and scientifically not sustainable. [ Sebastian Luening, Switzerland]	Accepted. Assessment of the change in the modes of variability based on reconstructions for the past few millennia in the final FGD have been enriched including also and additional figure.
76809	6	45	6	55	This statement is not consistent with recent palaeoclimate assessments of the IOD and ENSO. See Freund et al., 2019, Nature Geoscience (10.1038/s41561-019-0353-3); Grothe et al., 2019, GRL (10.1029/2019GL083906); Abram et al., 2020 Nature (10.1038/s41586-020-2084-4); Abram et al., 2020 Quaternary Science Reviews (10.1016/j.quascirev.2020.106302). In the case of the IOD there are multiple lines of evidence (palaeoclimate, observations, models) that show an increase in positive IOD events during the second half of the 20th Century that is projected to continue. For ENSO, coral data indicates that ENSO variability in the past 5 decades is significantly stronger than anytime during the past 7000 years, and that the frequency of central Pacific-type events is now higher than anytime in at least the last 400 years. [ Nerilie Abram, Australia]	Accepted. The recent paleoclimate assessments of the IOD and ENSO including the suggested literature have been incorporated in the assessment.
83593	6	45			<p>Changes in modes of variability</p> <p>A shortcoming of this Executive summary is that there is no reference to natural cycles of climate change (other than decadal and annular modes of atmospheric and ocean currents). Centennial and millennial cycles are reported in a significant body of literature and AR6 cannot be complete without reference to these. There may be reasons to argue against their significance but simply ignoring them is not sound science.</p> <p>A few useful references are</p> <p>Babich, V.V., A. V. Dar'in, I. A. Kalugin, and L. G. Smolyaninova, , 2016, Climate Prediction for the Extratropical Northern Hemisphere for the Next 500 Years Based on Periodic Natural Processes, Russian Meteorology and Hydrology Vol. 41 No. 9</p> <p>Cabedo-Sanz, P., Belt, S. T., Jennings, A. E., Andrews, J. T., and Geirsdóttir, Á. (2016). Variability in drift ice export from the Arctic Ocean to the North Icelandic Shelf over the last 8000 years: A multi-proxy evaluation. Quat. Sci. Rev. 146, 99–115. doi:10.1016/j.quascirev.2016.06.012.</p> <p>Ole Humlum , Jan-Erik Solheim , Kjell Stordahl, 2011, Identifying natural contributions to late Holocene climate change, Global and Planetary Change 79 (2011) 145–156</p> <p>Lüdecke H-J and , C.O.Weiss, 2017, Harmonic Analysis of Worldwide Temperature Proxies for 2000 Years. The Open Atmospheric Science Journal, 11, 44 -53.</p> <p>Scafetta, N., 2013, Discussion on climate oscillations: CMIP5 general circulation models versus a semi-empirical harmonic model based on astronomical cycles, Earth-Science Reviews 126 (2013) 321–357</p> <p>Scafetta N., Milani F., Bianchini A., Ortolani S. (2016). On the astronomical origin of the Hallstatt oscillation found in radiocarbon and climate records throughout the Holocene, Earth-Science Reviews, Vol. 162, pp. 24-43. DOI: 10.1016/j.earscirev.2016.09.004</p> <p>And dare I add a series of four items from 4 years of EGU presentations:</p> <p>Asten, M.W., 2017, Phase relations of natural 65 year SST variations, ocean sea level variations over 260 years, and Arctic sea-ice retreat of the satellite era – issues of cause and effect, Geophysical Research Abstracts, Vol. 19, EGU2017-9833, EGU General Assembly 2017.</p>	Taken into account. The cycles are addressed to some extent in "orbital drivers" and in recognition of glacial-interglacial cycles in CCB2.1. Understanding the meaning of trends, variability, and any underlying periodicity in climate indicators and climate forcings can be limited by the length of their observational time series, especially for components of the climate system that respond and operate over multi-decadal and longer time scales.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70253	6	47	6	50	This sentence is very difficult to read. Maybe "Outside of the SAM, which has become systematically more positive, other modes of climate variability such as El-Niño Southern Oscillation (ENSO) have exhibited fluctuations in mean state, frequency and amplitude at interdecadal timescales and show no sustained trends since the late 19th century." [ Shayne McGregor, Australia]	Noted. Edits made to clarify.
17081	6	47	6	50	I suggest these changes: Modes of climate variability, such as El-Niño Southern Oscillation (ENSO), have exhibited fluctuations in mean state, frequency and amplitude at inter-decadal timescales. However, it come but with the exception of the Southern Annular Mode, which has become systematically more positive and shows no sustained trends since the late 19th century. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in redrafting this text.
26599	6	47	6	50	This is a long and difficult sentence to read. Should be cutted. [ Eric Brun, France]	Taken into account in redrafting this text.
67657	6	47	6	51	One mode that is missing here is the QBO...it now has been disrupted twice, once a few years ago, and again now. Although detailed papers have not been written, it should be notes as another climate anomaly. [ Karen Rosenlof, United States of America]	Rejected. Discussion within the chapter and across the other chapters about the inclusion of QBO into assessment resulted in overall agreement to retain the present structure.
77201	6	47	6	51	What are the confidence levels on the Southern Annular mode statement? [ Emer Griffin, Ireland]	Accepted. A confidence statement is now included for the Southern Annular Mode.
77203	6	47	6	51	Reformulate for clarity [ Emer Griffin, Ireland]	Noted and edited for clarity
7145	6	47	6	51	I feel this sentence awkward. May be separate in two sentences the idea that the climate modes have exhibited fluctuations and that the Souther Annular mode have exhibited a systematic positive sign. [ Nicolas Kolodziejczyk, France]	Taken into account in redrafting this text.
105077	6	47	6	55	There is evidence for ENSO variations with past climate changes (e.g. the recent paper of <a href="https://www.clim-past-discuss.net/cp-2019-155/cp-2019-155.pdf">https://www.clim-past-discuss.net/cp-2019-155/cp-2019-155.pdf</a> , where references based on paleodata and models can be found) [ Masa KAGEYAMA, France]	Accepted. Evidence for ENSO variations with past changes is now better discussed.
73307	6	55	6	55	Change 'like' to 'such as' (poor English) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Addressed in edits.
73309	7	1	7	1	Capital 'O' for 'oceans'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial addressed in copy edits.
98917	7	3	7	3	While the longer term variability just described is interesting, what is really important with respect to observed changes are changes in extreme weather events, to tropical cyclones, extreme heat waves, extreme seasonal heat, etc. That these points seem to be avoided is just not acceptable as they relate to very important societal vulnerabilities and even limited indications need to be described so the public can have a sense of what is likely to lie ahead. [ Michael MacCracken, United States of America]	Rejected. Changes in extreme weather events, including tropical cyclones, extreme heat waves, extreme seasonal heat, etc are discussed in later regional chapters. Chapter 2 is concerned with trends in large-scale climate.
5513	7	35	8	25	In th abstracts, why dfo you presnet only the results of the RCP8.5 scenario [ Benoit Laignel, France]	Comment clearly not pertaining to chapter 2 and thus unactionable.
81049	8	1	8	1	This section would be more complete if there was a section on Biosphere where some of the biospheric indicators of ch2 could be included, perhaps others, as well as a place to show that the CO2 snks are also growing over time due to CO2, something that is now missing. Land CO2 sinks are driven by biospheric responses so it would naturall fall here too; unless addressed in a comment below. [ canadell pep, Australia]	Taken into account. There is a biosphere changes section so it is unclear what is being requested here. We have tried to strengthen the biospheric key findings arising from the ES.
132161	8	3	8	6	It is important that Chapter 2 provides an update not only up to 2019, but also to 2020 for some key indicators (e.g. mean warming on global scale, on land, in the ocean and over sea ice points). [ Sonia Seneviratne, Switzerland]	Taken into account. For all indicators possible the assessment has been updated through 2019. For specifically surface temperature and sea ice changes the assessment has been updated through 2020.
19691	8	8	8	20	Let me emphasize that an introduction to chapter X is not an adequate place to describe what is done in chapters Y≠X. Accordingly, lines 11-14 should in my opinion not be there. Similarly, lines 17-18 are not quite relevant: a chapter's value is based by its own contribution rather than the support it brings to other chapters. On the other hand, it would be interesting to learn about the selection process mentioned in line 8. [ philippe waldteufel, France]	Rejected. Given the matrix nature of the AR6 assessment it is necessary to inform the reader where material they may have expected to find in the present chapter may instead be found.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24353	8	11	8	13	This sentence is rather awkwardly phrased. It would sound better if the word “considering” came after “Chapter 10 through 12”, as follows: The adopted AR6 structure differs substantially from that in AR5, with Chapters 5 through 9 considering key climate processes, and Chapters 10 through 12 considering regional aspects and extreme events in a holistic manner that integrates observations, models, theory and projections (Section 1.8). [ Owen Cooper, United States of America]	Editorial. Addressed in edits.
71859	8	11	8	13	Delete this sentence - not relevant to this chapter. [ John Church, Australia]	See response to comment id 19691
18617	8	12	8	14	Discussion of regional chapters here is a good opportunity to underscore climatic impact driver approach that spans WGI. Could include "... and Chapters 10 through 12 and the Atlas regional aspects and extreme events in a holistic manner that integrates observations, models, theory and projections suitable for impact and risk applications." [ Alexander Ruane, United States of America]	Taken into account. Given that many other comments suggested that this text should be deleted we prefer to rely upon the cross-reference to chapter 1 here.
41549	8	18	8	21	Felicitations for the clarity of the paragraph, IPCC ambition for this chapter needs a special attention to the synthetic quality of the writing! Take it as a model [ Laurent Labeyrie, France]	Noted with thanks.
132167	8	23	8	27	Figure 2.1: Useful figure. But not including land in the structure of the report makes it disappear for the Earth System's components. This not conceptually acceptable, when one component is called "oceans" and a full IPCC special report was dedicated to "Climate change and land"- [ Sonia Seneviratne, Switzerland]	Taken into account. Land components are drawn out more clearly in several parts of the text but no changes in this regard are made to the figure.
126877	8	30	8	37	This paragraph should be written in a much more clear way using simpler language. [ Trigg Talley, United States of America]	Noted. Lack of suggested revisions makes this unactionable in specific terms.
6481	8	31	8	31	I am not convinced that "reanalysis data are used to the extent possible". For example, 1980-2018 trends from JRA-55 and ERA5 could have been included in Table 2.4. This would have introduced more independence, and would have highlighted further that the quoted 1980-2018 trend from HadCRUT5 is an outlier. In the FOD there was a map of trends from several datasets, including one reanalysis. Here, in Figure 2.11 of the SOD, we have a map of the trend only from HadCRUT5, despite it being an outlier according to Table 2.4. Further examples could be cited. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have replaced with the simpler 'where applicable' and relied more upon a substantive assessment undertaken in chapter 1 which we have contributed to in the FGD.
73311	8	32	8	32	The use of the term 'deep past' could be misleading. Geologists refer to 'deep time' or the 'deep past' to refer to time scales closer to the age of the Earth (effectively billions of years). I suggest here that the term is substituted for by 'the past 55 million years' which seems to be the period being referred to, and more immediately tells the reader what is going on. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Edits made for clarity.
7497	8	32			What is the meaning of “modern era” in the following sentences?: “...deep past through to the modern era is applied to ...” (Chapter 2, page 8, line 32) / “The modern era reanalyses exhibit SLP increases ...” (Chapter 2, page 55, line 21) / “... extent value in the modern era occurring in ...” (Chapter 2, page 59, line 5) / Figure 2.33 (Chapter 2, page 187) / “...most closely resembles the more modern era. Multiple ...” (captions of Figure 3.22, Chapter 3, page 44, lines 43-44”. This term is not present in Chapter 1, Chapter 9 and Annex II. Is Modern era in all these sentences a synonym of Present, as defined in Annex II, page 3, line 32? [ Alejandro Cearreta, Spain]	Accepted. Replaced with easier to understand 'recent past'
112395	8	34	8	34	There are other periods with data such Miocene and Oligocene, but may be not enough data coverage and/or man power to synthesize. So the review is not just limited by data availability. [ Feng Ran, United States of America]	Taken into account. The text is clarified here in edits to the paragraph more generally. Changes have also been made to Cross-chapter box 2.1 although as the reviewer notes we cannot consider all potential periods.
36937	8	36	8	36	State what it is evidence of. (If this had already been stated the comments of reviewers might have been different.) [ John McLean, Australia]	Editorial. The sentence goes on to do precisely this.
19693	8	39	8	40	The risk is that this paragraph be interpreted as a challenge to download AR5 (in such cases please indicate you are talking about WG1) in case one wants to learn how trends are estimated. [ philippe waldteufel, France]	Taken into account. This is consistent with the charge that we build upon prior assessment reports.
112397	8	43	8	43	The word resource is mistyped. [ Feng Ran, United States of America]	Rejected. The word recourse is correct here.
23853	8	47	8	47	There are a variety of uncertainties...' - Please fix plural vs. singular. [ Branko Grisogono, Croatia]	Editorial. Intent is to be plural.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73313	8	47	8	47	Replace 'are' with 'is' (better English). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	See response to comment ID 23853
19503	8	48	8	48	after climate change add " impacts" [ Hamideh Dalaei, Iran]	Rejected. The chapter charge is not to consider impacts.
32655	8	48	8	48	Add "impact" after "climate change" [ sadeh zeyaeeyan, Iran]	See response to comment ID 19503
126879	8	48	8	48	"sources" is unclear. Suggest "uncertainties" or "sources of uncertainty". [ Trigg Talley, United States of America]	Accepted. Edits made for clarity.
32985	8	48	8	48	Add "impact" after "climate change" [ Sahar Tajbakhsh Mosalman, Iran]	See response to comment ID 19503
105485	8	48	8	49	For example ...uncertainties. This sentence is unclear and should be rephrased. [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. How this sentence is unclear is not stated and thus comment is not actionable.
126881	9	3	9	4	Contrary to the text here, the Glossary contains fewer details about the types of uncertainty relevant to Chapter 2, not more. [ Trigg Talley, United States of America]	Accepted, Table 2.1 is deleted, details are in the Glossary
40879	9	3	9	4	Table 2.1: "More details can be found in the glossary". These terms are not in the SOD glossary. Should they be added? [ TSU WGI, France]	Accepted, Table 2.1 is deleted, details are in the Glossary
19695	9	3	9	4	About uncertainty, the glossary mentions as a first cause the imprecision on the data. Does not this make sense? [ philippe waldteufel, France]	Accepted, Table 2.1 is deleted.
10433	9	3	9	7	"Trend" : Some reference should be made to how an autoregressive character basically assumes a very simple interannual internal variability model (see box 2.2 in Hartman et al 2013 which describes the regression framework as a linear trend + noise - in this case the residual of the regression predominantly represents internal variability). This has consequences for interpretation of the trend uncertainty, which may be unintended. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have made minor edits to clarify this issue here.
29797	9	5	9	6	Please evaluate whether it would be relevant to include in Table 2.1 the uncertainty derived from the use of different techniques in data collection and processing over time (e.g., new technologies, algorithms, etc.). [ Hernan Edgardo Sala, Argentina]	Taken into account. This is covered under the various terms already present in the table.
126883	9	5	9	6	Comment on Table 2.1: The definition of observational uncertainty is meaningless; it claims to include almost all other types of uncertainty so as written it's almost a synonym for uncertainty. Say what distinguishes it, such as by adding "...prior to synthesis of observations." [ Trigg Talley, United States of America]	Taken into account. Have removed this table
3495	9	10	11	27	This is a very useful cross-chapter box and it is good to see reference made to monsoon systems within the discussions of the various reference periods. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
99187	9	13			there is a geographic bias in the authorship here which raises concerns about the breadth of the overview and consensus [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; authorship expanded
111083	9	13			Glacier discussion: this is nice if a bit brief. But I think starting the discussion for the glaciers in 1850 is a bit tricky as the latter was a peak advance - after which they would retreat. Maybe worth citing the recent paper by Stefan Broennimann on the glacier advance then retreat for the advance for context (apologies for two things a) self serving and b) i have now half an hour to finish typing up comments so cant find the exact location where it would sit well in body of chapter. sorry Broennimann S., Franke J., Nussbaumer S., Hegerl G. Schurer A. et al (2019): Last phase of the Little Ice Age forced by volcanic eruptions NATURE GEOSCIENCE 12 650-Pub [ Gabriele Hegerl, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; publication cited in CH1 with discussion regarding prelude to 1850 reference period. Attribution of temperature changes is out of scope for CH2.
78833	9	18	9	23	change in L 18: several selected periods; change in L23: are included [ MONICA TOLOTTI, Italy]	Accepted; as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41551	9	18	9	43	suggestion: distinguish the timing of the external forcing factors (continental drift “tipping points” (opening or closing of ocean gateways, mountain ranges apparition and continental surface control of atmospheric dynamic...), astronomical forcing... vs internal “non climatic” response/amplification factors (isostasy, erosion, carbon sequestration)... and vs climatic responses ( $\geq 1$ kyr versus $\leq 100$ yr) : ice dynamic (accumulation/erosion) ocean dynamic (surface vs deep, interhemispheric...) atmosphere dynamic, albedo and carbon feedback... Present redaction emphasize some processes, but do not take in consideration others which are as important in the context of the report. The interaction of feedbacks with multi time constants is the most difficult to apprehend and modelize, and that point is not sufficiently taken in account in what I read. ( this will be true also when considering anthropogenic forcing in the context of natural evolution from longer time processes (to be referred to a specific point) [ Laurent Labeyrie, France]	Noted; expanded text on causes of climate state, but textbook account of paleoclimate forcing is out of scope.
102723	9	21	9	21	"programed" should be "programmed" [ Philippe Tulkens, Belgium]	Accepted; as suggested
29525	9	22	9	22	unnecessary space following the parenthesis before citation of Lunt et al. 2017 [ Kevin Burke, United States of America]	Editorial; copyedit to be completed prior to publication.
29799	9	22	9	22	Delete space after the opening parenthesis in "( Lunt et al.". [ Hernan Edgardo Sala, Argentina]	Editorial; copyedit to be completed prior to publication.
69803	9	22	9	22	Delete space before "Lunt et al., 2017" [ Kaoru Kubota, Japan]	Editorial; copyedit to be completed prior to publication.
1967	9	22	9	22	there is a space before "Lunt". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication
93365	9	22	9	22	Delete space before Lunt [ Carles Pelejero, Spain]	Editorial; copyedit to be completed prior to publication
113099	9	22	9	22	Correct '( L' [ Diego Miralles, Belgium]	Editorial; copyedit to be completed prior to publication.
109005	9	22	9	22	remove space between bracket and 'Lund' [ Belen Martrat, Spain]	Editorial; copyedit to be completed prior to publication.
1971	9	22	9	23	LGM and mid-Pliocene and Last Millenium are also Tier 1, I believe. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; changed to "most are Tier 1"
57549	9	22	9	24	Literature citation recommendation: Otto-Bliesner et al. (2017). The PMIP4 contribution to CMIP6 - Part 2: Two Interglacials, Scientific Objective and Experimental Design for Holocene and Last Interglacial Simulations. Geoscientific Model Development. 10, pp.3979–4003. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; Kageyama et al. is the overview article that includes all PMIP time periods. Adding LIG/Holocene paper would be unbalanced.
126885	9	22		24	The two most recent interglacial periods (mid-Holocene and *Last Interglacial*) are *included* as Tier 1 experiments under CMIP6, so the paleoclimate simulations are directly comparable to those of 20th century and future climate simulations. [ Trigg Talley, United States of America]	Accepted; as suggested.
24355	9	23	9	23	"include" should be "included" [ Owen Cooper, United States of America]	Accepted; as suggested
29527	9	23	9	23	change "include" to "included" [ Kevin Burke, United States of America]	Accepted; as suggested
52103	9	23	9	23	Should be "are included" [ Kathryn Fitzsimmons, Germany]	Accepted; as suggested
126887	9	23	9	23	"are include" should be "are included". [ Trigg Talley, United States of America]	Accepted; as suggested
1969	9	23	9	23	"include" should be "included". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; as suggested
93367	9	23	9	23	Included [ Carles Pelejero, Spain]	Accepted; as suggested
105665	9	23	9	23	"...are include(d) as Tier 1 experiments under CMIP6" The midHolocene and lig127k (last interglacial) are PMIP tier 1 experiments, with PMIP the MIP/activity_id that is considered as a sub-MIP of the CMIP6 phase. The structure of CMIP6 differs from previous versions, as experiments (experiment_id) are sponsored by a MIP (PMIP) which are then endorsed by the CMIP6 project. No experiment is directly sponsored by CMIP6, as the CMIP/DECK activity_id sponsors the CMIP standard experiments amip, abrupt4xCO2, historical etc, see <a href="https://wcrp-cmip.github.io/CMIP6_CVs/docs/CMIP6_experiment_id.html">https://wcrp-cmip.github.io/CMIP6_CVs/docs/CMIP6_experiment_id.html</a> [ Paul Durack, United States of America]	Accepted; change to "PMIP4 Tier 1"
71627	9	23	9	23	Line should read, 'are included as Tier 1' rather than 'are include as Tier 1' [ Jessica Hargreaves, Australia]	Accepted; as suggested
83951	9	23	9	23	replace include by included. [ Marco Tulio Cabral, Brazil]	Accepted; as suggested
93487	9	23			change "include" to "included" [ Rahab KINYANJUI, Kenya]	Accepted; as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83953	9	25	9	25	it would be helpfull to indicate at least in which section of Chapter 3 this information can be found. [ Marco Tulio Cabral, Brazil]	Accepted; as suggested
126889	9	25		27	Which simulations? The mid-Holocene and Last Interglacial? The other geologic times have forcing other than radiative included so this should be specific about which time is being talked about or include feedbacks other than radiative. [ Trigg Talley, United States of America]	Noted; added "and boundary conditions".
83415	9	28	9	29	"Materials from other natural archives" is very vague phrasing for non-expert readers. I recommend to list a few examples such as ice cores, speleothems etc. or refer to Chapter 1.3.2 [ Antje H. L. Voelker, Portugal]	Accepted; referred to section in CH1 for list of proxy types
36939	9	32	9	32	You've forgotten the Medieval Warm Period, evidence for which can be found around the world. (See <a href="http://www.co2science.org/subject/m/subject_m.php">http://www.co2science.org/subject/m/subject_m.php</a> for a collection of articles about findings in various parts of the world.) You do mention the Little Ice Age and there is also a collection of articles about it too, at <a href="http://www.co2science.org/subject/l/subject_l.php">http://www.co2science.org/subject/l/subject_l.php</a> . I notice that the Medieval Warm Period is mentioned in Box 2.1 Table 1, which makes the failure to mention in the text introduction even more suspicious. [ John McLean, Australia]	Taken into account; revised section; LIA and MWP are defined in Glossary. But these periods have been deprecated in AR6 WG1 FGD.
126891	9	32	9	32	Word missing. Should be "Earth's temperature history" or "Earth's temperature record". [ Trigg Talley, United States of America]	Accepted; as suggested
1973	9	32	9	33	Cenozoic is not defined and this is the first time it appears in this chapter. Give the start and end dates. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; as suggested
1975	9	34	9	34	I really like this figure!! Especially the "24-hour" axis (although this could be better explained in a legend/caption). The caption should make clear how the conversion from e.g. d18O in Zachos et al or LR04 is converted to GMST. Given that the curves fit the assessed ranges exactly at the EECO, Pliocene, LIG etc, I assume that the curves have had some "correction/stretching" applied to match at these points. If so, this should be stated clearly in the caption. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; origin of GMST reconstruction stated in caption; 24:00 clock omitted in response to other comments.
459	9	34	9	36	In this statement on the cause of long-term cooling over the most recent tens of millions of years, it could be informative to add the collision of India with southern Asia and the resulting formation of the Himalayas and extensive chemical weathering of rocks (taking carbon from the atmosphere). [ Claire Parkinson, United States of America]	Taken into account; text includes 'drift of continents .. controlled the flux of carbon...'
126893	9	34	9	38	As written, this text incorrectly implies that ice sheets didn't develop in Antarctica until 3 Mya. [ Trigg Talley, United States of America]	Taken into account; revised section
112399	9	35	9	36	For cooling during the Cenozoic, weathering of rocks may also play an important role not just the ocean flux. [ Feng Ran, United States of America]	Taken into account; text includes 'drift of continents .. controlled the flux of carbon...'
100591	9	36	9	36	Insert: "(2) a reversal in global temperature trends around 16 million years ago associated with a moderate increase in pCO2 and major decrease in ice volume," [ Matthew Kohn, United States of America]	Rejected; not all reference periods are mentioned in this brief overview. See table for information on individual periods
126895	9	36	9	38	"(2) a shift to lower temperature around 3 million years ago, as climate feedbacks involving ice albedo and greenhouse gases caused ice sheets to *expand* in both hemispheres". Ice sheets certainly already existed in Antarctica and Greenland likely had some ice prior to 3 million years ago. 'Develop' seems like the wrong word choice. [ Trigg Talley, United States of America]	Taken into account; revised section
461	9	36	9	38	As written, this sentence makes it sound like there was a shift to lower temperatures about 3 million years ago, after which the ice sheets of both hemispheres started to develop. However, the Antarctic ice sheet had developed well before then. I suggest revising the sentence to something along the lines of: "the long-term cooling led to the development of the Antarctic ice sheet about 30-35 million years ago and to northern hemisphere ice sheets about 3 million years ago, in both cases affected by climate feedbacks involving ice albedo, snow albedo and greenhouse gases." [ Claire Parkinson, United States of America]	Taken into account; revised section
45295	9	36	9	39	Point (2) is a bit confusing because permanent Southern Hemisphere ice sheet developed a lot earlier than 3 million years ago (Eocene-Oligocene Transition; ~34 Ma). Only the permanent NH ice sheet was developed around 3 million years ago, which drove an intensification of ice age cycles [ Anson Cheung, United States of America]	Taken into account; revised section
112401	9	37	9	37	vegetation albedo also plays an role in driving the ice sheet variability [ Feng Ran, United States of America]	Taken into account; added 'land cover'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1977	9	37	9	38	This implies that Antarctic ice sheet expansion is a primary driver of global cooling in the Pliocene, which I don't think is supported by evidence. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; revised section
99189	9	37			The shift to colder temperature is relative of course but the last 3 M is not the beginning of the cooling nor its acceleration. Why was this time chosen? [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; revised section
102725	9	38	9	38	The word "developed" should be preceded by "be" i.e. "be developed" [ Philippe Tulkens, Belgium]	Taken into account; revised section
6483	9	38	9	38	"developed" should be "develop". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; revised section
73315	9	38	9	38	Replace 'developed' with 'develop' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; revised section
126897	9	38	9	38	"to developed" should be "to develop" [ Trigg Talley, United States of America]	Taken into account; revised section
1979	9	38	9	38	"developed" should be "develop" or "be developed" [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; revised section
113101	9	38	9	38	to developed' [ Diego Miralles, Belgium]	Taken into account; revised section
93489	9	38			change "developed" to "develop" [ Rahab KINYANJUI, Kenya]	Taken into account; revised section
7489	9	40			In the same way, the sentence "... glacial maximum to the present interglacial period (Holocene), with ..." should be better as "... glacial maximum to the present interglacial epoch (Holocene), with ..." [ Alejandro Cearreta, Spain]	Accepted; as suggested
463	9	41	9	41	"minor cooling since the middle of the current interglacial period" makes it sound like the cooling extends to the present. One possibility for avoiding this misrepresentation would be to change "since the middle of" to "within". [ Claire Parkinson, United States of America]	Taken into account; revised sentence
133	9	41	9	42	The line "minor cooling since the middle of the current interglacial period, culminating in (6) the Little Ice Age, a centuries-long period of heightened volcanic activity" seems to suggest that the Little Ice Age has 1) a link with the orbitally-driven cooling trend since the middle of the current interglacial period; and 2) was caused by increased volcanic activity. I do not know of any proof of (1) and the role of increased volcanic activity in driving the LIA (2) is still highly debated. Indeed on page 10 (lines 12-16) it is described how small the impact of solar irradiance was and similarly on page 13 (lines 18-23) it is described how small the impact of volcanic forcing was on the LIA in comparison with the MWP. Why is the possibility that internal climate dynamics drive these centennial-scale and spatially heterogeneous temperature changes not mentioned? A nice overview of research in this direction is given in the discussion section of Ljungqvist et al. (2019; Journal of Climate volume 32) [ Pepijn Bakker, Netherlands]	Taken into account; revised text; Assessment of LIA no longer included.
45293	9	41	9	42	I thought Little Ice Age was also a period marked by periodic substantial reduction in solar sunspots (e.g. Maunder and Dalton Minimum) [ Anson Cheung, United States of America]	Taken into account; revised text; LIA no longer included.
5625	9	41	9	42	a better way to put this is that the neoglacal cooling consisted of a long-term trend with superimposed oscillations, the Little Ice Age cooling, Mediaeval Warm Period warming, the Dark Ages cooling, Roman Warm Period, etc. I thought the LIA was not due to volcanoes only? [ Konrad Gajewski, Canada]	Taken into account; revised section
4507	9	42	9	42	The idea of the Little Ice Age (LIA) being caused by the clustering of volcanic eruptions is hard to defend. In reality, the second half of the Medieval Climate Anomaly (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. See Sigl et al. 2015, <a href="https://www.nature.com/articles/nature14565">https://www.nature.com/articles/nature14565</a> [ Sebastian Luening, Switzerland]	Taken into account; revised text; LIA no longer included.
126899	9	42	9	42	There needs to be a reference for the assertion that volcanoes caused the LIA. Is this a widely accepted explanation, or just one model study? [ Trigg Talley, United States of America]	Taken into account; revised text; LIA no longer included.
73317	9	43	9	43	Change 'gasses' to 'gases' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; as suggested
73873	9	43	9	43	greenhouse gases [ Dominique Raynaud, France]	Accepted; as suggested
30071	9	43			'gases' for homogeneity with the rest of the chapter [ Gilles Delaygue, France]	Accepted; as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83067	9				Table 2.1. I think it would be helpful to give some concrete examples in this table. For example, I would think that "structural uncertainty" would depend somewhat on choice of horizontal grid (geometry and resolution) and gridding method. Where would choice of climatology period sit in this table? I don't see any explicit mention of data quality control procedures or bias correction schemes. I think it would be good to mention these aspects somewhere (even if they are hard to quantify). [ Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Some effort at clarification has been made
4509	10	1	10	1	Table 1. It is not true that the HTM was most pronounced in the Northern Hemisphere. It is equally well described from the southern Hemisphere. Furthermore, the "global HTM" does not peak at 6.6k. This is probably from Marcott et al. 2013 which however is mostly based on heterogenous (mostly) coastal sea surface temperatures which are not really representative of the global land and ocean temperature evolution. [ Sebastian Luening, Switzerland]	Taken into account; "HTM" not included as a paleo reference period for reasons explained in text.
6485	10	1	10	11	The definition of the mid-Holocene could be reworded. How can it be defined as the middle of the present interglacial when it is not known when the present interglacial will end? [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted; 6 ka will be the approximate middle of the current interglacial period for a few more millennia, at which point, either a new epoch will have been designated, or this detail will be forgiven.
10403	10	1	10	15	Figures 1.27 and 2.11 don't show evidence of a "Medieval Warm Period". It appears to be warmer in the centuries before 950CE. The "MWP" also seems to be cooler than the start of the 20th century. What possible reason is there to continue using this misleading term? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; MWP no longer included as a paleo reference period. Instead, it is replaced by the last millennium, consistent with PMIP.
10407	10	1	10	15	On the inaccurate "Little Ice Age" term: The continued association of volcanic and solar forcing factors with the period, ignores growing evidence for the role of anthropogenic factors on the relatively small climate changes in the 15-18th centuries (e.g., Owen et al, The Maunder minimum and the Little Ice Age: an update from recent reconstructions and climate simulations, J. Space Weather Space Clim. 2017. ). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; MWP no longer included as a paleo reference period. Instead, it is replaced by the last millennium, consistent with PMIP.
57551	10	1			Cross-Chapter Box 2.1, Table 1: It would be instructive to have a few seminal literature citations outlined within the table under "Sketch of the climate state" for each reference period in addition to "AR6 sections". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; key modelling papers cited in table; table caption refers readers to sections elsewhere in the report for additional references.
57553	10	1			AR6 sections for Last Interglacial: Section 9.4.2.1 is on "Recent observations and model evaluation" for Antarctica and does not seem to contain the seminal literature citations for the climate state of LIG. Section 9.4.3.1 does not seem to exist in Chapter 9. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; updated call-outs to sections in other chapters.
40057	10	2	10	9	Cross-Chapter Box 2.1, Table 1: The glossary includes all these periods, except for 'Early Eocene climatic optimum'. Suggest to add an entry for that. [ TSU WGI, France]	Accepted; glossary entry for EECO now included.
19697	10	2	10	11	CCB 2.1, table 1; it is surprising that this table does not locate the time span covered by the Pleistocene. Fortunately, it is clearly shown on the figure associated to CCB 2.1 [ philippe waldteufel, France]	Noted; the table features paleo reference periods that are modelling targets for PMIP and other paleoclimate modelling initiatives. Pleistocene shown in Figure 1 with this Cross-Chapter Box.
81007	10	2	11	26	Is it possible to include realtive sea level rise estimates for the geological periods where a CO2 atmospheric concentration is mentioned? [ Jeffrey Philip OBBARD, Singapore]	Taken into account; sea level during each reference period is listed in figure 2.33, as stated in CCB2.1 Table 1 caption.
126901	10	4			In mid-Pliocene warm period (MPWP) section: "Warm period when atmospheric CO2 concentration was similar to now" is vague. Specify an estimate or similar to current anthropogenic levels. Can use the CO2 level in the stack simulations. [ Trigg Talley, United States of America]	Taken into account; changed "now" to "present", a period defined in CH1
112403	10	9	10	9	I found the definitions of Holocene thermal Maximum and mid-Holocene very confusing. In NOAA site, mid-Holocene is called mid-Holocene warm period. Holocene thermal Maximum has a "maximum" focus. I found the phrase "Holocene Climate Optimum" less confusing for this period. [ Feng Ran, United States of America]	Taken into account; Holocene thermal maximum omitted from table (both local and global versions).
36941	10	9	10	9	The statement in the table about the EECO being similar to the discredited RCP 8.5 should be removed because it false implies that RCP8.5 has credibility and is likely. [ John McLean, Australia]	Rejected; credibility of scenarios and range of projected CO2 values associated with each is outside of scope of CH2. The call-out to CH4 refers readers to RCPs.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17779	10	9	10	9	Cross-Chapter Box 2.1, Table 1: Suggest using MCA as primary term for Medieval climate instead of MWP. MWP suggests globally coherent warming comparable to today, which current evidence does not support. Also, MCA is more adequate to also include hydroclimate anomalies during this period. [ Raphael Neukom, Switzerland]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP. MWP and MCA are defined in the Glossary.
21193	10	9	10	9	"Ecological communities changed composition and shifted ranges" for the PETM sounds like a rather insignificant issue. The PETM, however, is tightly connected to the most significant biotic changes on land and in the sea since the recovery from mass extinction at the K/T boundary (syntheses in McInerney and Wing, 2011 - Annual Reviews; Speijer et al., 2012 - Austr.J.Earth.Sci). I'd suggest to rephrase this as: " Strong impact on ecology and evolution of marine and continental biota" or something similar. [ Robert Speijer, Belgium]	Taken into account; added "deep-sea species went extinct". The suggested revision, "impact on ecology..." is not sufficiently specific.
83417	10	9	10	9	Since you highlight correctly for the EECO that the continents were in a different position than today, I would mention for the MPWP that the continents were in a position similar to today because non-expert readers will not be aware of this "shift" in boundary conditions. [ Antje H. L. Voelker, Portugal]	Taken into account; MPWP continental geography described in more detail in CCB2.4. Also, Miocene Climate Optimum added with a statement about a step toward modern geography.
83419	10	9	10	9	Here you provide an age range for the LIG, whereas before you only referred to it with one age = 125 ka (which is not even a mean age). You should specify here also why 125 ka is chosen to be representative for the LIG. [ Antje H. L. Voelker, Portugal]	Noted; previous references to 125 ka were based on literature as cited in text.
100593	10	9	10	9	Insert: "Miocene climatic optimum (MCO) 16.9–14.7 Ma Prolonged warm period with atmospheric CO2 concentrations 400-600 ppm, similar to RCP4.5 and RCP6 mid-century projections, and RCP4.5 end-of-century projection. Continents were in broadly similar but not identical locations. At times, Arctic sea ice may have been absent, and the Antarctic ice cap was up to 80% smaller or perhaps even absent. [ Matthew Kohn, United States of America]	Accepted; Miocene climate optimum added as a paleoclimate reference period in Cross-Chapter Box 2.1
100595	10	9	10	9	Note: There are two liverwort estimates of 700 and 2000 ppm, but with large error: see Fletcher et al. (2008; Nat Geo); Paleosol estimates are highly erratic, and the paleo-pCO2 group (led by Bärbel Hönisch, Columbia University) has removed many estimates for insufficient documentation. [ Matthew Kohn, United States of America]	Taken into account. The assessment of CO2 changes is elsewhere in the chapter.
100597	10	9	10	9	Note: Paleosol estimates are highly erratic - best suited to changes in pCO2, but not absolute pCO2 [ Matthew Kohn, United States of America]	Taken into account. The assessment of CO2 changes is elsewhere in the chapter.
99335	10	9	10	10	Cross-Chapter Box 2.1, Table 1. While there has been a consensus within the development of CMIP that the Mid-Holocene was a useful pre-industrial period for comparison between data and models I would suggest there is low confidence that this period was the warmest of the pre-industrial within the Holocene. Firstly there are uncertainties on any absolute temperature proxies, as many respond to other factors such as precipitation, amount effects, water chemistry, source water, training set uncertainties etc. This means, much of the Holocene variability before the 18thC is within proxy errors. Furthermore, there are many records where the proxies indicate the thermal maximum could be in the early Holocene ~8-10 ka BP and not the mid Holocene. There are many examples of this from continental archives [ Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Noted; GMST during the mid-Holocene is assessed in the text. Agreed that many records show highest temperatures prior to the mid-Holocene, but global average highest temperature appears to have been later.
54537	10	9	10	10	In table 1, column "Sketch of the climate state (comparisons relative to pre-industrial)", row "Little Ice Age (LIA)", the last sentence states "Coldest decades generally coincide with more frequent volcanic activity and low total solar irradiance.". The Toohey and Sigl (2017) reconstruction of volcanic activity doesn't support the assertion that volcanic eruptions were more frequent during the LIA, only that they were of greater magnitude, releasing more sulfur into the stratosphere than other periods. [ Matthew Toohey, Canada]	Taken into account; LIA omitted as reference period in favour of last millennium as per PMIP.
23855	10	9	10	10	Box 2.1: please fix the English in '...temperatures were several degrees colder...' - Temperature cannot be colder or warmer but only lower or higher. [ Branko Grisogono, Croatia]	Accepted; as suggested



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78399	10	9	10	10	Why MWP and not MCA? I would think that given the spatiotemporally varying warming patterns during this period, it is more valid to use the Medieval Climate Anomaly (MCA). I would state it the other way round. [ Hans W Linderholm, Sweden]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP. MWP and MCA are defined in the Glossary.
73319	10	9	10	10	Box 2.1 Table 1: I suggest inserting 'Asian' before 'Monsoon' in the LIG box line, for clarity (other monsoons are available!). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; multiple monsoon systems were enhanced.
73321	10	9	10	10	Box 2.1 Table 1: insert 'the' before 'Medieval' in the MWP box line. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; table revised.
29801	10	9	10	10	Use capital letters at the beginning of each word in "last deglacial transition (LDT)", Cross-Chapter Box 2.1, Table 1. [ Hernan Edgardo Sala, Argentina]	Rejected; last deglacial transition is not a formally defined period.
105079	10	9	10	10	Cross-chapter box 2.1, table 1. This table is missing the period of large glacial instabilities (~60 to 30 kysrs ago) which is highly relevant for studies of rapid climate changes, on time scales of years to decade, partly related to changes in AMOC. These can be seen on Cross-chapter Box 2.1, Figure 1 (although this figure is showing global temperature changes, which are not the best measure of glacial abrupt changes for which the responses over different regions partly cancel out). [ Masa KAGEYAMA, France]	Noted; table focuses on targets used by PMIP for model comparison. CH9 includes information on rapid changes during the Quaternary.
52105	10	9	10	10	Suggest clarification for non-specialists regarding relative changes in climate states, e.g. by "warmer" does one mean "warmer than present" or "warmer than the preceding and subsequent periods" [ Kathryn Fitzsimmons, Germany]	Taken into account; column heading specifies, "comparisons relative to pre-industrial time".
52107	10	9	10	10	Should be "Last deglacial transition", "Mid-Holocene", "Post-glacial", "Mid-Pliocene warm period". Remove comma after "also known as (Medieval Climate Anomaly)". Add comma after "globally heterogeneous" (two instances) [ Kathryn Fitzsimmons, Germany]	Accepted; as suggested
126903	10	9	10	10	Comment on Table 1 of Cross-Chapter Box 2.1, mid-Holocene section: The word "perturbations" is meaningless here; every period had perturbations relative to every other period. Change to "differences in" and the sentence would imply "differences compared with pre-industrial", which is apparently what is meant. [ Trigg Talley, United States of America]	Taken into account; changed "perturbations" to "shifts" relative to pre industrial.
105667	10	9	10	10	The acronyms and terminology described here do not directly align with CMIP6 experiment names and definitions, as this is an obs-based chapter that is not a major problem, but it does introduce inconsistencies that may trip over an unfamiliar reader [ Paul Durack, United States of America]	Taken into account; simplified experiment names to agree with those in CH3.
465	10	9	10	10	Skipping from the EECO all the way to the MPWP seems to leave out too much. Perhaps add a row for the late Eocene, Oligocene, Miocene, and early Pliocene, 49 - 3.3 Ma, long-term overall cooling. [ Claire Parkinson, United States of America]	Taken into account; added Miocene Climate Optimum.
467	10	9	10	10	In the mid-Holocene row, 6,500 - 5,500 years ago, I suggest removing "and before the onset of major industrial activities" from the first sentence, given that it was thousands of years before the onset of major industrial activities. Eliminating those words makes the sentence fit much better with the subsequent sentence mentioning the "pre-industrial" period. [ Claire Parkinson, United States of America]	Accepted; as suggested
99191	10	9			the information across the climate state column (3) is not consistent and raises questions why this information was chosen. Biotic information is given for the PETM but not other events, why? [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; additional information on biosphere changes now included for other reference periods, along with call-out to WGII report.
99193	10	9			PETM the extinction of deep sea species not mentioned but a major characteristic of this event, no reference to terrestrial changes [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; additional information on biosphere changes now included for other reference periods, along with call-out to WGII report.
2507	10	9			For the LGM time slice you might also want to add a reduced communication between the ocean's interior and the surface/atmosphere, resulting in enhanced oceanic CO2 sequestration [ Thomas Ronge, Germany]	Accepted; as suggested
2509	10	9			For the LDT you might also add: increased overturning circulation [ Thomas Ronge, Germany]	Accepted; as suggested
30073	10	25			'selected' ('select' means of 'high quality') [ Gilles Delaygue, France]	Accepted; as suggested
2009	10	33	27	33	I think that this paragraph is missing a summary assessment of the LGM GMST change...or maybe signpost here that the summary statement in section 2.3.1.1.4 is coming up later. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; summary statements moved from section 2.3.1.1.4 into sections where the underlying text is first presented.

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26603	10		10		CROSS-CHAPTER BOX 2.1, TABLE 1 : LGM age might be 26-19 ka considering Clark P.U. et al., 2009, Science. How this age range (21-19 ka) has been defined? [ Eric Brun, France]	Taken into account; updated Glossary. LGM evidence reviewed in CH2 and in CH9 converge on 23-19 ka for peak glacial conditions from multiple archives.
26605	10		10		CROSS-CHAPTER BOX 2.1, TABLE 1 : Mentioning "plant communities" for LGM is OK but it should include the bio-community as a whole (i.e. from zooplankton -as shown through reconstruction of foraminifera assemblages for example- to plant communities, even humans?). [ Eric Brun, France]	Taken into account; added additional information about biosphere changes during paleoclimate reference periods.
26607	10		10		CROSS-CHAPTER BOX 2.1, TABLE 1 :The fact that ice-sheets gradually melted/disappeared during LDT should be added. [ Eric Brun, France]	Accepted, added "decreased cryosphere"
26609	10		10		CROSS-CHAPTER BOX 2.1, TABLE 1: It could be useful to define Pleistocene in this Table [ Eric Brun, France]	Noted; the table features paleo reference periods that are modelling targets for PMIP and other paleoclimate modelling initiatives. Pleistocene shown in Figure 1 with this Cross-Chapter Box.
115953	10		10		Please add to ccb 2.1 a description of the primary cause of variation for the past periods. I am not convinced by how the Holocene "thermal maximum" is defined. The notion of "global Holocene thermal maximum" is somehow misleading (given the spatial pattern of corresponding reconstructed temperature anomalies) (potential confusion with "global" warming). We used careful wording for these periods in AR4 and AR5. Check coherency of the description of the LIA (linked to irradiance minima and frequent major eruptions) with chapter 1 (suggesting that the LIA is driven by the recurrence of eruptions only). [ Valerie Masson-Delmotte, France]	Taken into account; (1) Expanded and standardized explanation of "characteristic climate forcing" for each period. (2) Omitted "global Holocene thermal maximum" in favour of "mid-Holocene" reference period. (3) Omitted LIA as reference period in favour of PMIP last millennium transient target.
99337	11	1	11	22	Cross-Chapter Box 2.1, Figure 1 is an odd construction that seems to significantly underestimate the variability in climate in the Early Holocene, to at least 8 ka BP. The use of large combined data sets of varying chronological quality, with different dating methods and approaches and proxies that are recording different elements of the environment, driven by many more factors than simply temperature is going to average out much of the important variability we know that exists from the best resolved and dated records, with the most sensitive proxies. We know that in the Greenland Ice core records there are significant shifts in climate at 11.4, 11.1 (NGRIP), 10.7, 9.3, and 8.2 ka BP. Many of these, in particular the 11.4 and 8.2 events are widely recognised. The choice to use an averaged data set for most of the Holocene but ice core records alone for the preceding glacial seems doubly odd. It implies that polar ice cores alone for the last glacial can on their own infer global climate. There is significant ongoing research to understand how the pattern of climate variability seen in the polar records is expressed away from the higher latitudes, and away from oceans and oceanic influenced regions. I fully understand what this figure is aiming to achieve but it is a misleading representation of the state of the science. Would it not be better to compile a stacked figure that represents as much of the state of the art knowledge as is present at the moment. For example for the last glacial, as well as the ice cores there are mid latitude records from speleothem archives, or long continental lakes that can also be shown. For the Holocene, would it not also be advisable to show the best high resolution data sets that are sub-centennially resolved (ice cores and varved lakes, with fast responsive proxies only (chironomids, isotopes) to show evidence of variability as well as the averaged trend. From the 12k paper it s clear that a significant amount of variability is lost and while much is probably local 'noise' there is good evidence from the highest resolution records that there is important variability that should be reported. [ Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; time series of site-level climate variables (proxy and instrumental) are always more variable than global averages. This figure presents the currently best-available estimates for global averages. Ice core data have been replaced with marine data for consistency, as pointed out by reviewer. Tendency for proxy records to smooth out variability is noted in text.
132169	11	1	11	22	Cross-chapter box 2.1, Figure 1: Excellent figure! [ Sonia Seneviratne, Switzerland]	Noted.
17083	11	3	11	17	The sentences " GMST is relative to ... to new product in FGD]." is not directly related to any bars, lines, axes, or markers in the graph. I sugested to put in the text paragaf instead on Figure comments. Other sentences are acceptable as Figure comment. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
26061	11	5	11	8	Please consider adding between brackets the color code used for temperature projections based on the different SSPs and extended RCPs scenarios. [ Don Alfonso Pino Maeso, Spain]	Accepted; as suggested

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29529	11	13	11	13	Citation of Raymo et al., 2018 is incorrect. This data is from: Lisiecki LE, Raymo ME (2005) A Pliocene-Pleistocene stack of 57 globally distributed benthic d18O records. <i>Paleoceanography</i> 20:PA1003. [ Kevin Burke, United States of America]	Accepted; as suggested
29531	11	13	11	14	rendering of the greek delta symbol for d18O is not present [ Kevin Burke, United States of America]	Editorial; copyedit to be completed prior to publication
81299	11	13	11	14	The isotopic “delta” symbol seems to have not worked here. [ Johannes Laube, Germany]	Editorial; copyedit to be completed prior to publication
13217	11	13	11	14	Unknown symbol before 180. [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication
69805	11	13	11	14	Typo (180) [ Kaoru Kubota, Japan]	Editorial; copyedit to be completed prior to publication
1981	11	13	11	14	d18O symbol is corrupted. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication
9923	11	13	11	14	Square signs appear in these 2 lines [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication
30075	11	13	11	17	1. replace ‘delta’ symbols; 2. drop parenthesis before Andersen; 3. drop parenthesis before Jouzel; 4. update reference to Kaufman et al. (also in the reference part of the chapter) [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication
2511	11	13			The delta sign in d18O here and in the next line is not displayed correctly [ Thomas Ronge, Germany]	Editorial; copyedit to be completed prior to publication.
73323	11	20	11	20	The text in this line ('along the base...2300.' does not make sense. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; Cenozoic clock omitted.
83957	11	20	11	22	Start the day (Time markers) at 00:00 and advancing up to midnight, so that it reads from past to present. [ Marco Tulio Cabral, Brazil]	Taken into account; Cenozoic clock omitted.
26611	11	21	11	21	We suggest to add that the final hour of the ‘Cenozoic day’ is the onset of the Pleistocene. [ Eric Brun, France]	Taken into account; Cenozoic clock omitted.
29539	11	22	11	22	Please include citation in figure legend attributing the original publication. E.g. Figure design adapted from Burke et al., 2018. Full Citation: Burke KD, Williams JW, Chandler MA, Haywood AM, Lunt DJ, Otto-Bliesner BL. 2018 Pliocene and Eocene provide best analogs for near-future climates. <i>Proc. Natl. Acad. Sci. USA</i> 115, 13 288–13 293. (doi:10.1073/pnas.1809600115) [ Kevin Burke, United States of America]	Taken into account; figure extensively revised.
77205	11	30	11	40	Include material on the energy balance here to frame this section but avoid detail continued in later chapters [ Emer Griffin, Ireland]	Noted. The 2.2 introduction has been modified.
30607	11	32	11	35	To be consistent with the rest of the sentences in this paragraph and Chapter 6, SLCFs should be mentioned here, instead of short-lived GHGs and aerosols. [ Hong Liao, China]	Accepted. We mention SLCF and chapter 6 in the revised paragraph.
19701	11	32	11	38	This does evidently not mean that other chapters should not be mentioned. They should definitely be mentioned in cases where the reader is naturally eager to obtain an information, and the structure of the report is such that this information is located elsewhere. For example, a surprising behaviour of CH4 concentration is related in subsection 2.3.2.2, and the reader will of course want to hear about possible causes. Then the text should indicate (and indeed it does) where the reader is likely to get answers. Same remark for the changing growth rate of n2O. [ philippe waldteufel, France]	Accepted. We added further references to other chapters where needed.
81463	11	33			need to use 'such as' instead of 'including' [ Kyaw Moe Oo, Myanmar]	Editorial; copyedit to be completed prior to publication.
30077	11	35	11	36	i find it odd that the corresponding ERFs are reported in this chapter, which, in my understanding, should only address changes in “forcing agents or drivers”. ERF is completely described in Chapter 7. Radiative forcing, and even more ERF, are complicated metrics: i find that they are not correctly addressed before Section 2.2.8 (see comments below). Especially, the starting points for calculating ERF values are often missing (1750, 1850, ‘pre-industrial period’). [ Gilles Delaygue, France]	Rejected. It is in Chapter 2 scope to report on ERF. Numbers are Taken from Chapter 7.
19699	11	36	11	37	as a general rule, distribution of domains among other chapters ought to be left to the introductory chapter. [ philippe waldteufel, France]	Rejected. While we agree in general, the cross-chapter nature of observations, is causing confusion to the readers, and some guidance is needed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81301	11	37	11	38	This is not correct. Many minor CFCs, HCFCs and other gases seem to be missing completely (-113a, even though it's referred to in the text: Adcock et al., 2018; -114a: Laube et al., 2016; -124: Simmonds et al., 2019; -133a: Vollmer et al., 2015; -31: Schoenenberger et al., 2015; isoflurane, desflurane, and sevoflurane: Vollmer et al., 2015; SF5CF3: Sturges et al., 2012; c-C4F8O: Vollmer et al., 2019; n-C4F10, n-C5F12, n-C6F14, i-C6F14, and n-C7F16: Droste et al., 2020). It appears that the choice of gases here (and especially in Annex V) is not entirely driven by radiative impact, but also by what the three dominant networks are measuring. I strongly suggest to either completely eliminate compounds with radiative forcings of less than a minimum value (e.g., 1 mW m-2) or to consider a fairer representation of the peer-reviewed literature (e.g., in the Annex, as in AR5). [Johannes Laube, Germany]	Accepted. We include in the chapter only components with forcing >0.001 wm-2; with two duly motivated exceptions.
36943	11	41	12	30	Other than TSI and (very briefly) UV, this section of text ignores other solar factors such as solar magnetism (which seems to be reflected in sunspot numbers) and solar particle flow. [John McLean, Australia]	Taken into account. The reviewer is right that solar magnetism drives the solar impacts on the Earth's climate. For more details, the reader is referred to Chapter 7.
112893	11	41	42	50	<p>Section 2.2.1 and Figure 2.2a-b discuss the solar forcing adopted in the GCM models. The section discusses several studies showing that there is great disagreement among solar scientists regarding the amplitude of the solar variability during the last decades and centuries. Despite the issue is open and still debated, the text paradoxically concludes that only the TSI records that show a small secular variability should be trusted, and in the end, the TSI forcing proposed by Matthes et al (2017) should be the one used to in the GCM. The entire argument is baseless and partisan because Matthes et al (2017) TSI model is based on an average between the NRLTSI2–NRLSSI2 and SATIRE, which are the ones that show the smallest TSI secular variability, and are not representative of the various TSI reconstructions.</p> <p>In particular, section 2.2.1 fails to notice that the performance of both NRLTSI2–NRLSSI2 and SATIRE models have been carefully analyzed against the actual TSI satellite measurements from 1980 to 2016 in Scafetta et al (2019) and these models were found severely inadequate in reproducing the multidecadal TSI variability which is driven by the quiet solar region. Note that both proxy TSI records take into account only the active solar regions (mostly sunspot and faculae) but do not have proper mechanisms to deal with the slow varying solar variability due to the changing of the quiet solar region that covers most of the solar surface.</p> <p>Moreover, contrary to the statement at line 6 of page 12 "Stronger variations cannot be ruled out completely (Egorova, T. et al., 2018; Karoff et al., 2018; Reinhold et al., 2019), but there is currently no evidence to suggest such changes have happened over the last 9 ka.", the text fails to acknowledge that the same can be said regarding the absence of such large variations. There is no evidence that solar activity has remained nearly constant over the last 9ka.</p> <p>For example, if such TSI variation were so small as the text suggests, results showing a strong correlation between solar variability and past climate change during the Holocene such as those of Steinhilber et al (2012), Hoyt and Schatten (1997), Neff et al. (2001), Scafetta 2012 and of many others would not be explained.</p> <p>Finally, the text also fails to acknowledge the evidence coming from the studies on the luminosity</p>	Taken into account. In light of the reviewer comment, we now add additional references to demonstrate the assessment in the SOD (essentially unchanged in the FGD) is correct.
81465	11	41			add Insolation in Solar and Orbtal forcing = Insolation (Solar and Orbital forcing) [Kyaw Moe Oo, Myanmar]	Rejected. It is preferred to keep the formulation that is explicit in what is meant.
57555	11	41			"2.2.1 Solar and Orbital forcing" subsection is not referenced within the 'curly brackets' in the Technical Summary subsection: TS.4.2.1.2 Natural forcing (solar, volcanic aerosols). The subsection should probably be added. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
4615	11	43	11	51	Changes in TSI to be provided in time -dependent units. Presnetly difficult to ascertain the time period over which the changes occurred. [Andries Kruger, South Africa]	Rejected. This is a mistake by the reviewer. Changes are discussed as absolute changes, not as trends.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111913	11	43	11	51	One could expect here something on relation between the solar and volcanic activity effects as discussed in the Section 1.2.1.2. (p.14, l. 23-24, see previous comment) [ Tomas Halenka, Czech Republic]	Rejected. It was impossible to find the statement in 1.2.1.2 the reviewer refers to. Chapter 1, p14 l23-24 is about the Paris agreement, the same spot in Chapter 2 about CO2.
10429	11	46			The incorrect date range has been given for Maunder Minimum. It should be 1645-1715 (Eddy, Science, 1976) [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Corrected as suggested by the reviewer.
1983	11	47	11	47	"sun was very quiet" is a bit informal?! Maybe "when solar activity was at a minimum" ? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
105669	11	47	11	47	"..sun was very quiet.." quiescent may be a better word selection [ Paul Durack, United States of America]	Editorial; copyedit to be completed prior to publication.
4513	11	47	11	51	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. Why are you not discussing this enigma here? Do we trust theoretical models more than the climatic reconstructions? Are thousands of papers wrong that documented a clear solar imprint on climate that cannot be reconciled with the extremely small forcing that IPCC assumes for solar activity changes? It is about time to actively address this "elephant in the room". I am disappointed that this chapter ignores this issue. Another missed opportunity. Why are you not discussing potential UV amplification effects on climate though stratosphere-troposphere interaction? Why do you not mention that solar effects on climate are likely non-linear and modulated through "modes of variability" namely multidecadal and shorter cycles such as AMO, PDO, NAO, SAM, ENSO? [ Sebastian Luening, Switzerland]	Taken into account. The author has a point with his statements. But the discussion of climate models and their evaluation is not the topic of Chapter 2, but of Chapter 3.
10435	11	47			I think you should consider using a different phrase to "very quiet". It assumes the reader will know that a "quiet" Sun is less active, has less sunspots and has lower TSI. They might also expect a "noisy" Sun at some point! [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
108295	11	48	11	48	best estimate of radiative forcing for the period of 1750 to 2011 was "0.05 to 0.10 W m <sup>-2</sup> " is somewhat different from Chapter 7 page 47 line 29 [ Won-Tae Kwon, Republic of Korea]	Rejected. We clearly refer to AR5 here, all our opening paragraphs report what was concluded in AR5.
10437	11	50	11	51	Is there a need to mention this here? No evidence of an effect (7.3.4.5) so how can they be "quantified"? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We now only write it is "discussed" but omit the "quantified and assessed" statement.
1985	11	51	11	51	Give section number instead of just "Chapter 7" [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
126905	11	53	11	57	Should there be a comment on the size of the summer insolation changes (e.g., 83 W/m2 over the past MY) relative to the size of radiative forcing (e.g. 2 W/m2)? [ Trigg Talley, United States of America]	Rejected; comparing global/annual with regional/seasonal can be misleading. RF since pre-industrial time is covered elsewhere.
14883	11	55	11	55	Is it really Läske? Isn't it rather Laskar? [ Marie-France Loutre, Switzerland]	Accepted, fixed
26613	11	55	11	55	Inappropriate reference. Should read "Laskar et al., 2011". [ Eric Brun, France]	Accepted, fixed.
19703	11	55	11	55	Are you sure about this Läske reference (title is " Total magnitude superior to bulge magnitude as Black Hole mass predictor")? [ philippe waldteufel, France]	Accepted. The reference is corrected. doi:10.1051/0004-6361/201116836
4617	11	55	11	56	Big difference should be explained as it can be misinterpreted. [ Andries Kruger, South Africa]	Taken into account; text specifies the latitude and season of the insolation.
126907	11	55	11	57	"but in global annual average" is awkwardly worded. [ Trigg Talley, United States of America]	Accepted; clarified wording
57557	11	55	11	57	It would be useful to to assign a confidence level to the variations in incoming solar radiation, if possible. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; text characterizes the calculations as "precise".
98733	11	59	12	2	Sentence could be clearer with a couple commas. Could rephrase as follows: A new multi-millennial reconstruction of solar irradiance, which extends back 9 kyr based on updated cosmogenic isotope datasets and improved models for production and deposition of cosmogenic nuclides (Poliunov et al., 2016), has reduced the uncertainty (Wu et al., 2018). [ Meredith Parish, United States of America]	Editorial; copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83421	11	59	96	55	Starting here but continuing throughout the rest of the chapter you jump between using years, ka/Ma and kyr/Myr, which is fine for a paleoclimate expert reader, but not the wider audience and stakeholders you want to reach with the AR6. In Chapter 1 the authors mostly used years -even for 800 000 years (ka appears only once in regard to the LIG)- as is also done for the FAQs of Chapter 2. For Chapter 2 I would use years for age less than 6000 years and than just ka and Ma since their meaning is explained in the footnote for Cross-Chapter Box 2.1 Table 1. Non-expert readers will not understand why you jump between using ka/Ma and kyr/Myr. If you want to keep the IUGS concept, you either need to add a text box early on in this chapter (my recommendation) or a glossary item/ Annex II item explaining that ka/Ma are used for a date/ fixed point in time and yr/kyr/Myr when referring to duration or in ratios. You could refer to Aubry, M.-P., Couvering, J.A.V., Christie-Blick, N., Landing, E., Pratt, B.R., Owen, D.E., Ferrusquía-Villafranca, I., 2009. Terminology of geological time: Establishment of a community standard. Stratigraphy 6, 100-105. if you want to point to a reference. Whatever decision is made should be passed on to the lead authors of other chapters, especially those of Chapter 5 who use kyr/Myr throughout, because there should be consenses throughout AR6. [ Antje H. L. Voelker, Portugal]	Editorial. In FGD we follow the style guide in these matters. Copy editing undertaken.
90141	11		11		CROSS-CHAPTER BOX 2.1, FIGURE 1 [ Jeannine-Marie St-Jacques, Canada]	Noted; no comment stated.
90265	11		11		Cross-Chapter Box 2.1, Figure 1 should be "Raymo" not "Raymon" in the figure [ Jeannine-Marie St-Jacques, Canada]	Taken into account; Raymo dataset no longer used.
126909	12	1	12	6	The text seems to be jumping around a bit. Maybe this paragraph on solar activity belongs above with the other solar activity material. [ Trigg Talley, United States of America]	Taken into account. Reformulated.
77207	12	1	29	22	A lot of this is dealt with in later chapters e.g. Chapter 7, is there room to reduce detail here? [ Emer Griffin, Ireland]	Rejected. Although Chapter 7 takes up some aspects of what is discussed in this section, this seems the necessary amount for a meaningful assessment. Other reviewers in turn asked for more detail. In balance we decide not to shorten substantially.
93369	12	2	12	9	There are two Wu et al., 2018 references in the list, so it should be Wu, C.-J. et al 2018 in the three cases that this ref appears. [ Carles Pelejero, Spain]	Accepted.
30079	12	2			make it clear which Wu et al 2018 is referred to: Wu C.-J. et al 2018 [ Gilles Delaygue, France]	Accepted.
30081	12	4			make it clear which Wu et al 2018 is referred to: Wu C.-J. et al 2018 [ Gilles Delaygue, France]	Accepted.
79141	12	5	12	5	Update Reinhold et al., published in 2020 [ Natalie Krivova, Germany]	Accepted.
73325	12	5	12	5	Edit the Egoriva reference to remove the 'T'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
57559	12	5	12	5	Remove letter "T" from Egorova, T. et al., 2018 for consistency with other literature citations that only refer to the last name of the authors. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
90269	12	5	12	9	Problem with citation "Egarova, T et al" should just be "Egarova et al. " to be similar to all your other citations [ Jeannine-Marie St-Jacques, Canada]	Editorial; copyedit to be completed prior to publication.
79143	12	5	12	15	Check reference format (partly with initials after the family name: Egorova, T.; Wu, C.-J.) [ Natalie Krivova, Germany]	Accepted.
68037	12	8	12	16	"where the higher value comes from a poorly-constrained model": this is not explained nor supported by Fig 2.2. [ Michael Evans, United States of America]	Accepted. The statement is reformulated and new references are included.
73327	12	9	12	9	Edit the Egoriva reference to remove the 'T'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30083	12	9			make it clear which Wu et al 2018 is referred to: Wu C.-J. et al 2018 [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4511	12	12	12	13	You are comparing total solar irradiance (TSI) for the MWP and LIA without defining the time intervals. You are referring to Fig. 2.2 in which it is very clear that solar activity was generally higher during the MWP than during the LIA. It therefore seems you have defined the two phases incorrectly or in a subjective way to claim there are hardly any changes. Your own figures shows that a MWP 800-1350 AD and a LIA of 1500-1850 have very different TSI averages. The LIA has much lower TSI than the MWP. Please do not mislead readers by choosing arbitrary time intervals. The only low TSI phase during the MWP is the well-known Oort solar minimum whilst the LIA contains multiple multidecade solar minima. [Sebastian Luening, Switzerland]	Taken into account by a more compact description of the historical variability in the FGD.
10409	12	12	12	16	Why are TSI values for these periods mentioned? No context is given. Solar activity is not caused by periods of climate. If there is a broader discussion about so called "MCA" and "LIA" then mention it then. Not needed here, remove. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	See response to 4511
126911	12	13	12	16	A little more context about the TSI numbers reported here would be helpful (since this is a topic of high interest). Authors have provided the range, and suggested the high value is because of a poorly-constrained model. Reword this instead to report the range of 'well-constrained' models, then specify in another sentence that, in addition, a much higher estimate of X was reported in a poorly constrained model. [Trigg Talley, United States of America]	Taken into account. This is a very useful comment, we revised the text accordingly, and in particular revised downward the larger value in light also of new evidence.
10439	12	14			I would recommend not starting to use a new term for a period of solar activity ("Modern maximum") with specific dates. Lean (2018) defines "modern maximum" as 1950-2009. You will find it hard to find an agreed period across studies. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We revised to "second half of 20th century."
79145	12	15	12	16	Check latest paper by Lockwood & Ball (2020), another evidence for the weaker forcing [Natalie Krivova, Germany]	Accepted.
1213	12	18	12	26	There are also other studies based on solar activity proxies, such as galactic cosmic rays (GCR), which conclude that there have been negligible persistents on earth's climate from changes in solar variability (doi:10.1088/1748-9326/8/3/035049). [Rasmus Benestad, Norway]	Noted. The reviewer supports the conclusion from Chapter 7.
108293	12	20	12	20	citation 'Wu, C.-J. et al., 2018' should be changed to 'Wu et al., 2018' [Won-Tae Kwon, Republic of Korea]	Editorial; copyedit to be completed prior to publication.
79147	12	21	12	21	"averaged over solar cycles" should be "averaged over the solar cycle" (this changes the meaning) [Natalie Krivova, Germany]	Accepted.
30017	12	22	12	22	Figure 2.26, how about 2009-present. It seems to continue to have a decrease of TSI. [Yihui Ding, China]	Taken into account. However, given the now prolonged dataset, we cannot detect any significant trend anymore (in contrast to AR5).
126913	12	22	12	22	When the placeholder for 0.04 W m <sup>-2</sup> is replaced, be sure to replace it with a likely range, since the word likely implies a range of likely values. [Trigg Talley, United States of America]	Accepted. However, no trend is found.
79149	12	23	12	23	the first paper to show the stronger UV variability dates back 2006 https://doi.org/10.1051/0004-6361:20064809 [Natalie Krivova, Germany]	Accepted.
13219	12	23	12	23	The reference Yeo et al. 2017a does not exist [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Corrected.
10441	12	24	12	26	"used in CMIP5" - what models actually used could differ from what was recommended. So rephrase to "recommended to be used in CMIP5" or similar. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
79987	12	29	12	30	How is this ERF value (+/- 0.2 W/m <sup>2</sup> ) calculated? What models and solar forcings have been used here? Has ozone been taken into account in this calculation? Overall, it would be good to provide a reference for the calculation of this value and its uncertainty range, other than just simply referring to Chapter 7. [Gabriel Chiodo, Switzerland]	Accepted. A more specific reference to Ch 7 is provided.
30085	12	29	12	30	i think this sentence is, at best, loose and ambiguous, because ERF is by definition a global change over a given period, as used in Section 2.2.8. Rather, i understand that the range of +/- 0.2 W/m <sup>2</sup> given here is the maximum change in ERF associated to solar cycles, over a loosely defined period starting from "the late 19th century". Suggestions: remove "global-mean"; replace "did not exceed" by "did not change by more than". [Gilles Delaygue, France]	Accepted. Reformulated following the reviewer's advice.
11449	12	29	12	30	"The global-mean ERF since the late 19th century did not exceed +/- 0.2 Wm <sup>-2</sup> " - is it possible to add a likelihood statement (likely or very likely range?) [Gerhard Krinner, France]	Accepted. A more specific statement is now used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57561	12	30	12	30	It could be useful to assign a confidence level to the level of global-mean ERF since late 19th century. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
10443	12	41	12	45	Surely you need to use the same scaling factor for SAOD as used in chapter 7.3.4.6 (-18). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised as suggested by the reviewer (the revised scaling factor in fact now is 20 Wm <sup>-2</sup> AOD-1).
73329	12	43	12	43	Edit the reference to move the misplaced ( (to before the '2'). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
34837	12	43	13	20	The solar TSI changes over the last 9k years quoted take no account of the possible amplification factors due to galactic cosmic rays, as proposed by Svensmark, nor other inter-planetary influences as proposed by Scafetta. Please see general comment #13 above. [ Jim O'Brien, Ireland]	Rejected. Galactic cosmic rays are discussed in Chapter 7. The resulting ERF is assessed to be negligible (section 7.3.4.5)
16477	12	45	12	45	This needs to explain clearly that what is shown here is not the AR6 assessed volcanic ERF and should not be taken as such. Or even better would be to re-plot using the methodology from 7.3.4.6. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now we use the ERF from Chapter 7 in the Figure (and explain it as such in the Caption).
54539	12	48	12	49	This statement is poorly phrased—the factor of four does not "diminish the forcing", it rather is a factor which relates 2 different quantities, the TSI which is measured perpendicular to the incoming solar radiation, and the global mean surface insolation [ Matthew Toohey, Canada]	Accepted. This half-sentence is dropped now.
30087	12	49			Solar ERF is calculated from TSI changes by dividing by 4, but also multiplying by the co-albedo (0.71) and by the stratospheric IR emissions (0.78), as explained in section 7.3.4.4. [ Gilles Delaigues, France]	Accepted. In light of the review comment (and other review comments), now ERF is shown and this sentence is improved.
115955	12		12		I suggest to report variations in total solar irradiance also in RF to avoid confusion for non specialist readers. This also applies to the corresponding figure. What is the uncertainty associated with TSI reconstructions, please provide it together with the mean values. [ Valerie Masson-Delmotte, France]	Accepted. Since ERF due to TSI changes is a simple scaling (as assessed in Section 7.3.4.4), a second axis is added to the plot, and the ERF values are provided in the text.
57563	13	1			"2.2.2 Volcanic aerosol forcing" subsection is not referenced in the 'curly brackets' in the Technical Summary subsection: TS.4.2.1.2 Natural forcing (solar, volcanic aerosols). Add the section to the TS subsection. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
45789	13	1			Section 2.2.2: For clarity it should be explained that the volcanic aerosol forcing is dominated by the effects of stratospheric aerosols, and that the contribution from tropospheric volcanic aerosols can be neglected. [ Twan van Noije, Netherlands]	Accepted.
111915	13	3	13	36	One could expect here something on relation between the solar and volcanic activity effects as discussed in the Section 1.2.1.2. ((p.14, l. 23-24, see previous comment) [ Tomas Halenka, Czech Republic]	Rejected. It was impossible to find the statement in 1.2.1.2 the reviewer refers to. Chapter 1, p14 l23-24 is about the Paris agreement, the same spot in Chapter 2 about CO2.
10445	13	5	13	8	Is this "radiative forcing" "RF" or "ERF"? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. 'ERF' is consistently used throughout the text
93491	13	11			change "from 2005-2011" to "from 2005 to 2011" [ Rahab KINYANJUI, Kenya]	Editorial; copyedit to be completed prior to publication.
68641	13	12	13	12	LIA should be spelled out here since it firstly appears in this Chapter. [ Tosiuyuki Nakaegawa, Japan]	Editorial. LIA now deprecated.
93493	13	12		13	Change "1999-2006" to "from 1999 to 2006" [ Rahab KINYANJUI, Kenya]	Editorial; copyedit to be completed prior to publication.
29537	13	15	13	15	The citation of Burke et al., 2019 is incorrectly inserted here. This publication does not mention SAOD or sulphates whatsoever. [ Kevin Burke, United States of America]	Accepted. Corrected to the right Burke paper.
99735	13	18	13	20	This sentence is difficult to comprehend. Possibly rephrase as "The return period for large volcanic eruptions, that decreased global radiative forcing by more than 1 W/m <sup>2</sup> , was around 30 years on average for the past 2.5 kyrs. The 1991 eruption of Mt Pinatubo was the most recent of those." [ Kira Rehfeld, Germany]	Accepted. Revised as suggested by the reviewer (with some additional edits to the sentence).
108297	13	19	13	21	what is the significance of interval between eruptions? [ Won-Tae Kwon, Republic of Korea]	Accepted. We now add a statement on the variability of the return time (and revised it to use ERF, rather than RF).
80249	13	21	13	22	Even if the uncertainties of provided SAOD values are low, error bars should still be given. [ Sophie Godin-Beekmann, France]	Accepted. Uncertainties are now better characterised in the revised text.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10411	13	21	13	22	Why is SAOD for these periods mentioned? No context is given. Volcanic activity is not caused by periods of climate. If there is a broader discussion about so called "MCA" and "LIA" then mention it then. Not needed here, remove. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Use of these periods has been omitted.
30089	13	23			'low': please replace by a less ambiguous term (small?) [ Gilles Delaygue, France]	Accepted. The uncertainty is more explicitly characterized now
99737	13	24	13	26	"Over the past 100 years SAOD was, on average, 14% lower than over the previous 24 centuries (back to 2.5ka)." [remove second half-sentence]. It is implicitly clear by the 14% that this estimate is within the range of centennial scale variability, so the second half-sentence can be omitted. [ Kira Rehfeld, Germany]	Rejected. Although there is some overlap in the two statements, we feel it is more explicit if we keep the half-sentence.
105671	13	28	13	38	The chapter is covering many of the forcing datasets that are being used in CMIP6 CMIP/DECK historical (and other) simulation configurations, however this point is not made clearly. It may be prudent to point this out, with a possible broad citation the input4MIPs introduction paper Durack et al 2018 doi: 10.1029/2018EO101751. In addition, the "official" volcanic forcing dataset being used in CMIP6 CMIP/DECK historical simulations is the Luo v3-0-0 dataset (findable at <a href="https://esgf-node.llnl.gov/search/input4mips/?institution_id=IACETH&amp;target_mip_list=CMIP">https://esgf-node.llnl.gov/search/input4mips/?institution_id=IACETH&amp;target_mip_list=CMIP</a> ) with an error associated with the Pinatubo reproduction leading to a subsequent dataset release which you use in the chapter (4-0-0, this is not yet available from input4MIPs). An aside, to investigate how sensitive model simulations are to this change in volcanic forcing Rieger et al 2020 doi 10.5194/gmd-2019-381 ran simulations using both the v3 and v4 datasets to compare differences. In addition, John Fyfe is leading a paper which compares CMIP5-forced simulations with their CMIP6-forced equivalents which shows that forcing matters [ Paul Durack, United States of America]	Rejected. It is in the realm of Chapter 3 to discuss models.
105673	13	33	13	34	The Sato et al 1993 (and Stenchikov et al 1998), Ammann et al 2003 and Ammann et al 2007 volcanic forcing datasets were used to force CMIP5 simulations [ Paul Durack, United States of America]	Rejected. We do not want to multiply references that are the basis of a cited paper.
54541	13	34	13	34	In addition to Amman et al., 2003, reference needs to be made here to Sato et al., 1996. [ Matthew Toohey, Canada]	Taken into account. Both references are dropped. (Reviewer is CA and took part in the decision)
95831	13	34	13	36	The paper by Bingen et al. (2017), Remote Sensing Env., doi:10.1016/j.rse.2017.06.002 could be added to the citations. [ Christine Bingen, Belgium]	Rejected. We want to avoid citing additional papers unless necessary.
95833	13	36	13	38	The paper Brühl et al. (2018), Atm. Chem. Phys., doi:10.5194/acp-18-1-2018 could be added to the citations. [ Christine Bingen, Belgium]	Rejected. We do not want to multiply references that are the basis of a cited paper.
80251	13	36	13	38	Provide the value of the detectable negative global radiative forcing produced by the small to moderate eruptions that occurred since 2000. [ Sophie Godin-Beekmann, France]	Accepted. very good comment. We revised this statement to report SAOD instead.
10447	13	36	13	38	It is not possible to observe radiative forcing, let alone detect it. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reformulated in the revision to report SAOD instead.
80253	13	40	13	48	The paragraph does not mention the new GLOSSAC global space-based stratospheric aerosol climatology provided by Thomasson et al. 2018. This reference is only mentioned about the saturation effects following large eruptions. [ Sophie Godin-Beekmann, France]	Noted. However, the Tomasson reference is cited where it is most pertinent, in the previous paragraph.
10449	13	40	13	48	The misidentification of global volcanic events should also be mentioned, i.e. global impact eruptions being missed, or eruptions misidentified as having a global impact. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. There is no evidence for any eruptions with strong impact being missing from the observational or proxy records. Mis-attribution or erroneous estimation of forcing (SAOD or ERF) is definitely an issue and is now included in the uncertainty estimates in the revised version. While doing so, we now converted the uncertainty ranges given in this section all to the standard 5-95% confidence intervals.
54543	13	41	13	41	replace "during" with "after"---the eruptions occurred over hours or days, the aerosols persist for months to years. [ Matthew Toohey, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1849	13	42			"due to saturation effects" is incorrect, or at least needs to be explained, as it is misleading jargon. I think you are referring to the inability of SAGE II to see the Sun in the tropics after the 1991 Pinatubo eruption when looking sideways through the atmosphere (limb scanning). So there was no saturation. Rather, the instruments on SAGE II were not sensitive enough to see the faint solar signal. So there was blocking, but no saturation. SAGE III is much more sensitive, and so would have less of these effects, but it and other new limb scanning satellites would still have some of these problems. Nevertheless, after the 1991 Pinatubo eruption, we still had good measurements of the total optical depth from the resulting aerosols. We just did not have good data on the vertical distribution in the tropics. If there had been lidars in the tropics, we would have had those data. So you could change "saturation effects" to "lack of lidar data." [ Alan Robock, United States of America]	Accepted. It's a fair point, and indeed there are more gaps in the satellite record than those caused by the strong eruptions.
30019	13	50	13	52	is it to add the latitudinal distribution of volcanic eruptions? [ Yihui Ding, China]	Noted. However, the space constraints do not permit to do this.
80289	13	50	13	52	Different AR6 chapters use "In summary," to introduce the concluding remark of each particular subsection. In the sake of a more uniform style accross the entire report, its should be also used in CH2. This applies to other different subsections. [ Paola Arias, Colombia]	Editorial; copyedit to be completed prior to publication.
73331	14	1	14	1	I suggest a capital 'G' for 'gases' in the section heading. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
469	14	1	14	1	The concept that CO <sub>2</sub> , CH <sub>4</sub> , and N <sub>2</sub> O are "well-mixed" and "homogeneously mixed in the troposphere" seems not to be the case, from both in situ and satellite measurements. Station data of CO <sub>2</sub> from Mauna Loa have noticeably different values and, especially, seasonal cycle magnitudes than those from the South Pole and other regions, and satellite-based global images of CO <sub>2</sub> show marked geographic differences. It would be helpful to include a statement on how the terms "well-mixed" and "homogeneously mixed" are being used. [ Claire Parkinson, United States of America]	Accepted. "homogenously mixed" has been deleted. The term "WMGHG" is under discussion with other LAs from multiple chapters
112895	14	1	14	15	Section 2.2.3 deals with well-mixed greenhouse gases. The section is supported by figure 2.3 (pag. 156) and 2.4 (page 157) in claiming that the CO <sub>2</sub> level during the present time has been "unprecedented" during the last million years. The claim and the figures are questionable because what os compared is the CO <sub>2</sub> measured in the atmosphere since 1958 and the CO <sub>2</sub> concentrations obtained from Ice cores. The two records are physically different because the ice core ones are not annual measurements but averages that span several centuries and even millennia. Thus, the ice core records are a kind of low-pass filtered signal that cannot be directly compared with the annual CO <sub>2</sub> measurements taken directly in the air during the last decades that in comparison represents a high-frequency signal. Thus, the figures are misleading. Data must be plotted by taking into account the same time scale for the entire period. In this case, it should be used the longest time scale of the observations and use it to run a moving-average curve and then plot this curve. By doing so, the anomalous picks in CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O observed at the end of figure 2.4a will likely disappear. [ Nicola Scafetta, Italy]	Rejected, but we note that the ice core records has been naturally smoothed by order of decades to thousands of years.
42877	14	1			gain it seems odd to capitalise Greenhouse but not gases, needs a decision. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
73333	14	3	14	3	'gases' does not need a capital here (as is done correctly further on in the section). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
36945	14	3	14	15	Why are you mentioning methane and nitrous oxide when they are utterly trivial in the atmosphere both because there are such small amounts of these gases and because their IR absorption bands are swamped by the far greater amount of water vapour? [ John McLean, Australia]	Rejected. Radiative forcing by CH <sub>4</sub> and N <sub>2</sub> O is not trivial.
36947	14	3	14	15	Water vapour should be included in this paragraph because it's certainly in the atmosphere and the amount does change. [ John McLean, Australia]	Rejected. Surface tropospheric water vapor change is included in section 2.3.1.3.2 and column water vapor in 2.3.1.3.3, with an emphasis on dynamical aspects. As water vapor responds strongly to climate change, but not driving it, it is not included in this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57565	14	6	14	9	Since the confidence level from AR5 is given for the mixing ratios for the past 800kyr, the confidence level for the increase in GHG over the past 22 kyr should be given as well. Add "(very high confidence)", as concluded in WGI AR5 TS (page 50) at the end of the sentence. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
4717	14	6	14	11	The statement is too long, kindly break up and needs to be recasted. [ Ibikunle Olaleru, Nigeria]	Accepted
30091	14	11			'from 2005-2011': over 2005-2011; or from 2005 to 2011 [ Gilles Delaygue, France]	Accepted
30093	14	12			'from 1999-2006': over 1999-2006; or from 1999 to 2006 [ Gilles Delaygue, France]	Accepted
57567	14	13	14	13	Replace CMIP6/PMIP6 with CMIP6/PMIP4 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted
73335	14	18	14	18	See my earlier comment regarding deep time. Very marginally 500 Ma could be considered deep time, but 800 ka isn't. Previously, the further boundary was 55 ma! I suggest just giving the period involved in the section heading. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "Deep time" has been deleted.
99195	14	18	14	42	What is the policy relevance of the CO2 during the last 500 Ma. The description of the data starts in the Triassic which is much younger. The data for this time interval is significantly less well constraint given uncertainties in the boron isotopic composition of seawater and the alkalinity of the ocean. There is a very long gap then, dismissing the information from the Cretaceous. All of this raises the question if the narrative would not better focus on the Cenozoic thereby covering the same interval as temperature on page 32 [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The relevance is to show what we know about how concentrations have varied in Earth's history.
52109	14	18	14	42	New CO2 proxy evidence from delta13C in terrestrial C3 plants suggests in fact last time CO2 exceeded present day levels was in the mid-Miocene c. 14 million years ago: see Cui Y, Schubert BA, Jahren AH. A 23 my record of low atmospheric CO2. Geology. 2020 May 29. DOI: <a href="https://doi.org/10.1130/G47681.1">https://doi.org/10.1130/G47681.1</a> [ Kathryn Fitzsimmons, Germany]	Taken into account. Text revised.
57569	14	20	14	42	The paragraph is rather 'heavy' compared to previous paragraphs. Suggestion: break it up after the full stop at line 30 to create two shorter paragraphs. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account in text edits.
1989	14	21	14	21	"fossil" sounds odd here. The trapped gas bubbles aren't fossils! [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "fossil atmosphere" has been changed to "fossil atmospheric air"
30095	14	21	14	22	Pedogenic carbonates provide most of the data shown in fig.2.3a (paleosols). Suggestion: "isotopes from continental and marine sediments" [ Gilles Delaygue, France]	Accepted.
8881	14	21	14	23	It'd be helpful to add a sentence explaining the basic ideas of using B and C to reconstruct CO2. [ Robert Kopp, United States of America]	Accepted. Text modified to clarify.
1991	14	23	14	23	Figure 2.3 panel c looks like it also includes some post-ice-core data (e.g. Keeling curve?), which isn't in the legend or caption. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Caption modified along with new FAIR data table entries which clarify.
93371	14	23	14	25	Since Triassic is mentioned in the phrase, I would also add 'during at least one prolonged interval within the Carboniferous and Permian (350–250 Ma)' [ Carles Pelejero, Spain]	Taken into account in text edits.
30097	14	23			'500 Ma' > '450 Ma' [ Gilles Delaygue, France]	Accepted. Title modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30099	14	24			The pertinence of the given maximum (3700ppm) and, more generally, of the data (paleosols, and even more stomata) seems doubtful to me. I am not specialist of these data: i comment the data based on the Supplementary dataset of Foster et al (2017), which provide most of the data shown by Figure 2.3a (except phytanes). Over the quoted period 220-200 Ma, CO2 levels reconstructed from paleosols range from -194 (between -97 and -388) to the quoted maximum of 3649 ppm (between 1824 and 7297). This range is clearly visible on fig2.3a (but not the quoted maximum, by the way). By comparing this fig. 2.3a with Fig.1 of Foster et al 2017, it seems that phytanes data constrain much more the CO2 levels than the other data. I have 3 remarks: 1. Foster et al state that the CO2 level distribution is right skewed, which is not accounted for in the Report (3700 +- 1600ppm); 2. How such maximum of 3700ppm (one single value) can be quoted with such a large range of possible values over the cited period 220-200 Ma?; 3. Which trust can be given to reconstructions of CO2 levels ranging from negative values to 7000ppm? Of course these data merit to be shown, but in my opinion the Report should stress out the very large uncertainty of these techniques. Foster et al (2017) write that "CO2 estimates from stomatal ratios should be considered semiquantitative only". [ Gilles Delaygue, France]	Taken into account. In the FGD this text part was re-edited and assessments of paleo reconstructions of CO2 levels were clarified (also in the Table2.1).
57571	14	25	14	25	Instead of "close to 1750 levels" consider writing "close to ~278.7 ±1.8 ppm (1750 levels)" to be stylistically consistent with the rest of the paragraph as well as clear. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
99197	14	25			reference missing for the data [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
30101	14	25			"close to 1750 levels": i think it is not clear at all that 1750 refers here to year 1750 (in a sentence devoted to hundreds of Ma). Suggestion: "close to 280 ppm (pre-industrial level)" [ Gilles Delaygue, France]	Accepted.
100599	14	26	14	26	Keep in mind that there's no independent evidence for this from pCO2 proxies. This is all based on C-cycle models. I would include some qualifying language or state the basis because it's unusual [ Matthew Kohn, United States of America]	Taken into account. Text edits made to clarify.
35509	14	27	14	27	Correct bibliographic citation [ Carlos Antonio Poot Delgado, Mexico]	Unclear what is being requested. No action possible.
126915	14	27	14	27	What emission process is being referred to here? Net fluxes from the fast carbon cycle (e.g., plants, surface ocean) or net anthropogenic emissions by people that were around 3-20Kya? [ Trigg Talley, United States of America]	Taken into account. Text has been edited to clarify.
36949	14	28	14	28	"7-35 times lower than" is nonsense. Mathematically it means the actual value is -6x to -34x where x is the starting value. [ John McLean, Australia]	Taken into account. The text has been modified to make clearer what was intended here.
8883	14	28	14	28	Ensure units used for CO2 emissions are consistent throughout the report. [ Robert Kopp, United States of America]	Taken into account. Efforts have been made to ensure consistency.
57573	14	28	14	30	The sentence on atmospheric CO2 during the EECO is missing a literature citation. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Citations now added.
13221	14	29	14	29	ECCO must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Rejected. It was defined in Cross-Chapter Box 2.1.
99199	14	29			is a likely statement really supported by the still limited data? [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revisited this assessment to ensure its rigor.
99739	14	30	14	30	"The last time CO2 mixing ratios were..." or "The last time the CO2 mixing ratio was", but as it is it sounds wrong [ Kira Rehfeld, Germany]	Accepted. Text edited for clarity.
1993	14	30	14	30	"at a rate of ~16ppmv/Ma" should be "at an average rate of ~16ppmv/Ma". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Edits made accordingly.
93375	14	33	14	33	There is only one Hollis et al., 2019 reference, so delete 'a'. [ Carles Pelejero, Spain]	Accepted.
57575	14	33	14	33	The literature citation "Hollis et al., 2019a" has been added twice to the reference list (p. 118, lines: 31-38). Remove one of the references in the reference list and remove the "a" in this text citation. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
57577	14	34	14	35	This sentence on atmospheric CO2 mixing ratio variations is missing a literature citation. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Citation now added.
90271	14	34			"the atmospheric CO2 mixing ratio" [ Jeannine-Marie St-Jacques, Canada]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
14885	14	35	14	35	A reference is missing for "decreased at a rate of ~40 ppm Myr <sup>-1</sup> " [ Marie-France Loutre, Switzerland]	See response to 57577
81303	14	35	14	35	This should be "Ma-1". [ Johannes Laube, Germany]	Editorial. We use conventions in line with the IPCC style guide.
57579	14	37	14	37	The literature citation "Dyez et al., 2018a" has been added twice to the reference list (p. 110, lines: 19-23). Remove one of the references in the reference list and remove the "a" in the text citation. [ APECS, MRI, PAGES ECN, PYRN and YES5 ECS group review, Canada]	Accepted.
8885	14	37	14	38	Blue ice records are new since AR5, and would merit an additional sentence or two explaining. [ Robert Kopp, United States of America]	Rejected. The following sentences explain it.
1997	14	39	14	40	"The last time atmospheric CO2 mixing ratio was as high as present was very likely over 2 Ma" is not consistent with the equivalent Executive Summary statement which does not have a likelihood statement but states "high confidence". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Consistency within the chapter and with chapter 5 has been addressed in FGD.
57581	14	39	14	42	Separate this last concluding sentence of the paragraph from the rest of the paragraph for clarity and consistency with previous sections. [ APECS, MRI, PAGES ECN, PYRN and YES5 ECS group review, Canada]	Accepted. Edits made accordingly.
98735	14	39	15	42	I don't understand this sentence. Why can't periods of high carbon dioxide be excluded? And excluded from what? [ Meredith Parish, United States of America]	Taken into account. Text redrafted for clarity.
1995	14	40	14	40	"very likely over 2 Ma" should be "very likely before 2 Ma" or "very likely more than 2 Myr ago". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
10415	14	45	15	5	Why are CO2 concentrations given for different past periods? More context is needed. Why does 1750 have a lower value than "LIA", when the former is in the middle of the latter? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in text edits. Term LIA has been deprecated.
10417	14	45	15	5	The rate of change of CO2 during the "LIA" seems to not reflect the complex behaviour of CO2 concentrations between 1450 and 1850 (Figure 2.4c). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rates now only given when relevant and LIA and MCA removed from table.
13223	14	49	14	49	LGM must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Rejected. LGM is defined in cross-chapter box 2.1.
57583	14	53	14	54	Table 2.2: Merge the two sentences on the likely ranges to shorten the text, i.e. "The ± values represent likely ranges for concentrations (ppm) and the likely uncertainty range for the rate of change (%/Myr)." [ APECS, MRI, PAGES ECN, PYRN and YES5 ECS group review, Canada]	Accepted.
115957	14		14		There are descriptions of CO2 changes in the deep past, where can explanations for these changes be found? A point of coordination with ch 5. [ Valerie Masson-Delmotte, France]	In the updated SOD, CH5 deals with deep past records and carbon emission rate.
1999	15	3	15	3	I don't believe the EECO rate of change of "35 ppm/century". This must be a typo. Although maybe it is in units of % per Myr? Mixing units in this column is a recipe for misunderstanding because it is not clear which values are in which units! [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	See response to 10417
100601	15	3	15	3	MPWP: be sure these values for pCO2 correspond across chapters and sections. Elsewhere it is described as up to 425 ppm (I gather that's from de la Vega et al., in review) [ Matthew Kohn, United States of America]	Taken into account in revising the text.
100603	15	3	15	3	Add: MCO 550±80 (d11B; Sosdian et al., 2018), 430 +150/-100 (Super et al., 2018; phytoplankton), 500±50 (Steinthorsdottir et al., 2020; stomata), 425±100 (Witkowski et al., 2018; phytane), possibly higher for phytoplankton (Stoll et al., 2019; Badger et al., 2019; Zhang et al., 2019), possibly higher for stomata (Konrad et al., 2020) [ Matthew Kohn, United States of America]	Taken into account. Papers have been assessed and included where relevant.
105081	15	3	15	4	Table 2.2: "PMIP6" reference for LGM does not exist. PMIP is at its fourth phase: PMIP4 [ Masa KAGEYAMA, France]	Accepted.
52111	15	3	15	4	MPWP CO2 values: the delta11B proxy is listed, though composite of all published CO2 proxies, including variations, is summarised in Fletcher, T.L., Warden, L., Damsté, J.S.S., Brown, K.J., Rybczynski, N, Gosse, J.C. & Ballantyne, A.P. (2019). Evidence for fire in the pliocene arctic in response to amplified temperature. Climate of the Past, 15(3), 1063–1081. doi:10.5194/cp-15-1063-2019 [ Kathryn Fitzsimmons, Germany]	Taken into account in making revisions to the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57585	15	3			Table 2.2: Mid-Holocene CO2 concentration (CMIP6) value: 264.4 ppm. Literature citation for this value: Otto-Bliesner et al. (2017). The PMIP4 contribution to CMIP6 - Part 2: Two Interglacials, Scientific Objective and Experimental Design for Holocene and Last Interglacial Simulations. Geoscientific Model Development. 10, pp.3979–4003. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
30103	15	10	15	11	Pedogenic carbonates and fossils of plants provide most of the data shown in fig.2.3a (paleosols and stomata). Suggestion: "reconstructed from continental rocks, marine sediment, and ice core records" [ Gilles Delaigüe, France]	Accepted.
99223	15	10			The largest part of the text focusses on the more recent part of the geological record raising questions of the policy relevance of the top panel of the figure , for example the discussion of temperature in the text focusses solely on the last 60 Ma. [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in text revisions to the section.
43021	15	11		14	Read (a) 0 to 450 Ma (Royer et al., 2001; Pagani, 2002; Pagani et al., 2005, 2011; Hönlisch et al., 2009; Bereiter et al., 2015; Anagnostou et al., 2016, 2019; Foster and Rae, 2016; Foster et al., 2017; Sosdian et al., 2018; Super et al., 2018; Witkowski et al., 2018; Dyez et al., 2019; de la Vega et al., 2019a,b) rather than (a) 0 to 450 Ma, data from (Royer et al., 2001; Pagani, 2002; Pagani et al., 2005, 2011; Hönlisch et al., 2009; Bereiter et al., 2015; Anagnostou et al., 2016, 2019; Foster and Rae, 2016; Foster et al., 2017; Sosdian et al., 2018; Super et al., 2018; Witkowski et al., 2018; Dyez et al., 2019; de la Vega et al., 2019a,b). [ Cyriaque Rufin Nguimalet, Central African Republic]	Taken into account. Papers have been assessed and included where relevant.
57587	15	12	15	12	In text literature citation "Hönlisch et al. 2009" but "Hönlisch" in the reference list. Drop the "e" in text citations for correct name. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
93495	15	12			change "an order of magnitude.." to "in order of magnitude" [ Rahab KINYANJUI, Kenya]	Unclear. Comment appears to refer to either a different page or a different chapter and is not actionable.
69807	15	14	15	14	Shuld be Dyez et al. "2018" [ Kaoru Kubota, Japan]	Accepted.
57589	15	14	15	14	In text literature citation "de la Vega et al., 2019a,b" but "de la Vega et al., submitted" in the reference list. Not sure which one is correct. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, references corrected in FGD.
43023	15	17			Read "See Royer et al. (2001) and Witkowski et al. (2018) for" rather than "See (Royer et al., 2001; Witkowski et al., 2018) for" [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
57591	15	19	15	19	In text literature citation "Anagnostou et al., 2016a": the corresponding citation "Anagnostou et al. (2016a)" and Anagnostou et al. (2016b) in the reference list (p.99, lines 6-11) are the exactly the same paper. Remove "a" or "b" from in text citations and one of the references in the reference list. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, references corrected in FGD.
57593	15	22	15	22	In text literature citation "Petit et al., 1999" is missing in the reference list. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, references corrected in FGD.
13225	15		15		Table. LIG and PETM must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Rejected. Terms are defined in Cross-chapter box 2.1.
115959	15		15		Please check the coherency between Table 2.2 and FAQ1.3. It would be good to indicate the cause of change in Table 2.2.§ [ Valerie Masson-Delmotte, France]	Taken into account in text edits. We do not wish to speculate as to causes though to avoid conflation with chapters 3 and 5.
42879	15				Table 2.2 rates of change can't be interpreted without knowing what period of change you refer to. For example during what period are you saying that the rate in the LIG of 0.01 occurred: it seems not to be in the termination into LIG or the inception out of it when significant rates are expected. If it's simply during the LIG, you need to give the interval covered. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	See response to 10417
8887	16	2	17	4	Although the title is 'from 800 ka', the sections here are focused on the LGM to present.  Would be helpful to assess the recent literature on pre-Industrial anthropogenic CO2 and CH4 emissions (e.g., Lorenz and Lal, 2018; Erb et al. 2018, 10.1038/nature25138) [ Robert Kopp, United States of America]	Accepted. Text has been redrafted accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115533	16	9	16	9	Should estimated of changes in CO2 emissions in early 2020 be an issue here? Or possible in 2.2.3.3.1 [ Rolf Müller, Germany]	Rejected. The decision has been made to consider this in a cross-chapter box hosted by chapter 6.
90273	16	9	16	55	for clarity, cite figure 2.4 in each subsection: 2.2.3.2.1, 2.2.3.2.2, and 2.2.3.2.3 [ Jeannine-Marie St-Jacques, Canada]	Accepted.
26615	16	10	16	10	We suggest to precise "glacier ice sheets" considering the following references [ Eric Brun, France]	Accepted. "Glacial ice" has been replaced with "ice sheet".
69809	16	12	16	12	Should be Rubino et al. "2019" [ Kaoru Kubota, Japan]	Accepted.
52175	16	12	16	12	Word "that" is repeated twice in the sentence "...physical principles are all in agreement that that land..." [ Maritza Jadrijevic Girardi, Chile]	Editorial; copyedit to be completed prior to publication.
42991	16	12	16	12	There is no "Rubino et al. (2018)" in the reference list. To the best of my knowledge, there is no Rubino et al. Paper published in 2018 and investigating the LGM or the LDT periods [ Mauro Rubino, Italy]	Accepted, it is Rubino et al. (2019).
57595	16	12	16	12	In text literature citation "Rubino et al., 2018" does not correspond to the citation in the reference list "Rubino et al. (2019)" (p. 138, lines 58-61). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	See response to 42991
73337	16	13	16	13	Recent decades is vague. Please quantify or give dates. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account and clarified in text edits.
73339	16	13	16	13	There is no year 0, I think you mean year 1. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
13227	16	14	16	14	CE must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Rejected. Period has been defined in cross-chapter box 2.1
39635	16	16	16	16	Consider citing Rubino et al., 2019, instead of MacFarling et al., 2006. Law Dome data have been updated by Rubino et al. (2019) and are now fully available online. [ Xavier Fain, France]	Taken into account. Relevant papers now cited.
57597	16	16	16	16	In text literature citation "MacFarling Meure et al., 2006" does not correspond to either of the two identical citations in the reference list "MacFarling Meure et al. (2006a)" and "MacFarling Meure et al. (2006b)" (p.128, lines 15-20). Remove one of those from the reference list. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Referencing corrected.
43025	16	18		19	Read "in 1850 (Siegenthaler et al., 2005; MacFarling Meure et al., 2006; Ahn et al., 2012, 2019; Bauska et al., 2015) » rather than « in 1850 (Siegenthaler et al., 2005; MacFarling Meure et al., 2006; Ahn et al., 2012, 2019; Bauska et al., 2015;)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
10451	16	18			The 1850 value is the same as 1850-1900 in table 2.2. Which period/date does it refer to? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account and clarified in revisions.
69811	16	19	16	19	Delete semi colon after "Bauska et al., 2015" [ Kaoru Kubota, Japan]	Editorial; copyedit to be completed prior to publication.
471	16	19	16	20	It is important to indicate the time frame for the 5.0 ppm and 4.6 ppm increases and decreases. I.e., are they ppm per year, ppm per decade, ppm per century, etc.? [ Claire Parkinson, United States of America]	Accepted and clarified.
57599	16	19	16	20	The sentence on MWP CO2 concentration increase is missing a literature citation. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. This text has been removed.
10413	16	19	19	20	Why is this the CO2 concentrations for different periods mentioned? No context is given. If there is a discussion about causes of climate change over last 1000 years, then mention it there. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The general mechanisms of atmospheric CO2 changes in the past are discussed in chapter 5.
8889	16	20	16	20	Confusing what is meant by the 'fastest rate over the CE' -- CE prior to 1850? Certainly not true over the 20th/21st centuries [ Robert Kopp, United States of America]	Accepted. Clarified intent to be up until pre-industrial.
2001	16	22	16	22	Not sure why there is an "Although" at the beginning of this sentence - should be "Because" ? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
11451	16	22	16	24	"the rate of increase... is far greater than... (very high confidence)". It is slightly inconsistent to use a strong confidence statement in a sentence that is a bit vague - could you try to quantify "far greater" a bit better? [ Gerhard Krinner, France]	Taken into account in text revisions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30105	16	22	16	25	I do not understand this sentence: for me, it means that the CO2 smoothing in the firn prevents the recent increase rate from being greater than in the past, which has no sense. Smoothing of CO2 variations may prevent comparing these rates; however the CO2 increase since 1850 is calculated from firn measurements (Fig. 2.4) so that its rate is directly comparable with ice core records. So I am not sure about the meaning of this sentence. [ Gilles Delaygue, France]	Taken into account. Text has been clarified.
2003	16	24	16	24	"cores" should be "core". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
36951	16	28	16	42	At very high concentration methane has an impact on temperature but in the atmosphere, with its mix of gases, it does not. I refer you to the first IPCC report (1990), pg 58, where issues with the Global Warming Potential (GWP) are discussed. Among the four issues is "[t]he dependence of the radiative forcing of a gas on its concentration and the concentration of other gases with spectrally overlapping absorption bands." The quantity of CH4 in the atmosphere is about 1.6 ppm and its narrow absorption band overlaps with water vapour, of which there is typically at least 15000ppm. An 0.02% increase in water vapour is equivalent, by volume, to a doubling of CO2. What's more the IPCC's calculation of a GWP for CH4 is bogus because it is based on methane in isolation rather than as one of many gases in an atmosphere. (For a simple reference, see <a href="https://commons.wikimedia.org/wiki/File:Atmospheric_Transmission.png">https://commons.wikimedia.org/wiki/File:Atmospheric_Transmission.png</a> ) [ John McLean, Australia]	Rejected. Further assessment is carried out in chapters 5 through 7 that supports its inclusion here in this context.
57601	16	28			2.2.3.2.2 CH4: this subsection is missing uncertainty language. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted and such language now added.
26617	16	29	16	29	We suggest to precise "glacier ice sheets" considering the following references [ Eric Brun, France]	Accepted.
39071	16	31	16	31	The expression 'c.450ppb' is not clear (is 'c' circa?) [ Federico Serva, Italy]	Accepted. 'c' is replaced with 'approximately'
26619	16	36	16	36	Kageyama is not the original publication for LGM CH4 concentrations. [ Eric Brun, France]	Rejected. It is the correct citation here.
73341	16	37	16	37	There is no year 0, I think you mean year 1. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
36953	16	45	17	4	Similar comments to those I made for pg16 lines 28-42 apply to N2O. It is not only in microscopic quantities but its major bandwidth (~3.5 microns) is where there's very little IR energy but the other two bandwidths in which it absorbs and scatters energy (8 microns and about 10.8 microns) water vapour is thousands of times more common and therefore the chances of a photon reaching a molecule of methane are tiny. (see <a href="https://commons.wikimedia.org/wiki/File:Atmospheric_Transmission.png">https://commons.wikimedia.org/wiki/File:Atmospheric_Transmission.png</a> ) [ John McLean, Australia]	Rejected. Further assessment is performed in chapters 5 and 7 that justify its inclusion.
11453	16	46	16	46	"glacier ice" - technically correct, as this is from Taylor Glacier in Antarctica, but still a bit misleading, as the word "glacier" will be understood by most people as "mountain glacier". Similar comment applies to CH4 paragraph above (p16, line 29). "Polar ice cores" might be clearer. [ Gerhard Krinner, France]	Accepted.
93497	16	46			add "is" in "N2Oconcentration changes" is " associated with... [ Rahab KINYANJUI, Kenya]	Editorial; copyedit to be completed prior to publication.
26621	16	50	16	50	Kageyama is not the original publication for LGM CH4 concentrations. [ Eric Brun, France]	Duplicate with 26619
57603	16	53	16	53	In text literature citation "Ryu et al., 2019b", but in the reference list: "Ryu et al. (2019a)" and "Ryu et al. (2019b)" are the exact same paper (p. 139, lines 16-19). Remove one of them from the reference list and the letters "a" and "b" from in text citations. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. This kind of issue will be fixed during the production phase of the report.
57605	16	54	16	55	End of sentence reference should be Figure 2.4 (Atmospheric well mixed greenhouse gases from ice cores) and not Figure 2.3 (Evolution of atmospheric CO2). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
29807	16	55	16	55	Replace "(Figure 2.3)" by "(Figure 2.4)". [ Hernan Edgardo Sala, Argentina]	Accepted.
115961	16		16		Missing assessment of the potential role of close off and gas diffusion effects on rates of changes from ice core records incl. recent work (eg. K. Fourteau et al, 2020 and related studies esp for low accumulation sites). Important for consideration of detectability of centennial variations and artefacts in ice core records. [ Valerie Masson-Delmotte, France]	Accepted. Text modified accordingly.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43027	17	1		2	Read "before 1900 CE (Machida et al., 1995; Sowers, 2001; MacFarling Meure et al., 2006; Ryu et al., 2019b). Multiple ice » rather than « before 1900 CE (Machida et al., 1995; Sowers, 2001; MacFarling Meure et al., 2006; Ryu et al., 2019b) Multiple ice » [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
24357	17	2	17	2	There should be a period before the words "Multiple ice cores..." [ Owen Cooper, United States of America]	Editorial; copyedit to be completed prior to publication.
39633	17	7	17	7	Fig 2.4 : not sure why the reference Rhodes et al. (2017) is cited here. This paper discusses centennial scale CH4 variability using previously published records. It does not report new data. [ Xavier Faïn, France]	Taken into account in edits to caption and FAIR data table.
29803	17	9	17	9	In the Figure 2.4 (particularly in 2.4 (b) and (c)) consider the inclusion of vertical bars or bands (as in Figure Cross-Chapter Box 2.1, Figure 1) or horizontal lines (as in Figure 2.21 (a)) to reference the periods mentioned in the main text (LGM, MWP, LIA, etc.). [ Hernan Edgardo Sala, Argentina]	Accepted. LGM, MWP, LIA and LDT are indicated with bars.
26623	17	9	17	9	Figure 2.4 : The x-axis in these figures is questionable, since the 'x10 <sup>3</sup> ' (for thousands of years) are 'lost' in the left part of the figure. A common age scale in kilo-years (even for the lower right panel) should be more explicit. [ Eric Brun, France]	Accepted. Now the x-axis labels in (a) and (b) are "thousands of years before 2000", and in (c) "Year (CE)"
73343	17	9	17	12	References should be listed in chronological order for consistency elsewhere. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
35511	17	10	17	10	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Accepted.
43029	17	12			Read « (c) Multiple high-resolution"rather than "(c). Multiple high-resolution » [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
1215	17	21	17	29	Acronyms such as WMGHGs make the text cryptic. Readers need to remember all the acronyms while reading a complicated text. It's better to spell out non-standard acronyms. This applies to the whole all chapters and the whole report. [ Rasmus Benestad, Norway]	Rejected. While we agree in general with this remark, in this particular case I think readers will understand. Moreover the term is spelled out in the section header of 2.2.3.
73345	17	23	17	23	Replace 'averages' with 'means' (as more scientific term and also for consistency elsewhere). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Average is correct.
73347	17	24	17	24	Replace 'averages' with 'means' (as more scientific term and also for consistency elsewhere). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Average is correct.
57607	17	28	17	28	In text literature citation "Meinshausen et al., 2017a". In the reference list: "Meinshausen et al. (2017a)" and "Meinshausen et al. (2017b)" are the exact same paper (p. 130, lines 33-38) Remove one of them from the reference list and the letters "a" and "b" from in-text citations. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected.
67661	17	28	17	28	it would be useful to say what data the CMIP6 historical dataset is based on (rather than the reader having to go to the reference). [ Karen Rosenlof, United States of America]	Accepted. While for space reasons we cannot provide too much detail on specific datasets, we include some more information in the Annex 3( former 5).
81307	17	34	17	34	All compounds in this table seem to be listed by decreasing mixing ratio/radiative forcing within each substance class – but why not the HFCs? Also, a footnote should be added to clarify that what is shown as "CFC-114" actually contains an unquantified amount of CFC-114a. [ Johannes Laube, Germany]	Accepted. HFCs have been listed in order of ERF, like for other gases. A footnote for CFC-114/114a has been added.
19003	17	34	17	45	Table 2.3: CO2 lifetime is estimated by model, see F. Joos et al., Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: a multi-model analysis. Atmos. Chem. Phys. 13, 2793-2825 (2013) [ Mengze Li, Germany]	Accepted. We changed the entry to reflect the issue of multiple adjustment and turn-over times involving multiple compartments. See also Glossary.
30107	17	34			Table 2.3: the starting year for ERF is missing (1750, 1850, other?), this is especially important given the level of precision on the ERF. [ Gilles Delaygue, France]	Taken into account - added 1750
57609	17	35	17	35	"ERF ≥ 1 Wm <sup>-2</sup> " instead of "ERF ≥ 1 mWm <sup>-2</sup> " [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Reject. For clarity changed to 0.001 Wm <sup>-2</sup> .
108299	17	35	17	35	Change 1 mWm <sup>-2</sup> to 0.001 W m <sup>-2</sup> to make it clear. [ Won-Tae Kwon, Republic of Korea]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126917	17	39	17	39	There are some overlaps between networks and this should be noted because it means the spatial and temporal extents of the networks might not be as large as it would seem. [ Trigg Talley, United States of America]	Accepted. Some networks are subsets of WMO/GAW. For example NOAA, AGAGE, SIO, and CSIRO are all subsets of GAW but not all measure on same calibration scale. A clarification is added in the table caption/FAIR data table.
16479	17	40	17	42	The decision of which network(s) to use for the ERFs needs to be more explicit. I would suggest an extra row for each species labelled "AR6" which is the assessed value to be passed down to table 7.5. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We note with an asterisk which network(s) was used for the ERF calculation.
26625	17	42	17	42	The unit for "lifetime" is missing [ Eric Brun, France]	Accepted. Lifetimes are reported in years, and included in the table footer.
15919	17	42	17	42	Tropospheric ozone should also be included [ Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Reject. This section is only about WMGHGs and a separate section is devoted to tropospheric ozone and other SLCFs.
29805	17	42	17	42	Please, add lifetime unit in the header of the first column of the Table 2.3 (in "Species (lifetime))". [ Hernan Edgardo Sala, Argentina]	Accepted. Added.
73349	17	42	17	42	Replace 'average' with 'mean' (as more scientific term and also for consistency elsewhere). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Average has been replaced by mean.
126919	17	42	19	1	Comment on Table 2.3: Consider adding a column showing the magnitude or percent change in ERF from 2011 to 2018 for each species. The rate of change of ERF is more policy-relevant than the ERF itself, and that value is not directly derivable from just the data in the table. The text does provide one summary value for synthetic GHGs (page 22, line 8). [ Trigg Talley, United States of America]	Rejected. For space reasons, it was not given here, but more information can be found in Chapter 7 and Annex 3.
473	17	42	19	1	In Table 2.3, please indicate the units for the Lifetime values (presumably years). [ Claire Parkinson, United States of America]	Accepted.
57611	17	42			Table 2.3: in first column, clarify that lifetime=years, e.g. Species lifetime (years) and in column four, put a bracket around "2011 to 2018". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Lifetimes are reported in years, and included in the table footer.
57613	17	42			Table 2.3: Recommendation: move the sixth column "Network/Dataset" to the second column next to "Species (lifetime)" for clarity. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We did not think it was improving clarity.
26049	17		17		Please, include units for lifetime [ Don Alfonso Pino Maeso, Spain]	Rejected. The unit of the lifetime is presented in section 2.2.3.
115963	17		17		What is the source of information for the reported lifetimes? Slightly different numbers are reported in ch 5, please check. Also, check glossary / lifetime / CO2. [ Valerie Masson-Delmotte, France]	Accepted. Lifetimes are now harmonized across ch. 2, 5,6, and 7. Specifically changed: the entries for CO2;N2O; SF6. The FAIR data table gives further information.
32657	17		18		It is better to convert Table 2.3 to a graph [ sadegh zeyaeyan, Iran]	Rejected. This table is providing detailed information on changes, and network differences along with other information.
32987	17		18		It is better to convert Table 2.3 to a graph [ Sahar Tajbakhsh Mosalman, Iran]	Rejected. This table is providing detailed information on changes, and network differences along with other information.
39575	18	1	18	13	Please compare Fig. 2.5 (d) with GSAT and explain the lag of CO2 with respect to temperature as shown for example by Humlum, O., Stordahl, K., Solheim, J.E., 2013. Global & Planetary Change 100, 51, updated each month in www.climate4you.com. The absence of this comparison and its analysis is a major lack of AR6 report. Does CO2 would be a so demonic gas that it would heat the Earth BEFORE it has been emitted? [ François Gervais, France]	Rejected. The mechanisms and relationships between CO2 and temperature are further discussed in Chapter 5 including the carbon budget concept.
43031	19	6			Read "boundary layer sites, updated from Conway et al. (1994), Dlugokencky et al. (1994), and Masarie and Tans (2004)." Rather than "boundary layer sites, updated from (Conway et al., 1994; Dlugokencky et al., 1994; Masarie and Tans, 2004)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57615	19	7	19	7	Table 2.3: "from Hall et al. (2011)" instead of "from (Hall et al., 2011)". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
126921	19	7	19	8	In both instances replace "11" with "12" sites. [ Trigg Talley, United States of America]	Accepted. It should indeed be 12 sites.
43033	19	7			Read "11 sites, updated from Hall et al. (2011)." Rather than "11 sites, updated from (Hall et al., 2011)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
43035	19	9			Read "GC-MS analysis, updated from Montzka et al. (2009)." Rather than "GC-MS analysis, updated from (Montzka et al., 2009)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
126923	19	11	19	11	Insert "four times per year" after "measurements". [ Trigg Talley, United States of America]	Accepted. The reviewer correctly notices that UCI data are derived from flask measurements collected four times per year at over 20 sites. Details available in the FAIR data table.
43037	19	11			Read "sites, updated from Rigby et al. (2014) and Prinn et al. (2018)." Rather than "sites, updated from (Rigby et al., 2014; Prinn et al., 2018)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
43041	19	12		13	Read "9 sites, updated from Langenfelds et al. (2002) and Kirschke et al. (2013)." rather than "9 sites, updated from (Langenfelds et al., 2002; Kirschke et al., 2013)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
43039	19	12			Read "from Simpson et al. (2012)." rather than "from (Simpson et al., 2012)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
73351	19	15	19	15	Format of date needs tidying up (at least separate date, month and year). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
43043	19	21			Read "and SF6 from Kovács et al. (2017) and Ray et al. (2017)." rather than "and SF6 from (Kovács et al., 2017; Ray et al., 2017)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. The layout of the references is corrected and the FAIR data table has been completed.
24359	19	27	19	27	The text says that CO2 has increased since 1958 and mentions Figure 2.5a. But this figure only shows observed CO2 since 1980. The Mauna Loa CO2 record is the most important and well known WMO GHG time series in the modern era, so it seems appropriate that Figure 2.5 should show the full record. [ Owen Cooper, United States of America]	Accepted. The figure now includes SIO data for CO2 since 1958
126925	19	27	19	27	Should the date be 1957? Keeling began collecting samples from Little Antarctica and Mauna Loa in that year. [ Trigg Talley, United States of America]	Rejected. Mauna Loa data are first available in 1958, so a representative global mean derived consistently from surface observations (South Pole and Mauna Loa) is available from 1958.
5335	19	27			The ployd in F2.5a start in 1980 not 1958 as stated here. Actually the figures should start in 1958 with the earliest Keeling observations. [ Bryan Weare, United States of America]	Accepted. The figure now includes SIO data for CO2 since 1958.
6487	19	28	19	28	Shouldn't it be "imbalance" not "balance". CO2 increases because sources and sinks are not in balance. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. It is an imbalance.
57723	19	28	19	39	Again the sentence "Since 2011, {...} in 2018" is confusing. Does this mean "The average increase in annual mean CO2 over the period 2011-2018 was 4.3%, reaching 407.2 +/- 0.17 ppm in 2018." or "The total increase in mean CO2 over the period 2011-2018 was 4.3%, reaching 407.2 +/- 0.17 ppm in 2018." or "The total increase in mean CO2 over in the year 2018 was 4.3%, reaching 407.2 +/- 0.17 ppm.". All seem possible interpretations of the current phrasing. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text clarified.
43045	19	30		31	Read "Annual growth rates have varied substantially (Figure 2.5d), " rather than "Annual growth rates, (Figure 2.5d), have varied substantially, " [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Changed accordingly.
16481	19	32	19	32	This could also discuss the more recent rates that exceed 2 ppm/yr. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The recent growth rates are included.
73353	20	6	20	6	Delete , before 'and' (x2). It is not required in this context. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We haven't understood the issue being raised by the reviewer here.
73355	20	7	20	7	Replace 'averages' with 'means' (as more scientific term and also for consistency elsewhere). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43047	20	8			Read "are shown in corresponding right-hand panels (Dlugokencky et al., 1994)," rather than "(Dlugokencky et al., 1994) are shown in corresponding right-hand panels," [ Cyriaque Rufin Nguimalet, Central African Republic]	Rejected. The text is clear as is.
126927	20	10	20	11	Add "the" after "prevent"; add "accurate growth rates for" after "of"; delete "growth rates" after "N2O". [ Trigg Talley, United States of America]	Accepted. Rephrased to: Insufficient and noisy data prevent the calculation of accurate growth rates for N2O prior to 1995.
57619	20	18	20	18	To shorten the text and be stylistically consistent with 2.2.3.2.3, replace "based on measurements from the NOAA network" with "(NOAA measurements)". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed accordingly.
29809	20	19	20	19	Add a point after "(Figure 2.5b)". [ Hernan Edgardo Sala, Argentina]	Accepted. We have added a point.
43049	20	19			Read "from 2.9-3.3% (Figure 2.5b). There are marked growth rate changes" rather than "from 2.9-3.3% (Figure 2.5b) There are marked growth rate changes" [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. We have added a point.
57617	20	20	20	20	Reference to "Figure 2.5d" is erroneous as this figure represents the growth rate of CO2, not CH4. Change the reference to "Figure 2.5e" which refers to the growth rate of CH4. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The review is correct, changed to 2.5e
29811	20	20	20	20	Replace "(Figure 2.5d)" by "(Figure 2.5e)". [ Hernan Edgardo Sala, Argentina]	Accepted. The review is correct, changed to 2.5e
19705	20	26	11	31	The text should indicate where in the report possible explanations for the changing growth rate can be found. [ philippe waldteufel, France]	Noted. The matter is further discussed in chapter 5.
73357	20	31	20	31	Delete 'in recent years' and replace with 'from 2009-2018' (less clumsy and more precise sentence). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Changed to precise years.
30021	20	36	20	36	delete "were" [ Yihui Ding, China]	Reject. The word is meant to be here.
126929	20	37	20	38	Doesn't the ice core record back to 800Kya tell us that pre-industrial levels and growth rates were lower than recent? This is a strange sentence for that reason. [ Trigg Talley, United States of America]	Accepted. Rephrased.
57621	20	39	20	42	It would be more instructive to display the mixing ratio increases by the difference (both concentration and percentage) between LGM and 1750, rather than from "what concentration" to "what concentration" between the periods. This would also be consistent with the second part of the sentence which displays the difference in the mixing ration between 1750-2018. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Reject. The summary statement needs to succinctly mention the key findings.
7407	20	40	20	44	Hard to read. Could you do a bullet list for the different periods. [ Jeremy PANTHOU, France]	Reject. While we agree that there is a somewhat long list of findings in these sentences, the summary statements can not work with bullets.
90275	20	42			"46.2% for CO2" reads better in English; similarly for rest of sentence with the other gasses [ Jeannine-Marie St-Jacques, Canada]	Accepted. Changed accordingly.
30023	20	43	20	43	add "have" before further [ Yihui Ding, China]	Accepted. Changed accordingly.
57623	20	43	20	45	Remove "over 2011" on line 43 since it is already mentioned in the beginning of the sentence, and add percentages in brackets for the increases in mixing ratio concentrations. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed accordingly.
30109	20	44	20	45	The starting year for ERF is missing (1750, 1850, other?) [ Gilles Delaygue, France]	Accepted. We add that the ERF is relative to 1750.
90277	20	45			relative to when? [ Jeannine-Marie St-Jacques, Canada]	See response to 30109
29813	20	48	20	48	"PFCs" have not been defined along this chapter. Please include a short definition in this section. [ Hernan Edgardo Sala, Argentina]	Reject. PFCs are defined in Figure caption 2.6 PFCs include CF4, C2F6, and C3F8; Halons include halon-1211, halon-1301, and halon-2402.
81305	20	48	20	48	This section, as opposed to most other parts of the chapter, seems to have acquired more inconsistencies during the first revision; notwithstanding that there have been improvements, too, of course. As a first comment, some coordination of the terminology with Chapters 6 and 7 would be advisable as various, partly overlapping terms are used (including WMGHGs, LLGHGs, synthetic GHGs, halocarbons, halogenated species, and even "halogens"). [ Johannes Laube, Germany]	Accepted. In this chapter for convenience the term WMGHGs is used- to avoid artificial division of GHG shorter and longer than 20 years (which is the definition of Short versus long-lived). We now use the term halogenated species throughout the section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67823	20	50	20	52	The statement was based on research, which proves the decline in CFCs until 2011 was due to the Montreal Protocol. However, it is also necessary to ensure that there are no other influences causing the decline. [ Ruandha Agung Sugardiman, Indonesia]	Noted. Further assessment on factors contributing to decline are given in Chapter 6
68257	20	50	20	54	HCFCs and HFCs are now both included under the Montreal Protocol. HCFC production and consumptions is currently being reduced in both developed and developing countries, and HFC production and consumption is being reduced in developed countries and will start being reduced in developing countries in 2024. See UNEP Fact Sheet, Kigali Amendment to the Montreal Protocol: HFC phase-down, available at: <a href="https://multimedia.3m.com/mws/media/13659240/unep-fact-sheet-kigali-amendment-to-mp.pdf">https://multimedia.3m.com/mws/media/13659240/unep-fact-sheet-kigali-amendment-to-mp.pdf</a> . [ Durwood Zaelke, United States of America]	Noted. Thank you for this information. Unfortunately, the scope of this chapter does not allow to provide this extensive information, which can be found in Chapter 6.
66747	20	50	20	54	HCFCs and HFCs are now both included under the Montreal Protocol. HCFC production and consumptions is currently being reduced in both developed and developing countries, and HFC production and consumption is being reduced in developed countries. [ Kristin Campbell, United States of America]	Noted. Thank you for this information. Unfortunately, the scope of this chapter does not allow to provide this extensive information, which can be found in Chapter 6.
69865	20	50	20	54	See latest fact sheet on Montreal Protocol Kigali Amendment for phasedown schedules for HFC and HCFC. UNEP Fact Sheet, Kigali Amendment to the Montreal Protocol: HFC phase-down, available at: <a href="https://multimedia.3m.com/mws/media/13659240/unep-fact-sheet-kigali-amendment-to-mp.pdf">https://multimedia.3m.com/mws/media/13659240/unep-fact-sheet-kigali-amendment-to-mp.pdf</a> . [ Gabrielle Dreyfus, United States of America]	Noted. Future changes are out of chapter scope.
19005	20	52	20	53	not all HCFCs are increasing, e.g.HCFC-142b [ Mengze Li, Germany]	Accepted. This sentence reports the situation at the time of writing of AR5. Major HCFCs were increasing at that time. To avoid confusion changed to: "Prior to 2011, abundances of most HCFCs were increasing ...
80255	20	52	20	53	The HCFC were already capped by the Montreal Protocol [ Sophie Godin-Beekmann, France]	Accepted. This sentence reports the situation at the time of writing of AR5, and the production CAPs were introduced around that time (and couldn't have influence the growth rates yet). Text clarified.
73359	20	56	20	56	Capital initials for 'industrial revolution' ('Industrial Revolution'). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Spelling harmonized through chapter and report as part of copy edits.
30609	20	56	21	1	"ozone-depleting substances" should be explained somewhere in the text. Not everyone knows about ODS. [ Hong Liao, China]	Reject. We refer to the glossary for a further explanation.
115965	20		20		Missing assessment of rates of increase in GHG linked to the assessment of contributions to rates of change of RF. [ Valerie Masson-Delmotte, France]	Rejected. A discussion on the contributions of some individual WMGHGs to overall ERF is given in section 2.2.8 for CO <sub>2</sub> , CH <sub>4</sub> , N <sub>2</sub> O, CFCs, HCFC and HFCs. Further assessment is undertaken in chapter 7 which we cross-reference. Annex 3 gives further quantification.
90279	21	1			ODSs is redundant since ODS is already plural [ Jeannine-Marie St-Jacques, Canada]	Accepted. Changed.
126931	21	2	21	2	Replace "Scientific Assesment of Stratospheric Ozone" with "Scientific Assessment of Ozone Depletion". [ Trigg Talley, United States of America]	Accepted. The reviewer is correct. Changed accordingly.
21233	21	3	21	3	Add the following reference after "3200 years": @article (Ravishankara194, author = {Ravishankara, A. R. and Solomon, S. and Turnipseed, A. A. and Warren, R. F.}, title = {Atmospheric Lifetimes of Long-Lived Halogenated Species}, volume = {259}, number = {5092}, pages = {194--199}, year = {1993}, journal = {Science} ) [ Michael Schmitt, Germany]	Rejected. Reference pre-dates the literature window we are charged with assessing.
105083	21	6	21	6	Chlorofluorocarbons (with an l after the f) => I am not giving all my editorial comments here, but I know the those in titles often stick out! [ Masa KAGEYAMA, France]	Accepted. Thanks for spotting this.
81309	21	6	21	6	"Chlorofluorocarbons" is spelled wrong (also in the ToC), but perhaps it would be better to just say "CFCs" here as this would be more consistent with the naming of the other sections. [ Johannes Laube, Germany]	Accepted. Thanks for spotting this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98737	21	6	21	6	Isn't it Chlorofluorocarbons? I think an 'l' is missing. [ Meredith Parish, United States of America]	Accepted. Thanks for spotting this.
81311	21	6	21	55	The gases included in these categories should at least be listed in the text, Annex and/or in the caption of Figure 2.6. For instance, SO <sub>2</sub> F <sub>2</sub> and NF <sub>3</sub> seem to have completely disappeared since the last draft, but are listed in Annex V and therefore presumably still included in the radiative forcing totals? C <sub>3</sub> F <sub>8</sub> has disappeared from this section but is still listed in the caption of Figure 2.6 – but not other PFCs (see also comment on that Figure). Some others (like H <sub>2</sub> 402, CH <sub>3</sub> CCl <sub>3</sub> , CH <sub>3</sub> Cl, CH <sub>3</sub> Br, CH <sub>2</sub> Cl <sub>2</sub> and CHCl <sub>3</sub> ) appear in Table 2.3 and/or in Figure 2.6, but are not mentioned or listed anywhere in the text – and only partly in Chapter 6. In summary, a more consistent approach would help. [ Johannes Laube, Germany]	Accepted. We include a sentence at the end of the section 2.2.3 heading: "... mixing ratios of the*radiatively* most important gases are reported in Table 2.3, while other gases are listed in Annex 5.
30111	21	6			'chlorofluorocarbons' [ Gilles Delaygue, France]	Accepted. Thanks for spotting this.
475	21	8	21	10	The values given here for CFC-11 match those in the referenced Table 2.3, using the AGAGE row in the table (which should be mentioned), but the values given for CFC-12 are not the same as what is in Table 2.3. Since Table 2.3 is referenced, it seems that either the text or the Table should be corrected. [ Claire Parkinson, United States of America]	Taken into account. Text and table have been reconciled.
13229	21	11	21	11	HFC must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Accepted. Acronym is spelt out now.
126933	21	12	21	12	Is it possible to put uncertainties on these two numbers (8% and 4%)? [ Trigg Talley, United States of America]	Accepted. Uncertainties on the changes in mixing ratios can be translated to uncertainties in ERF changes.
115535	21	14	21	18	Any new information on the issues regarding emission estimates and lifetime estimates for carbon tetrachloride? [ Rolf Müller, Germany]	Accepted. CCl <sub>4</sub> is now included.
126935	21	15	21	15	Insert "changes in CFC-11 loss rates consistent with" before "production". Replace "has" with "have". [ Trigg Talley, United States of America]	Accepted. We have modified the sentence into: "While there has been practically no reported production of CFCs since 2010 and the atmospheric abundance of CFC-11 is still decreasing, changes in emissions consistent with unreported production have been inferred from atmospheric observations (Montzka et al., 2018; Rigby et al., 2019)."
126937	21	15	21	15	Replace "has been detected" with "has been indicated by the unexpected emission increase documented after 2012". [ Trigg Talley, United States of America]	See response to 126935
32497	21	15	21	16	Please replace "unreported production has been detected" with "a slowdown in the decline of the atmospheric concentrations of CFC-11 after 2012 has been detected, suggesting unreported new production and use". [ Sophia Mylona, Kenya]	See response to 126935
4619	21	18	21	18	Units should be provided. [ Andries Kruger, South Africa]	Rejected. GWP is unitless.
16483	21	18	21	18	I don't think this needs to explicitly quote the GWP100. "increases the emission metrics (Global Warming Potential or Global Temperature-change Potential), see table 7.15." should be sufficient. Section 7.6 explicitly refrains from highlighting GWP100 as having any more relevance than any other metric. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Agree. Implemented.
30113	21	23			'selected' ('select' means of 'high quality') [ Gilles Delaygue, France]	Accepted. Corrected to selected.
43051	21	24		25	Read "NOAA (updated from Montzka et al. (2009))" rather than "NOAA (updated from (Montzka et al., 2009))" [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Reference format corrected.
105487	21	25	21	25	Extra bracket [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference format corrected.
43053	21	25			Read "AGAGE (updated from Rigby et al. (2014) and Prinn et al. (2018))" rather than "AGAGE (updated from (Rigby et al., 2014; Prinn et al., 2018))" [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Reference format corrected.
13231	21	26	21	26	PFC must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Accepted.
30115	21	28	21	29	What is the aim of this sentence about 1750 levels? (1750 is not shown, and these gases are not shown on the figures) Shouldn't it be placed in the text? [ Gilles Delaygue, France]	Accepted. Levels can be found in Annex 5.
126939	21	29	21	29	Replace "CH <sub>3</sub> Cl <sub>3</sub> " with "CH <sub>3</sub> CCl <sub>3</sub> ". [ Trigg Talley, United States of America]	Accepted. Corrected.
13233	21	36	21	36	HCFC must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Accepted. Introduced at first occurrence.
80257	21	36	21	37	Rates of increase of HCFC atmospheric abundance have decrease due to the enforcement of the Montreal protocol. [ Sophie Godin-Beekmann, France]	Taken into account in text edits.
43055	21	39			Read "For the 2011-2018 period, the UCI network " rather than "For the period 2011-2018, the UCI network " [ Cyriaque Rufin Nguimalet, Central African Republic]	Rejected. Not an improvement.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81313	21	52	21	55	Both HFC-125 and -143a increased by >10 ppt and have much higher radiative forcing than -152a and -32. It is therefore not clear to me, why only -32 and -152a are being discussed here – especially since they are both SLCFs which mainly belong to Chapter 6. Some coordination with Chapter 6 would help. [ Johannes Laube, Germany]	Taken into account in section text edits.
126941	21	53	21	53	Insert "owing to its comparatively short lifetime" after "abundance". [ Trigg Talley, United States of America]	Accepted. Included accordingly.
81315	22	1	22	2	Not all continue to increase, particularly notable being the steep decline of CH <sub>3</sub> CCl <sub>3</sub> . [ Johannes Laube, Germany]	Taken into account in section text edits.
81317	22	2	22	3	For SF <sub>6</sub> , please consider the update provided by Simmonds et al., 2020 (ACP). [ Johannes Laube, Germany]	Taken into account in section text edits.
126943	22	8	22	9	Replace "predominantly" with "both" and replace "overcompensated by" with "and". The sentence the way it is currently written sounds like a loss of something is causing its increase. [ Trigg Talley, United States of America]	Accepted. Included accordingly.
57625	22	8	22	11	Considering that this paragraph summarizes the whole subsection 2.2.4, it would be useful to separate it from 2.2.4.3, and give it its own title, e.g. "2.2.4.4 Summary of changes in Synthetic Greenhouse Gases", similarly to the previous section on WMGHGs. Adding uncertainty language would also be useful. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. New subsection introduced.
16485	22	8	22	11	If this really needs to list the halocarbon ERFs, it needs to refer to section 7.3.2.4 and table 7.5. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Include a reference to these sections.
81319	22	8	22	11	Chapter 7 states (as a main message) that "The net ERF attributable to halocarbons is smaller than the direct ERF due to their effect on ozone depletion, such that the range includes zero (0.0 to 0.16 W m <sup>-2</sup> )." While that statement is only supported by two new studies and it is unclear which compounds were included in those, there should be some coordination between the chapters in order to get a consistent message. [ Johannes Laube, Germany]	Accepted. Include a statement on the relationship between ERF of halocarbon and ozone depletion. (Chapter 7).
80259	22	9	22	11	I don't see any reference for this statement on radiative forcing of ozone depleting substances and their substitutes. Why not cite the WMO Assessment on the state of the ozone layer (2018) or reference therein? [ Sophie Godin-Beekmann, France]	Accepted. We added a reference to Chapter 7, where the references for the calculations are given.
81321	22	11	22	11	So what is actually included in the 0.02 W m <sup>-2</sup> from "remaining predominantly synthetic compounds"? This leads to multiple questions: Is CCl <sub>4</sub> counted as a CFC despite containing no fluorine? Are just the compounds listed in Table 2.3 included or all of those listed in Annex V? If the latter, one cannot retrace the total as ERFs there are given in W m <sup>-2</sup> and as zero for many compounds. Chapter 7 seems to have a different list of halocarbons as compared to the one here (Table 2.3). Some clarification and coordination with Chapter 7 would help to ensure a clearer and more consistent approach. [ Johannes Laube, Germany]	Accepted. Numbers are revised. Numbers refer to CFCs, HCFCs HFCs and other halogenated gases (including those not listed in the chapter, but presented in Annex 3).
30117	22	11			The starting year for RF is missing (1750, 1850, other?) [ Gilles Delaygue, France]	Rejected. The reference year for ERF calculations is 1750 as stated upfront in the chapter.
81323	22	14	22	14	Why are short-lived gases not mainly covered by Chapter 6, which explicitly deals with SLCFs? At the very least, section 2.2.5.1 should point towards that Chapter as is the case in the sections following it. Alternatively, a short introduction explaining why these are covered here would be nice – though I'm not sure why more than 4 pages are needed in this chapter. [ Johannes Laube, Germany]	Rejected. The scoping of climate drivers is explicitly included for Chapter 2, this has been done in coordination with Chapters 5, 6 and 7.
7977	22	14	24	55	Sitnov, S. A., Mokhov, I.I, Lupo, A.R. 2017: Ozone, water vapor, and temperature anomalies associated with atmospheric blocking events over Eastern Europe in spring - summer 2010. Atmospheric Environment, 164, 180 - 194. They demonstrate that atmospheric blocking can lead to short term positive water vapor anomalies in the upper troposphere, lower stratosphere, and negative Ozone anomalies in association with atmospheric blocking. [ Anthony Lupo, United States of America]	Rejected. Thank you for the reference. This short section focuses on long-term trends in stratospheric water vapor (SWV) and the mechanisms that drive them. Though atmospheric blocking events can produce variability in SWV, the changes are short-term, therefore the reference has not been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126945	22	14	24	55	Sitnov et al. (2017) demonstrate that atmospheric blocking can lead to short-term positive water vapor anomalies in the upper troposphere, lower stratosphere, and negative ozone anomalies in association with atmospheric blocking. Citation: Sitnov, S. A., Mokhov, I.I., Lupo, A.R. 2017: Ozone, water vapor, and temperature anomalies associated with atmospheric blocking events over Eastern Europe in spring - summer 2010. Atmospheric Environment, 164, 180 - 194. [ Trigg Talley, United States of America]	Rejected. This short section focuses on long-term trends in stratospheric water vapor (SWV) and the mechanisms that drive them. Though atmospheric blocking events can produce variability in SWV, the changes are, short-term, therefore the reference has not been added.
30611	22	14	25	26	Section 2.2.5 "Short-lived gases" includes only stratospheric water vapor, stratospheric O3 and tropospheric O3, which are not consistent with the definition of short-lived gas-phase climate forcers in Chapters 6 and 7. In chapters 6 and 7, we also have gas-phase climate forcers such as SO2, NOx, CO, NMVOCs, and NH3. Need some cross-chapter coordination here. [ Hong Liao, China]	Accepted. We include here only those components that exert a direct ERF, and not precursor components. This is explained in section introduction.
105489	22	16	22	16	mis-spelling of vapour [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Spelling is harmonized across the IPCC report and follows UK spelling rules.
73361	22	16	22	16	Change 'vapor' to 'vapour' (for parity with rest of section). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Spelling is harmonized across the IPCC report and follows UK spelling rules.
36957	22	16	22	51	Stratospheric water vapour is so minute in quantity that it is not worth mentioning. Or if you are going to mention it then you also need to discuss the overlapping bandwidths in which ALL greenhouse gases, including water vapour, operate at lower levels. [ John McLean, Australia]	Rejected. A good background reading reference for the importance of stratospheric water vapor as a climate forcer is found in: Solomon et al. (2010), "Contributions of stratospheric water vapor changes to decadal variations in the rate of global warming", Science, 327, 1219-1223.
9925	22	16	22	51	This section "Stratospheric Water Vapor" could be reduced somehow, especially given that only one-point record is considered and possible trend estimates in SWV have thus low confidence. [ Olga Zolina, France]	Reject. The section is already very short to bring across the main issues.
30119	22	16			homogenize 'vapor' or 'vapour' throughout [ Gilles Delaygue, France]	Accepted. Spelling is harmonized across the IPCC report and follows UK spelling rules.
126947	22	19	22	21	Statement needs reference at end of sentence. [ Trigg Talley, United States of America]	Reject: The sentence refers to a summary statement from the AR5 and does not need additional referencing.
115537	22	20	22	21	Is there a citation for the Boulder trend? Also it might be worth noting that Boulder is a rather unique location which can be both under mid-latitude and tropical influence. It is also not a good indicator for a zonal mean trends at 40N (e.f. Kunz et al., JGR, 2013 (Extending water vapor trend observations over Boulder into the tropopause region: trend uncertainties and resulting radiative Forcing). [ Rolf Müller, Germany]	Rejected. This is the AR5 finding, and underlying references can be found there.
3349	22	22	23	29	It is essential to draw on examples of a broadening of ideas [ Eduardo Erazo Acosta, Colombia]	Rejection. The suggestion is not clear.
93521	22	26	22	29	This text is confusing, since it seems to imply that the positive Boulder trends are correct (which would then result in these lines contradicting L23-25). It also misrepresents the discrepancies Hegglin et al (2014) and Lossow et al (2018). Both studies in fact agree with each other that subsampling a model or observations (which are in themselves consistent datasets) at the Boulder location does not lead to differences when compared to their full zonal means (Figure 3 in H et al and Fig 2 in L et al). H et al and L et al then come to the same conclusion that there must be another explanation for the differences in the trends between satellite and Boulder in-situ observations. H et al put the hypothesis forward that the Boulder location may not be resolved by the model so that the representativeness of Boulder could not be entirely proven. This hypothesis again was picked up by L et al., since they could not explain their findings either, namely that there are differences in the trends derived from the different model and observations available. [ Michaela Hegglin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence was changed into: "Hegglin et al. (2014) reported a latitudinal dependence of SWV trends and suggested that the upward trend over Boulder should not be considered representative of the global stratosphere, while Lossow et al. (2018) showed insignificant differences between SWV trends at Boulder and those for the 35-45°N zonal mean from 1980 to 2010 using model simulations and satellite observations. "



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82281	22	26	22	29	Given the results shown in Hegglin et al. (2014) I propose to slightly extend the current sentence: "While Hegglin et al. (2014) suggested a latitudinal dependence of SWV trends and that the upward trend over Boulder should not be considered representative of the global stratosphere, Losow et al. (2018) showed..." [ Schröder Marc, Germany]	Accepted. Sentence was changed into: "Hegglin et al. (2014) reported a latitudinal dependence of SWV trends and suggested that the upward trend over Boulder should not be considered representative of the global stratosphere, while Losow et al. (2018) showed insignificant differences between SWV trends at Boulder and those for the 35-45°N zonal mean from 1980 to 2010 using model simulations and satellite observations. "
70869	22	26	22	29	This sentence is in contradiction to what is said immediately before, on lines 24-26. In particular, this sentence suggests that the upward trends observed over Boulder *are* representative of the global stratosphere, yet on lines 24-26 we have a clear statement (based on several studies) that there is *no* upward trend in the global stratospheric water vapour. Suggest deleting the sentence on lines 26-29, which is rather convoluted anyway, [ Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	See response to 93521
43057	22	27			Read "the global stratosphere. Losow et al. (2018)" rather than "the global stratosphere, Losow et al. (2018)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted. Sentence was changed into: "Hegglin et al. (2014) reported a latitudinal dependence of SWV trends and suggested that the upward trend over Boulder should not be considered representative of the global stratosphere, while Losow et al. (2018) showed insignificant differences between SWV trends at Boulder and those for the 35-45°N zonal mean from 1980 to 2010 using model simulations and satellite observations. "
132325	22	30	22	41	This paragraph is not informative. It does not explain why aridity is a possible "climate-impact driver". No impacts are mentioned. Please clarify. [ Sonia Seneviratne, Switzerland]	Rejected. This comment seems to refer to a different chapter.
30121	22	33			'WarMings' [ Gilles Delaygue, France]	Editorial. Copyedits applied on finalisation.
73363	22	34	22	34	Capital 'T' for 'tropopause' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedits applied on finalisation.
81467	22	35			to add (The trace gas response to the MJO is strongly coherent with circulation anomalies showing strong seasonal differences. The stronger equatorial Kelvin wave front during the summer produces enhanced upwelling in the tropical tropopause layer, resulting in significant cooling of this region, reduced ozone and water vapor, and enhanced carbon monoxide. (Research paper : Seasonality of the MJO impact on upper troposphere/lower stratosphere temperature, circulation and composition - Olga V Tweedy, Luke D. Oman and Darryn W. Waugh- Journal of Atmospheric Science [ Kyaw Moe Oo, Myanmar]	Rejected. This short section focuses on long-term trends in stratospheric water vapor (SWV) and the mechanisms that drive them. The MJO can drive variability in lower SWV over shorter periods of few months, which are not relevant to the long-term changes assessed in CH2. Thus, the reference has not been added.
115539	22	36	22	36	For convective ice lofting see also results from the StratoClim campaign (Lee et al., 2019, ACP, Convective hydration in the tropical tropopause layer during the StratoClim aircraft pathway of an observed hydration patch; Krämer et al., ACPD, 2020) [ Rolf Müller, Germany]	Rejected. This short section focuses on long-term trends in stratospheric water vapor (SWV) and the mechanisms that drive them. References included here discuss how ice lofting can moisten the lower stratosphere over large regions. Though interesting, the suggested paper includes only smaller-scale experimental results.
73365	22	40	22	40	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Spelling is harmonized across the report.
73367	22	45	22	45	Please give the co-ordinates or position of the 'location' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Coordinates 40.0 N and 105.3 W included.
16487	22	49	22	51	The stratospheric water vapour ERF is entirely unrelated to the earlier discussion in this paragraph. You could make the point that the trend stratospheric water vapour attributable to methane is undetectable, but I see no reason to quote the ERF here. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. While we agree that the final ERF number is not based on the section, we do provide the reason for this. The number is further used in section 2.8

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69161	22	49	91	36	Please define "instrumental period". If it is the same as the usage in chap.1 P50 L51 ("since 1850"), please indicate so. Current implicit definition from cross-chapter box 2.1, Table1 is insufficient for a clear understanding. [ Kaoru Magosaki, Japan]	Accepted. We have clarified the period considered.
68259	23	1	23	50	Stratospheric ozone is starting to show signs of recovery, with noticeable improvements emerging expected by the 2030s with 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. [ Durwood Zaelke, United States of America]	Rejected. The statements pertain to future ozone change and are out of scope for Chapter 2.
66749	23	1	23	50	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. [ Kristin Campbell, United States of America]	Rejected. The statements pertain to future ozone change and are out of scope for Chapter 2.
69867	23	1	23	50	Stratospheric ozone is starting to show signs of recovery, with noticeable improvements emerging as expected by the 2030s withand 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. [ Gabrielle Dreyfus, United States of America]	Rejected. The statements pertain to future ozone change and are out of scope for Chapter 2.
16489	23	3	23	5	This is very dangerous mixing two concepts in a certainty statement (and I admit AR5 did say that). This reads as if it is *certain* that stratospheric ozone was was nearly constant in the mid-1990s to 2011 - it doesn't make sense to be certain something is nearly constant unless a range is given. So I assume the certainty should only apply to the decline. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The statement is taken from AR5. Certain here means there were no statistically significant trends. There are no grounds to retrospectively correct statements from the AR5, in particular since the finding is corroborated in the following text and figure.
126949	23	3	23	7	The 3.5% decline appears twice in the text, making this section seem a bit redundant. [ Trigg Talley, United States of America]	Rejected. The first number comes from AR5, and updates are provided here.
126951	23	7	23	12	What is changing and where? This paragraph needs some editing. What did the slight increasing? [ Trigg Talley, United States of America]	Accepted. We have divided the first sentences in two parts.
16491	23	7	23	13	It might be worth clarifying that Braesicke et al. is the WMO assessment [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is recommended to refer to the assessment by citing the authors. The interested reader can find from the reference that this is the WMO ozone assessment.
126953	23	7	23	17	Has the acronym "ODS" been defined at this point? [ Trigg Talley, United States of America]	Rejected. ODS has been defined in an earlier section and is described in the glossary.
93499	23	7			add ", " after annually [ Rahab KINYANJUI, Kenya]	Accepted. Changed into Annual mean.
90285	23	11			ODSs is redundant since ODS is already plural [ Jeannine-Marie St-Jacques, Canada]	Accepted.
93523	23	13	23	17	The last sentence should not indicate a controversy with Shepherd et al. Key to Shepherd et al is that it could explain why observations did not show pre-1980 ozone loss, namely due to the masking of the stratospheric ozone decline by tropospheric ozone increases over the same time period. The combined model-measurement approach used was able to disentangle the two trends of opposite sign. [ Michaela Hegglin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "However" was erroneously added to this sentence during editing of the final SOD which created a controversy with the previous sentence.
70875	23	16	23	17	Leaving aside the question of whether one station can be considered representative of global changes, this sentence suggests a potential contradiction with the previous sentence. However, that misunderstands the analysis in Shepherd et al. (2014). In fact, Shepherd et al. (2014) show that pre-1980 chemical depletion is entirely consistent with the lack of an observed decline in total column ozone prior to 1980, because of dynamical variability. So there is no contradiction with the Arosa record. [ Theodore Shepherd, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. "However" was erroneously added to this sentence during editing of the final SOD which created a controversy with the previous sentence.
93501	23	18			change "(Gromov et al., 2019)" to " Gromov et al.(2019) [ Rahab KINYANJUI, Kenya]	Rejected, Gromov reference is no longer used.
73369	23	22	23	22	Change 'Timeseries' to 'Time series'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The report uses harmonized spelling rules.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30123	23	22			maybe explain what means 'total' (vertically integrated), since this figure will be used by non experts [ Gilles Delaygue, France]	Noted. Total is explained in the figure caption.
13235	23	26	23	26	al missing . [ Maria Amparo Martinez Arroyo, Mexico]	Accepted. Corrected.
126955	23	31	23	32	It would be helpful to elaborate a bit on the altitude dependence of ozone radiative forcing. [ Trigg Talley, United States of America]	Rejected. For space limitations We refer to sections 6.2.2.5.2 and 7.3.2.5
73371	23	32	23	32	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Spelling is harmonized in chapter and report.
80261	23	32	23	33	According to the most recent assessment on long term trend of ozone vertical distribution in the stratosphere, ozone levels increased by 2.9% in the Northern mid-latitudes, 2.1% in the Tropics and 2.1 in the Southern mid-latitudes (Petropavlovskikh et al., SPARC/IO3C/GAW Report on Long-term Ozone Trends and Uncertainties in the Stratosphere, 2019 [ Sophie Godin-Beekmann, France]	Taken into account. The values in LOTUS Assessment, Table ES1 are given in % per decade for a 17-year long period. $2.1\% \times 1.7 = 3.6\%$ and $2.9\% \times 1.7 = 4.9\%$ . so 4% as in the text is not that far given that the uncertainties are about 3.4%. We include now a reference to Petropavlovskikh et al for completeness.
18255	23	35	23	36	This variability highlighted by Chipperfield et al. (2018) was discussed again in a more recent paper (Ball et al., 2019, ACP). Ball et al. argued this feature only concerned South Hemisphere midlatitudes, whereas the decrease in northern midlatitudes is significant at 90% and in the tropics at 95%. I found this precisions were important because they slightly change the take-home message. [ Yann Cohen, France]	Rejected. Ball et al., 2019 conclude that the probabilities cited were not entirely correct. "These decreases do not reveal an inefficacy of the Montreal Protocol; rather, they suggest that other effects are at work, mainly dynamical variability on long or short timescales, counteracting the positive effects of the Montreal Protocol on stratospheric ozone recovery. We demonstrate that large interannual midlatitude (30–60°) variations, such as the 2017 resurgence, are driven by non-linear quasi-biennial oscillation (QBO) phase-dependent seasonal variability. However, this variability is not represented in current regression analyses." Long-term natural variability simply was not reflected by the AR(1) autoregression model and therefore the probabilities were not entirely correct.
30025	23	38	23	43	it is possible to mention the Arctic ozone hole event in 2019 except for the case for 2011 [ Yihui Ding, China]	Accepted. However, we in particular mention the 2020 record low event.
80263	23	39	23	41	This statement is valid under the conditions of high chlorine loading, since the temperature will impact the volume of polar stratospheric clouds that provide chlorine activation in the polar stratosphere. [ Sophie Godin-Beekmann, France]	Rejected. While the comment is correct, Chapter 2 does not go into mechanistic details of the relationship between ozone and temperature and its modulation by chlorine.
5337	23	39			There is no evidence of a "recovery" for the October plot in e). At most there is a stabilization. [ Bryan Weare, United States of America]	Accepted. Indeed, the strongest signs of recovery are seen in September, not so much in October, while the plot shows October. We have removed the reference to the figure.
6489	23	41	23	43	Does this conclusion need to be changed due to the March 2020 event, even though it occurred too late for a citeable paper? [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We have included the 2020 event, and cited 2 papers.
115541	23	43	23	43	The statement about the Arctic polar ozone loss in 2011 is no longer true as there was very likely a stronger ozone depletion in 2020; se GRL/JGR special scetion; Manney et al, 2020; Grooß et al 2020 [ Rolf Müller, Germany]	Accepted. We have included the 2020 event, and cited 2 papers.
67665	23	45	23	50	I think it is misleading, and rather simplistic to say "In summary, ...ozone has declined..." If one looks at figure 2.7, and assumes that total ozone is a strat. ozone proxy, there is a decline into the mid 1990s, and then essentially no statistically significant trend, and the current value is lower than the 1960-1980 average. That's a whole lot of years with no "decline" and possibly an increase. The summary should reflect the temporal shape of the curve, and probably make mention of the Montreal Protocol in shaping the temporal evolution of stratospheric ozone. [ Karen Rosenlof, United States of America]	Accepted. We have included temporal aspects of the trends, also addressing other reviewer comments. We refer to the Montreal protocol and ODS in main section, but did not include this mechanistic/attribution aspect in the summary statement, as it is beyond the scope of chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16493	23	45	23	50	The statement on the stratospheric ozone ERF is unconnected to the rest of the paragraph. Unless the observational change can be compared to the models used in the ERF (which would be very useful) it doesn't seem necessary to quote a model result that is unconnected to the rest of the discussion. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. As all subsections in 2.2 mention the ERF, justifying the section, we keep the overall ERF (coming from Chapter 7).
115545	23	48	23	48	replace some by in particularly cold [ Rolf Müller, Germany]	Accepted. Included particularly cold.
79989	23	48	23	50	The unchanged value since AR5 is in contrast with the conclusions of Checa-Garcia, et al. (2018; <a href="https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL076770">https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL076770</a> ). In this paper, the stratospheric ozone radiative forcing increased from -0.03 in CMIP5 models to -0.07 in CMIP6 models. How is this discrepancy reconciled? Is it due to the forcing definition (ERF here, while it's RF in Checa-Garcia)? I suggest adding one short comment about this. [ Gabriel Chiodo, Switzerland]	Rejected. A more complete description of the literature taken into account to derive the ERF is given in Chapter 6 and 7.
73373	23	49	23	49	Insert 'times' after 'industrial'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. We replaced industrial by 1750, which is the exact year at the basis of the ERF calculations.
115967	23		23		Is it possible to define emergent signs of recovery (here, glossary)? [ Valerie Masson-Delmotte, France]	Rejected. It is beyond the section scope to define recovery to pre-ozone hole conditions.
68261	24	1	25	26	Include in this discussion that tropospheric ozone is a secondary pollutant that has numerous precursors, including methane, which is another powerful climate pollutant. [ Durwood Zaelke, United States of America]	Rejected. This is described extensively in Chapter 6, and we need to avoid excessive duplication.
66751	24	1	25	26	Include in this discussion that tropospheric ozone is a secondary pollutant that has numerous precursors, including methane, which is another powerful climate pollutant. [ Kristin Campbell, United States of America]	Rejected. While we agree with this statement, for brevity and to avoid excessive duplication, we refer to in Chapter 6.
69869	24	1	25	26	Note that tropospheric ozone is a secondary pollutant with methane as a major precursor, and that tropospheric ozone causes respiratory and heart disease as well as damage to crops. Hartmann D. L., et al. (2013) CHAPTER 2: OBSERVATIONS: ATMOSPHERE AND SURFACE, in IPCC (2013) CLIMATE CHANGE 2013: THE PHYSICAL SCIENCE BASIS, Working Group I Contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change, 172 ("Tropospheric ozone is a short-lived trace gas that either originates in the stratosphere or is produced in situ by precursor gases and sunlight (e.g., Monks et al., 2009). An important GHG with an estimated RF of $0.40 \pm 0.20$ W m <sup>-2</sup> (Chapter 8), tropospheric ozone also impacts human health and vegetation at the surface."). [ Gabrielle Dreyfus, United States of America]	Rejected. We agree with this statement but leave the discussion of impacts to Chapter 6.
90807	24	6			Model simulations show that recent weather conditions have reduced maritime ozone, counteracting the impact of the growing Southeast Asia's emissions (Reference: Twenty-Five Years of Lower Tropospheric Ozone Observations in Tropical East Asia: The Influence of Emissions and Weather Patterns) [ Vivien How, Malaysia]	Rejected. This comment refers to the summary of the findings from AR5 and therefore this statement can not be edited. The publication that is recommended by the referee is referenced by this assessment, as the ozone trend at the Hok Tsui site is included in Figure 2.8. The influence of shifting weather patterns on ozone interannual variability and trends is a well known phenomenon, as reviewed by Cooper et al. [2020] (also referenced by this review). However, if time series are long enough the long-term trend can be detected above the noise introduced by weather variability. For this reason the review mainly focuses on studies with time series longer than that are at least 20-years in length. Due to the tight word limit of this section, no discussion of this phenomenon is added.
16495	24	10	24	12	The statement on the tropospheric ozone ERF is unconnected to the rest of the paragraph. Unless the observational change can be compared to the models used in the ERF (which would be very useful) it doesn't seem necessary to quote a model result that is unconnected to the rest of the discussion. Where are the modelled percentage changes from? I couldn't see them in chapter 6. They need to be referenced. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. As all subsections in 2.2 mention the corresponding ERF, justifying the section, we keep the overall ERF in the text (coming from Chapter 7).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81325	24	14	24	18	Since there appears to be a clear contradiction between the findings of Yeung et al. and Gromov et al., more clarification is required here. [ Johannes Laube, Germany]	Rejected. We have removed the reference to Gromov et al., but assess a high uncertainty due to the use of only a single analysis.
30125	24	14			"do not exist" in the troposphere [ Gilles Delaygue, France]	Rejected. There are no reliable observations for troposphere (nor for stratosphere). In this section we discuss the troposphere.
43059	24	17		18	Read "further evaluation by Gromov et al. (2019) does not exclude " rather than "further evaluation by (Gromov et al., 2019) does not exclude " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Addressed in text edits.
81193	24	18	24	18	Possible re-wording required. [ Supriyo Chakraborty, India]	Editorial. Addressed in text edits.
36959	24	20	20	34	Do you really think that variations from 30% to 70% are conclusive or that a range from 2% to 17% is acceptable? All that those figures indicate are some very wide ranges and suggest significant variability in measurements. [ John McLean, Australia]	Rejected. We note that tropospheric ozone is a short live climate forcer, with a lifetime of about 1 month. For this reason a significant amount of variability can be expected. We assess and report the ranges. A large range results in a lower confidence in the findings.
30127	24	20			year and reference for TOAR are missing [ Gilles Delaygue, France]	Accepted. The reference is Tarasick et al (2019).
73375	24	31	24	31	Capital 'T' for 'troposphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. IPCC is harmonizing spelling across the report.
18259	24	33	24	34	This conclusion of an ozone decrease at high-altitude stations surprised me. It is not consistent with what had been reported in Cohen et al. (2018). In the latter, the authors notably discussed a lot of free-tropospheric trends found in the bibliography, observing that significant positive trends in ozone characterized all the ground stations higher than 2 km above sea level. May this disagreement be made clearer? e.g. Did Gaudel et al. (2019) have a word on it in their paper? (Since this paper is still under review in Elementa, I don't have access to the full text.) [ Yann Cohen, France]	Rejected. We do not longer include a discussion on high-latitude changes.
105491	24	36	24	36	Dates are confusing [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The early period was incorrectly listed as 1934-1944, however it should be 1934-1955. To clarify the date, this sentence was revised to: "The earliest observations of free tropospheric ozone (1934-1955) are from northern mid-latitudes where limited data indicate a tropospheric column ozone increase of $48 \pm 30\%$ , when compared to the modern period (1990-2012) (Tarasick et al., 2019). "
72179	24	43	24	43	it is "and tropics (2-14% 1-5 ppb per decade)" shouldn't it be ppbv? [ Joanna Wibig, Poland]	Accepted. Changed from ppb to ppbv

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83425	24	43	25	49	The list of references is very selective here and I am missing references like (to choose from - especially related to European Loess evidence): 1) Rousseau, D.-D., Chauvel, C., Sima, A., Hatté, C., Lagroix, F., Antoine, P., Balkanski, Y., Fuchs, M., Mellett, C., Kageyama, M., Ramstein, G., Lang, A., 2014. European glacial dust deposits: Geochemical constraints on atmospheric dust cycle modeling. <i>Geophysical Research Letters</i> 41, 2014GL061382, doi: 10.1002/2014GL061382. 2) Rousseau, D.-D., Boers, N., Sima, A., Svensson, A., Bigler, M., Lagroix, F., Taylor, S., Antoine, P., 2017. (MIS3 – 2) millennial oscillations in Greenland dust and Eurasian aeolian records – A paleosol perspective. <i>Quaternary Science Reviews</i> 169, 99-113, doi: <a href="https://doi.org/10.1016/j.quascirev.2017.05.020">https://doi.org/10.1016/j.quascirev.2017.05.020</a> . 3) Rousseau, D.-D., Antoine, P., Boers, N., Lagroix, F., Ghil, M., Lomax, J., Fuchs, M., Debret, M., Hatté, C., Moine, O., Gauthier, C., Jordanova, D., Jordanova, N., 2020. Dansgaard-Oeschger-like events of the penultimate climate cycle: the loess point of view. <i>Clim. Past</i> 16, 713-727, doi: 10.5194/cp-16-713-2020. 4) Stuut, J.-B.W., Temmesfeld, F., De Deckker, P., 2014. A 550 ka record of aeolian activity near North West Cape, Australia: inferences from grain-size distributions and bulk chemistry of SE Indian Ocean deep-sea sediments. <i>Quaternary Science Reviews</i> 83, 83-94, doi: <a href="http://dx.doi.org/10.1016/j.quascirev.2013.11.003">http://dx.doi.org/10.1016/j.quascirev.2013.11.003</a> . 5) Kjær, H.A., Dallmayr, R., Gabrieli, J., Goto-Azuma, K., Hirabayashi, M., Svensson, A., Vallelonga, P., 2015. Greenland ice cores constrain glacial atmospheric fluxes of phosphorus. <i>Journal of Geophysical Research: Atmospheres</i> 120, 810-810,822, doi: 10.1002/2015JD023559. 6) Simonsen, M.F., Baccolo, G., Blunier, T., Borunda, A., Delmonte, B., Frei, R., Goldstein, S., Grinsted, A., Kjær, H.A., Sowers, T., Svensson, A., Vinther, B., Vladimirova, D., Winckler, G., Winstrup, M., Vallelonga, P., 2019. East Greenland ice core dust record reveals timing of Greenland ice sheet advance and retreat. <i>Nature Communications</i> 10, 4494, doi: 10.1038/s41467-019-12546-2. 7) Lindhorst, S., Betzler, C., Kroon, D., 2019. Wind variability over the northern Indian Ocean during the past 4 million years – Insights from coarse aeolian dust (IODP exp. 359, site U1467, Maldives). <i>Palaeogeography, Palaeoclimatology, Palaeoecology</i> 536, 109371, doi: <a href="https://doi.org/10.1016/j.palaeo.2019.109371">https://doi.org/10.1016/j.palaeo.2019.109371</a> . 8) Kunkelova, T., Jung, S.J.A., de Leau, E.S., Odling, N., Thomas, A.L., Betzler, C., Eberli, G.P., Alvarez-Zarikian, C.A., Alonso-García, M., Bialik, O.M., Blättler, C.L., Guo, J.A., Haffen, S., Horozal, S., Mee, A.L.H., Inoue, M., Jovane, L., Lanci, L., Laya, J.C., Lüdmann, T., Bejugam, N.N., Nakakuni, M., Niino, K., Petruny,	Rejected. Unfortunately we do not have space for an exhaustive literature review about this topic (or any other) in our Chapter. Nor is it the remit of IPCC to undertake such a review.
23857	24	44	24	44	The statement: '...in the northern tropics downwind of the Asian continent.' - Unclear! What does it meant downwind of the Asian continent? This is totally out of dynamical, meteorological concept. If related to a certain monsoon season, or so, that should be stated clearly. [ Branko Grisogono, Croatia]	Accepted. To avoid any confusion associated with transport direction this sentence was changed to, "...with the largest increases (8-14% 3-6 ppbv per decade) in the vicinity of southern Asia and Indonesia."
126957	24	44	24	44	Units ppb and ppbv are mixed. It should be ppb (or nmol/mol) to be consistent with units of the NIST Standard Reference Photometer. [ Trigg Talley, United States of America]	Accepted. Ozone instruments based on UV-absorption measure ozone in units of parts per billion by volume. We follow IPCC TSU guidance on whether these values should be reported in units of ppbv, ppb or nmol mol <sup>-1</sup> .
115969	24		24		missing link to ch 6 on implications of tropo O3 and SLCF on air quality [ Valerie Masson-Delmotte, France]	Rejected. The link is given in the second paragraph of this section.
479	25	3	25	4	In Figure 2.8, the color coding gives two colors for each of the first three rows of p values. Either the caption should explain the reason for the two colors or, if there is no necessary reason, the figure should be changed to have only one color for each of the p value divisions. [ Claire Parkinson, United States of America]	Accepted. The use of two colours came from the original plots, but here it makes less sense.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24361	25	6	25	6	The caption to Figure 2.8 is missing a reference to Cohen et al. (2018), and some description of panel b. The full description of panel b should be as follows: b) Trends of ozone since 1994 as measured by IAGOS aircraft in 11 regions of the mid-troposphere (700-300 hPa; about 3-9 km) (Gaudel et al., 2020) and 7 regions of the upper troposphere (about 10-12 km) (Cohen et al., 2018), and as measured by ozonesondes above Hilo, Hawaii, which are representative of the central North Pacific region (Chang et al., 2020). The following reference needs to be added: Cohen, Y., et al. (2018), Climatology and long-term evolution of ozone and carbon monoxide in the upper troposphere–lower stratosphere (UTLS) at northern midlatitudes, as seen by IAGOS from 1995 to 2013, Atmos. Chem. Phys., 18, 5415–5453, <a href="https://doi.org/10.5194/acp-18-5415-2018">https://doi.org/10.5194/acp-18-5415-2018</a> , 2018. [Owen Cooper, United States of America]	Accepted. Text is changed into: "Trends of ozone since 1994 as measured by IAGOS aircraft in 11 regions of the mid-troposphere (700-300 hPa; about 3-9 km) (Gaudel et al., 2020) and 7 regions of the upper troposphere (about 10-12 km) (Cohen et al., 2018), and as measured by ozonesondes above Hilo, Hawaii, which are representative of the central North Pacific region (Chang et al., 2020)."
126959	25	12	25	12	Replace ppbv with ppb in caption and in Figure 2.8. [Trigg Talley, United States of America]	Rejected. Ozone observations are in ppbv.
36961	25	19	25	26	Do try to be honest. Say that the coverage of the data is too low to draw any meaningful conclusions. [John McLean, Australia]	Rejected. This assessment is based on a careful review of many peer-reviewed papers reporting current ozone trends at multiple surface and free tropospheric locations around the world, with additional evidence from satellite global surveys and a global composite product based on all available ozonesondes. If the data do not permit a conclusive statement on trends then such results have been clearly stated; for example, surface trends are variable at northern mid-latitudes and therefore we do not provide an overall trend value for this latitude band. All supporting studies have been clearly cited and trends are reported with ranges that reflect the variability across each region. We further note that in the summary statement the limited surface coverage in the southern hemisphere that precludes statements on zonal surface trends (e.g. tropics, mid-latitudes or high latitudes).
73377	25	23	25	23	Capital 'T' for 'tropics' (as a proper noun and for consistency elsewhere). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Spelling is harmonized across the report.
73379	25	25	25	26	References should be in alphabetical order. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There are no rules in IPCC to the alphabetic order of references.
67667	25	29	26	56	missing in the aerosol discussion is any sense of trends in stratospheric aerosol. Although volcanic aerosol is a significant part of the background, there is evidence of anthropogenic perturbations, in particular the existence of the ATAL (work by Vernier and Thomason, and modeling work on radiative forcing due to increases in organic aerosol (see <a href="https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL070153">https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL070153</a> ) [Karen Rosenlof, United States of America]	Taken into account. It is clarified that this addresses tropospheric aerosols.
36971	25	33	25	33	The ERF of 1750 was never measured and there is no way of knowing what it was. Attempting to use models is pointless unless the models can be shown to be accurate. [John McLean, Australia]	Taken into account. This is in the realm of Ch. 7 and now better referenced at this point.
89663	25	33	25	36	Section 2.2.6 on historical changes to AOD is nice, but what is the purpose of these couple of sentence in the first paragraph? It cites the assessed ERF <sub>air</sub> and ERF <sub>aci</sub> from AR5, but without any reference to the assessment in THIS report, which I find very strange. [Trude Storelvmo, Norway]	Rejected. Our Chapter starts each assessment by reporting what was said in AR5. The conclusions from this report come at the end of the section.
126961	25	34	25	34	Insert "(direct effect)" after the first "W/m <sup>2</sup> " and insert "(indirect effect)" after the second "W/m <sup>2</sup> ". This would provide consistency with terminology in previous assessments. [Trigg Talley, United States of America]	Rejected. This refers to AR5 which established the new nomenclature; there is no room to report the longer history of terminology.
19707	25	38	35	38	By "large-scale temporal evolution of aerosols" do you mean AOD? [Philippe Waldteufel, France]	Taken into account. A key focus is on AOD, but other properties are discussed as well.
35513	25	43	24	44	Bibliographic citations in chronological order [Carlos Antonio Poot Delgado, Mexico]	Editorial; copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58189	25	43	25	43	The citation 'Dornelas et al., 2018 (BioTIME: A database of biodiversity time series for the Anthropocene)' is a biodiversity paper. I can't find any reference or information within relating to aerosol proxy records. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
83423	25	43	25	43	Verify the use of Dornelas et al. (2018) as reference here. The reference provided in the list deals with a biodiversity data base. [ Antje H. L. Voelker, Portugal]	Accepted. Reference corrected.
58261	25	43	25	44	the references need to be put in order e.g the years is written as 2018, 2017, 2016 and then 2018 and 2016 while in the rest of the document its followed in acceding order. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
35879	25	43	25	54	I think this paragraph does not account for the finding from Markle et al. (Nature Geoscience, 2018) that a lot of the glacial-interglacial changes in dust deposition fluxes are due to changes in the hydrological cycle. This study implied that the glacial-interglacial changes in dust are thus smaller than previously reported (and repeated here). [ Jasper Kok, United States of America]	Rejected. This short section does not address attribution nor process analysis which is the realm of Chapter 6.
30129	25	43			'Aerosol proxy records' [ Gilles Delaygue, France]	Accepted.
30131	25	45			'aid' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
58113	25	46	25	49	combine these two sentences and address the use of "new constructions" at the first beginning [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. It seems easier to grasp if we have two sentences.
30133	25	48			'ratio of loadings over mid and high latitude oceans' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
83427	25	49	25	52	If you want to broaden the geographic range for this comment and include evidence from the eastern side of the Sahara (Red Sea, Arabian Sea) look at Palchan, D., Torfstein, A., 2019. A drop in Sahara dust fluxes records the northern limits of the African Humid Period. Nature Communications 10, 3803, doi: 10.1038/s41467-019-11701-z. [ Antje H. L. Voelker, Portugal]	Accepted. The reference is added.
58263	25	49	25	54	here the authors have concerned about sulfate concentration only but volcanic eruption can increase carbon hydrogen and other pollutants also. Why not considering them? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We cannot go into more detail on this and prefer to stick to sulphate only in this sentence.
58115	25	52	25	54	move or combine this sentence into line 49 to address "concerning" what kind of "dust variability" [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
30135	25	53			drop 'variability in' [ Gilles Delaygue, France]	Accepted.
58117	25	56	25	56	does "ice-core" provide the glacial/interglacial ratio? If so, better to address "ice-core" early at line 46, and use "the new reconstruction" in line 56 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This is a different, more direct, method.
126963	25	56	25	56	Leave out the word "indicative". [ Trigg Talley, United States of America]	Editorial; copyedit to be completed prior to publication.
78679	26	2	26	2	This and the next comment go together: Icecore-work on ice nucleating particles (the first, to our knowledge) could be added here as well: "... and Arienzo et al. (2017), and for ice nucleating particles, an important sub-group, by Hartmann et al. (2019)." --- citation: Hartmann, M., Blunier, T., Brügger, S. O., Schmale, J., Schwikowski, M., Vogel, A., Wex, H., and F. Stratmann (2019). Variation of ice nucleating particles in the European Arctic over the last centuries, Geophys. Res. Lett., 46, doi:10.1029/2019GL082311. [ Heike Wex, Germany]	Rejected. This chapter does not have the room or the scope to dwell on aerosol-cloud interactions (which is worked on in Ch. 6 and 7).
58237	26	2	26	9	I believe that more works have actually detailed these regional changes in black carbon composition, e.g. Mouillot et al. 2005, Painter et al. 2013, Lehndorff et al. 2014, Neupane et al. 2019. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The four papers mentioned are not pertinent to our analysis. Mouillot and Field (2005) and Lehndorff et al. (2014) only specifically investigate fire. Painter et al. (2013) do not produce data themselves, but only use them in the context of a modelling study; and Neupane et al. (2019) do not produce a time series.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58265	26	6	26	7	Concentration of BC is discussed here but there is no such record for Himalayan region, especially the glaciers. How the BC concentration is varying around these mountains should be also included here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Records from the Himalayan region/Tibetan plateau were not included for the following reasons: In case that the same technique was used (SP2), the BC concentrations are likely to be systematically underestimated, since the samples were stored in the liquid phase after melting, and prior findings indicate that storage in the liquid phase can result in as great as 80% reduction in measured BC concentrations (Mt. Muztagh Ata, the Eastern Pamirs, Wang et al., 2015; Rongbuk glacier, Mt. Everest, Kaspari et al., 2011; Guoqu glacier on Mt. Geladaindong, Central Tibetan Plateau, Jenkins et al., 2016). For the other record from Mt. Muztagh Ata, the Eastern Pamirs (Liu et al., 2008) a thermal combustion method was used, resulting in EC concentrations. Overall all the records are not comparable in absolute values, only z-scores could be used to look at the trends. Therefore they were not included.
73399	26	6	26	7	No capitals required for 'Black Carbon' (as per elsewhere in the Chapter). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
69791	26	6	26	9	Confidence statement on global/hemispherical/continental scale based on these observations would be useful. [ Bhupesh Adhikary, Nepal]	Taken into account. A statement is added.
78681	26	9	26	9	This goes together with the previous comment. If the suggested text is added, then add at the end of the paragraph here: "Arctic ice nucleating particles active down to -25°C, however, seemed to not have been influenced by anthropogenic emissions in the previous centuries prior to the year 2000." [ Heike Wex, Germany]	Rejected. No room to discuss aerosol-cloud interactions in detail and also the scope of other chapters.
73381	26	9	26	9	Please quantify 'very low' (poor scientific expression). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised the statement.
126965	26	11	26	12	Not sure this statement is true. There is a major paper submitted in mid-December 2019 by much of the community monitoring aerosol properties globally (i.e., largely through the WMO/GAW network) that suggests quite a bit can be said about large-scale trends from the in situ observations, which, in aggregate, are largely consistent with the AOD observations. Citation: Coen, M.C. et al, Atmos. Chem. Phys. Discuss., <a href="https://doi.org/10.5194/acp-2019-1174">https://doi.org/10.5194/acp-2019-1174</a> , 2020. That paper is supported by several others submitted before the end of 2019. [ Trigg Talley, United States of America]	Accepted. The statement is removed, and the reference is added to the assessment of in situ aerosol concentration trends. A new sentence is added on the absorption trends, referring to the publication the reviewer suggests..
58119	26	13	26	13	in text is "2001-2018", while in the Figure 2.9 is "2000-2018", which one is correct? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The period is corrected (start in 2000 is correct).
29815	26	13	26	13	Typo in "staellites". [ Hernan Edgardo Sala, Argentina]	Editorial; copyedit to be completed prior to publication.
481	26	13	26	13	MISR and MODIS are satellite instruments, not satellites. "staellites" (sic) should instead be "satellite instruments". [ Claire Parkinson, United States of America]	Accepted.
58267	26	13	26	14	Authors are representing the data from MODIS however the MODIS Terra data is available from 1999 and Aqua from 2002 it would benefit the readers if you can just add which MODIS data was used here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The clarification is added to the FAIR data table explanations.
58121	26	13	26	16	the "East Asia" discussions seem to be brought up spontaneously that has little linkage with the above and following context. Maybe address the linkage. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text is rearranged for a more logical flow.
58269	26	13	27	13	figure 2.9c and 9 c, here the authors caption the figure 2.9c which I think represent MODIS data and 2.9d represents the AERONET data kindly check the figure. Also in the line 13 the date is written as 2001 to 2018 while as in the figure its mentioned from 2000 to 2018. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Description is clarified – indeed the description that Aeronet data is added as the coloured circles was missing! - and start year (2000) is corrected.
30137	26	13			'satellites' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
30139	26	18			'in situ' in italics (Latin expression) [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24363	26	21	26	21	"report" should be "reports" [ Owen Cooper, United States of America]	Editorial; copyedit to be completed prior to publication.
30141	26	21			'reports' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
58191	26	24	26	25	Citations Sogacheva et al., 2018; Filonchyk et al., 2019; Ma et al., 2019 and Samset et al., 2019 lead to no references in reference list. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Corrected.
58123	26	24	26	28	the "comparison between MODIS and MISR" as well seems spontaneous for me, maybe slim the sentence down and move it to the line 13 to line 16? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The order of sentences is revised.
30143	26	26			'shows' [ Gilles Delaygue, France]	Rejected – both show this, i.e. plural.
24365	26	27	26	27	This is the first time in the chapter that OMI is mentioned and it is not clear that this is a polar orbiting satellite instrument. Please describe it as, "...from the polar orbiting Ozone Mapping Instrument (OMI)" or "...from the Ozone Mapping Instrument (OMI) on the Aura satellite" [ Owen Cooper, United States of America]	Accepted.
58193	26	27	26	27	OMI' is Ozone 'Monitoring' Instrument, instead of 'Mapping'. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
58195	26	27	26	27	Can't find any mention of OMI being used in citation 'Zhao et al., 2017', perhaps move this citation next to 'Li et al., 2014'? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
483	26	28	26	29	I recommend speaking with either J. G. Corbett or N. G. Loeb to appropriately reword this sentence referencing their 2015 paper, so that it no longer comes across as sounding like calibration drifts in general are unlikely to affect satellite derived trends. [ Claire Parkinson, United States of America]	Taken into account. I discussed with CA Norman Loeb and he confirmed the SOD statement is good.
58125	26	28	26	31	since the whole paragraph is about AODf but not sulphate aerosol, maybe it's better to introduce AODf first and then mention that it consists mainly sulphate aerosol to show the focus at the beginning [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We want to convey the message that looking at AODf is interesting since it is a marker for sulfate, and feel the best way to do so is to say this upfront. In light also of other reviewer remarks, we generalize the statement to "anthropogenic".
58239	26	30	26	30	There is a missing reference on CERES dataset here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We think the Paulot reference is sufficient for our context.
18291	26	33	26	33	diameters (not radii) [ Yugo Kanaya, Japan]	Rejected. Radii is correct.
45791	26	33	26	33	"radii" should be "diameters". [ Twan van Noije, Netherlands]	Rejected. Radii is correct.
35939	26	33	26	38	Satellite retrievals have complex sampling, so it would be good to balance that analysis with evidence from "globally complete" reanalyses, like MERRA-2 and CAMS. Although they indicate large regional trends, globally-averaged trends seem smaller than indicated here (BAMS State of the Climate 2019; Bellouin et al. 2020 ESSD). [ Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. A statement on this is added.
30613	26	33	26	38	Why talking about sulfate only in this paragraph? There should be lots of references showing the contribution of OC and nitrate to AOD in Asia. [ Hong Liao, China]	Accepted. The statement is generalized to write "anthropogenic".
30145	26	33			'found in the fraction of AOD': AOD is a radiative metric, so it cannot contain a 'fraction' of aerosols. Suggestion: 'found in the fraction of particles with radii <1 µm, responsible for the fine-mode AOD (AODf)' [ Gilles Delaygue, France]	Accepted/editorial.
58197	26	45	26	45	Oliver et al., 2006 citation leads to no reference in reference list. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted/editorial.
43061	26	46			Read "17.43°E, (Wendl et al., 2015; Osmont et al., 2018))," rather than "17.43°E, (Wendl et al., 2015; Osmont et al., 2018)," [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
37857	26	48	26	48	It is unclear what the 'mean state' refers. Is it intended to denote ENSO's influence on the mean (or climatological) state? Or, is it intended to denote that the ENSO fluctuation is dependent on the given mean state? Or, is it to point out some aspect of averaged (mean) ENSO events? [ Junhee Lee, Republic of Korea]	Not applicable. We do not speak about ENSO in this section.
43063	26	48			Read " 42.43°E, (Lim et al., 2017))," rather than " 42.43°E, (Lim et al., 2017)," [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43065	26	51			Read "Mernild et al., 2015))" rather than "Mernild et al., 2015)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
43067	26	54			Read "0.06°E (Arienzo et al., 2017)." rather than "0.06°E (Arienzo et al., 2017))." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
105493	26	55	26	56	use of the word presented is not appropriate in either context [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
58127	26				Figure 2.9: better to introduce the regions in the same order of the legends in the plot [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
58129	26				Figure 2.9: where is the stippled area? Are they the solid circles in the plots? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The figure is improved for clarity.
44209	27	0	27	0	NOTE: It should be Klein Goldewijk K. and NOT Goldewijk K.K. [ Marie-José Gaillard, Sweden]	Editorial; copyedit to be completed prior to publication.
105495	27	5	27	5	date format is inconsistent [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The date in the text is corrected.
485	27	5	27	6	MODIS should be spelled out as "Moderate-Resolution Imaging Spectroradiometer" (or as "MODerate-resolution Imaging Spectroradiometer"). [ Claire Parkinson, United States of America]	Accepted. Corrected.
30147	27	9			'Superimposed with circles' [ Gilles Delaygue, France]	Accepted. This is added.
58131	27	17	27	19	which part is addressed at the beginning? and which part is the "other parts of the world"? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The northern hemisphere mid-latitudes are clearly specified.
36963	27	17	27	23	The quantity of aerosols in the atmosphere will impact the formation clouds because aerosols are the microparticles on which water vapour condenses. A reduction in aerosols, which is to be expected in the last quarter of the twentieth century when air pollution was increasingly addressed, will therefore mean a reduction in cloud cover, which in turn means higher daytime maximum temperatures. I refer you to McLean (2014) "Late Twentieth-Century Warming and Variations in Cloud Cover" Atmospheric and Climate Sciences ( <a href="http://dx.doi.org/10.4236/acs.2014.44066">http://dx.doi.org/10.4236/acs.2014.44066</a> ), which incidentally refutes your unsubstantiated final sentence of this paragraph. [ John McLean, Australia]	Taken into account. A statement is added that aerosol-cloud interactions are addressed in Chapters 6 and 7.
73383	27	21	27	21	Capital 'H' for 'hemisphere' (x2). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
24367	27	22	27	22	"Deceasing" should be "Decreasing" [ Owen Cooper, United States of America]	Editorial; copyedit to be completed prior to publication.
18257	27	22	27	22	"deceasing" --> decreasing [ Yann Cohen, France]	Editorial; copyedit to be completed prior to publication.
35935	27	22	27	22	Suggest repeating here the statement on page 26 line 33 that fine-mode AOD is predominantly related to anthropogenic aerosols. That could be the opportunity to note that we do not really know what total AOD and fAOD are, and still do not have a confident separation of natural and anthropogenic trends in AOD. [ Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised as suggested by the reviewer, although no extra statement on the identification of anthropogenic aerosols is added.
81327	27	22	27	22	Hopefully this should be "Decreasing". [ Johannes Laube, Germany]	Editorial; copyedit to be completed prior to publication.
58241	27	22	27	23	Either put a reference to this sentence, or detail. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
30149	27	22			'DecReasing' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
30151	27	22			1. The sentence is not correct (this is not true for BC); 2. 'abundance' has a loose meaning. Suggestion: 'Decreasing AOD implies...', or at least 'Decreasing total aerosol abundance implies...'. [ Gilles Delaygue, France]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23307	27	26	28	7	SRCL (Special Report on Climate Change and Land) has reported that the natural vegetation has responded to rising CO <sub>2</sub> and climate change, showing increased vegetation activity (Earth Greening (Zhu et al., 2017 Nature Climate Change). The greening has significantly impacted the climate system by intensifying water cycle (Zeng et al. 2018 Journal of Climate) and mitigating global warming (Zeng et al. 2018 Nature Climate Change). These info should be updated in this new assessment. References: 1. Zhu, Z., et al. (2016). "Greening of the Earth and its drivers." Nature Clim. Change; 2. Zeng, Z., et al. (2018). "Impact of Earth greening on the terrestrial water cycle." Journal of Climate; 3. Zeng, Z., et al. (2017). "Climate mitigation from vegetation biophysical feedbacks during the past three decades." Nature Climate Change 7: 432–436 [ Zhenzhong Zeng, China]	Rejected - attribution and mitigation are outside the scope of the chapter.
5649	27	26	28	7	Please include an explicit statement on the role of carbon emitted due to land-use and land cover change. Here, you refer mostly to albedo as main effect of LUC / LCC, but it does not become clear whether e.g. carbon emissions from deforestation and revegetation have been considered or not. [ Joachim Rock, Germany]	Taken into account - combined with comment 70813.
108077	27	26	28	7	Section 2.2.7. Land use and land cover: I recommend to improve this section. It is necessary to provide more detailed researches on global land use and land cover changes. It is limited with explanations. [ Asylbek Aidaraliev, Kyrgyzstan]	Taken into account - combined with comment 70813.
42109	27	26	28	7	Please clarify this section regarding the different effects of land-use change on the climate, and clearly separate biogeochemical and (different) biogeophysical changes and their effects and estimated magnitudes. Reading the section it was not clear to me in which parts only biogeophysical impacts (particularly the albedo effect) were addressed and when also effects of CO <sub>2</sub> release. Furthermore, it seems that mainly the global effects (useful for mitigation) but not the local effects (required for adaptation) are considered. Potentially interesting additional literature: Pongratz et al. (2010), doi:10.1029/2010GL043010; Winckler, J. et al. (2019), https://doi.org/10.1029/2018GL080211; Bright et al. (2017), https://doi.org/10.1038/nclimate3250; Winckler et al. (2019b), https://doi.org/10.1029/2018JD030127; Perugini et al 2017 Environ. Res. Lett. 12 053002; Devaraju et al.(2015), doi:10.1111/pce.12488. [ Julia Nabel, Germany]	Taken into account - combined with comment 70813.
70813	27	26	28	7	the subchapter is labelled land use and land cover, but it deals mostly with albedo only - would one not expect in such a passage also the description of the biogeochemical land-use (including management) effects - or adapt the title? [ Karlheinz Erb, Austria]	Taken into account - text revised (section now distinguishes between biophysical and biogeochemical effects).
8891	27	26	28	7	carbon emissions associated with land use change also matter! this needs to be discussed coherently with albedo changes to avoid risk of confusion [ Robert Kopp, United States of America]	Taken into account - combined with comment 70813.
11455	27	26	28	7	Maybe somewhere in this subsection it might be worth stating that while the global net impact of LUC is small, it can be important regionally, especially wrt extremes (and possibly refer to Ch11 or 12)? [ Gerhard Krinner, France]	Taken into account. Text revised (section now mentions regional importance of extremes).
83711	27	26	28	7	It would be helpful for policymakers if an explanation is given here for what albedo is in the context of land-use change. Is this also the appropriate section to include the role of forestry as a carbon sink, including outlining how deforestation and reforestation have impacted the size of the global forest sink? [ Dan Zwart, New Zealand]	Rejected. The reader is assumed to know the meaning of technical terms like albedo. Carbon sinks are outside the scope of the chapter.
4719	27	28	27	29	References required [ Ibikunle Olaleru, Nigeria]	Rejected. Comment is ambiguous and does not include actionable input.
90287	27	28	27	29	bad break at end of sentence - separated from its number 0.15 [ Jeannine-Marie St-Jacques, Canada]	Editorial; copyedit to be completed prior to publication.
36965	27	28	28	7	Unsupportable claptrap unless one can also determine natural land-surface changes and show that the models to derive ERF are accurate in every way. [ John McLean, Australia]	Rejected. Comment is ambiguous and does not include actionable input.
30153	27	28			use a non-breaking hyphen for -0.15 [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
5341	27	28			The minus sign needs to be connected to the value. [ Bryan Weare, United States of America]	Editorial; copyedit to be completed prior to publication.
81519	27	29	27	30	Recommend to revise "...AR5 also concluded that a net cooling at the surface was about as likely as 30 not after accounting for the effects of albedo and other processes related to land-use change..." as it is unclear. [ Ee Ling Lee, Malaysia]	Taken into account - text revised (reverted to exact phrasing from AR5).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126967	27	29	27	31	The phrase "related to land-use change" needs to be moved to after "cooling at the surface" to qualify the whole statement, since there wasn't net overall surface cooling. For the same reason, "caused surface cooling" should be replaced with "had a cooling effect". [ Trigg Talley, United States of America]	Taken into account - combined with comment 102727.
369	27	30	27	30	after „related to land-use change“ include „(e.g. changes in GHG emissions, evapotranspiration, and roughness length)“ [ Wolfgang Obermeier, Germany]	Taken into account - combined with comment 70813.
42111	27	30	27	30	other biogeophysical processes? [ Julia Nabel, Germany]	Taken into account - combined with comment 70813.
102727	27	30	27	32	The wording "caused surface cooling" may be understood that it has caused an absolute cooling of the surface. It would seem more appropriate to say that it "had a cooling effect". [ Philippe Tulkens, Belgium]	Accepted. Text revised.
102729	27	30	27	32	It is not clear whether the second part of the sentences refers only to the albedo effect of assessed land use change. [ Philippe Tulkens, Belgium]	Taken into account - combined with comment 70813.
42113	27	31	27	31	biogeophysical and biogeochemical? [ Julia Nabel, Germany]	Taken into account - combined with comment 70813.
30155	27	31			'increase in global albedo caused surface cooling': this is wrong since there is no net cooling. SRCL concluded on the 'effect' of albedo (as written above L.30). Suggestion: 'the increase in global albedo has the effect of cooling the surface'. [ Gilles Delaygue, France]	Taken into account - combined with comment 102727.
70811	27	34	27	44	I think it is important to also mention that not only land-cover changes have impacts on the globale climate system, but also changes in managment (that occur within land-use types), see eg. Erb et al., 2018 10.1038/nature25138 for biogeochemical impacts, Luyssaert et al., 2016 DOI: 10.1038/NCLIMATE2196, Luyssaert et al., 2018 as examples for biophysical impacts); other recent papers might add to this passage: Naudts et al., 2016 doi10.1126/science.aad7270, Luyssaert et al., 2018 10.1038/s41586-018-0577-1 [ Karlheinz Erb, Austria]	Taken into account - text revised (introductory sentence now explicitly mentions land management).
44199	27	34	27	44	General comment: very confusing text because it is a mix of references that present empirical evidence and references that present model scenarios of land-cover change. Land-cover scenarios as those by Klein Goldewijk and Kaplan do not provide EVIDENCES of land-cover change, they provide SCENARIOS or MODEL estimates of land-cover change. Only empirical data such as pollen or archaeological data provide EVIDENCES. It is wrong to refer to Klein Goldewijk or Lawrence 2016 for evidences . Klein Goldewijk and Kaplan estimates anthropogenic land-cover change in the past by modelling. Hurtt et al. and Lawrence et al. use data from Klein Goldewijk et al. to establish Land Use Harmonization Schemes for climate modellers, these are not empirical reconstructions! For instance at lines 37-38: " From a global perspective, changes in land use were gradual prior to the mid-19th century, and accelerated markedly thereafter (e.g., Lawrence et al., 2016)." This is wrong; the land-cover curves look like this in LUH2 used in Lawrence et al., 2016; but these curves are scenarios based on Klein Goldewijk et al. 2017 for the last 1k BP , then it is interpolated down to 2k BP. It is assumed that there is no land use before 2k BPonly . One cannot say, therefore, that changes in land use were gradual prior to the mid-19th century. One should say that changes are estimated by the HYDE scenarios and the LUH2 land-use scheme to have been small between 1k and 1850. I therefore suggest to rewrite the text from line 34 "Historical land cover.... To line 44 "...and forest management.", and to add a few references (emphasized in yellow) + delete a few: see next comment. [ Marie-José Gaillard, Sweden]	Taken into account - text revised (reviewer suggestions incorporated into text).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
44201	27	34	27	44	Historical scenarios of anthropogenic land-cover change (ALCC) (e.g., Klein Goldewijk et al., 2017) are used to establish Land Use Harmonization (LUH) schemes for model simulations of past climate (pre- and post-1850 CE) (Lawrence et al., 2016; Hurtt et al., 2017). However, uncertainties in ALCCs remain large and quantification of global deforestation prior to industrialisation is still very uncertain (e.g., Kaplan et al., 2017). From a global perspective, LUH2 estimates that changes in land use and related deforestation were small between 1 ka and the mid-19th century (Lawrence et al., 2016). Syntheses of land-use and anthropogenic land-cover reconstructions based on pollen, archaeological and historical data provide evidence of deforestation since at least 6 ka (Early Neolithic) and earlier in some regions of the world (e.g., Marquer et al., 2017; Li et al., 2020; Harrison et al., 2020). Pollen-based quantitative reconstructions of land cover indicate that natural vegetation probably covered most of the Earth's ice-free terrestrial surface over early and mid Holocene, but that deforestation over late Holocene was larger than was assumed earlier (e.g., Marquer et al., 2017; Gaillard et al. 2018; Li et al., 2020). As assessed in SRCCL, at present nearly three-quarters of the surface is under some form of land use, particularly in agriculture and forest management. [ Marie-José Gaillard, Sweden]	Taken into account - combined with comment 44199.
44207	27	34	27	44	NEW REFERENCES for comment 3 above: Hurtt, G., Chini, L., Sahajpal, R., Froliking, S., Bodirsky, B. L., Calvin, K., Doelman, J., Fisk, J., Fujimori, S., Klein Goldewijk, K., Hasegawa, T., Havlik, P., Heinimann, A., Humpenöder, F., Jungclaus, J., Kaplan, J., Krisztin, T., Lawrence, D., Lawrence, P., Mertz, O., Pongratz, J., Popp, A., Riahi, K., Shevliakova, E., Stehfest, E., Thornton, P., van Vuuren, D., and Zhang, X.: Harmonization of global land use scenarios (LUH2): SSP585 v2.1f 2015–2100, Earth System Grid Federation, <a href="https://doi.org/10.22033/ESGF/input4MIPs.1662">https://doi.org/10.22033/ESGF/input4MIPs.1662</a> , 2017 Furong Lia, Marie-José Gaillard, Xianrong Cao, Ulrike Herzsuh, Shinya Sugita, Pavel E. Tarasov, Mayke Wagner, Qinghai Xu, Jian Ni, Weiming Wang, Yan Zhao, Chengbang An, A.H.W. Beusen, Fahu Chen, Zhaodong Feng, C.G.M. Klein Goldewijk, Xiaozhong Huang, Yuecong Li, Yu Li, Hongyan Liu, Aizhi Sun, Yifeng Yao, Zhuo Zheng, Xin Jia. Towards quantification of Holocene anthropogenic land-cover change in temperate China: A review in the light of pollen-based REVEALS reconstructions of regional plant cover. Earth-Science Reviews 203 (2020); <a href="https://doi.org/10.1016/j.earscirev.2020.103119">https://doi.org/10.1016/j.earscirev.2020.103119</a> Sandy P. Harrison, Marie-José Gaillard, Benjamin D. Stocker, Marc Vander Linden, Kees Klein Goldewijk, Oliver Boles, Pascale Braconnot, Andria Dawson, Etienne Fluet-Chouinard, Jed O. Kaplan, Thomas Kastner, Francesco S. R. Pausata, Erick Robinson, Nicki J. Whitehouse, Marco Madella, and Kathleen D. Morrison. Development and testing scenarios for implementing land use and land cover changes during the Holocene in Earth system model experiments Geosci. Model Dev., 13, 805–824, 2020 ; <a href="https://doi.org/10.5194/gmd-13-805-2020">https://doi.org/10.5194/gmd-13-805-2020</a> [ Marie-José Gaillard, Sweden]	Taken into account - combined with comment 44199.
73385	27	35	27	35	Delete , before 'and'. It is not required in this context. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
58133	27	36	27	44	clearer distinguish discussions on global and regional change may be easier to follow. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - combined with comment 44199.
52113	27	37	27	37	See also, more recently, Archaeological assessment reveals Earth's early transformation through land use L Stephens, et al Science 365 (6456), 897-902 AND Roberts, N., Fyfe, R.M., Woodbridge, J., Gaillard, M.J., Davis, B.A., Kaplan, J.O., Marquer, L., Mazier, F., Nielsen, A.B., Sugita, S. and Trondman, A.K., 2018. Europe's lost forests: a pollen-based synthesis for the last 11,000 years. Scientific reports, 8(1), pp.1-8. [ Kathryn Fitzsimmons, Germany]	Taken into account - text revised (reference added).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96209	27	38	27	38	Lawrence et al 2016 is the reference to the LUMIP simulation setup, also mentioning the historical land use forcing -- but it is not a reference for land use data. The mentioned Goldewijk 2017 paper shows it (though the HYDE dataset keeps per-capita land use area more or less constant, which is deemed implausible), Hurtt et al, GMDD 2020 is the reference for the CMIP6 land-use forcing mentioned in Lawrence (and was submitted prior to the IPCC deadline), Pongratz et al 2008 doi:10.1029/2007GB003153 is a dedicated reference to the last millennium -- it is older than the other references, but unlike them accounts for region-specific and temporally dynamic per-capita land use. It shows the mentioned acceleration. Please amend. [ Nicole Wilke, Germany]	Taken into account - combined with comment 44199.
23305	27	38	27	41	Zeng et al. (2018 Nature Geoscience) reported that agriculture is expanding rapidly in the tropics in the 21st century, causing rapid deforestation in mountains. It will be good to update this info to readers. Reference: Zeng, Z., et al. (2018). "Highland cropland expansion and forest loss in Southeast Asia in the twenty-first century." Nature Geoscience. [ Zhenzhong Zeng, China]	Taken into account - text revised (reference added).
81521	27	38	27	41	There are an additional spacing and semicolon in the sentence. [ Ee Ling Lee, Malaysia]	Editorial; copyedit to be completed prior to publication.
44203	27	39	27	42	"Generally, agricultural land use (such as cropland and pasture) has expanded at the expense of natural forests and grasslands, with historical events (such as disease outbreaks and conflict) also causing land-cover change (Krausmann et al., 2013; Goldewijk et al., 2017; Dawson et al., 2018; Gaillard et al., 2018; Koch et al., 2019);." For what period of the Holocene is this statement? And what does this mean? What disease outbreaks? Human disease? If so, why would it lead to deforestation? And conflicts? Why would they lead to deforestation? Human disease and conflicts have rather led to reforestation, i.e. expansion of secondary woodland. Neither Dawson et al. or Gaillard et al state something like that. If this is kept it should be better explained and the references should be adjusted to what is stated. I would rather delete this unless it is something that is considered important in the context. [ Marie-José Gaillard, Sweden]	Taken into account - text revised (sentence deleted).
371	27	40	27	40	remove space before Krausmann et al. [ Wolfgang Obermeier, Germany]	Editorial; copyedit to be completed prior to publication.
42115	27	40	27	40	however, these might cause abandonment of crop and pasture areas [ Julia Nabel, Germany]	Noted.
96211	27	40	27	40	"also causing land-cover change": please specify that such events could temporarily and regionally lead to a *reversal* of land-cover change like reforestation. [ Nicole Wilke, Germany]	Taken into account - combined with comment 44203.
43069	27	40		41	Read "change (Krausmann et al., 2013; Goldewijk et al., 2017; Dawson et al., 2018; Gaillard et al., 2018; Koch et al., 2019)." rather than "change ( Krausmann et al., 2013; Goldewijk et al., 2017; Dawson et al., 2018; Gaillard et al., 2018; Koch et al., 2019);." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
105497	27	41	27	41	semi-colon to be deleted [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
81329	27	41	27	41	There is an unnecessary semicolon here. [ Johannes Laube, Germany]	Editorial; copyedit to be completed prior to publication.
90289	27	41			extra ; [ Jeannine-Marie St-Jacques, Canada]	Editorial; copyedit to be completed prior to publication.
42117	27	43	27	43	maybe cite primary literature for "nearly three-quarters of the surface", e.g. Luyssaert et al 2014, <a href="https://doi.org/10.1038/nclimate2196">https://doi.org/10.1038/nclimate2196</a> [ Julia Nabel, Germany]	Taken into account - text revised (primary literature now cited).
96213	27	43	27	43	We think the primary literature rather than SRCL should be cited. The "3/4 of the ice-free land surface is under management" was from a compilation of land use areas by Luyssaert et al 2014 DOI: 10.1038/NCLIMATE2196. [ Nicole Wilke, Germany]	Taken into account - combined with comment 42117.
58271	27	43	27	44	authors here quote a statement from Special Report on Climate Change and Land (SRCL). I think its not properly cited. I recommend to insert date, etc., when you quote the SRCL report. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - combined with comment 42117.
30157	27	43			'of this surface' (the one defined above) [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
16497	27	46	27	46	It would be more appropriate for this chapter, and more useful if the albedo change could be assessed, rather than the ERF. The ERF conflates lots of different factors, whereas it should be relatively straightforward to assess how the albedo changes. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - ERF is also the purview of chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42119	27	46	27	46	biogeophysical factor (the release of CO2 due to deforestation is also a dominant climate factor of land use, but with opposite sign) -- Devaraju et al.(2015) doi:10.1111/pce.12488. Pongratz et al. (2010), doi:10.1029/2010GL043010; Perugini et al 2017 Environ. Res. Lett.12 053002 [ Julia Nabel, Germany]	Taken into account - combined with comment 70813.
96215	27	46	27	46	The statement that the dominant relevant factor associated with land cover changes is albedo is not correct in this generality. First, it should please be specified right away (it is only made clear in the second half of the paragraph) that this statement refers to only biogeophysical effects, ignoring biogeochemical ones. Second, it needs to be specified, whether the authors mean historical land use change or any (including hypothetical and natural land cover changes) and whether they refer to the global mean or to regions. Third, some discussion is needed to explain that albedo's importance is large only for non-local effects of land cover changes. For local effects, recent observational evidence shows that roughness and other non-radiative mechanisms dominate over albedo effects in most regions of the world and for most land cover/use transitions (Fig. 3 in Bright et al 2017 DOI: 10.1038/NCLIMATE3250); albedo acts foremost non-locally, because at the location of land cover change, the reduction in absorbed net shortwave radiation is balanced by reduced turbulent heat transfer (Winckler et al 2019 10.1029/2018JD030127). There should be a reference and consistency check with Chapter 5, p. 94 where biophysical effects of land use change are discussed and where CO2 effects of land-use change are quantitatively assessed. [ Nicole Wilke, Germany]	Taken into account - text revised (introductory sentence now states that the focus is on global-scale assessments of historical changes in land use; however, local effects are not discussed because they are beyond the scope of the chapter).
30615	27	46	27	55	Many forcing values are presented here without details, Are these forcings caused by albedo effect or biogeochemical processes? Need to be clarified. [ Hong Liao, China]	Taken into account - combined with comment 70813.
96217	27	46	28	3	Beyond the impact via the albedo, the impact of the LULUCF-sector via emissions on the climate system could be elaborated in more detail please. The statement "The dominant climate relevant factor associated with land cover change is albedo, which increased gradually prior to the mid-19th century and then strongly through the mid-20th century, with a slightly slower rise thereafter (Ghimire et al., 2014)." is surprising given the significant sources of GHG from LULUCF. Please explain. [ Nicole Wilke, Germany]	Taken into account - combined with comment 70813.
19709	27	46	28	3	Please indicate whether the snow cover is considered as a part of land cover. [ philippe waldteufel, France]	Taken into account - text revised (discussion now indicates studies that considered snow cover).
93861	27	46	28	7	There is newer evidence available that supports the overall conclusions of this paragraph.  Based on the RFMIP simulations for 12 CMIP6 models, Smith et al. (2020) estimate the ERF of historical land use to equal -0.08 (+/-0.14) W/m2. This estimate however includes the effects of some land surface adjustments to the land use perturbation as it includes all surface albedo changes, i.e. also from changes in snow, ice, LAI, etc.  Lejeune et al. (2020) reconstructed the local albedo changes induced by historical conversions between trees and crops/grasses for 15 CMIP5 models. Then using a simple kernel parameterisation they derived estimates of the RF from albedo variations due to land cover changes since preindustrial times ranging between 0 and -0.22 W/m2, with a mean value of -0.07 W/m2. Constraining the albedo response to transitions between trees and crops/grasses from the models with satellite-derived data leads to an increase in this range, however after excluding two models with unrealistic conversion rates from trees to crops/grasses we obtain a revised model mean estimate of -0.11 W/m2 (with individual model results between -0.04 and -0.16 W/m2)  Smith et al. (2020). Effective radiative forcing and adjustments in CMIP6 models. in discussion in Atmospheric Chemistry and Physics Lejeune et al. (2020). Biases in the albedo sensitivity to deforestation in CMIP5 models and their impacts on the associated historical Radiative Forcing. in discussion in Earth System Dynamics [ Quentin Lejeune, Germany]	Taken into account - text revised (paragraph now assesses the evidence from these new papers).



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10453	27	46	28	7	Are the forcing values here "RF" or "ERF" or "adjusted ERF"? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised (discussion now states whether forcing values are RF or ERF).
10455	27	46	28	7	Up to now it seemed the description of factors influencing climate was more on the observational side of things, but this section is definitely model derived. More needs to be made of that. To be honest I don't know why this is in this chapter? Chapter 7 seems far more logical place for it. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - text revised (paragraph now explicitly states that land-use effects are generally assessed using model simulations).
96219	27	49	27	49	The reference to Unger needs specification and change please. The value of 0.11 W/m2 refers only to the *additional effect of BVOC*, not to albedo forcing as Ghimire (which is also quantified, however) and neither to the full RF effect. Further, Unger indeed speaks of cropland expansion only, seemingly ignoring pasture and rangelands expansion; the authors should please check if this is really what Unger means and if so discuss how cropland-only relates to total land-use change. [ Nicole Wilke, Germany]	Taken into account - text revised (reference deleted and replaced with discussion of more recent papers).
58135	27	49	27	52	give some examples on processes of land use change that result in the "forcings" [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - text revised (paragraph now provides brief examples of biophysical and biogeochemical forcings).
30159	27	51	27	52	"biophysical and biochemical processes": these processes need to be explained, or even better this sentence removed (since only the biophysical effect has been shown here). [ Gilles Delaygue, France]	Taken into account - combined with comment 70813.
126969	27	56	28	1	"led to" should be replaced by "contributed" and "even larger warming" by "even larger warming contribution", since what's being talked about here is a component of the warming. [ Trigg Talley, United States of America]	Accepted - text revised.
115971	27		27		missing link to chapter 1 box on pre industrial climate in relationship to aerosol records. [ Valerie Masson-Delmotte, France]	Accepted. The link is added.
44205	28	2	28	3	Biophysical forcings at the regional and seasonal scale are potentially much larger (Strandberg et al., 2014). [ Marie-José Gaillard, Sweden]	Editorial; copyedit to be completed prior to publication.
96221	28	5	28	6	Why is the ERF limited to albedo changes here, when Andrews et al. and 7.3.4.1 calculate ERF for all biogeophysical effects and showed effects other than albedo to be large as well? Please be consistent across the report. [ Nicole Wilke, Germany]	Taken into account - text revised (summary now refers to biophysical effects rather than just albedo).
58137	28	5	28	7	conclude how the "changes" in land use would be better, i.e. increase, decrease, regionally, globally, etc. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected -- comment is ambiguous and does not include actionable input.
96223	28	6	28	7	It needs to be specified what the "net effect" refers to: all biogeophysical effects or all biogeophysical + biogeochemical effects? [ Nicole Wilke, Germany]	Taken into account - text revised (sentence now specifies that 'all' refers to the combination of biophysical and biogeochemical effects).
4621	28	10	28	36	The possible effects or not of changes in tropospheric water vapour could be touched on in this section. [ Andries Kruger, South Africa]	Rejected. The changes of tropospheric water vapour are not considered a driver of climate change, but rather a feedback.
1217	28	10	28	36	Need to define effective radiative forcing (ERF). How it differs from radiative forcing (RF). Otherwise, the report is for experts who already know all this... [ Rasmus Benestad, Norway]	Accepted. ERF is now clearly explained upfront in the Chapter, before the ES statements on drivers.
5343	28	10	28	36	Fig. 2.10 needs to be explicitly referred to in this section. In particular the dominance of volcanoes in the record until nearly 1990. [ Bryan Weare, United States of America]	Accepted. The figure is now referenced at the end of the second paragraph.
36973	28	10	29	22	Make it clear that every ERF mentioned in this section is nothing more than an estimate derived from either proxy indicators or models (or both) and that some, such as land-surface changes could be completely natural. [ John McLean, Australia]	Accepted. A reference to Ch 7 is provided.
36967	28	12	28	13	AR5 states that in the executive summary on page 661 (in the Executive Summary to Chapter 8) and in the SPM (pg 11), but neither refers the reader to any text in the main body to support the claim. Where changes in ERF are discussed in chapter 8 and where they might be implied by comments in AR5 chapter 2, they refer to "1970's" or even "late 1970's". The difference between "1970" and 1970's and late 1970's" is important because the Great Pacific Climate Shift occurred around 1976 and it caused global changes to climate. [ John McLean, Australia]	Rejected. The reviewer refers to AR5, not to the current report.
16499	28	12	29	22	Section 2.2.8 essentially repeats a lot of section 7.3. There needs to be a discussion of what goes in which chapter. [ William Collins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Discussed in detail with Ch 2 CA/ Ch 7 LA.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36969	28	17	28	17	Please remove all the nonsense statements in this chapter that purport to know climatic conditions and other data in or prior to 1750. Instruments were rarely available to measure anything and if they were available, they were not in anything even remotely approaching global use (e.g. Australia's east coast hadn't yet been discovered, so Australia wasn't inhabited by any Europeans and indigenous people certainly weren't using standard thermometers with celsius or fahrenheit scales.) [ John McLean, Australia]	Taken into account. The reader is referred yet more clearly to Chapter 7.
90293	28	18			cite figure 2.10 here so that the poor reader knows to go looking for it.... [ Jeannine-Marie St-Jacques, Canada]	Accepted.
73387	28	21	28	22	Delete 'the' and 'period' (to remove the tautology). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted/editorial.
58139	28	23	28	23	which period does this finding belong to? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Clarified in the revision.
58199	28	23	28	27	Consider breaking up sentence into: Strong volcanic eruptions (Section 2.2.2) with strong negative ERF lasting 2-5 years in duration occurred in the late 19th and early 20th centuries. This was followed by a relatively quiescent period between ~1920 and 1960, and then by three strong eruptions in 1963, 1982 and 1991, and only small-to-moderate eruptions thereafter (Schmidt et al., 2018). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
7221	28	28			Consistency on citing references. A number of references are stated as "Submitted" "in rev", "in review", or "accepted". It is highly recommended to cite published literature that has undergone a rigorous peer-review process. (E.g. 1. Chapter 2. P.43, L. 28 ; P.44 L.9 ; P.51, L.27 ; P.62, L.26 ; P.69, L.10) [ Asaad Irawan, Indonesia]	Accepted.
67825	28	28			There is a need for consistency in citing references. A number of references are stated as "Submitted" "in rev", "in review", or "accepted". It is highly recommended to cite published literature that has undergone a thorough peer-review process. (E.g. 1. Chapter 2. P.43, L. 28 ; P.44 L.9 ; P.51, L.27 ; P.62, L.26 ; P.69, L.10) [ Ruandha Agung Sugardiman, Indonesia]	Accepted.
81331	28	30	28	31	The recent acceleration of CO2 mixing ratios (as the main driver of net ERF acceleration) is only mentioned here. Why is this not discussed in Section 2.2.3.3.1? [ Johannes Laube, Germany]	Taken into account. Better consistency in FGD.
90291	28	31			add comma after "the 20th century" [ Jeannine-Marie St-Jacques, Canada]	Editorial; copyedit to be completed prior to publication.
30617	28	34	28	34	What exactly are the "some other man-made components"? [ Hong Liao, China]	Accepted. A reference to the relevant section is provided.
7495	28	34			The sentence "... mixing ratios of HFCs and some other man-made components are ..." should be better as "... mixing ratios of HFCs and some other human-made components are ..." [ Alejandro Cearreta, Spain]	Editorial. The reviewer is right.
10457	28	39	28	54	Why is figure 2.10 in this chapter? It obviously needs to be in chapter 7! [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Retained as is following discussion.
10459	28	39	28	54	Why does the volcanic forcing in figure 2.10 appear to be different to that shown in figure 2.2? For instance The positive volcanic forcing between eruptions needs to be explained (I suspect this might be due to the recommendation to have a background stratospheric aerosol in CMIP6 piControl experiments, so for periods with no volcanic eruptions in the historical period, it will be a positive radiative forcing. But this plot should not be effected by that recommendation.) [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The offset is now explained in the FAIR data table notes.
10461	28	39	28	54	How much does the positive volcanic forcing in 1750, influence the assessment of volcanic forcing subsequently? If the figure is showing forcing changes since 1750, then it seems more logical to have everything be zero at 1750! [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The offset is now explained in the FAIR data table notes.
30161	28	42	28	43	'The global annual mean temporal evolution since 1750 is shown as the central assessment value.' i do not understand the meaning of this sentence. [ Gilles Delaygue, France]	Accepted. The sentence is revised.
58141	28				Figure 2.10: introduce categories with the same order as the legend in plot [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The suggestion is implemented to the extent meaningfully possible without messing up the figure.
18293	29	1	29	2	Chapter 6 defines methane as SLCFs. To be consistent, "among the gaseous short-lived climate forcers, except for methane, tropospheric ozone is ..." [ Yugo Kanaya, Japan]	Accepted/editorial.
30163	29	3			'have small contributions to ERF': either 'have small ERF values' or 'have small contributions to total ERF' [ Gilles Delaygue, France]	Accepted/editorial.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
126971	29	6	29	6	In Figure 2.10, should the line in the inset box at at year = 2000 extend back to 1950 (period 1950-2000)? [ Trigg Talley, United States of America]	Accepted. The figure is clarified.
91117	29	9	29	9	should be Section 7.2.2, not Section 7.2.1 (or specifically Section 7.2.2.3 Changes in Earth's surface energy budget) [ Martin Wild, Switzerland]	Accepted. Reference corrected.
96225	29	13	29	16	This paragraph is not consistent with 7.3.4.1, in particular the values differ (0.15 here, 0.12 in Ch. 7). Please be consistent across the report. [ Nicole Wilke, Germany]	Accepted. Made consistent (by taking the Ch 7 value).
8893	29	13	29	17	carbon emissions associated with land use change also matter! this needs to be discussed coherently with albedo changes to avoid risk of confusion [ Robert Kopp, United States of America]	Rejected. CO2 is considered separately, and attribution of its sources is considered elsewhere in the report.
126973	29	16	29	16	adjustments' to what? Hard to understand the evidence-base for the confidence statement in this sentence without a little more context. Particularly since the total ERF isn't mentioned in Section 2.2.7 (only the ERF due to albedo, which is also mentioned in this section). [ Trigg Talley, United States of America]	Taken into account. We write now "rapid adjustments" so that even more precisely the term of the effective forcing concept is referred to.
81333	29	18	29	22	This appears to contradict messages from Chapter 7 (e.g., Figure 7.10) where some forcings from NOx, SO2, and organic carbon look more negative than in AR5. [ Johannes Laube, Germany]	Rejected. This seems to be a misunderstanding by the reviewer. We do not mean, the values of the forcings published over time have changed, but the ERF itself has changed.
73389	29	20	29	20	Replace 'grew' with 'has grown'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
132177	29	25			Would revise the structure of this section as follows: A) Include a "global climate" section, focused only on global climate indicators (GMST is not an atmospheric-only indicator, cannot be reported under "atmosphere"); B) include a "land" section. [ Sonia Seneviratne, Switzerland]	Rejected. The scope of the chapter is global and large scale and this suggestion would also serve to significantly break the flow. The chapter team discussed this and unanimously agreed to retain present structure.
132173	29	27	32	5	All three global chapters (2, 3 and 4) should be using land as one of the realms for subset of analyses: Atmosphere, oceans, land, cryosphere, biosphere. Land and biosphere are not synonyms, many land indicators are not related to biosphere: e.g. soil moisture, runoff, lakes, land heat storage, land surface temperature. Ignoring land is not acceptable after having a full report on "Climate change and land". [ Sonia Seneviratne, Switzerland]	Rejected. Adding land as a domain would require splitting material in such a manner as to be less and not more accessible. In the FGD further efforts have been made to stress the land components.
132323	29	27	32	5	On the Earth System's realms considered in chapters 2-4 and in particular chapter 2, and the fact that they do not include Land: Note that the GCOS Essential climate variables (ECVs) clearly highlight a list of Land variables, along with ocean and atmosphere variables ( <a href="https://www.ncdc.noaa.gov/gosic/gcos-essential-climate-variable-ecv-data-access-matrix">https://www.ncdc.noaa.gov/gosic/gcos-essential-climate-variable-ecv-data-access-matrix</a> ). It would seem very strange for the IPCC WG1 report to be inconsistent with the structure followed by the GCOS ECVs. [ Sonia Seneviratne, Switzerland]	Rejected. Prior assessment reports have diverged from GCOS ECV structure through e.g. including a cryospheric changes chapter.
36975	29	35	29	50	Define them, don't have vague comments about what their definitions include. [ John McLean, Australia]	Noted. Unclear what reviewer means by 'them' and no specific actionable suggestions made.
73391	29	36	29	36	Delete , before 'and'. It is not required in this context (cf line 48). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Addressed in edits.
58143	29	36	29	42	I prefer to see the definition at the first beginning rather than at the end [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This suggestion makes it harder to follow this paragraph.
58243	29	39	29	39	ocean basin scale —> ocean basin and hemispheric scale [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The modes we assess are driven by Ocean effects and thus hemispheric here obfuscates rather than elaborates.
58245	29	46	29	46	These indicators are chosen not only because of their societal relevance, but also because they provide a synthesis of the climate system evolution, thus improve our understanding of the response of the system to an external forcing. This should be mentioned here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Edits made to this end.
3497	29	53	30	8	This tabel on placement of major phenomena within the AR6 is very useful and it is encouraging to see that the monsoons receive such comprehensive coverage. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. No specific action requested.
102731	30	1	30	1	P-E should be explicitly defined in the box (it is defined in the text) [ Philippe Tulkens, Belgium]	Accepted. Edits made to this end.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73393	30	1	30	1	Capital 'C' required for 'chapters' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Addressed in edits.
113103	30	1	30	1	Having soil moisture (is that what is meant by 'surface humidity'?) or runoff as 'atmospheric' makes no sense. Please add 'land' to the table. The land biosphere is not the only climate component over the continents. [ Diego Miralles, Belgium]	Noted, the structure of Table 1 in the X-chapter box 2.2 was extensively discussed with CHs 1, 3, 4 and it was finally decided not to pose 'Land' as a separate component as also discussed in several additional responses.
113105	30	1	30	1	Terrestrial evaporation (E) is a GCOS ECV. It should be considered as a diagnostic here. Now P is considered, and P-E, but not E. I am the GCOS steward of the Land Evaporation ECV, so I feel the need to highlight this issue. There are multiple articles comparing climate models and observation-based data of E. Some: <a href="https://www.nature.com/articles/srep19124">https://www.nature.com/articles/srep19124</a> , <a href="https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-18-0583.1">https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-18-0583.1</a> , <a href="https://www.nature.com/articles/s41558-018-0207-9">https://www.nature.com/articles/s41558-018-0207-9</a> , <a href="https://iopscience.iop.org/article/10.1088/1748-9326/11/10/104006">https://iopscience.iop.org/article/10.1088/1748-9326/11/10/104006</a> , <a href="https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2013GL058055">https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2013GL058055</a> , <a href="https://www.nature.com/articles/s41558-020-0717-0">https://www.nature.com/articles/s41558-020-0717-0</a> , [ Diego Miralles, Belgium]	Noted, of course land evaporation is an important variable. However, CH.2 (and Box 2.2) along with Chs 3 and 4 do not consider fluxes (over both land and ocean), leaving these issues for Chs 8 and 9. Specifically Ch. 8 includes consideration of evapotranspiration. The references (extremely useful) have been passed to Ch.8. Chapter 8 is the more appropriate place to assess this diagnostic than chapters 2 through 4.
36977	30	1	30	6	The list might look impressive but it duplicates multiple factors. For example, terrestrial growing season changes are a reflection of temperature, rainfall, and seasonal snow cover (and even these are a reflection of atmospheric circulation) [ John McLean, Australia]	Noted but no action requested and nor is any effort made in the substantive assessment text to claim otherwise so it is unclear what the reviewer is alluding to.
105085	30	1	30	7	Cross-chapter Box2.2 Table 1: the term "large scale" is not defined here, and could have a different meaning for different communities. Furthermore, most of these variables are also interesting at smaller scales. It would therefore be good to specify what "large scale" is, and over which type of regions the averages should be computed (maybe referring to other chapters/atlas) [ Masa KAGEYAMA, France]	Taken into account. Large scale is defined in the text that precedes citation of the box.
19713	30	1	30	8	CCB2.2 Table 1: one of the main interests of this table is that it gives hints about how the climate is defined from IPCC's end. To most people, major properties of what they understand the climate to be are the cloud cover, or the seasonality of the weather. Are and will these properties be affected by climate change? WG1 ought to be able to say something somewhere about such "climate for the layman" issues, why they are not considered. [ philippe waldteufel, France]	Rejected. While the reviewer raises interesting points these are not the intended scope of the box and the feeling is that their addition would not be helpful.
132179	30	1	30	8	Need to distinguish "Sea surface temperatures" from "surface air temperatures". There is not single entry for "SST" in the table, would lead to a further confusion about the distinction between SST and 2-m temperature. [ Sonia Seneviratne, Switzerland]	Rejected. Surface temperatures as defined by GCOS is used here.
132181	30	1	30	8	Include a category "global climate indicators" to cover "global warming" and some global changes in the water and energy cycles [ Sonia Seneviratne, Switzerland]	Rejected. We discussed this in depth and prefer to remain as was presented in the SOD where the assessment already focuses anyway on these scales.
132183	30	1	30	8	Include a category "land indicators", including "land surface temperature", "land heat storage", "soil moisture", "runoff", "lake temperatures", ... [ Sonia Seneviratne, Switzerland]	Rejected. Land aspects have been better drawn out but as noted in responses elsewhere after substantive discussion we prefer not to add a land category.
19711	30	1	30	8	CCB2.2 Table 1: while every item in the first column is relevant for climate change, it is not always clear what the indicators are. Please insert "indicator" in the glossary, so that the reader will understand what you mean by this word. Let us take the permafrost: are we talking about surface, depth, volume, some integrated quantity? What about storm tracks? [ philippe waldteufel, France]	Accepted. The glossary now includes the term climate indicator.
1219	30	1	30	24	The large-scale indicator of climate change needs to include the global area of precipitation which is important for both the hydrological cycle and rainfall patterns. The global area of daily precipitation is related to the statistics of extreme precipitation amounts. [ Rasmus Benestad, Norway]	Rejected. There are few if any studies of this and, anyway, there is a whole chapter dedicated to the hydrological cycle and another to extremes where this would be more apposite.
9927	30	1	30	24	box2.2. table 1. Please add ch.8 to "Additional regional or process-based assessments...." for Seasonal snow cover and Glacier mass and extent [ Olga Zolina, France]	Editorial. Addressed in edits.
90299	30	3			solid fill of what color? [ Jeannine-Marie St-Jacques, Canada]	Editorial. Addressed in edits.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68193	30	6	30	6	note that glacier mass and extent is included in CID snow and land ice in Chapter 12, so perhaps add 12 to glacier mass and extent in table? [ Guðfinna Aðalgeirsdóttir, Iceland]	Accepted, Ch 12 added.
83167	30	6	30	6	Under Line 6 and in the box for Cryosphere under "Selected large-scale indicator of climate change" - please change from "Sea-ice extent / area and thickness" to "Sea-ice extent / area, seasonality and thickness". Sea ice seasonality (i.e., the timings of annual advance and retreat and the resultant annual duration of coverage) is a crucially-important indicator and variable, in addition to sea-ice extent and area. [ Robert Massom, Australia]	Accepted. Text modified accordingly.
73395	30	6	30	7	Define P-E, either in the table or in the legend. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, spelt out as 'precipitation minus evaporation' in the table.
18619	30	6	30	7	Some additional large-scale indicators are discussed regionally by Chapter 12 (would be helpful to add CH12 to right column): mean winds, glacier mass and extent, ice sheet mass and extent, ocean salinity, [ Alexander Ruane, United States of America]	Accepted, Ch 12 added.
126975	30	6	30	7	Comment on Table 1 of Cross-Chapter Box 2.2: All indicators by definition should be variables or metrics. Some of the indicators listed here are not expressed that way, so they're actually not indicators. "Permafrost" should be "Permafrost temperature and extent". "Global greening" should be "Green leaf area and photosynthetic activity". "Marine and terrestrial ecosystems" should probably be expanded into several different categories such as "Latitude and altitude range limits". [ Trigg Talley, United States of America]	Accepted. All entries have been reviewed accordingly.
5345	30	6			Surely, ocean temperature and heat content estimates are available for Ch. 4 and should be discussed. [ Bryan Weare, United States of America]	Noted. Chapter 4 checked all entries.
73397	30	12	30	12	Define UNFCCC. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This context should be clear from chapter 1.
36979	30	12	30	13	Don't talk nonsense. The Paris Climate Agreement defines no global average pre-industrial temperature or how it could be derived from the (pitifully few) observations back then. Without a baseline it is impossible to know how much change has occurred. What's more data from the GISP-2 project indicates that the last 1000 years has been the longest cold period in 10,500 years. [ John McLean, Australia]	Taken into account. The Paris Agreement is articulated around an ambition to limit warming to certain levels with a target of keeping below 2C. But the phraseology was odd and has been changed.
73401	30	15	30	15	Please give the date of the Second AR for reference. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
58147	30	15	30	20	listing every subset of indicators again in the text seems repetitive, maybe delete the brackets in the table to slim down the page [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We believe that the table needs to be standalone and the small degree of repetition is justified here.
113107	30	16	30	17	From the atmospheric indicators (ocean and land P-E, global precipitation, total column water vapour, surface humidity, and global river runoff) the only one that is really atmospheric is the 'total column water vapour'. Not even precipitation is atmospheric if measured in the ground. To me this feels a rather awkward way to structure this content. [ Diego Miralles, Belgium]	Noted. We agree this is not ideal and discussed in some depth alternatives but feel this is the least worst option. We have clarified that this is not just atmospheric.
13237	30	17	30	17	P-E must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	See comment ID 73395
73403	30	17	30	17	Define P-E. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	See comment id 73395
113109	30	17	30	17	Please clarify what is meant by 'surface humidity'. [ Diego Miralles, Belgium]	Accepted.
58247	30	18	30	20	These features of the atmospheric large-scale circulation in particular are primarily linked to global and local energy/mass/momentum constraints. A mention of the conservation properties leading to all these indicators to be linked with each other shall be briefly mentioned. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; text modifications made.
15855	30	19	30	19	instead of "...Monsoon systems, or the position and trength..." use "...Monsoon systems, and/or the position and trength..." [ Fei Luo, Netherlands]	Accepted.
3499	30	19			No need for capitalization of "Monsoon" [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
90301	30	19			should Monsoon be capitalized? [ Jeannine-Marie St-Jacques, Canada]	See comment id 3499
100531	30	22	31	7	Ice and snow have strong impacts on the surface energy budget via thei high albedo. This should be mentioned here,too. [ Peter Lemke, Germany]	Accepted. Some clarifying text has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36981	30	23	30	23	The text says only part of the story. Ice sheets will also change due to (a) ocean currents and wind (as the loss of Arctic ice in or about 2002 showed), (b) subsurface sources of heat such as volcanoes, and (c) the flow of ice from glaciers (which in turn depends on precipitation and subsurface heat. [ John McLean, Australia]	Noted. Here the aim is to very briefly elaborate on the importance of such and such indicator, i.e. to justify its use as a global climate indicator. This comment suggests to describe the mechanisms of changes in this indicator which are covered in Chapter 9 and not chapters 2 through 4.
115973	30		30		why referring to global greening and not browning trends too as done in SRCCL? It gives a more nuanced perspective. [ Valerie Masson-Delmotte, France]	Accepted. Edits made to this end.
58145	30				Cross-Chapter Box2.2, Table 1: replace the solid fill with "X" or other forms of symbols may be better [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We feel that solid fill works better here.
126977	31	1	31	1	What are 'knock-on' effects? [ Trigg Talley, United States of America]	Noted. Text modified for clarity.
83169	31	1	31	2	Once again, it is crucial to highlight the importance of change in the annual duration of sea-ice coverage - Please change "Changes in sea-ice extent and thickness have potential impacts for hemispheric-scale circulation...." to "Changes in sea-ice extent, annual duration of coverage (seasonality) and thickness have potentially important impacts on hemispheric-scale atmospheric and oceanic circulation....." [ Robert Massom, Australia]	Accepted. Edits made accordingly.
83171	31	1	31	2	What is meant here by "local effects in the polar regions"? Please be more specific. [ Robert Massom, Australia]	Taken into account. Phrase has been deleted.
58149	31	1	31	4	swap these two sentences to follow the order in the table [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
105693	31	1	31	14	The role of sea ice formation in driving deep ocean convection is not mentioned in either of the indicators here (cryospheric and oceanic) [ Inga Jane Smith, New Zealand]	Noted, mechanistic understanding is the scope of latter chapters and we do not feel that mention of this is warranted here.
73965	31	5	31	7	See a comment to Chapter 1, page 67. [ Elena Kozlovskaya, Finland]	Comment lacks sufficient context to be actioned.
78835	31	5	31	7	The degassing processes related to permafrost thaw are not an issue for mountain permafrost. But mountain permafrost is a key regulator of mountain hydrological cycles and slope stability at global scale. I suggest to add some words in order to make clear that polar and mountain permafrost are different in their implications. The mention to the sole latitude is insufficient as it does not allow any clear discrimination. [ MONICA TOLOTTI, Italy]	Noted, again, this relates to the role this indicator plays and associated processes, the aim of this box is a bit different to justify the use of such and such indicator and describe it very generally.
71155	31	5			changes in permafrost and seasonally thawed active layer". The active layer thaw seasonally by definition and as such it must not be repeated. ON the other hand, this sentence could potentially imply that the permafrost thaws seasonally. [ Lukas Arenson, Canada]	Accepted, edits applied to clarify.
73405	31	7	31	7	Defibe WMGHGs [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Has been defined previously.
35035	31	10	31	11	Ocean Heat Content is an indicator of change not a process. It would be better to say ""...in the climate system is taken up by ocean temperature change resulting in changes in Ocean Heat Content" [ W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Edits made for clarity
73407	31	11	31	11	Capitals not required for 'Ocean Heat Content'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
8895	31	11	31	11	GLOBAL MEAN sea-level change [ Robert Kopp, United States of America]	Accepted.
58249	31	13	30	13	The energy budget is even more intimately related to the ocean heat uptake or the change than to changes in the ocean heat content. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Energy budget assessment is given principally in Chapters 7 and 9.
82283	31	13	31	13	Please change "Cross-Chapter Box 9.2" into "Cross-Chapter Box 7.2". [ Schröder Marc, Germany]	Rejected. The box reference is the box in chapter 9 (now cross-chapter box 9.1)
78837	31	16	31	22	The chapter sections on Biosphere (Summary and section 2.3.4.) totally ignore freshwater ecosystem, especially lakes. I consider this as a serious shortcoming, considering that there is evidence at global scale of climate-related changes at physical level (e.g. increase in surface and bottom, ice cover and phenology and a plethora of related biological phenomena). Though local differences are pronounced there is abundant literature on coherent behaviour of lake physics at large-regional and global level. The response pool lakes to climate changes is crucial for humans and organism, being freshwater ecosystems recognized biodiversity hotspots. [ MONICA TOLOTTI, Italy]	Rejected. There is a need to avoid overt clashes / overlap with WG2 chapters 2 and 3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102733	31	18	31	18	It reads: 'Changes in ocean pH and oxygenation may portend changes in marine ecosystems'. The verb usage of 'may' is unfortunate. The evidence is already there, and also present in parts of the IPCC AR6 WG1 reprints, that changes in the ocean pH negatively impact on marine ecosystems. Also the usage of the verb 'portend' might be changed to a verb that is more commonly used by non-native English speakers. [ Philippe Tulkens, Belgium]	Accepted. Edits made to clarify.
58201	31	18	31	21	Consider breaking up sentence into: 'Changes in marine and terrestrial ecosystems can also be directly observed at large scales. For small, free-floating organisms such as phytoplankton, the dynamics can be rapid in nature, whereas on land, changes in plant assemblages may occur, with commensurate changes in altitude and latitude of the tree-line.' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Edited for clarity and readability.
30165	31	18			'may portend changes': it seems that 'portend' means announce, but i guess here the meaning is 'may cause changes'. [ Gilles Delaygue, France]	Taken into account. Text modified.
73409	31	19	31	19	Move 'directly' to after 'observed' (better English). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
73411	31	20	31	20	Move , from after 'land' to after 'nature'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
29817	31	21	31	22	Please, consider modifying the sentence "Lengthening of the growing season would be expected in most of the extratropics." in the following way: "Lengthening of the growing season and the associated changes in phenology, distribution and abundance of species would be expected in most of the extratropics.". [ Hernan Edgardo Sala, Argentina]	Accepted, edits to this end applied.
5621	31	24	31	39	About the modes of variability, maybe use the publication of Rossi et al., 2011 Global Planetray Change: Rossi et al did a synthesis of the time scale variability of commonly used climate indices [ Benoit Laignel, France]	Noted, but the text here is simply defining the choice of modes and this reference is not apposite here. It has been considered in 2.4 and TA VI where it is more apposite.
30167	31	25			'Many modes of climate variability affect global, hemispheric and regional climate': apart from ENSO i cannot see any other mode affecting the global climate. Suggestion: 'or regional climate' [ Gilles Delaygue, France]	Accepted.
126979	31	27	31	28	What is the difference is between criteria i and ii. They seem to be synonymous. Clarify. [ Trigg Talley, United States of America]	Noted. The difference is one is spatial, the other is temporal.
67669	31	39	31	40	Shouldn't modes of variability include the QBO? [ Karen Rosenlof, United States of America]	Rejected. The list of modes to be considered was defined and agreed across chis 2-3-4. QBO is assessed elsewhere in the report where it is important.
100791	31	39	31	40	Cross Chapter BOX 2.2. table 2. – Changes in the Atlantic Meridional and Zonal Modes are assessed in CH4 in sections 4.4.3.6 (near-term) and 4.5.3.6 (medium-to-long term). [ Corti Susanna, Italy]	Accepted.
105087	31	39	31	42	Maybe these models should be clearly defined (references could be added to the table for this, so that the table remains compact) [ Masa KAGEYAMA, France]	Rejected. The modes are defined in the technical annex pointed to in the text that cites the table.
81469	31	39			need to use 'Oscillation' instead of ' Variability' [ Kyaw Moe Oo, Myanmar]	Rejected. Standard naming applied across the report is used.
58251	31	41	31	42	Why are the outputs from climate models not addressed here, unlike in AR5? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Comment is not clear as to context - the table is agnostic with regards to whether based upon observations or models.
58151	31				Cross-Chapter Box 2.2, Table 2: replace the solid fill with "X" or other forms of symbols may be better [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. We prefer to retain the solid fill.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79975	32	1	32	3	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? I raised this point for the FOD but it has not been addressed. At present, the SOD is missing key components of the climate system (evaporation, wind stress, heat flux noted in this and the preceding comments) that were considered in AR5. If the authors are not going to consider them in AR6, they should state why they feel they are no longer worth including. [ Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted, fluxes are covered in chapters 8 and 9. So they are assessed in AR6 in an appropriate context.
79977	32	1	32	3	Likewise, 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). This is a potentially important development that is omitted from the SOD. Can the panel please assess these papers and provide an informed assessment regarding their significance/accuracy? Or state why they consider this development in understanding of climate related variability in a key component of the system that mediates global warming to be unimportant? 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). Journal of Climate, 29(10), 3647–3660. <a href="https://doi.org/10.1175/JCLI-D-15-0626.1">https://doi.org/10.1175/JCLI-D-15-0626.1</a> 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, J. Geophys. Res. Atmos., 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. Geophysical Research Letters 45:7, 3221-3227. [ Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. These aspects are assessed in detail in chapters 7 and 9. We do not perform this assessment here to avoid a redundancy in assessment.
126981	32	1	36	1	[PRECISION] The section "Temperatures of the deep past (Cenozoic)" uses abbreviations for past climatic events without giving the name, defining the event, or providing the ages of these events. For example, the PETM is not referred to as the Paleocene-Eocene Thermal Maximum. Similarly, the LIG is not defined as the Last Inter-Glacial. [ Trigg Talley, United States of America]	Accepted; spelled out abbreviations where first presented; table listing ages in called-out is first paragraph of section.
24413	32	8			Inconsistency among the format for temperature. For example, 13.3° ± 1.0°C, 7 ± 2°C, -0.58°C ± 0.17°C, 4°C to 7°C, 9° to 14°C [ Zhou Botao, China]	Editorial. Copyedit to be completed prior to publication.
19715	32	10	32	33	This subsection might take advantage of the beautiful figure 1 CCB 2.1, which includes almost every relevant information except confidence and likelihood estimates. [ philippe waldeufel, France]	Rejected. This section is reporting the findings of AR5 and other previous reports so the new findings reported in CCB2.1 are not directly linked to it. The figure is referenced extensively in section 2.3.1.1.1.
15167	32	10	32	33	Something to consider: While you need to start with this AR5 point of departure material, the way this is currently laid out, after ending this small opening section with AR5's assessment of surface temperature trends since 1880, the updated assessment of those trends doesn't come until the bottom of page 39, six pages later. You may want to consider slightly tweaking this to tell the full story (from AR5 to today) for each time period (deep past, post-glacial, modern) on their own? This is critical material, and the narrative gets lost, especially with the GMST vs. GSAT question mixed in. [ Simon Donner, Canada]	Accepted. The paleo and instrumental sections are separated in FGD.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112897	32	10	34	40	Section 2.3.1 and Figure 2.11 at page 164 deals with temperature records. In figure 2.11a, it is shown a temperature reconstruction for the last 2000 year that is very unlikely. It shows nearly constant temperatures from year 0 to 1000, a very slight cooling of about 0.2 °C from 1000 to 1900, followed by a 1 °C warming since 1900. This hockey stick function contradicts a large number of alternative regional and global paleoclimatic reconstructions (for example, Figure 5.7 of the IPCC AR5, <a href="https://www.ipcc.ch/report/ar5/wg1/information-from-paleoclimate-archives/graphics-produced-by-idl/">https://www.ipcc.ch/report/ar5/wg1/information-from-paleoclimate-archives/graphics-produced-by-idl/</a> ; Christiansen & Ljungqvist, 2012; Ljungqvist et al., 2012) both regional and global. The pattern shown by the depicted proxy reconstruction can be easily explained when inhomogeneous records are merged which could in part be faulty or very noisy. Christiansen B. & Ljungqvist F.C. (2012) - The extra-tropical Northern Hemisphere temperature in the last two millennia: reconstructions of low-frequency variability. <i>Clim. Past</i> , 8: 765-786. Ljungqvist, F. C., Krusic, P. J., Brattström, G., and Sundqvist, H. S., 2012: Northern Hemisphere temperature patterns in the last 12 centuries, <i>Clim. Past</i> , 8, 227-249, <a href="https://doi.org/10.5194/cp-8-227-2012">https://doi.org/10.5194/cp-8-227-2012</a> [ Nicola Scafetta, Italy]	Taken into account; the 2000-year-long temperature reconstruction shown in Fig. 2.11 is very similar to the reconstructions in AR5 Fig. 5.7. CH2 text explicitly highlights the similarities in the major features of the more recent global reconstruction in Fig. 2.11 and previous versions.
15171	32	10			The surface temperature trend assessment (since 1880) does not common on variability or drivers of variability in GMST. It is worth pointing to literature on ENSO and GMST variability, for example, and data demonstrating that (global) warming is observed for El Niño, La Niña and neutral years, as well as for EP and CP El Niño years. Beyond rounding out the assessment of trends, the additional material is important for debunk persistent misconceptions that warming trends are driven by El Niño events. [ Simon Donner, Canada]	Rejected. Attribution of changes in GMST is outside the scope of this section. The influence of ENSO on GMST/GSAT is mentioned (briefly) in Cross-Chapter Box 3.1.
132175	32	10			It does not seem to make sense to include here GMST when most of the signal over the ocean is an ocean signal (SST) not an atmospheric signal. Maybe start with a section on "global climate" in which GMST, GSAT, and any other measures of global climate change can be addressed. [ Sonia Seneviratne, Switzerland]	Rejected. Paleoclimate reconstructions of global temperature are with respect to GMST, so GMST is the relevant indicator here.
100605	32	12	32	12	Note: The way this section is written restricts it to consideration of only AR5 periods. If the structure were changed a little, the MCO could be added here. [ Matthew Kohn, United States of America]	Rejected. This paragraph is a summary of AR5 findings so material not covered by AR5 is outside its scope.
100607	32	12	32	12	Add: "The MCO is estimated to have been 8.7±2.3 °C warmer than PI" [see Goldner et al., 2014, corrected for PI vs. 2014] [ Matthew Kohn, United States of America]	Rejected. This paragraph is a summary of AR5 findings so material not covered by AR5 is outside its scope.
36983	32	12	32	13	The IPCC author doesn't know the difference between Global Mean Surface Temperature and Global Mean Surface (temperature anomaly) - the brackets indicate the grouping of the words. [ John McLean, Australia]	Rejected. GMST is conventionally defined as an anomaly.
126983	32	12	32	14	Since you spelled out GMST again, why not do the same for PETM, EECO, and MPWP and the other paleo markers? This would be helpful to the reader, especially those that are not experts in the paleo-climate literature. [ Trigg Talley, United States of America]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
36985	32	12	32	14	There was no accurate GLOBAL mean temperature between 1850 and 1900. Even the HadCRUT4 data indicates that global coverage was less than 50% until 1904, what's more the global coverage from 1850 to 1900 was dominated by weather recording from Europe, where the Little Ice Age was still receding. (A simple check of the HadCRUT4 grid cells that contain data shows this.) [ John McLean, Australia]	Rejected. The larger uncertainty in global temperature estimates arising from limited sampling in earlier parts of the record is known, and incorporated in the uncertainties reported in this assessment.
2005	32	12	32	26	I am not sure that such a comprehensive (i.e. long) summary of AR5 is needed here. Maybe shorten? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There are a large number of AR5 findings that need to be summarised here, given the number of different periods under consideration.
81471	32	12			need to describe clearly the Mean Surface Temperatures. Please use "Global Mean Surface (Land and Ocean) Temperature (GMST)" instead of Global Mean Surface Temperature (GMST) . [ Kyaw Moe Oo, Myanmar]	Rejected. This is a standard definition of GMST.
35515	32	13	32	14	* C repeats [ Carlos Antonio Poot Delgado, Mexico]	Editorial. This highlights that the use of units is inconsistent where temperature ranges are quoted. This is made consistent in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36987	32	14	32	16	Complete nonsense. There was no "global mean surface temperature" in pre-industrial times, nor is 1850-1900 a valid substitute for it (as I said above when commenting on lines 12 to 14 of this page). [ John McLean, Australia]	Rejected. Uncertainty arising from limited sampling in earlier periods is already incorporated in the uncertainty assessment in this section.
36989	32	17	32	17	It make no sense to compare and estimated temperature in the distant past (the LGM) to an unknown and unknowable pre-industrial global mean temperature. [ John McLean, Australia]	Rejected. Uncertainties in the assessment of temperatures in the relevant periods are incorporated in the overall uncertainty assessment.
35517	32	17	32	18	° C repeats [ Carlos Antonio Poot Delgado, Mexico]	Editorial. This highlights that the use of units is inconsistent where temperature ranges are quoted. This is made consistent in FGD.
81473	32	21	32	22	change to average annual Northern Hemisphere mean surface temperatures [ Kyaw Moe Oo, Myanmar]	Rejected. It is clear from the context that this refers to surface temperatures.
31459	32	21	32	33	The AR5 used area-weighted averaging to compute both the global mean of the land surface air temperature and also its temporal trends. Such an averaging procedure has no physical justification, as is illustrated by the following simple example. Consider a calorimeter (Dewar's flask) whose volume is half occupied by dry air at 25°C and the other half by dry air at 5°C, each half at the same pressure. (These two halves are akin to two grid cells of equal area.) Whereas the area-weighted average would clearly be 15°C, this is biased in favour of the warmer half, because in reality the colder half contains about 7% more air ( $n = PV/RT$ ). The true average is only about 14.65°C, and should be calculated as the final temperature when the two halves are allowed to mix (i.e., exchange heat only with each other). In other words, the heat capacity should be used as the weighting factor (which allows for the variable composition of moist air). Kowalski, A. S., 2012, Exact averaging of atmospheric state and flow variables, Journal of the Atmospheric Sciences, 69, 1750-1757. [ Andrew Kowalski, Spain]	Rejected. This is of negligible relevance to averaging of anomalies from a baseline (as opposed to averaging of absolute values).
81475	32	22	32	23	need to describe the range of period of 800 years and 1400 years [ Kyaw Moe Oo, Myanmar]	Rejected. It is unclear what point this comment is making.
36991	32	29	32	30	Wrong again. The poor global coverage makes your comments nothing more than speculation. Why won't you show a graph of the data coverage both for each hemisphere and globally? You mention the "substantial gaps in global coverage" on line 22 of page 35 and mention poor coverage again on page 39, so in effect by ignoring the coverage issue on page 32 you are misleading the reader. [ John McLean, Australia]	Rejected. Uncertainty arising from limited sampling in earlier periods is already incorporated in the uncertainty assessment in this section.
487	32	31	32	31	"trend of 0.86 C" should be revised to change the unit to a trend unit or to change "trend of" to "increase of". [ Claire Parkinson, United States of America]	Taken into account. Some minor rewording has been done to make it clearer that this is over the full period. This is explained further in the AR5 material which is being summarised here.
126985	32	31	32	32	Are these trends or overall amounts of warming? If they're trends, shouldn't they be expressed as rates? [ Trigg Talley, United States of America]	Taken into account. The wording states clearly that they are trends; they are expressed here as a total change over the period of the trend (so can be converted to a per year/decade amount by dividing by the length of the time period). This is explained further in the AR5 material that is being summarised here. Some minor rewording makes this clearer.
19717	32	32	32	33	This sentence might take advantage of figure 2.11c, which illustrates precisely this diverging evolution. [ philippe waldteufel, France]	Rejected. This is reporting the findings of SRCCL, whereas the figure supports the AR6 assessment of this question (found at P39 L54-55)
36993	32	32	32	33	Yours is a specious claim given that the pre-industrial land surface air temperature and global average temperature average temperature are unknown and unknowable. [ John McLean, Australia]	Rejected. This is a direct reporting of the SRCCL findings.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58273	32	32	32	33	here authors conclude with the statement about land surface air temperature and average air temperature if I am correct here the average air temperature is the temperature measured by meteorologic station which measure the temperature 2m above the surface. And here there is no such differentiation between land use land cover as the LST varies with the LU/LC is this a general statement for land use land cover changes over entire globe or is as the place where changes have occurred as you have also mentioned the per-industrial period therefore it may have a link towards the buildup changes ? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This figure is a global average for 2m air temperature over land areas and does not attempt to break down by LU/LC categories. Rewording has been carried out to make this clearer.
81477	32	33			global average temperature > SPM approved draft (IPCC SRCCL) say global mean surface (land and ocean) temperature (GMST). [ Kyaw Moe Oo, Myanmar]	Accepted. Replaced by GMST in FGD.
126987	32	36	33	49	Figure 1 of Cross-Chapter Box 2.1 includes "best estimates for GMST for reference periods [that] are assessed in Section 2.3.1.1". But Section 2.3.1.1 does not include an assessed best estimate and range for the PETM, it does not discuss the EETM at all, it does not include an assessed best estimate and range for the LIG, it includes inconsistent values and no assessed range for the LGM, and it does not include an assessed best estimate and range for the MH. Apart from being required for consistency, such assessments are important and valuable and should be made. [ Trigg Talley, United States of America]	Taken into account; final assessments had been integrated with assessments of recent temperatures as part of section 2.3.1.1.4 "overall assessment" below. These summary statements have now been moved into their respective paleo reference period paragraphs.
99733	32	36	34	18	can you assess the literature here with statements of confidence or likelihood? [ Peter Clark, United States of America]	Taken into account; final assessments had been integrated with assessments of recent temperatures as part of section 2.3.1.1.4 "overall assessment" below. These summary statements have now been moved into their respective paleo reference period paragraphs.
8897	32	36	34	18	No assessments are provided here. [ Robert Kopp, United States of America]	Taken into account; final assessments had been integrated with assessments of recent temperatures as part of section 2.3.1.1.4 "overall assessment" below. These summary statements have now been moved into their respective paleo reference period paragraphs.
93055	32	36			A more careful assessment of the LIG GMST, including importantly the uncertainties, is needed. The LIG GMST is quoted throughout Chapter 2 and also Chapter 9. This is especially critical as it relates to the CMIP6 lig127k experiment, where the primary forcing is the seasonal and latitudinal insolation changes associated with orbital changes. An important question is how/if feedbacks in the Earth system translate this forcing to a annual and global temperature signal. [ Bette Otto-Bliesner, United States of America]	Accepted; re-evaluated land vs sea value used by Fischer et al., 2018. New values now based on assessment in section 3.3.1.1
93057	32	36			Turney et al., 2020 derive average SST temperatures for 129-125ka (over 4 millennia!). Considering age uncertainties and the time scale of the Southern Ocean response to the H11 event, doing so is averaging records that are not temporally synchronous, nor represent 1 millennium, and incorporate both the LIG orbital forcing and potentially effects of the H11 forcing. If used in this assessment, this needs to be clearly stated. The CMIP6 lig127k simulations assessed in Chapter 3 and other chapters do not include the H11 forcing. [ Bette Otto-Bliesner, United States of America]	Accepted; stated the age range used for reconstruction by Turney et al.
93059	32	36			The Hoffman et al. reconstruction includes only a few annual records in the Pacific, the largest ocean basin. Those available in the Pacific Ocean basin are predominantly located in the upwelling zones off California and South America and near New Zealand. Accordingly, an assessment of their uncertainty of +/-0.3C given in their reconstruction needs caveats. [ Bette Otto-Bliesner, United States of America]	Accepted; clarified uncertainty range.
93061	32	36			The relevance of the Snyder scaling factor to past time periods when the primary forcing is orbital has not been truly verified. The Snyder scaling factor used in Fischer et al. is based on model simulations for Pliocene (high CO2) and LGM (lowCO2). Snyder justifies its use for the LIG based on the Turney and Jones 2010 reconstruction, which has been criticized in that it considered LIG records representing warmest temperatures to be temporally synchronous across the globe. [ Bette Otto-Bliesner, United States of America]	Accepted; re-evaluated land vs sea value used by Fischer et al., 2018. New values now based on assessment in section 3.3.1.1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93063	32	36			The literature assessed in this section are estimates of the Global SST changes. Little data is available for annual temperature anomalies over land, and for those available there are still large uncertainties associated with chronology, elevation, seasonality. [ Bette Otto-Bliesner, United States of America]	Taken into account; uncertainties are accounted for in overall assessment of confidence, none of which are "high confidence".
93065	32	36			Capron et al., QSR, 2017 address the LIG seasonal temperature anomalies, very relevant to the CMIP6 lig127k simulations discussed in Chapter 3. Her quantitative estimates should be included here, even if not annual and not global. [ Bette Otto-Bliesner, United States of America]	Rejected; CH2 priorities are for mean annual at the large scale, not seasonal over some regions. CH3 is home to model evaluation.
58153	32	38	32	38	maybe give some examples further on "i.e., proxy records" [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; we now refer to CH1 list of proxy types.
105089	32	38	32	38	the term "proxy" should not be used without writing what it is a proxy for. For instance here, "climate proxy" or "climate indicator" could be used. [ Masa KAGEYAMA, France]	Noted; the word "proxy" is followed by "records".
30169	32	38	32	39	i do not understand at all the meaning of this sentence. i guess 'deep-ocean temperatures' are reconstructed from proxy records. [ Gilles Delaygue, France]	Accepted; clarified.
69163	32	40	32	43	Since this message is valid throughout the chap.2, it would be appropriate to move the sentence to the introduction. [ Kaoru Magosaki, Japan]	Noted; a similar message, but more general is provided in the chapter introductory statements.
126989	32	41	32	41	"represented directly" requires clarification. The role of climate models in formulating the assessments in this chapter is not intentionally hidden, but the statement could be read that way. [ Trigg Talley, United States of America]	Accepted; clarified.
58203	32	51	32	51	Caballero & Huber, 2013 citation leads to no reference in reference list. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, added reference.
29819	32	51	32	51	Consider adding "conditions" or "era" after "modern". [ Hernan Edgardo Sala, Argentina]	Accepted, added "conditions".
29821	32	51	32	51	"Caballero and Huber, 2013" has not been included in the References section. [ Hernan Edgardo Sala, Argentina]	Accepted, added reference.
30171	32	51			'(assumed = 14°C)' what does it mean? [ Gilles Delaygue, France]	Editorial, clarified.
90303	32	52			I think you mean that you are assuming that the preIndustrial had a GMST of 14oC but it could be worded more clearly. It's not totally clear what the (assumed = 14oC) actually is. [ Jeannine-Marie St-Jacques, Canada]	Accepted, clarified.
104697	32	53	32	53	Add: "For the MCO, temperatures are estimated to have been 8.7±2.3 °C warmer than pre-industrial, with only 2-3 °C ascribable to boundary conditions. Models with CO2 levels of c. 800 ppm are necessary to explain the ~6 °C temperature difference, much higher than indicated from pCO2 proxy studies (c. 500 ppm). These results point to either missing feedbacks that amplify that impact of increasing CO2 or missing forcings, with the role of aerosol forcings (both direct and indirect) during the Miocene being one of the least well constrained but potentially impactful." [ Matthew Kohn, United States of America]	Rejected; attribution, feedbacks and modelling are out of scope for CH2. Accepted estimate for GMST.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42881	32	53	33	8	This analysis of LIG temperatures is a little woolly as it doesn't really assess what was done. Turney et al, while "comprehensive" does not attempt to synchronise data but just averages everything it has across what seems to be a subjective and variable time period of the LIG plateau. It's a little hard to know what that means. Friedrich's paper doesn't even reach much of the LIG (stops at 125 ka) and gives little detail of how values were estimated - I find his high values unlikely when taken in the context of the other estimates that take in the whole of the LIG. Hoffmann did the most thorough job by explicitly making a synchronised time series and to my mind his lower limit should be within the uncertainty you cite, which would then put a lower limit (after multiplying by the highly uncertain SAT/SST value of 1.6) closer to 0.5 degrees not 1 degree. In addition by combining the estimates you are mixing 3 different estimates: Turney's claims to be an average value across the LIG plateau, Friedrich's is the value for a period after 125 ka, and Hoffmann's is the value for the peak of the LIG at 127 ka. And much of the uncertainty concerns the modern reference value which was done differently in each case despite your attempts to allow for this. In any case you need to spell out why you treated all the data as equal (when I would judge Friedrich inconsistent with the others), you need to explicitly state that 1.5 shown on page 5 line 16 is the result of 1.1 scaled by 1.6 (which of course it isn't!), and how you derived the very small uncertainty of 0.5 which appears in the summary at page 5, line 16. Given the range of values, and the uncertainty in the factor 1.6, a 2 sigma uncertainty of 0.5 degrees is too small. 1.5+/- 1 might be appropriate, though personally I would have assessed as 1+/-1 based on Hoffmann and Turney, and noting that Friedrich's estimate for 125 ka is inconsistent with the others but still within this error bar. Finally you should not ignore the estimate for the LIG of mean ocean temperature (Shackleton, S., et al. (2020), Global ocean heat content in the Last Interglacial, Nature Geoscience, 13(1), 77-81, doi:10.1038/s41561-019-0498-0) - at similar to modern for most of the LIG. I know MOT is assessed later, but this value is likely not consistent with a GMST increase of 2 degrees or more. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; the midpoint of the reviewer's estimate (+1C) is now equal to the midpoint of the AR6 assessment. The suggestion that the uncertainty should be 2X the AR5 uncertainty is inconsistent with the substantial progress since AR5. On the other hand, the AR6 uncertainty ( $\pm 0.5C$ ) is greater than Sock's $\pm 0.25$ uncertainty.
36995	32	55	32	55	A baseless comment given that no "global" average SST is available for 1850-1900. HadSST3 global coverage briefly reached about 43% in 1886 but that was its maximum over the 51 year period. It was as low as about 10% in 1861. Only since about 1961 has it been over 60% and over 70% since 2008. (refer <a href="https://crudata.uea.ac.uk/cru/data/temperature/HadSST3-gl.dat">https://crudata.uea.ac.uk/cru/data/temperature/HadSST3-gl.dat</a> ) [ John McLean, Australia]	Taken into account; uncertainties arising from the sparse coverage in the 1850-1900 period are incorporated into the uncertainties quoted in the assessment.
30173	32	55			'waRmer' [ Gilles Delaygue, France]	Accepted, fixed.
36997	33	2	33	3	There was insufficient data in 1870-1889 and 1850-1900 to make such claims. [ John McLean, Australia]	Taken into account; uncertainties arising from the sparse coverage in the 1850-1900 period are incorporated into the uncertainties quoted in the assessment.
58155	33	4	33	4	the "1.1 °C" here seems not so intuitive to me, maybe address more to the values that mentions above [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; 1.1°C is the reported value.
126991	33	4	33	6	The relevance of Fischer et al. (2018) to the discussion here is unknown, so it shouldn't even be mentioned. [ Trigg Talley, United States of America]	Rejected; Fischer et al. (2018) is the major source of paleotemperature values for warm intervals cited in SR1.5.
8899	33	10	33	26	GMST of -6°C, global-average land temperature of -6°C, and global average SST of -2°C seem inconsistent – some assessment needs to be made here [ Robert Kopp, United States of America]	Taken into account; assessment made.
99339	33	10	49		This section discusses attempts to reconstruct temperatures in the LGM and Postglacial. These are based on either the integration of a range of proxies and records. It would be useful to have a clearer indication of what the temperature proxies actually are, how these proxies, which are often driven by factors other than temperature have been converted to GMST, how the errors have been calculated and what the full ranges are (these are often much larger than the computed errors). This is especially the case when estimates are based on submitted and not published papers [ Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; all estimates are based on published accounts. CH2 remit does not include textbook accounts.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37001	33	11	33	11	Wrong. There is no accurate and reliable pre-industrial temperature. [ John McLean, Australia]	Taken into account; uncertainties arising from the sparse coverage in the 1850-1900 period are incorporated into the uncertainties quoted in the assessment.
58275	33	11	33	11	" Two use marine proxies to reconstruct global SST," seems to be wrong if you are saying two (2) proxies, later in the para you seems to be taking about more. See line 12 to 14 "one indicates" ..... "and the other" ..... " A third new"..... [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; clarified.
93503	33	11		12	Revise sentence "Two use marine proxies to reconstruct..." [ Rahab KINYANJUI, Kenya]	Accepted, fixed.
30175	33	12			'Of these' > 'Of these two' (or else this refers to 'assumptions') [ Gilles Delaygue, France]	Accepted, fixed.
37003	33	13	33	13	The Holocene Period started about 11,700 years ago and, according to some, ended in 1800 but others, like the IPCC say it is the current epoch. Define "late Holocene" so that the reader is not confused or left wondering. It's not even defined in the AR6 Glossary. [ John McLean, Australia]	Accepted. Added early, middle and late to glossary definition of Holocene.
30177	33	17			'from ocean heat content' > 'from mean ocean temperature' (Bereiter et al 2018) [ Gilles Delaygue, France]	Accepted, fixed.
37005	33	18	33	18	During the period 1850-1900 the global coverage of HadCRUT4 data did not exceed 50% therefore it is really stretching credulity to claim that a specific temperature was the *global* average during that time. The logical consequence of this is that no meaningful comparison relative to this temperature can be made. [ John McLean, Australia]	Taken into account; uncertainties arising from the sparse coverage in the 1850-1900 period are incorporated into the uncertainties quoted in the assessment.
4515	33	18	33	20	This does not seem to be right. Average Holocene temperature was significantly warmer than the average. See Lüning & Vahrenholt 2017 (doi: 10.3389/feart.2017.00104) for details. [ Sebastian Luening, Switzerland]	Noted; Fig. 1 in Lüning & Arnold (2017) show essentially the same value as stated: pre-industrial reference was about 0.4C colder than average Holocene.
98739	33	19	33	19	Kaufman is published now. [ Meredith Parish, United States of America]	Accepted, fixed.
73413	33	19	33	19	Give dates of 'early' and 'late'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, Added early, middle and late to glossary definition of Holocene.
23465	33	20	22	20	Since all of the other studies are based on data, it should be noted that the estimates referenced in Fig 1c of Harrison et al are modelled values. It would also be good to give their global LGM MAT (or GMST) value which is more comparable to the GMST in the observational studies quoted. [ Jean Lynch-Stieglitz, United States of America]	Rejected; Fig 1c of Harrison is not only modelled values. It includes reconstructions as well (also see Fig 7 in doi: 10.1002/jqs.2842). Harrison does not provide a GMST value.
30179	33	20	33	23	What is the point of comparing 4 different estimates of GMST to 2 much more specific studies? Please explain. I note that a land cooling by -6K and a SST cooling by -2.2K should average to a global cooling of -3.3K, a value very different from the -5.8K given at L.20. [ Gilles Delaygue, France]	Noted; more specific studies are included as part of comprehensive treatment, even if not global.
58157	33	24	33	25	couldn't directly locate LDT from the figure [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; clarified LDT in figure.
26627	33	26	33	27	For the LDT, the Shakun et al., 2012, Nature (Global warming preceded by increasing carbon dioxide concentrations during the last deglaciation) reference might be appropriate here. Importantly, it shows a GMST transitioning by about 3.5°C in about 10 ka, suggesting that "the rate of GMST change during this period" is only a fraction of a degree. [ Eric Brun, France]	Taken into account; Shaken et al. was source of AR5 estimates and is also shown in Cross-Chapter Box 2.1 Fig. 1. Added reference to text.
126993	33	30	33	49	There must be discussion and expert assessment of the "Holocene temperature conundrum". Relevant papers include Liu et al. (2014, PNAS) and Marsicek et al. (2018, Nature); see also Hou et al. (2019, Science Bulletin). [ Trigg Talley, United States of America]	Accepted; possible NH summer bias now stated; Liu, Marsico and Hour are referenced. However, the "temperature conundrum" refers to the mismatch between proxies and models; the latter is out of scope for CH2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4517	33	30	33	49	It is unfortunate that authors cite a paper that was just "submitted" at the time of writing (Kaufman et al., submitted, b). It finally got published on 14 April 2020 in Scientific Data. Originally the review period for the AR6 SOD was to end on 26 April. Does this paper actually qualify for the literature cut-off date? It also means that reviewers do not have sufficient time to study that paper in detail before having to comment. I am myself very intensely involved in Holocene temperature analysis and I cannot confirm the results of the Kaufman and Marcott groups. The HTM was globally much more pronounced than claimed, as indicated by our data review. The biggest challenge may actually be lack of reproducibility. When averaging over a great number of data series - of which quite a few will have issues with age models, proxy and data validity - it is clear that anomalies will always appear less pronounced than they actually were in reality. Our own results indicate that the HTM was mostly global with a few specific regions (mostly ocean areas) in which the temperatures fell during the HTM. Modern temperatures do not have these data issues, neither with regards to timing nor temperature proxy quality. I would strongly advise against far-reaching conclusions when comparing proxy and modern measured data. I also noticed that Kaufman et al did not illustrate their individual proxy series. This makes it hard to spot outliers and check for plausibility. Obviously, the new data compilation is very much welcome and useful. Nevertheless, rushing to conclusions should be avoided as documentation of the Holocene temperature composites is limited and regional data distribution is regionally very uneven and implications not fully discussed. [ Sebastian Luening, Switzerland]	Noted; the "submitted" articles did meet the Dec 31 cut-off date for submission and were made available for expert reviewers via standard IPCC channels. The overall results of the new Holocene temperature reconstruction support previous multi-proxy global compilations.
85015	33	30	34	19	No comments [ Katrine Husum, Norway]	Noted.
135	33	31	33	32	For the Holocene it is nicely described that the thermal maximum occurred at different times in different regions. The same issue is very likely at hand during all other paleoclimatic periods described in this report (LGM, LIG etc). That these cannot be resolved by proxy-reconstructions doesn't mean that they are not important. Modelling efforts in this direction should be mentioned (e.g. Bakker and Renssen, Climate of the Past, 2014). [ Pepijn Bakker, Netherlands]	Noted; agree with comment, however, paleoclimate models out of CH2 scope.
105749	33	31	33	49	I find it strange that there is no acknowledgement that there has been a debate about the representativeness of the MHTO. This has been highlighted from both the modelling and data communities. For example, Liu et al (2014) introduced the term "Holocene Conundrum" around the fact transient models continue to warm right through to the present-day. Other work such as Hessler et al. (2014) suggests that there is so much uncertainty between different proxy techniques that it is not possible to detect a globally averaged signal of such a small size. This debate started after AR5, and I feel that even if you assess it has been resolved, this should be mentioned in the report - even if only as a throw-away caveat. [ Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; possible NH summer bias now stated; Liu, Marsico and Hour are referenced. However, the "temperature conundrum" refers to the mismatch between proxies and models; the latter is out of scope for CH2.
37013	33	31	34	18	This section contains many findings about temperatures and despite them all being estimates only once (p 34 In 7) does the word "estimate" appear. Every finding that refers to temperature should be described as an estimate. [ John McLean, Australia]	Taken into account; in addition to using the word "estimate" more frequently, other words are used to convey the same meaning, such as "approximately" and "indicates". In addition, "reconstructions" are by nature, estimates.
73415	33	33	33	33	Define 'MH'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; all paleo reference period abbreviations are defined in CCB2.1.
8901	33	34	33	43	The discordance here between Harrison et al (2014, 2015) and Kaufmann et al needs assessment. [ Robert Kopp, United States of America]	Accepted; clarified that Harrison's estimate are based on AR5-generation reconstructions
30181	33	34			'modelling target' [ Gilles Delaygue, France]	Accepted, fixed.
21239	33	36	33	36	replace "Kaufman et al., submitted, a" by Kaufman et al.,2020a [ Michael Schmitt, Germany]	Accepted, fixed.
58159	33	36	33	49	would be nicer to focus on global scale and then mention about regional results [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; regional results are out of scope for large-scale indicator remit of CH2.
90305	33	36			Kaufman et al b has been published this year already 2020 [ Jeannine-Marie St-Jacques, Canada]	Accepted, fixed.
30183	33	37	33	38	'median of the Holocene-long... around 6 ka': remove 'of the Holocene-long' since the change is given for 6K [ Gilles Delaygue, France]	Accepted, as suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37007	33	38	33	38	Using data from the period 1850-1900 as a "global mean" is invalid for reasons stated above, which in summary are less than 50% global coverage, and bias towards European temperature measurements and a specific shipping route in the southern hemisphere, in both cases because those are where the majority of data came from. (A simple check of which HadCRUT4 grid cells contained data - i.e. were not flagged as missing data - will show you this.) [ John McLean, Australia]	Taken into account; uncertainties arising from the sparse coverage in the 1850-1900 period are incorporated into the uncertainties quoted in the assessment.
37009	33	42	33	42	No-one knows what pre-industrial temperatures were from anywhere other than at three sites in Europe - Berlin (Germany), De Bilt (Netherlands) and Uppsala (Sweden). To claim that global average preindustrial temperatures are known is downright dishonest. [ John McLean, Australia]	Taken into account; pre-industrial temperature is addressed in Cross-Chapter Box 2.3. There are multiple lines of evidence , not just instrumental.
30185	33	43			Routson et al 2019a and b point to the same reference [ Gilles Delaygue, France]	Accepted, fixed.
23859	33	46	33	46	recsonstruction' typo error, it should be 'reconstruction' or alike. [ Branko Grisogono, Croatia]	Accepted, fixed.
29823	33	46	33	46	Typo in "recsonstruction". [ Hernan Edgardo Sala, Argentina]	Accepted, fixed.
99741	33	46	33	46	"reconstruction" [typo] [ Kira Rehfeld, Germany]	Accepted, fixed.
43071	33	46			Read "method recosntruction based on " rather than "method recsonstruction based on " [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted, fixed.
90307	33	46			misspelling of recsonstruction for reconstruction [ Jeannine-Marie St-Jacques, Canada]	Accepted, fixed.
30187	33	46			'reconstruction' [ Gilles Delaygue, France]	Accepted, fixed.
29825	33	48	33	48	Check the use of parentheses. Consider using "Marcott et al. (2013)" instead of "(Marcott et al., 2013)". [ Hernan Edgardo Sala, Argentina]	Accepted, fixed.
37011	33	48	33	48	Using data from the period 1850-1900 as a "global mean" is invalid for reasons stated above, which in summary are less than 50% global coverage, and bias towards European temperature measurements because coverage was greatest there. (A simple check of which HadCRUT4 grid cells contained data - i.e. were not flagged as missing data - will show you this.) [ John McLean, Australia]	Taken into account; uncertainties arising from the sparse coverage in the 1850-1900 period are incorporated into the uncertainties quoted in the assessment.
73417	33	49	33	49	Change 'centered' to 'centred'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, fixed.
35937	33	51	33	51	It would be good to indicate in this paragraph the time resolution achieved by the studies cited, as it is an important consideration when discussing the uniqueness of current warming. [ Nicolas Belouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, added "annually resolved".
5347	33	51	34	18	This section must specifically discuss and refer to to the highly complex Fig. 2.11.b)-d), which seems to be treated as an afterthought. Discussion of surface temperture is critical to the entire report and needs to be expanded. [ Bryan Weare, United States of America]	Noted; this section specifically discusses Fig. 2.11. Unclear what information reviewer is requesting.
10419	33	53	33	54	The sentence misrepresents what the AR5 said, it did not say the "MWP"/"LIA" were mostly warm/cold. I would strongly recommend looking at chapter 5 in the AR5, or at least the technical summary, to accurately represent the AR5. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP. MWP and LIA are defined in the Glossary.
30189	33	53			'the mostly warm conditions': the word 'mostly' is not clear to me; isn't the problem the spatial cover of this warmth? What about 'the globally warmest conditions'? [ Gilles Delaygue, France]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP. MWP and LIA are defined in the Glossary.
551	33	54	34	18	As a former member of IPCC I know space is limited and many facts were already discussed in the former AR's. Nevertheless, I would like to discuss a few aspects concerning the MWP (MCA), the LIA and the MWP-LIA transition: [ Heinz Wanner, Switzerland]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP. MWP and LIA are defined in the Glossary. (Note: Only the first line of this comment came through IPCC channels. I requested the full (uncut) comment from Winner.
30191	33	54			idem above: 'the mostly colder conditions' , what about 'the globally coldest conditions'? [ Gilles Delaygue, France]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP. MWP and LIA are defined in the Glossary.
115975	33		33		See earlier remark, I suggest not to use the term "global Holocene thermal maximum" which is confusing given the spatial pattern of mid Holocene temperature anomalies. [ Valerie Masson-Delmotte, France]	Accepted, omitted term.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58207	34	1	34	1	Schneider et al., 2015 citation: is this Schneider et al., 2015a or b? From reference list this looks as though it is supposed to be a. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, fixed.
83429	34	1	34	1	You could add to McGregor et al. 2015, the following reference: Abrantes, F., Rodrigues, T., Rufino, M., Salgueiro, E., Oliveira, D., Gomes, S., Oliveira, P., Costa, A., Mil-Homens, M., Drago, T., Naughton, F., 2017. The climate of the Common Era off the Iberian Peninsula. Clim. Past 13, 1901-1918, doi: 10.5194/cp-13-1901-2017. [ Antje H. L. Voelker, Portugal]	Rejected; list is for large-scale reconstructions, not regional scale.
93457	34	1	41	2	CC Box 2.3 and section 2.3.1.1.3 Temperatures during the instrumental period – surface. Primary reference for comments below is Clarke and Richardson (2020, submitted to Earth and Space Science) hereafter CR2020  <a href="https://doi.org/10.1002/essoar.10502294.1">https://doi.org/10.1002/essoar.10502294.1</a> [ David Clarke, Canada]	Noted; see other comments on this topic.
30193	34	1			comma missing before 'quasi-hemispheric'? [ Gilles Delaygue, France]	Accepted, fixed.
4519	34	3	34	3	Other relevant regional syntheses for the MWP in Antarctica, Africa and Oceania: 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. (2019c): The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012, Lüning et al. (2019d): The Medieval Climate Anomaly in Antarctica. Palaeogeogr., Palaeoclimatol., Palaeoecol., 532, doi: 10.1016/j.palaeo.2019.109251. [ Sebastian Luening, Switzerland]	Rejected; list is for large-scale reconstructions, not regional scale.
36345	34	4	34	6	Somewhere around here, it should be noted that the latest synthesis (Newkom et al. 2019) gives essentially the same hockey-stick shape as Mike Mann's original study in the late 1990s. Experts may not enjoy rehashing an old story, but policymakers are still being exposed to the meme that "the hockey stick was disproven." It's important to state clearly that whatever flaws may have existed in Mann's statistical methods, subsequent studies using a wide variety of methods have come to basically the same conclusion. [ Curt Covey, United States of America]	Taken into account; text states that this reconstruction "generally agrees with the patterns reported in AR5", consistent with the reviewer's suggestion that "subsequent studies have come to the same conclusion" as previous work.
10421	34	9	34	10	Are we really going to continue using "MWP" and "LIA" terms when the differences relative to 1850-1900 are so insignificant in the global mean temperature? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP.
30195	34	9	34	10	'multi-centennial intervals of slightly higher and lower temperatures during the MWP and LIA, respectively': Figure 2.11A shows that the 950-1250 period defined as MWP is not the warmest multicentennial period of the CE. This should be discussed because Fig.2.11 questions the 950-1250 period as the warmest. [ Gilles Delaygue, France]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP.
30197	34	13			'Neukom et al (2019)': what about 'PAGES 2K Consortium (2019)', as it appears in the paper, and to be consistent with other PAGES Consortium papers referenced in this Report? (Neukom et al 2019 is a reference to a paper in Nature.) [ Gilles Delaygue, France]	Accepted, fixed.
83945	34	14	32	14	I suggest that it is made clear that "subsurface temperature" refers to "ground subsurface temperature", otherwise it may be confused to oceanic subsurface temperatures. [ Marco Tulio Cabral, Brazil]	Accepted, fixed.
35519	34	15	34	15	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Taken into account; only published sources are used.
55419	34	15			modelling (Cuesta-Valero et al., submitted).(similar cases all over the text, figures and captions) [ Maria del Pilar Bueno Rubial, Argentina]	Noted, Not clear what comment is suggesting.
105499	34	16	34	16	2015), [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, fixed
30199	34	16	34	17	use a non-breaking hyphen for -0.15 [ Gilles Delaygue, France]	Accepted, fixed
10425	34	17			It is good that the differing definitions of "LIA" are mentioned here : "during the LIA (here limited to 1580-1850)". This should be standard throughout the chapter when referring to other studies using "LIA" (and "MWP") to make clear there is no consistent definition, as climate changes were not coherent in either periods (Neukom et al., "No evidence for globally coherent warm and cold periods over the preindustrial Common Era", Nature 2019). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; MCA/MWP not used as paleo reference period in favour of last millennium as per PMIP. MWP and LIA are defined in the Glossary.
5349	34	17			-0.15 [ Bryan Weare, United States of America]	Accepted, fixed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37017	34	23	34	40	Part (b) of Figure - The grey shading to indicate the uncertainty is false because no statistical analysis of the data can properly incorporate (a) errors in the source data (either raw or processed data) and (b) data that is missing due to incomplete and inhomogenous coverage [ John McLean, Australia]	Taken into account; basis for estimating uncertainty discussed in text.
37019	34	23	34	40	Part (c) of Figure C -Using data from the period 1850-1900 as a "global mean" is invalid for reasons stated above, which in summary are less than 50% global coverage, and bias towards European temperature measurements because coverage was greatest there. (A simple check of which HadCRUT4 grid cells contained data - i.e. were not flagged as missing data - will show you this.) [ John McLean, Australia]	Taken into account; assessed uncertainty takes into account data coverage.
37021	34	23	34	40	Part (c) of Figure - This graph should start from 1949 or 1950 because Southern Hemisphere data coverage did not exceed 50% until 1949. In 1850, at the start of this graph, only one weather station in the Southern Hemisphere reported data, by the end of that decade only 10, and only 21 stations were operational in the SH by 1867. (See the CRUTEM4 station data at <a href="https://crudata.uea.ac.uk/cru/data/temperature/crutem4/landstations.htm">https://crudata.uea.ac.uk/cru/data/temperature/crutem4/landstations.htm</a> or McLean (2018), "An Audit of the Creation and Content of the HadCRUT4 Temperature dataset") [ John McLean, Australia]	Taken into account; assessed uncertainty takes into account data coverage.
36341	34	23	34	40	The left side of Panel A in Figure 2.11 has three vertical bars representing the Last Interglacial, Last Glacial Maximum and Mid-Holocene eras. Why not add a horizontal bar around +2degC with an upward-pointing vertical arrow, representing the mid-Pliocene? You could alternatively add a scale break for the full mid-Pliocene "error bar" and perhaps add the Eocene. None of these additions would increase the figure's clutter in any significant way. If I showed Figure 2.11 as-is to a skeptical audience, I could be accused of covering up the fact that Earth's surface temperature has often been as warm as "alarmist" global warming assessments. [ Curt Covey, United States of America]	Rejected; this figure focuses on more recent reference periods. Cenozoic temperature history is covered in CCB2.1 Figure 1 and Figure 2.34, which includes the warmer periods in the deeper past.
39577	34	23	34	42	Among the 1°C of average temperature increase since the pre-industrial period, it is seen in Figure 2.11B that about 0.6°C has been achieved between 1910 and 1945 when the CO2 concentration increased by 11 ppm only. As a result, Ring, M.J., Lindner, D., Cross, E.F., Schlesinger, M.E., 2012 (Causes of the global warming observed since the 19th century. Atmos. Clim. Sci. 2, 401–415) consider that this increase was mainly natural. This was confirmed in IPCC FAR. Since the acceleration of emissions starting in 1945, the increase of temperature has been only about 0.4°C up to the plateau before the El Niño peak of 2016, among which one half might be anthropogenic. This observation strongly disagree with too large climate sensitivity and radiative forcings retained in IPCC AR6. [ François Gervais, France]	Rejected; implications of temperature changes on climate sensitivity is outside the scope of CH2.
35521	34	28	34	28	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Rejected. The use of papers submitted by December 2019 is in accordance with IPCC requirements. Only papers accepted by January 2021 are included in FGD.
37015	34	31	34	33	Part (b) of figure - The agreement of five datasets is meaningless given that they use the same data. [ John McLean, Australia]	Rejected. The datasets use different land and SST analyses, although they are not fully independent as there are only two underlying SST datasets. This is detailed in the revised text and associated table.
126995	34	34	34	34	[GSAT] Box 2.3 discusses the difference between using GMST and GSAT. This box seems to be highly relevant to this report. Recommend moving this box upward in the chapter or in the report. Also, this box is referenced throughout the report, thus it should be clearly and carefully explained. Anticipate some confusion/skepticism for why units are switched from AR5 to AR6. [ Trigg Talley, United States of America]	Taken into account. This box has been substantially expanded. GMST and GSAT are no longer assessed as having had different changes which addresses some of the concerns.
35523	34	34	34	34	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Rejected. The use of papers submitted by December 2019 is in accordance with IPCC requirements. Only papers accepted by January 2021 are included in FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36347	34	37	37	34	Cross-chapter Box 2.3 clearly explains the difference between GMST and GSAT, notes that AR6 for the first time is treating these fields consistently in models and observations, and ends by saying that end-of-century global warming is thus boosted from a range 4.6-6.4degC to a range 4.8-6.5degC. This is all OK, but left unsaid in all the verbiage is an obvious point: the bottom-line effect of correcting this inconsistency in definitions is swamped by the uncertainty in the projections. I recommend stating this point explicitly. We don't want policymakers to get the mistaken impression that previous IPCC reports made a significant error here. [ Curt Covey, United States of America]	Taken into account. This box has been substantially expanded. GMST and GSAT are no longer assessed as having had different changes which addresses some of the concerns.
29903	34	39	34	39	"OLS" has not been previously defined in this chapter (it is defined in page 40). [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
73419	34	40	34	40	Edit reference so ( comes before 2008. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. References have been corrected.
5351	34	47	37	8	This is overlong and thus confusing. Reduce by half. [ Bryan Weare, United States of America]	Rejected. The chapter team have assessed that the length of the box is appropriate.
126997	34	47	37	36	[GSAT] Regarding the adjustment of global temperature to make it appear it is using marine air temperature, it is unclear how the adjustment is made. Is this simply taking the global warming amount and adding 4%, or is something done to the time series or something even more sophisticated? [ Trigg Talley, United States of America]	Not applicable. The comment refers to an adjustment which is no longer being applied.
18477	34	47	37	36	As one who has been favouring GMST (models should in principle reproduce observations) I am pretty convinced by the statement that it will take decades to detect the differences between GMST and GSAT. Two things: a) it would be nice to have small table that summarises the characteristics/advantages/disadvantages of each metric. Stability of data over time? Amount of modelling post-processing needed to estimate GMST etc. b) since the Title of the box poses a question it would be good to answer it: in this report we use GMST when addressing X,YZ; GSAT when addressing A,B,C. [ Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The uses of GMST and GSAT are discussed in the expanded cross-chapter box 2.3. The comment is largely superseded as GSAT and GMST are no longer assessed as having different amount of warming.
10463	34	47	37	36	I fear that much of Cross-chapter box 2.3 is an exercise in counting angels on a pinhead. The evidence of there being a significant effect to be accounted for between the definitions of "GMST" and "GSAT" is almost entirely based on the Cowtan (2015) study, or from studies that use data/analysis/code produced by that study. I have tried to look at this issue, and believe nuances in modelling and observational datasets can be overlooked and lead to over-confident statements about the differences between "GMST" and "GSAT" (Jones, 'Apples and oranges': on comparing near surface temperatures from climate models with observations, submitted Q.J.R.Meteorol. Soc., 2019). More studies, that have independently looked at this issue, are needed before a robust assessment can be made. Given the lack of critical assessment, the high amount of confidence given to the "4%" factor to be applied to "GMST" to get to "GSAT" is misplaced. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The Jones 2020 paper is part of the expanded evidence base for CCB2.3. The uncertainty range in the GMST/GSAT assessment has been widened.
42883	34	47	37	37	I wonder if this box should also include a discussion of the corrections made to proxy data to convert largely SST data between limited latitude bands (eg 60N to 60S) to GMST. I realise this is a slightly different topic but it is related and solved in a similar way (using models to derive a scaling factor). It is actually a major uncertainty especially for the LGM and LIG temperature estimates on page 33. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; the land-sea temperature contrast based on model output from paleoclimate reference periods, is shown in Figure 3.2b
78293	34	47	37	37	The cross box chapter does not suggest clearly which should be the suggested indicator, but merely list why they are different. Suggest to draw out an implication and provide a recommendation. [ Leonie Lee, Singapore]	Taken into account. The uses of GMST and GSAT are discussed in the expanded cross-chapter box 2.3. The comment is largely superseded as GSAT and GMST are no longer assessed as having different amount of warming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50675	34	47	37	37	Cross chapter box 2.3 would benefit from a more thorough explanation of the advantages and disadvantages of using GMST / GSAT to measure global temperature. For example, how consistent are they with observational data, how consistent are they with impacts projections, from where do their uncertainties arise? This would then help inform the justification of the primary use of GSAT in the SPM. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The uses of GMST and GSAT are discussed in the expanded cross-chapter box 2.3. The comment is largely superseded as GSAT and GMST are no longer assessed as having different amount of warming.
132185	34	47	37	38	This box will require a careful revision. It would also need to be valid for all of the AR6, i.e. also address how the scaling changes in projections. A key element would be to report separately the warming over land, ocean, sea ice and explain why there are departures in estimates of global warming depending on how these different elements are combined. Another key question, not addressed at the moment are changes in land surface temperatures. Finally, it should be discussed how the blending across Earth System's realms lead to different estimates, e.g. when blending anomalies vs absolute temperatures. [ Sonia Seneviratne, Switzerland]	Taken into account. These points are addressed in the expanded box.
37023	34	47	37	39	re: Global Mean Surface Temperature (GMST) and Global Mean Surface Air Temperature (GMSTA). You can only mean temperature ANOMALIES because there is insufficient global coverage to determine a global mean temperature at any point in time or even daily mean value. [ John McLean, Australia]	Rejected. GMST and GSAT are considered to be means of anomaly fields here.
37035	34	47	37	39	It is quite obvious from a comparison of CRUTEM4 and HadSST3 data that the global averages of the two are not an exact match and it is quite obvious from basic physics that the air will change temperature faster than the oceans. As I show in McLean (2018), "An Audit of the Creation and Content of the HadCRUT4 Temperature dataset" there are serious uncertainties with both datasets, both in the processing and the source data. The cross-chapter box is taking the position that the air temperature is correct, but this is not a matter decided by the number of papers that support a particular view. Neither dataset is perfect but the HadSST3 data is better mainly because much the station data used in CRUTEM4 has been the subject of multiple dubious adjustments to historical temperature, individually at each station (see section 9.9 of McLean (2018)). [ John McLean, Australia]	Rejected. This comment misses the point of the box as it is comparing air temperature over land (CRUET) with sea surface temperature, whereas the purpose of the box is to assess differences between air temperature over the ocean and sea surface temperature.
89453	34	47			GMST vs. GSAT are not as important than the changes that have been implemented in the observational record since the AR5. No line of sight to this is provided here. This needs to be done and the including of a temperature metric that relates back to the IPCC AR5 that has informed the Paris Agreement should be considered. See comments on Box SMP2 for further details. [ Carl-Friedrich Schleussner, Germany]	Accepted. An expanded box covers all of these matters in FGD.
100007	34	47			We would like to applaud the authors for achieving scientific progress regarding temperature metric, we are recommending a clearer and direct link is explained to the temperature metric used in AR5 which informed the Paris Agreement. While the box points to the important implications the switch to corrected GMST/GSAT has for the Paris Agreement, it offers no guidance on how to allow for a clear and transparent line of sight to the AR5 estimates informing the Paris Agreement. All information on combining observations and projections has to be also provided based on the metrics used in AR5 to allow for transparency and a clear traceability of IPCC warming estimates. [ Caroline Eugene, Saint Lucia]	Taken into account. This is now covered in the expanded cross-chapter box 2.3.
84147	34	47			This box is somewhat troubling as outlined. It offers no guidance on how to allow for a clear and transparent line of sight to the AR5 estimates informing the Paris Agreement. This cannot stand and is a grave oversight from the author team. While the authors must be commended for achieving scientific progress regarding temperature metrics, but abandoning the metrics that informed the Paris Agreement long-term temperature goal is unacceptable. Transparency and traceability of IPCC warming estimates must be demonstrated. [ Jeffers Cheryl , Saint Kitts and Nevis]	Taken into account. The scope of the box has been expanded and now incorporates the multiple factors contributing to the change in estimates of warming between AR5 and AR6 (of which the GMST/GSAT issue is a small part).
37025	34	55	34	55	Observed global surface temperature has traditionally ... Don't make me laugh. There is insufficient monitoring to claim that it is global and it's less of a tradition than a deceitful construct of the last 10 or 20 years. [ John McLean, Australia]	Rejected. This is a statement of opinion by the reviewer with no evidence presented. Uncertainties arising from limited coverage are incorporated into the uncertainties stated in the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1543	34	55	34	56	This cross-cutting box is talking about temperature anomalies. The word 'anomalies' needs to be in at the beginning and also at the beginning of the caption to Figure 2.11 [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. As the assessment findings only refer to changes, the fact that they are anomalies (and the baseline which the anomalies are with respect to) is not material here.
6491	34	55	34	56	Same comment as 74. Is GMST derived from air temperature over sea ice as well as land? This is the definition given in Chapter 3 and what Simmons et al. (2017) assumed in calculating GMST from reanalysis data. When values are created over sea ice in the "conventional" analyses used to calculate GMST, are these values over sea ice calculated using extrapolation only of air temperatures over land, not SST? I have checked this only for HadCRUT5, which extrapolates the land temperature over sea ice if the concentration of the latter is over 25%. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Issues associated with temperature over sea ice are discussed more specifically in the revised text for FGD.
10465	34	55	34	56	Given the "full coverage" datasets discussed later in this box, estimated air temperature over sea ice should be mentioned here. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Issues associated with temperature over sea ice are discussed more specifically in the revised text for FGD.
50677	34	55	34	56	The first sentence of the cross-chapter box refers to 'air temperature at screen height over land'. It might be more helpful to explain what screen this is referring to (presumably a Stevenson screen, not a computer screen), and add 'in order to proxy global surface air temperature' after 'nominally 2 metres' to help explain why this is done. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is clarified in the redefined/expanded box. Reference to screen height has been replaced by 2m.
30201	34	55	34	56	It seems fundamental to me to introduce here the principle that only anomalies of temperature are 'blended', not temperature themselves. Otherwise, 1. the sentence is wrong, and 2. the GMST vs. GSAT problem is trivial and difficult to grasp for readers. [ Gilles Delaygue, France]	Rejected. The definitions are valid whether referring to anomalies or absolute values, but anomalies are used in the assessment.
112841	34	55	35	3	After this initial statement on the non-equivalence of GMST and GSAT, the box continues in a very technical direction. The lay reader would be helped by a few sentences that explain the non-equivalence a bit further: why is the warming more pronounced if you take the calculated temperature at 2m rather than SST? (I assume it's because the SSTs have a slightly stronger influence of the slower-warming oceans than at 2m?) [ Maarten van Aalst, Netherlands]	Taken into account. This is explained further in the expanded cross-chapter box 2.3.
1223	34	55	35	18	There are methods for dealing with different types of temperatures over sea and land such as the projection of spatio-temporal covariance structures. What may be an even greater concern than this issue is how the uneven sampling of earth temperature has changed over time. It turns out that the early records consist of temperatures sampled from regions with moderate temperature variance (e.g. Europe) whereas more sites with pronounced variance (e.g. Siberia) have been included over time. This inhomogeneous sampling of temperature is associated with a warm bias in the 1900th Century and a cold bias towards the end of the record, giving the impression that the global warming has been less than the true trend. Also, this explains why there seems to be a uniform amplitude in the random variations throughout the record despite the fact that the global coverage only was 20% in the early part as opposed to 80% towards the end (reference: GRL, DOI:10.1029/2019GL083474). Moreover, the discussion on GMST vs GSAT seems to me to be subordinate to the effect of global sampling. [ Rasmus Benestad, Norway]	Taken into account. To some extent the results of the Benstead 2019 paper have been superseded by subsequent data set development (in particular, only spatially infilled data sets have been considered for the core GMST assessment, not the HadCRUT4 dataset that Benstead draws on). The Benstead 2019 reference is relevant to the discussion of changes between the AR5 and AR6 temperature assessment in CCB2.3 and is cited there.
1545	34	55	36	55	This box is all about 0,04 deg C! Why is there a need to increase GMST by 0.04 deg C? Show how different GMST and GSAT are as anomalies. If the latter are calculated correctly, the difference will likely be smaller. Starting on line 38 of p35 that Reanalyses increase over 1979-2016 by 2-4% more is very easily misunderstood. Trends and uncertainty ranges given in Cross Chapter Box 2.3 Table 1 (and also Table 2.4) show that the 0.04 deg C is way within the uncertainty range. What is the point of this? Is it so the models can make calculations quicker? Reanalyses use absolute SST fields. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The comment is partly superseded by the fact that a difference is no longer assessed between changes in GMST and GSAT. The expanded cross-chapter box 2.3 now covers the full range of reasons for the change in assessment of observed temperature change between AR5 and AR6.
26051	34	56	34	56	Please, consider replacing "nominally 2 metres" by "nominally between 1.25 and 2 metres" [ Don Alfonso Pino Maeso, Spain]	Taken into account. This is made specific in the expanded cross-chapter box 2.3.
73421	34	56	34	56	No need to spell out 'metres' (use 'm'). [ Burt Peter, 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
58161	34				Figure 2.11: the red and gold lines in the time series can be easily confused with the lines with the large circles outside of the plot. Maybe split the descriptions more [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; figure extensively revised.
81479	35	1	35	2	global surface air temperature (GSAT)> Glossary - Special Report: Global Warming of 1.5°C say "Global mean surface air temperature (GSAT)" [ Kyaw Moe Oo, Myanmar]	Taken into account. Terms are defined through the glossary.
126999	35	3	35	6	[GSAT] The differences between GMST and GSAT are insignificant for some applications and significant for others. A blanket statement of "can no longer be justified" is itself not justifiable. It depends on the purpose and application. [ Trigg Talley, United States of America]	Taken into account. The applications of GMST and GSAT are discussed in the expanded cross-chapter box 2.3.
30203	35	3			'are not directly equivalent.': this term is too loose here, and further in the Box. Why not just writing that SST and surface air variations are not exactly equal? [ Gilles Delaygue, France]	Rejected. In this context, 'not equivalent' means that for some applications, one cannot necessarily be used as a direct substitute for the other.
10467	35	4			Merchant (2013) is not relevant here. As far as I can tell this position paper does not refer to a lack of equivalence between air temperature and blended land/sea/ice temperature. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The Merchant paper refers to various potential definitions of "surface temperature", although it does not attempt to assess differences between them numerically.
10469	35	5	35	11	There is at least one study that deduces that the 'effect' is not that important for historical temperature analysis, especially when compared to all the other observational, modelling and analysis uncertainties (Jones, 'Apples and oranges': on comparing near surface temperatures from climate models with observations, submitted Q.J.R.Meteorol. Soc., 2019). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The extended Cross-Chapter Box 2.3 takes in the broader range of uncertainties in temperature, as outlined in the paper the review comment refers to.
6493	35	11	35	11	"GMST in observations" is clumsy wording, and not quite correct. SR1.5 used GMST from those observationally-based datasets that provided only GMST, such as GISTEMP and HadCRUT4, but it used GSAT from the observationally-based reanalyses. See also general comments 7 and 8 on the entire report. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text is no longer in the revised box 2.3.
99403	35	13	35	15	"Historical changes" could in principle refer to modelled historical change too. Please consider if explicitly stating "observed historical changes" or similar could clarify the message. [ Herman Fuglestad, Norway]	Not applicable. This text is no longer in the revised box 2.3.
37027	35	13	35	19	The logical order of a review shows the evidence first, then draws conclusions and only then, after having shown that some action is needed, might take that action. This "review" is trying to put the action before having proved that action is necessary. [ John McLean, Australia]	Rejected. The box as currently structured puts forward the evidence and concludes with its implications. The introduction is to demonstrate the potential importance of the issue.
127001	35	15	35	17	[GSAT] While a decent case is made that a single global temperature metric should be used for both models and observations, it is far from obvious why that metric should be GSAT. As the cross-chapter box explains, GMST is observable but GSAT can only be estimated from observations, while both GMST and GSAT can be calculated from model output. On its face, this argues strongly for using GMST. As page 35, lines 42-43 notes, this introduces an additional source of uncertainty that "propagates through to analyses that rely upon it in later chapters". Stakeholders cannot afford unnecessary uncertainty. Richardson et al. (2018, ERL), who pointed out the policy implications, thought it "unlikely" that GSAT would be preferred over GMST for the same reason. The argument here for using GSAT is completely absent. This choice (using a metric that's essentially impossible to observe) is deeply troublesome, and the absence of strong (or any) justification is even more disturbing. [ Trigg Talley, United States of America]	Taken into account. The uses of GMST and GSAT are discussed in the expanded cross-chapter box 2.3. The comment is largely superseded as GSAT and GMST are no longer assessed as having different amount of warming.
30205	35	16	35	17	For me the justification of GSAT (instead of GMST) is very poorly done in this Box. The difference is not significant compared to the model spread of sensitivity (Table 1). I cannot understand why GSAT has been chosen (except to save few lines of code when calculating it from model outputs!). Instead, i can see many drawbacks for dropping SST: SST variations are more directly related to ocean heat content variations (the most important climatic metric if any); SST have direct impact on marine ecosystems; oceanic air temperature has (probably) a stronger spatial variability than SST, a source of uncertainty; oceanic air temperature has been scarcely measured so that GSAT essentially relies on models. [ Gilles Delaygue, France]	Taken into account. The uses of GMST and GSAT are discussed in the expanded cross-chapter box 2.3. The comment is largely superseded as GSAT and GMST are no longer assessed as having different amount of warming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10487	35	20	35	38	More discussion is needed about caveats, e.g. - Models don't produce consistent estimate of "SSTs", and they differ with observations which can make comparisons more tricky than stated here (Kennedy, Reviews of Geophysics 2014; Large and Caron, Journal of geophysical research: oceans, 2015; Hausfather et al., Science Advances, 2017, Lauer et al., Remote Sensing of Environment, 2017) There is conflicting evidence that the depth of the top ocean layer may exaggerate differences between trends in marine air temperature and 'SST' (Richardson et al, Nature Climate Change, 2016; Jones, Q.J.R.Meteorol. Soc., submitted 2019). These effects are small compared to observational and model uncertainties (Jones and Kennedy, Journal of Climate, 2017), especially when considering a model 'ensemble of opportunity'. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The variation in model "SST" definitions is an additional source of uncertainty incorporated into the revised assessment in CCB2.3.
98781	35	21	35	21	There are only 2 different SST datasets used in the "numerous datasets" so this overstates the diversity of estimates. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A discussion of the limited number of independent underlying SST data sets for GMST data sets is now included in the main chapter body.
98783	35	21	35	22	This could be interpreted as saying that recently all the datasets became complete throughout the record. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text is no longer in the revised cross-chapter box 2.3.
10471	35	21	35	23	A discussion about what is meant by "data" is needed here. Interpolated/extrapolated data from regions where there is observational data into regions without observational data have upsides and downsides. This must be mentioned. e.g., variability is generally lower in regions which have no observational data to start with, due to the interpolation methodology smoothing out variations (Jones, Advances in Atmospheric Sciences, 2016), which is likely to be unphysical (See also figure 3.3 in chapter 3). This can have implications when comparing with model variability. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Implications of uncertainties arising from the development of quasi-globally complete fields in new data sets are incorporated as part of the broader global temperature assessment.
1553	35	22	35	22	Use a different word than substantial. Substantial to me means a lot, more than half. See the later point on lines 31 and 32. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text is no longer being used.
6495	35	22	35	23	This sentence is not quite correct. Aside from the reference to "entirely observational" discussed in the following comment, there is a published estimate of GSAT by Jones et al.(1999) based on direct analysis of observations. It is referred to mistakenly as an analysis of GMST in Chapter 1, for which comment 59 applies. I note there that comparison has been made (but not published) of Jones et al.'s estimate with values from two reanalyses. Agreement is to within about 0.2°C for absolute values. Figure 1.7 shows that some of the CMIP6 models are as much as 1.5°C different. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The main premise of the comment is incorrect in that the Jones 1999 paper, although it also presents MAT data, uses SST for its core global analysis (the paper is less clear about this than it could be). However, the MAT data cited therein (and the Parker 1995 paper it draws on) are relevant to the SST/MAT discussion in CCB2.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6497	35	22	35	23	It can be argued that there is also no "entirely observational" GMST dataset either. The GMST datasets blend observations within a grid square by averaging the monthly anomalies for each observing station. It is assumed that anomalies are independent of the height of the observing station. This can be regarded as an elementary model. More seriously, the latest datasets use some form of mathematical model to derive values for grid squares that contain no usable observations. Values for these grid squares are not "entirely observational", and may be substantially wrong, as in the case of summertime values over the Arctic Ocean discussed in the next comment. Reanalyses follow an essentially similar procedure, except that they blend and interpolate the differences between synoptic observations and model forecasts that carry forward the retained information from observations made at earlier times. The forecast is made using a physically-based model, which is both a strength (as in the case of the summertime Arctic Ocean) and a weakness (due to biases in model, for example). I recommend avoiding reference to "entirely observational" datasets by rewriting the sentence in question, and the following one, to state that analysing marine air temperature is difficult, and in the absence of any long-term observationally-based GSAT datasets other than from reanalysis, recourse is also made to models. This would give the paragraph a better balance, by mentioning reanalysis up front. There is, after all, no evidence presented that shows that the leading reanalyses give less trustworthy results than ESMs for GMST/GSAT differences, and for the well-observed recent decades it might be expected that the observational constraints on reanalysis fields (which include marine winds and sea-surface temperature) renders them more reliable than the ESMs, even if those constraints do not come from direct observations of marine air temperature. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The term "observational-based" is used where applicable.
50679	35	22	35	23	It might be helpful to briefly explain why traditionally GSAT has been used for observations but GMST has been so for models to help justify why this has been done in the past, as opposed to an oversight. Ending the sentence 'There is, however, currently no regularly updated entirely observational dataset for GSAT' with a brief statement such as 'Due to differences in the methodology of collecting temperature data at sea compared to on land' would help this. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The uses of GMST and GSAT are discussed briefly in the expanded cross-chapter box 2.3, although space limitations preclude a more detailed explanation for the use of GSAT in models.
44297	35	23	35	24	It seems strange to be referring to "currently no regularly updated entirely observational dataset for GSAT". This isn't possible, GSAT is by definition a construct of model output. [ Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. In theory an observational dataset is possible through use of marine air temperature (ship and other platforms), in practice no suitable operational data set exists.
10473	35	23	35	24	The risk of circular reasoning (e.g., Rodhe et al, Climatic Change, 2000) should be noted here. Models are being used to adjust observational datasets which are then being compared with models! [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Circular reasoning is not directly applicable here since the models are being used to adjust historical data, which is then being compared with future projections for a different period.
127003	35	24	35	28	[GSAT] This sentence is absolute nonsense. When the marine boundary layer is near-neutral, the relative humidity is typically below 100%, so the expected lapse rate would naively be dry-adiabatic rather than moist adiabatic. Even if one were to suppose that the near-surface marine lapse rate is moist adiabatic, the resulting change in temperature difference over 2 m altitude is two orders of magnitude smaller than the observed difference in temperature trends. At present there is no "basic physical understanding" of the observed difference, leading to speculation that it's a consequence of the differential warming rates of land and sea surfaces and the (already documented by Cowtan et al. 2015) differential warming rates of air and sea surface when sea ice retreats. [ Trigg Talley, United States of America]	Taken into account. This section has been extensively revised in the expanded cross-chapter box 2.3.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98789	35	24	35	38	The evidence presented doesn't seem strong enough to preclude the possibility that the rate of change of MAT and SST are similar, the uncertainty range has a minimum of 2% faster increase in MAT. Evidence from reanalyses doesn't seem strong due to unknown interactions between the main model and the boundary layer model which may assimilate different variables to the main model. The result is some hybrid between the model physics and the observations as constraint near the surface from the assimilation of satellite observations is likely to be small. This leaves climate models as the main or only source of evidence. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. All available sources of evidence are considered in revised box, including suitable NAT-based datasets. This evidence has led to the revised assessment (of equivalent warming rates of GMST and GSAT) in cross-chapter box 2.3.
58209	35	25	35	25	Can't locate a definition for 'ESM' elsewhere in document. Presuming this stands for Earth System Model? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
98785	35	26	35	26	The mean air-sea temperature difference is about 1 degree so the atmosphere over the ocean will remain slightly unstable/near neutral on average through to 2100 (according to figure on page 165) so this constraint on atmospheric stability seems rather weak. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A fully redrafted section on physical considerations is included in the revised cross-chapter box 2.3.
37029	35	27	35	27	An explanation is required as to why the moist adiabatic lapse rate should decrease with increased temperature when it's fundamental atmospheric physics that raising the temperature will decrease the density of the air. [ John McLean, Australia]	Rejected. The explanation is in the cited reference (Yang and Smith 1985)
30207	35	27			'decreasing' (absolute value) [ Gilles Delaygue, France]	Rejected. This comment appears to be incomplete.
73423	35	28	35	28	Delete , after al. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
99405	35	28	35	30	Does the term "air temperature" used in this sentence refer to the 2m air temperature (=SAT?). I find the use of "air temperature" imprecise and confusing since it could mean the temperature anywhere in the atmospheric column, especially considering that the cited study (Richardson et al., 2018) consequently uses the term "near-surface air temperature". [ Herman Fuglestad, Norway]	Rejected. The definition of air temperature in this context as being at 2m is in the introduction of cross-chapter box 2.3.
10475	35	28	35	31	I would recommend checking the Richardson (2018) numbers. I think they refer to rcp26 CMIP5 experiments. Was that intentional? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is clarified in the revised text. Richardson 2018 presents numbers for both historical-RCP2.6 and historical-RCP8.5 simulations - as the period under discussion is historical differences between the two are minimal (reflecting the availability of different model runs).
10477	35	28	35	31	The effect of changes in sea ice coverage over time needs to be mentioned here. This will impact trends at a gridbox when sometimes air temperature over ice is used, and in other times simulated SST. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sea ice issues are dealt with in more detail in the expanded cross-chapter box 2.3.
93459	35	28	35	32	Percentages in Richardson et al (2018) were relative to GSAT not GMST, so should be adjusted upwards here to be relative to GMST i.e. 6.2% (3.3 %-7.9%). CR2020 finds a similar ratio for 1850-1900 to 2019 based on 24 CMIP6 models: 5.8% (3.3 - 7.9%) [ David Clarke, Canada]	Accepted. Values corrected.
79225	35	28	35	32	It seems the text refers to the ratio of GSAT/GMST, whereas Table 1 of Richardson et al. (2018) reports the GMST/GSAT ratio. Change description or adjust values. [ Martin Stolpe, Switzerland]	Accepted. Values corrected.
50681	35	28	35	36	This section describing the findings of Richardson et al and Gillet et al is confusing due to the use of different terms for the same concept i.e. 'using air temperature over oceans rather than SST' versus 'globally complete GSAT' and 'globally complete GSMST'. It would be helpful to use the same phrase of terminology in both sentences to improve clarity. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This text is rewritten in the revised cross-chapter box 2.3.
37671	35	28	35	38	A summary, rather than enumeration of individual study results, is desired. [ Masahide Kimoto, Japan]	Rejected. As this is a new area for IPCC reports the full range of available evidence needs to be set out in some detail.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37031	35	29	35	29	Yet more tiresome junk claims about 1861-1880 supposedly global average temperature anomalies when in fact the data was far from global. And didn't AR5 show, in text box 9.2, that CMIP5 climate models exaggerated warming? You'd better show some evidence that the model used by Riochardson et al (2018) accurately included every forcing and influence on temperature. [ John McLean, Australia]	Rejected. The AR5 box 9.2 findings (which in any case were only applicable to short-term changes post-1998) have been superseded by the findings reported in AR6 Cross-Chapter Box 3.1. Uncertainty arising from limited coverage in the pre-1900 period is already incorporated into the reported uncertainties for GMST and GSAT.
10479	35	30	35	32	The difference between a model temperature trend when masked and not masked by observational coverage is well known, e.g., fig 12.7 in IPCC WG1 2001 compares model with observed 20th century temperatures, by masking the model. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The role of coverage bias (and its reduced role in more recent observational data sets) is included in the expanded box.
112245	35	30	35	34	Is it not misleading to provide numbers here also for the comparison between full-coverage GSAT vs HadCRUT4-covered GMST, where the main part of the difference is clearly due to the coverage difference concerning the polar regions? If that difference shall also be reported in this box (which I'm not sure about because the box is supposed to be about GMSTvsGSAT), to me it would make more sense not to mix it with the GSATvsGMST effect, that is, to provide numbers for the effect of the coverage difference EITHER consistently for GMST OR consistently for GSAT (or both). [ Helge F. Goessling, Germany]	Taken into account. The purpose of this comparison is to provide a linkage between GSAT and a commonly-reported number (as was also done, loosely, in SR1.5). This specific comparison is no longer included in cross-chapter box 2.3.
1549	35	31	35	32	Why pick out HadCRUT4 as being globally incomplete? All datasets are, but some are more explicit about it than others. Some datasets infill in a variety of ways. The only way to believe that they are doing this better than designating some areas as missing, is to show this with left-out data. Try this in the Antarctic. If you think ERA5 is correct for the whole Antarctic south of 65S from 1979, or JRA-55 is from 1958, then you are deluding yourselves. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text is no longer in the revised box 2.3 (it was, in any case, reporting the results of the cited paper, which referred to HadCRUT4 only). A broader discussion of coverage bias and its reduction in more recent data sets is in the revised box.
37033	35	32	35	32	This sentence shows yet again a delusion that 1850-1900 temperatures were global. Any papers that don't take into account the low coverage of global temperature data prior to 1950 should be excluded from being cited by 6AR. [ John McLean, Australia]	Rejected. The larger uncertainty in global temperature estimates arising from limited sampling in earlier parts of the record is known, and incorporated in the uncertainties reported in this assessment.
10481	35	34	35	35	This is important. It shows how the sample of models can influence the results when comparing trends between simulated "GMST" and "GSAT". Should highlight the issue that CMIP are 'ensembles of opportunity' (Knutti, Journal of Climate, 2008), so one should not over emphasise small differences when bigger uncertainties are present. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. All known model-based evidence is considered in the revised assessment.
79227	35	37	35	37	I suggest to also cite Adams and Dessler (2018). They find a similar GSAT/GMST ratio in an idealized 1pctCO2 experiment with MPI-ESM; see their Table 2. Adams and Dessler (2018): "Estimating Transient Climate Response in a Large-Ensemble Global Climate Model Simulation", doi:10.1029/2018GL080714 [ Martin Stolpe, Switzerland]	Accepted. Reference added.
29827	35	37	35	37	Delete the comma in "Simmons et al., (2017)". [ Hernan Edgardo Sala, Argentina]	Editorial. Copyedit to be completed prior to publication.
73425	35	37	35	37	Delete , after al. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
10483	35	37	35	38	Is this correct? I was unable to find the "2-4%" estimate in Simmons (2017) [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The % values are not reported explicitly in Simmons et al 2017 but are derived from warming amounts reported in that paper (the authors supplied more precise warming amount as they are only reported to 2 decimal places in the paper text).
90309	35	37			Delete comma after Simmons et al., (2017) [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
79035	35	40	35	40	It is inappropriate to apply a model based correction to an observed estimate. It should really be applied to the models where the comparison is made and where GMST is not available from the models. [ John Kennedy, France]	Taken into account. As no conversion is now applied the comment is largely superseded.
37673	35	40	35	42	The inflation can be dependent on waming levels? [ Masahide Kimoto, Japan]	Noted. No inflation factor is now used so this comment is superseded.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10515	35	40	35	43	Need to make clear that this estimate is only valid for specific circumstances. According to Figure S6 in Cowtan et al. (2015) the 'effect' is not spatially uniform. So any climate changes with specific patterns will induce differing impacts. So warming from GHGs will cause a different pattern to that of historic forcings, to future warming, ("Space and timescale limitations"), to an ENSO event, or to a volcanic eruption. They can't all have a 4% correction (see Richardson et al 2018). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The assessment (now with no difference) is for comparison between global-scale variables (GMST and GSAT). No assessment is made as to whether it holds for relationships between SST and marine air temperature at smaller spatial scales.
10517	35	40	35	43	It is not ideal that the assessment of a "4%" adjustment to "GMST" was not in the FOD, or in any previous studies. It worries me that a lot of significance is being put on something that has not really been tested by the climate research community. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. There was in fact an assessment in FOD (Chapter 2 page 40), although it was part of the main chapter text and not in a separate box.
112579	35	40	35	43	Having just cited 4 papers that find the difference between GMST and GSAT to be >5%, and one (based on very different observational products, the reanalysis) suggesting 2-4%, it seems very odd to use a 4% as the default estimate. 5% would be more consistent with both modelled and observational evidence available. [ Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The revised assessment includes the full range of available evidence and hence has a broad uncertainty range.
10489	35	40	35	43	Is this "4%" assessment based on analyses comparing HadCRUT4 with climate models? If so, then can the assessment be extended to the other observational datasets, with their different processing, blending of air and SST techniques, and missing data infilling methods (Jones, Advances in Atmospheric Sciences, 2016)? This needs discussing. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The 4% assessment is no longer made.
8903	35	40	35	45	Yet the paleo is still in GMST terms at the moment, correct? [ Robert Kopp, United States of America]	Noted. Paleoclimate results are indeed in GMST terms.
98799	35	42	35	42	As noted above, the "high agreement" seems an overstatement - none of the sources of evidence are strong, so the evidence is based only on climate model output. To retain this statement further details about exactly what the evidence that agrees is required. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The high agreement, limited evidence assessment indicates that evidence is indeed limited (as the reviewer states) but that the evidence which does exist is consistent.
10485	35	42			"High agreement": this seems to be based almost entirely on the analysis, methods, code, and data of Cowtan et al (2015). Does that warrant such high confidence? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This language is no longer used in the assessment in the revised box.
98791	35	47	35	47	There is a new NMAT dataset under review which is an update of HadNMAT2 (Cornes et al.). This sections should be updated to include those datasets which extend past 2010. Cornes et al. took a different approach to the WW2 period which is likely to give improved results for this period, although issues with the observing system in the period are not yet fully resolved. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The Cornes data set is now cited in cross-chapter Box 2.3.
50683	35	47	35	50	Is there a causal link between World War 2 and the difference between NMAT and SST? If not, 'World War 2' would be better replaced with the precise years in question in order to avoid drawing a connection. If there is a connection, this should be briefly explained. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The causal link here is that the mix of ships (and hence the measurement methods used) used for marine observations changed dramatically during the war relative to periods before and after. This is well documented in the papers describing the NAT and SST data sets which are being described here.
79033	35	47	36	8	It is probably important to mention here that the network for monitoring marine air temperature - the voluntary observing ship programme - is in severe decline. It's not just the case that currently no GSAT dataset exists, it's that the value of such a data set would be compromised by a lack of observations. Berry, D.I. and Kent, E.C. (2017), Assessing the health of the in situ global surface marine climate observing system. Int. J. Climatol., 37: 2248-2259. doi:10.1002/joc.4914 [ John Kennedy, France]	Taken into account. Assessments of the health (or lack thereof) of the observing system are the responsibility of Chapter 1.
58211	35	50	35	50	Kennedy et al., 2019 citation: is this Kennedy et al., 2019a or b? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference issues have been resolved.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98793	35	51	35	51	The divergence between SST and NMAT only appears as a "step change" in the global mean and the particular choice of the climatological period. Examination of the SST/NMAT differences regionally suggests that the cause of the apparent divergence in the early 1990s may actually be earlier, during a period when uncertainties in both variables are larger. There is a clear difference in recent trends in the 2 variables but it's too simplistic to call it a step change at a particular point. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This part of the text has been substantially restructured, and reference to the early 1990s shift reworded.
98795	35	55	35	55	The impact of the use of NMAT for adjustment in ERSSTv5 in the period after about 1990 is likely small due to the very high weighting given to moored and drifting buoys in their analysis. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This does not affect the validity of the statement, which applies also to pre-1990 data.
10491	35	55	36	1	Does any dataset not include adjustments to bucket SST measurements that to differing degrees account for differences between the SST bucket measurement and NMAT climatologies in different periods (Kennedy, Reviews of Geophysics 2014)? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. NAT is not used as a reference for bias adjustments in the HadSST4 dataset.
10493	35	55	36	1	This sentence is a little weak. My impression was that the bias corrections to SST in ERSST (v4, Huang et al, Journal of Climate (2015), and perhaps v5 as well), more or less removed most of the difference between SST and NMAT global trends over the 20th century. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. As comment 98795 states, NAT is mostly not used for post-1990 adjustment, so a conclusion (in effect) that some, but not all, of the difference is aliased into the dataset still holds.
37675	36	3	36	3	Uncertainties in NMAT and SST are mentioned. How is the situation in models and reanalyses when diurnal cycle is considered ? (Or is it considered already? Daily average is used when GSAT is talked about?) [ Masahide Kimoto, Japan]	Noted. GSAT is generally considered at monthly and longer timescales.
1547	36	3	36	8	This says the differences between GMST and GSAT will be unimportant for some decades, so why bring this whole issue up? [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The different uses of GMST and GSAT are discussed in the expanded box.
127005	36	3	36	8	[GSAT] The quantified uncertainties in NMAT and SST arise in part from highly correlated sampling uncertainties, so observational evidence of different trends may be much more readily obtained than is assessed here. [ Trigg Talley, United States of America]	Noted. Even if the uncertainties were totally correlated, the total uncertainty would still be substantially greater than the difference to date, so the conclusion here stands.
98797	36	7	36	8	The text states there are 3 sources of evidence for the difference between GMST and GSAT: theoretical, physical and model-based. Is the evidence described briefly early (a stability constraint and lapse rate) theoretical or physical? I think the contention that there are 3 lines of independent evidence here is going too far. For the model evidence to be strong there would need to be confidence that all the processes and feedbacks were correctly simulated - given the large biases in some surface parameters in some climate models its clear that surface processes are not always well simulated. In comment 13 I noted that the stability constraint as noted did not seem strong, the lapse rate constraint may also not be strong very near the surface (the paper quoted from 1985 used gridded data over the atmosphere and ocean and presented results as means in latitude bands averaged between the surface and 200mb). I guess I'm asking for the evidence to be spelled out more clearly, exactly what the physical and/or theoretical mechanism for the difference is, and why we have confidence in the models - because that is what we are relying on. Evidence from observations over the open ocean is lacking as noted, and the overall relationship between GMST and GSAT depends on the real relationship between SST and MAT over the open ocean, how far the effect of increased temperatures over land stretches over the ocean, and assumptions that are made on the treatment of the sea-ice zone. Representing this with a scaling factor seems optimistic. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been rewritten in the revised box.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10495	36	10	36	23	Something not discussed here or in any of the studies using the analysis of Cowtan et al (2015) is the impact of the lack of consistency of sea ice coverage across models and reality. Sea ice coverage differ markedly between models, and observations, in both climatology and in trends. This inconsistency was found to substantially contribute to the difference between models "GMST" and "GSAT" 20th century trends (Jones, Q.J.R.Meteorol. Soc., submitted 2019). This will have a large bearing on any "adjustment" factor being considered by the authors. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Whilst the specific uncertainties noted by the reviewer are not discussed in detail, issues related to the treatment of sea ice more generally are discussed specifically in the revised box.
1551	36	12	36	23	The issue here relates to changes in sea-ice distributions. The issue also relates to using either the SSTs of the SATs. SATs are fixed, but SSTs are taken by moving ships. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text relating to treatment of sea ice issues has been substantially rewritten.
93461	36	14	36	18	Cowtan et al (2015) find ~3% additional increase in GSAT from GMST where anomalies are used with variable sea ice over time (i.e. former SAT areas become SST). This issue affects Berkeley Earth, NASA GISTEMP and possibly HadCRUT5 (but not Cowtan-Way which uses a fixed monthly median sea ice mask). Also NOAA GlobalTemp does not interpolate over Arctic sea ice and so would be biased even lower. HadCRUT5 appears to interpolate very little before 1950 and may still retain some coverage bias. The 4% increase may therefore be an overly conservative adjustment. I would suggest using the model-based adjustment (~6%) which is already conservative considering all the above factors. [ David Clarke, Canada]	Taken into account. The uncertainty range has been expanded considerably in the revised assessment (partly driven by the different treatments of sea ice issues).
1841	36	14			What does / mean? Change "/" to "or" [ Alan Robock, United States of America]	Accepted.
52759	36	17	36	18	It would be helpful to explicitly mention here that there is a large discrepancy among future projections of climate models in sea ice extent, which makes it problematic to calculate GMST for future warming levels, where sea ice changes differently in different models/ [ Katarzyna Tokarska, Switzerland]	Taken into account. A reference to model spread is included in the revised text.
98787	36	19	36	19	affect? [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
30209	36	19			'affect' (the impact will not produce the estimates) [ Gilles Delaygue, France]	Accepted.
1843	36	19			Change "effect" to "affect" [ Alan Robock, United States of America]	Accepted
112581	36	20	36	23	For balance, it should also be noted that Tokarska et al do not see and effect of changing sea ice concentration in future in mitigation scenarios. As written, it looks as if this 0.1C is a quantification of this effect across all scenarios. [ Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The current wording states that the 0.1C value applies to RCP8.5 scenarios (and hence by implication not to other scenarios).
6499	36	21	36	22	The statement in the sentence that spans these lines is open to question. It is true that the largest changes in Arctic sea-ice cover occur in summer (and early autumn), but temperature anomalies are largest in winter, over regions where sea-ice used to be present, but is no longer. Air-sea interaction is strong in such regions. In contrast, temperature anomalies are small over the Arctic Ocean in summer. This is because air temperatures are predominantly close to 0°C, constrained by the temperature of the underlying sea ice, which tends to be melting, or cold open sea where ice has recently melted. Serreze and Barry's 2011 review of arctic amplification notes that the phenomenon is weakest in summer because temperature rise is limited there. Simmons and Poli (2015) presents observational evidence and document how reanalyses capture this behaviour. The GMST datasets that extrapolate anomalies from land out over the Arctic Ocean perform less well, as they do not take into account the physical processes that constrain the temperature over sea. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This text has been deleted.
52761	36	25	36	25	It would be helpful to also mention in this section the effects of internal variability - since the observed warming and the simulated warming is subject to internal variability, which is however, not relevant to the definition of the Paris Agreement. (e.g. more detail can be found in Tokarska et al., Uncertainty in carbon budget estimates due to internal climate variability (in review at ERL)). [ Katarzyna Tokarska, Switzerland]	Taken into account. Paris Agreement targets are no longer explicitly discussed in the box, although 1.5 C global warming levels are.
58163	36	25	36	51	this section seems to share a similar topic with the section "translating observed GMST ..." on page 35, maybe combine them [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The restructuring of the box means that this structure no longer exists.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1555	36	27	36	28	So it is a relatively minor effect, so why does it need a 3pp box? [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The expanded box includes the full range of reasons for the change in temperature assessment between AR5 and AR6 (which, as the reviewer notes, the GMST/GSAT difference is a relatively minor contributor to)
87291	36	27	36	31	The assessed increase in surface air temperature is higher than in previous reports (even if comparing the same periods). Only a fraction of the difference can be explained by the new concept GSAT instead of GMST (about 0.04C). The largest difference of about 0.1C is due to dataset innovations and new products. This is a very policy relevant statement, as it brings us in fact closer to the 1.5C target. It is by far not clear whether the reassessment or innovations of global surface air temperatures are in ocean temperatures or in land temperatures (or both). So, please make also better descriptions in the text of Chapter.2 in section 2.3.1.1.3 and add the information in the summary of Ch.2. [ Marcel Berk, Netherlands]	Accepted. These matters are covered at greater length in a restructured box in FGD.
10499	36	27	36	37	I am disappointed that so much text is being written and time wasted by authors on a "'definition gap' of 0.04 (0.02-0.08)C". This is tiny compared to all the other observational, model, forcing, and scenario uncertainties, let alone differences due to methodological choices. Have we lost sight of the forest for the trees? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The expanded box includes the full range of reasons for the change in temperature assessment between AR5 and AR6 (which, as the reviewer notes, the GMST/GSAT difference is a relatively minor contributor to).
29829	36	28	36	28	Use "Cross-Chapter Box" instead "cross-chapter box" (two times in this line). [ Hernan Edgardo Sala, Argentina]	Editorial. Copyedit to be completed prior to publication.
10497	36	30			Do not use the term "coverage biases" in reference to observations. We don't know what the real bias is as we don't have full spatial coverage observations to compare with (Or maybe we do e.g., Rayner et al, BAMS in press 2020). The term can be used when referring to models which have not had their spatial coverage reduced to where there are observations. That the "bias" is inferred from models is an important point to highlight. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The inability to accurately quantify a bias does not mean it does not exist.
127007	36	31	36	32	This estimate should be included in the opening paragraph of the box. Potential location would be page 2-35, line 7. [ Trigg Talley, United States of America]	Not applicable. As GMST and GSAT warming are now assessed as being the same the definition gap no longer exists.
102735	36	31	36	33	This sentence is not clear: The best-estimate of the effect currently stands at 0.04C, but has grown since the 3rd Assessment Report cycle when it was only 0.02C – reflecting the rapid overall GMST warming experienced in the last 25 years. If GSAT is the warmer of the two series, and the gap between GSAT and GMST has grown, this implies that either that GMST has increased by less than expected, or that GSAT has increased by more. Surely rapid GMST warming would narrow the gap rather than increase it. Please explain. [ Philippe Tulkens, Belgium]	Not applicable. This text is no longer being used now that GMST and GSAT changes are assessed as being the same.
127009	36	31	36	35	[GSAT] This could be made clearer. The point authors are making is that this boils down to a simple arithmetic problem. If the definition gap is approximately constant at 4%, then the absolute value of the gap will increase as temperature increases. So with 0.7°C warming, the gap should be approximately 0.028°C, 0.8°C about 0.032°C, etc. [ Trigg Talley, United States of America]	Not applicable. As GMST and GSAT warming are now assessed as being the same the definition gap no longer exists.
127011	36	32	36	33	The text here needs wordsmithing; it presently implies that TAR was 25 years ago. [ Trigg Talley, United States of America]	Not applicable. This text is no longer being used now that GMST and GSAT changes are assessed as being the same.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127013	36	33	36	37	[GSAT] The assessed definition gap of 0.04 (0.02-0.08) appears to arise from the first entry of Table 1, where it matches the difference between "Warming 1850-1900 to 2009-2018" in GMST and GSAT. But the definition gap is by definition zero near the midpoint of the reference period by which anomalies are calculated (1985-2004 for AR5), so the gap consists of whatever difference between GSAT and GMST arises between the middle of the reference period and the switchover date, not from the 1800s to the switchover date. For AR5 this interval was about 16 years. Since the reference period is moved forward with successive ARs, an increase in the definition gap requires not an ever-expanding difference between GSAT and GMST but an ever-accelerating difference. Assuming the gap is proportional to temperature itself (kind of shaky), an increasing definition gap requires accelerating temperature increases, which is a scenario-dependent outcome. [ Trigg Talley, United States of America]	Not applicable. As GMST and GSAT warming are now assessed as being the same the definition gap no longer exists.
50685	36	33	36	37	The term 'missing warming' could be misleading in this context - as it is only 'missing' if you are trying to get an accurate figure of GSAT, whereas the definition gap would lead to an overestimate if you were trying to get an accurate figure for GMST. Using the phrase 'missing GSAT', or 'underestimation in the GSAT value' might be better here. The same is true in the summary section. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. As GMST and GSAT warming are now assessed as being the same the definition gap no longer exists.
102737	36	35	36	37	Why would the switchover create an artificial suggestion of cooler temperature projections? Because the GSAT projections would be adjusted downwards to match the GMST observations? [ Philippe Tulkens, Belgium]	Not applicable. As GMST and GSAT warming are now assessed as being the same the definition gap no longer exists.
41553	36	35	36	39	"modelled GSAT fields currently experience a 'definition gap' of 0.04 (0.02-0.08)°C of missing warming at the switchover. I do not understand which switchover: needs precise definition [ Laurent Labeyrie, France]	Not applicable. As GMST and GSAT warming are now assessed as being the same the definition gap no longer exists.
73427	36	36	36	36	You cannot have a 'cooler temperature'. It can be warmer or cooler, or the the temperature can be higher or lower. Please change to "lower temperature" and, ideally, quantify. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
52763	36	39	36	41	It could be clarified here that the Paris Agreement refers to (or is assumed to refer to) the human-induced warming only (free of internal variability) [ Katarzyna Tokarska, Switzerland]	Taken into account. The Paris Agreement is no longer explicitly discussed in cross-chapter Box 2.3, although the 1.5 C global warming level is.
44091	36	39	36	42	The switch in temperature metrics in WGI AR6 is a major concern because the line of sight to the AR5 metrics that informed the Paris Agreement is lost. While the authors point to important implications of this metric switch, no guidance at all is provided on how to tackle this issue and how to ensure a clear traceability and comparability between assessment reports. For the most vulnerable countries an additional 0.1 degC is of grave concern as the 1.5degC goal will slip out of reach much earlier. There needs to be a clear and transparent explanation for this, including detailing all relevant implications. The authors have to make a much better job in this regard. [ Lamin Mai Touray, Gambia]	Taken into account. The Paris Agreement is no longer explicitly discussed in cross-chapter Box 2.3, although the 1.5 C global warming level is.
58213	36	39	36	42	Consider breaking up sentence into: 'This definition gap has potentially important implications for aspects such as: the remaining carbon budget to reach the 1.5°C Paris Agreement target as assessed in SR1.5. This is especially critical given the increased proximity to that target arising from dataset innovations assessed within this report; observation-based estimates of the Transient Climate Response; and projections under various scenarios on centennial timescales.' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This text is no longer being used.
30213	36	39	36	51	i think it is necessary to recognize here that, as shown by Table 1, the most important source of uncertainty in projections for a given objective (or scenario) (including for 'remaining carbon budget' etc.) is the climatic sensitivity spread among models. The GSAT-GMST difference is not significant in this spread. [ Gilles Delaygue, France]	Not applicable. As GMST and GSAT warming are now assessed as being the same the definition gap no longer exists.
18475	36	40	36	40	There are no targets in the Paris Agreement and the target that isn't in the PA isn't 1.5 deg C! [ Jim Skea, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The equivalent text in the expanded box has been reworded.
30211	36	43			'is' (or 'impacts' L.42) [ Gilles Delaygue, France]	Editorial. Copyedit to be completed prior to publication.
52765	36	45	36	46	I think it may be important to clarify it that the sea-ice responses largely differ among the models. [ Katarzyna Tokarska, Switzerland]	Taken into account. A reference to model spread is included in the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1845	36	45			"e.g." used improperly. Do you mean "equilibrium in, for example, 4xCO2 simulations" ? [ Alan Robock, United States of America]	Not applicable. This text is no longer being used.
52767	36	46	36	47	It is unclear how ECR introduced here is different from ECS [ Katarzyna Tokarska, Switzerland]	Not applicable. The term is no longer being used.
44299	36	53	37	8	Could remark on the utility of reporting key numbers in BOTH GSAT and GMST forms for consistency between previous generations of IPCC reports and Paris Agreement text be added to the summary? [ Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The expanded box goes into more depth in accounting for the range of factors contributing to the difference between AR5 and AR6 temperature assessments, as well as the different uses of GMST and GSAT.
1557	36	55	37	1	Don't these use the same SSTs, so this is a circular argument. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. There are a number of different underlying SST data sets in use for these data sets.
6501	37	1	37	1	Change "shall" to "will". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
64737	37	2	37	2	For more clarity on this important topic and its summary, « since the 1850-1900 reference period » might be added after « to date ». [ Serge PLANTON, France]	Not applicable. This text is no longer included.
6503	37	4	37	4	"observed GMST" is a phrasing that should be avoided. GMST is not an observable. It is a quantity deduced from observations, and its value depends on how information is spread from well-observed regions into sparsely observed regions. "Observationally-based estimates of GMST" would be better wording. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. "Observed" as a description of data sets derived from observations (whatever the level of post-processing required) is standard through the report.
52769	37	5	37	7	This paragraph can be easily confusing. It would be helpful to explicitly state which temperature metrics should be used - e.g. for the standard definitions of TCR and TCRE estimates GSAT is used (since, by definition, they are educate [ Katarzyna Tokarska, Switzerland]	Taken into account. Uses of GMST and GSAT are discussed in the expanded box, although as GMST and GSAT are now assessed as having the same change the text has been revised accordingly.
79037	37	8	37	8	There are two other new NMAT data sets now available. UAHNMATv1 (Junod et al 2019), and CLASSnm1 (Cornes et al. submitted) [ John Kennedy, France]	Taken into account. The assessment in FGD includes all known available data sets.
73429	37	8	37	8	Define 'ECS'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
10501	37	11	37	22	Is the model chosen one with the biggest "GMST"/"GSAT" difference? The differences are very model dependent (Cowtan et al. 2015) [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer being used.
10503	37	11	37	22	Individual ensemble spread of model should be shown to highlight the significance of the difference, and show if detectable in simulated data or not. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer being used.
10505	37	11	37	22	CMIP spread should be shown to highlight how important difference is compared to model uncertainty. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer being used.
10507	37	11	37	22	Observational uncertainty should be included, to show significance of effect next to other uncertainties. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer being used.
10509	37	11	37	22	Are you really showing a projection as a single line? Show uncertainties! E.g., <a href="https://link.springer.com/article/10.1007/s10584-011-0178-6">https://link.springer.com/article/10.1007/s10584-011-0178-6</a> [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer being used.
105501	37	13	37	16	Sentence is unclear [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer being used.
58165	37	13	37	16	what is the reference level for the changes in GSAT and GMST? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This figure is no longer being used.
73431	37	27	37	33	Legends should stand alone. Please define acronyms and abbreviations. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
37677	37	27	37	34	Impact on metrics other than presently shown, ECS, TCR, carbon budgets, etc. are necessary, together with brief explanations of how they arise. [ Masahide Kimoto, Japan]	Taken into account. This is part of the scope of the expanded box.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2929	37	27	37	35	Table 1 shows that GSAT is not equal to GMST. How did AR6 select and why? [ Zong Ci Zhao, China]	Not applicable. This table is no longer being used.
37623	37	27	37	35	Additional key metrics should be added to Cross Chapter Box 2.3 Table 1, especially wrt TCRE and carbon budget, in order for readers to better capture the implications. [ Masahide Kimoto, Japan]	Taken into account. Additional metrics are included as part of an expanded box, as flagged in the SOD text.
10511	37	27	37	39	What are the sources of the uncertainties on the observed trends? Spread across the three datasets? Why are the ranges for GSAT wider than for GMST, and non-symmetric? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This table is no longer being used in its current form. The reasons for the expanded uncertainty range for GSAT are discussed in the expanded box.
10513	37	27	37	39	What are the model uncertainties? Are they uncertainties on the multi-model mean, or ensemble spread? How are they calculated? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This table is no longer being used.
52771	37	29	37	39	Is this based on weighted projections or raw CMIP6 model output? It should be clarified here for consistency, given that chapter 4 discusses in depth that raw CMIP6 warming is likely to be overestimated (due to the high ECE models) [ Katarzyna Tokarska, Switzerland]	Not applicable. This table is no longer being used.
68041	37	42	39	27	Add more introduction to the raw data sources for the products that are described and for which the trends are estimated in this Table. To what extent are these products independent? From what differences, then, in their development, do the differences in the estimation of trends shown in Table 2.4 arise? [ Michael Evans, United States of America]	Taken into account. A new paragraph is included which considers the independence of the data sets.
18321	37	42	41	23	I'm surprised to see no mention of the decadal variations in global warming rates, given the tremendous efforts put into studying the recent and past slowdown in global warming rates. I understand this chapter is mainly for documenting the changes, but it will read like a Technical Report, rather than a scientific review, without any mention of the causes of the changes for both T and other variables. [ Aiguo Dai, United States of America]	Taken into account. The 1998-2012 slowdown in warming is covered in a cross-chapter box (Box 3.1) in Chapter 3.
96227	37	42	41	23	The re-assessment of the observational record of the historical warming level has huge implication for many highly policy relevant statements. We urge the authors to raise the quintessence of this change to SPM and TS (e.g. Box SPM.1 or Box TS.1). We also request the authors to add some more information on the re-assessment in these summary documents. Has there been a re-assessment on the preindustrial warming level (here 1850-1900) or has there been changes also in the course of the observational record in the 20th century or both? Please see our related comment on the Entire Report on _Historical global temperature. [ Nicole Wilke, Germany]	Taken into account. These matters form part of the expanded brief for the revised cross-chapter box 2.3.
37037	37	43	28	2	The review of SAR revealed that the IPCC did not audit the HadSST or CRUTEM temperature data that it used in that report (see AR5 review of chapter 2, comment 1106) so, (a) Did the IPCC audit the temperature data that it used in 6AR? (b) Did the IPCC take note of what seems to be the only published audit of that data (McLean (2018), "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" and if not why not? (Please don't try to claim that it was published too late when some of your references were published in 2019 and don't try to claim that it is not per-reviewed when you already cite AMAP(2014), chapters of books e.g. Armand et al (2017) and chapters of reports e.g. Azorin-Molina et al (2017 and 2019) which are unlikely to also be peer-reviewed to journal standards). [ John McLean, Australia]	Rejected. The IPCC's role is to assess the available literature, which includes a number of independent lines of evidence. Whilst McLean (2018) finds a number of potential errors at the individual data point level, no evidence is presented that these are material to large-scale variables such as annual GMST.
1559	37	43	37	43	Define 'substantial'? Is this the same 'substantial' as the coverage issues in the cross-box Table. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The term here is used here in a general sense and no formal definition beyond its standard English usage is required in this context.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1561	37	43	38	2	Just giving numbers of stations (35K) or of numbers of marine observations now in ICOADS is not that helpful. What matters is where they were taken, and when they were taken. Need to show this in terms of how many more areas now have real coverage. This can't be seen with dataset that are spatially infilled by various techniques. Rather than quoting meaningless numbers, show Hawkins and Jones (2013) and the diagram of Callendar's 1938 paper against CRUTEM4. We don't need thousands of stations if all we are after is the hemispheric mean. I know we want spatial patterns, but stating large numbers you also need to show where the stations are, and where coverage is inadequate (for example in Africa than it was in the 1990s). [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The purpose of this paragraph is illustrative, to demonstrate that there have been developments in data sets since AR5. It is not intended to quantify the benefits of these developments. Rewording makes it clearer that greater spatial coverage is one of the benefits.
98801	37	48	37	48	ICOADS have not incorporated any digitised data since the cut off date for release 3, so before 2014. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. "recently digitised" replaced with "previously undigitized" to make the point clearer.
115979	37		37		CCB 2.3 needs to provide more clarity and transparency to causes for changes compared to AR5, SROCC SRCCL and SR15, including corrections to SST datasets; extending datasets to the present by a few years (effects on GSAT, SST, GMST, GSAT). This needs to build on what we need to have in the SPM and TS too (clear description of progress / clear communication). [ Valerie Masson-Delmotte, France]	Accepted. The box has been restructured to include a broader assessment of these matters as stated in the comment.
37039	38	1	38	2	What does "enhanced consideration of ... quality" mean and how can you determine quality if you haven't audited the data? [ John McLean, Australia]	Taken into account. "quality" is not the best word in this context and the text has been reworded.
1563	38	5	38	18	Need to state somewhere here that for a given accuracy over a region, more NMAT readings are needed than for SST, as the day-to-day variability is substantially higher than for SST. 'Substantially' here means 2 or 3 times as large. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A reference is included in the revised text to SST being less variable than MAT, although no explicit reference is made to the relative number of observations required. The only assessment finding MAT contributes to is the uncertainty assessment of the GMST-GSAT comparison carried out in the expanded cross-chapter box 2.3.
87899	38	5	38	31	This section needs to include a brief summary of the rates of coverage of the world's oceans and the inherent data quality problems. Prior to WWII at least 50% of the ocean surface is completely unsampled, and the parts that are sampled have gaps in documentation that frequently make it uncertain how the measurements were taken (e.g. Hirahari et al. 2014). The discussion as currently written makes it sound as if the only problems with SST products are the integration of recent buoy data and a temporary postwar period of uncertainty regarding use of engine intake data. [ Ross McKittrick, Canada]	Taken into account. Changes in coverage bias over time are considered in the expanded cross-chapter box 2.3.
37041	38	5	38	31	Are you unaware of errors in the ICOADS dataset, such as incorrect latitude and longitude - inland more than 50km from the coast -, incorrect transcription of hand-written ships' logs, large variations in data in ships just a few km apart and (supposedly) taking readings simultaneously, meteorological data being recorded when ships were in port for extended periods? See McLean (2018) "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" [ John McLean, Australia]	Rejected. The IPCC's role is to assess the available literature, which includes a number of independent lines of evidence. Whilst McLean (2018) finds a number of potential errors at the individual data point level, no evidence is presented that these are material to large-scale variables such as annual GMST.
37043	38	5	38	31	You also fail to mention the large variation in ST coverage over time. Coverage didn't increase in linear fashion from 1850 but dipped markedly during World War II (e.g. Southern hemisphere coverage in March 1939 was 53% but in October that year it was just 23%). Further, when the war was being fought in the Pacific Ocean it meant less data from that region, and that's important because 50% of Earth's surface area lies between latitudes 30N and 30S. See chapter 8 of McLean (2018), "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" for details. These important caveats should be mentioned prominently in the report. [ John McLean, Australia]	Taken into account. Changes in coverage bias over time are considered in the expanded cross-chapter box 2.3. The additional uncertainty arising from limited data coverage during the stated period is already incorporated into the uncertainties quoted for GMST changes.
30215	38	5			remove parenthesis before 'Kennedy' [ Gilles Delaygue, France]	Editorial. Copyedit to be completed prior to publication.
43073	38	5			Read "HadSST4 (1850-present, Kennedy et al., 2019b), " rather than "HadSST4 (1850-present, (Kennedy et al., 2019b), " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Copyedit to be completed prior to publication.
90311	38	5			delete ( in front of Kennedy [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
73433	38	6	38	6	Another ) required after '2017'. [ Burt Peter, 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
58217	38	8	38	8	Kent et al., 2013 citation: is this Kent et al., 2013a or b? From reference list this looks as though it is supposed to be b. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References have been clarified.
79017	38	8	38	8	There are now other MAT data sets, including Junod and Christy 2020. Junod, RA, Christy, JR. A new compilation of globally gridded night-time marine air temperatures: The UAHNMAV1 dataset. Int J Climatol. 2020; 40: 2609–2623. <a href="https://doi.org/10.1002/joc.6354">https://doi.org/10.1002/joc.6354</a> [ John Kennedy, France]	Taken into account. FGD includes assessments of new data sets which have emerged since SOD.
98803	38	8	38	8	Also NMAV datasets from Junod and Christy and Cornes et al. (under review) [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. FGD includes assessments of new data sets which have emerged since SOD, including the two named.
79965	38	8	38	10	Same question as the one I put to the FOD as it has not been answered : 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? Important to resolve this as the text is ambiguous at the moment on this point. [ Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Which observation platform has the greatest "absolute" accuracy is not relevant to this assessment - for changes over time, homogeneity through time is what matters (i.e. a non-time-varying bias has no impact on observed changes).
79003	38	8	38	13	The engine room intake biases change all the way from the first clearly labelled measurements in the 1930s to the present. This was a key difference between HadSST3 and HadSST4 in the period since the early 2000s and was one reason for differences between HadSST3 and ERSSTv4 identified by Hausfather et al. 2017. The difference in engine room biases between the period up to the 1970s and after the 1970s is a key change between HadSST3 and HadSST4. [ John Kennedy, France]	Taken into account. The text has been reworded to make it clear that engine room intake biases also exist outside the 1940-1970 period.
35037	38	8	38	13	Does the term buoys refer to all types of platforms (surface drifters, fixed stations and Argo floats) or just to surface drifters? Suggest the need to be specific. [ W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The 0.12 C figure comes from Huang et al 2015 and refers to the average across all buoys. The potential differences between drifting and moored buoys are noted, but moored buoys in practice make only a marginal contribution to global SST data given their small numbers.
58219	38	9	38	9	Rayner & Kennedy (submitted) citation leads to no reference in reference list. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References have been corrected.
87901	38	9	38	10	While the global average buoy differential is estimated to be 0.12 C, Kennedy et al point out it ranges + or - 0.17C depending on location. [ Ross McKittrick, Canada]	Rejected. This text specifically refers to global averages so regional differences are not relevant here.
79005	38	11	38	11	Kent and Kaplan looked at the North Atlantic only at night during windy conditions. Kennedy et al. 2019 performed a global analysis over a longer period. Also, it is the fleet-average behaviour here that is important rather than the set up in individual ships. [ John Kennedy, France]	Taken into account. The point being made here is that the bias is not consistent or of uniform sign. Rewording has been done to make this clearer.
98805	38	13	38	13	ERI biases remain an issue through the 1990s at least. [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been reworded so as not to preclude engine room intake biases outside the 1940-1970 period.
98807	38	15	38	15	Not truly independent - based on the same source datasets [ Elizabeth Kent, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The use of "independently-derived" indicates that it is the derivation from the source data which is independent, not necessarily the data themselves.
79007	38	20	38	21	Kennedy et al. 2019 and Rayner and Kennedy (submitted) also corroborate components of the SST record. [ John Kennedy, France]	Accepted. References added.
79011	38	20	38	31	There are multiple new papers by Chan and Huybers looking at uncertainty in SST. They highlight, amongst other things, errors that correlate at a national scale or the level of individual data "decks" in ICOADS in the early 20th century with potential implications for regions rates of change. Uncertainties in ship locations are also dealt with. e.g. Chan D., E. C. Kent, D. I. Berry and P. Huybers, Correcting datasets leads to more homogeneous early 20th century sea surface warming, Nature, 571, 393–397, doi: s41586-019-1349-2. Further to questions of basin scale variability Davis et al. 2019 point out significant uncertainties. Davis, L.B.B., D.W.J. Thompson, J. J. Kennedy and E. C. Kent, 2019: The importance of unresolved biases in 20th century sea-surface temperature observations, BAMS, 100, 621–629, doi: 10.1175/BAMS-D-18-0104.1. [ John Kennedy, France]	Taken into account. Findings from the Chan 2019 and Davis 2019 papers are incorporated into the revised text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79013	38	20	38	31	There are a number of papers from the HOSTACE project which are relevant here, which have helped to assess and reduce key uncertainties in SST analyses, including papers such as Carella, G., Kennedy, J. J., Berry, D. I., Hirahara, S., Merchant, C. J., Morak-Bozzo, S., & Kent, E. C., 2018: Estimating sea surface temperature measurement methods using characteristic differences in the diurnal cycle. Geophysical Research Letters, 45, 363-371, doi: 10.1002/2017GL076475 and Carella, G., A. K. R. Morris, R. W. Pascal, M. J. Yelland, D. I. Berry, S. Morak-Bozzo, C. J. Merchant and E. C. Kent, 2017: Measurements and Models of the Temperature Change of Water Samples in Sea Surface Temperature Buckets, QJ, 143, 2198–2209, doi: 10.1002/qj.3078. and Carella, G., E.C. Kent and D.I. Berry, 2017: A probabilistic approach to ship voyage reconstruction in ICOADS, Int. J. Climatol., 37, 2233–2247. <a href="http://doi.org/10.1002/joc.4492">http://doi.org/10.1002/joc.4492</a> [ John Kennedy, France]	Noted. To the extent that the findings of these papers influence the datasets used in this assessment, they are incorporated in the references already cited.
29831	38	21	38	21	The acronym "ATSR" is not defined in this chapter. Consider adding its full name. [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
79009	38	23	38	24	It would be useful to indicate over what periods consistency is claimed. Cowtan et al. 2018 actually highlighted a number of inconsistencies. [ John Kennedy, France]	Taken into account. Has been changed to "broadly consistent" to incorporate some level of uncertainty.
89863	38	25	38	29	I suggest adding Cowtan et al. (2018) after "particularly during the World War II period", as they clearly show that large biases still exist (see Fig. 9 therein). While based on HadSST3, HadSST4 still carries some of the same WWII bias given the rather moderate change during this period compared to what Cowtan et al. (2018) found. [ Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The text already states that differences remain in this period so the additional citation is unnecessary (the citations in this part relate to specific potential underlying causes for the differences).
1565	38	28	38	28	The fact that SST measurements during WW2 are disproportionately US Navy is incorrect. For 1945-1947 there are more Royal Navy readings. Is it worth pointing out that someone told the US Navy to physically destroy 7000 cu ft of records from 1940 to 1947 as being of no scientific value in 1974 (Wilkinson et al, 2019, Best Practice Guidelines for Climate Data Rescue - C3S, Lot 1) [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The 1945-1947 period quoted here is predominantly after WW2. The cited loss of records is extremely disappointing.
45297	38	28	38	29	Chan and Huybers (2019) show that bucket temperature measurements are biased based on the source of data doi: <a href="https://doi.org/10.1175/JCLI-D-18-0562.1">https://doi.org/10.1175/JCLI-D-18-0562.1</a> [ Anson Cheung, United States of America]	Taken into account. This reference has been included in expanded discussion of uncertainty.
79015	38	29	38	31	The key difference identified in Kennedy et al. 2019 is between SST and night marine air temperatures (NMAT). NMAT cools relative to SST in the early 1990s. Given that ERSST is adjusted using NMAT, this has implications for uncertainty in SST changes from the mid 20th century to present. Instrumentally homogeneous data sets such as those used in Hausfather et al. 2017 don't extend back far enough to resolve this uncertainty. Rayner and Kennedy (submitted) use an extended satellite data set to compare with ERSST5 and HadSST4, but uncertainties in the early satellite data preclude a neat resolution of the problem. [ John Kennedy, France]	Taken into account. This is reporting the findings of these papers that some differences remain, notwithstanding the relatively high data density during this period that the reviewer notes. Further research may identify underlying reasons for these differences but this has not taken place to date.
68039	38	29	38	31	"Kennedy et al. (2019) and Rayner and Kennedy (submitted) also identify differences between the new HadSST4 dataset and other SST datasets in the 1980s and 1990s, indicating that some level of structural uncertainty remains during this period." This statement is inconsistent with the rest of this section that indicates a resolution of inconsistencies between independent data sets, and especially needs explaining, as this is within the satellite era and dense observational coverage; what is meant by structural uncertainty in this context? Systematic differences in bias corrections? [ Michael Evans, United States of America]	Taken into account. This is reporting the findings of these papers that some differences remain, notwithstanding the relatively high data density during this period that the reviewer notes. Further research may identify underlying reasons for these differences but it is not the IPCC's role to carry out research.
29833	38	34	38	34	The acronym "GHCN" is not defined in this chapter. Consider adding its full name. [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
35525	38	36	38	37	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Rejected. The use of submitted papers in SOD is within the guidelines for SOD. Only Accepted papers are included in FGD.

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1227	38	36	38	46	The use of reanalyses to study trends is not recommended due to changes in inputs from observational network, as noted. But this effect is the same for all reanalyses (because they assimilate the same observational data), so it does not help if different reanalyses give the same trends. The problem is the uneven addition of external information to the models and this unevenness is the same for the different models/analyses. Skip this part? [ Rasmus Benestad, Norway]	Not applicable. There is no reference to reanalyses in the identified lines. On the more general point re: reanalyses, the reviewer's concern is noted, but is not unique to reanalyses (e.g. thermometer screen changes affect all GMST data sets, but different data sets deal with this issue in different ways).
37045	38	37	38	37	How can you claim that the new version of CRUTEM is of better quality if you don't audit it? And why are you citing a dataset that doesn't seem to be available for independent examination? (FWIW, two months ago the data for weather station Golden Rock Airport in St Kitts and Nevis in the Caribbean showed mean monthly temperatures of 0.0C in December 1981 and December 1984, which means they are included in the calculation of the long-term average temperature for that month and the related standard deviation. This is more than 15 months after the CRU was advised of these and many more errors) [ John McLean, Australia]	Rejected. The data set met the SOD requirements that the relevant paper was submitted by 31 December 2019. It subsequently met the acceptance requirements for its inclusion in the FGD. Rewording makes it clear that it is additional quality control processes being referred to here.
3995	38	37	38	40	This sentence could be revised to read "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 (Ref.: 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). [ Guoyu Ren, China]	Rejected. Only a brief summary is possible here for space reasons. It is expected that readers requiring the suggested level of detail will refer to the cited papers.
58221	38	42	38	47	Consider breaking up sentence into: 'The most recent analysis of Thorne et al. (2016a, 2016b) compared a broad range of gridded estimates of change in DTR. This included a new estimate derived from the ISTI databank release using the pairwise homogenization algorithm employed to create GHCnv4, and grids derived from the Vose et al. (2005), HadEX2 (Donat et al., 2013a), HadGHCND (Donat et al., 2013a), GHCNDEX (Donat et al., 2013b), Berkeley Earth (Rohde et al., 2013), and CRU TS (Harris et al., 2014).' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence has been reworded as suggested.
37047	38	42	38	53	Why are you using estimates of DTR when daily maximum and minimum temperatures are widely available from national meteorological services (NMSs)? [ John McLean, Australia]	Rejected. While it is true that maximum and minimum temperatures are available at national level in many countries, there is currently no global data set of these.
8691	38	45	38	45	Could add HadEX3 data here too (Dunn et al, 2020, submitted) as an update to HadEX2. [ Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This reference is to the datasets cited by the Thorne et al papers, so is specific to HadEX2. Clearly HadEX3 will be relevant to any future studies of this type.
37049	38	50	38	53	Isn't this final sentence also based on nothing more than estimates? If it is then say so. [ John McLean, Australia]	Rejected. The view of the authors is that adding the word "estimates" would not add anything to the findings reported here.
73435	38	51	38	51	Change 'northern hemisphere' to 'Northern Hemisphere'. [ Burt Peter, 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
37051	38	55	39	3	This sentence is dubious. It ignores the problem that the WMO's recommended practice of adjusting temperature data, and methods similar to the WMO's, adjust previous data by a constant amount when in fact the data might have been distorted by gradually increasing non-meteorological influences, such as increasing urbanization, deterioration of screens or the growth of nearby vegetation. These situations - or more correctly the adjustments that followed the change to rectify these situations - probably account for more than 80% of all data adjustments. In short, most temperature data is very likely to have been adjusted in excess of the amount required and the problem compounded by multiple adjustments. And by the way, this false adjustments retain the spurious trends of the original data. See section 9.9 of McLean (2018), "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" (and appendix 5). Your sentence is an attempt to glibly dismiss a situation that few people seem prepared to investigate. [ John McLean, Australia]	Rejected. No evidence is presented for the assertion that more than 80% of adjustments are affected (including in the cited McLean 2018 reference).
87903	38	55	39	7	I find the discussion of urbanization inadequate. First if you are going to merely restate the AR5 conclusion then you have to quote it accurately, especially since AR5 went a small way to acknowledging a scandalous fabrication in AR4. In AR4 the Lead Authors invented evidence to dismiss a pair of papers finding evidence of correlation between warming rates and industrialization. They claimed "McKittrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanisation and related land surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4), which exhibit large-scale coherence. Hence, the correlation of warming with industrial and socioeconomic development ceases to be statistically significant." This claim that our results were insignificant was made up and was one of the topics subject to inquiry by the Muir Russell panel following the leak of Climategate emails which implicated Jones and Trenberth as having been "determined to keep [the papers] out of the IPCC report even if we have to redefine what the peer review literature is." Jones' only defence was that there is "no need to calculate a p-value for statements based on the laws of physics." [ Ross McKittrick, Canada]	Rejected. The wording here is consistent with the AR5 assessment finding (p189 of Chapter 2 in AR5)
87905	38	55	39	7	Between AR4 and AR5 I published a pair of papers refuting the AR4 claim and another paper that had challenged our earlier work (McKittrick 2010, McKittrick & Nierenberg 2010). Later, AR5 (ch 2 p. 34) conceded the AR4 claim was groundless: "AR4 concluded that this correlation ceases to be statistically significant if one takes into account the fact that the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes but provided no explicit evidence for this overall assessment result." It then went on to concede that we had shown significant evidence for contamination of the surface record: "Subsequently McKittrick and Michaels (2007) concluded that about half the reported warming trend in global-average land surface air temperature in 1980–2002 resulted from local land-surface changes and faults in the observations. Schmidt (2009) undertook a quantitative analysis that supported AR4 conclusions that much of the reported correlation largely arose due to naturally occurring climate variability and model over-fitting and was not robust. Taking these factors into account, modified analyses by McKittrick (2010) and McKittrick and Nierenberg (2010) still yielded significant evidence for such contamination of the record." [ Ross McKittrick, Canada]	Not applicable. This comment discusses differences between AR4 and AR5 and makes no comment on the AR6 text.
87907	38	55	39	7	Thus, the conclusion of the matter as of AR5 was not that urbanization accounted for less than 10% of the global trend (itself a baseless claim that originated in Jones' 1990 Nature paper as an offhand conjecture in the conclusions, not a quantitative result in the body of the paper) but that previous papers had attributed as much as half the post-1980 land warming to data contamination, that the AR4 had claimed otherwise with no supporting evidence, and subsequent research had affirmed significant contamination of the surface record exists. If you are going to claim that nothing has been published since AR5 that changes the conclusion since AR5, that is the conclusion you must cite. [ Ross McKittrick, Canada]	Rejected. This text is specific to the AR5 conclusion regarding the effect of urbanisation on global-scale averages.

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87909	38	55	39	7	However another important paper since AR5 is my 2013 Climatic Change paper [McKittrick, Ross R. (2013) Encompassing Tests of Socioeconomic Signals in Surface Climate Data. Climatic Change doi 10.1007/s10584-013-0793-5. Volume 120, Issue 1-2.], which was published only a couple of weeks after the deadline for inclusion in AR5. The IPCC has relied heavily in the past on Parker's argument that the similarity of warming trends between windy and calm nights refutes the claim that urbanization drives warming. Also other teams (including Berkeley) have relied on tests of warming differences between rural and urban locations to support the same view. In my CC paper I showed that these tests are poorly structured because they depend on unstated model restrictions and they can be shown to fail to find contamination patterns even in data sets where it is known a priori that contamination exists. I set up a statistical model which shows that the Parker-type windy/calm equivalence, and the rural/urban equivalence, can be shown to be testable restrictions in general regression models, and the restrictions are rejected on a couple of relevant data sets. [ Ross McKittrick, Canada]	Rejected. While the McKittrick 2013 paper is correct in asserting that the lack of difference between calm- and windy-night trends does not, on its own, create a statistical inference of a lack of urban signal, that inference arises from the combination of those statistical results and physical results about the weak urban signal on windy nights (e.g. Johnson et al 1991, cited in Parker 2004).
87911	38	55	39	7	Another paper that proves the importance of urbanization over the land record (and which was not included in AR5) is [McKittrick, Ross R. and Lise Tole (2012) "Evaluating Explanatory Models of the Spatial Pattern of Surface Climate Trends using Model Selection and Bayesian Averaging Methods" Climate Dynamics, 2012, DOI: 10.1007/s00382-012-1418-9], in which we combined GCM-generated spatial warming patterns with observed socioeconomic growth measures which are omitted from climate models, and showed that the latter have very significant explanatory power over land even after controlling for the model-predicted patterns. We also showed that when the different explanatory groups are used in combination, we can almost always omit the climate model-generated pattern as insignificant, but we can never omit some of the socioeconomic measures. [ Ross McKittrick, Canada]	Rejected. The papers cited use GDP per unit area at the national level as an implied proxy for urbanisation, but using national-level data makes the connection between the two weak (for example, it would imply weak urbanisation signals in the United States, and almost none in Australia, Russia and Canada, as these countries have large areas and low/very low population densities relative to European countries, while having broadly comparable GDP per capita). The existing text acknowledges (P39 L2-3) that the 10% is a global average and that some rapidly urbanising regions may have a stronger urbanisation signal.
87913	38	55	39	7	Therefore, if your position is that nothing has changed since AR5, that means you still take the view that AR4 made a groundless claim to dismiss evidence of surface data contamination and that the available analysis up to that point indicated the contamination is indeed significant. You cannot quote the 10% number since it has never been substantiated, even though the IPCC has repeatedly used it. In earlier AR's the citation to Jones 1990 was provided, which in addition to being long obsolete is a deception since that paper provides no support for the claim. The evidence that has emerged since AR5 further reinforces the view that the land surface record after 1980 has been significantly contaminated by urbanization, that this evidence is not rebutted by making static comparisons of windy/calm or rural/urban sites, and that a significant fraction of the spatial warming pattern over land cannot be explained by climate models but can be explained by measures of socioeconomic development. [ Ross McKittrick, Canada]	Rejected. The reviewer's implied assertion that no evidence was presented in AR5 in support of their 10% assessment is false.
87915	38	55	39	7	Sources: McKittrick, Ross R. and Lise Tole (2012) "Evaluating Explanatory Models of the Spatial Pattern of Surface Climate Trends using Model Selection and Bayesian Averaging Methods" Climate Dynamics, 2012, DOI: 10.1007/s00382-012-1418-9; McKittrick, Ross R. and Patrick J. Michaels (2007) Quantifying the influence of anthropogenic surface processes and inhomogeneities on gridded global climate data. Journal of Geophysical Research-Atmospheres, 112, D24509, doi:10.1029/2007JD008465. ; McKittrick, Ross R. (2010) Atmospheric Oscillations Do Not Explain the Temperature-Industrialization Correlation. Statistics Politics and Policy Vol 1. No. 1., July 2010. ; McKittrick, Ross R. and Nicolas Nierenberg (2010) Socioeconomic Patterns in Climate Data. Journal of Economic and Social Measurement, 35(3,4) pp. 149-175. DOI 10.3233/JEM-2010-0336; McKittrick, Ross R. (2013) Encompassing Tests of Socioeconomic Signals in Surface Climate Data. Climatic Change doi 10.1007/s10584-013-0793-5. Volume 120, Issue 1-2. [ Ross McKittrick, Canada]	Rejected. The papers cited use GDP per unit area at the national level as an implied proxy for urbanisation, but using national-level data makes the connection between the two weak (for example, it would imply weak urbanisation signals in the United States, and almost none in Australia, Russia and Canada, as these countries have large areas and low/very low population densities relative to European countries, while having broadly comparable GDP per capita). The existing text acknowledges (P39 L2-3) that the 10% is a global average and that some rapidly urbanising regions may have a stronger urbanisation signal.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3997	38	55	39	7	<p>In China and East Asia, there are many publications on this topic, and the authors should pay more attention to them. Majority of the works have confirmed that a large and significant urbanization effect existed in the current surface air temperature data series as averaged in a large area. In particular, the research group from the National Climate Center of CMA has made a huge effort to investigate this issue in the last two decades, and they reported a 20-40% urbanization contribution to the overall annual mean warming in China's mainland over the last five to six decades. The present assessment is incorrect. The conclusion comes from the Chinese authors who cooperated with P. Jones. They hold that the urbanization contribution is less than 10%. This has been frequently confirmed wrong in China and East Asian countries. I would suggest to revise it to read as: "....., 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) (Ref. : References: Ren and 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., et al., 2016: Contribution of urbanization to warming in China. Nat. Climate Change, 6, 706–709, <a href="https://doi.org/10.1038/nclimate2956">https://doi.org/10.1038/nclimate2956</a>; 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, <a href="https://doi.org/10.1029/2010JD015452">https://doi.org/10.1029/2010JD015452</a>; Ren, G.Y., et al., 2015: An integrated procedure to determine a reference station network for evaluating and adjusting urban bias in surface air temperature data. J. Appl. Meteor. Climatol., 54, 1248–1266, <a href="https://doi.org/10.1175/JAMC-D-14-0295.1">https://doi.org/10.1175/JAMC-D-14-0295.1</a>; Wen, Kangmin, et al., 2019. Recent surface air temperature change over mainland China based on an urbanization-bias adjusted dataset, Journal of Climate, 32: 2691-2705. DOI: 10.1175/JCLI-D-18-0395; Fujibe, F., 2011: Urban warming in Japanese cities and its relation to</p>	Rejected. The text specifically states (P39 L2-3) that larger urbanisation contributions occur locally in China. An additional reference (Shi et al 2019) has been added. This does not contradict a global-scale finding.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68775	38	55	39	7	The AR5 conclusion on LULC/UHI (set out from P.188 of AR5) was inconsistent with some of the studies cited in that section. Comments on the SOD directed the IPCC to consider Zhang et al (2010) (1) whose key conclusion was that, taking China as a whole, the annual mean temperature increasing trend induced by the urbanisation effect is 0.076°C/(10 year), accounting for 27.33% of the overall warming (He, Y., Jia, G., Hu, Y. et al. 2013 <a href="https://doi.org/10.1007/s00376-012-2135-3">https://doi.org/10.1007/s00376-012-2135-3</a> ) find 44.1% of warming from 69 Chinese stations attributable to urbanisation). Similarly Fall et al 2010 (2) is cited. This paper shows that as "most of the warming trends that we identify can be explained on the basis of LULC changes, we suggest that in addition to considering the greenhouse gases-driven radiative forcings, multi-decadal and longer climate models simulations must further include LULC changes." Members of this study group have published prolifically on non-GHG forcing effects on the LSAT (e.g. Klotzbach, P. J., R. A. Pielke Sr., R. A. Pielke Jr., J. R. Christy, and R. T. McNider (2009), An alternative explanation for differential temperature trends at the surface and in the lower troposphere, J. Geophys. Res., 114, D21102, doi:10.1029/2009JD011841. Pielke et al: Land use/Land cover changes and climate: modeling analysis and observational evidence, WIREs Clim Change 2011. doi: 10.1002/wcc.144; Fall et al 2011 "Analysis of the impacts of station exposure on the US Historical Climatology Network temperatures and temperature trends (doi:10.1029/2010JD015146.)). As recommended in Mahood et al 2013 Int. J. Climatol. (2013) (DOI: 10.1002/joc.3736) "climate change metrics of LCC should become part of any climate assessment. In addition, there are other metrics to be considered such as the magnitude of moist enthalpy changes, magnitude of the spatial redistribution of land surface latent and sensible heating (i.e. Bowen ratio), the magnitude of the spatial redistribution of precipitation and moisture convergence, and the normalized gradient of regional radiative heating changes (Mahmood et al., 2010). In summary, humans are changing the face of the planet at an accelerated rate and the findings from [land cover change] LCC studies for all spatial scales should be incorporated into developing climate change and variability metrics that address impacts on atmospheric circulations, hydrologic cycles, and water resources." These variables lead to the uncertainties referenced in Brohan et al 2006 (p.3) (doi:10.1029/2005JD006548) and are conventionally accommodated by way of desktop equations. But these, and other, studies demonstrate that such equations are no	Rejected. The reference in the Fall et al 2010 paper to most of the "warming" being associated with LULC changes refers to warming in an observations-minus-reanalysis series, not warming in absolute values; the warming amounts concerned are typically an order of magnitude lower than the overall warming trend and thus have no substantial impact on large-area trends. The text already notes (P39 L2-3) that rapidly urbanising areas such as eastern China have urbanisation signals in local trends which are substantially larger than 10%; as there are already post-AR5 citations for this it is not considered necessary to add additional pre-AR5 citations.
1569	39	1	39	7	It is far easier to show that the issues of urbanization and screens are less important by using a limited number of stations. Back to the Callendar example from his 1938 paper (Hawkins and Jones, 2013). [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The quoted reference contributes to the AR5 finding which is being reiterated here.
1567	39	3	39	3	There are numerous papers on urbanization issues in China. These are quite good, as they look at land-use changes as well as population issues. If you look at more papers the range is from little significance to 'substantial'. Substantial here is over half the warming. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The relatively high degree of urban contribution to warming in China is already referred to. One additional citation (Shi et al 2019) has been added.
58223	39	3	39	3	Li et al., 2013 citation: is this Li et al., 2013a or b? From reference list this looks as though it is supposed to be b. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Initial added to reference.
1571	39	5	39	5	The screen issue needs very good metadata. It may not be fully resolved in Europe. It is largely unimportant after 1910 globally. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. These contribute to the AR5 findings which are being summarised and reiterated here.
37053	39	10	39	10	You are avoiding the elephant in the room, the level of coverage at global and hemispheric levels, across the entire record since 1850. Strangely enough you mention it down on line 35 of this page, so why not here? [ John McLean, Australia]	Rejected. Uncertainties arising from limited sampling are already incorporated in the uncertainty assessments used in this report.
1573	39	10	39	11	There is one station just north of 80N, and one station south of 80S. You seem happy with interpolation from these sites. I wouldn't be. We could try and put more AWSs across Antarctica, and see who is right. There are about 25 long records in the Antarctic south of 65S. The Arctic has loads more. It is better to check infilled datasets and Reanalysis over the Antarctic. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	See response to 10525
10521	39	10	39	12	Need to add caveat along lines - assuming warming in Arctic regions with no observations is also high. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The text at this point is stating that coverage is limited at high latitudes, not discussing the implications of that.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10525	39	10	39	27	More needs to be said about how all the datasets described here use infilling techniques of one kind or another. It does not make that clear at the moment, and the text implies that those datasets that just have coverage constrained to where there is actual observational data are somehow inferior. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A broader discussion of infilling and coverage bias takes place within the expanded cross-chapter box 2.3.
10533	39	10	39	27	Are there good reasons for not including two other global datasets? (Japan Meteorological Agency. Global average surface temperature anomalies, 2019; Yun et al., A new merge of global surface temperature datasets since the start of the 20th century. Earth System Science Data, 2019.) [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. At the time of SOD, the JMA land/ocean data set was not covered by a publication which meets IPCC peer-review requirements, while the Yun et al data set only extends back to 1900 and therefore cannot be used for assessments of changes since the pre-industrial period. Since then the Chinese data set has been extended back to 1850 but uses climatological values in sea ice areas, so is used only for land temperatures (now stated explicitly in the caption of Table 2.5).
105503	39	11	39	11	which, ...2010), [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. It is unclear what change is proposed here.
73437	39	13	39	13	Insert , after 'al.'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
99581	39	15	39	15	After "... polar regions", add the following sentences showing a recent finding: Huang et al. (2017) recently reconstructed a global dataset with improved representation of Arctic Ocean. The new dataset incorporates the available International Arctic Ocean Buoy Program (IABP; Rigor et al., 2000) observational data using the Data Interpolating Empirical Orthogonal Functions (DINEOF) method and shows an increased global and Arctic warming rates compared with those in AR5. References: Huang, J. X. Zhang, Q. Zhang, Y. Lin, M. Hao, Y. Luo, Z. Zhao, Y. Yao, X. Chen, L. Wang, S. Nie, Y. Yin, Y. Xu, and J. Zhang, 2017: Recently amplified arctic warming has contributed to a continual global warming trend. Nat. Clim. Change, 7, 875-880, doi:10.1038/s41558-017-0009-5; Rigor, I. G., R. L. Colony, and S. Martin, 2000: Variations in surface air temperature observations in the Arctic, 1979–97. J. Clim., 13, 896–914 (2000). [ Xiangdong Zhang, United States of America]	Taken into account. This section of text focuses on the operationally updated global products used in the core GMST assessment, so the cited reference is out of scope. However, it is relevant to Cross-Chapter Box 3.1 and is included there.
10523	39	15	39	16	How much are reanalysis datasets dependent on model configuration? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Different underlying models and model configurations are a potential source of differences between different reanalysis products.
79233	39	16	39	17	Some more details on the "Berkeley Earth merged product" would be helpful. Is it a new product or is it "BEST"? If it is a new analysis, how does it differ from BEST? [ Martin Stolpe, Switzerland]	Noted. "Merged product" in the context of this paragraph means a combined land and ocean dataset. For Berkeley Earth this combines the BEST (sic) land product with an SST dataset.
79039	39	17	39	17	Berkeley Earth does not provide spatially complete estimates. Spatially the coverage is greater but there are still large gaps especially early on in the record. [ John Kennedy, France]	Taken into account. A broader discussion of infilling and coverage bias takes place within the expanded cross-chapter box 2.3.
35527	39	19	39	25	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Rejected. The use of submitted papers in SOD is within the guidelines for SOD. Only Accepted papers are included in FGD.
54453	39	19		25	Morice et al submitted (similar cases all over the text, figures and captions) [ Maria del Pilar Bueno Rubial, Argentina]	Rejected. The use of submitted papers in SOD is within the guidelines for SOD. Only Accepted papers are included in FGD.
6505	39	20	39	20	"extrapolation" would be a more appropriate word than "interpolation" when referring to spreading values "over reasonable distances into data sparse regions". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Either word could be used here but "interpolation" has been retained (but with "across" rather than "into").

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4567	39	20	39	20	I suggest to use and cite the recent released China Merged Surface Temperature (CMST) both in main text (including Table 2.4 and Figure 2.11B) and Annex_I, which has been developed by combining the CLSAT and ERSST v5 (Yun et al., 2019 in ESSD), and papers on the updated GMST trend evaluation have been accepted by Science Bulletin in May 2020 and submitted to Climate Dynamics in Jan 2020. We have updated the dataset and the GMST trend evaluation for the periods of any section in 1850-2019 now. The updated datasets will get the doi in several weeks in PANGAEA database. Ref: 1) Yun X, B Huang, J Cheng, W Xu, S Qiao and Q Li, 2019, A new merge of global surface temperature datasets since the start of the 20th Century, Earth System Science Data, 11, 1629–1643, DOI: <a href="https://doi.org/10.5194/essd-11-1629-2019">https://doi.org/10.5194/essd-11-1629-2019</a> 2) Li Q, Sun W, Huang B, Dong W, Wang X, Zhai P and Phil Jones, 2020, Consistency of global warming trends strengthened since 1880s, Science Bulletin, accepted. 3) Li Q, Yun X, Huang B et al, 2020, An update evaluation of the global Mean Surface Temperature trends based on CMST, Climate Dynamics, in review [ Qingxiang Li, China]	Taken into account. The CMST data set is included in the discussion. It is excluded from the core assessment of GMST changes from 1850-1900 to the present because of its use of climatological values in sea ice areas, but is used as part of the land assessment.
23861	39	22	39	25	It is inconceivable that so many cited unpublished papers are considered in this Report. Although I saw a rough, clumsy and scientifically unfair, i.e., not peer review answer on the bulk of such comments, I strongly disagree that the authors of this Report may take the liberty to mostly cite themselves prior to being near-by published. This is a bad practice. [ Branko Grisogono, Croatia]	Rejected. The use of submitted papers in SOD is within the guidelines for SOD. Only Accepted papers are included in FGD.
23863	39	22	39	70	Since it is difficult to write in and save easily, including simple corrections, into this document, I should like to comment more [ Branko Grisogono, Croatia]	Not applicable. This comment does not refer to anything specific.
79041	39	23	39	23	"random uncertainties" is ambiguous. Are the errors independent, partially-independent, locally-correlated? [ John Kennedy, France]	Noted. Random uncertainties in this context are those which are not systematic. A discussion of the nature and structure of these uncertainties is not necessary here.
79019	39	24	39	25	Morice et al. is not benchmarked against test cases. [ John Kennedy, France]	Accepted. Reference has been removed.
10527	39	25	39	27	Without knowing what the observations would give if we had full coverage we can't be as confident as is being stated. Also previous datasets did not ignore "data void regions", as there was no data there to ignore! Rephrase. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rephrased to "regions with limited data".
10529	39	25	39	27	We found (Jones and Kennedy, Journal of Climate, 2017) in a detection and attribution analysis that infilling techniques had a disproportionate influence on attributed trends, despite no extra observational information being included. This could be referred to as a cautionary example of the use of infilling techniques in observational datasets. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reference added to revised cross-chapter box 2.3 in the context of remaining uncertainties with infilling.
6507	39	25	39	27	This general statement is endorsed. However, the spatial scales of some surface temperature variations are small. Using broad structure functions to spread temperature anomalies over Arctic seas has limitations. As discussed in comments 86 and 87, such spreading from land to sea is inappropriate in summer because of the constraints on the surface air temperature over sea ice and over open water where ice has recently melted. And in winter, the largest anomalies occur where open sea occurs over a region that was ice-covered in former years. Infilling methods cannot capture this fully. For example, the infilling approach of Cowtan and Way has been shown to overestimate Arctic warming in summer and underestimate it in winter, compared with ERA-Interim. There is published peer-reviewed literature that makes these points, and a sentence that counterbalances the positive general statement would not go amiss. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Issues with temperature analyses over sea ice are discussed in detail in the revised cross-chapter box 2.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104551	39	29	39	29	The impact of low temporal sampling on the estimate of global trend should be clarified after incomplete data coverage issue. As such, text would be added at the beginning of this paragraph: 'Zhou and Wang (2016) reconstructed highly-sampling global land mean air temperature using the observations at 0, 6, 12 and 18UTC and revealed an underestimation of recent warming trend. This provides an additional source for the underestimation of recent warming hiatus, besides incomplete spatial sampling (Karl et al., 2015).' References: 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. Karl, T. R., and Coauthors, 2015: Possible artifacts of data biases in the recent global surface warming hiatus. Science, 348, 1469-1472. [ Chunlüe Zhou, United States of America]	Taken into account. This is indeed a potential additional source of uncertainty, but the paper cited in the comment does not appear to account for differences in data coverage between the data sets it compares, making the comparison of limited value in this context.
37055	39	29	39	29	Wrong again. It is not the Global Mean Surface Temperature that you are dealing with, it is the Global Mean (Surface Temperature anomaly) - the parentheses added to show how the wordsa should be grouped. [ John McLean, Australia]	Rejected. GMST is conventionally defined as an anomaly.
10531	39	29	39	35	It would be useful to describe what uncertainties are included in the ensembles and what are not. e.g., are correlated measurement and sampling errors included (Morice et al, JGR, 2012)? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Readers interested in this level of detail are referred to the cited papers.
6509	39	30	39	30	HadCRUT5 is by no stretch of the imagination a longstanding product. It is very new - it has yet to be released generally at the time I am writing this - and it differs from earlier versions of HadCRUT in that it is extended spatially away from regions where observations are used. What is longstanding is the experience of the data providers. So the sentence should be reformulated. It could, for example, begin "Three products from centres with longstanding experience ...". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is worded to make it clear that the "longstanding" refers to the general products, not the specific versions (all of which are recent).
79043	39	33	39	33	"random uncertainties" is ambiguous. Are the errors independent, partially-independent, locally-correlated? [ John Kennedy, France]	Noted. Random uncertainties in this context are those which are not systematic. A discussion of the nature and structure of these uncertainties is not necessary here.
6511	39	34	39	35	I don't have access to HadCRUT5, but for HadCRUT4 the ensemble has larger spread for the latest three decades than for 1961-1990. That's because all values are anomalies relative to 1961-1990. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is reworded to make it clearer that the main improvements in uncertainty were over the earlier part of the record.
37057	39	37	39	38	I can reanalyse temperature data too but would you cite that? The issue is not that a reanalysis has been undertaken but whether we should have any greater confidence in the reanalysed data than in the previous interpretation/processing of the data. [ John McLean, Australia]	Rejected. The reviewer has not, to our knowledge, submitted a reanalysis for publication by the SOD deadline. The comment also appears to indicate a misunderstanding of what a reanalysis is.
1577	39	37	39	49	Surely it's better to emphasise the consistencies between datasets (Conventional, infilled, Reanalysis) than harp on about what might be minor issues. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The issues described here with JRA-55 and MERRA-2 (which are sufficiently significant so as to warrant their exclusion from quantitative parts of this assessment) are stated explicitly to provide justification for their exclusion.
37059	39	37	39	51	There is no such thing as GSAT, nor can there be. What you discuss are temperature ANOMALIES. [ John McLean, Australia]	Rejected. GSAT (and GMST) are conventionally defined as anomalies.
79021	39	39	39	41	It would be good to note here that both ERA-interim and JRA-55 applied some post processing to the reanalysis output in order to get a reliable global temperature estimate for monitoring. [ John Kennedy, France]	Taken into account. The Simmons paper indicates that the corrected version of ERA-Interim is used but gives no such statement for JRA-55. It is considered that this level of detail is best referred to the cited paper.
1575	39	40	39	40	Aren't ERA5 and JRA-55 mostly consistent as they use the same SST fields and sea-ice extents? You've talked so much about the Arctic, it would be useful to show Arctic trends from 1958 and the Antarctic as well. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. ERA5 and JRA-55 use different SST analyses. Regional trends are generally the domain of Chapter 10 through the Atlas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10519	39	40	39	43	Another issue that should be mentioned is the "addressing" of "lack of coverage" generally leads to artificially lower variability in those regions with no direct observations (Jones, Advances in Atmospheric Sciences, 2016, Figure 3.3 in Chap 3 of AR6). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Whilst the matters raised in the 2016 Jones paper are relevant to patterns of spatial variability, and temporal variability on short (interannual) timescales, neither are assessed in this section so the Jones 2016 paper is not relevant here.
79023	39	43	39	45	Nor does the earlier version of COBE-SST deal with changes in the ship data associated with changing engine room biases. [ John Kennedy, France]	Taken into account. Have used "issues such as" to indicate that the quoted example is not exhaustive.
105505	39	45	39	45	delete extrafull-stop [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
127015	39	45	39	45	There are two dots in this sentence. [ Trigg Talley, United States of America]	Editorial. Copyedit to be completed prior to publication.
29835	39	45	39	45	Typo in "...". [ Hernan Edgardo Sala, Argentina]	Editorial. Copyedit to be completed prior to publication.
43075	39	45			Read "recent transition towards buoy SST measurements." rather than "recent transition towards buoy SST measurements.. " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Copyedit to be completed prior to publication.
54455	39	45			measurements.. (there is an extra point that should be deleted) [ Maria del Pilar Bueno Rubial, Argentina]	Editorial. Copyedit to be completed prior to publication.
90313	39	45			double periods .. [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
105507	39	49	39	49	data-sparse -inconsistent with earlier text [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text reworded to make it clearer that the "potential" refers to contributing to global-scale temperature assessments (the observations themselves are already being made).
489	39	49	39	49	Saying that satellites "have the potential to monitor data-sparse areas" seems to be too suggestive of something in the future. I suggest changing it to something like "can monitor traditionally data-sparse areas just as readily as traditionally data-rich ares. [ Claire Parkinson, United States of America]	Taken into account. Text reworded to make it clearer that the "potential" refers to contributing to global-scale temperature assessments (the observations themselves are already being made).
6513	39	54	39	54	To avoid confusion with land surface temperature, "Land areas as a whole have" could be changed to "Temperatures over all land areas have increased on average by" and it would be better in the next line to change "SST warming" to "SST increase". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text reworded.
113639	39	55	39	55	"higher than the SST warming of 0.89 °C" -- according to the Table 2.4 it's 0.91 °C not 0.89 °C, right? [ Agnieszka Kowalczyk, Poland]	Accepted. These values are reconciled in the final text (in which the 2009-2018 values have been superseded).
6515	39	55	40	1	There is reference here to "The five conventional datasets" being in "high agreement". This raises several questions. The first is why "The five"? Why is the JMA dataset not included? This would introduce diversity, as it uses a different SST. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The criteria for inclusion of data sets are now stated explicitly in the caption of the Table (JMA is excluded due to the lack of pre-1890 data and the lack of a peer-reviewed publication). There is now an ERSST-based data set (NOAA).
6517	39	55	40	1	The second question is why do the five include Cowtan and Way as well as HadCRUT5. Cowtan and Way's dataset is a spatially extended version of HadCRUT4. HadCRUT5 is an updated version of HadCRUT4 which has spatial extension built in. So is not HadCRUT5 to be regarded as an updated version of the Cowtan and Way dataset? Multiple versions of NOAA GlobalTemp and GISTEMP are not included. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Cowtan and Way and HadCRUT5 use the same source data but apply different methods to extend its coverage (analogous to the distinction between NOAA and GISTEMP, which both use the same underlying GHCN and ERSST data)
6519	39	55	40	1	The third question is why is there no mention of the fact that the other four datasets are not fully independent. GISTEMP and NOAA Global Temp use the same SST analysis. Berkeley Earth uses an SST analysis from the Hadley Centre. These and other commonalities make the high agreement between these datasets a less convincing result than it would otherwise have been. This should be made clear. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. A new paragraph and table is included which considers the independence of the data sets.
37061	39	55	40	2	This sentence is so banal it is not worth stating. It would however be quite remarkable if five datasets that draw on the same temperature data were *not* in agreement. [ John McLean, Australia]	Rejected. The data sets do not all draw on the same data (although there is substantial overlap).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37063	39	55	40	4	This ignores the very significant question of whether temperature data has been correctly adjusted. If the methods described by the WMO, or methods derived from them, are used then it is most unlikely that this is correct. (see my comments for page 38 line 55 to page 39 line 3) [ John McLean, Australia]	Rejected. No evidence is presented in support of this assertion.
115981	39		39		Please link the statements on urbanization to the assessment in chapter 10 and chapter 10 related FAQ (I suggest to add a statement on the fact that urban heat island has very limited effect on estimates of global temperature change in the FAQ building on this paragraph here). [ Valerie Masson-Delmotte, France]	Accepted. Cross-reference to Box in chapter 10 added.
115983	39		39		Please compare LSAT here to SRCCL. [ Valerie Masson-Delmotte, France]	Taken into account. In SOD the findings of previous reports (including the SRCCL finding on land temperatures) were reported at the start of the overall temperature section on p32, but with the section reorganisation these are now reported at the start of the instrumental section.
37065	40	2	40	4	What is it that has been warmer over the four years? If it's global average temperature anomaly then the comparison to 1850-1900 is nonsense because according to HadCRUT4 annual average global coverage during that period ranged from 14.3% to 48.1% (it exceeded 50% in just 3 of the 612 months!) and the average temperature anomalies for the 1860's and 1870's in particular are skewed towards European and the shipping route to Indonesia and nearby because these are where much of the data was from. See chapter 4 of McLean (2018), "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" for details. [ John McLean, Australia]	Rejected. Uncertainties arising from limited sampling are already incorporated in the uncertainty assessments used in this report.
6521	40	6	40	11	The opening half of this paragraph is very interesting, but rather at odds with other things in the SOD. Firstly, if the targets of the Paris Agreement are predicated upon AR5 estimates of change since the late 19th Century, why did SR1.5 work on the basis of a different definition of the warming since 1850-1900? SR1.5 uses four not three datasets and updated versions that were not available at the time of AR5, although it gives much the same result, as noted in comment 3. Secondly, why does Chapter 4 present the projected date of reaching the 1.5°C level using an even newer and more different estimate of the change from 1850-1900 to the recent past? Please also see my first few comments on the entire report. As I stated there, it is still important to have improved estimates of warming from the pre-industrial to the present such as discussed here in Chapter 2, as this affects estimates of loss and damage, as well as providing data for testing climate models. But it is even more important to have a clear and sensible interpretation of the targets of the Paris Agreement. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. These matters are discussed further as part of the expanded scope for cross-chapter box 2.3. Chapter 4 has substantial input into this box.
37067	40	6	40	14	This whole paragraph about changes from 1880 to 2012 (a) ignores the great difference in global coverage over the period, (b) ignores the change in the number of reporting weather stations, (c) ignores the virtually certainty of incorrect temperature adjustments, (d) applies linear trends when the influence of CO2, which you blame elsewhere for the warming, has a logarithmic influence on temperature, (e) assumes, without any justification, a change of 0.85C by 2012 and refuses to accept that that it might be incorrect and (f) fails to mention that a trend can only be extrapolated if every factor that contributed to the trend is accurately known, how they will change in future is accurately known and how they will interact is accurately known. [ John McLean, Australia]	Rejected. Uncertainties arising from limited sampling are already incorporated in the uncertainty assessments used in this report.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89859	40	6	40	17	Now this is one crucial paragraph. I'm glad it's in here, but perhaps it deserves its own little box. I recommend adding something along the following lines at the beginning: Owing to the outlined substantial improvements in the availability and the processing of the instrumental data since AR5, earlier warming estimates have been revised and with it, those estimates provided in AR5. The Structured Expert Dialogue, [...]. Interestingly, here it is clearly stated that the targets of 1.5°C and 2°C, quote "will not be changed even if the estimates are modified, [...]" This includes the change from GMST to GSAT, as stated further down, quote: "Additionally accounting for effects of GMST versus GSAT (Cross-Chapter Box 2.3) would further modify this number to 0.99 (0.71-1.23) °C." [ Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Cross-Chapter Box 2.3 has been expanded to incorporate these matters and in general discuss the various influences on the different assessments of global temperature change used for the SED and AR5, and those in AR6.
89861	40	6	40	17	Continued: How is that consistent with the discussion above re GSAT, where it's policy relevance is stressed a few times? What I read in this paragraph is that it isn't relevant. Rightly so! Either I misinterpret what is said earlier, or some additional clarification might have to be added here. It goes without saying, that I strictly agree with the notion herein, quote: "While recognising that the targets of 1.5 and 2°C are predicated upon the assumption of 0.85°C change by 2012, [...]" [ Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Cross-Chapter Box 2.3 has been expanded to incorporate these matters and in general discuss the various influences on the different assessments of global temperature change used for the SED and AR5, and those in AR6.
58167	40	6	40	17	maybe address more on the importance of the new estimates and insights for future applications [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. This is incorporated in the expanded cross-chapter box 2.3.
112575	40	6	40	17	This is a very important paragraph, in particular the recognition that the targets of 1.5°C and 2°C are predicated on the assumption of 0.85°C change by 2012, and that this 0.85°C referred to the increase in GMST, not GSAT. The logical corollary is that, to be relevant to the Paris temperature goals, findings should be expressed in terms of GMST changes relative to the decade 2006-2015, which is approximately centred on 2012. I expect you will get many passionate requests to delete this paragraph, but it is clearly true and highly policy relevant, and at the very least a much less convoluted interpretation of global temperature change as referred to in the Paris Agreement than many of the alternatives proposed. [ Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The Paris Agreement is no longer explicitly referred to in the expanded cross-chapter box 2.3, but there is discussion of 1.5 C warming levels.
52773	40	6	40	22	It would be helpful to include here and in Table 2.4 a comparison with a newer product of Kadow et al 2020 that that uses image reconstructions to get an infilled dataset. (see Kadow et al. 2020 Nature Geoscience (in press) Artificial intelligence reconstructs missing climate information. [ Katarzyna Tokarska, Switzerland]	Taken into account. We are now aware of the Kadow et al product (which was not available to us at the time of SOD) and it is included in the assessment for FGD.
43077	40	8			Read "trend metric to the current versions of the NOAA" rather than "trend metric to the then current versions of the NOAA" [ Cyriaque Rufin Nguimalet, Central African Republic]	Rejected. "Then current" is correct as it refers to the versions which were current as of the date of the SED.
102739	40	9	40	12	On what basis is it accurate to say that "the targets of 1.5°C and 2°C are predicated on the assumption of 0.85°C change by 2012"? Consider removing unless this can be demonstrated. [ Philippe Tulkens, Belgium]	Taken into account. Text deleted.
30217	40	13			'Cowtan and Way' (2014)? [ Gilles Delaygue, France]	Rejected. Here "Cowtan and Way" is being used as the name of a data set, for which citations are given elsewhere.
10535	40	14	40	15	I think it is over confident to apply the "adjustment" to start with, but more so to think it can be applied in same way to the differently processed, blended, infilled observational datasets. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The model-based studies are predicated on comparing a globally complete GMST and GSAT. Uncertainties arising from infilling are incorporated into the broader uncertainty assessment underlying the quoted numbers.
24369	40	16	40	17	"The dominant factor is thus new insights into the historical observational record and the provision of an enhanced set of estimates thereof." This sentence is awkwardly phrased and I really don't understand the message it is meant to convey. [ Owen Cooper, United States of America]	Taken into account. The different components underlying the change in temperature change estimates from AR5 to AR6 are set out more clearly in the expanded cross-chapter box 2.3.
79229	40	20	40	20	Table 2.4: The GMST trend of HadCRUT5 for 1980-2018 appears to be too high. Please check. [ Martin Stolpe, Switzerland]	Accepted. All numbers have been recalculated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79285	40	20	40	27	Here and elsewhere: I suggest to also add the CMST global mean temperature dataset (Yun et al., 2019 "A new merge of global surface temperature datasets since the start of the 20th century") to the analysis: <a href="https://www.earth-syst-sci-data.net/11/1629/2019/">https://www.earth-syst-sci-data.net/11/1629/2019/</a> [ Martin Stolpe, Switzerland]	Taken into account. CMST is excluded from the GMST assessment because of its use of climatological values in sea ice areas, but is used in part of the land assessment.
34839	40	20	41	1	The SOD Table 2.4 provides crucial evidence of the distortion in global land temperatures due to the Urban Heat Island (UHI) and other local effects. Please see general comment #1 above. [ Jim O'Brien, Ireland]	Noted. The reviewer proposes no amendment to the text.
10537	40	20	41	2	Where datasets have been provided as ensembles, the uncertainties should be calculated by sampling those. Using a AR(1) process on trends is implicitly assuming a basic form of internal variability (ignoring longer timescales), and so is in effect providing a detection result, which I don't think is what is intended. See Jones and Kennedy, Journal of Climate, 2017. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The primary assessment of change now uses differences between period means and incorporates ensemble estimates of uncertainty where available.
10539	40	20	41	2	I strongly recommend trying to find some sensible names and version numbers for "Cowtan-way" and "Berkeley Earth". Otherwise there is no way of knowing what version is actually being used. Additionally "Cowtan-Way" is a small spatial extension on HadCRUT(?), this should be reflected in its name in some way. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Detailed information on versions and other supporting information is the domain of Annex I.
10541	40	20	41	2	Some discussion is needed about calculating averages across the datasets. These are not independent datasets. With HadCRUT5 and 'Cowtan-Way' being used, more weight is being put on HadCRUT for instance. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. An unweighted average continues to be used. New text has been included to reflect the fact that the data sets are not fully independent.
87917	40	21	40	27	An OLS trend with an AR1 correction is quite inadequate here. Among other problems it contradicts claims in the AR5 attribution chapter that the surface temperature record is I(1). At the least you should use a more robust autocorrelation-consistent estimator such as Vogelsang and Franses (2005) Testing for Common Deterministic Trend Slopes, Journal of Econometrics 126 <a href="https://doi.org/10.1016/j.jeconom.2004.02.004">https://doi.org/10.1016/j.jeconom.2004.02.004</a> . [ Ross McKittrick, Canada]	Rejected. The use of ordinary-least-square linear trends with AR(1) correction has been used as a chapter-wide standard. For variables whose changes are clearly not linear in time, the change in means between two periods is used in preference to trends
93463	40	21	41	2	Table 2.3 (1) 1850-1900 baseline: Like SR1.5, CR2020 includes NASA GISTEMP and NOAA GlobalTemp in all metrics. CR2020 matched NOAA and NASA to the average of HadCRUT4, Berkeley Earth and Cowtan & Way over 1880-1900 to create a pseudo 1850-1900 baseline. Since it appears that temperature estimates relative to 1850-1900 will be used as the main "headline" GMST/GSAT estimate, as well as for all follow-on analysis (e.g. remaining carbon budget), omission of the two datasets effectively excludes them from this assessment. [ David Clarke, Canada]	Taken into account. A version of NOAA GlobalTemp which extends back to 1850 is now available and is included.
93465	40	21	41	2	Table 2.3 (2) GMST rise estimation: AR5 ch 2 Box 2.2 outlined severe problems with long term linear OLS trends and presented an alternative in the form of smoothing spline, but nevertheless retained AR5 OLS trends for the main estimate. To replace the IPCC period (1850-1900 to 2010-2019) method, CR2020 propose a "baseline" LOESS method 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 1.14C for three full global datasets (Berkeley, Cowtan & Way and NASA GISTEMP) versus 1.05C OLS trend over 1880-2018. LOESS trend over 1880-2019 gives a similar result. LOESSbsln has several advantages over the period method, including robust statistical uncertainty and more intuitive interpretation. LOESSbsln also outperforms the period method when validated against longer 20 or 30 year averages in both observations and large model ensembles. [ David Clarke, Canada]	Taken into account. The primary assessment of change now uses differences between period means.
93467	40	21	41	2	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 = ~7.5. [ David Clarke, Canada]	Rejected. The use of ordinary-least-square linear trends with AR(1) correction has been used as a chapter-wide standard. For variables whose changes are clearly not linear in time, the change in means between two periods is used in preference to trends



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93469	40	21	41	2	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 (CR2020). 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. The use of ordinary-least-square linear trends with AR(1) correction has been used as a chapter-wide standard. For variables whose changes are clearly not linear in time, the change in means between two periods is used in preference to trends
93471	40	21	41	2	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 1980-2019 in any case. An additional column, "1850-1900 to 2019" should be added to the right of "1850-1900 to 2010-2019". This would show the non-linear trend 1880-2019 but relative to the 1850-1900 baseline, rather than the rise from 1880 to 2018 per se as explained in CR2020. This metric (converted as necessary to GSAT) would be more intuitively applicable than the 2010-2019 estimate in certain cases, an obvious example being the remaining carbon budget calculation in Chapter 5. [ David Clarke, Canada]	Rejected. The use of ordinary-least-square linear trends with AR(1) correction has been used as a chapter-wide standard. For variables whose changes are clearly not linear in time, the change in means between two periods is used in preference to trends
93473	40	21	41	2	Table 2.3 (7) Spatial coverage:CR2020 defines quasi-global coverage as 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 "quasi-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. It is not clear that HadCRUT5 meets this criterion; this should be carefully evaluated. NOAA GlobalTemp does not meet the quasi-global definition. [ David Clarke, Canada]	Taken into account. Completeness criteria are now explicit in the caption to Table 2.4.
1579	40	22	40	26	Some of the numbers in this Table seem wrong. Looki at the trends for 1980-2018. Maybe some haven't been properly updated. HadCRUT5 now has by far and away more warming than the others - including Cowtan and Way. For HadCRUT5 one of GMST, Land or SST is wrong. Similarly for some of the others. Shouldn't SST trends for NOAA and GISTEMP be similar as they are using the same SST dataset? [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The values in the table have been recalculated (the SOD value was indeed in error).
24371	40	22	40	26	There are two items in the Table 2.4 caption that I don't understand: 1) The caption says that it reports GMST and GSAT, but when looking at the entries in the table, they only show GMST. Where are the GSAT values? 2) The caption states that GISTEMP and Berkeley Earth SST values are not shown separately because they are the same as the NOAA values. However, the table does indeed show the GISTEMP SST values, but they are not the same as the NOAA values. [ Owen Cooper, United States of America]	Accepted. These are both errors in the caption - it is Cowtan/Way (not GISTEMP) for which no SST data are reported, and there are no longer GSAT data in this table.
24373	40	22	40	26	In Table 2.4 the HadCRUT5 GMST increase for 1980-2018 is reported as 0.97 degrees. This is much higher than the other estimates, and it doesn't seem consistent with a land increase of 1.03 and a SST increase of 0.61 (the SST value should dominate of the land value). Is this an error? [ Owen Cooper, United States of America]	Accepted. The values in the table have been recalculated (the SOD value was indeed in error).
35941	40	22	40	26	Like Ch3, Ch2 uses the word "trend" to mean differences/anomalies between two time periods. We should probably sharpen the language and say "anomaly" or something similar, and reserve trends to quantities having units of yr-1. [ Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The table caption has been edited to make it clear that this is a total change over the stated period.
112577	40	22	40	26	Given its prominence in the Structured Expert Dialogue and AR5, the table could very usefully also include the linear trend 1880-2012 [ Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is covered as part of the expanded cross-chapter box 2.3.
6523	40	22	40	27	Comments 95 to 97 above apply to Table 2.4. Please also see comment 209 on Chapter 9 concerning apparent inconsistencies in values for the SST trend, and comment 249 on Chapter 12 on the same point. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Unclassifiable. As the order of review comments has not been preserved it cannot be determined what this refers to.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6525	40	22	40	27	It is furthermore very disconcerting to see in Table 2.4 that HadCRUT5 gives a temperature change from 1980 to 2018 that is such an outlier, with a central estimate of 0.97°C for GMST that is far from the range of 0.63-0.74°C derived from all other datasets shown. As noted in comment 5 on the entire report, the ERA5 and JRA-55 analyses give GMST changes of 0.71°C and 0.65°C respectively, values which serve only to emphasize what an outlier HadCRUT5 is. Yet it is HadCRUT5 that is singled out to be used for the maps of trends shown in Figure 2.11, and it is HadCRUT5 that is used as "observations" in key figures in Chapter 4. The discrepancy (if not the result of a simple miscalculation for HadCRUT5) needs careful discussion in Chapter 2, and if convincing evidence cannot be produced that HadCRUT5 is superior to all the other datasets, then results from multiple datasets need to be shown where only HadCRUT5 is shown at present. And other figures in the SOD that show HadCRUT4 should not be updated to HadCRUT5 for the FGD without careful consideration. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The values in the table have been recalculated (the SOD value was indeed in error).
6527	40	22	40	27	Also with regard to Table 2.4, how are ice-covered seas treated? [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Issues associated with temperature over sea ice are discussed more specifically in the revised text for FGD (in cross-chapter box 2.3).
37069	40	22	40	27	These confidence ranges are meaningless because they assume that whatever data was incorporated into the processing was correct and they implicitly assume that coverage was homogenous, but neither assumption is true. McLean (2018), "An Audit of the Creation and Content of the HadCRUT4 Temperature Dataset" showed more than 70 areas of uncertainty about the HadCRUT4 data, many of which will not be correctly handled by a simple confidence limit derived from statistics about the data that one has. [ John McLean, Australia]	Rejected. The additional uncertainty arising from limited spatial coverage, particularly in the early part of the record, is already incorporated into the uncertainty assessment here. McLean 2018 identifies a number of issues at the individual data point level but presents no evidence that these have a material systematic impact on global-scale means.
108301	40	22	41	0	Table 2.4 is difficult to understand. Unit of trend is not °C. [ Won-Tae Kwon, Republic of Korea]	Rejected. The caption states that the trend is expressed as an overall change over the whole period so degrees C is the correct unit, not degrees C/year (or decade).
89455	40	22			This table should also include HadCRUT4 for comparison and line of sight to the AR5. [ Carl-Friedrich Schleussner, Germany]	Taken into account. An assessment of the contribution of changes in data set versions and linkages to AR5 findings is part of the expanded cross-chapter Box 2.3.
42885	40	22			What are the units for the trend columns? I assumed degrees per century because I expect trends to have units of per time? But then it says they are the total over the stated period. Anyway whatever it is should be explained more clearly -so if these are actually differences you should clarify that. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The caption states that the trend is expressed as a total change over the full trend period so degrees C is the correct unit, not degrees C/year (or decade).
491	40	23	40	27	Since lines 23-24 make it clear that the values listed are the total change over the period, not "trends", the headings for the last three columns of the table should replace the word "Trend" by "Increase". [ Claire Parkinson, United States of America]	Taken into account. It is now explicitly stated in the caption to table 2.4 that the values are total estimated change over the period.
29837	40	24	40	26	Please, check consistency between the sentence "Sea surface temperature (SST) changes are not shown separately for GISTEMP and Berkeley Earth as these use the same underlying SST data sets as NOAA GlobalTemp and HadCRUT respectively." in the legend of Table 2.4, and the data shown in the corresponding table. [ Hernan Edgardo Sala, Argentina]	Accepted. This is an error in the caption - it is Cowtan/Way (not GISTEMP) for which no SST data are reported.
115985	40		40		The paragraph on the SED needs to be rewritten consistent with the CCB with more clarity. [ Valerie Masson-Delmotte, France]	Taken into account. This section has been absorbed into the expanded cross-chapter box 2.3.
50687	41	5	41	23	Policymakers will inevitably compare the warming to date in this section with the findings of SR1.5. It would therefore be helpful to compare the most recent estimate of warming 1.06C of GMST up to 2009-2018, with the equivalent figure in SR1.5 (0.87C up to 2006-2015), and explain the main drivers behind this i.e. how much of this is historical revision of data, how much is warming and how much is variability - otherwise it appears as if we have seen nearly 0.2C of warming in only 3 years. This explanation should also then be included in the executive summary and SPM. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is covered in the expanded cross-chapter box 2.3.
29839	41	6	41	10	At the end of this paragraph, I suggest including "(see Cross-Chapter Box 2.1, Figure 1)" for visual reference. [ Hernan Edgardo Sala, Argentina]	Accepted; added call-out to Cross-Chapter Box 2.1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69165	41	6	41	23	Inserting "In summary" at the top of the paragraph, like the other sections in 2.3.1. is suggested. [ Kaoru Magosaki, Japan]	Rejected. This is implied by the section heading "overall assessment".
58169	41	6	41	23	summarize with a clearer timeline as former sections (i.e. deep past, post-glacial, instrumental period) would be better [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Paleoclimate findings have been removed from this section and are now addressed earlier.
93067	41	9	10		Considering the significant uncertainties in the LIG temperature reconstructions presented in Section 2.3.1.1.1, as noted in previous comments for that section, a more appropriate estimate of LIG GMST is 1.0 +/- 1.0C. [ Bette Otto-Bliesner, United States of America]	Taken into account; the midpoint of the reviewer's estimate (+1C) is now equal to the midpoint of the AR6 assessment. The suggestion that the uncertainty should be 2X the AR5 uncertainty is inconsistent with the substantial progress since AR5. On the other hand, the AR6 uncertainty ( $\pm 0.5C$ ) is greater than Sock's $\pm 0.25$ uncertainty.
73439	41	9	41	9	The Cenozoic is not 'deep past'. Better to quantify. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; 'deep past' omitted from revised section.
105091	41	9	41	10	The LIG is characterised by large changes in seasonality, which should be mentioned here. The global average flattens this message out. As a general message, work on the seasonal cycle and winter/summer temperatures should also be summarised here. (I am sorry, it might be touched on elsewhere, but I haven't seen it yet if this is the case). The seasonal cycle is touched upon for the sections on the circulation and precipitation, but for temperatures if I am not mistaken. [ Masa KAGEYAMA, France]	Rejected; CH2 focuses on GMST for the paleo reference periods, and only based on proxy observations. Discussion of LIG seasonality is largely informed by model simulations; little is known about winter from proxy evidence.
42887	41	9	41	10	As with my previous comment, I think the LIG value, or at least its uncertainty, needs another look. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; land-sea contrast factor used to translate global SST to GMST has been re-examined.
30219	41	9			'to 1850' (space between words) [ Gilles Delaigue, France]	Accepted, fixed.
30221	41	12	41	13	I wonder whether the conclusion here (L.12-13) could not be more likely, given the result of PAGES 2K Consortium (2019), quoted p.34 L.11-13: "the rate of increase directly observed during the second half of the 20th century exceeded the 99th percentile of any 51-year trend over the past 2 kyr", Suggestion: "at an observed rate unprecedented for any 50-year period in at least..." [ Gilles Delaigue, France]	Rejected; while PAGES 2k analysis and database are recent and authoritative, they have limitations beyond those represented by the "99th percentile" estimate. Another comment (4521) suggests instead to downgrade from "medium" to "low" confidence. Accepted revised wording, as suggested.
45299	41	12	41	13	I'm a little bit surprised that there is only medium confidence the GMST rate of increase is unprecedented in at least the last 2ka. My understanding is all the global reconstruction studies thus far have found an unprecedented rate of GMST increase in recent years, even though the baseline is slightly different (the most 30 years vs 50 years) [ Anson Cheung, United States of America]	Rejected; while PAGES 2k analysis and database are recent and authoritative, they have limitations beyond those represented by the "99th percentile" estimate. Another comment (4521) suggests instead downgrading from "medium" to "low" confidence. Accepted revised wording, as suggested.
8905	41	12	41	14	Comparison of levels averaged over vastly different periods of support (multi-centennial vs decadal) is hard to justify. [ Robert Kopp, United States of America]	Noted; while there are important assumptions, the "justification" is that there is no other alternative. The recent rapid warming makes a centennial average of the most recent century a meaningless metric from a policy perspective.
4521	41	12	41	15	Authors write: "Over the last 50 years, GMST has increased at an observed rate unprecedented in at least the last 2 ka (medium confidence)." It is hard to compare trends from measured and reconstructed datasets. In many parts of the world, warming rates in the ramp-up to the Medieval Climate Anomaly were similar as modern warming rates. Uncertainties with proxy validity, C14 ages and areal representativeness of data downgrade pre-industrial warm phases compared to modern ones. Therefore "medium confidence" is exaggerated and should be replaced by "low confidence". In reality we are far away from being able of making such comparisons in a robust manner. [ Sebastian Luening, Switzerland]	Rejected; Two other comments (30221, 45299) suggested instead that the certainty should be upgraded from "medium" to "high" confidence. While some "parts of the world" might have experienced higher rates than represented by GMST, this statement refers to GMST and is based largely on annually resolved (not C14) records.
37071	41	14	41	20	You repeat your false implications that the 1850-1900 so-called global average anomalies were both accurate and global (see also my comments above for page 40 lines 2 to 4). The data was not global and coverage was not homogenous so your stated figures and their confidence limits are nonsense. [ John McLean, Australia]	Taken into account; the evidence for the 1850-1900 reference period is presented in Cross-Chapter Box 1.2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29841	41	18	41	18	Please check consistency between the temperature shown in this line (0.89 °C) and in Table 2.4 above (0.91 °C). [ Hernan Edgardo Sala, Argentina]	Taken into account. All values have been recalculated with updated data for FGD.
58253	41	18	41	19	I found that there is no univocal definition of “multi-century interval” anywhere. This should be addressed somewhere in the chapter. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; the words "multi" and "century" have non-technical meanings (multi = more than one or many; century = 100-year-long period); these are appropriate to describe the duration of intervals with time uncertainties.
54883	41	18	41	20	Please be explicit about when this multicentury warm period was (mid-Holocene?) to ensure there is no confusion with recent century scale climate warming. [ Nancy Hamzawi, Canada]	Taken into account; timing of warmest interval stated and section revamped so paleo reference periods and recently century are now in different paragraphs.
127017	41	19	41	19	What multi-century period was this? [ Trigg Talley, United States of America]	Accepted; added "sometime around 6 and 7 ka".
4523	41	20	41	21	Authors write: “Mean annual surface temperatures over the Northern Hemisphere (high confidence), and the Southern Hemisphere (low confidence), have decreased over at least the past 6 ka culminating in the LIA).” 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. It would also be importance to acknowledge the Holocene Thermal Maximum (HTM) here that made this Neoglaciation actually possible. During the HTM temperatures were 1-3°C warmer than today in most land areas globally. [ Sebastian Luening, Switzerland]	Accepted; clarified that trend was not monotonic by adding that "... temperature decreased in general, albeit with multi-century variability". Regarding the comment about HTM temperatures: this is taken into account by the earlier statement that this section concerns the GMST during a multi-century period, not a time-transgressive warming that occurred at different times in different places anywhere between 10 and 5 ka.
81335	41	20	41	21	This presumably refers to the 6 ka before 1850? [ Johannes Laube, Germany]	Accepted; added "sometime around 6 and 7 ka".
105509	41	22	41	23	Sentence beginning "Averaged globally..." is not clear and should be rephrased. [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; clarified sentence.
10427	41	22	41	23	Is this correct? Looking at figure 1 in Box 2.1 suggests cooler temperatures at the start of the Holocene, normally considered the post glacial period. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; CCB2.1 Table 1 and Glossary define "post-glacial" as since 8 ka.
52817	41	26	44	16	Again I point out that this "free atmosphere" section is written in a way to promote those indicators of greatest warming without addressing other evidence that warming is not remarkable (one example is the Sherwood reference described in another box). Christy and McNider (2017, the update of Christy and McNider 1994 Nature) show that when the impacts of volcanoes and ENSOs are accounted for, the remaining global lower-tropospheric trend is quite modest - about +0.12 C/decade (using RSS, UAH and ERA5). This point is entirely appropriate for this section as it provides the reader with an estimate of the trend once major, ephemeral natural variations are identified and removed (which is consistent with the discussion of forcing mechanisms also discussed in this chapter). This adjusted value was recalculated and updated for the latest, heavily reviewed, BAMS State of the Climate 2019 report, specifically stating in the near-final version as "Taking into consideration the temporary cooling due to volcanic aerosols caused by eruptions in 1982 and 1991, as well as the El Nino/La Nina cycle, there remains a global warming trend since 1979 of +0.12 +/- 0.04 C/decade unexplained by these ephemeral, natural phenomena (Christy and McNider 2017, updated and calculated using ERA5, RSS, and UAH datasets.)". This is a vital piece of information that would apply to several of the sections and chapters. Additionally, when models depict the warming of the Earth System, the troposphere (especially in the tropics) is the metric with the largest magnitude (see McKittrick and Christy 2018 showing all models have this signature and it is the dominate signature of warming). So, this section should have more words to indicate that models tell us that the bulk-atmospheric temperature, especially in the tropics, is the place to look for responses to extra GHGs. [ John christy, United States of America]	Rejected. The purpose of this section is to document the changes which have occurred - the extent to which they are influenced by natural variability (e.g. ENSO) or forcing (e.g. volcanoes) is the domain of Chapter 3. The Christy and McNider 2017 paper does attempt to quantify this influence over the 1979-2017 period. It should also be noted that surface temperatures, and hence a lower tropospheric-surface comparison, will also be influenced by the same forcing mechanisms. The assessment findings of this section still stand even if the adjusted Christy and McNider 2017 values are used. Comparison of observations with models are outside the scope of Chapter 2.
73441	41	32	41	32	Capital 'T' for 'troposphere (it is used as a proper noun here). [ Burt Peter, 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
30223	41	36			'RAOBCORE/RICH' projects? [ Gilles Delaygue, France]	Taken into account. This text is edited to make it clearer that these are part of the same project (and covered by the same citation)
5355	41	36			RAOBCORE needs a citation. [ Bryan Weare, United States of America]	Taken into account. This text is edited to make it clearer that these are part of the same project (and covered by the same citation)
104553	41	37	41	37	The second sentence would be added: 'A new homogenized radiosonde daily temperature dataset was recently created through 2018 (Zhou et al. 2020).' Reference: Zhou, C., J. Wang, A. Dai, and P. W. Thorne, 2020: A new approach to homogenize global twice-daily radiosonde temperature data from 1958 to 2018. J. Clim., under review. [ Chunlüe Zhou, United States of America]	Taken into account. This dataset has been incorporated in the assessment.
29843	41	37	41	37	The acronym "AMSU" has not been defined in this chapter. Consider including its full meaning at least once. [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
52811	41	37	41	41	The authors will see a theme in my comments on section 2.3.1.2 - the text reads as if the intent is to promote any point that supports model-type warming and dismiss or ignore the evidence for the contrary. With that, the following comments are made for specific points. In line 37ff, the explanation ignores considerable research on dataset quality. In particular, Christy et al. 2018 in a thorough, globally comprehensive comparison, identified spurious warming biases in satellite products (particularly RSS, NOAA and UW) in the NOAA-14 period relative to three sets of radiosondes (VIZ, AUST VAISALA and Global). That Christy et al. is not cited gives the impression of bias in this and related areas. Of note also is the spurious warming shift after 2009 in Vaisala RS92 radiosondes noted in Christy et al. 2018. The documentation from the Vaisala company is poor (deliberately I believe) in "Improved Measurement Accuracy of Vaisala Radiosonde RS92" by Jauhainen, Turunen and Wahrn (corporate document 185/2011). In the temperature profile comparison, the resolution is extremely poor (0.25 °C) but one can see several individual levels that were 0.25C colder in the "old" sonde software vs. the "new" sonde software. The main purpose in this adjustment was to increase the humidity in the tropospheric layers, which obviously had an impact also on temperature. L. Haimberger (U WIEN) tested the Australian sondes and determined the tropospheric levels shifted to warmer values by 0.1 °C to 0.2 °C when Australia implemented the new software. In direct comparison with UAH, RSS and NOAA microwave data, the shift in the MT layer was +0.135, +0.136 and +0.146 °C respectively between 2009 and 2010 (Christy et al. 2018 Fig. 12). The correction for this shift has NOT yet been applied to RAOBCORE and RICH, thus these datasets contain the impacts of this spurious warm shift. The Reanalyses do better as they incorporate the AMSU temperatures during that period which mitigates some of the Vaisala shift. The recommendation here is to rewrite this paragraph to include the evidence from Christy et al. 2018 which is the most comprehensive evaluation of radiosondes and satellites available to the IPCC - especially noting the evidence showing spurious warming in satellite datasets during the NOAA14 period and the warm-shift in Vaisala RS92 sondes which are utilized around the world. Without doing so, the section fails to provide information necessary for analysis and will be vulnerable to dismissal in the sure-to-come rigorously independent assessments of IPCC AR6 content. [ John christy, United States of America]	Taken into account. A reference to residual differences between data sets in the troposphere has been added, using Christy et al 2018 as a reference. The approach of this section is to report each data set individually; all data sets are consistent with the assessment findings of this section. Model-observation comparisons are outside the scope of Chapter 2.
93525	41	40	41	40	A study that showed this result already before Maycock et al. (2018) and should be cited here is McLandress et al (2015) [McLandress, C., Shepherd, T. G., Jonsson, A. I., von Clarmann, T., and Funke, B. (2015a). A method for merging 16 nadir-sounding climate records, with an application to the global-mean stratospheric temperature data sets from 17 SSU and AMSU. Atmos. Chem. Phys. 15, 9271–9284. doi:10.5194/acp-15-9271-2015]. [ Michaela Hegglin, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The McLandress paper was already cited at P43 L10.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73443	41	40	41	40	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
37073	41	41	41	45	You cite reanalyses without any justification of why they should be considered more accurate than the data that they replace. [ John McLean, Australia]	Rejected. In this context, reanalyses incorporate numerous data sources which are not in common with any of the other data sets used for this assessment, including aircraft observations and various remote sensing products.
52819	41	44	41	46	While I have hope for Sherwood's UNSW radiosonde dataset (and have advocated for it to be updated), it is one that hasn't been updated in five years and unfortunately was the worst-performing dataset in terms of inter-comparisons to satellite, other radiosonde and reanalyses. Trendwise, UNSW matched the datasets with the lower trends for the deep tropospheric layer because while the trend at 300 hPa might have increased in the homogenization process, trends at other levels decreased. Please examine the chart Supplement FigS2.10 in the BAMS SotC report for 2016 and you will see where UNSW trends at 300 hPa actually fall - and it is certainly more current than the reference here to 2012! UNSW's updated 300 hPa tropical trend is +0.16 C/decade vs. a surface trend of +0.13 C/decade for 1979-2016 (2016 values were estimated by comparison with RAOBCORE/RICH). More importantly, UNSW's 200 hPa trend is +0.07 °C/decade - much less than the surface trend and in direct contradiction to model results which show the trend at 200 hPa over twice the model surface trend - this fact must be stated in this report (as mentioned, not stating such facts leaves this report exposed to clear allegations of bias of which the IPCC is already known for.) My suggestion here is to be transparent and open with the information that has already been published on vertical trends in the tropics - and to update the supplementary figure on profile trends in the tropics from AR5 using the State of the Climate information. Claiming "low confidence" is not a route to take regarding tropical trends - even with the small observational spread, the value is well below models. Good grief, if Climate Models have a range of a FACTOR of THREE in their ECS's, they should be described as having "No Confidence" if observations are described as "low confidence". Anyway, specifically, the text should point out the very modest warming throughout the troposphere and the huge mismatch with the modeled amplification, especially above 400 hPa as shown in Fig. 3.9. The BAMS SotC Report for 2019 should contain an updated figure (without UNSW because it was not updated) and the story stays the same - models amplify the tropical signal with a highly significant difference. This is important for IPCC AR6 to acknowledge and present - because the truth of this result is certain to be publicized simply because it is real and important. The consequences of hiding such information will not be viewed favorably in any objective venue - I've hit the point several times, so I hope it sinks in. [ John christy, United States of America]	Taken into account. The UNSW data set has been downgraded in the core assessment as it has not been updated beyond 2012. The strongest assessment here is over the 2002-2018 period, which draws heavily on RO evidence. While it is true that the upper tropospheric warming is less in observations than models, this is true also at the surface so does not invalidate a conclusion that observed warming in the upper tropical troposphere is stronger than that at the surface.
73445	41	45	41	45	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73447	41	45	41	45	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
6529	41	46	41	47	ERA5.1 has been finalised, and public data release is imminent. ERA5.1 is discussed briefly (with one figure) in the paper on ERA5 by Hersbach et al., which is referred to earlier in the paragraph as submitted, but has now been accepted. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. ERA5.1 is used in FGD.
115987	41		41		For the overall assessment of surface temperature for past warm periods, missing is to have explicitly somewhere the order of magnitude of polar temperature change, as there are discussions of implications of levels of warming for GMSL, but in response to orbital forcing, large polar warming can occur with small global change in temperature. This is an important aspect which was not well addressed in SROCC and should be here (global T / polar T/ GMSL). [ Valerie Masson-Delmotte, France]	Taken into account. The primary assessment of Arctic amplification is in the Atlas. A cross-reference to this has been added in cross-chapter Box 2.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
52813	42	5	42	17	The use of RO will become important as time goes on for the upper troposphere and stratosphere. However, to discuss this method as applying to a period of less than 20 years in length is improper due to the spike at the end of the time series due to the major ENSO in 2016 (and a modest ENSO in 2019). Climate variations need much more time than 17 years, and the impression to an independent observer here is that the use of RO allows for "cherry-picking" of a period that has a strong warming trend due to the major ENSO in 2016. A Figure like the left panels of Fig. 3.9 is more informative (I have commented on the use of the 1998-2014 period in the right Panel of Fig. 3.9 as inappropriate for the same reason above - 17 years does not give a useful sample size for trend analysis, which in the case of Fig. 3.9 right panel gives the impression of no tropospheric warming, but that is due to the major ENSO in 1998 - the beginning of the period.) [ John christy, United States of America]	Taken into account. The short period is the maximum possible with RO data. The most substantial issue with the short-term trends was that associated with the rare Southern Hemisphere sudden stratospheric warming in 2002, but the occurrence of a comparable event in 2019 alleviated the impact of the 2002 event on short-period trends.
127019	42	7	42	7	What is an 'SI'? [ Trigg Talley, United States of America]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
29849	42	7	42	7	Please include "SI" meaning at least one time. [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
26053	42	7	42	7	It would be useful to have an explanation of "SI traceable" [ Don Alfonso Pino Maeso, Spain]	Taken into account. This is handled through the glossary.
30225	42	7			what means 'SI' here? [ Gilles Delaygue, France]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
73449	42	8	42	8	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73451	42	9	42	9	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
39073	42	9	42	10	Are Steiner 2019 a and b referring to the same work? [ Federico Serva, Italy]	Accepted. References have been updated. There are two separate Steiner et al papers (now Steiner et al 2020 after final publication).
6531	42	10	42	12	GNSS RO provides valuable data on temperature in the upper troposphere and lower/middle stratosphere. Humidity information, mentioned in line 10, relates more to the lower troposphere as I understand it. So does the comment on line 12 that best agreement is found for trends for the region from 8km to 25km apply only to temperature? If so, this should be made clear. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference to temperature added.
54459	42	10		15	Steiner et al., submitted). Check references and text (citations in the text are confuse) [ Maria del Pilar Bueno Rubial, Argentina]	Accepted. References have been updated. There are two separate Steiner et al papers (now Steiner et al 2020 after final publication).
35529	42	15	42	26	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Rejected. These papers were submitted by 31/12/2019 and therefore met criteria for SOD inclusion.
54457	42	15			Danzer et al., submitted (similar cases all over the text, figures and captions) [ Maria del Pilar Bueno Rubial, Argentina]	Accepted. References have been updated.
73453	42	22	42	22	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73455	42	22	42	22	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
105511	42	26	42	26	submitted - full stop and space missing [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
127021	42	26	42	26	This sentence needs a period. [ Trigg Talley, United States of America]	Editorial. Copyedit to be completed prior to publication.
102741	42	26	42	26	A "." is missing in this sentence, [ Philippe Tulkens, Belgium]	Editorial. Copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6533	42	26	42	26	Should the reference to Ho, submitted, in fact be to Ho et al., doi:10.1175/BAMS-D-18-0290.1? I ask because the second paper includes a figure I was invited to provide that indeed shows how the introduction of RO data coincides with improved consistency between reanalyses in the lower stratosphere and at the tropical tropopause. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The reference was correct, but the other Ho paper referred to by the reviewer is also relevant to this point and a citation has been added. The report referred to in the Ho, submitted reference is no longer cited.
29845	42	26	42	26	Please, separate "submitted)The". [ Hernan Edgardo Sala, Argentina]	Editorial. Copyedit to be completed prior to publication.
113113	42	26	42	26	Correct 'd)Th' [ Diego Miralles, Belgium]	Editorial. Copyedit to be completed prior to publication.
30227	42	26			period after the parenthesis, before 'The effective' [ Gilles Delaygue, France]	Editorial. Copyedit to be completed prior to publication.
43079	42	26			Read "middle stratosphere (Long et al., 2017, Ho, submitted). The effective vertical resolution" rather than "middle stratosphere (Long et al., 2017, Ho, submitted)The effective vertical resolution" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Copyedit to be completed prior to publication.
90315	42	26			(Long et al., 2017, Ho, submitted)The should be corrected to (Long et al., 2017; Ho, submitted). The [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
73457	42	27	42	27	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73459	42	27	42	27	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
36349	42	35	42	35	Does "truncated below the cloud-tops" mean "entirely omitted below the cloud-tops" or "partly but not entirely omitted below the cloud-tops"? Might readers used to American English vs British English interpret "truncate" differently? [ Curt Covey, United States of America]	Noted. It refers to entirely emitted below the cloud tops. This is standard usage.
58255	42	35	42	39	These sentences are already about trends, therefore shall be moved to the next section. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This section is about the use of trends to define uncertainties, not the size of the trends themselves.
37077	42	41	43	32	This section needs to clearly account for the substantial difference in trends between UAH and RSS (as shown in Table 2.5) in light of them using the same raw data. Data processing is not a democracy where a number of higher trends make a lower trend incorrect. You need to explain the cause of the differences and give good reasons to reject one, because obviously both can't be correct. [ John McLean, Australia]	Rejected. This section reports all available evidence and makes no claims that any one product is preferred, taking the spread between products as an indication of structural uncertainty. The assessment findings would still stand if using the UAH data alone.
73461	42	42	42	42	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
58171	42	42	42	52	describe in an order of layers would be easier for readers to digest (lower troposphere -> mid- to upper- troposphere -> stratosphere) [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This order is already largely in place across this and the succeeding paragraphs, with some minor deviations which reflect linked outcomes in adjacent layers.
87919	42	42	42	52	The warming rate at the 300hPa level over 1979-2012 from Sherwood and Nishant (2015) is not supported in other data products over longer intervals. McKittrick and Christy (McKittrick, Ross R and John Christy (2018) A Test of the Tropical 200-300mb Warming Rate in Climate Models. Earth and Space Science doi: 10.1029/2018EA000401) use RAOBCORE, RICH and RATPAC and find warming of 0.17 +/- .06 C/decade over 1958 to 2017, which drops to 0.14 +/- 0.12 C/decade allowing for a break point at 1979, neither of which is significantly different from the 0.14 near-surface rate. [ Ross McKittrick, Canada]	Taken into account. All available radiosonde data are now used for this assessment (including the new SUNY dataset). It should be noted that (a) the trend cited in the comment is not directly comparable with that in SOD as they are for different periods and (b) the relevant comparison here is between the observed upper-tropospheric temperature trend and the observed surface trends, not a comparison with models which was the primary focus of the McKittrick and Christy paper.
65075	42	42	43	32	Why has the stratosphere cooling seemingly ceased, while the greenhouse effect is enhanced in the last decades? This point should be addressed. [ Magnus Joelsson, Sweden]	Rejected. Attribution of this type falls within the scope of Chapter 3.
68777	42	42	43	32	This section should address the findings of Christy et al 2018 ( <a href="https://doi.org/10.1080/01431161.2018.1444293">https://doi.org/10.1080/01431161.2018.1444293</a> ) that analyses the linear trend in tropospheric temperatures from four satellite data sets against radiosondes. They conclude the "tropical result is over a factor of two less than the trend projected from the average of the IPCC climate model simulations..." - Sean Rush, New Zealand [ sean rush, New Zealand]	Rejected. Observation-model comparisons are the domain of Chapter 3.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6535	42	43	42	44	The statement that radiosonde warming rates are higher than rates from satellite products is not quite right, at least on the evidence presented in Table 2.5. One can see there that it is only one of the satellite products, from UAH, that shows a lower trend for 1980-2018 than the radiosonde data. The RSS satellite product has a 1980-2018 trend that is larger than that for one of the radiosonde data sets and smaller than that for the other. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This wording is reassessed using the full range of data products in FGD (including reanalysis data which were excluded from SOD while the ERA5 lower stratospheric issues were resolved)
6537	42	43	42	44	The same sentence refers to data from reanalyses, but reanalysis data are not included in Table 2.5. Why? There is a corresponding table in the BAMS State of the Climate article for 2018 that shows trends from 1979 to 2018 and does include reanalysis data. The ERA5, JRA-55 and MERRA-2 reanalyses can be seen to have similar lower tropospheric trends. These trends are lower than RSS trends but higher than UAH trends. If reanalyses are not to be included in Table 2.5 but are to be mentioned in the text, a reference to the BAMS SOC article should be added. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reanalyses were not included in the SOD treatment of this area because at the time of SOD finalisation ERA5.1 was not yet available. They have been added for FGD.
73463	42	45	42	45	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
104555	42	47	42	47	Text after '... per decade near the surface.' would be added: 'The latest homogenized data from Zhou et al., (2020) also suggests tropical warming maximum around the 300 hPa level for the 1979-2018 or 1958-2018 periods.' Reference: Zhou, C., J. Wang, A. Dai, and P. W. Thorne, 2020: A new approach to homogenize global twice-daily radiosonde temperature data from 1958 to 2018. J. Clim., under review. [ Chunlüe Zhou, United States of America]	Taken into account. This dataset is now incorporated in the assessment.
30229	42	48			'at almost all latitudes': fig 2.12ab is restricted to 70N-70S [ Gilles Delaygue, France]	Taken into account. Text edited to clarify exclusion of polar regions.
90317	42	48			close space in "upper- troposphere" to "upper-troposphere" [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
73465	42	50	42	50	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73467	42	54	42	54	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73469	42	54	42	55	Delete 'the' and 'period' (to remove the tautology). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The extra terms are not required here to clarify what is meant.
30231	42	54	43	2	I guess if the volcanic influence on the general trend is to reinforce it, it should correspond to a negative trend, not positive (-0.06K per decade)? [ Gilles Delaygue, France]	Taken into account. Text edited to make the sign of the influence here clear.
58225	42	54	43	29	Most datasets presented show no significant trend in rate of lower stratosphere cooling. However new data from over tropical south India region show a strong cooling rate and no sign of weakening trend, with max. rate of 1.3±0.86K/decade. Reference: RavindraBabu, S., Akhil Raj, S. T., Basha, G. and Venkat Ratnam, M. (2020) 'Recent trends in the UTLS temperature and tropical tropopause parameters over tropical South Indian region', Journal of Atmospheric and Solar-Terrestrial Physics, 197, pp. 105164. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. National-scale assessments are outside the scope of this section.
81009	42	54	55	4	Is it possible to give a brief explanation for the reason for stratospheric cooling in this paragraph to assist the reader, or at least provide reference to a sub-section where a fuller explanation is given to this phenomenon? [ Jeffrey Philip OBBARD, Singapore]	Rejected. Attribution is the domain of Chapter 3.
80265	43	1	43	4	The impact of the stabilization of the ozone layer could be accounted for in this paragraph (WMO, 2018). [ Sophie Godin-Beekmann, France]	Rejected. Attribution of this type falls within the scope of Chapter 3.
73471	43	3	43	3	Delete 'the' and 'period' (to remove the tautology). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The additional wording was added for clarity.
73473	43	3	43	4	Delete 'the' and 'period' (to remove the tautology). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The additional wording was added for clarity.
90319	43	3			remove comma in Philipona et al., (2018) [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
73475	43	4	43	4	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, 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
73477	43	6	43	6	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
58227	43	6	43	10	Consider breaking up sentence into: 'Zou & Qian (2016) report cooling in the middle and upper stratosphere with a trend of $-0.58^{\circ}\text{C} \pm 0.17^{\circ}\text{C}$ per decade for the mid-stratosphere and $-0.63^{\circ}\text{C} \pm 0.32^{\circ}\text{C}$ per decade for the upper stratosphere over 1980-2018, although both cooling rates have slowed substantially since the mid-1990s. The overall post-1980 trend is reduced in magnitude by about $0.10^{\circ}\text{C}$ per decade at both levels if the influence of the El Chichon and Pinatubo eruptions is removed (Zou and Qian, 2016).' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
80267	43	6	43	12	Same remark as above. [ Sophie Godin-Beekmann, France]	Rejected. Attribution of this type falls within the scope of Chapter 3 (we assume this is a reference to comment 80265)
35531	43	6	46	7	* C repeats [ Carlos Antonio Poot Delgado, Mexico]	Editorial. Copyedit to be completed prior to publication.
90323	43	6			replace & by "and" in Zou & Qian (2016) [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
73479	43	7	43	7	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
30233	43	8	43	10	I'm not an expert but have the impression from Fig.11 of Zou & Qian (2016) that the high solar cycle at the beginning of the 1980s, followed by cycles with lower and lower amplitude, should have a much stronger influence on the general trend than the small warmings due to both eruptions? Anyway the decrease by $0.1\text{K/dec}$ in the general trend quoted here is due to removing both effects of the eruptions and solar cycles, as written in the legend of Table 3 of Zou & Qian; i think this should be acknowledged here. [ Gilles Delaygue, France]	Taken into account. Reference to the removal of solar cycle effects added to text. It should be noted here that there is an inconsistency in the Zou and Qian paper between the text at the start of section 5, which implies the change is wholly due to the removal of eruption effects, and the Table 3 caption, which as the reviewer states also refers to the solar cycle.
90321	43	10			delete repeat of (Zou and Qian, 2016) [ Jeannine-Marie St-Jacques, Canada]	Accepted. Text has been edited.
73481	43	11	43	11	Delete , after al. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
58257	43	11	43	11	Replace "broadly" with a quantitative measure with an uncertainty range, if possible. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Quantitative values from these studies are not directly comparable to the Zou and Qian data set as they are not updated past 2015/2016, but they reinforce the Zou and Qian results during the periods of common record.
73483	43	14	43	14	Capital 'T' for tropopause (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
5357	43	14	43	22	There needs to be an explanation of the discrepancy between RO and raob trends near 15km. Are the differences dominated by a region or radiosonde type shift? [ Bryan Weare, United States of America]	Taken into account. New text has been added to note the results of the Steiner et al 2020 paper that trends from the latest generation of radiosondes largely matched the RO results (implying that the difference in Figure 2.12 largely arises from as-yet-unresolved inhomogeneities in the larger radiosonde data sets).
90325	43	15			add comma in (Xian and Homeyer 2018) [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
58229	43	16	43	16	Chen et al., 2019 citation: is this Chen et al., 2019a or b? From the reference list this looks as though it is supposed to be b. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Reference resolved
73485	43	17	43	17	Capital 'T' for tropopause (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73487	43	18	43	18	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73489	43	18	43	18	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73491	43	19	43	19	Capital 'T' for tropopause (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73493	43	20	43	20	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, 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
73495	43	20	43	20	Capital 'T' for tropopause (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73497	43	20	43	21	Delete 'over the period'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. 'Over the period' makes it clear that this encompasses the full period, not just the two endpoint years.
19721	43	20	43	22	The most spectacular feature on figure 2.12 seems to be, on the b) subplot, a strong maximum of the temperature trend, centred near 30°S at a 17km altitude, higher than the climatological tropopause. Comments and interpretations whenever possible of this feature should be given. [ philippe waldeufel, France]	Rejected. While this feature is interesting, it is not specifically relevant to any of the assessment findings and is therefore not discussed for space reasons. It appears to be connected to local stratospheric temperature responses to a 2015 volcanic eruption in Chile (Stocker et al 2019, doi: 10.1029/2019GL084396) and is considerably diminished when 2019 data are added.
73499	43	21	43	21	Capital 'T' for tropopause (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73501	43	21	43	21	Replace 'over the period' with 'from'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. 'Over the period' makes it clear that this encompasses the full period, not just the two endpoint years.
73503	43	24	43	24	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
36351	43	24	43	26	"Medim confidence" in this particulaer observation (with "low confidence" in its magnitude) is a fairly tentative conclusion. But doesn't it (and the RO data in Figure 2.12c) reverse an earlier consensus that "in the majority of observed data sets, the surface has warmed more rapidly" [Temperature Trends in the Lower Atmosphere - Understanding and Reconciling Differences, U.S. Climate Change Science Program Synthesis & Assessment Project 1.1 (2005) p. 11]? If so, this reversal would be worth highlighting. Fundamental considerations of adiabatic lapse rates imply that the tropical troposphere should warm faster than the surface. Evidently, better data is now reconciling theory and observation. [ Curt Covey, United States of America]	Noted. This text focuses upon the tropical upper troposphere, which is a smaller layer than those reported on in the 2005 report cited. The 2005 report also only includes data to 1999, and noted that the radiosonde datasets used (all of which are now superseded) are likely to have residual biases. Satellite RO measurements and reanalyses provide additional lines of evidence which were not available in 2005.
30027	43	24	43	32	It is possible to add some words to relate the changes in warming in troposphere and cooling in stratosphere reflects the enhanced warming effect by CO2. Also see Fig.2.12 [ Yihui Ding, China]	Rejected. Attribution of this type falls within the scope of Chapter 3.
80269	43	24	43	32	Same remark as above. [ Sophie Godin-Beekmann, France]	Unclassifiable.
73505	43	25	43	25	Capital 'T' for troposphere (it is used as a proper noun here) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
90327	43	25			you are inconsistent in whether or not you hyphenate "upper troposphere". Please be consistent [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
73507	43	31	43	31	Delete 'the' and 'period' (to remove the tautology). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The text is written to emphasise that this is a multi-year period as opposed to the single years referred to elsewhere in this paragraph.
37079	43	37	43	40	The averaging of this data is madess. There can only be (at most) one correct trend but you merge that with incorrect values. [ John McLean, Australia]	Rejected. The comment implies there is a single point of truth whereas the purpose of this report is to assess multiple lines of evidence.
34841	43	37	44	1	The SOD Table 2.5 provides further valuable evidence that the lower troposphere UAH satellite temperatures have risen only 0.49°C 1980-2018, equivalent to just over 0.1°C per decade, lower than model projections by a factor of 2 to 3. Please see general comments #1, #2 and #3 above. [ Jim O'Brien, Ireland]	Noted. The comment does not propose any amendment to the text. An assessment of differences between observations and models is outside the scope of Chapter 2 and is covered in Chapter 3.
19719	43	37	44	2	Table 2.5 illustrates the disagreement indicated above (P42 L43-44) between radiosondes and satellite measurements concerning the warming rate in the lower troposphere. Is this discussed anywhere in the report? It seems to me that when diverging results are simply reported (which of course is quite useful and legitimate), this corresponds to a quite restricted notion of an assessment. Note that some acronyms present in the table are not spelled out. [ philippe waldeufel, France]	Taken into account. This is reassessed with the range of updated data sets used for FGD (which includes reanalyses). It is mostly the UAH data set which is an outlier, but the UAH confidence ranges still overlap with all other datasets.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87921	43	37	44	2	Again an AR1 correction was inadequate a decade ago and is unacceptable now. Additionally, the presentation in this table is confusing. If you have estimated warming trends then they should be presented in a uniform format, such as C/decade. The 3 columns show, respectively, C/49 years, C/39 years and C/19 years, making it difficult to compare. [ Ross McKittrick, Canada]	Rejected. The use of ordinary-least-square linear trends with AR(1) correction has been used as a chapter-wide standard. For variables whose changes are clearly not linear in time, the change in means between two periods is used in preference to trends
493	43	38	43	41	As on p. 40, the word "Trend" in the table should be replaced by "Increase" (three times). [ Claire Parkinson, United States of America]	Taken into account. The table caption has been edited to make it clear that this is a total change over the stated period.
73509	43	39	43	39	Delete ( before 'Santer'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
73511	43	40	43	40	Delete , after al. and insert 'l'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
42889	43	41			I again think trend is a confusing word - I equate it with rate and expect it to have units of degrees per unit time. Why do you use the word difference or change in some places and trend in others for what is essentially the same concept? [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The table caption has been edited to make it clear that this is a total change over the stated period.
90329	43		44		Table 2.5 inconsistent use of long dash -- versus short dash - [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
4623	44	7	44	7	Fig 2.12 seems to be missing from the draft report. [ Andries Kruger, South Africa]	Rejected. Figure 2.12 is present. The reviewer may have missed it because the figure from the box is between 2.11 and 2.12.
6539	44	7	44	14	2002-2018 is a rather short period for trend calculations. The altitude/latitude cross-section included in Figure 2.12 shows a large warming trend in the lower stratosphere between 20S and 30S, and a large cooling trend at high southern latitudes. Nothing similar is seen in the northern hemisphere. How representative is this thought to be? The year 2002 was highly unusual, with a wavenumber 2 sudden warming observed for the first time ever in this hemisphere. More discussion is needed in the text. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The relatively short time period is obviously less than ideal but is the longest possible time period which incorporates RO and AIRS data. It should also be noted that this issue is now less acute as the exceptional SH stratospheric warming which occurred in 2002 also occurred in 2019.
90711	44	14	44	16	There have been recent developments over the pioneering drought atlas work. There's a new drought atlas that should be included - Cook et al. (2020) - The European Russia Drought Atlas (1400–2016 CE). Climate Dynamics 54, 2317–2335. <a href="https://doi.org/10.1007/s00382-019-05115-2">https://doi.org/10.1007/s00382-019-05115-2</a> and there are now longer precipitation reconstructions from stable isotopes - see Loader et al. (accepted) - Summer Precipitation for the England and Wales region, 1201 - 2000CE, from Stable Oxygen Isotopes in oak tree rings in the Journal of Quaternary Science (Article ID: JQS3226; Article DOI: 10.1002/jqs.3226; accepted 30 May 2020). [ Iain Robertson, United Kingdom (of Great Britain and Northern Ireland)]	Unclassifiable. This comment appears to be on the wrong figure.
90713	44	18	44	20	Although it is stated that "Much of Africa also experienced a reduction in precipitation during the MWP" the situation is more complex with "limited evidence and low agreement for the assessment of the Southern Hemisphere." With this in mind, it is important to refer to the main review article by Nash et al. (2016) - African hydroclimatic variability during the last 2000 years. Quaternary Science Reviews 154, 1-22. DOI: 10.1016/j.quascirev.2016.10.012. Another important paper for the region with differing results for the MWP is Woodborne et al. (2015) A 1000-Year Carbon Isotope Rainfall Proxy Record from South African Baobab Trees (Adansonia digitata L.). PLoS ONE 10(5): e0124202. <a href="https://doi.org/10.1371/journal.pone.0124202">https://doi.org/10.1371/journal.pone.0124202</a> . [ Iain Robertson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The suggested literature was reviewed and included in the section.
68045	44	19	44	19	Add section on upper tropospheric water vapor? For AR4 and AR5, the result of conserved relative humidity in AGCM simulations driven with warmer SSTs predicted a moistening of the upper troposphere that was consistent with remote sensing observations (Soden et al 2005), albeit with a short record that was influenced by ENSO-driven interannual variations. Is there an update for the 16 years since? [ Michael Evans, United States of America]	Noted. Section 8.3.1.2 includes the changes in upper tropospheric water vapor.
73513	44	22	44	22	Replace 'a' with 'an'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
93035	44	27	44	27	The new global reconstruction of hydroclimate proxies for the LIG published by Scussolini et al., Sci Adv, 2019 should be included. [ Bette Otto-Bliesner, United States of America]	Taken into account. The suggested literature was reviewed and included in the section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
137	44	27	45	28	In the context of the section "Paleo perspective of the global hydrological cycle" the new precipitation compilation of Scussolini et al. (2019; Science Advances volume 5) [ Pepijn Bakker, Netherlands]	Taken into account. The suggested literature was reviewed and included in the section.
105513	44	30	44	32	Sentence beginning "The paleoclimate ..." is confusing. [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was clarified.
100611	44	32	44	32	Add: "In contrast, during the MCO, some ecosystems became wetter, while others became drier (Harris et al., 2020)." [ Matthew Kohn, United States of America]	Noted. The spatial heterogeneity complicates identification of wetting and drying signals during periods before the MPWP. The section was modified to clarify this.
52115	44	37	44	40	Additional example from the Australian land context: Fitzsimmons KE, Stern N, Murray-Wallace CV, Truscott W, Pop C (2015) The Mungo Mega-Lake Event, Semi-Arid Australia: Non-Linear Descent into the Last Ice Age, Implications for Human Behaviour. PLOS ONE 10(6): e0127008. <a href="https://doi.org/10.1371/journal.pone.0127008">https://doi.org/10.1371/journal.pone.0127008</a> AND Barrows, TT, Fitzsimmons KE, Mills SC, Tumney J, Pappin D, Stern N. "Late Pleistocene lake level history of Lake Mungo, Australia." Quaternary Science Reviews 238 (2020): 106338. [ Kathryn Fitzsimmons, Germany]	Taken into account. The suggested literature was reviewed and included in the assessment.
52117	45	2	45	2	More recent examples from the Australian land context: Fitzsimmons KE, Stern N, Murray-Wallace CV, Truscott W, Pop C (2015) The Mungo Mega-Lake Event, Semi-Arid Australia: Non-Linear Descent into the Last Ice Age, Implications for Human Behaviour. PLOS ONE 10(6): e0127008. <a href="https://doi.org/10.1371/journal.pone.0127008">https://doi.org/10.1371/journal.pone.0127008</a> AND Barrows, TT, Fitzsimmons KE, Mills SC, Tumney J, Pappin D, Stern N. "Late Pleistocene lake level history of Lake Mungo, Australia." Quaternary Science Reviews 238 (2020): 106338. [ Kathryn Fitzsimmons, Germany]	Taken into account. The suggested literature was reviewed and included in the assessment.
26629	45	5	45	6	This is also the case of the Southern Hemisphere low latitudes such as South America (Mollier-Vogel et al., 2013, QSR) Southeastern Africa (Schefuss et al., 2011, Nature) and probably in many other places too. [ Eric Brun, France]	Taken into account. The suggested literature was reviewed and included in the assessment.
2011	45	5	45	12	I was surprised that there wasn't any mention of evidence of the african humid period in the Holocene here - the "Green Sahara". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We included references indicating a drying trend during the Holocene in the Northern Hemisphere tropics.
98741	45	8	45	8	Should Liefert and Shuman (2019) be cited here? It's a new North American lake-level database. <a href="https://doi.org/10.1029/2019GL086412">https://doi.org/10.1029/2019GL086412</a> [ Meredith Parish, United States of America]	Taken into account. The suggested literature was reviewed and included in the assessment.
29957	45	9	45	10	The statement about humid southern tropics in the early Holocene is incorrect. These references are not representatives of the tropics. Liu et al 2018 refers to Tanzania while Pfeiffer et al document local conditions in the particular environment of the Atacama desert. On the contrary, there is robust and consistent evidence from lake sediments, pollen, marine sediments and speleothems that the South American monsoon was strongly reduced during the early and mid-Holocene as a response to insolation (Bird et al., EPSL 2011; Kanner et al., QSR, 2013; Mollier-Vogel et al., QSR 2013) [ Matthieu Carré, France]	Taken into account. The suggested literature was reviewed and included in the assessment. We rephrased the section according to the new results.
52119	45	10	45	11	Refers to early Holocene wetting peak in extratropical Australia but drying thereafter: Fitzsimmons, K.E. and Barrows, T.T., 2010. Holocene hydrologic variability in temperate southeastern Australia: an example from Lake George, New South Wales. The Holocene, 20(4), pp.585-597. [ Kathryn Fitzsimmons, Germany]	Taken into account. We decided to include a more recent study that shows this drying during the Holocene (Barr et al., 2019: <a href="https://doi.org/10.1038/s41598-019-38626-3">https://doi.org/10.1038/s41598-019-38626-3</a> ).
58173	45	14	45	14	maybe address a little more on "imporved proxy record", like what has been mainly improved. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence was rephrased.
17781	45	16	45	16	Include "regional" or similar, to make sure the droughts are not understood to be of hemispheric or global extent [ Raphael Neukom, Switzerland]	Accepted. The sentence was modified according to your suggestion.
10423	45	16	45	20	That these studies define "MWP" and "LIA" differently to each other and to the IPCC should be acknowledged. E.g., Cook (2015) uses periods 100-200 years shorter than used by IPCC and Shuman (2018) uses periods 100-200 years longer than used by IPCC. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Even considering that the definitions of the MCA and LIA differ from those of the glossary of the IPCC, the evaluation of the proxy time series of Cook et al. (2015) and Shuman et al. (2018) led to the same results. The terms LIA and MWP have been deprecated for the FGD.
98743	45	18	45	18	Rodysill et al. (2018) also show a North American synthesis of hydroclimate proxies. <a href="https://doi.org/10.1016/j.gloplacha.2017.12.025">https://doi.org/10.1016/j.gloplacha.2017.12.025</a> [ Meredith Parish, United States of America]	Taken into account. The suggested literature was reviewed and included in the section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17783	45	18	45	19	According to Nash et al. (2016) This is not true for the Sahel area, for example. So maybe be more precise, e.g. "Much of Africa south of the Sahel". Suggest including Nash et al. (2016, doi: 10.1016/j.quascirev.2016.10.012) here, as it assesses the MCA in a wider context in contrast to Lüning et al. [ Raphael Neukom, Switzerland]	Taken into account. The sentence was rephrased and the reference was included according to your suggestion.
78401	45	20	45	20	To add some balance, maybe you could add: "In the Arctic, the Medieval Climate Anomaly seems to have been wetter than the Little Ice Age (LIA), but with obvious regional differences (Linderholm et al. 2018). Taken from the following source: Arctic hydroclimate variability during the last 2000 years: current understanding and research challenges. <a href="https://doi.org/10.5194/cp-14-473-2018">https://doi.org/10.5194/cp-14-473-2018</a> [ Hans W Linderholm, Sweden]	Taken into account. Due to space limitations and considering the regional characteristic of the suggested study, we were not able to include it in the assessment of section 2.3.1.3.1.
68043	45	24	45	28	I'm not sure this summary is accurate. It might be so for the Holocene (Shuman et al 2018), but it is probably not so for the Common Era. Much of the basis cited is from Common Era reconstructions of variance, not so well constrained for estimates of changes in mean over centuries. For the occurrence of megadroughts, these also rely on drought atlases, and my own assessment of them is that it would be difficult to demonstrate a trend in their frequency because they are rare. I realize that the confidence is assessed: medium, but perhaps a figure that integrated all these results to clearly show the basis for this summary is in order. [ Michael Evans, United States of America]	Taken into account. The summary statement was modified based on new evidence. We included a statement both for the Holocene and the Common Era. Given the regional heterogeneity of precipitation trends and space limitations within the chapter, we are not able to provide a figure to describe the section.
4625	45	31	46	11	Can be beneficial for laymen to be explained the difference between specific and relative humidity and how do these measure the actual water vapour content. [ Andries Kruger, South Africa]	Noted. The definitions of relative humidity and specific humidity can be found in the Glossary.
30235	45	32			Is it possible to clarify what means 'near surface' here, esp. with respect to the boundary layer? [ Gilles Delaygue, France]	Noted. Near-surface typically corresponds to 2 meter height. Given that this is value is the typical standard for observations, reanalyses and model simulations, we don't think there is a need to clarify that.
37081	45	33	45	34	An increase in specific humidity would cause an increase in relative humidity if temperatures were unchanged, so I don't see how you can claim that specific humidity increased but relative humidity decreased. Please explain. [ John McLean, Australia]	Noted. Warmer regions exhibit larger increases in specific humidity for a given temperature change under conditions of constant relative humidity, based on the Clausius-Clapeyron equation. The physical causes of declining relative humidity over land are discussed in sections 8.2.1 and 8.3.1.2
113115	45	33	45	34	Rephrase 'This abatement was assessed to have arisen in part from a decline in relative humidity', not sure what is meant here. Relative humidity as a driver of the specific humidity? [ Diego Miralles, Belgium]	Taken into account. The sentence was rephrased to indicate that the abatement of specific humidity resulted in a recent decline in relative humidity over land.
99743	45	34	45	40	The analysis of PMIP3 model output shows multi-model agreement at regionally higher precipitation at the LGM than for preindustrial conditions due to changing circulation patterns, therefore the expectation of a 'dry LGM' is to some extent only valid at the global scale (see Rehfeld, K., Hébert, R., Lora, J. M., Lofverstrom, M., and Brierley, C. M.: Variability of surface climate in simulations of past and future, Earth Syst. Dynam., 11, 447–468, <a href="https://doi.org/10.5194/esd-11-447-2020">https://doi.org/10.5194/esd-11-447-2020</a> , 2020., and references therein). [ Kira Rehfeld, Germany]	Rejected - outside the scope of the chapter. Assessment of PMIP runs is the purview of Chapter 3 and is not covered here. The assessment indicates "This agrees with models and moisture-sensitive proxies, suggesting an overall decrease in global precipitation during the LGM relative to recent decades, albeit with regional-scale heterogeneity (Cao et al., 2019). "
30237	45	36			It would be interesting to have an indication of the spatial coverage of this monitoring, especially with respect to high latitudes. [ Gilles Delaygue, France]	Taken into account. Due to the variety of products and limited space, we are not intended to provide such details. However, we highlighted the uncertainties associated to observation coverage.
30239	45	37			a parenthesis is missing before 'reanalysis' [ Gilles Delaygue, France]	Editorial - copyedit to be completed prior to publication
82299	45	38	45	38	Please change "HOAPS3" into "HOAPS4". [ Schröder Marc, Germany]	Noted. Given that there is no publication for the HOAPS4 at the time of the FGD, we opted to keep the HOAPS3 product with the Liman et al. (2018) reference.
82287	45	39	45	41	I propose to remove "between all products" from this sentence in order to more clearly separate this conclusion with the summary given on page 46, lines 8-10. [ Schröder Marc, Germany]	Accepted. The sentence was rephrased according to your suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30241	45	40			Any conclusion on the specific humidity trend after 2000? (instead of relative humidity) [ Gilles Delaygue, France]	Noted. The final FGD version addresses changes in the last decades as follows: "Since 2012, specific humidity over land and ocean has remained well above the 1973–2019 average and reached record or near-record values (Figure 2.13b), with the strong 2015–2016 El Niño event boosting surface moisture levels (Byrne & O’Gorman, 2018). The abatement from around 2000 to 2012 reported in AR5 has not persisted."
37459	45	43	45	43	Kindly explain how a strong El Nino event might increase surface moisture levels when increased rainfall under El Nino conditions is only reported for regions in and around California and Peru. [ John McLean, Australia]	Noted. The increase in specific humidity responded to the increase in global temperature, boosted by an El Niño event. Assessment of the physical aspects of surface moisture is covered in Chapter 8.
37083	45	44	45	44	Being "consistent with" is not proof of cause. Humidity is not driven by temperature alone but by anything that influences the rate of evaporation (e.g. winds, mixing of drier air, surface moisture, state of the ocean surface) [ John McLean, Australia]	Noted. Trends and variability in specific humidity/water vapour were mostly attributed to increases in temperature, particularly over the tropics and the Northern Hemisphere (Held & Soden, 2006; Willett et al., 2008). Moreover, both the temperature and humidity changes observed over land between 1979 and 2016 are linked to warming over neighbouring oceans. However, assessment of the physical aspects of surface moisture is covered in Chapter 8.
81011	45	45	46	20	Should there be reference in this section to the importance of wet bulb temperatures in the context of humidity given the important human health implications of exceeding tolerable physiological limits for human health from elevated wet bulb temperatures? It is my understanding that more widespread exceedance of tolerable wet bulb temperature around the planet is having significant implications for human and animal health. Maybe this is covered elsewhere, but a sub-section reference at least in this section would be helpful. [ Jeffrey Philip OBBARD, Singapore]	Rejected - outside the scope of the chapter. Chapter 12 analyses the wet bulb globe temperature index.
1581	45	46	45	47	Does this follow through to the modelling sections? It was thought that RH remained relatively constant. This would be a good issue for a cross-chapter box. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Sections 3.3.2 and 4.5.1.3 follow the assessment considering climate model simulations.
30243	45	46			briefly explain how is calculated the 'global land averaged relative humidity' (i.e., is it the arithmetic mean of RH or the ratio of globally averaged specific humidity to saturation, or else?) [ Gilles Delaygue, France]	Noted. The reader can refer to the references within the section to clarify those computational aspects.
90809	45	46			Relative humidity had remained broadly constant over the 1973–2003 period (Dai, 2006; Willett et al., 2008, 2010) and the expectation was that it would continue to do so in the near term (Reference: Comparison of land surface humidity between observations and CMIP5 models) [ Vivien How, Malaysia]	Noted. Sections 3.3.2.2 and 4.5.1.3 follow the assessment considering climate model simulations.
19723	45	49	45	49	There ought to be a comma following "1973" [ philippe waldteufel, France]	Editorial - copyedit to be completed prior to publication.
73515	45	49	45	49	Insert , after '1973'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
24375	45	52	45	52	Should extra-tropics be sub-tropics? The definition of extra-tropics is anything outside of the tropics, including mid-latitudes and high latitudes. So in this sentence we have a statement that says relative humidity is decreasing in the extra-tropics (which implies mid-latitudes and high latitudes), whereas the previous sentence says relative humidity is increasing in the high latitudes. It can't be both. [ Owen Cooper, United States of America]	Taken into account. The sentence was rephrased replacing extra-tropics by sub-tropics.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82285	45	54	45	55	Prytherch et al. (2015) does not provide actual trend values. The time series plots exhibit diverse behaviour in terms of temporal changes, with clearly negative and potentially positive trends. Thus, if associated text is kept, I propose to reformulate into: "Satellite estimates over ocean exhibit little consistency in interannual variability or trends and even negative trends (Prytherch et al., 2015)." In contrast, Robertson et al. 2019 (submitted Dec 2019, under review) comes to partly different conclusions (their Figure 7 and Table 4) and it might be stated: "Robertson et al. (2019) observes agreement in response to ENSO and a general increase over the period 1990-2010 with values ranging between 2.3 %/K and 9.6 %/K for recently updated satellite records, ERA5 and ensemble mean of reduced observation reanalyses. It is noted in Robertson et al. (2019) that these values depend on the considered period and that subtle differences are present." As Robertson et al. uses updated versions of data considered in Prytherch et al. (except GSSTF) I propose to only include results from Robertson et al. (see above for proposed text) only. Ref.: Robertson, F. R., J. B. Roberts, M. G. Bosilovich, A. Bentamy, C. A. Clayson, K. Fennig, M. Schröder, H. Tomita, G. P. Compo, M. Gutenstein, H. Hersbach, C. Kobayashi, L. Ricciardulli, P. Sardeshmukh, L. C. Slivinski, 2019: Uncertainties in Ocean Latent Heat Flux Variations Over Recent Decades in Satellite-Based Estimates and Reduced Observation Reanalyses. J. Climate, submitted 2019, under review. [Schröder Marc, Germany]	Taken into account. We modified the sentence and included the suggested reference.
115989	45		45		For 2.3.1.3.1 and for other paleo sections, it would be good to report more explicitly what are the main differences compared to AR5 and why (summary statements, ES statements). [Valerie Masson-Delmotte, France]	Taken into account. The section was modified to highlight differences compared to AR5.
82321	46	8	46	11	In case results from Robertson et al.. (2019) will replace results from Prytherch et al. (2015) (see entry #3) it is adequate to reformulate the summary. I propose to change into: "In summary, observations since the 1970s show a very likely increase in near surface humidity over land and ocean with low confidence on the magnitude, in particular over ocean. A very likely..." [Schröder Marc, Germany]	Taken into account. We reformulated the summary statement according to your suggestions.
24377	46	10	46	11	This sentence makes it sound like relative humidity is decreasing over all land areas, especially over mid-latitudes. But the previous text and Figure 2.13 clearly show increases at high latitudes. This sentence needs to be re-written so that it reflects increases at northern high latitudes and decreases at mid-latitudes. [Owen Cooper, United States of America]	Taken into account. The sentence was rephrased according to your suggestion.
45223	46	10	46	11	A very likely decrease in relative humidity was observed over land areas since 2000, particularly over mid-latitude regions of the Northern Hemisphere. Please check consistency with Chapter 8 regarding the timing and the region. Chapter 8 (pg 33, lines 38-39) only mentions that "relative humidity has decreased over many land regions". Additionally, Chapter 3 (pg 27, lines 51-52) says that "Owing to the limited number of studies and model biases we conclude that there is low confidence in the attribution of changes in the surface humidity". Consistent assessment of observed surface humidity changes and its attribution may be take care across Chapters 2, 3 and 8. [Krishnan Raghavan, India]	Taken into account. We ensured consistency between Chapters 2, 3 and Chapter 8 regarding changes in humidity.
19725	46	14	46	22	Figure 2.13: it might be more convenient for the reader to express the relative humidity trend (bottom map), as usual, as the trend of the ratio of water partial pressure to saturating pressure. [Philippe Waldeufel, France]	Taken into account. The map is consistent with the available literature showing trends in %RH per decade. The units for the SOD were wrong (g/kg per decade) and now the label of the colorbar was corrected.
37085	46	16	46	20	Presenting overall trends is unsatisfactory because they can mask significant variation. The variation has to be shown. [John McLean, Australia]	Noted. The regions with non-significant trends in specific humidity and relative humidity are indicated by a X in the FGD following discussions across chapters.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18315	46	25	47	5	Radiosonde data provide the only in-situ long-term global records of tropospheric water vapor before and after 1979. They provide a strong constraint on modern reanalysis products. However, due to inhomogeneities in these records, the radiosonde humidity data and the reanalysis products such as NCEP/NCAR, ERA40, JRA55, etc. are not suitable for water vapor trend analyses (Dai et al. 2011). There have been efforts to homogenize the radiosonde humidity records for quantifying water vapor trends and for improving reanalysis products (e.g., Dai et al. 2011). The homogenized humidity data have been used to quantify water vapor trends (including PW) over China (Zhao et al. 2012) and the globe (Wang et al. 2016). There are also many efforts to homogenize radiosonde temperature data for better quantifying tropospheric warming trends and for use in atmospheric reanalyses (see Zhou et al. 2020 and refs cited there). Relevant refs: Dai, A., J. Wang, P.W. Thorne, D.E. Parker, L. Haimberger, and X.L. Wang, 2011: A new approach to homogenize daily radiosonde humidity data. J. Climate, 24, 965-991. Wang, J. A. Dai, and C. Mears 2016: Global water vapor trend from 1988-2011 and its diurnal asymmetry based on GPS, radiosonde, and microwave satellite measurements. J. Climate, 29, 5205-5222. DOI: <a href="http://dx.doi.org/10.1175/JCLI-D-15-0485.1">http://dx.doi.org/10.1175/JCLI-D-15-0485.1</a> . Zhao, T., A. Dai, and J. Wang, 2012: Trends in tropospheric humidity from 1970-2008 over China from a homogenized radiosonde dataset. J. Climate, 25: 4549-4567. Zhao, T., J. Wang, and A. Dai, 2015: Evaluation of atmospheric precipitable water from reanalysis products using homogenized radiosonde observations. J. Geophys. Res., 120, 10,703–10,727, doi:10.1002/2015JD023906. Zhou, C., J. Wang, A. Dai, and P. W. Thorne, 2020: A new approach to homogenize global twice-daily radiosonde temperature data from 1958 to 2018. J. Climate, submitted (available from <a href="mailto:adai@albany.edu">adai@albany.edu</a> ). [ Aiguo Dai, United States of America]	Taken into account. The premise for the assessment is to include relevant literature based on observed changes in total column water vapor, analysing the longest period possible. With this in mind, we needed to include information prior to the start of radiosonde measurements, even considering data issues and limitations. We also need to include information based on satellite estimations as complement to radiosonde observations. We assessed the results from Wang et al. (2016) given that it is a global study. Regional variations in TCWV are assessed in section 8.3.1.2.
30249	46	30	46	31	Reverse the order of the reanalysis vs. SST use as they are developed afterwards. [ Gilles Delaygue, France]	Accepted. The sentence was rephrased.
4627	46	30	46	40	Refer to Fig 2.10. As water vapour is such a powerful GHG, it should be motivated why not included in Fig 2.10 as possible factor in changes in global surface temperature. [ Andries Kruger, South Africa]	Taken into account. The amount of water vapour in the atmosphere is controlled mostly by air temperature, rather than by emissions. For that reason, it is considered a feedback agent, rather than a forcing to climate change. Figure 2.10 shows changes in the effective radiative forcings. The assessment of stratospheric water vapor can be found in section 2.2.5.1
30245	46	30			Please add some information on when the coverage can be reliably considered as 'quasi-global' (is it 'middle of the 20th century' as in L.38?) [ Gilles Delaygue, France]	Noted. This aspect is clarified in Chapter 1.
30247	46	30			'require the use...': please make it clear for which goal this is required (to get a global average?) [ Gilles Delaygue, France]	Taken into account. The sentence was rephrased.
127023	46	31	46	33	If, as stated, the statistically-based estimates are constrained to track global SSTs, the two periods of positive trends are merely a reflection of the two periods of positive SST trends and only indicate positive TCWV trends by assumption. This is not noted sufficiently prominently. [ Trigg Talley, United States of America]	Accepted. The sentence was changed to include the suggested information.
30251	46	31			What means here 'Statistically-based'? (SST-based wrt the previous sentence?) [ Gilles Delaygue, France]	Noted. Statistically-based indicates that the TCWV was determined through the SSTs using statistical methods. This was clarified in the text.
90333	46	31			insert comma in (Smith and Arkin 2015) [ Jeannine-Marie St-Jacques, Canada]	Editorial - copyedit to be completed prior to publication.
58231	46	33	46	33	Zhang et al., 2013 citation: is this Zhang et al., 2013 a or b? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - copyedit to be completed prior to publication. There is only one Zhang et al. (2013) paper cited in the chapter.
30253	46	42			'assessment of TCWV' [ Gilles Delaygue, France]	Accepted. The sentence was changed according to the suggestion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36353	46	48	46	49	Although literally true, this sentence is open to criticism. I can imagine a climate skeptic saying "Well of course SOME subset of the data can be found consistent with theory!" The Schröder et al. paper says "Trend estimates were assessed on (near) global scale and for a number of regions. It can be concluded that these trend estimates are generally significantly different among the data records (TCWV, q and T) and are also typically outside the theoretically expected range dictated by Clausius–Clapeyron (TCWV)." Better to begin with "Although some TCWV products exhibit changes which scale at around 7.5% ..." [ Curt Covey, United States of America]	Accepted. The sentence was modified to indicate that inhomogeneities in satellite observations led to trend estimates that are not in line with theoretical expectations imposed by Clausius-Clapeyron.
79641	46	49	46	52	Relevant reference to be cited here (see Figure 4): <a href="https://link.springer.com/article/10.1007/s10584-014-1100-9">https://link.springer.com/article/10.1007/s10584-014-1100-9</a> [ Rodrigo Manzananas, Spain]	Rejected. The focus of the section is the assessment of changes in TCWV, not precipitation.
82289	46	52	46	54	In view of the comment on "low confidence" (line 54) in I think it is important to more precisely define the region, i.e., only parts in the centre of South America exhibit suspicious behaviour. I further do not agree with the indirect interpretation, that e.g. TCWV from microwave observations has low confidence over ocean (for confirmation see also page 9, last sentence first paragraph in Schröder et al., 2019). Thus, please change "South America and many global ocean regions (Schröder et al..." into "central South America (Schröder et al..." [ Schröder Marc, Germany]	Accepted. The sentence was changed according to the suggestion.
82311	47	1	47	1	It is stated here that positive trends in TCWV are likely while it is stated in chapter 8, page 33, line 36 that increases in water vapour are very likely. Given the expectation from theory, the fairly large number of papers revealing an increase in TCWV, and the still large number of data records exhibiting positive trends (and, if not, break points are evident, Schröder et al., 2019) it might be adequate to conclude here that positive trends in TCWV are very likely. Then, the subsequent statement clarifies that due to presence of break points some data records do not exhibit positive trends and thus, confidence is medium. [ Schröder Marc, Germany]	Accepted. The summary statement was changed according to the suggestion.
45225	47	1	47	3	"In summary, positive trends in "global" total column water vapour are likely since the 1979". On the other hand, Ch.3 assesses the human influence on "tropical" moistening in the upper troposphere since 1979 with medium confidence. Consistency across Chapters 2 and 3 to be taken care. [ Krishnan Raghavan, India]	Noted. However, both conclusions are different given the purposes of the chapters and can't be compared.
18317	47	8	48	27	Dai and Zhao (2017) compared and evaluated various land precipitation products and concluded that CRU TS precipitation product had much lower gauge coverage since the middle 1990s than GPCC or GPCP products, leading to a wet biases in global land precipitation in CRU TS for the period since the 1990s. For estimating land precipitation changes for the recent periods, they recommended to use GPCC or GPCP products. Another relevant point is that historical precipitation changes over most of the globe either since 1979 or since 1950 are still dominated by natural decadal climate variability (Dai and Zhao 2017; Dai et al. 2018; Dai and Bloecker 2019), such as those associated with the Interdecadal Pacific Oscillation (IPO) (Dai 2013; Dong and Dai 2015). Thus, we need to caution any comparisons between observed and model-simulated historical precipitation changes in order to avoid misleading conclusions such as those made by Wentz et al. (2007, Science). Relevant refs: Dai, A., 2013: The Influence of the Inter-decadal Pacific Oscillation on U.S. precipitation during 1923-2010. Climate Dynamics, 41: 633-646, doi:10.1007/s00382-012-1446-5. Dong, B., and A. Dai, 2015: The influence of the Inter-decadal Pacific Oscillation on temperature and precipitation over the globe. Climate Dynamics, 45, 2667–2681. DOI 10.1007/s00382-015-2500-x. Dai, A. and T. Zhao, 2017: Uncertainties in historical changes and future projections of drought. Part I: Estimates of historical drought changes. Climatic Change, 144, 519–533. DOI: 10.1007/s10584-016-1705-2. Dai, A., T. Zhao, and J. Chen, 2018: Climate change and drought: A precipitation and evaporation perspective. Current Climate Change Reports, 4, 301-312. DOI: 10.1007/s40641-018-0101-6. ( <a href="http://link.springer.com/article/10.1007/s40641-018-0101-6">http://link.springer.com/article/10.1007/s40641-018-0101-6</a> ). Dai, A., and C.E. Bloecker, 2019: Impacts of internal variability on temperature and precipitation trends in large ensemble simulations by two climate models. Climate Dynamics, 52, 289–306. <a href="https://doi.org/10.1007/s00382-018-4132-4">https://doi.org/10.1007/s00382-018-4132-4</a> . [ Aiguo Dai, United States of America]	Taken into account. The main characteristics of the datasets, including the number of stations used and the improvements from previous versions were included in the section. We included the suggested references in the assessment of global precipitation. Comparison between observations and model simulations is the purview of Chapter 3 and is not covered here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
499	47	9	47	50	With the map of Figure 2.14f being so different from the 2.14d and 2.14e maps, the text should discuss why. [ Claire Parkinson, United States of America]	Taken into account. The difference between the panels is merely as a consequence of comparing satellite data (global coverage) with in-situ land observations.
95901	47	15	47	25	It is good the point of low inter-data consistency between various data sets is pointed out here. Indeed, precipitation data sets out there have very different quality attributes. From the assessed literature, authors can indicate the major qualities -e.g. "a diagnostic" Which data sets used in the assessed studies might have had. This requires a comparative check on the data sub-section of the various studies/papers. This will be another quality to ensure robustness of assessments of key global quantities like precipitation in this case, and also globally important regional phenomena like global monsoons Index on page Pg.57. [ Joseph Mutemi, Kenya]	Taken into account. The main characteristics of the datasets, including the number of stations used and the improvements from previous versions were included in the section.
58233	47	18	47	20	Consider rewording sentence into: 'The resulting temporal evolution of global annual land precipitation anomalies shows low inter-dataset consistency which is particularly marked prior to 1950 and is associated with limitations in data coverage.' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence was changed according to the suggestion.
58175	47	20	47	25	would be nicer to specify the plots that are referred to (Figure 2.14 a? b? c? ...) [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We modified the section based on your suggestion.
82293	47	23	47	23	The contrast between station-based and GPCP based trend estimates is striking. I assume that spatially collocated GPCP data is used only. If so I propose to change "Global trends" into "Global trends over land..." and accordingly, the caption of Table 2.6 into "Globally averaged trend estimates over land". If computed over different regions I propose to explicitly mention the land based nature of the station data and the global coverage of GPCP. [ Schröder Marc, Germany]	Taken into account. The caption of Table 2.6 was modified based on your suggestion.
58177	47	27	47	35	specify the plots (Figure 2.14 a? b? c? ...) [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. We modified the section based on your suggestion.
82291	47	27	47	35	The text, Table 2.6 and Figure 2.14 would benefit from a clarification and update: two GPCC versions are mentioned, i.e., version 8 and version 8+. Please use consistent version or explain the reason for utilisation of different versions. I further propose to consistently analyse the period 1901-2018, i.e., update figure 2.14b accordingly. [ Schröder Marc, Germany]	Noted. The "+" following the 8 is a typo and it was corrected. We updated the precipitation trends for CRU, GPCC and GHCN datasets.
80291	47	27	47	35	CH8 also shows this info (with more detail) [ Paola Arias, Colombia]	Noted. Chapter 8 focused more on the regional precipitation trends after the 1950s, while Chapter 2 provides a longer term perspective.
98017	47	27	47	45	Knutson and Zeng (2018; see their Fig. 3-5) could be cited here. That paper compares observed trend magnitudes for all gridpoints to the distribution of trends from natural variability as estimated by combining natural forcing ensemble mean trends with control run trend distributions from climate models. This is used to estimate where a linear trend signal has emerged from natural variability background (i.e., the "emergence" question posed in Box SPM.1.) This analysis concludes there have been detectable regional trends in land-based precipitation since 1901 as follows: detectable decreases over the Mediterranean region, northern tropical Africa, far southwest Australia, Tasmania, parts of the Caribbean and Maritime Continent, parts of Chile, Japan, southwest Africa, and Sri Lanka, plus a few other smaller regions (with lower confidence due to their small size). Detectable increases (far more common than detectable decreases) include large regions of the extratropics in northern Eurasia (in regions with sufficient data for trend analysis), the north-central to northeastern United States, southern to southeastern Canada, southeast South America, and northern Australia. Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, <a href="https://doi.org/10.1175/JCLI-D-17-0672.1">https://doi.org/10.1175/JCLI-D-17-0672.1</a> . [ Thomas Knutson, United States of America]	Taken into account. The suggested literature was added to the section.
73517	47	29	47	29	Delete , after 'America'. It is not required in this context. [ Burt Peter, 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
497	47	29	47	30	The statement that "Negative trends are strongest across tropical western equatorial Africa and southern Asia" does not appear correct according to Figure 2.14. [ Claire Parkinson, United States of America]	Noted. The statement is valid only for the new period 1901-2019 (new Figure 2.15 panels a and b).
30255	47	29			'tropical western and equatorial'? [ Gilles Delaygue, France]	Accepted. The sentence was modified according to your suggestion.
105515	47	32	47	33	Commas missing "since 1980, .... Since 1901, are evident" [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
3999	47	35	47	38	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. [ Guoyu Ren, China]	Taken into account. Uncertainties related to the assessed datasets were included based on previous comments. The suggested literature is particularly regional to local and falls outside the scope of the assessment being performed by chapter 2.
19727	47	38	47	52	On figure 2.14, the consistency between both components of the subplot c) is problematic. The change of scales complicates things. Also, the missing decadal value (1920-1930) for CRU is a significant drawback. In case this missing value cannot be recovered, you might consider removing the graph for decadal values. Unless an explanation is provided... [ philippe waldteufel, France]	Taken into account. The decadal value of CRU for the 1920-1930 period was added to the figure. We opted for using different scales in both plots.
81195	47	43	47	43	to separate "withan" [ Supriyo Chakraborty, India]	Editorial. The typo was corrected.
29847	47	43	47	43	Typo. [ Hernan Edgardo Sala, Argentina]	Editorial. The typo was corrected.
30257	47	43			'with an' [ Gilles Delaygue, France]	Editorial. The typo was corrected.
43081	47	43			Read "masked to regions with an observational constraint." rather than "masked to regions withan observational constraint." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. The typo was corrected.
105517	47	46	47	50	This caption is very confusing and the format p = is inconsistent with earlier text. [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The caption of the new Figure 2.15 was modified for the FGD.
30259	47	46			Make it clear what is shown by Fig 2.14c: 'means' of what, all data, land only (as in Table 2.6)? I find this section confusing because the data sets are so diverse that it is difficult to grasp what represent 'means' and 'trends'. [ Gilles Delaygue, France]	Taken into account. The caption of the new Figure 2.15 was modified for the FGD.
73519	47	47	47	47	Delete , after al. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
43083	47	47			Read é Project (GPCP, Adler et al., 2018) datasets " rather than " Project (GPCP, Adler et al., (2018) datasets " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial - copyedit to be completed prior to publication.
58179	47				Figure 2.14: it's hard to locate the 1950s, 1970s, and mid-1990s on the figure, leading to some extent of confusions in the descriptions in text from line 15 to 25 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The new figure 2.15 was improved for the FGD.
87923	48	2	48	27	Once again use of an AR1 correction here is woefully inadequate. No one would seriously try to use that in a hydrology application. If you don't want to compute a persistence-robust measure (such as Cohn, Timothy A. and Harry F. Lins (2005) Nature's Style: Naturally Trendy. Geophysical Research Letters 32, <a href="https://doi.org/10.1029/2005GL024476">https://doi.org/10.1029/2005GL024476</a> .) or use a HAC-robust estimator then don't show confidence intervals at all since AR1 is seriously biased downwards in precip and hydrology applications. [ Ross McKittrick, Canada]	Rejected. The method for trend estimation was homogenised across the chapter.
106523	48	3	48	9	In this table, the "plus or minus" is generally much greater than the "prime part". Does this not lower the "trust" of the audience/ reader. Usually we want to cope with a "smaller plus or minus". [ Joseph Mutemi, Kenya]	Noted. Based on the trend ranges the confidence was assessed as "medium".
95903	48	3	48	9	Page 48, Lines 3 to 9: In this table, the "plus or minus" is generally much greater than the "prime part". Does this not lower the "trust" of the audience/ reader. Usually we want to cope with a "smaller plus or minus". [ Joseph Mutemi, Kenya]	Noted. Based on the trend ranges the confidence was assessed as "medium".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17085	48	3	48	9	In Table 2.6, authors present the globally average trends of precipitation change. Is it possible to scrutinize it for specific regions that are mentioned in the paragraph? For example: tropical western equatorial Africa, southern Asia, North America, Australia, etc. This effort will support the precipitation changes claims in the paragraph, won't it? [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - outside the scope of the chapter. Assessment of regional precipitation changes is the purview of Chapter 8 and is not covered here.
30261	48	3			'globally averaged' over land only? (as written p.47 L.24) [ Gilles Delaygue, France]	Noted. The table caption was corrected to clarify this issue.
30263	48	4			'in Figure 2.14' [ Gilles Delaygue, France]	Noted. For the FGD is Figure 2.15.
127025	48	7	48	8	Comment on Table 2.6: Global-average absolute changes are not as meaningful as fractional changes. It would be helpful to add a column expressing the global mean annual precipitation over a reference period such as 1981-2010. [ Trigg Talley, United States of America]	Noted. We used the standard graph that is widely used in the scientific literature.
1225	48	14	48	22	There have also been some studies of the daily semi-global (50S-50N which is 77% of earth's area) precipitation area between 1998 and 2016 (DOI: 10.1088/1748-9326/aab375) based on TRMM. The analysis suggests that the daily area of precipitation has declined by ~7% over two decades, which has profound implications for the hydrological cycle and rainfall statistics. It's likely due to a changed cloud climate and increased greenhouse effect (DOI: 10.1007/s00704-016-1732-y), and when the water evaporated over the oceans are returned over a diminishing area, then the mean rainfall intensity is expected to increase (a "funnel effect"). It also implies more frequent droughts where it does not rain. I think the IPCC report would be incomplete if it does not include this finding. [ Rasmus Benestad, Norway]	Rejected - outside the scope of the chapter. Assessment of daily precipitation changes is the purview of Chapters 8 and 11 and is not covered here. Attribution of these changes is the purview of Chapter 3.
82295	48	15	48	19	As far as I know CMORPH and GSMaP utilise CPC, i.e., gauge-based data. Thus, I propose to reformulate this sentence into: "The majority of these are based on combinations of in situ observations and satellite retrievals..." [ Schröder Marc, Germany]	Noted. What CMORPH and GSMaP use from CPC is the IR retrievals from the CPC, not the gauge-based data. Nevertheless, some versions (typically not the real-time or standard products) of the datasets include gauge-calibrated products.
79643	48	19	48	21	The same reference mentioned is my previous comment is again relevant here: <a href="https://link.springer.com/article/10.1007/s10584-014-1100-9">https://link.springer.com/article/10.1007/s10584-014-1100-9</a> [ Rodrigo Manzanar, Spain]	Noted. The suggested literature has a regional focus and falls outside the scope of the chapter.
127027	48	19	48	21	This is true, but none of the studies authors cite explicitly evaluate the newer blended products like PERSIANN-CDR and CHIRPS. Have the merging procedures improved to the point that the affect on estimated trends has been reduced or eliminated? [ Trigg Talley, United States of America]	Rejected. This assessment is not expected to provide such details.
127029	48	23	48	23	Li et al. (2016) should be Li et al. (2015b). [ Trigg Talley, United States of America]	Editorial - copyedit to be completed prior to publication. The reference is correct: Li, X., Hu, Z.-Z., Jiang, X., Li, Y., Gao, Z., Yang, S., Zhu, J. and Jha, B. (2016), Trend and seasonality of land precipitation in observations and CMIP5 model simulations. Int. J. Climatol., 36: 3781-3793. doi:10.1002/joc.4592
58235	48	23	48	23	Li et al., 2015 citation: is this Li et al., 2015a or b? From the reference list this looks as though it is supposed to be b. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The reference was removed from the section.
58181	48	25	48	27	Which aspect is improved as compare to AR5 that leads to "likely increased" global precipitation as compare to "no significant trend"? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The main difference can be attributed to the different periods for the trends assessment. AR5 considered the period 1951-2008 while AR6 considered 1960-2018 and 1980-2018.
79967	48	25	48	27	A summary statement needs to be included for global ocean precipitation in addition to that for global land precipitation. This would probably say that it's not possible to reliably determine ocean precipitation trends, if that's the case it still needs to be stated. [ Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We included a summary statement for ocean precipitation according to your suggestion.
79969	48	25	48	27	Likewise there is no assessment of whether evaporation has changed over the ocean so this needs to be assessed as well. I raised this point for the FOD but it has not been addressed. It really does not make any sense to assess changes in P and E-P in this Chapter without including assessment of E as well. [ Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Changes in evaporation were included in section 2.3.1.3.5

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45227	48	25	48	27	"In summary, global land precipitation has likely increased since the middle of the 20th century (medium confidence)". This is inconsistent with the assessment in Chapter 3 (Page 25, lines 14-16) which says that "The observed trend for precipitation averaged over NH land areas throughout the 20th century is negligible (Wu et al. 2013). Also, no significant trend is found in the global precipitation mean value during the satellite era". This consistency issue between Chapters 2 and 3 need to be addressed. [ Krishnan Raghavan, India]	Taken into account. The comparison between both chapters is not entirely correct. Our assessment evaluates changes in global precipitation from satellite products considering several publications. Adler et al. (2017) showed a non-significant increase in land and ocean precipitation, while Adler et al. (2018) found a significant increase in global precipitation based on GPCP. Table 2.6 also shows an increase in global precipitation based on GPCP, although non-significant, confirming the substantial interannual variability of the precipitation records given the differences in the trend significance considering different periods. Wu et al. (2013) paper consider trends for the period 1901-2011, including the records prior to 1950s, which were considered to have substantial differences among precipitation products. Thus, our statement of precipitation changes since the middle of the 20th Century can't be compared with the conclusions from Wu et al. (2013).
73967	48	25	48	27	Trend in precipitation is very important information in the context of decision making, particularly at regional scale. This sentence can be misinterpreted by decision makers: from one hand it states that global land precipitation has likely increased (I guess that most people will notice "increased" and will not pay attention on "likely"), and then it is written about large interannual variability and regional heterogeneity which means large uncertainty in the global trend. [ Elena Kozlovskaya, Finland]	Taken into account. The summary statement was modified to avoid any misinterpretation.
113117	48	30	48	30	The section 'Precipitation minus Evaporation' neglects 15 years of research on deriving and studying trends in terrestrial evaporation from satellite records. There are multiple high impact articles on this. As an example: Jung, M., et al.: Recent decline in the global land evapotranspiration trend due to limited moisture supply, Nature, 1–4, doi:10.1038/nature09396, 2010.   Cheng, L., et al.: Recent increases in terrestrial carbon uptake at little cost to the water cycle, Nature Communications, 1–10, doi:10.1038/s41467-017-00114-5, 2017.   Miralles, D. G., et al. : El Niño–La Niña cycle and recent trends in continental evaporation, Nature Climate Change, 4(1), 1–5, doi:10.1038/nclimate2068, 2014.   Zhang, Y., et al.: Multi-decadal trends in global terrestrial evapotranspiration and its components, Sci. Rep., 1–12, doi:10.1038/srep19124, 2015. [ Diego Miralles, Belgium]	Taken into account. Section 8.3.1.4 covers the changes in land-surface evapotranspiration, while section 8.3.1.6 covers the changes in soil moisture. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E.
72185	48	30	49	8	strange P-E jump over the ocean at the beginning of the 21st century in two data sets (Fig. 12.15 b & d should be at least commented [ Joanna Wibig, Poland]	Noted. The section includes the following: "assessment of global P-E trends is generally performed using reanalyses, although changes in the observing system add considerable uncertainty (Dyn et al., 2014). This limitation is evident considering the global trends from CFSR and MERRA-1 (Figure 2.15c) which exhibit positive trends (precipitation exceeding evaporation) mainly resulting from an apparent discontinuity in the late 1990s"
6541	48	30	49	29	Section 2.3.1.3.5 left me bemused. Discussion of reanalyses should be confined to a short paragraph with no figure. The paragraph should refer to, as the existing text does not, what reanalysis producers have written in their peer-reviewed papers about how well or badly their reanalysis does at representing the long-term behaviour of P and P-E. One of the aims of the producers in publishing papers describing their products is to avoid the type of mis-use of them that one sees in this section of the SOD. More detail is discussed in the following three comments. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Section 1.5.2 (New developments in reanalyses) assesses the evolution of different reanalysis products. In this sense, our objective is to use the most adequate tools for the evaluation of changes in P-E, being one of these tools the ERA5 reanalysis.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6543	48	30	49	29	The map based on ERA5 included in Figure 2.15 should not be shown as the reader has no way of knowing what is a reasonable representation of reality and what is an artefact. Firstly, please read sections 9.2 and 9.3 of Hersbach et al, submitted, a paper on ERA5 referenced in Chapter 2 and now accepted for publication. What is presented there shows ERA5 to be an advance on ERA-Interim, but still far from problem-free. In particular, it is pointed out that ERA5 precipitation declines relative to GPCC and GPCP over a few years centred on the year 2000, especially over the Congo Basin and south-eastern China. It is almost certainly this decline in precipitation relative to other datasets that contributes to the red-coloured "statistically significant" negative trend in P-E shown over these two regions in Figure 2.15. The trend is an artefact caused by an inherent high bias of precipitation in ERA5's assimilating model over the regions concerned, and by the assimilation of observations that are sufficient to correct this bias from around 2000 onwards. Confirmatory evidence is provided by the fact that precipitation over these regions increases during forecasts for dates in the post-2000 period, since the constraining effect of the observations reduces as the forecast proceeds. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. With the description of the confidence level associated with the changes in P-E we referred to Hersbach et al. (2020) to mention the differences between ERA5 and GPCC/GPCP precipitation datasets.
6545	48	30	49	29	Also regarding the map based on ERA5 in Figure 2.15, the trends over the tropical ocean must be treated with caution. Precipitation over the tropical oceans is distinctly higher in the 2000s than earlier, and the fact that spin-down of this precipitation as the forecast range increases is also higher in the 2000s indicates again that assimilation of more-constraining observations is a factor. An additional contribution from a real effect cannot be ruled out, however. Various other aspects of the behaviour of P-E in ERA5 are reported by Hersbach and al.. A general conclusion was that more effort is needed to understand the findings. Without improved understanding I cannot support presenting the ERA5 P-E trend map in AR6. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section 1.5.2 (New developments in reanalyses) assesses the evolution of different reanalysis products. In this sense, we don't intend to provide detailed aspects of reanalyses. We provided information about the possible differences between products for P-E calculation.
6547	48	30	49	29	Global-mean P-E should be in essence zero, decreasing only very slowly over time to account for the increased moisture carried by a warmer atmosphere, and fluctuating slightly to increase and later decrease atmospheric moisture during El Nino events. The lower left time series of Figure 2.15 reflects the facts that MERRA-2 constrains P-E to be zero in the global average, and that the ECMWF model used to produce the ERA-20CM model simulations and ERA-20C has a reasonable balance between P and E, and one that is not disturbed by assimilating surface pressure and wind observations. All this has already been noted in the papers published by the producers. It also shows, as demonstrated by Hersbach et al.(2020), that ERA5 has quite a good balance between P and E from the mid-1990s to the mid-2010s, and is an improvement over the other reanalyses in this regard. But all this is more for those interested in the detailed workings of reanalysis, rather than the general reader of AR6. Likewise the other two panels of Figure 2.15. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Section 1.5.2 (New developments in reanalyses) assesses the evolution of different reanalysis products. In this sense, we don't intend to provide detailed aspects of reanalyses. We provided information about the possible differences between products for P-E calculation.
9929	48	30	49	29	section 2.3.1.3.5 "Precipitation minus Evaporation" This section is considerably overlapping with section 8.3.1.1 "P-E over land and oceans" ch.8. Coordination is needed also with respect to the figures. In Ch.2 "although changes in P-E exhibit a very likely "wet-get-wetter, dry-get-drier" pattern over the tropical oceans." In Ch.8 "In the tropics, there is evidence of increasing P-E in the wet regions and decreasing P-E in the dry regions over land (medium confidence), although this enhanced spatial contrast in P-E is more obvious over the tropical oceans (high confidence, see also Chapter 2). These conclusions should be coordinated, also with respect to the usage of calibrated language. [ Olga Zolina, France]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E. Consistency between both chapters has been checked.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79971	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 I raised this point for the FOD but it has not been addressed. It needs to be changed this time around as the statement as included at present is inconsistent with the last assessment and likely to cause confusion. [ Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The statement comes from AR5 Chapter 3 p. 273, section 3.3.4: "both the mean salinity pattern and the distribution of mean evaporation – precipitation (E – P; Figure 3.4) indicates, with medium confidence, that the large-scale pattern of net evaporation minus precipitation over the oceans has been enhanced." "It is very likely that since the 1950s, the mean regional pattern of upper ocean salinity has been enhanced: saline surface waters in the evaporation-dominated mid-latitudes have become more saline, while the relatively fresh surface waters in rainfall-dominated tropical and polar regions have become fresher."
7223	48	32	48	34	How to account for this statement?. How salty is 'more salty' or how fresh is "fresher". Here, an accurate number of salinity should be cited in order to reduce the obscurity of the text. The second part of the paragraph is less clear as well. The cited research have high uncertainty, thus the results can be categorized as 'low confidence' result. [ Asaad Irawan, Indonesia]	Taken into account. The AR5 summary statement is a way to provide a baseline to compare the results from the AR6, therefore, is not intended to provide detailed numbers given that this section focus on P-E changes and not salinity changes directly. The summary statement from this section was categorized as "low confidence".
67827	48	32	48	34	There is a need for an explanation regarding this statement: How salty is 'more salty' or how fresh is "fresher". In this regard, an accurate value of salinity should be cited in order to reduce the obscurity of the text. The second part of the paragraph is also not clear. The cited research have high uncertainty, thus the results can be categorized as 'low confidence'. [ Ruandha Agung Sugardiman, Indonesia]	Taken into account. The AR5 summary statement is a way to provide a baseline to compare the results from the AR6, therefore, is not intended to provide detailed numbers given that this section focus on P-E changes and not salinity changes directly. The summary statement from this section was categorized as "low confidence".
58183	48	40	48	49	specify the letter of plots that are being described in the text [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The reference to the figure was modified according to the suggestion.
105675	48	48	48	48	"Over the oceans, a negative global trend of P-E is identified.." I have a hard time parsing this statement. By negative do you mean that E has increased in isolation? In reality both the P and E fields are changing, and not consistently across the globe. This statement would benefit from an expansion with more definitive text. Another editorial point to note, globally E is a larger term than P, so E-P may be a more accurate way to present the quantity, however I do note that this would then introduce a disconnect with notation through Ch8 and prior literature [ Paul Durack, United States of America]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E.
37859	48	48	49	2	It might be worthwhile to mention that the diverse global trend of P-E might be caused by the cancelling between positive P-E over the equator and negative P-E over the mid-latitude. [ Junhee Lee, Republic of Korea]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E.
82305	48	48	49	29	Here, the conclusion that wet-get-wetter and dry-get-dry is very likely originates from two papers using GCM and results based on ERA5 only. Thus, I propose to include more evidence here and add a zonal plot of trend estimates from recent reanalyses to figure 2.15, similar as in figure 8.4 of chapter 8. [ Schröder Marc, Germany]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence and the suggestion is no longer applicable.
82303	48	49	48	49	I propose to change "...and over 1988-2005 in satellite data (Andersson et al., 2011)." into "...and in satellite data (Andersson et al., 2011; Robertson et al., 2014). Though a general decrease in satellite-based global E-P is observed, Robertson et al. (2014) found considerable differences in the variability of the estimations since 1979." Ref.: Robertson, F.R., M.G. Bosilovich, J.B. Roberts, R.H. Reichle, R. Adler, L. Ricciardulli, W. Berg, and G.J. Huffman, 2014: Consistency of estimated global water cycle variations over the satellite era. J. Climate, 27, 6135–6154, <a href="https://doi.org/10.1175/JCLI-D-13-00384.1">https://doi.org/10.1175/JCLI-D-13-00384.1</a> [ Schröder Marc, Germany]	Taken into account. The sentence was modified based on new assessed evidence and the suggestion is no longer applicable.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
115991	48		48		Table 2.6 shows uncertain sign of trends for 1960-2018, and more robust increasing trends for the recent period 1980-2018. This is not exactly reflected in the summary statement, suggesting "a faster increase" observed since the 1990s. Please check when is the "inflection point" where positive trends are detected with confidence. The statement suggests an acceleration, while I have the impression that, on climate time scales (30 years), it is just the emergence of a trend from a state without a clear trend. [ Valerie Masson-Delmotte, France]	Taken into account. The summary statement was modified accordingly and accounts for a new evidence, also trend estimates were recomputed consistently with updated global records.
18327	48		49		Given the large amount of literature on historical aridity and drought changes (see Dai and Zhao 2017 and Dai et al. 2018 and cited refs there), I'm surprised to see no discussion on this important topic on these pages, while the less studied P-E over oceans is covered by a sub-section! Relevant refs.: . Dai, A. and T. Zhao, 2017: Uncertainties in historical changes and future projections of drought. Part I: Estimates of historical drought changes. Climatic Change, 144, 519–533. DOI: 10.1007/s10584-016-1705-2. Dai, A., T. Zhao, and J. Chen, 2018: Climate change and drought: A precipitation and evaporation perspective. Current Climate Change Reports, 4, 301-312. DOI: 10.1007/s40641-018-0101-6. ( <a href="http://link.springer.com/article/10.1007/s40641-018-0101-6">http://link.springer.com/article/10.1007/s40641-018-0101-6</a> ) [ Aiguo Dai, United States of America]	Rejected - outside the scope of the chapter. Assessment of aridity and drought is the purview of Chapters 8, 11, 12 and is not covered here.
36355	49	1	49	2	In addition to the tropical oceans, zonally averaged GPCP data show a "wet get wetter, dry get drier" pattern in broad latitude bands--as well as poleward expansion of climate zones. See K Marvel and C Bonfils (2013) Identifying external influences on global precipitation, Proceedings of the National Academy of Sciences 110 (48), 19301-19306. [ Curt Covey, United States of America]	Taken into account. The suggested literature has a mechanistic focus and we decided not to include in the section.
70255	49	1	49	2	I question the use of this terminology ("wet gets wetter") when analysing data over such a short period of time as I believe that natural PDO/ENSO variability has contributed largely to this spatial structure. This possibility is also raised in CH8, see lines 22-28 of page 32 in the SOD and can be viewed as an inconsistency. [ Shayne McGregor, Australia]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E.
113119	49	1	49	8	The conclusion about the 'dry gets drier and wet wetter' seems oversimplified. One of the papers cited as supporting that thesis is Greve at Seneviratne, which reads in its Abstract 'Only 10.8% of the global land area shows a robust 'dry gets drier, wet gets wetter' pattern, compared to 9.5% of global land area with the opposite pattern, that is, dry gets wetter, and wet gets drier.' Please correct. [ Diego Miralles, Belgium]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E.
81197	49	1	49	29	In addition to salinity data, paired measurement of salinity and d18O of sea surface water could provide better information about the hydrological cycle. Long term d18O surface water data is now available ( <a href="http://data.giss.nasa.gov/o18data/">data.giss.nasa.gov/o18data/</a> ) which may be useful to address the issue of inconsistency by reanalysis data. [ Supriyo Chakraborty, India]	Noted. Section 2.3.3.2 assess ocean salinity changes in a more detailed way.
113123	49	1	49	29	The general neglect of evaporation (also over oceans) seems striking to me. It is an equally large flux as P at the global scale and receives barely any attention. Evaporation is the direct link between the energy and water cycles, responds quick to radiative forcing changes and triggers all other hydrological impacts (including all those caused by P). It furthermore regulates land feedbacks on temperature, specially during drought and heatwaves. In this chapter E it is limited to a role of 'P correction' to achieve P-E, to then draw conclusions on the "wet-get-wetter, dry-get-drier" that I do not think are supported by current literature as mentioned above. [ Diego Miralles, Belgium]	Taken into account. More evidence was included in the assessment regarding P-E over the oceans. The section considers equally both precipitation and evaporation changes. Section 8.3.1.4 covers the changes in land-surface evapotranspiration, while section 8.3.1.6 covers the changes in soil moisture.
105677	49	2	49	8	"The surface salinity trends are found to be more spatially coherent." It's not just the near-surface (not surface, as most measurements are ~5 m depth or deeper, other than satellite "measurements") but also the deeper ocean (see Durack and Wijffels 2010 doi: 10.1175/2010JCLI3377.1 amongst numerous other estimates: Curry et al 2003 doi: 10.1038/nature02206; Boyer et al 2005 doi: 10.1029/2004GL021791; Hosoda et al 2009 doi: 10.1007/s10872-009-0049-1) with more recent ~1992 onward trends also for the abyssal (>2000 m) ocean also now available (see Purkey and Johnson 2013 doi: 10.1175/JCLI-D-12-00834.1) [ Paul Durack, United States of America]	Noted. Section 2.3.3.2 assess ocean salinity changes in a more detailed way. We modified the sentence to clarify that salinity trends were quantified for near-surface.
29905	49	19	49	19	Insert a comma in "...2019) CFSR" after the closing parenthesis. [ Hernan Edgardo Sala, Argentina]	Editorial - copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45229	49	26	49	29	"In conclusion, observational uncertainty yields low confidence in globally averaged trends in P-E, although changes in P-E exhibit a very likely "wet-get-wetter", "dry-get-drier" pattern over the tropical oceans". This sentence appears to be inconsistent with the Executive Summary statement of Chapter 2 (page 5, line 34) which says "The global hydrological cycle has strengthened since atleast 1980 (high confidence)". This internal inconsistency within Chapter 2 may be resolved. [ Krishnan Raghavan, India]	Noted. We ensured consistency between section 2.3.1.3.5 and the Executive Summary.
70257	49	27	49	27	Again, I question the use of the terminology "wet gets wetter, dry gets drier", especially when analysing data over a short period of time (40 yrs). I believe that it is an over-simplification. [ Shayne McGregor, Australia]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E.
113121	49	27	49	27	What do you mean by "wet-get-wetter, dry-get-drier" pattern over the tropical oceans? Oceans are always wet. I certainly believe there need to be more work on this entire section [ Diego Miralles, Belgium]	Taken into account. Section 2.3.1.3.5 was substantially modified considering the assessment of new evidence for the evaluation of global changes in P-E.
37861	49	27	49	27	Authors may want to cite Held and Soden (2006), which first denote the 'WWDD' mechanism. Held, I. M., & Soden, B. J. (2006). Robust responses of the hydrological cycle to global warming. Journal of climate, 19(21), 5686-5699. [ Junhee Lee, Republic of Korea]	Taken into account. The assessment is mostly based on new literature published after AR5. In this sense, we prefer to avoid citing literature that was previously cited in past IPCC reports.
18319	49	32	50	13	Please note: while dams may greatly alter the seasonal cycle of the river streamflow, its annual-mean flow rates are usually not significantly affected by dams (unless the reservoirs lead to a significant evaporative water loss). This is because the water mass has to be balanced on an annual basis before and after the damming. In fact, Dai et al. (2009) showed that for most of world's large rivers, direct human influences (through damming or withdrawal of streamwater, etc.) are small compared with natural variations and changes. I would not trust any reanalysis products for simulating precipitation, evaporation and streamflow. Again, any historical precipitation and streamflow changes should not be interpreted as a response to historical external forcing such as increases in CO <sub>2</sub> . Relevant ref.: Dai, A., T. Qian, K. E. Trenberth, and J. D. Milliman, 2009: Changes in continental freshwater discharge from 1949-2004. J. Climate, 22, 2773-2791. [ Aiguo Dai, United States of America]	Taken into account. The text was revised highlighting the minor influence human activities on long-term streamflow trends. The reference provided was included in the section.
9931	49	32	50	13	section 2.3.1.3.6 "Streamflow" Effective coordination with section 8.3.1.5 "Runoff, streamflow and floodings" ch.8 is needed. Conclusions are different and controversial in some places. [ Olga Zolina, France]	Taken into account. Section 2.3.1.3.6 was modified considering the assessment of new evidence for the evaluation of global streamflow changes. Consistency between both chapters was ensured for the FGD.
68047	49	32	50	13	section 2.3.1.6: are there no analyses of streamflow from historical ESM simulations to give this summary of observations some context? Should we expect to see changes in streamflow if not for the difficulty of doing so given damming, land use changes, changes in instrumentation, etc? [ Michael Evans, United States of America]	Noted. Streamflow simulations are assessed in Chapter 8 and are outside the scope of Chapter 2.
17087	49	34	49	35	Authors noted that non-climatic human influences such as dam construction or land-use change result in low confidence to the assessment of changes in global streamflow. It is totally true for dam construction case, as the flow is controlled by human. However, it is still useful to present flow change although there is land use modification, is not it? At least authors can present some data to support our low confidence state. As far as my knowledge, please correct me if I am wrong, land use change can occur naturally too but stream flow study is still can be conducted. There are many hydrological papers that study stream flows in decades period, e.g. from 1980 to 2010. The land use indeed changed during the studied period, but it is usually can be taken into consideration. Furthermore, please compare this statements to those in Chapter 3, Section 3.3.23 too. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Streamflow simulations analysing changes in land use and other effects are assessed in Chapter 8. The statements from Chapter 3 were compared to ensure consistency across the report.
73521	49	37	49	37	Capital 'W' for 'world'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
19729	49	50	49	52	The influence of ENSO on precipitation was not mentioned in subsection 2.3.1.3.4; one would expect however that this ENSO influence is fairly global. [ philippe waldteufel, France]	Noted. The influence of ENSO on global precipitation can be found in the Technical Annex IV, section AIV2.3

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5623	49	50	49	52	Of course, the streamflow variability is modulated by ENSO and PDO, but not only. For example, for the European and French rivers, the NAO influences the hydrological variability. Moreover, for example in The Mississippi, it is the combining of the several oscillations who modulates the hydrological variability. Cf ref biblio: Massei et al., 2011 International journal of climatology; Rossi et al., 2011 Global Planetary Change; Fritter et al., 2012 CR Geoscience; Chevalier et al., 2014 Hydrological Sc. J.; Laignel et al., 2010 IAHS publ, Massei et al., 2017 Journal of Hydrology...and other... [ Benoit Laignel, France]	Taken into account. The objective of this section is to provide an assessment of global streamflow and its main drivers. For a detailed description of regional changes in streamflow please refer to Chapter 8, section 8.3.1.5
99583	49	54	49	54	Three representative papers about increase in Northern Hemisphere high latitude river discharges should be cited: Shiklomanov, A. I., and R. B. Lammers, 2009: Record Russian river discharge in 2007 and the limits to analysis. Environ. Res. Lett., 4, 045015; Rawlins, M. A., M. Serreze, R. Schroeder, X. Zhang, and K. C., McDonald, 2009: Diagnosis of the record discharge of Arctic-draining Eurasian rivers in 2007. Environ. Res. Lett., 4, 045011, doi:10.1088/1748-9326/4/4/045011; Zhang, X., J. He, J. Zhang, I. Polaykov, R. Gerdes, J. Inoue, and P. Wu, 2013: Enhanced poleward moisture transport and amplified the northern high-latitude wetting trend. Nature Climate Change, 3, 47-51, doi: 10.1038/nclimate1631. [ Xiangdong Zhang, United States of America]	Noted. The suggested literature has a regional focus and falls outside the scope of the assessment performed in the section.
52121	49	55	50	1	Are all references cited decoupled from river regulation and purely climatically driven? [ Kathryn Fitzsimmons, Germany]	Taken into account. The references account for natural and human-related variations in streamflow. However, it was clarified that the role of human activities on long-term annual discharge trends is negligible.
73523	49	56	49	56	Delete , after 'basin'. It is not required in this context. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
58185	49				Figure 2.15: horizontalize the label bar and move it to below the figure b). c) and d) would be easier to interpret [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The figure was improved for the FGD.
105519	50	1	50	1	contrast with would be better than are contrasting [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was modified based on new assessed evidence and the suggestion is no longer applicable.
58187	50	1	50	3	maybe list some regions after "in many regions...". If the regions don't fit in large-scale definition, then maybe move this sentence to latter chapters [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. We removed the sentence.
30265	50	4	50	5	'Uncertainties in global streamflow trends arise predominantly from the global role of human-controlled flow regulation and irrigation': to me this sentence should be clarified. It suggests that trends may exist (due to P-E trends on land?) but are hidden by the variability induced by human activities. I think if the variability of streamflow can be trusted, then the conclusion should be that there is no trend (to a great 'certainty'). Another aspect would be to relate streamflow variability to P-E variability on land, by correcting the former from human influence, but i understand this is currently not possible. I am not sure what is the exact meaning behind the sentence. [ Gilles Delaygue, France]	Taken into account. The sentence was revised according to your suggestion and the section was modified for FGD.
30267	50	6	50	7	'cautionary interpretation of trends from global streamflow databases is required': which interpretation? Again, this conclusion is confusing to me. Does it mean that data still contain too much uncertainty for analysis, or that trends of streamflows are not significant? [ Gilles Delaygue, France]	Taken into account. The sentence was revised according to your suggestion and the section was modified for FGD.
30269	50	10	50	11	'the absence of global streamflow databases without large-scale direct human interference gives low confidence to the assessment of changes in global streamflow during the 20th century.' For me this claim is wrong (whereas the above sentences are only confusing). An assessment is reliable if we have confidence in the data (i.e. small uncertainty on measurements), not because we cannot detect some expected trends (which are not found significant). About 10% of global streamflow ('blue waters'), 30% of global evapotranspiration ('green waters') are affected by human activities, hence i cannot see the point of this conclusion. [ Gilles Delaygue, France]	Taken into account. The sentence was revised according to your suggestion and the section was modified for FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
45231	50	10	50	20	It may be noted that "Chapter 8 assesses large-scale as well as regional aspects of circulation components"; not just regional aspects. This sentence may be suitably modified. [ Krishnan Raghavan, India]	Taken into account. The sentence was modified according to your suggestion.
4629	50	11	50	13	Add on to sentence "..., due to non-climatic influences." [ Andries Kruger, South Africa]	Taken into account. The summary statement for this section was modified based on new assessed evidence and the suggestion is no longer applicable.
127031	50	16	56	23	Could there be more discussion of which changes manifested in the observations are consistent with anthropogenic climate change? Are all of the changes catalogued here consistent with higher radiative forcing? For example, there's discussion of the stability of the polar vortex: What do we expect under climate change? There needs to be more context and relation to the major issue, not just accounting of what's changing. [ Trigg Talley, United States of America]	Rejected. Outside the scope of Ch.2. Attribution is covered in Ch.3, future projection is covered in Ch.4.
19731	50	18	50	22	This paragraph is nearly identical to the introductory paragraph when beginning subsection 2.3.1.3, creating a somewhat unpleasant feeling. Hopefully there is a way to spare these warning lines at the beginning of 2.3.1.4 [ philippe waldteufel, France]	Noted. Editorial.
9935	50	24	51	20	section 2.3.1.4.1 "Tropical circulation characteristics". Coordination with sections 8.3.2.2 and 8.3.2.3 ch.8 is needed, especially for the conclusions regarding Walker circulation. [ Olga Zolina, France]	Taken into account. We ensured consistency between Chapters 2 and Chapter 8 regarding changes in the Walker Circulation.
37089	50	24	52	5	A graph of some ENSO index is required in this section so that readers can observe any changes in patterns. [ John McLean, Australia]	Rejected. ENSO indices are described in section 2.4.2 (see Figure 2.36) and in the Technical Annex IV (section AIV2.3).
24415	50	25	52	5	There are some overlappings with 8.3.2.2 (CH8) for the assessment on Hadley circulation and Walker circulation. The confidence of assessment conclusion here for Walker circulation is inconsistency with that in 8.3.2.3. [ Zhou Botao, China]	Taken into account. The conclusions regarding the changes in the Walker circulation were modified according to the new assessed evidence. We ensured consistency with Chapter 8.
57657	50	27	50	27	„tropical band“ could be changed to “tropics” (as used e.g. in Seidel et al., 2008) or “tropical belt” (as in Alfaro-Sanchez et al., 2018). Alfaro-Sánchez, R., Nguyen, H., Klesse, S., Hudson, A., Belmecheri, S., Köse, N., ... & Trouet, V. (2018). Climatic and volcanic forcing of tropical belt northern boundary over the past 800 years. Nature Geoscience, 11(12), 933-938. Seidel, D. J., Fu, Q., Randel, W. J., & Reichler, T. J. (2008). Widening of the tropical belt in a changing climate. Nature geoscience, 1(1), 21. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Changed to "tropical belt".
57725	50	27	50	27	evidence indicated a likely widening of the tropical band since the 1970s... Alfaro-Sanchez 2018 tree ring study refs another Nature study by Broennimann et al 2015 (DOI: 10.1038/NGEO2568) which identifies a contraction from 1945 to 1980 in the Northern tropical belt. Is this widening simply a return from the previous contraction? Does this recent expansion make up for earlier contraction? Could be construed as a positive trend without the wider context. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Due to limited evidence and differences among the datasets used it is hard to put Broennimann et al 2015 and the papers that analysed the expansion since 1979 in the same context. Chapters 3 and 4 evaluate the attribution of the recent widening and its future projections, factors that are outside the scope of Chapter 2.
71629	50	32	50	44	This section is very interesting and a nice succinct piece of information on the proxy records of the HC. There is a paper by Denniston et al. (2016), which provides an additional piece of research on the LIA displacment. This paper shows that during the LIA there is a contraction of the ITCZ and then also provides a width index for the last three Millennia, and should probably be added to this paragraph. [ Jessica Hargreaves, Australia]	Accepted. The reference was included according to your suggestion.
30271	50	32			'proxy records, most of which are indirect': which proxy could be a 'direct' one, since by definition it is an indirect record of climate? [ Gilles Delaygue, France]	Taken into account. The sentence was removed.
73525	50	33	50	33	Change 'trade winds' to 'Trade Winds'. [ Burt Peter, 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
83431	50	35	50	35	You could add the following reference, especially to include a study led by a South American scientist: Strikis, N.M., Cruz, F.W., Barreto, E.A.S., Naughton, F., Vuille, M., Cheng, H., Voelker, A.H.L., Zhang, H., Karmann, I., Edwards, R.L., Auler, A.S., Santos, R.V., Sales, H.R., 2018. South American monsoon response to iceberg discharge in the North Atlantic. Proceedings of the National Academy of Sciences, 115 (15) 3788-3793, doi 10.1073/pnas.1717784115. [ Antje H. L. Voelker, Portugal]	Accepted. The reference was included according to your suggestion.
105521	50	38	50	40	Sentence beginning " Tree ring ..." is very confusing [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was rephrased for the FGD.
37087	50	38	50	40	Not all ENSO indices have positive values indicating El Nino and negative indicating La Nina so take this into account in any discussion. [ John McLean, Australia]	Noted. Alfaro-Sanchez et al. (2018) quantified two ENSO indices (for eastern and central Pacific events) and the PDO index. Considering the annual resolution of tree rings, it is expected that there would exist a large agreement between ENSO indices in this time scale.
57727	50	39	50	39	this sentence is very close to what is written in the paper, perhaps this should be reworded, e.g. "the tropical belt was narrower (wider) during the warmer (colder) phases of el nino pp. 2-50 line 39 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence was rephrased according to your suggestion.
57781	50	39	50	40	The effect of ENSO on the position of the northern edge of the Hadley Cell is stated here, but the reference to Alfaro-Sanchez et al. (2018) also states that North American teleconnection patterns affects its position, so I would suggest also stating it here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence was modified to also include the effect of the PDV.
57659	50	40	50	40	"Similar chronologies" sounds odd, because Abram et al. (2014) use ice-core deuterium isotope record and temperature--sensitive proxy records for the Antarctic and South America continental region, but not tree-rings width like Alfaro-Sanchez et al. (2018). Abram, N. J., Mulvaney, R., Vimeux, F., Phipps, S. J., Turner, J., and England, M. H. (2014). Evolution of the Southern Annular Mode during the past millennium. Nat. Clim. Chang. 4, 564–569. doi:10.1038/nclimate2235. Alfaro-Sánchez, R., Nguyen, H., Klesse, S., Hudson, A., Belmecheri, S., Köse, N., ... & Trouet, V. (2018). Climatic and volcanic forcing of tropical belt northern boundary over the past 800 years. Nature Geoscience, 11(12), 933-938 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence was removed from the assessment.
73527	50	41	50	41	Define 'SAM'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was rephrased and now there is no need to define SAM.
83433	50	41	50	41	I believe the acronym SAM is used here for the first and has not been defined, yet. [ Antje H. L. Voelker, Portugal]	Noted. The sentence was rephrased and now there is no need to define SAM.
24379	50	42	50	42	It's not clear which hemisphere is being discussed. A southward shift in the Hadley Cell in the northern hemisphere would mean it is moving towards the equator. But a southward shift in the southern hemisphere would mean it is moving toward the polar region. Please specify the hemisphere. [ Owen Cooper, United States of America]	Taken into account. The sentence was rephrased according to your suggestion.
18123	50	42	50	43	Note that there are robust studies which support an equatorward contraction (rather than simply displacement) of the ITCZ and strengthened Walker Circulation during LIA, with the opposite occurring during MWP. See Griffiths et al 2016, Nature Communications DOI: 10.1038/ncomms11719 or Asmerom et al 2020 Science Advances DOI: 10.1126/sciadv.aax3644 [ Ersek Vasile, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We included a sentence based on the evidence of the contraction of the ITCZ.
57661	50	43	50	43	Study by Lechleitner et al. (2017) can be cited, as it reconstructs ITCZ migration dynamics during the previous two millennia and combines 25 precisely-dated high-resolution paleorainfall records into overall ITCZ-stack, by bringing them onto a common timescale and averaging their signal. The pronounced southward shift of the ITCZ is unequivocally confirmed. Lechleitner, F., Breitenbach, S., Rehfeld, K. et al. Tropical rainfall over the last two millennia: evidence for a low-latitude hydrologic seesaw. Sci Rep 7, 45809 (2017). <a href="https://doi.org/10.1038/srep45809">https://doi.org/10.1038/srep45809</a> [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The reference was included according to your suggestion.
90339	50	54			Hemispheres should be lower case here. [ Jeannine-Marie St-Jacques, Canada]	Editorial - copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4723	50		51		<p>"Widening of Hadley circulation (HC) has drawn large attention over the past studies. The authors give a nice review on the recent processes of this issue. From my understanding, the HC is deduced from the globally zonal average, it inevitably hides the regional diversity as its variations and dominant mechanisms. However, the HC variation and its relation to regional meridional circulation (RMC) are not mentioned in current version. Two recent studies demonstrated the regional characteristic of HC variations.</p> <p>At the interannual time scale, there are studies trying to shed light on the regional cause of HC that comes from the RMC within monsoon domains. Sun and Zhou (2014) pointed out two leading models of variation of HC in boreal summer is under the control of East summer monsoon circulation in developing and decaying phases of ENSO. Sun et al. (2019) further show (a) the diverse contribution of RMCs within global monsoon domain to variations in strength and extent of HC (b) distinguish the geographic sectors where dominates the variations in HC strength from those responsible for the HC extent; (c) assess the relative role of ENSO and mid-latitude eddy in shaping HC variations (d) distinguish the geographic sectors where ENSO plays the dominant role from those dominant role played by mid-latitude eddy.</p> <p>References:  Sun, Y., and T. J. Zhou, 2014: How does El Niño affect the interannual variability of the boreal summer Hadley circulation? J. Climate, 27, 2622–2642, doi:https://doi.org/10.1175/JCLI-D-13-00277.1.  Yong Sun, Laurent Z. X. Li, Gilles Ramstein, Tianjun Zhou, Ning Tan, Masa Kageyama, and Shaoyin Wang. (2019) Regional meridional cells governing the interannual variability of the Hadley circulation in boreal winter. Climate Dynamics 52:1-2, 831-853." [ Yong Sun, China]</p>	Noted. We thank the reviewer for the suggested literature. However, the regional aspects of the HC expansion are assessed in section 8.3.2.2.
67671	51	1	51	2	<p>It is not simply biases, but also which metric is used. Grise et al 2019 shows that agreement between models and reanalyses is better using the surface wind metric than the stream function metric. [ Karen Rosenlof, United States of America]</p>	Taken into account. The biases are expected to affect the reliability in the representation of diverse atmospheric circulation features that are observed in the metrics used for the description of the Hadley Circulation.
57729	51	2	51	3	<p>Study Feng et al 2016 is CMIP5 study, discusses differences in HC among coupled models, does not invoke a comparison of reanalysis data sets (only uses one reanalysis dataset for comparison with model outputs: 'The results show that most of 26 models perform well in simulating the spatial structure of the climatology of the annual mean Hadley circulation, but the results derived from these models are generally weaker than that derived from the reanalysis dataset.' Should use only the Allen and Kovilakam study. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]</p>	Rejected. Feng et al. (2016) also discusses differences between several reanalyses products. Reference: Feng, J., Zhu, J., and Li, F. (2016). Climatological Vertical Features of Hadley Circulation Depicted by the NCEP/NCAR, ERA40, NCEP-DOE, JRA25, ERA-Interim, and CFSR Reanalyses. Sola 12, 237–241. doi:10.2151/sola.2016-047.
57731	51	2	51	3	<p>Is it appropriate to point out discrepancies in the poleward edge of the HC between metrics? Does a difference between metrics of the HC truly correspond to an uncertainty of HC extent? Or does it simply correspond to different things being measured? I suggest removing metrics from this sentence and using only the Allen and Kovilakam 2017 reference. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]</p>	Taken into account. Several papers have revisited the relationship between the different metrics to describe the variations in the position of the HC, finding that the subtropical sea level maximum, the subtropical transition between surface easterlies and westerlies, and the subtropical transition from net evaporation to net precipitation closely covary with the commonly used zero-crossing of the 500-hPa mass stream function (Davis & Birner, 2017; Staten et al., 2018; 2020; Waugh et al., 2018). Those metrics are subjected to uncertainties given that its latitudinal averages differ between hemispheres.
105523	51	3	51	3	<p>"reanalyses analysed" should be rephrased [ Heather Pardoe, United Kingdom (of Great Britain and Northern Ireland)]</p>	Editorial. The sentence was rephrased

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57783	51	5	51	7	The magnitude of change of the Hadley Cell position does not show a strong correlation and I think it would be useful to explicitly state that the positive trend of Hadley Cell position in the Northern Hemisphere is weak, with interannual variability superimposed on this, [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The correlation of the magnitude of change of the HC position is metric dependent (see for example Davis and Birner, 2017; DOI: 10.1175/JCLI-D-16-0371.1; see also response to comment 57731). The rate of expansion was assessed as 0.1° to 0.5° lat/decade. We included a sentence to clarify regarding the uncertainty in the HC edge over the NH and its interannual variability.
30273	51	6			'sign and estimated magnitudes': of what, streamfunction or meridional trend? [ Gilles Delaygue, France]	Noted. The agreement in sign and magnitude is referred to the trends.
57785	51	15	51	17	It might also be worth noting here that each reanalysis product produces Hadley Cell 'intensity' and 'position' of the same sign i.e. increased intensity will also lead to a polewards movement of the Hadley Cell. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. However, given the low confidence on reanalysis products for the representation of the intensity of HC, the suggested relationship might be inaccurate.
57787	51	15	51	17	A reference to Figure 2.16 should be made here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. A reference to the figure was added.
57733	51	23	51	23	fig 2.16 the NH label needs to be moved down into the y-axis, currently conflicts with sup title. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The new Figure 2.17 was improved for the FGD.
19733	51	23	51	33	This legend of figure 2.16 can be corrected and made simpler by replacing "(top right) NH and (bottom right) SH Hadley Cell intensity" by "intensity (right)" on line 26. At the same time care should be taken so that the "NH" annotation on the right side of the figure is not obliterated by the title. [ philippe waldteufel, France]	Taken into account. The new Figure 2.17 was improved for the FGD.
26055	51	25	51	26	Please consider replacing "Northern Hemisphere (NH) and Southern Hemisphere (SH) Hadley Cell extent (left) and (top right) NH and (bottom right) SH Handley Cell intensity" by "Northern Hemisphere (top) and Southern Hemisphere (bottom) Hadley Cell extent (left) and intensity (right)"... [ Don Alfonso Pino Maeso, Spain]	Taken into account. The new Figure 2.17 was improved for the FGD.
57789	51	25	51	31	The title of Figure 2.16 should state 'Mean Annual Hadley Cell Extent and Intensity' to make it absolutely clear you are talk about mean position rather than including seasonal variability. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. The legend of the new Figure 2.17 clarifies that it is the annual mean for both characteristics.
30275	51	29			'weakened overturning peak value': weakened by how much? Please explain or give a reference to a technical description of this metric. [ Gilles Delaygue, France]	Taken into account. Details on data sources and processing are available in the associated FAIR data table (Table SM2.Figure 2.17).
30277	51	30			'maximum of the vertically average' instead? [ Gilles Delaygue, France]	Taken into account. Details on data sources and processing are available in the associated FAIR data table (Table SM2.Figure 2.17).
30279	51	41	51	42	Given the 'considerable decadal variability', how could a 'suggested' trend over the period 1972-1998 'supports' a trend over the period 1920-2010? This has non sense to me. [ Gilles Delaygue, France]	Taken into account. The sentence was removed due to the inconsistency.
57663	51	42	51	43	"...overall reduction in the east-west SST gradient over 1972-1998." Could you provide the reference? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Based on comment 30279 the sentence was removed from the assessment.
81199	51	43	51	43	A suitable paper in support of this statement may be cited. [ Supriyo Chakraborty, India]	Taken into account. Based on comment 30279 the sentence was removed from the assessment.
37863	51	51	51	54	Authors may want to add the information that the weakening in the Atlantic Ocean is related to the strengthening of the Pacific Walker Circulation during recent 20 years (McGregor et al. (2014)). McGregor, S., Timmermann, A., Stuecker, M. F., England, M. H., Merrifield, M., Jin, F. F., & Chikamoto, Y. (2014). Recent Walker circulation strengthening and Pacific cooling amplified by Atlantic warming. <i>Nature Climate Change</i> , 4(10), 888-892. [ Junhee Lee, Republic of Korea]	Taken into account. The suggested literature was included in the assessment.
30281	51	52	51	53	This latter conclusion of a weakening in Atl+Indian oceans and little change in the Pacific seems to contradict the conclusion few lines above of a strenghtening? [ Gilles Delaygue, France]	Noted. We rephrased the sentence including new evidence that supports the seasonal strengthening of the Walker circulation.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57735	51	56	52	1	In summary, there has been a very likely widening of the Hadley Circulation since the 1980s' again, not sure this should be identified as important trend in absence of the context of narrowing during the precedent decades. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Due to limited evidence and differences among the datasets used it is hard to put Broennimann et al 2015 and the papers that analysed the expansion since 1979 in the same context.
57791	51	56	52	1	With reference to Figure 2.16, I suggest using the term 'poleward widening' of the Hadley Circulation to ensure the description is accurate. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. In the context of the Hadley Circulation expansion, it is redundant to clarify that the cells expanded towards the poles. 'Widening' might be uncertainly interpreted in this context.
1229	51	56	52	5	The strengthening in the Hadley cell can be related to the upper tropical tropospheric temperature trends noted in 2.3.1.2.2 (p. 42 L.46-47) [ Rasmus Benestad, Norway]	Noted. A cross reference to section 2.3.1.2.2 would imply that we are attributing the strengthening to the tropical tropospheric temperature trends, which is outside the scope of the chapter.
73529	52	2	52	2	Change 'northern hemisphere' to 'Northern Hemisphere' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
73531	52	3	52	3	Capital 'C' for 'circulation' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
73533	52	4	52	4	Capital 'C' for 'circulation' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
73535	52	5	52	5	Capital 'C' for 'circulation' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
115527	52	8	52	8	Global monsoons are important, but a significant fraction of the worlds population (arguably close to 50%) is affected by the Indian summer monsoon. So it might be worth highlighting this region more than in the present draft. It has been argued that the elevated aerosol layer over South Asia (the so-called ATAL) worsens Indian droughts (Fadnavis et al., Sci. Reports, 9:10268, 2019) [ Rolf Müller, Germany]	Noted. Regional monsoons are covered in Chapters 8 and 10 and are beyond the scope of the assessment performed here.
39923	52	8	52	8	Add a definition to the glossary for 'Global monsoon' [ TSU WGI, France]	Accepted. The definition of "Global monsoon" was provided to the Glossary.
15169	52	8	52	51	The imbalance in the length of monsoon section (2.3.1.4.3) and the extratropical jets/storm tracks section (2.3.1.4.3 - more than twice as long), and lack of material on south asian monsoon is noticeable. I'd strongly recommend expanding the monsoon assessment. Otherwise this seen by readers and governments as a result of a developed world bias of the author team. [ Simon Donner, Canada]	Noted. Regional monsoons are covered in Chapters 8 and 10 and are beyond the scope of the assessment performed here. We updated the section with new literature, broadening the assessment of global monsoon.
3517	52	8	52	51	It would be worth assessing the recent large-scale comprehensive review of Bin Wang et al. in BAMS: "Monsoon Climate Change Assessment". The review includes expert assessment of observed trends in the global monsoon and offers further evidence to the statements made here regarding the 1950s-1980 decline in global land monsoon precipitation, mainly arising from the northern hemisphere and an intensification thereafter. See <a href="https://doi.org/10.1175/BAMS-D-19-0335.1">https://doi.org/10.1175/BAMS-D-19-0335.1</a> [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The suggested literature was included in the section.
17923	52	8	52	52	somewhere in this section [2.3.1.4.2] the role of aerosols in weakening both the South Asian and West African monsoons should be discussed. While the weakening may have occurred during different epochs in the 20th century, and the aerosol source may be different, this weakening is an important counterpoint to the more recent strengthening that is emphasized in this section, e.g., in the final paragraph of the section, [ Alessandra Giannini, France]	Noted. The role of aerosols in the weakening of the South Asian and West African monsoons is covered in Chapter 10.
95905	52	9	52	12	Tropical (Global) areas of monsoons are well defined, and there should be no uncertainty on this. It is true many studies use reanalysis data sets. For Typical Monsoon areas like Asia (Centred over Indian Sub-continent, there is substantial conventional (in-situ) data sets which has been used in many studies. Such studies should have been assessed in CH2 to reduce the uncertainty issue. [ Joseph Mutemi, Kenya]	Noted. The uncertainty in the definition of global monsoon comes not only from the different reanalyses or gridded precipitation products used for domain delineation but also from the metric used for definition. We provided a definition of global monsoon to the Glossary. Regional monsoons are covered in Chapter 8 and are beyond the scope of the assessment performed here.
1231	52	9	52	12	Be more specific on the monsoon circulations and mention each region: Southeast Asian Monsoon, +? [ Rasmus Benestad, Norway]	Noted. Regional monsoons are covered in Chapters 8 (including specific Supplementary material) and 10 and are beyond the scope of the assessment performed here.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3501	52	10	52	11	Uncertainties in reanalysis products would not tell us anything about confidence in observed rainfall trends, since reanalyses (hopefully) are not being used to measure rainfall trends. Sentence needs refining to ensure intended meaning is presented. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The uncertainty in reanalysis products is related to the monsoon circulation changes.
37865	52	14	52	14	Readers want to know the definition of the GM defined as the difference in the seasonal mean precipitation. [ Junhee Lee, Republic of Korea]	Not applicable. The definitions were removed from section 2.3.1.4.2. A definition of global monsoon can be found in the Glossary.
73537	52	15	52	15	Remove line break split of number and units [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The definitions were removed from section 2.3.1.4.2. A definition of global monsoon can be found in the Glossary.
30283	52	15	52	16	Suggestion: 'with local' ... 'and the local' [ Gilles Delaygue, France]	Not applicable. The definitions were removed from section 2.3.1.4.2. A definition of global monsoon can be found in the Glossary.
3503	52	17			By results do you mean "similar domains"? Be specific - no other have been discussed in this paragraph. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The definitions were removed from section 2.3.1.4.2. A definition of global monsoon can be found in the Glossary.
3505	52	18	52	20	Be explicit whether this is the index to be used and discussed in remainder of this chapter/subsection. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The definitions were removed from section 2.3.1.4.2. A definition of global monsoon can be found in the Glossary.
57793	52	18	52	20	The question that leads on from this section if when are these definitions used and for what reason? In what capacity do they influence the results of different studies? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. The definitions were removed from section 2.3.1.4.2. A definition of global monsoon can be found in the Glossary.
18125	52	22	52	32	A recent study by K. Thirumalai (GRL 2020 <a href="https://doi.org/10.1029/2020GL087613">https://doi.org/10.1029/2020GL087613</a> ) that while precession modulates indeed the millennial scale variability in methane, but not in Chinese speleothem $\delta^{18}O$ which show more similarities with Antarctic $\delta^{18}O$ record. [ Ersek Vasile, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The suggested literature deals more with regional monsoon systems, which is outside the scope of Chapter 2.
30285	52	22			'New research based on high-resolution proxy evidence shows the influence of orbital cycles': such influence was shown 40 yrs ago (eg, Kutzbach 1981), so this sentence is too vague, and this paragraph should address only new results since AR5. [ Gilles Delaygue, France]	Noted. The sentence was rephrased to emphasize the results based on new evidence. All the analysed papers were published between 2014 and 2020, therefore, showing new results since AR5.
5363	52	22			"... of orbital cycles on millennial time scales." [ Bryan Weare, United States of America]	Accepted. The sentence was modified according to your suggestion.
26631	52	25	52	25	We suggest to mention supporting previous results of Toucanne et al. (2015) for the last 0.5 Myr: Toucanne, S., Minto'o, C. M. A., Fontanier, C., Bassetti, M. A., Jorry, S. J., & Jouet, G. (2015). Tracking rainfall in the northern Mediterranean borderlands during sapropel deposition. Quaternary Science Reviews, 129, 178-195. [ Eric Brun, France]	Taken into account. The suggested literature was included in the assessment.
127033	52	25	52	25	Extra space between "~" and "100". [ Trigg Talley, United States of America]	Editorial - copyedit to be completed prior to publication.
57665	52	25	52	28	Gebregiorgis et al. (2018) argues against the hypothesis that NH tropical monsoon variability is dominated by and directly responds to NH summer radiation (see also Clemens et al., 2018). Instead, new South Asian Monsoon (SAM) precipitation record demonstrates that obliquity forcing has played a much larger role than previously considered and was triggered by Southern Hemisphere warming and cross hemispheric moisture transport. Gebregiorgis, D., Hathorne, E.C., Giosan, L. et al. Southern Hemisphere forcing of South Asian monsoon precipitation over the past ~1 million years. Nat Commun 9, 4702 (2018). <a href="https://doi.org/10.1038/s41467-018-07076-2">https://doi.org/10.1038/s41467-018-07076-2</a> Clemens, S.C., Holbourn, A., Kubota, Y. et al. Precession-band variance missing from East Asian monsoon runoff. Nat Commun 9, 3364 (2018). <a href="https://doi.org/10.1038/s41467-018-05814-0">https://doi.org/10.1038/s41467-018-05814-0</a> . [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The suggested literature deals more with regional monsoon systems, which is outside the scope of Chapter 2.
73539	52	26	52	27	References should be in chronological order [ Burt Peter, 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
3507	52	30	52	32	It is not obvious whether the statement made here on ENSO and PDV/AMV is a general one or related specifically to proxy evidence on orbital cycles, which is the subject of this paragraph. If it is related, it needs to be made clear. If it is not related, it needs to be in a different paragraph (e.g. the next one?) or introduced correctly. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The links between global monsoon precipitation and ENSO, PDV/AMO were removed from the text.
19735	52	31	52	31	Either "posited" is a typo, or it is a seldom used word unfamiliar to dictionaries [ philippe waldteufel, France]	Noted. The links between global monsoon precipitation and ENSO, PDV/AMO were removed from the text.
98753	52	31	52	31	What is PDV? I believe this is the first mention of it. Should put full name. [ Meredith Parish, United States of America]	Accepted. PDV was defined.
30287	52	31			Expand the acronyms 'PDV-AMO/V' since they have not been addressed so far in this Chapter [ Gilles Delaygue, France]	Noted. The links between global monsoon precipitation and ENSO, PDV/AMO were removed from the text.
30289	52	31			'posited': 'assumed' is maybe a more common term? [ Gilles Delaygue, France]	Noted. The links between global monsoon precipitation and ENSO, PDV/AMO were removed from the text.
29959	52	34	52	46	The case of West African monsoon, which underwent the worst drought of the instrumental record, should be mentioned. The instrumental record is dominated by multidecadal variability, whose decreasing and increasing trends have been successively attributed to climate change. However, a multicentennial record recently showed that the Sahel drought has dramatically increased in the past 200 years and emerged from the natural variability (Carré et al., Clim. Dyn. 2019). [ Matthieu Carré, France]	Noted. Regional monsoons are covered in Chapter 8 and are beyond the scope of the assessment performed here.
3509	52	41			Is the southern hemisphere statement that is made here covering the same period as the northern hemisphere in the sentence above, i.e. since 1979, or a longer period (and therefore underlining the lack of overall trend in the SH monsoons)? (This longer period relevance is implied by the closing confidence statement on P2-52 L51.) [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The summary statement was modified to clarify the uncertainty in long-term centennial GM precipitation trends.
80293	52	43	52	43	South African monsoon is not among the monsoons assessed in CH8 [ Paola Arias, Colombia]	Noted. The commonly used definitions for delineating the global monsoon indicate the existence of the South African monsoon. See figure 2 from Wang et al. (2017): <a href="http://dx.doi.org/10.1016/j.earscirev.2017.07.006">http://dx.doi.org/10.1016/j.earscirev.2017.07.006</a>
45235	52	48	52	50	"In summary, new evidence shows that GM precipitation has likely increased over the last 40 years, mainly due to a positive trend in the Northern Hemisphere summer monsoon precipitation (medium confidence)". This assessment is inconsistent with Chapter 3 (Page 32, lines 8-9) which says "In summary, there is medium confidence that anthropogenic aerosols contributed to weakening of global land summer monsoon precipitation intensity from the mid-to-late 20th century". Consistency in the assessment of the observed global monsoon changes between Chapters 2 and 3 is to be taken care. [ Krishnan Raghavan, India]	Taken into account. The decrease in GM precipitation documented both in CH2 and CH3 is for the period ~1940 to ~1980, while the recent increase is documented during satellite era (1979 to present). Therefore, the assessment is consistent in both chapters.
127035	52	53	53	33	Section 2.3.1.4.3 covers recent reseach quite well. [ Trigg Talley, United States of America]	Noted.
7979	52	53	54	26	Material here (Ch 2.3.1.4.3) Extratropical jets, stormtracks, and blocking: covers recent reseach quite well here. [ Anthony Lupo, United States of America]	Noted.
45233	52	56	53	1	"In summary, there has been a very likely widening of the Hadley circulation since the 1980s". On the other hand, it is mentioned in Chapter 8 (page 48, line 34) that "There is an almost certain expansion of the HC in both the hemispheres over the last several decades". Consistency in the assessment of the widening of Hadley circulation between Chapters 2 and 8 is to be taken care. [ Krishnan Raghavan, India]	Taken into account. We ensured consistency between Chapters 2 and Chapter 8 regarding changes in the Hadley Circulation.
115993	52		52		Coordination is needed for monsoons with ch 8 and 10 to avoid inconsistencies / duplication. [ Valerie Masson-Delmotte, France]	Taken into account. We ensured consistency between Chapters 2, 8 and 10 regarding changes in the global monsoon.
73541	53	2	53	2	Remove , after 'variables'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30291	53	2			'the HC widening' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
98745	53	8	53	8	Should Liefert and Shuman (2019) be cited here? It's a new North American lake-level database. <a href="https://doi.org/10.1029/2019GL086412">https://doi.org/10.1029/2019GL086412</a> [ Meredith Parish, United States of America]	Accepted. Cited to support a drier MH over western Northern America.
45301	53	8	53	8	Hermann et al. 2018 only focused on *Western* North America but not the whole North America. [ Anson Cheung, United States of America]	Accepted. Text modified to clearly indicate the western North America.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1233	53	9	53	9	What is 'MH'? Also, the extensive use of other acronyms is not good. [ Rasmus Benestad, Norway]	Noted. Editorial.
30293	53	9			'imply' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
30295	53	11			'and zonally symmetric': what is meant here? Maybe: 'were homogeneously stronger over 14-5 ka' [ Gilles Delaygue, France]	Noted. Text modified by clearly indicate "the westerly winds were stronger over 14-5 ka"
57795	53	16	53	17	Is it possible to state the magnitude of the poleward shift in the Pacific storm tracks? This would enable a comparison to changes in e.g, the Hadley Circulation. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Both works cited here focus on historical hydroclimate anomalies over north-western North America, where the temporal-spatial wet/dry conditions are indicative of the north-south shifting in storm tracks. However, the latitude locations of the storm tracks has not been reconstructed directly. We have modified the text slightly for clarity.
1953	53	16	53	23	The changes in storm tracks mentioned in this paragraph are based on limited evidence with relatively large regional disparities. To my knowledge, there is no study making a general synthesis and indicating a coherent, general shift in the position of the storm track during the Medieval Climate Anomaly. I would thus recommend to use at best 'low confidence' when this is mentioned in the technical summary (page 32) and in the executive summary of this chapter (page 5). [ Hugues Goosse, Belgium]	Accepted. Given the available evidence for the jet stream during Medieval Warm Period, the confidence level has been changed to 'Low'.
30297	53	17			'to the interval 1979-2015': 'to the one over the period 1979-2015' ? [ Gilles Delaygue, France]	Noted. Text modified accordingly.
30299	53	18			'European' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
17369	53	19	53	20	It is for "the Medieval Climate Anomaly and Little Ice Age", what about recent changes in "Central Asia"? [ Mostafa Jafari, Iran]	Noted. Text modified accordingly. To focus on North Atlantic storm tracks. Central Asian excluded.
30301	53	19			'and central Asia': how evidence in central Asia could constrain shift of storm tracks in the north Atlantic - European sector? [ Gilles Delaygue, France]	Noted. Text modified accordingly. To focus on North Atlantic storm tracks. Central Asian excluded.
7981	53	25	53	33	Kononova, N.K., Lupo, A.R. 2020: Changes in the Dynamics of the Northern Hemisphere Atmospheric Circulation and the Relationship to Surface Temperature in the 20th and 21st Centuries. Atmosphere, 11, 14 pp, art:00255. - this discusses a trend toward more meridional flows in the NH in all seasons since the mid-1990s. [ Anthony Lupo, United States of America]	Accepted. Literature has been assessed and cited at the end of Blocking paragraph, as a case showing different trend when a specific data period (e.g., mid-1990s to 2018) considered.
127037	53	25	53	33	Kononova and Lupo (2020) discuss a trend toward more meridional flows in the NH in all seasons since the mid-1990s. Citation: Kononova, N.K., Lupo, A.R. 2020: Changes in the Dynamics of the Northern Hemisphere Atmospheric Circulation and the Relationship to Surface Temperature in the 20th and 21st Centuries. Atmosphere, 11, 14 pp, art:00255. [ Trigg Talley, United States of America]	See comment #7981
78403	53	29	53	29	To the jet stream section, I think it would be useful to add: "A reconstruction of the high-summer North Atlantic Jet back to 1725 CE suggests unprecedented increase in its variance and meridional variability since the late twentieth century (Trouet et al. 2018). Taken from the following source: Recent enhanced high-summer North Atlantic Jet variability emerges from three-century context. <a href="https://doi.org/10.1038/s41467-017-02699-3">https://doi.org/10.1038/s41467-017-02699-3</a> [ Hans W Linderholm, Sweden]	Accepted. Literature cited to support the anomalous variance in jet shifting after 1960s.
67369	53	29	53	34	In this paragraph, a few references are missing: A number of studies have found a connection between the observed jet strength and measures of 'waviness' over the past few decades, which, together with model results, suggest that a weaker jet will result in larger amplitude waves and an increase in blocking events and vice versa (Francis and Vavrus, 2012; Cattiaux et al., 2017; Peings et al., 2017, 2018; Woollings et al., 2018). However, this is not unambiguous, apparently depending on the degree of idealization in the models (Hassanzadeh et al., 2014). There may also be a dependence on the latitude of the jet (Barnes, 2013). According to Blackport and Screen (2020), internal variability may have been misinterpreted as correlations between surface temperature gradients and Rossby wave amplitudes. [ Martin Stendel, Denmark]	Noted. Ch.2 aims to report what have observed in the jet stream and meandering, rather than to review whether/why/how they are physically linked. This is a important question, and also one under debate. We modified text slightly and cited Hassanzadeh et al 2014.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30303	53	30			'at the hemispheric scale and over the Eurasian sector': what's the point of adding the last sector? [ Gilles Delaygue, France]	Noted. Text modified for clarity by deleting 'Eurasian sector'.
37867	53	31	53	31	Synoptic wavenumbers' might be changed to 'synoptic disturbances' for easy understanding. [ Junhee Lee, Republic of Korea]	Noted. Because the summer meandering is under debate, we removed this sentence. See #227
57797	53	32	53	34	The meandering trends have been observed in multiple studies and it might be useful to briefly mention the role of sea ice here in that the loss of Arctic sea ice in particular has led to greater meandering of the jet stream, This is an extremely important signature of change in the Northern Hemisphere weather patterns. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The sea-ice extent and jet stream meandering has been addressed in Cross-chapter Box 10.1.
73543	53	33	53	34	References should be in chronological order [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
99585	53	34	53	34	Regional dependence should be mentioned here. After "... Vavrus, 2018)", add a sentence: The jet stream changes and its meandering are also regional dependent subject to background atmospheric state (Zhang et al., Basu et al., Cohen et al., 2020). References: Zhang, X., C. Lu, and Z. Guan, 2012: Weakened cyclones, intensified anticyclones, and the recent extreme cold winter weather events in Eurasia. Environ. Res. Lett., 7, 044044, doi:10.1088/1748-9326/7/4/044044; Basu, S., X. Zhang, I. Polyakov, and U. S. Bhatt, 2013: North American winter-spring storms: Modeling investigation on tropical Pacific sea surface temperature impacts. Geophys. Res. Lett., 40, 5228-5233, doi: 10.1002/grl.50990; Cohen, J., X. Zhang, J. Francis, T. Jung, R. Kwok, J. Overland, T. Ballinger, U.S. Bhatt, H. W. Chen, D. Coumou, S. Feldstein, D. Handorf, G. Henderson, M. Ionita, M. Kretschmer, F. Laliberte, S. Lee, H. W. Linderholm, W. Maslowski, Y. Peings, K. Pfeiffer, I. Rigor, T. Semmler, J. Stroeve, P. C. Taylor, S. Vavrus, T. Vihma, S. Wang, M. Wendisch, Y. Wu, and J. Yoon, 2020: Divergent consensus on the influence of Arctic Amplification on mid-latitude severe winter weather. Nature Climate Chang, 10, 20-29. doi:10.1038/s41558-019-0662-y. [ Xiangdong Zhang, United States of America]	Noted. Text modified to indicate that "the meandering regionality depends on the background atmospheric state" and Cohen et al. 2020 cited..
108303	53	34	53	34	Cross Chapter Box 10.1 is "Influence of the Arctic on mid-latitude climate". It does not seem to assess subtropical jet. [ Won-Tae Kwon, Republic of Korea]	Noted. Though the subtropical and polar jets are not explicitly and separately assessed, Cross-chapter Box 10.1 does contain information on subtropical jets as supported by cited literature, particularly when discussing the enhanced equator-northern pole temperature gradient in upper troposphere in the context of global warming and Arctic Amplification.
127039	53	36	53	46	This paragraph is well written and accurate insofar as it goes, but there should be more emphasis on the statistically significant and robust decrease in storm track activity in the Northern Hemisphere in summer. This has been shown for different metrics including the frequency of strong cyclones (Chang et al, GRL, 2016), the variance of sea level pressure change (Chang et al GRL 2016), and eddy kinetic energy (Coumou et al, Science, 2016), and it is consistent with a decreasing trend in available potential energy (Gertler and O'Gorman, PNAS, 2019). Given that a weakening storm track in summer would have implications for temperature extremes and air quality among other things, it seems important to highlight this robust change. Citations: Chang et al: doi:10.1002/2016GL068172; Coumou et al: 10.1126/science.1261768; Gertler et al: 10.1073/pnas.1812312116 [ Trigg Talley, United States of America]	Accepted. Text has been revised to highlight the consistent trends in NH summer storm track activity.
81201	53	40	53	40	A recent paper may be cited in this context: Patwardhan, Sooraj et al. 2020 Synoptic Scale Systems In: Assessment of Climate Change over the Indian Region; R. Krishnan, J. Sanjay, Chellappan Gnanaseelan, Milind Mujumdar, Ashwini Kulkarni, Supriyo Chakraborty Editors 2020 . https://doi.org/10.1007/978-981-15-4327-2 [ Supriyo Chakraborty, India]	Rejected. Suggested literature does not address hemispheric/global assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
11251	53	46	53	46	"there is overall low confidence for recent changes in global extratropical storm tracks" - I'm not sure that is the case for the NH summer storm tracks, at least since the satellite era when data time series are more homogenous and reliable. Results of Chang et al (2016) and Coumou et al (2015) showed that EKE, eddy variances, and frequency of strong cyclones consistently show substantial and significant decreases in NH summer since 1979 - I would consider storm track decrease in NH summer since 1979 to be not of "low confidence" category. Whether it is a forced signal is debatable but the decreasing trend is significant in all metrics that has been examined (unlike changes in the cool season), including both Eulerian storm track metrics and Lagrangian cyclone track metrics using multiple reanalysis data sets, and is also supported by weakening of the jet. References: Chang, E.K.M., C.-G. Ma, C. Zheng, and A.M.W. Yau, 2016: Observed and projected decrease in Northern Hemisphere extratropical cyclone activity in summer and its impacts on maximum temperature. Geophys. Res. Lett., 43, 2200-2208. Doi:10.1002/2016GL068172 Coumou, D., J. Lehmann, and J. Beckmann (2015), The weakening summer circulation in the Northern Hemisphere mid-latitudes, Science, 348, 324. [ Edmund Kar-Man Chang, United States of America]	See comment #127039
4169	53	48	54	4	There are two types of jet stream in northern hemisphere, the extratropical jet stream associated with the northern flank of Hadley circulation and the eddy-driven jet related to storm tracks (recommend to refer to Athanasiadis et al. 2010, JAS, Patterns of Wintertime Jet Stream Variability and Their Relation to the Storm Tracks). I think it's worth mentioning and this is significant if the author may elaborate on the variability of the two kinds of jet stream separately. [ Wenqi Zhang, China]	Noted. We focused on what has been observed based on the published references. As highly variable systems, it is difficult to distinguish subtropical jet stream and the polar jet stream, particularly in North Pacific sector and southern hemisphere. Therefore we did not assess the subtropical jet stream and the polar jet stream separately.
80271	53	48	54	4	Again influence of stratospheric ozone depletion could be cited in this paragraph, especially for the Southern Hemisphere (WMO, 2018). [ Sophie Godin-Beekmann, France]	Accepted. The suggested reference has been cited to support the poleward shifting of the extratropical jets in the Southern Hemisphere.
5365	53	48			Given this very strong statement concerning jets, why is there no figure here? This would seem imperative. [ Bryan Weare, United States of America]	Accepted. A new figure has been added to show the zonal-mean zonal wind trend in the troposphere using ERA5 data.
30305	53	50			'satellite observations' of what? (if it simple to express) [ Gilles Delaygue, France]	Noted. Text modified for clarity.
227	53	53	31	32	The increase in synoptic wavenumbers in boreal summer is debated, in particular because the underlying assumptions in the analysis of Kornhuber et al. (2019) are questionable Wirth (2020; doi: 10.5194/wcd-2020-3) and because there is no increase in the number of associated synoptic weather systems. [ Sebastian Schemm, Switzerland]	Accepted. Text has been revised accordingly and the statement for summer wavenumbers has been excluded.
229	53	53	45	45	Neu et al. 2013 should be added next to Grieger et al. 2018 [ Sebastian Schemm, Switzerland]	Accepted. Neu. et al. 2013 added.
231	53	54	55	1	In Line 25–34 is argued that the NH jet is more meandering in summer, which can only happen when it weakens for example due to the Arctic Amplification. In Line 55 it is argued that the jet in the SH has increased and shifted poleward, so there should not be a similar trend in the meandering of the SH jet, assuming that the meandering is proportional to the jet strength. No literature is presented that looks into the meandering of the SH jet and associated trends. Consider adding studies on SH jet meandering trends. [ Sebastian Schemm, Switzerland]	Noted. Limited peer-reviewed literature hampers a robust assessment for southern hemisphere meandering. Mechanisms and attribution in jet meandering are important issues, however, that is beyond the scope of Chapter 2.
127041	54	1	54	4	There's some repetition of the idea that storm tracks are moving polewards. [ Trigg Talley, United States of America]	Noted. Text modified for improvement.
57799	54	6	54	8	I think it is important to mention that the increase in blocking frequency over Greenland has important implications for melt, so that enhanced blocking leads to greater seasonal mass loss of the Greenland Ice Sheet. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. If mention the blocking's climate impacts over Greenland, seasonally we should also mention situations over other regions as well as impacts of other atmospheric circulation indicators as assessed in Ch.2. But this is significantly overstepping into the charge of other chapters.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127043	54	6	54	18	Given the presence of multidecadal variability in basin-scale modes, it is not obvious that trends that are only present in recent decades should be characterized as robust, even if they meet a test of statistical significance. [ Trigg Talley, United States of America]	Noted. Trends and significance are taken from peer-reviewed papers.
100885	54	8	54	8	The result from Hanna et al. 2016 has been recently confirmed by Davini ad D'Andrea 2020 (under revision Journal of Climate). They found a significant increase in blocking frequency in boreal summer over Greenland. They also found a (non significant) blocking decrease over the same region in boreal winter. The fact that the signal is not significant during the winter is mainly due to the higher internal variability of blocking frequency in this season. [ Corti Susanna, Italy]	Noted. Text has been modified to avoid talking specific local phenomenon, and Information on Greenland blockings has removed.
127045	54	8	54	19	It is not obvious whether the datasets discussed in the text have any relationship to the datasets plotted in panels b and d of Figure 2.17. The text and figure should be made consistent. [ Trigg Talley, United States of America]	See comment #501.
73545	54	9	54	9	Insert 'of' before 'longer' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
50049	54	9	54	10	The recently published paper by Tyrlis et al (2020) <a href="https://doi.org/10.1002/qj.3784">https://doi.org/10.1002/qj.3784</a> adds evidence regarding the upward trend of Ural Blocking. [ Eftychia (Efi) Rousi, Germany]	Accepted. The literature has assessed and cited to support the increasing of blocking in high-latitude Eurasian continent.
30307	54	13	54	14	This claim about a global trend contradicts the conclusion (L.16-17) that "Hemispheric or global features in the blocking frequency show diverse trends and they are sensitive to datasets and methods". [ Gilles Delaygue, France]	Noted. This statement, however, is for a single dataset. Edits for clarity have been made.
30309	54	14	54	16	'Inter-annual variations ... have been enhanced': not clear what has been enhanced, the variability, the amplitude of variations, or something else? [ Gilles Delaygue, France]	Accepted. Text modified for clarity. Variations replaced with 'variance'.
5367	54	20	54	21	The medium confidence assigned here seems contrary to the statement in page 53, line 48. [ Bryan Weare, United States of America]	Noted. With the uncertainties in the poleward shifting of jets subject to data types, periods, regionality and measurement metrics, a medium confidence level is given.
80273	54	20	54	26	Same remark as above. [ Sophie Godin-Beekmann, France]	Noted. Text modified for clarity.
9937	54	23	54	24	"The total number of extratropical cyclones has likely increased since the 1980s in the Northern Hemisphere," Coordination with ch.8 with respect to similar conclusion is needed. [ Olga Zolina, France]	Accepted. The final statement on the changes in the number of ETCs was coordinated with CH8.
79973	54	29	54	33	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? I raised this point for the FOD but it has not been addressed. I appreciate that there is a focus on specific variables in this chapter but it really doesn't make sense to assess wind speed and ignore the potentially major change in S Ocean wind stress. [ Simon Josey, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Wind stress not explicitly assessed in AR6 Ch.2. Wind stress and wave heights are directly related to wind speed. Over oceans, some assessed wind speed datasets in AR6 rely on observed ocean waves.
2961	54	29	54	46	Please add more references: Jiang, Y., Y.Luo and Z.C.Zhao, 2013, Maximum wind speed changes over China, Acta Meteor. Sinica, 27(1), 63-74, doi: 10.1007/s13351-013-0107-x [ Zong Ci Zhao, China]	Rejected. Ch.2 assess global/hemispheric/continental scale changes, instead of specific regions.
72187	54	29	54	46	in Figure 2.17 (bottom maps) are regions where trends are statistically significant but with opposite signs. This should be at least commented [ Joanna Wibig, Poland]	Noted. Actually, the diverse trend estimates over oceans are more evident when more reanalysis datasets are plotted and compared. It would be a better way to highlight the regions showing consistent changes. Text has been revised for clarity by indicating "Overall, most products suggest positive trends over the Southern Ocean, western North Atlantic and the tropical eastern Pacific since the early 1980s".
23299	54	29	55	33	They authors negelected the recent findings that surface wind speed over land are increasing rapidly and globally in the 21st century. The current conclusion is very bad for global wind enery production as they gave wrong info to policy maker! More details refer to: Zeng, Z., et al. (2019). "A reversal in global terrestrial stilling and its implications for wind energy production." Nature Climate Change. [ Zhenzhong Zeng, China]	Accepted. Text modified and new literature cited to support the recent recovery in windspeed over land areas.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73547	54	33	54	33	Delete 'time' to remove tautology. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Deleted.
501	54	35	55	19	It is difficult to match the text on surface wind speed versus Figure 2.17, other than the statement on p. 55 line 8 that "Over oceans, multiple datasets demonstrate considerable disagreement in surface wind speed trends." More explanation should be given of the contrasts among the four maps of Figure 2.17. [ Claire Parkinson, United States of America]	Noted. Not all cited datasets are plotted in the Figure. According to the data updating and representativeness, we selected 4 different types of data in the Figure. Actually, the diverse estimates over the oceans are more evident when more reanalysis datasets plotted and compared. Text has been revised for clarity by indicating "Overall, most products suggest positive trends over the Southern Ocean, western North Atlantic and the tropical eastern Pacific since the early 1980s".
30311	54	37	54	39	These claims seem to me at odd with Fig.2.17a, which shows a very strong heterogeneity of trends in the world. Is this 'global mean land' trend (-0.063) significant? [ Gilles Delaygue, France]	Noted. The assessment is based on peer-reviewed references. Text has modified to indicate the general decreasing over land in the Northern Hemisphere.
23783	54	38	54	42	Given that climatic impact drivers are mentioned here and at several places in Ch1 (and as stated, are mainly used in Ch12 as part of the main handover to WGII), a much clearer definition needs to be given here of the meaning of the term. Most readers will naturally fall back on the term "climate hazards" if this is not done. The imprecise definition here could encompass such drivers as the PDO, solar forcing, greenhouse gases etc. Perhaps some examples of CIDs could be given. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable, Hard to interpret and attribute, likely a misplaced comment?
127047	54	39	54	43	A fairer characterization of the stilling trend would be that the majority of stations north of 30N, where observational coverage is adequate, show stilling. South of 30N, the trend is mixed or even majority increasing. [ Trigg Talley, United States of America]	Accepted. Text modified to indicate the general decreasing over land in the northern hemisphere.
73549	54	39		39	Delete 'time' to remove tautology. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Deleted.
73551	54	40		40	I don't understand what is meant by 'stilling' and cannot find any reference to the process elsewhere. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. References added.
30313	54	41	54	42	'The strongest decreases are reported in Central Asia': again, this is at odd with fig.2.17a, which shows mostly positive values in Central Asia. Does this sentence refer to station or regional mean feature? [ Gilles Delaygue, France]	Noted. Text has modified for clarity.
57801	54	41	54	43	The values stated here appear lower than those in Figure 17a, with values approaching -0.4 m s-1 in places. I'm not sure if I have misinterpreted the graph. [ APECS, MRI, PAGES ECN, PYRN and YES5 ECS group review, Canada]	Noted. Values in the Figure are for stations, and in the text the values are for regional means.
17371	54	42	54	42	Which date is for "Central Asia"? [ Mostafa Jafari, Iran]	Noted. Text modified accordingly.
73553	54	44		44	I don't understand what is meant by 'stilling' and cannot find any reference to the process elsewhere. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. References added.
233	54	54	6	18	Robust trends in the blocking have been found for East Antarctica during Sep-Oct-Nov (Fig. 5d) in Schemm et al. (2018; doi: 10.1029/2018GL079109) [ Sebastian Schemm, Switzerland]	Accepted. Text modified and literature cited.
8693	54	55	54	55	As HadISD is a station-based dataset, a sentence describing how the stations were combined to produce the grids (spatial blending) along with the already stated temporal completeness would be helpful. [ Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Has been modified to indicate "To improve readability of plots, all datasets (including land station data) are interpolated onto a uniform 4x4 longitude-latitude grid."
115995	54		54		Is there an explanation why surface winds have weakened? I could not find anything in Chapter 3 on that. [ Valerie Masson-Delmotte, France]	Noted. In some of cited papers, there are suggested causes for the observed wind decline, spanning from global warming, changed land-cover roughness, instrumental error/change/re-locate, unreal boundary process in reanalysis, aerosol, weather system, regional circulation, and so on. Anyway, there are quite large uncertainty in quantitatively attributing regional wind changes. It better to find more details by reviewing cited references. Regardless, this is beyond the scope of chapter 2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73555	55	8	55	8	Insert 'the' after 'Over'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30315	55	8	55	19	This paragraph does not refer to Fig. 2.17, although this figure contains 3 plots over 4 dedicated to the paragraph topic. Also, datasets described in this paragraph (WASwind & NOC) are not the ones shown by Fig2.17 (or the datasets names are not consistent?). [ Gilles Delaygue, France]	Noted. Not all cited datasets are plotted in Fig 2.17. Among all available datasets, we selected 4 different types of dataset with respect to the data updating and representativeness.
1235	55	8	55	33	The ocean wind speed could be linked to trends in ocean surface wave heights. [ Rasmus Benestad, Norway]	Noted. Ch.2 does not assess the ocean wave height explicitly despite some assessed wind products which have used wave heights to derive surface wind speed.
8683	55	11	55	11	Dunn et al, 2016 in the references is for a land surface dataset - there would be State of the Climate reference which maybe what was intended (Dunn, Azorin-Molina, Mears, Berrisford & McVicar, "Surface Winds" in "BAMS State of the Climate 2015", 2016) . Alternatively, the two Azorin-Molina references (2017 and 2019) already included may be best placed here to summarise the assessment of the marine wind data. I note that there is a forthcoming BAMS SotC for 2019 (Blunden & Arndt 2020) - including an update - Azorin-Molina et al 2020. [ Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified and Dunn et al 2016 replaced by Azorin-Molina et al. 2017 and 2019.
82307	55	12	55	15	I propose to adapt this part based on Azorin-Molina et al. 2019 and by this consistently focus on recent versions of reanalysis. I propose to change into: "ERA5 and JRA55 show consistently increasing global marine wind speeds over 1979-2015, though flattening since 2000, with MERRA2 being in agreement with ERA5 and JRA55 until 1998, but then exhibiting stronger variability and an overall decrease in the last two decades (Azorin-Molina et al., 2019)". [ Schröder Marc, Germany]	Accepted. Text modified accordingly.
6549	55	12	55	15	Can this be linked to the discussion of differences in trend between GSAT and GMST, as a strengthening over time of marine winds would be expected to increase heat transfer from ocean to atmosphere and cause marine air temperature to rise faster than SST? [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. No peer-reviewed literature available to support wind contributes a faster GSAT warming and anyway mechanisms are out of scope.
73557	55	12		12	delete 'with each other' to remove tautology [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text modified as suggested.
99587	55	18	55	18	Include windspeed analysis for the Arctic Ocean. Before "Overall, ...", add one sentence: An increase in surface windspeed is also detected in autumn over the Chukchi and Beaufort seas (Stegall and Zhang, 2012). Reference: Stegall, S., and J. Zhang, 2012: Wind field climatology, changes, and extremes in the Chukchi-Beaufort seas and Alaska North Slope during 1979-2009. [ Xiangdong Zhang, United States of America]	Rejected. Ch.2 aims to assess large-scale changes.
73559	55	18		19	Move 'since the early 1980s' to after 'Pacific'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text modified as suggested.
5369	55	19			Fig. 2.17 does not suggest consistency of positive trends over the western N. Atlantic or tropical western Pacific. [ Bryan Weare, United States of America]	Noted. Text has been revised to indicate 'positive trends over the Southern Ocean, western North Atlantic and the tropical eastern Pacific since the early 1980s'.
90341	55	21			Southern Hemisphere subtropics [ Jeannine-Marie St-Jacques, Canada]	Editorial; copyedit to be completed prior to publication.
73561	55	22		22	Change 'northern hemisphere' to 'Northern Hemisphere'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30317	55	22			'stronger changes': 'stronger increases' to make it clearer? [ Gilles Delaygue, France]	Editorial. Text modified for clarity.
90343	55	22			Northern Hemisphere [ Jeannine-Marie St-Jacques, Canada]	Editorial; copyedit to be completed prior to publication.
23301	55	30	55	31	As for the recent global recovery of wind speed, the authors should cite the recent findings by Zeng et al., 2019 NCC: Zeng, Z., et al. (2019). "A reversal in global terrestrial stilling and its implications for wind energy production." Nature Climate Change. [ Zhenzhong Zeng, China]	Accepted. Reference added.
10963	55	30	55	31	This statement claiming worldwide weakening of surface wind does not seem to be supported by Figure 2.17, which shows several locations with significant positive trends. [ Tim Woollings, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text modified to indicated that "weakening of surface wind has likely occurred over land in the Northern Hemisphere".



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
503	55	30	55	32	Since winds don't stop and reset at the land-ocean boundary, it would be interesting to have an explanation of the weaker surface winds over the land but stronger surface winds over the oceans (if any published studies have provided an explanation). [ Claire Parkinson, United States of America]	Noted. In the final version of FGD surface wind trends are assessed separately over land and ocean. However, no peer-reviewed literature available to assess the trend differences (!) between land and ocean, and the corresponding causes (which are out of scope of CH2).
24381	55	30	55	33	This section is about surface winds and sea level pressure, but the summary statement makes no mention of sea level pressure trends. [ Owen Cooper, United States of America]	Noted. Large-scale SLP changes are related to modes such as NAO, SAM, Southern Oscillation, which are covered in Section 2.4. Text has been modified to refer to Section 2.4.
73969	55	30	55	33	This is also the statement that can be misinterpreted by decision-makers who would like to see the trends in surface wind on their regional scale. [ Elena Kozlovskaya, Finland]	Noted. Under IPCC-AR6 scope, Chapter 2 should assess the global or large-scale changes, rather than the local phenomenon. The continental scale changes have been assessed in the text despite not being highlighted in the summary.
71861	55	30		33	What about changes in the latitude of the westlies? Perhaps a figure of zonally averaged zonal wind stress would be useful. [ John Church, Australia]	Noted. Westerlies covered in Section 2.3.1.4.3. A new figure has been added, which shows the trend in the zonal-mean zonal wind from 1000hPa to upper troposphere.
5371	55	30			.. Has likely occurred over land in the Northern Hemisphere, ... [ Bryan Weare, United States of America]	Editorial. Text modified as suggested.
81337	55	36	55	36	I find it strange that AR5 messages for Antarctica are summarized first, but then the entire AR6 part only focuses on the Arctic. Also, why have the sections on the BDC and QBO completely disappeared? Are these no longer considered important parts of the climate system? [ Johannes Laube, Germany]	Noted. IPCC author team decided not to include BDC and QBO in the SOD. To save space and focus on large-scale circulations with directly observed robust changes and of direct policy relevance.
37091	55	36	55	36	If you discuss sudden warming events then you also need to discuss sudden cooling events. [ John McLean, Australia]	Noted. Cooling is associated with strong polar vortex.
39927	55	36	55	36	Add a definition to the glossary for 'Stratospheric polar vortex' [ TSU WGI, France]	See comment #131503
73563	55	40		40	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73565	55	44		44	Remove , after 'satellites'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30319	55	44			'stratospheric' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
73567	55	51		51	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73569	55	51		51	Space required between number and units (10 hPa). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73571	55	51		51	Please quantify 'significantly'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text modified.
43085	55	51			Read "zonal winds north of 60°N at 10 hPa have been significantly " rather than "zonal winds north of 60°N at 10hPa have been significantly " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
30321	55	51			space between 10 and hPa (maybe also between latitude?) [ Gilles Delaygue, France]	Editorial. Assessed literature does not provide changes in latitude.
73573	55	53		53	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73575	56	2	56	2	Capital 'S' for 'stratosphere' (it is used as a proper noun here). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
6551	56	9	56	9	The word "cause" is perhaps best avoided. The warming and the possible vortex breakdown are both part of a phenomenon known as the SSW. "include" is one alternative to "cause". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Text modified to indicate that "is tightly associated with the reversal of upper stratospheric zonal winds, and a resulting collapse or substantial weakening of the stratospheric polar vortex."
26057	56	9	56	9	Please consider replacing "air temperature warming" by "stratospheric air temperature rising". [ Don Alfonso Pino Maeso, Spain]	Accepted. Text modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19737	56	9	56	12	Somebody who relies on IPCC reports for learning about climate might be led to believe that SSW have been discovered by Butler, whereas Amy Butler begins her 2015 paper by stating that, six decades after discovering SSW, it is time to define these events in a consistent way. This is of course not the only example. This SOD contains about 1400 references, which is enormous; still, it should be possible to abandon a few of them in order to make room for references at a small number of major breakthrough papers. [ philippe waldteufel, France]	Rejected. Chapter 2 aims to assess the latest changes in observations and is not intended to perform a review.
505	56	17	56	18	I suggest expanding the sentence "There has been considerably less study of trends in the southern hemispheric stratosphere vortex strength" by adding "despite the interest in the ozone hole and the likely impact of the southern hemispheric stratosphere vortex strength on it." [ Claire Parkinson, United States of America]	Accepted. Text modified.
73971	56	20	56	21	One more example of a statement for misinterpretation: it is likely, but with low confidence [ Elena Kozlovskaya, Finland]	Noted. The confidence level is judged under IPCC guidelines.
24383	56	20	56	23	This section, as in AR5, discusses changes in both hemisphere, yet the summary statement only mentions the northern hemisphere. Why no mention of the southern hemisphere? [ Owen Cooper, United States of America]	Noted. Compared to the Northern Hemisphere, limited literature provides direct observational evidence for the changes in stratosphere atmospheric circulation over southern pole. Some works reported changes in upper temperature and ozone, which assessed in Section 3.3.1.2. Few studies reported the SSW over the Antarctica, there is only a couple of events in instrumental period.
115529	56	20	56	23	This summary and the entire section focusses on trends. However, especially for Arctic ozone loss, the occurrence of very cold Arctic winters is important and should be mentioned. The Arctic winter 2019/2020 was particularly cold and a special section of GRL/JGR is organised at the moment (Manney et al., 2020; Grooß et al., 2020; Bernandt et al., 2020) I suggest to discuss also especially cold winters [ Rolf Müller, Germany]	Noted. Chapter 2 aims to assess the general features of change in stratospheric circulation during the instrumental period, rather than to assess single events. The strengthening of northern polar vortex in later winter has been mentioned in the text. Changes of stratospheric ozone and temperature can be found in other sections of chapter 2 and in Section 3.3.1.2.
73577	56	23		23	Delete 'season' (winter is a season). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
15163	56	26			A large scale change not assessed here is the length of the freshwater (lake and river) ice season. Given that area of the northern hemisphere covered is seasonally frozen water, and that the signal is hemispheric, with data from across North America, Europe and Asia, it requires a least a short assessment here (or a reference to where it is assessed in WG1/2). [ Simon Donner, Canada]	Rejected; Chapter 2 does not aim at assessing the complete system (see page 8, line 8), therefore not all components are included.
46587	56	28	56	28	maybe "changes" is better? [ Dirk Notz, Germany]	Accepted; the wording is now changed.
83205	56	35	56	35	Change the title of 2.3.2.1 to "2.3.2.1 Sea ice extent, area, duration and thickness". The reason is that suggestions are made in comments below to include key information about change in annual sea-ice season duration, which are not adequately captured by extent and area alone. [ Robert Massom, Australia]	Noted; The title is changed and simplified, also for better consistency with chapter 9, but not in exactly the way the reviewer suggested.
34843	56	37	57	47	The SOD claims an unprecedented loss in Arctic sea ice over the last 1000 years. Please see rebuttal comment #7 above. [ Jim O'Brien, Ireland]	Unclear comment. No further action taken.
12147	56	37	59	18	Fig 2.18 shows multiple sources of SIA (OSISAF/CCI, Walsh, Bootstrap, NasaTeam). Yet the citation of the sea-ice area trends are based on Fetterer 2017. For the Arctic, a sentence states: "These estimates are broadly supported by other passive microwave products (Figure 2.18a)" but not in the Antarctic section. I strongly recommend to base the trend citation in the text on one (possibly several) of the datasets plotted on Fig 2.18, or to plot the Fetterer dataset on Fig 2.18. [ Thomas Lavergne, Norway]	Noted. The statement on trend is not anymore included.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12159	56	37	59	18	SROCC SPM states: "Arctic sea ice extent continues to decline in all months of the year (very high confidence);" (note "in all months of the year"). However this Chapter concludes only upon SIA decreasing "both in summer and winter". Why do we not conclude "in all months of the year" here as well? Is it because SIA would be more uncertain than SIE? or because we did not look/plot the other months? Going "back" to "winter and summer" for AR6 will be interpreted wrt SROCC's statement, so we should be clear of the reasons why we do not write "in all months of the year". I have no opinion here, just raising the issue. I also note that Chapter 9 also uses "in all months". [ Thomas Lavergne, Norway]	Accepted; Chapter 9 is able to go more into detail here, while chapter 2 summarized changes focusing on large scale signals and selected times of the year (seasons with maxima and minima). We have reworded the statements including "across the seasonal cycle" and "all months" in text (with cross-ref. to Fig. 9.13) and summary statement, respectively.
93037	56	37			A discussion of LIG sea ice should be included here. It is relevant to the discussions in Chapter 3 of the CMIP6 lig127k experiment. Several recent papers since the AR5 use the IP25 biomarker, as well as other biomarkers, to reconstruct Arctic sea ice during the LIG: Stein et al., Nature Comm., 2017; Kremer et al., QSR, 2018. Additionally, Kageyama et al., CPD, 2020, review proxy records for the LIG. [ Bette Otto-Bliesner, United States of America]	Accepted; A statement about LIG Arctic sea ice is now added. The paleo discussion of sea ice is more limited in chapter 2, while more details are given in chapter 9. Reference suggestions were forwarded to chapter 9.
57803	56	42	56	43	The definition of 'summer' is quite broad. Might be best to define this from here, presumably the period of time between the onset of melt (May-June) to the refreezing period (September-October), and that this varies between regions. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; This is reworded to "September", which also corresponds to the respective statement in SROCC.
16301	56	44	56	44	why miss recent data after 2008 [ Cunde Xiao, China]	Noted; More recent data than 2008 are not missing, but the statement addresses an outcome of AR5. This is now explicitly mentioned.
46589	56	47	56	47	I suggest to remove the sentence on sea-ice drift, as it's outside the scope of this section and as it reflects a finding that can be misunderstood as the primary cause of acceleration seems to be reduced concentration, not reduced thickness [ Dirk Notz, Germany]	Rejected; this connects to a statement on page 57, line 45-47 (numbers as in SOD), which expands on this.
57667	56	50	56	51	There is evidence for ice-free summers in the late Miocene central Arctic Ocean (Stein et al., 2016). In addition, more recent study by Stein et. al (2017) reconstructs sea ice extent during the last interglacial (ca. 125 kyr) and showed that under such warmer climate conditions sea ice existed in the central Arctic Ocean during summer, whereas sea ice was significantly reduced along the Barents Sea continental margin influenced by Atlantic Water inflow. Millennial-scale sea ice variability was also studied in a sediment core from the southeastern Norwegian Sea (Hoff et al., 2016). Expansion and retreat of sea ice varied consistently in pace with the rapid climate changes 90 kyr ago to present. Stein, R., Fahl, K., Schreck, M. et al. Evidence for ice-free summers in the late Miocene central Arctic Ocean. Nat Commun 7, 11148 (2016). <a href="https://doi.org/10.1038/ncomms11148">https://doi.org/10.1038/ncomms11148</a> . Stein, R., Fahl, K., Gierz, P. et al. Arctic Ocean sea ice cover during the penultimate glacial and the last interglacial. Nat Commun 8, 373 (2017). <a href="https://doi.org/10.1038/s41467-017-00552-1">https://doi.org/10.1038/s41467-017-00552-1</a> Hoff, U., Rasmussen, T., Stein, R. et al. Sea ice and millennial-scale climate variability in the Nordic seas 90 kyr ago to present. Nat Commun 7, 12247 (2016). <a href="https://doi.org/10.1038/ncomms12247">https://doi.org/10.1038/ncomms12247</a> [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; Some additional paleo Arctic sea ice information is now included in chapter 2, but the paleo discussion of sea ice is more limited here. More details are given in chapter 9. Reference suggestions were also forwarded to chapter 9.
83435	56	50	56	51	In case you would like to include here also a deep-time perspective you have the following publication: Stein, R., Fahl, K., Schreck, M., Knorr, G., Niessen, F., Forwick, M., Gebhardt, C., Jensen, L., Kaminski, M., Kopf, A., Matthiessen, J., Jokat, W., Lohmann, G., 2016. Evidence for ice-free summers in the late Miocene central Arctic Ocean. Nat Commun, 7, doi 10.1038/ncomms11148. [ Antje H. L. Voelker, Portugal]	Rejected. Additional content not included here due to length limits. More details on paleo sea ice can be found in ch. 9.
11629	56	50	56	54	The text acknowledges several paleoclimate reconstructions of Arctic sea ice that extend from the Younger Dryas to present. However, a much more detailed treatment of this topic is given in Chapter 9.3.1.1 (p. 42, lines 12-27). This should be acknowledged here so that readers can be redirected for additional detail. [ Ellie Broadman, United States of America]	Rejected; a cross reference to section 9.3.1.1 is given at the end of this section.
2013	56	50	56	54	Is it worth mentioning here that sea ice at the MPWP is assessed in the Pliocene box? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted; Information about sea ice in the MPWP is now explicitly added to this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35137	56	51	56	54	It may also be noted that during the LIA, sea ice in the Arctic likely thickened, as evidenced in new marine sediment core proxy records from north of Iceland that show thick sea ice insulated the ocean and reduced the flux of heat to the overlying atmosphere (Harning et al., 2019). REFERENCE: Harning, D.J., Andrews, J.T., Belt, S.T., Cabedo-Sanz, P., Geirsdóttir, Á., Dildar, N., Miller, G.H., Sepúlveda, J., 2019b. Sea ice control on winter subsurface temperatures of the North Iceland Shelf during the Little Ice Age: A TEX86 calibration case study. <i>Paleoceanog. Paleoclimatol.</i> 34, 1006-1021. [David Harning, United States of America]	Rejected; this section is kept relatively brief here in ch.2 and more details (incl. regional studies) can be found assessed in ch.9.
57805	56	51	56	54	The references provided in this section do not explicitly reconstruct sea ice coverage and only infer potential changes. I would thus change 'indicate' to 'suggest' just to avoid any over-interpretation. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The respective sentence is not anymore included.
73579	56	51		52	Delete hyphen between sea and ice [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; Hyphen is used consistently sea ice is followed up by a connected word.
2977	56	54	57	1	These findings are supported by the “fact that the 1985–2015 Baltic Sea ice extent distribution differs from any other preceding 30 winter period since 1720 with a high confidence” (Uotila et al. 2015). Reference: Uotila, P., Vihma, T., & Haapala, J. (2015). Atmospheric and oceanic conditions and the extremely low Bothnian Bay sea ice extent in 2014/2015. <i>Geophysical Research Letters</i> , 42(18), 7740–7749. <a href="https://doi.org/10.1002/2015GL064901">https://doi.org/10.1002/2015GL064901</a> . [Petteri Uotila, Finland]	Rejected; the mentioned reference has a regional scope, which is not central in chapter 2. Information is now given to chapter 9 for consideration.
5373	56	54			I could not find a clear distinction between pan-Arctic and Arctic. [Bryan Weare, United States of America]	Noted. The paragraph was reworded in the meantime. At one place, pan-Arctic is still used. By this a clear distinction to more regional focus in the sentence before is made.
83189	57	1	57	47	Please include a map of the Arctic somewhere with the different seas and sectors (mentioned in the text) marked. [Robert Massom, Australia]	Rejected; Adding an extra figure would exceed our length limits, the respective information can be found in textbooks and atlases.
73581	57	3	57	4	SIE should be defined earlier on first usage (the previous page, line 38). [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; SIE is now introduced at the first occasion in the section.
30323	57	3	57	4	SIA vs. SIE: is it possible to explain the key difference in few words? [Gilles Delaygue, France]	Rejected; Due to space limits this is not done here, but information can be found in glossary.
127049	57	3	57	5	Could there be simple discussion of why SIE and SIA are different? [Trigg Talley, United States of America]	Rejected; Due to space limits we do not add a sentence, but we have added a cross-reference to ch. 9, section 9.3.1, where more information can be found.
83187	57	3	57	16	Are there any (strong) regional patterns to the trends? Please include a qualifying sentence here. [Robert Massom, Australia]	Rejected; Since chapter 2 is supposed to focus on large scale and not regional scale changes, assessment of regional changes is kept to a minimum. More details can be found in chapter 9.
78839	57	6	57	6	change: a record of ... [MONICA TOLOTTI, Italy]	Accepted; This is now changed to "record-low".
57807	57	6	57	7	It is confusing to discuss both SIA and SIE without displaying both in Figure 2.18. Because the discussion until now has suggested that SIA is less biased, I would try to focus on these changes. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Showing both SIE and SIA in the figure would make the figure unreadable. However, we focus the discussion now more on SIA.
57697	57	6	57	16	Data are presented for both summer (september) and winter (march) showing a negative trend in both, even if with different magnitude. But causes are commented only for summer trend (lines 14 to 15) while nothing is said on winter trends. It should be addressed, even if to only say there is no study on causes of winter negative trend. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; The connections mentioned in line 14-15 are only highlighting cross-correlation, not direct attribution. That statement is now reworded. Attribution is detailed in chapter 9, therefore no more details are added here.
24177	57	7	57	7	Since then, summer extent has been variable. ' seems like a meaningless statement. I think either say there hasn't been any new record lows or just drop this line. [Alek Petty, United States of America]	Accepted; the sentence is now removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57809	57	7	57	7	I don't think the statement that summer extent has been more variable is correct - the SIA has continued on its negative trend after it diverged from this pattern between 2007 and 2012. Thus I would change this sentence to: "Summer sea ice extent has since returned to the negative trend that it was following between 1997 and 2007.". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; the sentence this comment was about was removed from the text.
83201	57	7	57	11	Where do the Arctic trends for 1979-2018 come from i.e., which source/reference? Or were they specifically and originally derived for this report? [ Robert Massom, Australia]	Noted; SIA trends are not anymore included here, instead different levels of decadal means are presented.
100533	57	8	57	8	I think, it should read: (equivalent to -15.1 +/- 1.3% per decade relative to ...) [ Peter Lemke, Germany]	Noted. The trend information was removed in the meantime. Instead differences between decadal means are presented.
52009	57	8	59	9	The sea ice percentage declines given in parentheses are incorrect/misleading because the percentages are decadal whilst the raw numbers are annual but "per decade" is not included in the text. For example the Antarctic summer (February) decline is given as "7000 +/- 4000 km <sup>2</sup> yr <sup>-1</sup> , (equivalent to 3.4 +/- 2.0% relative to the 1981-2010 mean)". If the percentage values given in parentheses were annual that would make the 1981-2010 mean ~206k sq km (7000/0.034). If however the percentage values are decadal then the 1981-2010 mean would be ~2.06 million sq km (70,000/0.034) which fits with the data in Fig 2.18b. This problem occurs 5 times during Sections 2.3.2.1.1 and 2.3.2.1.2. The text should be changed to "...equivalent to X +/- Y% PER DECADE relative to..." [ Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The trend information was removed in the meantime. Instead differences between decadal means are presented.
57811	57	9	57	9	I'm curious as to why the Fetterer et al. (2017) data is not plotted in Figure 2.18? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; The sea ice index of Fetterer et al. (2017) is based on the NASA Team dataset, which is shown in Fig. 2.18 (now Fig. 2.20). Further details on data sources and processing are available in the chapter data table (Table 2.SM.1)
12145	57	9	57	10	The sentence for March SIA does not read as well as the one preceeding (for September) (e.g. twice "SIA"). Consider re-writing this sentence to follow the structure of the preceeding sentence. [ Thomas Lavergne, Norway]	Noted. The sentence is not anymore included.
100535	57	10	57	10	I think, it should read: (equivalent to -2.5 +/- 0.3% per decade relative to ...) [ Peter Lemke, Germany]	Noted. The trend information was removed in the meantime. Instead differences between decadal means are presented.
39091	57	11	57	13	There may be a trend before 1990. Brennan et al., 2020 find "substantial loss of sea ice between 1910 and 1940". Brennan, M. Kathleen, Gregory J. Hakim, and Edward Blanchard-Wrigglesworth. "Arctic Sea-Ice Variability During the Instrumental Era." Geophysical Research Letters 47.7 (2020): e2019GL086843. [ Ola Kalen, Sweden]	Accepted; This additional information is now included.
79029	57	11	57	13	The HadISST2 data set has been released since AR5 and has corrected for inhomogeneities between passive microwave estimates of sea ice extent and those from ice charts. Titchner, H. A., and Rayner, N. A. ( 2014), The Met Office Hadley Centre sea ice and sea surface temperature data set, version 2: 1. Sea ice concentrations, J. Geophys. Res. Atmos., 119, 2864– 2889, doi:10.1002/2013JD020316. [ John Kennedy, France]	Rejected; since HadISST2 has a different land mask (see <a href="https://www.metoffice.gov.uk/hadobs/hadisst2/data/download.html">https://www.metoffice.gov.uk/hadobs/hadisst2/data/download.html</a> ) than other datasets, this dataset is not included in the SIA presentation.
57813	57	11	57	13	Because the sea ice charts have an inherently larger uncertainties compared to satellite products I wonder if a slightly different description could be made here. Suggest change to: "A longer baseline, using sea ice charts for pre-satellite era information since 1850 (Walsh et al., 2017), suggests that there was no significant trend before the 1990s (Figure 1.28a), but the uncertainty of these estimates is larger and the real trend may be masked beneath the errors.". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; The sentence is now reworded and revised, slightly different than suggested.
83173	57	12	57	12	How accurate/reliable are these ice charts? [ Robert Massom, Australia]	Accepted. The statement is now reworded, mentioning that the uncertainty of estimates based on ice charts is large.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57695	57	13	57	13	Most of the decline in SIA has occurred since 2000: it is not clear if this refers to previously cited Walsh et al 2017 or deduced from what seen in Figure 2.18a. I think it is better to either add some references for this or remove it as it is more a conclusion than data exposition. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. We find this information useful and therefore keep it. The wording is now tried to be written clearer, stating in the previous sentence "since 1979" and then referring to this.
24179	57	14	57	16	The earlier melt onset is I would say 'very likely' the driver of some of the decline and variability in summer SIA due to the positive melt albedo feedback mechanism, but some of it is also probably due to warming SSTs around the ice edge (positive open water feedback mechanism) along with continued heat/moisture intrusions into the Arctic. The freeze onset/open water duration is by contrast an impact of this loss of sea ice from increasing heat absorption. I think it's worth making this distinction clear. [ Alek Petty, United States of America]	Noted. The sentence is now revised to make clearer that the information is meant to be highlighting coinciding changes, not attribution.
57815	57	14	57	16	The end of this paragraph starts to attribute causality to the observed sea ice decline. Thus it may also be worth mentioning that extreme events, such as the 2012 anticyclonic behaviour above Greenland, have also become more common and increase the potential for large melting and sea loss in individual years. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Attribution is not supposed to be included in this chapter. The sentence addressed only coinciding changes in seasonal sea ice changes. The sentence is now revised to make this clearer.
83175	57	16	57	16	Please add Stammerjohn et al. (2012) and Maksym (2019) to these references - they are primary references regarding change in Arctic sea ice seasonality (annual timings of advance and retreat and sea ice duration). Please add that these studies show that across the Arctic and in almost all regions (apart from the Bering Sea), autumn sea-ice advance is trending later and spring-summer retreat earlier since 1978. In the regions of greatest change i.e., the Kara, Barents, Beaufort, Chuckchi and eastern Siberian seas (Stammerjohn et al., 2012), the sea-ice season length has shortened by approximately 2-3.5 months since 1978 (Maksym, 2019). REFERENCES: Stammerjohn, S., R. Massom, D. Rind and D. Martinson. 2012. Regions of rapid sea ice change: An inter-hemispheric seasonal comparison. Geophysical Research Letters, 39, L06501, doi:10.1029/2012GL050874. Maksym, T. 2019. Arctic and Antarctic sea ice change: Contrasts, commonalities, and causes. Annu. Rev. Mar. Sci., 11, 187-213. [ Robert Massom, Australia]	Taken into account; Stammerjohn et al. (2012) is now added, while the overview paper by Maksym (2019), which also refers to Stammerjohn et al. (2012), is not included.
12141	57	21	57	28	Caption to Fig 2.18 need a clean-up. Example: two versions of Walsh cited, Bootstrap not mentioned for panel a although it is plotted, months missing for Antarctic. I would suggest to re-arrange the caption to first introduce a. Arctic, b. Antarctic, then list the common parts (input data, "computation from raw sea-ice concentration data",...) [ Thomas Lavergne, Norway]	Accepted; The figure caption is now revised.
12143	57	21	57	28	Caption to Fig. 2.18 : "Sea-ice area values have been calculated from raw sea-ice concentration fields". I understand the use of "raw" here, but it might be mis-understood (as e.g. less filtered, less processed) and the sentence works without this word. Suggestion: remove "raw". [ Thomas Lavergne, Norway]	Noted; The caption is revised and the respective sentence is not included anymore.
113641	57	24	57	24	It should probably read "Bootstrap from NOAA CDR 3.0" as it is in line 27 on the same page. [ Agnieszka Kowalczyk, Poland]	Noted; The caption is revised and the respective sentence is not included anymore.
26059	57	25	57	25	Please consider adding "for September and February" [ Don Alfonso Pino Maeso, Spain]	Accepted; We revised the figure caption and added missing information.
12149	57	33	57	33	"(for sea ice terminology see WMO, 1970)". The IPCC AR6 WGI report comes with a Glossary where some sea-ice terms (including FYI) are defined. Please refer the reader to this Glossary that is easier to browse through than the WMO nomenclature. Also, the Glossary requires some edits (see my other comments). [ Thomas Lavergne, Norway]	Noted; For saving space, neither WMO nor glossary are referred to here. More information on the terms can be found in the Glossary, and also in textbooks.
11457	57	33	57	34	"for sea ice terminology see WMO, 1970)" - fine, but isn't that also what the glossary is supposed to be for? [ Gerhard Krinner, France]	Noted; For saving space, neither WMO nor glossary are referred to here. More information on the terms can be found in the Glossary, and also in textbooks.
83177	57	33	57	34	The correct terms are "first-year" ice and "multi-year" ice - please change throughout the report. [ Robert Massom, Australia]	Rejected; "First-year" with hyphen and "multiyear" without are common ways to write this, since "multiyear" is a word in English, and "firstyear" is not.
5375	57	33	57	47	There is no mention of Fig. 2.19 [ Bryan Weare, United States of America]	Rejected; Fig. 2.19 (now 2.21) was in the SOD referred to in line 40 on page 57, and it is still referred to, now as Fig. 2.21.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127051	57	34	57	34	While WMO (1970) may be the definitive source, it is surely not the most accessible one. One solution would be to improve the AR6 Glossary. [ Trigg Talley, United States of America]	Noted; For saving space, neither WMO nor glossary are referred to here. More information on the terms can be found in the Glossary, and also in textbooks.
52011	57	34	57	35	In this sentence you say that MYI was previously "16% of the ice cover" but that it is now "less than 1% of the Arctic Ocean". This doesn't exactly allow the reader to compare apples with apples. It would make things clearer if you were to add "(XX% of the Arctic Ocean)" after the initial "16% of ice cover" so the reader could directly compare to see the decline. (Where of course XX% is to be calculated!) [ Ed Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; As written, it is the oldest MYI which is addressed here, not all MYI. The mentioning of "Arctic Ocean" is now removed.
57817	57	34	57	35	Reference to Blunden and Arndt (2019) is incorrect. Reference should be Perovich et al. (2019). State of the Climate in 2018, BAMS. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The reference is now updated/corrected (with the newer Perovich et al. 2020 citation).
24175	57	36	57	36	I think it's worth making it clear that the multi-year ice (e.g. ice greater than 2 years in age) is also becoming thinner, so it's not just a loss of multi-year ice that explains the thickness declines. [ Alek Petty, United States of America]	Accepted; The sentence is now revised and a statement on thinning of older ice with reference included.
57819	57	36	57	36	Can you say for certain that loss of older ice is indicative of a thinner ice cover? For example, in a cold year, first year sea ice could become thickner, and thus second year sea ice could also be thickner than older ice. It might be better to say: "In a given year, the loss of older ice is indicative of a thinner ice cover". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; this sentence has been revised and extended.
83181	57	36	57	36	Change ""The loss of older ice is indicative of a thinner ice cover" to "The loss of older ice is indicative of a thinning ice cover". [ Robert Massom, Australia]	Accepted; The sentence has been revised and extended.
83179	57	36	57	38	Confusing in that a statement that direct observations of sea ice thickness in the Arctic are limited is followed by an assessment of change. How reliable is this assessment, given the apparent lack of observations? This requires clarification. Does direct observations refer here to drill hole measurements? Also, what about submarine sonar data that figured prominently in previous ARs? - there's no mention of these data here. [ Robert Massom, Australia]	Accepted. The sentence about direct measurements is now replaced by a sentence about in situ measurements. The submarine record has not been updated recently with submarine-based measurements, but it is included in Fig. 2.19 (now 2.21).
57699	57	37	57	39	It is not clear what the reference for this statement is. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The statement is reworded with additional information, and references are added.
83183	57	37	57	39	The negative trend figure given (~0.8 m) needs backing up with a source reference. [ Robert Massom, Australia]	Accepted; The statement is reworded with additional information, and references are added.
24181	57	38	57	38	Replace ~2000s with late 2000s. [ Alek Petty, United States of America]	Rejected; Changes are reported for around 2006-2007, therefore we keep 2000s.
12151	57	38	57	38	"0.8" is missing a unit. [ Thomas Lavergne, Norway]	Rejected; 0.8 was accompanied with m for meter. The sentence is now revised, a depth change information is given in the following sentence, with unit.
24183	57	39	57	39	Since 2010, there has been no discernable trend' should be considered low confidence considering this is based on the analysis of CryoSat-2 data which carries large uncertainties. We could mention the introduction of ICESat-2 which could help us constrain this further. I do think it's also strange that a lot of this section doesn't include confidence statements? [ Alek Petty, United States of America]	Noted; It is now mentioned that this statement is based both on satellite altimetry and airborne data. References are updated. ICESat-2 perspectives for future time series are not mentioned due to space limits.
57821	57	39	57	40	What does Figure 2.19 represent? Is it averaged sea ice thickness? If so, it is likely that regional changes are more important due to local atmospheric conditions. Making a reference to this would be beneficial. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; This chapter is not supposed to include attribution, therefore we do not discuss why changes occurred. The figure shows seasonal means for the time spans as given in the legend. The legend is not slightly reworded for more clarity.
83191	57	40	57	41	Change "Altimeter-derived ice thickness from airborne and spaceborne data are..." to "Sea-ice thicknesses derived from airborne and spaceborne altimeter data are..." [ Robert Massom, Australia]	Accepted; This is now reworded.
57823	57	40	57	42	Uncertainties also exist in the penetration of the electromagnetic wave through snow, so this should also be explicitly stated here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; This uncertainty factor is now mentioned, with relevant citations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57737	57	41	57	41	King et al. 2018 should be either 2018a or 2018b, 2018a is Jennifer King's comparison of in situ ice and snow thicknesses with laser and radar altimetry so this one is recommended. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; This is correct, but the second King et al. 2018 as included in the SOD is not included anymore, therefore the citation has no "a".
57739	57	41	57	41	perhaps it should be noted that the direction of the uncertainty in altimeter derived ice-thickness is biased to showing thicker ice than reality. e.g. "uncertainties in snow loading which bias these measurements to thicker than reality" [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; an addition about specifying the uncertainty is now included.
90349	57	41	57	42	reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Accepted; Citations are now listed chronologically.
83185	57	42	57	43	Regarding "also reported from the Greenland Sea and north of Svalbard", does this mean that the previous text refers to the central Arctic Basin? This is now clear as written. [ Robert Massom, Australia]	Accepted; geographical information in the previous statement is now added in the text (in addition to the map in the connected figure).
57825	57	44	57	45	It is not clear to me why sea ice data from the Fram Strait is important to highlight here. If it shows a particular trend, this should be stated. If it is the mechanisms of sea ice drift and the effects of storms on sea ice that is important here, then explicitly mention this. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; It is now explained in the previous sentence that the Fram Strait is a part of the Greenland Sea.
3449	57	44	57	47	I suggest to add the following sentence: "A reduction of survival rates of sea ice exported from the Siberian shelves by -15% per decade also has been observed, which interrupts the transpolar drift and affects the long-range transport of sea ice and ice-rafted matter (Krumpen et al., 2019)" with the reference "Krumpen et al., Sci Rep, 2019, Arctic warming interrupts the Transpolar Drift and affects long-range transport of sea ice and ice-rafted matter" ( <a href="https://doi.org/10.1038/s41598-019-41456-y">https://doi.org/10.1038/s41598-019-41456-y</a> ). [ Georgi Laukert, Germany]	Accepted; More detail (slightly shorter version than suggested due to space limits) and reference is now added.
12153	57	46	57	47	join the two groups of citations. [ Thomas Laverne, Norway]	Accepted; format and order of citations is now updated.
90351	57	46	57	47	merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Accepted; format and order of citations is now updated.
43087	57	46		47	Read " more mobile ice cover (Kwok et al., 2013; Hakkinen et al., 2008; Spreen et al., 2011; Vihma et al., 2012). " rather than " more mobile ice cover (Kwok et al., 2013) (Hakkinen et al., 2008; Spreen et al., 2011; Vihma et al., 2012). " [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted; format and order of citations (chronological) is now updated.
57827	58	8	58	10	Reference to Webster et al. (2018) incorrect. Instead, use: Boisvert et al. (2018), doi: 10.1175/jcli-d-18-0125.1. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Webster et al. 2018 does show both reanalysis data and observations from field campaigns for the respective area, while Boisvert et al. 2018 focuses only on reanalysis data.
78841	58	8	58	15	The use of Eastern and Western Arctic may be confusing to non specialists. I suggest to use Atlantic and Pacific Arctic. [ MONICA TOLOTTI, Italy]	Accepted; Western Arctic is now explained at first use, and "Ocean" is added. Eastern Arctic is not used. Pacific and Atlantic Arctic is not common and would not be equivalent with Western or eastern Arctic Ocean. For clarification, "Atlantic sector" is added when describing snow on sea ice north of Svalbard.
30325	58	8			'Atlas.5.10.3': what is it? [ Gilles Delaygue, France]	Noted; Atlas is another chapter of the AR6 of IPCC WG1. The cross reference was removed since the content of the Atlas changed after the SOD.
24185	58	9	58	10	This Webster (2018) study mainly demonstrated higher snowfall, not snow depth, in the Atlantic Sector. [ Alek Petty, United States of America]	Rejected; in Webster et al. 2018 both snow fall and snow depth are included for the respective region.
12155	58	12	58	13	There are two groups of citations in this sentence. Join the two groups of citations. [ Thomas Laverne, Norway]	Accepted; the references are now merged (and reduced).
12157	58	12	58	13	Are all those citations relevant to this (short) sentence? E.g. Panzer et al seems like a technical paper on an airborne instrumentation with some OIB data, that might already be covered by Brucker et al? Recommendation: keep only the most relevant citations to this sentence. [ Thomas Laverne, Norway]	Accepted; the references are now merged and reduced.
5379	58	14			Need a better geographic designation than Svalbard, which is hardly a household name. [ Bryan Weare, United States of America]	Accepted. Additional information is now added.
73583	58	15	58	15	Replace 'is' with 'are' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This is now changed.
83193	58	15	58	15	Change "data is too sparse" to "data are too sparse" [ Robert Massom, Australia]	Accepted. This is now changed.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24187	58	17	58	17	Worth stressing here that summer declines are much higher than the winter declines. [ Alek Petty, United States of America]	Accepted. Respective information is now added.
127053	58	17	58	18	A decrease in Arctic sea ice is supported by multiple lines of evidence and is statistically highly robust. Are you sure your confidence is only "very high"? [ Trigg Talley, United States of America]	Rejected; "Very high" is the highest level for confidence statements without numbers, therefore this is kept.
68263	58	17	58	22	Emphasize that reduced Arctic sea ice allows greater solar 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 more fragile first-year 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 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 Cyclones, GEOPHYSICAL RESEARCH LETTERS 45:3673–3681, 3680 ("Further, because climate change is increasing land-sea contrasts in the Arctic, it seems highly likely that the circulation patterns typical of years with strong AFZ will become more common as the climate warms. Indeed, strengthening of the mean temperature gradients in the AFZ is a robust feature of future climate projections as is an increase in the strength of the Arctic Front Jet (Mann et al., 2017; Nishii et al., 2014). This study shows that this linkage between surface temperature gradients and atmospheric circulation is important for Arctic cyclones, adding weight to previous studies."). An ice-free Arctic is possible in the next decade or two, according to Overland and Wang (2013) When will the summer Arctic be nearly sea ice free?, GEOPHYSICAL RESEARCH LETTERS 40:2097–2101, 2097 ("Time horizons for a nearly sea ice-free summer for these three approaches [for estimating future ice loss covered in the study] are roughly 2020 or earlier, 2030 ± 10 years, and	Rejected. Since this chapter focuses on observations without details on attribution, processes and feedbacks, we are unable to include that degree of detail. Chapter 9 includes more information on attribution, processes and feedbacks.
19739	58	18	58	18	Concerning thickness, figure 2.19 show however a stabilisation, acknowledged in the text. [ philippe waldteufel, France]	Rejected. This information is of secondary importance for to the summary statement. Due to length limits, the summary statement cannot contain the details that are given on the previous page (p. 57, l. 39 in SOD), where thickness is discussed.
83195	58	18	58	18	This is the first mention that Arctic sea ice has become faster moving. This needs to be substantiated with source references. Also, what data wre analysed to determine this? [ Robert Massom, Australia]	Rejected; The more mobile ice cover was already mentioned on page 57, line 45-47 (SOD). Here, only short summary statements are given.
24189	58	19	58	19	How exactly are the western and central Arctic defined here? I think it's easier to state this as western Arctic Ocean as this was based on OIB observations taken across the western (not Eastern) Arctic Ocean sector. [ Alek Petty, United States of America]	Accepted; Ocean is added to western Arctic now.
11459	58	19	58	20	"Proxy indicators show that Arctic sea ice has fluctuated on multiple time scales" While this is certainly true, this statement might be a bit too vague to be really useful, even in a summary paragraph. Could it be possible to indicate what amplitude of natural variability in climates similar to the present (or something of that kind?). Note that I didn't find a mention of "multiple time scales" in the paragraphs above where the paleo evidence was presented. [ Gerhard Krinner, France]	Noted. The sentence is reworded in a way that information is added about which time span is addressed.
73585	58	21		21	Delete 'period'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence is changed now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6553	58	26	58	31	The word "significant" appears twice in this paragraph. Quite what "significant " means is not clear. As regards the first occurrence, "but significant" could perhaps be deleted, as the IPCC's qualified language is used just afterwards, and the word "significant " does not appear in the AR5 Technical Summary on this point. The second "significant" could be removed, but if it stays could perhaps be preceded by the word "statistically". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; The wordings are referring to the earlier assessments AR5 and SROCC and are taken from the respective statements. More information can be found there.
5381	58	26	59	18	This text does not explain well the apparent inconsistency of very high confidence in Antarctic sea ice growth for 1979-2012 and high confidence of no trend for 1979-2018. There is no figure to help to understand this apparent turn around. Fig. 2.23 for Antarctic ice is no help. [ Bryan Weare, United States of America]	Rejected; Fig. 2.18 (in SOD, now 2.20, lower diagram) shows SIA for the Antarctic, including the change as discussed. Fig. 2.23 addresses the Antarctic ice sheet (and Greenland).
34845	58	26	59	18	The SOD claims that the Antarctic has lost ice mass since the early 1990s. Please see rebuttal comment #9 above. [ Jim O'Brien, Ireland]	Rejected; The subsection this comment is connected to is sea ice, not land ice.
83225	58	28	58	28	Change "...Antarctic sea ice cover exhibits" to ".....Antarctic sea ice coverage exhibits" [ Robert Massom, Australia]	Accepted; this change is now made, and the sentence slightly reworded.
83437	58	33	58	33	Why is there no text for/reference to any of the pre-whaling paleo-reconstructions around Antarctica? Recent references for such a text addition could be: 1) Benz, V., Esper, O., Gersonde, R., Lamy, F., Tiedemann, R., 2016. Last Glacial Maximum sea surface temperature and sea-ice extent in the Pacific sector of the Southern Ocean. Quaternary Science Reviews 146, 216-237, doi: 10.1016/j.quascirev.2016.06.006. 2) Nair, A., Mohan, R., Crosta, X., Manoj, M.C., Thamban, M., Marieu, V., 2019. Southern Ocean sea ice and frontal changes during the Late Quaternary and their linkages to Asian summer monsoon. Quaternary Science Reviews 213, 93-104, doi: https://doi.org/10.1016/j.quascirev.2019.04.007. 3) Chadwick, M., Allen, C.S., Sime, L.C., Hillenbrand, C.D., 2020. Analysing the timing of peak warming and minimum winter sea-ice extent in the Southern Ocean during MIS 5e. Quaternary Science Reviews 229, 106134, doi: https://doi.org/10.1016/j.quascirev.2019.106134. 4) Barbara, L., Crosta, X., Schmidt, S., Massé, G., 2013. Diatoms and biomarkers evidence for major changes in sea ice conditions prior the instrumental period in Antarctic Peninsula. Quaternary Science Reviews 79, 99-110, doi: https://doi.org/10.1016/j.quascirev.2013.07.021. 5) Tesi, T., Belt, S.T., Gariboldi, K., Muschitiello, F., Smik, L., Finocchiaro, F., Giglio, F., Colizza, E., Gazzurra, G., Giordano, P., Morigi, C., Capotondi, L., Nogarotto, A., Köseoğlu, D., Di Roberto, A., Gallerani, A., Langone, L., 2020. Resolving sea ice dynamics in the north-western Ross Sea during the last 2.6 ka: From seasonal to millennial timescales. Quaternary Science Reviews 237, 106299, doi: https://doi.org/10.1016/j.quascirev.2020.106299. 6) Lamping, N., Müller, J., Esper, O., Hillenbrand, C.-D., Smith, J.A., Kuhn, G., 2020. Highly branched isoprenoids reveal onset of deglaciation followed by dynamic sea-ice conditions in the western Amundsen Sea, Antarctica. Quaternary Science Reviews 228, 106103, doi: https://doi.org/10.1016/j.quascirev.2019.106103. 7) Denis, D., Crosta, X., Barbara, L., Massé, G., Renssen, H., Ther, O., Giraudeau, J., 2010. Sea ice and wind variability during the Holocene in East Antarctica: insight on middle-high latitude coupling. Quaternary Science Reviews 29, 3709-3719, doi: https://doi.org/10.1016/j.quascirev.2010.08.007. 8) Etourneau, J., Collins, L.G., Willmott, V., Kim, J.H., Barbara, L., Leventer, A., Schouten, S., Sanninghe Damsté, J.S., Bianchini, A., Klein, V., Crosta, X., Massé, G., 2013. Holocene climate variations in the western Antarctic Peninsula: evidence for sea ice extent predominantly controlled by changes in insolation	Accepted; Information on paleo reconstructions is added now, and several of the suggested references are cited now.
57829	58	33	58	40	Each of the studies referenced in the first sentence show a stabilisation trend in sea ice coverage since 1980 which I think deserves a mention in the second sentence. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; The section is now revised. Beyond that, the respective reconstructions mentioned in this paragraph address in the first place changes prior to 1980, only the last point (iii) addresses that changes in winter SIE were small in the 20th century, along the lines that the reviewer comment suggests.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83197	58	33	58	46	This paragraph is not logically placed, and its placement is inconsistent with the placement of similar pre-satellite material in the preceding Arctic sea ice section. Please move this paragraph to after the paragraph ending on Line 13 on Page 59. See also my Comment 35 below. [ Robert Massom, Australia]	Rejected; The paragraph on paleo and historic changes is following summaries from AR5 and SROCC as it is done for the Arctic, before details on the time since 1979 follow.
83207	58	33	59	18	In my capacity as a Contributing Author for this section 2.3.2.1.2 Antarctic Sea Ice, I propose that the text (apart from paragraph 1, lines 26-31 on page 58) needs substantive changes/amendments. These are required to (1) make the text more consistent with the prior Arctic sea ice section (2.3.2.1.1); (2) provide important updates; (3) correct current grammatical errors, errors, ambiguities and discrepancies; (4) provide/include key missing information relating to change in Antarctic sea ice duration, seasonal and regional dependencies of change and variability in sea ice coverage; and (5) provide new Antarctic fast ice information. PLEASE SEE THE FOLLOWING COMMENTS BELOW about the selected sequence of changed text. Each of the sequential comments below will contain a paragraph of the amended text in the order which is suggested - to replace current text Page 58 line 33 to Page 59 Line 18 inclusive - starting with new paragraph 2. NB These new paragraphs replace the current text from Line 33 on Page 58 to Line 18 on Page 59 inclusive. [ Robert Massom, Australia]	Taken into account; Some of the suggested changes and additions were used, some not. Limited consideration is due to length limits and focus of ch. 2 on large-scale observations.
83209	58	33	59	18	NEW PARAGRAPH 2 - "For the period 1979 to 2015, the continuous satellite passive-microwave record shows that there were modest significant increases in both overall Antarctic SIE of $1.7\% \pm 0.2\%$ per decade and in overall SIA of $2.5\% \pm 0.2\%$ per decade (Comiso et al., 2017). The corresponding trend in overall Antarctic SIA for austral winter (September) for 1979 to 2015 was $17,000 \pm 6,000$ km <sup>2</sup> yr <sup>-1</sup> , equivalent to $1.2 \pm 0.4\%$ relative to the 1981-2010 mean (Figure 2.18b). For overall ice coverage and for this period, positive long-term trends were most pronounced and only statistically significant during austral autumn advance (Maksym, 2019), being moderate in summer and winter, and lowest in spring (Holland, 2014; Turner et al., 2015; Hobbs et al., 2016a, 2016b; Comiso et al., 2017)." [ Robert Massom, Australia]	Noted; parts of the suggested new text is used now.
83211	58	33	59	18	NEW PARAGRAPH 3 - "Since AR5, overall (total) Antarctic sea-ice coverage has exhibited major fluctuations from record-high to record-low extents (Massonnet et al., 2015; Reid and Massom, 2015; Reid et al., 2015; Comiso et al., 2017; Parkinson, 2019). After setting record-high maximum overall extents each September from 2012 through 2014, Antarctic sea ice coverage dipped rapidly in mid-2016 and has remained largely below average through 2019 (Reid et al., in press), with frequent record-low seasonal values e.g., 2.08 million km <sup>2</sup> on 1 March 2017 (Reid et al., 2018). This recent change to high variability has substantially affected the magnitude and significance of net overall trends for the period 1979 to 2018 in both Antarctic SIE (Parkinson, 2019) and SIA (Figure 2.18b, based in Fetterer et al., 2017). For this period, SIA trends relative to the 1981-2010 mean changed to become non-significant for both austral winter (September i.e., $7000 \pm 6000$ km <sup>2</sup> yr <sup>-1</sup> or $0.5 \pm 0.4\%$ ) and summer (February, $7000 \pm 4000$ km <sup>2</sup> yr <sup>-1</sup> or $3.4 \pm 2.0\%$ ) (Figure 2.18b). The regional and seasonal manifestations of the recent changes have also been marked e.g., with a recovery of the winter sea-ice coverage in the Amundsen-Bellingshausen seas (Parkinson, 2019; Maksym, 2019; Reid et al., in press)." [ Robert Massom, Australia]	Noted; parts of the suggested new text is used now.
83213	58	33	59	18	NEW PARAGRAPH 4 - "For Antarctica, the modest non-significant increase in spatially-averaged sea ice coverage for 1979 to 2018 is not only made up of strongly-differing seasonal contributions; it also obscures large and opposing regional trends around the continent (Stammerjohn et al., 2012; Holland, 2014; Turner et al., 2015; Parkinson, 2019; Stammerjohn and Maksym, 2017; Maksym, 2019). Antarctic sea-ice trends show strong dependency on the season and time period (Parkinson, 2019; Maksym, 2019), with only opposing SIE and SIA trends in the Amundsen-Bellingshausen seas sector (negative) and the Ross Sea (positive) being relatively consistent over time (Comiso et al., 2017; Parkinson, 2019). The Ross Sea is also notable as being the only sector with a significant mean annual trend (for the period 1979 to 2013; Yuan et al., 2017)." [ Robert Massom, Australia]	Rejected; The content of this part is mainly regional, therefore it is not included here, but it was communicated further to ch. 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83215	58	33	59	18	NEW PARAGRAPH 5 - "Consistent with observed regional variability in extent, annual ice-season duration has increased by 2-3 months in the western Ross Sea sector from 1978 to 2016 (due to a 1-2 month earlier advance and 1 month later retreat) (Stammerjohn and Maksym, 2017). In contrast and in the Amundsen-Bellingshausen seas, the ice season shortened by ~2-3 months (a 1-2 month later advance and 1 month earlier retreat). Patterns of change in annual sea-ice duration across the extensive East Antarctic sector exhibit considerable spatial complexity by comparison (Massom et al., 2013). The magnitude of Antarctic annual ice-season duration trends also varies regionally over different time-scales, with a relatively consistent trend in the Ross Sea contrasting with sub-decadal variations in the Bellingshausen Sea (Simpkins et al., 2013). The regional trends in sea ice seasonality are substantially more variable in the Antarctic than the Arctic (Stammerjohn et al., 2012; Maksym, 2019)." [ Robert Massom, Australia]	Rejected; The content of this part is mainly regional, therefore it is not included here, but it was communicated further to ch. 9.
83217	58	33	59	18	NEW PARAGRAPH 6 - "For coastal Antarctica, a new time series of stationary landfast sea ice (fast ice) extent based on cloud-free satellite MODIS visible and thermal infrared imagery and for 2000 to 2018 (Fraser et al., in press) enables a first analysis of complete circum-Antarctic trends and variability at high resolution (Fraser et al., in prep.). The climatological minimum and maximum annual extents occur in early March (~2.2 x 105 km2) and early October (~6.3 x 105 km2), respectively. For this short 18-year time series, overall fast-ice extent shows a marginally-significant negative linear "trend" of $-882 \pm 823$ km yr <sup>-1</sup> (or $-0.19 \pm 0.18\%$ yr <sup>-1</sup> ). Again, this circumpolar trend is made up of distinct and contrasting regional contributions. These range from a marginally-significant positive trend of $342 \pm 281$ km yr <sup>-1</sup> ( $0.67 \pm 0.55\%$ yr <sup>-1</sup> ) for the Amundsen-Bellingshausen seas sector to a significant negative trend of $-1,006 \pm 180$ km yr <sup>-1</sup> (or $-1.43 \pm 0.25\%$ yr <sup>-1</sup> ) for the Ross Sea." [ Robert Massom, Australia]	Rejected. This suggestion was not included (length limits, regional aspects).
83219	58	33	59	18	NEW PARAGRAPH 7 - "The satellite passive-microwave record of Antarctic SIE has been extended back into the 1960s using snapshots from early Nimbus satellite visible and infrared imagery (Meier et al., 2013; Gallaher et al., 2014). This indicates higher overall SIE in the 1960s compared to 1979-2013 (Hobbs et al., 2016a, 2016b), but with large uncertainties and unknown biases (NAS, 2017). Longer-term proxy SIE reconstructions are based on whaling records (De La Mare, 1997, 2009; Cotté and Guinet, 2007), old ship logbooks (Ackley et al., 2003; Edinburgh and Day, 2016), ice-sheet ice core records (Curran et al., 2003; Abram et al., 2010; Sinclair et al., 2014) and fast-ice records (Murphy et al., 1995, 2014). To varying degrees, these reconstructions indicate: i) a decrease in summer SIE across all Antarctic sectors since the early- to mid-20th Century; ii) a decrease in winter SIE in the Amundsen-Bellingshausen seas and Western Pacific Ocean sectors starting in the 1960s; and iii) small changes in winter SIE in the Weddell Sea over the 20th Century (Hobbs et al., 2016a, 2016b). Ice-core data further indicate that the pronounced Ross Sea increase in the modern satellite era dates back to the mid-1960s (Sinclair et al., 2014; Thomas and Abram, 2016). While there is reasonable broad-scale agreement across these estimates, the uncertainties are large, however, and reconstructions require further validation (Hobbs et al., 2016a, 2016b; NAS, 2017). New reconstructions from Antarctic Ice Sheet ice cores (Thomas et al., 2019) indicate that SIE in the Ross Sea increased between 1900 and 1990, while that in the Bellingshausen Sea decreased. This dipole pattern is consistent with the satellite record from 1979 to 2019, but the implied rate of change is larger." [ Robert Massom, Australia]	Accepted; Main parts of the suggested text are included now, but with some reorganisation and rewording.
83221	58	33	59	18	NEW PARAGRAPH 8 - "Information on Antarctic sea ice thickness and snow cover thickness remains sparse in space and time, and so far no significant trends can be detected from available observations (Worby et al., 2008; Webster et al., 2018). Several model studies, however, show broadly-consistent and region-dependent trends in ice thickness (Massonnet et al., 2013; Holland et al., 2014; Schroeter et al., 2018; Kusahara et al., 2019), indicating winter-time thinning in the Bellingshausen Sea and thickening in the Ross Sea and inner Weddell Sea." [ Robert Massom, Australia]	Rejected; The corresponding sentence was removed now due to length limits and since no observations are available for saying more. Ch. 2 is not including modelling results.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83223	58	33	59	18	NEW PARAGRAPH 9 - "In summary, Antarctic sea ice has experienced interannual and decadal variability but no significant trend in its overall coverage for the period of continuous satellite observations (1979-2018) but interannual and decadal variability (very high confidence). This overall pattern is made up of contrasting regional and seasonal contributions, with an unanticipated shift to record positive overall coverage from 2012 to 2014 then record negative coverage from 2016 (very high confidence). There remains low confidence in all aspects of Antarctic sea-ice prior to the satellite era owing to large uncertainty and a paucity of evidence that is highly regional in nature and often contradictory. Observational datasets remain too sparse to determine whether Antarctic sea-ice and snow-cover thickness are changing." [ Robert Massom, Australia]	Rejected; This subsection was revised, giving more detail on SIA changes. The second last sentence from the comment is included. However, due to length limits no other additional information can be included.
83227	58	33	59	18	ADD NEW REFERENCES/CITATIONS INCLUDED IN THE AMENDED PARAGRAPHS ABOVE: (1) Fraser, A.D., R. A. Massom, K. I. Ohshima, S. Willmes, P. J. Kappes, J. Cartwright, and R. Porter-Smith. In press. High-resolution mapping of circum-Antarctic landfast sea ice distribution, 2000-2018. Earth System Science Datasets. (2) Fraser, A. D., R. A. Massom, K. I. Ohshima, M. S. Handcock, M. N. Raphael., J. Cartwright, A. Klekociuk, P. A. Reid, C. Greene and R. Porter-Smith. In prep. An 18-y record of Antarctic landfast sea ice distribution allows first circum-Antarctic baseline characterization, reveals trends and variability. Journal tbd. (3) Kusahara, K., Williams, G.D., Massom, R., Reid, P. and Hasumi, H. 2019. Spatiotemporal dependence of Antarctic sea ice variability to dynamic and thermodynamic forcing: A coupled ocean-sea ice model study. Clim. Dyn., 52(7-8), 3791-3807, doi.org/10.1007/s00382-018-4348-3. (4) Maksym, T. 2019. Arctic and Antarctic sea ice change: Contrasts, commonalities, and causes. Annu. Rev. Mar. Sci., 11, 187-213. (5) Massom, R.A., P. Reid, S. Stammerjohn, B. Raymond, A. Fraser and S. Ushio. 2013. Change & variability in East Antarctic sea ice seasonality, 1979/80-2009/10. PLoS ONE, 8(5), e64756, doi:10.1371/journal.pone.0064756. (6) NAS. 2017. Antarctic sea ice variability in the Southern Ocean-climate system. Washington, DC: The National Academies Press, doi:10.17226/24696. (7) Reid, P., S. Stammerjohn, R. A. Massom, S. Barreira, T. Scambos, and J. L. Lieser. In press. (Antarctica) Sea ice extent, concentration, and seasonality [in "State of the Climate in 2019"]. Bull. Am. Met. Soc. (8) Schroeter, S., W. Hobbs, N.L. Bindoff, R. Massom and R. Matear. 2018. Drivers of Antarctic sea ice volume change in CMIP5 models. Journal of Geophysical Research – Oceans, 123(11), 7914-7938 https://doi.org/10.1029/2018JC014177. (9) Stammerjohn, S., R. Massom, D. Rind and D. Martinson. 2012. Regions of rapid sea ice change: An inter-hemispheric seasonal comparison. Geophysical Research Letters, 39, L06501, doi:10.1029/2012GL050874. (10) Worby A.P., Geiger C.A., Paget M.J., Van Woert M.L., Ackley S.F., and DeLiberty T.L. 2008.	Noted; Those references of the listed that were now cited are also included in the reference list. The suggested references were also communicated to ch. 9.
509	58	36	58	42	I suggest mentioning that all of these sea ice results come with considerable interannual variability. One way of doing that would be to extend "the uncertainties are large and" in line 42 to "the uncertainties are large, there is considerable interannual variability, and ..." [ Claire Parkinson, United States of America]	Accepted; the suggested wording is now added.
30327	58	37		38	'since' and 'starting': it is important to indicate when these reconstructions end, to compare with the direct observations by satellite in the next paragraph [ Gilles Delaygue, France]	Noted; The reconstructions are partly combined with recent observations, this information is now added.
12161	58	38	38	38	The Bellinghausen and Amundsen seas/sectors are mentioned twice in this sentence. [ Thomas Lavergne, Norway]	Accepted; The sentence is now reworded.
29851	58	38	58	38	For easier understanding, insert a comma before "and Amundsen-Bellinghausen Seas sectors starting in the 1960s;" [ Hernan Edgardo Sala, Argentina]	Accepted; The sentence is now reworded.
57831	58	45	58	46	The dipole pattern discussed here is actually quite prominent and I think the sentence should acknowledge the fact there is almost an order of magnitude difference between the two sectors. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; Some more detail is now added to this paragraph, and the specific sentence is reworded for more clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57833	58	45	58	46	A reference for the satellite sea ice record used should be provided. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The statement still connects to the citation in the previous sentence. The sentence is now reworded to clarify and connect to the following paragraph, and a citation is added.
511	58	45	58	46	It should be clarified which implied rate of change is the larger one (e.g., "the implied rate of change from the dipole pattern"). [ Claire Parkinson, United States of America]	Accepted; The sentence is now reworded to clarify and connect to the following paragraph, and a citation is added.
57835	58	48	58	50	The spatial pattern of SIE is also heterogeneous and it is important to point out that in some regions, such as near Law Dome, there are very large differences in the time periods considered. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; this paragraph has been revised, however, the focus of chapter 2 is on large scales. For regional information see ch. 9.
83199	58	48	58	50	Move this sentence to the start of the paragraph beginning on current Line 33 Page 58. See also my Comment 34. [ Robert Massom, Australia]	Rejected; This subsection on historic Antarctic sea ice information is supposed to be where it is placed, after the paleo and before the satellite era subsections.
12163	58	51	59	10	This paragraph has many numbers, trends, and citations, and this partly blurs the message. I would suggest to drop the first section on < 2015 trends and focus on the recent estimates. [ Thomas Lavergne, Norway]	Accepted; some of the earlier included SIE numbers and trend information is removed and replaced by information about decadal means, resulting in less numbers and trends in this paragraph.
30329	58	52			'modest increase': is it significant? [ Gilles Delaygue, France]	Noted; we cannot say whether it is significant or not, since we do not know enough about the internal variability. More details on this are included in ch. 9.
30331	58	52			'net': not clear net of what? [ Gilles Delaygue, France]	Accepted; "net" is now removed.
73587	58	53		54	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was revised and this is not included anymore.
30333	58	53			'winter': what about 'end-of-winter' or 'maximum'? [ Gilles Delaygue, France]	Noted. The sentence was revised and this is not included anymore.
127055	58	54	58	54	There's an extra "t" in net. [ Trigg Talley, United States of America]	Noted. The paragraph was revised and this is not included anymore.
29853	58	54	58	54	Typo. [ Hernan Edgardo Sala, Argentina]	Noted. The paragraph was revised and this is not included anymore.
100537	58	54	58	54	I think, it should read: (equivalent to 1.2 +/- 0.4% per decade relative to ...) [ Peter Lemke, Germany]	Noted. The paragraph was revised and this is not included anymore.
11461	58	54	58	54	Nett -> Net [ Gerhard Krinner, France]	Noted. The paragraph was revised and this is not included anymore.
30335	58	54			'Net' [ Gilles Delaygue, France]	Noted. The paragraph was revised and this is not included anymore.
90353	58	54			Net not Nett [ Jeannine-Marie St-Jacques, Canada]	Noted. The paragraph was revised and this is not included anymore.
115997	58		59		Paleo sea ice : there was a small assessment of that in SROCC too, please build on SROCC and avoid duplicating the assessment including from papers already available at the time of AR5. The fact that paleo evidence suggests contrasted regional trends at the centennial scale, consistent with recent observations only available for 40 years, could be highlighted. I suggest as in SROCC to stress contrasted regional trends without an overall Antarctic sea ice trend explicitly here (important for regional impacts for ecosystems in WGII, as seen in SROCC). [ Valerie Masson-Delmotte, France]	Noted; More information on available Antarctic paleo sea ice information for selected paleo periods (as done in other parts of the chapter) was added. Contrasting regional changes are still mentioned when assessing 20th century changes.
57837	59	2	59	3	It would be useful to state an example of a year in which record high or low values were recorded e.g. the 2014 high record. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The text is revised giving more information on the period 2012-2014. In addition, Fig. 2.18b (SOD, now Fig. 2.20b) shows SIA for each year.
24191	59	2	59	6	I thought the language used here was overly complex. Please simplify. [ Alek Petty, United States of America]	Accepted; The text is reworded now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39093	59	4	59	9	The extreme reduction in Antarctic Sea ice in recent years needs to be expanded a bit upon here and not just characterized as "non-significant". From Parkinson, C. L. (2019): "The decline in yearly average Antarctic sea ice extents from 2014 to 2017 (followed by a slight rebound) was at a linear least squares rate of $-729,000 \text{ km}^2\text{-y}^{-1}$ , well exceeding the rate of change for either hemisphere in any other 4-y period during the 40 y (1979–2018) of the satellite multichannel passive-microwave record" [ Ola Kalen, Sweden]	Noted; the discussion of Antarctic SIA changes since 2014 is now revised for more clarity, and information is added.
513	59	5	59	6	I suggest expanding "Recent years' levels reduce Antarctic SIE (Parkinson, 2019)" to the following far more informative statement referencing the same source: "For the 40-year 1979-2018 period, the record high annual average Antarctic SIE, reached in 2014, was followed by a record low SIE just three years later, with a slight rebound in 2018 (Parkinson, 2019). This reduced the 1979-2014 SIE trend of $22,400 \pm 4,300 \text{ km}^2\text{/yr}$ to the far lower 1979-2018 trend of $11,300 \pm 5,300 \text{ km}^2\text{/yr}$ (Parkinson, 2019)." [ Claire Parkinson, United States of America]	Taken into account; The sentence and paragraph is now reworded (but not exactly as suggested), with more details, and also taking initial SIA data (OSISAF) for 2020 into account.
11463	59	5	59	7	"... reduce Antarctic SIE trends and their significance to be assessed as non-significant" - I'm not a native speaker, but this really sounds strange to me. [ Gerhard Krinner, France]	Noted; the paragraph is now revised and this sentence is not anymore included in this form.
57839	59	5	59	8	I find the wording in the first part of this sentence awkward. Suggest change to: "The lower values of SIE and SIA observed in recent years' reduce the trends observed in SIE (Parkinson, 2019) and SIA (1979-2018, based on Fetterer et al., 2017). However, these lower values are not significant for winter (September), leading to $7,000 \pm 6,000 \text{ km}^2 \text{ yr}^{-1}$ , (equivalent to $0.5 \pm 0.4\%$ relative to the 1981-2010 mean);". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; the paragraph is now revised and this sentence is not anymore included in this form.
65657	59	5	59	9	Suggest including the seasonality of sea ice – particularly the duration as described in Stammerjohn, S. E., D. G. Martinson, R. C. Smith, X. Yuan, and D. Rind, 2008.  Trends in Antarctic annual sea ice retreat and advance and their relation to El Niño–Southern Oscillation and Southern Annular Mode variability, J. Geophys. Res., 113, C03S90, doi:10.1029/2007JC004269. The duration trend is updated annually in the BAMS SotC report. See for example Reid, P., S. Stammerjohn, R. A. Massom, S. Barreira, T. Scambos, and J. L. Lieser, 2019: Sea ice extent, concentration, and seasonality [in "State of the Climate in 2018"]. Bull. Amer. Meteor. Soc., 100 (9), S178-S181.  While the trends in overall net SIE and SIA have diminished somewhat over the last few years of low sea-ice cover, there are still regions of statistically significant trends in duration. [ Kushla Munro, Australia]	Noted; aspects of seasonality are mentioned slightly earlier in this paragraph, where autumn advance and more is discussed, along with additional citation of a recent publication (Maksym 2019). Chapter two focuses on large scales, more information on regional changes is included in chapter 9.
127057	59	6	59	9	Standard errors apply to statistical estimates. Assessed values should have likely ranges attached to them, but they are not standard errors. [ Trigg Talley, United States of America]	Noted; the paragraph is now revised and this sentence is not anymore included.
78843	59	7	59	8	Wrong use of brackets. [ MONICA TOLOTTI, Italy]	Noted; the paragraph is now revised and this sentence is not anymore included.
30337	59	7			'winter': what about 'end-of-winter' or 'maximum'? [ Gilles Delaygue, France]	Noted; the paragraph is now revised and this sentence is not anymore included.
100539	59	8	59	8	I think it should read: $0.5 \pm 0.4\%$ per decade relative to .... [ Peter Lemke, Germany]	Noted; the paragraph is now revised and this sentence is not anymore included.
100541	59	9	59	9	I think, it should read: (equivalent to $3.4 \pm 2.0\%$ per decade relative to ...) [ Peter Lemke, Germany]	Noted; the paragraph is now revised and this sentence is not anymore included.
73589	59	10	59	10	Edit reference so ( is before 2017 and remove , [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted; the paragraph is now revised and this sentence is not anymore included.
57841	59	15	59	16	I don't think tis statement can be said with 'very high confidence'. Some of the trends are emerging, although there is no clear trend. Thus, 'High Confidence' would be a better judgement. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The confidence level in first sentence of the summary statement is now changed to high confidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36339	59	15	59	16	Here, and in the Executive Summary, proclaiming "very high confidence" in "no significant trend" is accurate but not informative. It can even sound contradictory. Following years of significant (albeit modest) increases in Antarctic sea ice, wouldn't it would be more informative to say that the increase stopped around 2015 and became something more complicated? [ Curt Covey, United States of America]	Accepted; The summary statement is now reworded, and the confidence level in the first sentence of the summary statement is now changed to high confidence.
115531	59	15	59	18	Does this statement imply that the Antarctic ozone hole ha no impact on Antarctic sea ice, although an impact on Antarctic surface temperatures is discussed? [ Rolf Müller, Germany]	Rejected; The paragraph in line 15-18 (SOD) did not address temperature or any attribution. Details on attribution can be found in chapter 9.
515	59	18	59	18	It is probably the interpretation of the evidence rather than the evidence itself that is contradictory. For an easy fix, I suggest changing "and often contradictory" to "and often seemingly contradictory". [ Claire Parkinson, United States of America]	Accepted. "Seemingly" is added now.
34847	59	21	60	12	The SOD claims a reduction in NH snow cover since 1978, with an anthropogenic influence since the 1950s. Please see rebuttal comment #11 above. [ Jim O'Brien, Ireland]	Rejected - Unclear what comment #11 is. This section only assesses observed change and does not assess attribution (note attribution is covered by other chapters (e.g. 3 and 9))
29609	59	21	60	12	No reference to snow cover in the Southern Hemisphere? For example, Cordero et al 2019 (Scientific Reports) or Saavedra et al (2018, The Cryosphere) and references therein may help to provide information on snow cover trends in the Andes. [ Villaseñor Tania, Chile]	Noted - Chapter covers large scale/ hemispheric scale and snow cover is more regional for Southern Hemisphere. Extent measurements are less meaningful since in most years there is no persistent seasonal snow cover in non-Antarctic areas outside of mountains.
9939	59	21	60	12	section 2.3.2.2 "Terrestrial snow cover". Coordination with section 8.3.1.7.2 "Seasonal snow cover" ch.8 is needed. There are different levels of confidence for the same assessments in these 2 chapters. [ Olga Zolina, France]	Noted - Ch 2 confidence levels are in agreement with Ch 9. Ch 8 makes conclusion regarding attribution and also considers different time period in its statement so not quite comparing the same things.
1237	59	21	60	12	The Arctic Monitoring and Assessment Program (AMAP), under the Arctic council, recently published two reports (Snow-water-Ice-Permafrost in the Arctic, SWIPA; Adaptive Actions in a Changing Arctic, AACA) which provide more details and are useful references. This applies to the whole section on the cryosphere. For the southern hemisphere, the situation is different as there is only snow on Antarctica and the Andes mountain range. The snow trends differ at different elevations. [ Rasmus Benestad, Norway]	Taken into account: The AMAP report (Brown et al. 2017) is cited in this section. Additional references were considered in revisions made for FGD. Chapter 2 considers large scale/hemispheric scale change. More information on snow in mountains is provided in Ch 9.
73973	59	23	59	23	This parameter is affected by regional differences, but it is not mentioned in this statement. That is why the statement can be misinterpreted by decision-makers. [ Elena Kozlovskaya, Finland]	Noted - Large scale change is considered in chapter 2 and AR5 conclusion with respect to hemispheric scale change has been provided.
52823	59	23	59	40	Figure 2.20 depicts April snow cover as supporting a loss of NH snow cover. To be open and transparent the authors must also show fall snow cover (which is increasing in some datasets) or winter which has no real trend. This is an example that under critical review may be shown to be a biased way of viewing the observations. Please include the other months=, not simple the one that supports a particular point of view. The dismissal of the NOAA dataset by Robinson is unprofessional and does not address deficiencies NOAA finds with the datasets of Brown and the others. Though on a much smaller scale, Christy (2012 J Hydrometeorology, updated through 2019) finds no significant trends in snowfall in the Sierra of California nor in the Cascades of Oregon and Washington beginning in 1890. [ John Christy, United States of America]	Taken into account - Fall SCE is mentioned in text and figure in ch 9 (note reference is made to Ch 9) shows SCE for all months. Data for pre-satellite period is only available for two months (note no data for fall) and is the reason April was used to show longer term trend in spring SCE for Northern Hemisphere (note large scale change is focus of Ch 2). NOAA has not been dismissed but rather we have indicated that similar results were not found with other products. Section has been revised to indicate there is greater uncertainty in autumn trends.
11465	59	29	59	40	It is true that the NH April snow cover has declined since 1922, but the trend is actually concentrated in Eurasia, while, over the entire period, there is no trend (at least until the mid-2000s) in North America. Does the hemispheric mean trend really convey physical meaning if there are two different realities hidden below? If one takes the trend since about 1950, then both continents agree, and I think that trend is more meaningful. [ Gerhard Krinner, France]	Taken into account - Ch 2 is concerned with large scale/hemispheric change. Text revised in FGD to indicate there are regional differences with reference to Ch 9 which provides more information on Regional variability.
43089	59	29		30	Read "based on Mudryk et al. (2017) indicates " rather than "based on (Mudryk et al., 2017) indicates " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial - copy edit to be completed prior to publication (sentence has also been revised)



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57843	59	30	59	31	It is important to also note that there is considerable interannual variability here as well. I would thus suggest to change to: "April SCE in the Northern hemisphere has declined by 0.27 million km <sup>2</sup> per decade, with significant interannual variability (Figure 2.20).". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account in FGD.
57845	59	30	59	31	Could you clarify the origin of the 0.27 million km <sup>2</sup> per decade value? If it is based on the best fit trend line then, because of the interannual variability, the 2018 SCE was actually larger than 1922. It might be better to say 'in general, April SCE in the Northern Hemisphere has declined by 0.27 million km <sup>2</sup> per decade (Figure 2.20).'. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - This refers to best fit trend line shown in figure 2.20 as indicated in the caption.
54885	59	30			Incorrect reference (and reference not listed in Reference section). Reference should be Mudryk et al., 2020: Mudryk, L., Santolaria-Otín, M., Krinner, G., Ménégos, M., Derksen, C., Brutel-Vuilmet, C., Brady, M., and Essery, R.: Historical Northern Hemisphere snow cover trends and projected changes in the CMIP-6 multi-model ensemble, The Cryosphere Discuss., <a href="https://doi.org/10.5194/tc-2019-320">https://doi.org/10.5194/tc-2019-320</a> , in review, 2020. [ Nancy Hamzawi, Canada]	Accepted - reference corrected in FGD.
54887	59	32	59	34	Not all datasets show negative SCE trends in all seasons. The NOAA climate data record shows positive trends in fall as is acknowledged on line 39. [ Nancy Hamzawi, Canada]	Noted - Text has been revised to be clear with respect to greater uncertainty in fall trends.
54889	59	32	59	34	Whether all seasons show a decrease depends on the product. I think something along the lines of the following would be more accurate: "Examining SCE trends for all seasons, there is larger uncertainty in the trends from October through February when the trend sign is product-dependent. Analyses using the NOAA climate data record [ref] show increases in SCE over this time period (Hernandez-Henriquez et al., 2015, Kunkel et al., 2016) while trend analyses based on satellite-borne optical sensors (Hori et al, 2017) or blended data (Mudryk et al. 2020, submitted) show decreasing trends in all seasons (Section 9.5.3.1).". [ Nancy Hamzawi, Canada]	Accepted - Text revised to indicate dependence on products and to be clear about the uncertainty in fall trends.
57847	59	36	59	37	I wonder if it is possible to represent some of the seasonal SCE data sets in Figure 2.20. Whilst the current figure has been designed with simplicity in mind, the inclusion of seasonal SCE estimates would be a useful addition. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - Figure 2.20 is meant to show long-term change in spring SCE. However the longer-term data is not available for other seasons (only 2 months available). SCE in other seasons is discussed in the text and reference is made to Ch 9 which has a figure showing trends for all seasons during satellite era.
73591	59	40		40	References should be in chronological order. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
90355	59	40			reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Editorial - copy edit to be completed prior to publication.
54891	59	46			Incorrect reference (and reference not listed in Reference section). Reference should be Mudryk et al., 2020: Mudryk, L., Santolaria-Otín, M., Krinner, G., Ménégos, M., Derksen, C., Brutel-Vuilmet, C., Brady, M., and Essery, R.: Historical Northern Hemisphere snow cover trends and projected changes in the CMIP-6 multi-model ensemble, The Cryosphere Discuss., <a href="https://doi.org/10.5194/tc-2019-320">https://doi.org/10.5194/tc-2019-320</a> , in review, 2020. [ Nancy Hamzawi, Canada]	Accepted - reference corrected in FGD.
54893	59	47			The Brown 2002 data as referenced wasn't used as is. The following is more accurate: "...in situ data (Brown 2002, recalibrated to the multi-observation data as described in Mudryk et al, 2020).". [ Nancy Hamzawi, Canada]	Taken into account in revisions for FGD in documentation included for figure in FAIR data table.
57849	59	53	59	55	The reference of Hammond et al. (2018) is not provided and so I cannot review it. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - reference included in list in FGD.
57851	59	55	60	2	The reference to Barichivich et al. (2013a) is used to show that vegetation growing season can be used as a proxy for snow cover duration. This is fine, but the text must state that snow cover duration is only inferred in this sentence i.e. "Inferred Arctic snow-cover...". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - section has been revised to include more recent references based on observed snow cover duration.
30339	60	1	60	2	"12.6 days' and '6.2 days' per decade? [ Gilles Delaygue, France]	Taken into account in revised text.
57853	60	4	60	5	The Kunkel et al. (2016) study only found a robust negative trend for North America, so generalising to the Northern Hemisphere is not suitable. I suggest change to: "Maximum snow depth has generally decreased since the 1960's across North America and generally decreased across the Northern Hemisphere (Kunkel et al., 2016).". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Revisions have been made to section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54895	60	4	60	7	A number of the references in this section should be re-examined as I don't believe all these studies used satellite passive microwave data. In general, this paragraph understates the amount of regional and inter-dataset variability in historical snow water equivalent trends. There is also new literature which will emerge soon on updated terrestrial snow trends, particularly snow water equivalent. [ Nancy Hamzawi, Canada]	Taken into account - Revisions made to indicate that results are based on microwave and other products and to be clear about variability. Additional recent references considered in revisions made for FGD.
54897	60	4	60	7	I think this section needs to acknowledge the substantial uncertainty in trends in maximum snow depth and SWE which can vary substantially depending on the region, dataset and period of analysis. E.g. Brown et al., 2017 reports general increases over the Arctic in maximum depth until the mid 1990s after which there is evidence for a reversal to decreasing trends. [ Nancy Hamzawi, Canada]	Taken into account - Revisions made regarding uncertainty and additional references considered in revisions for FGD.
54899	60	4	60	7	I'm not sure any of the cited studies examine trend magnitude or significance as a function of latitude or make statements in line with the claim. [ Nancy Hamzawi, Canada]	Taken into account in revisions to section.
2815	60	6	60	6	Marty et al. (2017) is missing in the ref list [ Antoine RABATEL, France]	Editorial - reference list has been corrected.
98747	60	7	60	7	Do you want to add paleo-SWE perspective from western North America? Pederson et al. (2011). <a href="https://science.sciencemag.org/content/333/6040/332">https://science.sciencemag.org/content/333/6040/332</a> [ Meredith Parish, United States of America]	Noted - Reference provided is Pre AR5 ref - doesn't fit large scale change which is focus of Ch 2.
73593	60	11	60	11	Delete , after 1981. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
73975	60	11	60	12	The same comment: general decline in Northern Hemisphere does not mean that regionally the trend would be different. There is a danger of misinterpretation: does this statement mean that the risk of flood during spring is generally decreasing in Northern Hemisphere? [ Elena Kozlovskaya, Finland]	Noted - Chapter 2 covers large scale/hemispheric change rather than regional changes or impacts of change or processes. Regional change is covered more in Ch 9. Text has been revised to mention regional differences in trends.
71863	60	15	61	30	Give the numbers in sea level equivalent, including in the Figure. [ John Church, Australia]	Rejected; this section focuses on glacier mass, sea level related information is given in other parts of the report.
57681	60	15	64	6	Lack of observational data on glacier terminus/length change and grounding-line retreat: Sections 2.3.2.3 and 2.3.2.4 include "extent" in the section headings but the measurement/observational data essentially just focuses on mass balance. While I recognise that mass balance changes are most relevant in terms of sea-level change, I am surprised that these sections do not include short paragraphs on observations of glacier terminus change. There is a vast body of literature on glacier terminus change and the data are highly important in terms of understanding ice-ocean-atmosphere interactions as well as the role of topography and glacier geometry in modulating the impacts of climate change. I would therefore recommend the inclusion of short paragraphs on glacier terminus change, if space allows. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; Due to length limits, addition of terminus/length change is not included. We changed the subsection heading removing "and extent", so that the heading is more corresponding with the text content.
88051	60	15			I find this section to a large extent rather repetitive to respective sections in Ch9, partially also in Ch12. Also Ch 1, Ch2, Ch3, Ch8 seem to repeat or even "re-invent" (particularly Ch12) statements on glaciers. In any case, there is need to pay much attention on harmonizing glacier numbers and references under the lead of Ch9. [ Georg Kaser, Austria]	Noted. Some repetition between chapters is unavoidable. Here in chapter 2, the main focus is on large spatial scales. New consistency checks with chapter 9 on updated numbers have been made.
88047	60	17	60	17	It was agreed in AR5 to not use the misleading term "ice caps" - see Glossary AR5 WG1. So, there is no need for a formulation from "ice caps and glaciers" to collectively called "glaciers". Also the term mountain glaciers is superfluous except when separating the glaciers on mountains from all other glaciers which in turn brings up the issue of defining mountains (see SROCC Ch2). So just use "glaciers" and "ice sheets" by following the AR5 WG1 Glossary and in accordance with SROCC. [ Georg Kaser, Austria]	Accepted; Glaciers is now used here alone.
57855	60	17	60	20	This sentence contains too much information and it is describing separate topics in my opinion. Split into two sentences instead, the first containing "Glaciers were smaller during the Holocene", the second containing "Worldwide loss of glacier mass since 1970.". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The sentence is now split into two sentences.
15475	60	17	60	45	The units "kyr" and "ka" are used interchangeably. Please consider whether harmonization of unit is required. [ SAI MING LEE, China]	Rejected; the terms are used as follows: "ka" = thousands of years ago; "kyr" = thousand of years.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73977	60	19	60	23	The sentence states that general trend is that total mass loss has been increased, but there are considerable inter-annual and regional variations. These variation can be more important for decision makers and climate change mitigation and adaptation that the reported global trend. [ Elena Kozlovskaya, Finland]	Rejected; chapter two has a focus on large spatial scales, regional scale aspects are mainly dealt with in chapter 9. Therefore this is not changed here.
11467	60	20	60	20	"globally coherent picture" - I seem to remember that a native speaker once told me that "consistent" was better in such situations. [ Gerhard Krinner, France]	Rejected. The wording is part of the citation of SROCC, therefore it is kept.
57857	60	24	60	25	I'm interested to know why the Canadian Arctic has been singled out in this sentence. This ice masses in this region have generally been stable but recently losing more mass due to a reduction in SMB (Gardner et al., 2011). Other regions of the world (e.g. Svalbard, European Alps) deserve a mention here as well. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The sentence is now removed.
16303	60	24	60	25	why single this region out? This should be appeared in the following paragraph [ Cunde Xiao, China]	Accepted; The sentence is now removed.
18405	60	24	60	25	"There was limited evidence (high agreement) that the current rate of glacier mass loss in the Canadian Arctic is larger than at any time during the past 4 kyr." - I would add the references provided by SROCC for this statement, namely (Fisher et al., 2012; Zdanowicz et al., 2012) to show that the evidence is based at the ice core records. Please notice that in the ch. 9 (page 71 lines 37-38 ) a shorter or much larger periods of glacier retreat in the Arctic are mentioned ("Exposure of plants emerging from beneath glaciers confirm that glaciers are retreating from areas that have been covered with glaciers for at least 1300 years in Svalbard (Miller et al., 2017) and 40,000 years in Canada (Pendleton et al., 2019)"). Although I can see the difference between the two messages, a slight contradiction still exists. [ Olga Solomina, Russian Federation]	Noted. The sentence about the Canadian Arctic referring to SROCC was removed.
52167	60	24	60	28	Values for the assessment of glacier mass changes are taken from SROCC. Please take into account that section 9.5 is updating these values with new findings (table 9.3). For the sake of consistency between chapters we need to agree in the values for the FGD. [ Lucas Ruiz, Argentina]	Accepted; this part is now revised, and it is now stated more clearly where findings from SROCC are summarized.
32113	60	26	60	28	Interchange sentences,, first continue about arctic glaciers and then about non-polar ones. [ Anja Wendt, Germany]	Noted; this paragraph was revised and reduced in length, and the details as commented are not included anymore.
73595	60	26		27	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; The respective part is not included in the paragraph anymore.
2817	60	27	60	28	Should precise the time period. Is it the same 2006-2015 period as for the former sentence? [ Antoine RABATEL, France]	Noted; this paragraph was revised and reduced in length, for the part where a glacier mass change is given, also the period it is valid for is listed.
89367	60	27	60	28	Is this estimate for glaciers distinct from the Greenland Ice Sheet? If so, where does the value come from? With gravimetry, it can be difficult to separate the signals of peripheral glaciers and the ice sheet, which can heavily bias the final estimate if only talking about glaciers as defined here. If this value is based on Zemp et al. (2019), I think the uncertainty value should be a bit higher than what is given here. [ Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Noted; this part with information from earlier assessments is now revised and reduced in length. The finding from SROCC on Arctic glaciers is not included here anymore. More details on this can be found in SROCC.
96229	60	30	61	3	The usage of kyr and ka is a bit inconsistent, and it is not always clear whether the reference is present day. [ Nicole Wilke, Germany]	Rejected; the terms are used as follows: "ka" = thousands of years ago; "kyr" = thousand of years.
2819	60	32	60	32	LDT needs to be defined [ Antoine RABATEL, France]	Rejected. LDT is explained earlier in the chapter specifically in cross chapter box 2.1. Due to length limits it is not explained here again.
11469	60	32	60	32	I know that "LDT" is defined on page 10 of this chapter, but most readers of this section will not have read page 10 of the chapter, and if so, they will have forgotten what LDT means. So spelling this out would probably be preferable. [ Gerhard Krinner, France]	Rejected. LDT is explained earlier in the chapter specifically in cross chapter box 2.1. Due to length limits it is not explained here again.
73597	60	32		32	Define LDT or insert a flag to show where the definition can be found. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. LDT is explained earlier in the chapter specifically in cross chapter box 2.1. Due to length limits it is not explained here again.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18407	60	33	60	33	"Glaciers worldwide expanded thereafter" - I would not use the word "worldwide" due to the great regional variability of the timing of Neoglacial advances. Probably: "in many regions" instead. [ Olga Solomina, Russian Federation]	Accepted. The wording is changed now.
57859	60	33	60	34	It might be better to be explicit in what caused the expansion of glaciers: "Glaciers worldwide expanded thereafter as the climate cooled, with varying extent and timing of advances regionally.". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Ch. 2 is not supposed to detail attribution (which can be found more about in ch. 9).
517	60	34	60	34	It is a mistake to say that "Glaciers have essentially all retreated", as then a few exceptions can be used to invalidate the statement. It would be far better to use phrasing as on p. 61, line 25, in the summary statement saying that "... with few exceptions, glaciers worldwide have retreated ..." [ Claire Parkinson, United States of America]	Accepted; The sentence is now reworded and consistent with the statement in the summary at the end of the section.
69561	60	35	60	35	"glaciers in most places is still larger than those of the early to middle Holocene minimum." Solomina et al 2015 make this statement only about the northern hemisphere, I think. And there is a huge spatial bias in sampling for these glacier records. So "most places" should probably be rephrased. [ gerard Roe, United States of America]	Accepted; NH is now included as suggested, the sentence is slightly reworded ("the" removed ahead of "modern glaciers"), but most places is kept in the sentence.
78845	60	36	60	37	The sentence "However, when comparing Holocene and present glaciers extents it is important to take into account the response time of glaciers" is confusing as it does not provide a hint on response time of glaciers, and the following sentences do not help. [ MONICA TOLOTTI, Italy]	Accepted; The sentence is now reworded, using "adjustment time", and detailing that the adjustment time is relatively long.
68195	60	36	60	38	Note that due to reviewer comments on FOD and that response time is defined as adjustment from one steady state to another, it was decided in Ch9 not to use the term "response time" but adjustment time and state that glaciers are presently in disequilibrium due to the warming of recent decades and that the disequilibrium will persist as warming continues (see p 9-68 line 8-23) suggest to be consistent in report describing this disequilibrium of glaciers [ Guðfinna Aðalgeirsdóttir, Iceland]	Accepted. The wording is changed now. It is also now referred here to chapter 9.
69563	60	37	60	38	"account the response time of glaciers; the majority of modern glaciers are currently not in balance with respect to current climatic conditions, and, hence, committed to further ice loss (Jóhannesson et al., 1989)." Jóhannesson doesn't really address disequilibrium, so the citation is not the best. There are plenty that do address it specifically. Christian et al. (2018, Journal of Glaciology, v 64) is a recent paper that addresses this, and has a ton of other earlier references in it. [ gerard Roe, United States of America]	Accepted; The citation of Christian et al., 2018 and more relevant citations are now included here.
52169	60	37	60	38	Recent findings regarding the commitment glacier changes were published by Marzeion et al (2014;2018 and submitted). Please include them as a citation to support this statement with more updated literature.  Marzeion, B. et al. Partitioning the Uncertainty of Ensemble Projections of Global Glacier Mass Change. Earth's Futur. (submitted, 9999).  Marzeion, B., Cogley, J. G., Richter, K. & Parkes, D. Glaciers. Attribution of global glacier mass loss to anthropogenic and natural causes. Science 345, 919–921 (2014).  Marzeion, B., Kaser, G., Maussion, F. & Champollion, N. Limited influence of climate change mitigation on short-term glacier mass loss. Nat. Clim. Chang. 8, 305–308 (2018). [ Lucas Ruiz, Argentina]	Accepted; The sentence is now reworded, and more recent citations are added, among them one of the suggested publications by Marzeion et al. (2018). Due to length limits and main focus of chapter 2 not on attribution and projections, the two other suggested citations are not included.
24131	60	38	60	38	There are better and more recent evidences with quantitative estimates such as Marzeion et al. (2018). Rereferences: (1) Solomina, O., Haeberli, W., Kull, C. and Wiles, G., 2008. Historical and Holocene glacier-climate relations: general concepts and overview. Global and Planetary Change 60, 1-9. (2) Marzeion, B., Kaser, G., Maussion, F., Champollion, N., 2018. Limited influence of climate change mitigation on short-term glacier mass loss. Nature Climate Change, Vol. 8, Letters, pp. 305-308. Available at: <a href="http://doi.org/10.1038/s41558-018-0093-1">http://doi.org/10.1038/s41558-018-0093-1</a> . [ Wilfried Haeberli, Switzerland]	Accepted; The sentence is now reworded, and a more recent citations are added (including one of the two suggested here, Marzeion et al. 2018).
11471	60	40	60	40	"plants that are now emerging from their protective glacier cover" - strange wording. Sounds like plants grow under glaciers... [ Gerhard Krinner, France]	Accepted; This is now reworded.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57861	60	40	60	41	In Figure 2.21 it looks like more glaciers advanced during the MWP which suggests this period was cooler, or potentially has more precipitation. Is this an issue of sampling? Some discussion of what is happening here would be useful as the term Medieval Warm Period implied warming and Figure 2.21 slightly contradicts this. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The statement is now reworded and MWP not used as a term/abbreviation anymore.
35139	60	40	60	45	Similar records are also available from an ice cap in NW Iceland that show it is now smaller than any time in the last 2 kyr (Harning et al., 2016, 2018). REFERENCES: Harning, D.J., Geirsdóttir, Á., Miller, G.H., Anderson, L., 2016. Episodic expansion of Drangajökull, Vestfirðir, Iceland, over the last 3 ka culminating in its maximum dimension during the Little Ice Age. Quat. Sci. Rev. 152, 118-131. Harning, D.J., Geirsdóttir, Á., Miller, G.H., 2018. Punctuated Holocene climate of Vestfirðir, Iceland, linked to internal/external variables and oceanographic conditions. Quat. Sci. Rev. 189, 31-42. [ David Harning, United States of America]	Accepted; information from Iceland is now added.
18411	60	40	60	45	I would distribute the specific references for each region, e.g. Liubinski et al.,1999 for Franz Josef Land, Miller et al., 2013 for Spitsbergen etc. Moreover, the reference (or references) needs for the following statement: "in northeast Canada and across Greenland to Svalbard, many are smaller than they have been in 4 kyr." The literature that I know shows a more limited interval. For instance, Lowell et al., 2013 reported that "Istorvet cap (east Greenland) was smaller than at present from AD 200 to AD 1025". Miller et al. 2017: "The widespread exposure of entombed plants dating from the first millennium AD suggests that Svalbard's average summer temperatures of the past century now exceed those of any century since at least 700 AD, including medieval times". Pendleton et al., 2019: "Similar to the Svalbard study, plant radiocarbon ages and aerial imagery suggests that Divide Ice Cap (Baffin Island) reached its maximum Neoglacial extent during the LIA and that warming since the early 1900s has reduced glacier dimensions to a smaller size than anytime since 1000 CE." In Schweinsberg et al., 2017, 2018 the 14C moss ages in the Sukkertoppen region (Greenland) are interpreted as the evidence of a series of glacier advances, but the authors say nothing about the scale of retreat. Thus, my suggestion is to be more precise and limit the statement by citing the regional dates. [ Olga Solomina, Russian Federation]	Noted; This paragraph is now revised, and individual regions are not detailed. Length limits and chapter scope did not allow inclusion of regional details.
30341	60	40			'are now emerging': 'have been emerging'? (i guess some of the dated plants are very old) [ Gilles Delaygue, France]	Noted; This is now reworded, and the term "emerging" is not used here anymore.
2821	60	41	60	41	MWP needs to be defined [ Antoine RABATEL, France]	Noted; The term MWP has been deprecated.
2823	60	41	60	41	Should precise where Franz Josef Land is located [ Antoine RABATEL, France]	Noted; Franz Josef Land is not anymore listed explicitly, therefore no further explanation is added to the text.
18409	60	41	60	41	"some" instead of "most" is more accurate [ Olga Solomina, Russian Federation]	Taken into account; the sentence is now reworded and neither some nor most is added here.
11473	60	43	60	44	"in Arctic Canada, at least 30 glaciers are now smaller than any time in more than 40 kyr": Given the huge number of glaciers in Arctic Canada, this number doesn't really mean anything except if you can say that "among X studied glaciers, 30 (or "all", or "all but Y") are now smaller than before" [ Gerhard Krinner, France]	Noted; the sentence is now revised and this detail is not included explicitly anymore.
57863	60	47	60	48	Zemp et al. (2019) not referenced in the bibliography. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; The reference is now added to the reference list.
89371	60	47	60	48	Reference Zemp et al. 2019 is missing from the list of references. [ Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Noted; The reference is now added to the reference list.
52171	60	47	60	49	Please rewrite this sentence for clarification. As stated, it looks like if Farinotti et al. 2019 quantified the glacier mass changes. Meanwhile, they assess the present glacier volume. [ Lucas Ruiz, Argentina]	Accepted; This is now reworded and citation of Farinotti removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57685	60	47	60	51	I would recommend expanding from "direct observations" and including a sentence that references the numerous studies that have employed historical maps and geomorphic evidence in order to quantify glacier area and/or volumetric change since the Little Ice Age (e.g. Hannesdóttir et al., 2015; Weber et al., 2019, 2020). While these studies have greater uncertainty, there have been efforts to quantify these errors in recent publications (e.g. Weber et al., 2020) and they still indicate overall glacier recession and thinning since the Little Ice Age with high confidence. References: Hannesdóttir, H., et al. (2015). Changes in the southeast Vatnajökull ice cap, Iceland, between ~1890 and 2010. The Cryosphere 9, 565-585. Weber, P., et al. (2019). Evolution of the Norwegian plateau icefield Hardangerjøkulen since the 'Little Ice Age'. The Holocene 29, 1885-1905; Weber, P., et al. (2020). An ~1899 glacier inventory for Nordland, northern Norway, produced from historical maps. Journal of Glaciology 66, 259-277. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; This paragraph is revised now, and instead of "direct observations" the wording "observations from in situ and remote sensing data" is used. Due to length limits (and main focus on large scale on observations) we cannot include these references here. However, they were communicated to chapter 9.
52125	60	47	61	3	Arguably some reference to high Asian glaciers in arid Central Asia could be considered, see Hoelzle, M., Barandun, M., Bolch, T., Fiddes, J., Gafurov, A., Muccione, V., Saks, T. and Shahgedanova, M., 2019. The status and role of the alpine cryosphere in Central Asia. In The Aral Sea Basin. Taylor & Francis. AND Shahgedanova, M., Afzal, M., Hagg, W., Kapitsa, V., Kasatkin, N., Mayr, E., Rybak, O., Saidaliyeva, Z., Severskiy, I., Usmanova, Z. and Wade, A., 2020. Emptying Water Towers? Impacts of Future Climate and Glacier Change on River Discharge in the Northern Tien Shan, Central Asia. Water, 12(3), p.627. [ Kathryn Fitzsimmons, Germany]	Rejected; this paragraph is now revised. Due to length limits (and main focus on large scale on observations) we cannot include the suggested references here. However, they were communicated to chapter 9.
2825	60	48	60	48	No need to quote Farinotti et al. (2019) here. The sentence refer to new global compilation of glacier mass and areal changes, when Farinotti et al. (2019) present a new global scale estimation of glacier volume [ Antoine RABATEL, France]	Accepted; This is now reworded and citation of Farinotti removed.
89369	60	49	60	49	Adding 'globally' here feels awkward. I would remove it and add a comma after 'regions' [ Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; The sentence and this wording is not anymore included as it was in the SOD.
57683	60	49	60	49	It would be useful to clarify what is meant be "direct observations" in this context (e.g. by specifying in brackets). For example, do "direct observations" also include measurements made from remotely-sensed data (e.g. satellite images)? To my mind, direct observations would, strictly speaking, only refer to in-situ glaciological measurements. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; the term "direct observations" is now replaced by "observations from in situ and remote sensing data".
57741	60	49	60	51	This sentence is difficult to read 'Globally, for the majority of regions post-LIA glacier retreat started, with regional differences of several decades, between the 1850s and 1900s (Solomina et al., 2016), and the rate of mass loss has accelerated since the 1980s (Zemp et al., 2015) (Figure 2.21).' Perhaps The majority of global post-LIA glacier retreat started btw 1850-1900 with regional differences in start time of several decades. The rate of mass loss has accelerated since the 1980's. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The paragraph is now reworded, and the sentence addressed here is not anymore included as it was in the SOD.
57865	60	51	60	54	The Zemp et al. (2015) study only reviews glacier data up to 2010. Therefore the comparison should really be made to Zemp et al. (2019) which is more up to date. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The reference Zemp et al. (2015) is not included anymore. In addition to Zemp et al. (2019), also a newer reference Zemp et al. (2020) is cited now earlier in the same paragraph. The paragraph has been revised, and it includes a statement on change of global glacier mass loss with a different, new reference.
15477	60	51	60	54	Re: the glaciology unit "m w.e." (metre water equivalent). Usually, ice/mass loss is given in Gt (Gigatonnes) in IPCC reports. Presenting the mass loss in Gt or a note to explain how to translate m w.e. to Gt would help the readers appreciate the significance of change. As mentioned in lines 51-52, glacier mass loss during 2001-2010 is the greatest since observations began in the 1930s. It is recommended to reflect such significant change in the Executive Summary of the chapter as well as the Summary for Policymakers. [ SAI MING LEE, China]	Accepted; The unit w.e. (water equivalent) is avoided now.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16037	60	51	60	54	after "Observations beginning in the 1930s indicate that the greatest decadal mass loss occurred during 2001-2010 (Zemp et al., 2015), when the mass balance averaged $-0.5$ m w.e. (water equivalent) per year based on in situ measurements (glaciological method) and $-0.8$ m w.e. per year based on volume changes (geodetic method).", I suggest to add "In addition, the increases of melting estimated from these studies are underestimated given that the glacier-wide mass balances are influenced by glacier geometry changes controlled by the dynamic response of each glacier (Vincent et al., 2017)". Vincent C., A. Fischer, C. Mayer, A. Bauder, S. P. Galos, M. Funk, E. Thibert, D. Six, L. Braun, M. Huss. 2017. Common climatic signal from glaciers in the European Alps over the last 50 years. Geophys. Res. Lett., 44, doi:10.1002/2016GL072094 [Christian Vincent, France]	Rejected; The paragraph is now revised, but length limit and chapter focus did not allow for this addition.
2827	60	54	61	1	For High Mountain Asia instead of / or on top of Brun et al. (2017, 2018) you should consider quoting Shean et al. (Shean, D. E., Bhushan, S., Montesano, P., Rounce, D. R., Arendt, A., & Osmanoglu, B. (2020). A Systematic, Regional Assessment of High Mountain Asia Glacier Mass Balance. Front. Earth Sci, 7, 363.) and you can also refer to Dussailant et al. (Dussailant, I., Berthier, E., Brun, F., Masiokas, M., Hugonnet, R., Favier, V., ... & Ruiz, L. (2019). Two decades of glacier mass loss along the Andes. Nature Geoscience, 12(10), 802-808.) and Braun et al. (Braun, M. H., Malz, P., Sommer, C., Farias-Barahona, D., Sauter, T., Casassa, G., ... & Seehaus, T. C. (2019). Constraining glacier elevation and mass changes in South America. Nature Climate Change, 9(2), 130-136.) for regional estimates in the Andes. You can also refer to Chapter 12 where regional assessments are provided. [Antoine RABATEL, France]	Rejected; this paragraph was now revised, but length limits and chapter focus did not allow to add references to the suggested studies. Neither the Brun et al. 2017/2018 citations are included anymore.
57867	60	54	61	1	It may also be pertinent here to mention that some glacierized regions exhibit positive mass balance trend e.g. the Karakoram anomaly. This is discussed in Chapter 9 but is also important here. I suggest change to: "Observed glacier mass balances for 2010-2016 (the last year of complete surveys) are similar or even more negative (e.g. for Asian high mountain glaciers (Brun et al., 2017, 2018)), whilst some regions even experience positive mass balance trends (e.g. the Karakoram anomaly, Farinotti et al., 2019)". [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; this paragraph has been revised. Due to length limits and scope, regional exceptions are not detailed here. This and other regional aspects are detailed in chapter 9.
116001	60		60		has the rate of glacier mass loss increased or accelerated? Please check. [Valerie Masson-Delmotte, France]	Accepted; Information on changes of rates is now included in the third paragraph of this section, also with the new reference Hugonnet et al. 2021.
18413	61	1	61	2	"Holocene deglaciation" sounds a little confusing in this context implying the glacial-interglacial cycles rather than multidecadal and centennial variations that are discussed in Solomina et al., 2015. [Olga Solomina, Russian Federation]	Accepted; This is now reworded and moved within the subsection.
18395	61	8	61	10	The figure 2.21 still looks a little confusing - at least it needs additional explanations. First of all it is important to notice that the number of moraines in the LIA is greater because of the partly erased older moraines - this is not a message that the reader will immediately see from the figure. Probably it is also necessary to show the sample depth at the background or even normalize the number of advances by the number of records. The decrease of the number of advances up to zero in 20th century is not quite correct. There were many advances in 20th century, even in 2000s in some regions but of limited magnitude, so the curve for the 20th century that drops abruptly up to zero is not quite correct. I would suggest to show the line in 20th century as a dashed one and drop it somewhere in the middle. [Olga Solomina, Russian Federation]	Accepted; Fig 2.21a (now 2.23a) and caption are now revised, with clearer illustration and description.
66467	61	8	61	15	Comment to the description of Figure 2.21b: One important information is missing regarding the Accumulated Mass Balance, i.e. the "zero" year. For example, WGMS (2020) graph states "Cumulative values relative to 1976." (here 1970?). Without this information, it might be not clear why cumulative mass balance is $\sim +5$ m.w.e. at the beginning of the analysed time span. [Barbara Barzycka, Poland]	Accepted; Fig. 2.21b (now 2.23b) has been changed, showing now annual and decadal global glacier mass change.
93039	61	11			it should be noted here that the LIG and LGM GrIS shown in Figure 2.22 are based on model simulations constrained by ice core and sea-level data. [Bette Otto-Bliesner, United States of America]	Noted; What was Fig. 2.22 in the SOD is not anymore included in this chapter, since a similar figure is included in chapter 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
337	61	20	61	20	I was unsure what "flow" meant in this context. Do the authors mean glacier velocity? I checked Zemp et al. 2015 and I did not find any comment/data about glacier velocity in their study. So maybe another reference should be cited if a discussion of change in glacier velocity want to be made (but this is more process-related so maybe fit better elsewhere in the AR6). Also Figure 2.21 does not really illustrate "seasonal variability". Overall the sentence could be clarified/improved. [ Etienne Berthier, France]	Accepted; this section is now revised, and the respective sentence and wording "flow" are not included anymore.
57869	61	20	61	21	Probably good to emphasise here that Figure 2.21 does not present any seasonal data. This, suggest change to: "Glacier mass balance and flow undergo short-term inter-annual variability (Figure 2.21) and seasonal changes, which implies these are both superimposed on the overall negative trends (Zemp et al., 2015; 2019)". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; this section is now revised, and the respective sentence is not included anymore.
19741	61	20	61	21	Actually Figure 2.21 tells us nothing about seasonal variability [ philippe waldteufel, France]	Accepted; this section is now revised, and the respective sentence is not included anymore.
130477	61	20	61	21	Glacier advance may not equivalent to glacier flow, so Figure 2.21a is not appropriate to say the seasonality of glacier flow. [ Panmao Zhai, China]	Accepted; this section is now revised, and the respective sentence and wording "flow" are not included anymore.
52173	61	20	61	23	I have my concern about how the term "flow" is used in this sentence. The flow of glaciers (for example, expressed as surface velocity) could undergo substantial seasonal and short-term inter-annual variability. But glacier flow is not depicted in figure 2.21. So I think, here flow, is used to describe the advance or retreat of the front. If this is the case, please use the "fluctuation of front position" instead of "flow." [ Lucas Ruiz, Argentina]	Accepted; this section is now revised, and the respective sentence and wording "flow" are not included anymore.
2829	61	21	61	21	Should replace "seasonal" by "variability" [ Antoine RABATEL, France]	Accepted; this section is now revised, and the respective sentence is not included anymore.
69565	61	21	61	22	"There is insufficient evidence to conclude when the large-scale glacier retreat was last as rapid as recent changes" Check this for consistency with similar statements in Chapter 9. Also, to my ear, this is a puzzling way to frame the thought. Might say: there is no known past interval when glacier retreat was as rapid or as extensive. [ gerard Roe, United States of America]	Accepted; The sentence is now removed. The subsection was revised, and related information is included earlier in the subsection.
2831	61	21	61	23	I would remove this sentence (see next comment) [ Antoine RABATEL, France]	Accepted; the paragraph was revised, and the respective sentence is now removed.
127059	61	21	61	23	This assessment statement is not as helpful as saying that large-scale glacier retreat is more rapid than any time in at least the past XX years. [ Trigg Talley, United States of America]	Accepted; The sentence is now removed. The subsection was revised, and related information is included earlier in the subsection.
18397	61	21	61	23	"There is insufficient evidence to conclude when the large-scale glacier retreat was last as rapid as recent changes (Solomina et al., 2015)". I would add "in the Holocene" here. I wonder if there is some information on the rate of changes in the Late Glacial time. It would make sense to mention it here if there are good evidences. [ Olga Solomina, Russian Federation]	Accepted; The sentence is now removed here. The subsection was revised, and related information is included earlier in the subsection, with reference to the Holocene.
30343	61	21			'thus implying seasonality in the overall negative trends': not clear to me, i think it necessary to expand/explain (does it mean that the trend is due to ablation more than to accumulation?) [ Gilles Delaygue, France]	Accepted; this section is now revised, and the respective sentence is not included anymore.
89373	61	25	61	25	Unnecessary comma after 'retreated' [ Robert McNabb, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; This is now changed.
127061	61	26	61	26	Anomalous doesn't seem like the right word and could be too easily misinterpreted. Perhaps use "unusual". [ Trigg Talley, United States of America]	Accepted; The sentence is now reworded, and the term "anomalous" is not used anymore.
102743	61	26	61	27	"The number retreating is highly anomalous in the context of the last 2000 years (high confidence)". I have no doubt about this conclusion, but considering the evidence presented in the rest of this section it appears undocumented. [ Philippe Tulkens, Belgium]	Accepted; The summary statement is now revised, and the number of glaciers retreating is not commented here anymore.
24133	61	27	61	28	This important statement is highly questionable - where is the "high confidence from" and what are the references? [ Wilfried Haerberli, Switzerland]	Noted. Since this is a summary statement, no citations are included here. The statement is connected to the assessment earlier in the subsection, with literature cited. The subsection was revised relative to the version in the SOD.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69567	61	27	61	28	"Although most surveyed mountain glaciers are currently more extensive than during the middle Holocene " Agains, Solomina make this statement about the northern hemisphere, I think, so recommend a careful check. [ gerard Roe, United States of America]	Taken into account; the statement is now reworded, now addressing "many surveyed glaciers". More details are included in the text earlier in the subsection.
18399	61	27	61	28	"Although most surveyed mountain glaciers are currently more extensive than during the middle Holocene". I am uncomfortable with "most" here. Can we change to "many" or even "some" just because we do not have information from the majority of early-mid Holocene glaciers - we know something only about a few of them. [ Olga Solomina, Russian Federation]	Accepted. "most" is changed to "many". The statement was also revised slightly more.
81167	61	27	61	30	This sentence could be made more explicit in that more retreat is expected until glaciers are in balance with current climate and that the comparison to middle Holocene glacier stages is thus not direct. For instance "... Holocene (high confidence), they are projected to shrink (substantially?) more until they are in balance with and thus reflect current climate conditions, and ...". Or something like that? [ Andreas Käb, Norway]	Accepted; an addition about the disequilibrium and commitment to further ice loss is made to the end of the sentence to make this point clearer.
16041	61	27	61	30	"Although most surveyed mountain glaciers are currently more extensive than during the middle Holocene (high confidence), they generally are not in balance with respect to current climate conditions, and the rate of glacier retreat in the latter 20th and early 21st century appears unusual in a context of the Holocen (high confidence)." I do not think the cited papers (Solomina et al) prove that the present rate of glacier retreat cannot be found during the Holocene given the scarcity of data especially during the period of retreat. I suggest to be cautious here and to mention "medium confidence" [ Christian Vincent, France]	Rejected; the review in the cited publication includes data from different studies for a large number of glaciers from different regions, therefore we did not change the confidence statement.
57743	61	28	61	29	they generally are not in balance with respect to current climate conditions this is based on one 1989 study using model-based inference from physical and numerical first principals (no observations). Should attach low confidence" to this statement. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The respective statement earlier in the subsection is updated with another, newer reference (which cites again more references on the topic).
73599	61	29	61	29	Change 'century' to 'Centuries'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted; the respective wording is not anymore included.
2833	61	29	61	30	This sentence seems to contradict the sentence L.21-23 of the same page (previous comment). I would remove the sentence from L21-23 of this page 61. [ Antoine RABATEL, France]	Accepted; the sentence which was in lines 21-23 is not included anymore.
24385	61	30	61	30	The following sounds awkward, "appears unusual in a context of the Holocene (high confidence)." It would sound better as, "appears unusual in the context of the Holocene (high confidence)." [ Owen Cooper, United States of America]	Noted; the respective wording is not anymore included.
30345	61	30			This conclusion seems too loose to me: how 'unusual' is the rate to get 'high confidence'? This claim also contradicts the one at L.21-22 that past rate over the Holocene is not known enough to be compared with the recent one. [ Gilles Delaygue, France]	Accepted; this subsection is now revised, the wording of that "the global character of glacier mass loss is highly unusual" is explained with additional text, and the sentence earlier in lines 21-23 of the same page (in SOD) is not included anymore.
105093	61	33	61	33	The section on ice sheets includes a discussion on the mean ice sheets found today (Greenland and Antarctica), which is fine for this chapter. But it would be worth mentioning that other ice sheet existed during the last glacial - interglacial cycles, and that those were responsible for sea level changes. Actually, this would also make sense because in the following section on sea level, rates of sea level rise during the deglaciation are mentionned, without the actual ice sheet changes during the deglaciation being explained in the ice sheet section. [ Masa KAGEYAMA, France]	Noted - Comment is generally beyond scope of section. Discussion has been limited to indicators of recent change (Greenland and Antarctica Ice Sheets) to assess large scale change over long-term. Given space constraints it is not possible to include detailed discussion of other ice sheets that no longer exist.
105095	61	33	61	33	Fig 2.22 could make the reader believe there was no other ice sheet. This fact could be added in the caption? [ Masa KAGEYAMA, France]	Not Applicable - Figure removed from chapter 2 and appears in chapter 9 in FGD (reference made to ch 9 in text).
83229	61	35	61	35	Change "Greenland ice sheet" to "Greenland Ice Sheet" here and throughout the text - it is capitals as it's a proper name. [ Robert Massom, Australia]	Editorial - copy edit to be completed prior to publication.
34849	61	35	62	42	The SOD claims that the Greenland ice sheet state is unprecedented over centuries. Please see rebuttal comment #8 above. [ Jim O'Brien, Ireland]	Noted - Not sure what comment 8 is. The text cited by reviewer is summarizing conclusions of SROCC assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83439	61	36	61	36	In Chapter 1 Cross-Chapter Box 1.1, Table 1 the acronym GIS is used for the Greenland ice sheet on page 1-21, whereas Chapter 2 (see comment below) and Chapter 9 (e.g. p. 11 line 12) use GrIS. [ Antje H. L. Voelker, Portugal]	Editorial- copy edit to be completed prior to publication (GrIS is now used).
93041	61	36			The paleoclimate chapters in the AR4 and AR5 also assessed GrIS - for the LIG. [ Bette Otto-Bliesner, United States of America]	Taken into account - revision made in summary of AR5 assessment to include past few million years
57871	61	38	61	39	In this sentence, the other major SMB component, precipitation changes, is not included. I think it might also be useful to state the role of precipitation changes here so that it can be placed in the context of the other SMB parameters. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable - Sentence has been removed from summary of AR5 conclusions. Attribution and process are beyond scope of Ch 2 and is covered in Ch 9.
29855	61	39	61	39	Consider adding "calving"; for instance: "...and outlet glacier discharge (calving)". [ Hernan Edgardo Sala, Argentina]	Not Applicable - Sentence has been removed from summary of AR5 conclusions. Attribution and process are beyond scope of Ch 2 and is covered in Ch 9.
68197	61	40	61	42	Note that 2012 was not record mass loss YEAR (according to IMBIE) maybe an explanation that high accumulation and refreezing of the melt caused it not becoming record, see Ch9 p49 line 32-34, maybe it is redundant to state this two times in report and delete in one of the places? [ Guðfinna Aðalgeirsdóttir, Iceland]	Taken into account. No reference to 2012 is made on page 61 but later in section. The reviewer's comment is noted and reference to 2012 has been removed to focus more on the longer-term changes.
73601	61	40		40	Fully italicise 'likely' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
90359	61	40			italics on "extremely likely" [ Jeannine-Marie St-Jacques, Canada]	Editorial -copy edit to be completed prior to publication.
2015	61	44	61	53	Just check that this is consistent with what is said in Chapter 9. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted - consistency checked in FGD preparation
57873	61	45	61	47	I actually think that both of these studies argue for at least a partial glaciation in East Greenland, rather than offering opposing views. I suggest reframing this sentence to: "Oscillations through the glacial-interglacial cycles of the Pleistocene are not well constrained; over the last 7.5 Myr, Greenland was at least partially glaciated, but the majority of Greenland was deglaciated." [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text revised.
2835	61	47	61	47	LIG needs to be defined [ Antoine RABATEL, France]	Noted - LIG is defined in Cross Chapter Box 2.1 Table 1 and has been deprecated in the FGD.
90361	61	49	61	50	merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial - copy edit to be completed prior to publication.
43091	61	49		50	Read "not well constrained (Helsen et al., 2013; Stone et al., 2013; Goelzer et al., 2016; Yau et al., 2016; Clark et al., in press; Sinclair et al., 2016; Vasskog et al., 2015)." rather than "not well constrained (Helsen et al., 2013; Stone et al., 2013; Goelzer et al., 2016; Yau et al., 2016) (Clark et al., in press; Sinclair et al., 2016; Vasskog et al., 2015)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial - copy edit to be completed prior to publication.
3949	61	50	61	50	merge references in the two brackets into one bracket [ Sabine Baumann, Germany]	Editorial - copy edit to be completed prior to publication.
13239	61	50	61	50	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial - copy edit to be completed prior to publication.
73603	61	50		50	remove extra )( [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
73605	61	50		50	In press' reference should be at the end of the list of references. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
57875	61	51	61	53	I would like to know what the peak ice volume was (with error bars) during the LGM alongside its SLE value. This would provide a nice comparison to present day values. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - A range is provided for mass as SLE for LGM based on literature cited.
29857	61	55	61	56	Consider modifying last part of this sentence, for instance: "...show that the GrIS retreated rapidly during the early Holocene but halted periodically and at different times and ice-margins positions" or "...show that the GrIS retreated rapidly during the early Holocene but halted periodically and following a complex ice-margin chronology". [ Hernan Edgardo Sala, Argentina]	Taken into account in revised text.
18415	62	2	62	3	I would recommend to consider here an additional reference of one important paper Lecavalier, B. S., Fisher, D. A., Milne, G. A., Vinther, B. M., Tarasov, L., Huybrechts, P., ... & Dyke, A. S. (2017). High Arctic Holocene temperature record from the Agassiz ice cap and Greenland ice sheet evolution. Proceedings of the National Academy of Sciences, 114(23), 5952-5957. [ Olga Solomina, Russian Federation]	Noted - A number of additional references were considered and included in FGD.
2837	62	4	62	4	MH needs to be defined [ Antoine RABATEL, France]	Noted - MH is defined in cross Chapter Box 2.1 Table 1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57877	62	4	62	6	Is it possible to also mention the extent of the GrIS during the Medieval Warm Period? Given that this is prominent in the discussion about glaciers and ice caps it might be worthwhile to include it here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - A description of general changes have been provided to provide context for recent trends rather than a more detailed discussion of ice sheet extent.
57879	62	11	62	17	I suggest putting dates on this figure to make it clear what time periods are being considered. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable - Figure removed from chapter 2 and appears in chapter 9 in FGD (reference made to ch 9 in text).
30347	62	11			Figure 2.22: maybe add the information that the map line corresponds to the current sea level (z=0). [ Gilles Delaygue, France]	Not applicable - Figure removed from chapter 2 and appears in chapter 9 in FGD (reference made to ch 9 in text).
29859	62	12	62	12	Typo, add space in "Annex II for details". [ Hernan Edgardo Sala, Argentina]	Not applicable - Figure removed from chapter 2 and appears in chapter 9 in FGD (reference made to ch 9 in text).
29861	62	15	62	15	The acronym "AIS" has not been previously defined in this chapter, please include here its meaning. [ Hernan Edgardo Sala, Argentina]	Not applicable - Figure removed from chapter 2 and appears in chapter 9 in FGD (reference made to ch 9 in text).
57881	62	22	62	24	This statement is generally true. However, there is working come out now that is starting to build a picture of previous ice dynamics at key outlets glaciers. Vermassen et al. (2020) (doi: 10.1029/2019gl085954) used sediment cores and historical documents to document the collapse of the Kangerlussuaq Glacier. This might be a useful reference to discuss the new observations being produced. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - A number of additional references were considered in preparation of FGD and included if appropriate.
71867	62	22	62	26	This assessment of 20th century change is incomplete and very unsatisfying. There are other manuscripts on 20th century change and need to discuss the results not just say various methods were used. [ John Church, Australia]	Noted - Subsequent paragraphs discuss 20th century change and also mass loss since LIA. Additional references were considered in preparing FGD and included if appropriate.
30349	62	25			'the last time' over which period? I guess during the deglaciation the rate was higher (not least because the volume was much higher than today) [ Gilles Delaygue, France]	Noted - We don't disagree with reviewer but in this statement we are only considering when was the last time rates were similar.
35533	62	26	62	26	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Noted - all references were checked and updated in FGD.
43093	62	29		30	Read " (AMAP, 2017; van den Broeke et al., 2017; Bamber, 2018a; Mougnot, 2019; IMBIE, 2019)." rather than " (AMAP, 2017; van den Broeke et al., 2017 Bamber, 2018a; Mougnot, 2019; IMBIE, 2019)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial - copy edit to be completed prior to publication.
2839	62	30	62	30	"et al." is missing in the ref. "Bamber et al. 2018; Mougnot et al., 2019". Also, remove the "a" in the reference to Bamber et al. 2018. [ Antoine RABATEL, France]	Editorial - copy edit to be completed prior to publication.
57883	62	30	62	30	Inconsistent reference to IMBIE and Shepherd et al. (2019). For these group reviews can I suggest using 'IMBIE' as this gives credit to the entire consortium rather than a subset. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - citation revised.
32115	62	30			reference IMBIE (2019) is (Shepherd et al., 2019) in Chapter 2 and (The IMBIE Team 2019) in Chapter 9, as it is proposed in the publication itself. Please harmonize across chapters. [ Anja Wendt, Germany]	Accepted - citation revised.
30351	62	30			references: Bamber 2018a & Mougnot 2019 (et al?) missing [ Gilles Delaygue, France]	Editorial - copy edit to be completed prior to publication.
90365	62	30			add ; after 2017 [ Jeannine-Marie St-Jacques, Canada]	Editorial - copy edit to be completed prior to publication.
57885	62	32	62	32	It is not immediately obvious in Figure 2.23 that the rate of mass loss has slowed in recent years. I suggest stating that since ~2016 the rate of mass loss has slowed. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account in revisions made to text.
29863	62	32	62	32	Use "century" instead of "Century". [ Hernan Edgardo Sala, Argentina]	Editorial - copy edit to be completed prior to publication.
5385	62	32			Much more needs to be said and shown concerning the statement "but decreased in recent years." The cumulative mass change in Fig. 2.23 makes this difficult to see. There is no discussion of when or where this shift occurred. Does it suggest a reversal of the losses? [ Bryan Weare, United States of America]	Taken into account in revisions made to text.
90367	62	32			lower case on century [ Jeannine-Marie St-Jacques, Canada]	Editorial - copy edit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57887	62	34	62	34	The 3800 +/- 339 Gt value is inconsistent with the values in IMBIE (2019). The value should be 3,902 +/- 342 Gt, unless additional analysis has been conducted. If this value is generated by multiplying 150 m yr <sup>-1</sup> by 26, this yields 3900, which is also inconsistent. The value of 3,902 +/- 342 Gt should be used here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Values in this section checked and errors corrected in FGD revisions and also to ensure consistency with chapter 9.
57889	62	34	62	34	The 10.6 +/- 0.9 is inconsistent with the IMBIE (2019) values. The value should be 10.8 +/- 0.9. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Values in this section checked and errors corrected in FGD revisions and also to ensure consistency with chapter 9.
57705	62	34	62	42	It is difficult to read the rates of mass change when they are put down like this. I think it would be more readable if they were somehow integrated in the figure or in a table as to quickly show correspondence between rate of loss and GMSL equivalent. If this is not possible, it should be written more clearly, separating ice loss from GMSL equivalent. Something like: the rate of ice sheet loss for 1995-2005 was 71±67 Gt/yr and rose to 273 ± 11 Gt/yr for 2005-2015. This is notably higher than the rate calculated for the 20th century: 75±29 Gt/yr. This would correspond to a GMSL equivalent of etc. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Agree with reviewer that this could be improved. Revisions made to improve clarity.
39097	62	34	62	42	In discussion about mass loss, add Smith et al and Velicogna et al. Smith, Ben, et al. "Pervasive ice sheet mass loss reflects competing ocean and atmosphere processes." Science (2020)., Velicogna, Isabella, et al. "Continuity of Ice Sheet Mass Loss in Greenland and Antarctica From the GRACE and GRACE Follow-On Missions." Geophysical Research Letters 47.8 (2020): e2020GL087291. [ Ola Kalen, Sweden]	Noted - Additional references for this section considered and added to revised text if appropriate.
30353	62	34			'lost 3800 Gt ... between 1992 and 2018': this does seem consistent neither with Fig 2.23 (showing loss over 1992-2017) nor with the loss of 3900 Gt given by Shepherd et al (2019) over 1992-2018. [ Gilles Delaygue, France]	Taken into account - Values in this section checked and errors corrected in FGD revisions and also to ensure consistency with chapter 9.
57891	62	35	62	37	The dates 1992-1997 correspond to the IMBIE (2019) study, Table 1, but the values don't match. Thus I would like clarification as to whether any additional analysis was undertaken to produce the value of -18 +/- 28 Gt yr <sup>-1</sup> and if so, can this be included in the report. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Values in this section checked and errors corrected in FGD revisions and also to ensure consistency with chapter 9.
2841	62	37	62	37	GMSL needs to be defined [ Antoine RABATEL, France]	Noted - section has been revised and acronym is no longer used.
57893	62	37	62	37	The value of 273 +/- 11 Gt yr <sup>-1</sup> is not the same as the one quoted in the WCRP Global Sea Level Budget Group (2018) . The value should be 272 +/- 11 Gt yr <sup>-1</sup> . [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Values checked in this section and any errors corrected in preparation of FGD
73607	62	37	62	38	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
57895	62	38	62	38	I can't trace the origin of this value. If you take the value from Kjeldsen et al. (2015) (doi: 10.1038/nature16183) then the value between 1983 and 2003 is 73.8 +/- 40.5 Gt. This may also be a better value given that the originally quoted value (71 +/- 67 Gt yr <sup>-1</sup> ) given that the error bars are very large on those. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - Paragraph has been revised to provide values for different time periods. All values have been checked and are consistent with Chapter 9.
73609	62	38		39	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
30355	62	39			'in the previous' [ Gilles Delaygue, France]	Taken into account in revisions to section.
57707	62	40	62	42	I think that the data about mass loss of just one year (2012) should not be included as this is an assessment on changing state of the climate system and one year anomaly does not bring useful information to the text. We should only be interested in trends. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - Agree with reviewer that focus should be on longer term trend. Text revised to remove reference to mass loss in 2012.
73979	62	40	62	42	The one-season mass loss was the greatest in 2012, but the annual variability is still existing. For decision makers it is important to know: how large this annual variability would be in the future? But the report does not answer to this question. [ Elena Kozlovskaya, Finland]	Noted - Ch 2 does not consider future conditions. This is considered in Ch 9. Reference to single year (2012) loss has been removed to focus on longer term trend.
90369	62	41	62	42	reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Editorial - copy edit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39579	62	45	62	51	Please add in Figure 2.23 the evolutions in PERCENTAGE of the total mass because it will be more informative since most readers do not know the total mass. [ François Gervais, France]	Reject - Figure is a fairly standard way to present these data.
57897	62	47	62	49	I would try to make the start and end of the graph begin and end at the dates of the data timescale for absolute clarity. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Reject - Figure is fairly clear and caption also indicates data range.
57669	62	47	62	55	Recently, first mean ocean temperature record for the last interglacial (LIG) has been obtained from noble gas measurements in ice cores (Shackleton et al., 2020). The study suggests that mean ocean temperature reached maximum value of $1.1 \pm 0.3$ °C warmer-than-modern values at the end of the penultimate deglaciation at 129 ka. Shackleton, S., Baggenstos, D., Menking, J.A. et al. Global ocean heat content in the Last Interglacial. Nat. Geosci. 13, 77–81 (2020). <a href="https://doi.org/10.1038/s41561-019-0498-0">https://doi.org/10.1038/s41561-019-0498-0</a> [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. Comment does not correspond to content.
73611	62	47		47	Change 'Greenlandic' to 'Greenland'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - revision made.
57687	62	54	63	4	I would recommend adding a short paragraph on glacier terminus/length change either before or after lines 22-42, if space allows. There are numerous important studies on terminus fluctuations of marine-terminating outlets of the Greenland Ice Sheet that have identified the key role that topography (bed and fjord geometry) has in modulating or amplifying glacier terminus change (e.g. Carr et al., 2017; Bunce et al., 2018; Hill et al., 2018). Briefly summarising the observational data from such studies is relevant in terms of (a) linking observations of the changing state of the cryosphere with driving mechanisms and (b) projections of future glacier change in Greenland. References: Carr, J.R. et al. (2017) Threefold increase in marine-terminating outlet glacier retreat rates across the Atlantic Arctic: 1992–2010. Annals of Glaciology 58, 72–91; Bunce, C. et al. (2018) Ice front change of marine-terminating outlet glaciers in northwest and southeast Greenland during the 21st century. Journal of Glaciology, 64, 523–535; Hill, E.A. et al. (2018) Dynamic changes in outlet glaciers in northern Greenland from 1948 to 2015. The Cryosphere, 12, 3243–3263. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable - Paragraph has been removed as this material is covered in Chapter 9 which discusses process in more detail. (Ch 2 assessment focusses on evidence of large scale change rather than process).
71865	62	54	63	4	Rewording of this paragraph is required as both the dominance of glacier discharge and mass balance are quoted. Not sure what is meant and this seems contradictory. [ John Church, Australia]	Not Applicable - Paragraph has been removed as this material is covered in Chapter 9 which discusses process in more detail. (Ch 2 assessment focusses on evidence of large scale change rather than process).
18417	62	55	63	6	Please take a look at a couple of papers in this context (Larsen, N. K., Levy, L. B., Strunk, A., Søndergaard, A. S., Olsen, J., & Lauridsen, T. L. (2019). Local ice caps in Funderup Land, North Greenland, survived the Holocene Thermal Maximum. Boreas, 48(3), 551–562.) Schweinsberg, A. D., Briner, J. P., Miller, G. H., Lifton, N. A., Bennike, O., & Graham, B. L. (2018). Holocene mountain glacier history in the Sukkertoppen Iskappe area, southwest Greenland. Quaternary Science Reviews, 197, 142–161. [ Olga Solomina, Russian Federation]	Not Applicable - Paragraph has been removed as this material is covered in Chapter 9 which discusses process in more detail. (Ch 2 assessment focusses on evidence of large scale change rather than process).
71869	63	2	63	6	This seems somewhat unsatisfactory. I thought there was clear coastal mass loss in parts of Antarctica - a recent paper by Smith et al., Science 10.1126/science.aaz5845 (2020) might be useful. [ John Church, Australia]	Unclear. Comment refers to Antarctic but cited text considers Greenland. It is unclear what the comment is in reference to. (loss of mass in parts of Antarctic are mentioned in the next section). (Comment is Not Applicable as Paragraph has been removed as this material is covered in Chapter 9 which discusses process in more detail. (Ch 2 assessment focusses on evidence of large scale change rather than process).)
57899	63	3	63	4	It may be important to note that there are discrepancies in studies attributing changes to SMB and ice dynamics, and future ice sheet change may be dominated by SMB processes rather than ice dynamics, but currently some studies suggest mass loss through ice dynamics and DMB are equal. This is discussed further in Chapter 9 but I would like to see a sentence placed here as well. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not Applicable - Paragraph has been removed as this material is covered in Chapter 9 which discusses process in more detail. (Ch 2 assessment focusses on evidence of large scale change rather than process).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68199	63	3	63	4	I find confusing to say that "decrease in surface mass balance" as it is increasingly negative surface mass balance that is contributing to the total mass loss, suggest to edit to clarify [ Guðfinna Aðalgeirsdóttir, Iceland]	Not Applicable - Paragraph has been removed as this material is covered in Chapter 9 which discusses process in more detail. (Ch 2 assessment focusses on evidence of large scale change rather than process).
30357	63	3			suppress 'over the last two decades' (confusing) [ Gilles Delaygue, France]	Not Applicable - Paragraph has been removed as this material is covered in Chapter 9 which discusses process in more detail. (Ch 2 assessment focusses on evidence of large scale change rather than process).
32117	63	6	63	7	"has increased fourfold from the 20th to the 21st century" Please specify the time span for this increase more exactly. [ Anja Wendt, Germany]	Noted - Substantial revisions have been made to this paragraph.
57709	63	6	63	10	I think the conclusions don't reflect well what is said in the previous text: for examples the considerations on outlet glaciers are missing. Also I think it would be more linear to have the conclusions in chronological order: to say first that during LIG the GrIS was smaller than present, and then that it peaked at LIA and then begun its mass loss to present. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Statements have been reordered in concluding statement with revision to time periods considered. Revisions have also been made to previous text and paragraph focussing on outlet glaciers has been removed as this material is covered in Ch 9 which focusses more on process.
68201	63	7	63	9	see comment above, it was not record mass loss year, but mass loss summer, This is a repeat from p62 I40-42, suggest to delete in one place [ Guðfinna Aðalgeirsdóttir, Iceland]	Accepted- reference to 2012 has been removed to focus more on longer-term change.
18401	63	7	63	9	"The total mass loss from Greenland reached a historical record value during summer 2012 (627± 89 Gt), which was likely the greatest one-season loss since at least 9 1840". - Does "historical" mean "after 1840"? Please clarify. [ Olga Solomina, Russian Federation]	Noted - Reference to 2012 has been removed to focus more on longer-term change
83231	63	13	63	13	Change "Antarctic ice sheet" to "Antarctic Ice Sheet" here and throughout the text - it is capitals as it's a proper name. [ Robert Massom, Australia]	Accepted. This is now reworded across the section.
34851	63	13	64	6	The SOD claims that the Antarctic has lost ice mass since the early 1990s. Please see rebuttal comment #9 above. [ Jim O'Brien, Ireland]	Rejected; unclear comment. Details are given in this subsection and its summary at the end. The Antarctic ice sheet and sea ice are treated in different subsections, since the nature of the ice is fundamentally different.
83233	63	13	64	7	For the Antarctic Ice Sheet mass and extent section, there also needs to be inclusion of the significant changes in recent decades relating to accelerating ice shelf melt, the contribution of iceberg calving, and the major ice-shelf disintegration events on the Antarctic Peninsula and subsequent acceleration in outlet glacier discharge following removal of the ice-shelf buttressing effect. This key information on change is currently missing here. [ Robert Massom, Australia]	Rejected. Chapter 2 is not supposed to focus on attribution and details with regional character.
57901	63	15	63	15	The range of values stated here, 0.16 to 0.37 mm yr <sup>-1</sup> should be quoted as a mean value value with error bars i.e. 0.26 +/- 0.1. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Since this connects to the cited AR5 and the lower and upper ranges are not exactly symmetric, the way this is written is not changed. It is done exactly as in AR5.
73613	63	15	63	16	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; This is now corrected.
90371	63	15			bad break in exponent [ Jeannine-Marie St-Jacques, Canada]	Accepted; This is now corrected.
83443	63	21	63	21	Acronym of WAIS (instead of West AIS) used in Chapter 1 Cross-Chapter Box 1.1, Table 1 (p. 1-21). [ Antje H. L. Voelker, Portugal]	Accepted. Connects to line 24. WAIS is now introduced.
2843	63	21	63	30	In this paragraph "Kyr" is used, whereas "ka" is used in the rest of the text. Choose one way and keep it for consistency. [ Antoine RABATEL, France]	Rejected. The two terminologies have different meanings and are used accordingly.
127063	63	21	63	30	Figure 2.22 seems to show that Pine Island and Thwaites glaciers were alive and well during the LIG. This finding should be commented on in the text. What is the confidence level? [ Trigg Talley, United States of America]	Noted; Fig. 2.22 (as in the SOD) is now removed from ch. 2, since a similar figure is included in ch. 9.
127065	63	22	63	25	This is trivially true: the AIS was absent during most of the Cretaceous, for example. Be more precise. [ Trigg Talley, United States of America]	Accepted. The sentence is now reworded, including more detail.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57903	63	25	63	25	I would summarise in the Figure caption of Figure 2.22b how the data available from these studies have been used to obtain the spatial extent of the AIS and whether a statistical analysis was conducted to combine the data sets. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; Fig. 2.22 (as in the SOD) is now removed from ch. 2, since a similar figure is included in ch. 9.
32119	63	28	63	29	Wrong reference: (Wilson et al, 2018) is about glacial lakes in Patagonia [ Anja Wendt, Germany]	Accepted; The correct reference is now included.
90373	63	29			close space between ka ) [ Jeannine-Marie St-Jacques, Canada]	Noted; the sentence is not included anymore.
57905	63	30	63	30	several is not very informative. The Weber et al. (2014) study highlights evidence for large dynamic discharge, thus I would rephrase this to: "Episodes of rapid mass loss, as evidenced by large IRD, have been documented between 8-21 ka, pointing towards raised sea levels higher than today." [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted; the sentence is not included anymore.
73845	63	30	63	30	Good sentence. I propose to refer model studies as well as geological reconstructions, which show this rapid ice sheet retreat is physically feasible. (Golledge et al 2014:Antarctic contribution to meltwater pulse 1A from reduced Southern Ocean overturning, Nature Communications. ; Fogwill et al. 2017: Antarctic ice sheet discharge driven by atmosphere-ocean feedbacks at the Last Glacial Termination, Scientific Reports) [ Takashi Obase, Japan]	Rejected; The respective sentence is not included anymore, and due to length limits and chapter 2 focus, details on processes and modelling are not included.
18419	63	30	63	30	meters [ Olga Solomina, Russian Federation]	Noted; The sentence is not anymore included in the text.
57907	63	32	63	36	This section focusses on Pine Island Glacier and with good reason. However, the growing body of knowledge that suggests Thwaites Glacier could be the weak link in the Amundsen sea sector should also be acknowledged. A reference to historic changes at Thwaites Glacier and current mass loss processes from the region would be useful, particular if there are early warning signs of the controversial Marine Ice Cliff Instability (MICI) hypothesis. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; Chapter 2 is not supposed to focus on attribution and details with regional character. The subsection has been revised, and some other regional information content was removed.
57689	63	32	63	36	I would recommend adding a reference to Kingslake et al. (2018), who provide evidence for rapid grounding-line retreat (and then re-advance) in the Ross Sea and Weddell Sea sectors during the Holocene. Reference: Kingslake, J., et al. (2018) Extensive retreat and re-advance of the West Antarctic Ice Sheet during the Holocene. Nature, 558, 430-434. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This information is now added.
57711	63	38	63	45	The GMSL should be given, as it has been given for GRIS. Both Shepherd et al (2017) and Rignot et al (2019) report it. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; SLE numbers are now added along with numbers for AIS mass changes.
32445	63	39	63	39	There is by now observational evidence, that temperature and accumulation are increasing on the East Antarctic plateau, see Medley, B., McConnell, J. R., Neumann, T. A., Reijmer, C. H., Chellman, N., Sigl, M., & Kipfstuhl, S. (2018). Temperature and snowfall in western Queen Maud Land increasing faster than climate model projections. Geophysical Research Letters, 45, 1472–1480. <a href="https://doi.org/10.1002/2017GL075992">https://doi.org/10.1002/2017GL075992</a> . This observational evidence has to be included here to balance the Frezzotti finding (which was in 2013 not aware of the emerging picture in Dronning Maud Land, as the data were still too noisy). [ Olaf Eisen, Germany]	Rejected; The suggested addition was not made due to focus of ch. 2 on large scales, and length limits. However, another publication addressing the entire AIS area by Medley and Thomas 2019 is now included in this paragraph.
11475	63	40	63	40	7 ± 0.13 Gt: should be given as 7.xx ± 0.13 or 7.y ± 0.1 (same number of digits for a number and its uncertainty) (same comment applies one line further) [ Gerhard Krinner, France]	Accepted; the format of number is adjusted to 1 digit after the comma.
5387	63	40	63	41	How can the uncertainties in the trend estimates going back to either 1800 or 1900 be so much less than those since 1979 based on satellite observations, which in line 54 are around 50%? [ Bryan Weare, United States of America]	Rejected. Uncertainties in source material connects to different methods, input data and length of time period.
57909	63	40	63	41	The reference to Marquer et al. (2017) is not required here as the numbers are taken directly from Thomas et al. (2017). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The reference to Marquer et al. 2017 is now removed.
100543	63	41	63	41	Delete "CE" [ Peter Lemke, Germany]	Accepted; CE is now removed.
73615	63	43		44	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; The paragraph is now revised and no line break in exponent included anymore.
90375	63	43			bad break in exponent [ Jeannine-Marie St-Jacques, Canada]	Accepted; The paragraph is now revised and no line break in exponent included anymore.
18421	63	47	63	48	please specify what kind of changes occurred [ Olga Solomina, Russian Federation]	Taken into account; the sentence is now revised and the respective part is not included anymore.
57911	63	47	63	49	Reference to Rignot et al. (2019) would also be useful here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; Rignot et al. 2019 is now cited twice in the discussion of contributions to recent AIS changes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57913	63	47	63	49	I think this sentence should explicitly state that Totten Glacier is an important glacier in the Wilkes Land sector given that it is one of the few glaciers in East Antarctica exhibiting highly dynamic change. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; details on regional and local scales are not supposed to be assessed in ch. 2. The respective information can be found in cited references.
57691	63	47	63	55	I would recommend adding a short paragraph on glacier terminus/length change and grounding-line change either before or after lines 47-55, if space allows. Observations of glacier terminus change in East Antarctica have shown retreat in 1974-1990 and advance in 1990-2010, with the exception of glaciers in the Wilkes Land sector, where most glaciers retreated between 2000 and 2012 (Miles et al., 2013, 2016). This has been linked to a reduction in sea ice and the incursion of warmer deep water. In West Antarctica, by contrast, there have been observations of widespread, rapid grounding-line retreat in some sectors (e.g. Christie et al., 2016). References: Miles, B.W., et al. (2013). Rapid, climate-driven changes in outlet glaciers on the Pacific coast of East Antarctica. Nature 500, 563-566; Christie, F.D.W. et al. (2016) Four-decade record of pervasive grounding line retreat along the Bellingshausen margin of West Antarctica. Geophysical Research Letters, 43, 5741-5749. Miles, B.W., et al. (2016). Pan-ice-sheet glacier terminus change in East Antarctica reveals sensitivity of Wilkes Land to sea-ice changes. Science Advances, 2, e1501350. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Providing information on terminus, length, grounding line is not done due to length limits.
39099	63	47	63	55	In discussion about mass loss, add Smith et al and Velicogna et al. Smith, Ben, et al. "Pervasive ice sheet mass loss reflects competing ocean and atmosphere processes." Science (2020)., Velicogna, Isabella, et al. "Continuity of Ice Sheet Mass Loss in Greenland and Antarctica From the GRACE and GRACE Follow-On Missions." Geophysical Research Letters 47.8 (2020): e2020GL087291. [ Ola Kalen, Sweden]	Accepted; The suggested two references are now cited/included.
11477	63	48	63	48	"where glacier changes have occurred": what are "glacier changes"? Changes in ice dynamics increased ice discharge? [ Gerhard Krinner, France]	Taken into account; the sentence is now revised and the respective part is not included anymore.
18403	63	48	63	48	".. with the exception of the Wilkes Land sector, where glacier changes have occurred" - what kind of changes? Please specify [ Olga Solomina, Russian Federation]	Taken into account; the sentence is now revised and the respective part is not included anymore.
11479	63	49	63	50	This sentence only talks about SMB, while the rest of the paragraph talks about total mass changes. This is a bit confusing. [ Gerhard Krinner, France]	Accepted; the sentence is now removed.
57915	63	50	63	52	I am surprised that neither MISI nor MICI is mentioned here given that these processes could be highly important processes in the coming century. It also does not form a large section in Chapter 9 but a reference to these potentially important processes should be made here as potentially significant mass loss processes. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Ch. 2 is not supposed to go into details about processes and attribution, therefore this is not included here.
18423	63	53	63	53	"an accelerated mass balance loss" - please delete the word "balance" [ Olga Solomina, Russian Federation]	Accepted; The paragraph was revised, and the commented wording is not included anymore.
57917	63	54	63	55	These numbers are taken directly from Table 1 of IMBIE (2018) but show a slight discrepancy. Adding the uncertainties in Table 1 gives +/- 42, which should be the value used in this section. The value of the mass balance loss should be 192, which also differs from the value quoted here. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; The subsection is now revised, mainly focusing on the entire AIS, and numbers are updated, also to be consistent with information given in chapter 9, where details on the West and East Antarctic Ice Sheets can be found.
73617	63	54		55	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; The paragraph is now revised and no unit break is included anymore.
127067	64	1	64	1	In view of the statement on page 63, line 48, shouldn't this say "in most major parts"? [ Trigg Talley, United States of America]	Noted; the summary statement is revised now, and the sentence addressing East and West Antarctica specifically is not included anymore.
2845	64	3	64	3	"decrease in" instead of "decrease of" [ Antoine RABATEL, France]	Noted; the summary statement is revised now, and the sentence addressing East and West Antarctica specifically is not included anymore.
2847	64	5	64	5	I would remove this sentence. For me, it is useless. [ Antoine RABATEL, France]	Accepted; the summary statement is revised now, and this sentence is not included anymore as it was.
127069	64	5	64	5	The temperature of the ice sheet has not been discussed in this subsection; does it belong in 2.3.1.1.1? [ Trigg Talley, United States of America]	Accepted; the summary statement is revised now, and this specific part was removed.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8907	64	5	64	6	as phrased, this is a certainty, as AIS did not exist during EECO [ Robert Kopp, United States of America]	Accepted; the summary statement is now revised. Respective information on the extent of the AIS is now worded with addressing the time periods the statement refers to specifically.
11481	64	5	64	6	"It is very likely that the ice sheet was warmer and less extensive during at least some past warm climates." This actually doesn't say much. One could even write "It is virtually certain that the ice sheet has vanished during at least some warm past climates" because it wasn't there when the dinosaurs were around. Can you add "during the last 3 My" or something like that to make this statement a little bit more precise? [ Gerhard Krinner, France]	Accepted; the summary statement is now revised. Respective information on the extent of the AIS is now worded with addressing the time periods the statement refers to specifically.
18425	64	5	64	6	"It is very likely that the ice sheet was warmer and less extensive during at least some past warm climates." - In the previous description there was no information supporting the conclusion that the ice sheet was warmer. [ Olga Solomina, Russian Federation]	Accepted; the summary statement is revised now, and this specific part was removed.
5391	64	5			Where is the evidence that the ice sheet was warmer? [ Bryan Weare, United States of America]	Accepted; the summary statement is revised now, and this specific part was removed.
32121	64	5			"ice sheet was warmer": nothing has been said about the temperature of the ice sheet in the subsection [ Anja Wendt, Germany]	Accepted; the summary statement is revised now, and this specific part was removed.
30359	64	5			'was warmer': what does it mean? Temperature of air, of ice? Confusing if not more explained. [ Gilles Delaygue, France]	Accepted; the summary statement is revised now, and this specific part was removed.
127071	64	6	64	6	This is trivially true: the AIS was absent during most of the Cretaceous, for example. Be more precise. [ Trigg Talley, United States of America]	Accepted; the summary statement is now revised. Respective information on the extent of the AIS is now worded with addressing the time periods the statement refers to specifically.
83235	64	9	64	17	What about areal loss of permafrost coverage too? [ Robert Massom, Australia]	Noted: Permafrost temperature is an ECV and accepted indicator of change. Observations of permafrost extent over time do not really exist as it is a subsurface phenomena that can't be observed like other cryospheric components with remote sensing. Estimates of permafrost extent are obtained through modelling rather than observation (see chapter 9).
78847	64	9	65	24	Section 2.3.2.5. fails to mention large scale changes in extensions of both polar (sub-polar) and mountain permafrost. However, permafrost extension is a crucial quality especially of mountain permafrost. I'm aware that information available is still sparse and records short, but a mention may be worth, as future scenarios are predicting a reduction of the permafrost extension and a shifting toward higher latitudes and altitudes. [ MONICA TOLOTTI, Italy]	Noted- Section focusses upon large scale hemispheric change and detailed regional assessment is out of scope of ch 2 but is covered in ch 9. Changes in key ECV, permafrost temperature and active layer thickness is assessed for mountain permafrost.
15921	64	10	65	24	The general discussion on permafrost should be split into a section covering terrestrial permafrost and shallow subsea permafrost (e.g. in the East Siberian Arctic Sea and the Laptev Sea). The shallow subsea permafrost is being thawed out rapidly due to the heat flowing into the Arctic in the upper layers of the ocean and it is well established that the subsea permafrost forms a barrier to further methane releases. The mechanism for methane release in deeper waters outwith the Arctic region is discussed in "Analysis of bubble plume distributions to evaluate methane hydrate decomposition on the continental slope, Johnson et al. <a href="http://dx.doi.org/10.1002/2015GC005955">http://dx.doi.org/10.1002/2015GC005955</a> ," and indicates that only a very minor temperature rise on the sea floor can lead to methane releases. [ Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - beyond the scope of the chapter. Ch2 assesses selected ECVs or indicators of change and only terrestrial permafrost is considered here. Ch 5 considers subsea permafrost and methane hydrate decomposition. Section title has been changed to "Terrestrial Permafrost"
57919	64	12	64	13	I would suggest to expand this sentence to: "Changes in permafrost temperatures is monitored at selected sites across the world and negligible change was observed here, mainly where permafrost temperatures were close to OC, with slight cooling at a limited number of sites.". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - revisions have been made to paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18127	64	19	64	25	It would be worth mentioning here the studies by Vaks et al 2020 and 2013 ( <a href="https://doi.org/10.1038/s41586-019-1880-1">https://doi.org/10.1038/s41586-019-1880-1</a> and DOI: 10.1126/science.1228729) which reconstructed the long term history of the Siberian permafrost and made links between its development and sea ice extent. [ Ersek Vasile, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Revisions have been made to include discussion of earlier time periods and suggested reference has been included..
57921	64	20	64	23	I suggest including the dates of permafrost in brackets: 3,000 BP, 1,500 BP and 250 BO, and also state that these were formed during neoglaciation periods and the LIA. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account in revisions made to paragraph.
23751	64	24	65	25	... over the past several decades' - the time period should/could be expressed more specific [ Annett Bartsch, Austria]	Taken into account in revisions made to paragraph.
57923	64	25	64	25	Holloway and Lewkowicz (2019) is not referenced in the bibliography. Reference should be: Holloway, J., Lewkowicz, A. (2019). Field and laboratory investigation of electrical resistivity-temperature relationships, southern Northwest Territories, Cold Regions Engineering. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial - All citations checked in preparation of FGD and bibliography updated. (note reviewer cites the wrong article, the article in Permafrost and Periglacial Processes is the article cited in the text).
18563	64	25	64	25	additional evidence of thawing permafrost in recent decades: Jones, BM et al., 2016 Cryosphere Jones, B.M., Baughman, C.A., Romanovsky, V.E., Parsekian, A.D., Babcock, E.L., Stephani, E., Jones, M.C., Grosse, G. and Berg, E.E., 2016. Presence of rapidly degrading permafrost plateaus in south-central Alaska. The Cryosphere, 10(6), pp.2673-2692. [ Miriam Jones, United States of America]	Accepted - Additional references included in revised text.
54901	64	27	64	29	suggest deleting "across the northern polar regions" l. 28-29. This is redundant, as indicated earlier in sentence. [ Nancy Hamzawi, Canada]	Accepted - text revised
18565	64	27	64	36	In addition to increasing permafrost temperatures, there is evidence of rapid thaw of ice-rich permafrost even in cold permafrost in the high Arctic that coincides with prolonged Arctic heat waves. See for example, Farquharson et al., 2019: Farquharson, L.M., Romanovsky, V.E., Cable, W.L., Walker, D.A., Kokelj, S.V. and Nicolsky, D., 2019. Climate change drives widespread and rapid thermokarst development in very cold permafrost in the Canadian High Arctic. Geophysical Research Letters, 46(12), pp.6681-6689. [ Miriam Jones, United States of America]	Noted - Evidence for thaw in Arctic is discussed later in section. Additional references considered in preparation of FGD and included if appropriate.
30361	64	27	64	36	Since the goal of Chapter 2 is to compare different records, and because changes at depth are due to the diffusion of surface changes, i guess some more words would help readers understand how these changes compare to surface ones. Three suggestions. 1. Modify 'acquired from several boreholes' to 'measured in several boreholes at constant depth'. 2. Clarify the time lag between changes at surface and at 20-30 m depth (shown in Figure 2.24). 3. If heat consumption by thawing partly explains (?) the regional difference in trends, this should be briefly stated (not to explain these differences, but to underline that these trends may not reflect surface trends). [ Gilles Delaygue, France]	Taken into account - Some revisions have been made to the text. The text indicates that temperatures were measured at depth where seasonal variation is negligible (i.e. depth of zero annual amplitude) which is the ECV used for assessment of long-term trends. Lags at measurement depth are generally not longer than a year. Attribution of trends is discussed further in Ch 9.
57925	64	29	64	30	The reference to Romanovsky et al. (2019) is incorrect, it should be Romanovsky et al. (2018), BAMS State of the Climate 2017. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - Romanovsky et al (2019) is correct which was the latest State of Climate report available when SOD prepared. FGD includes the most recent results and cites Romanovsky et al. (2020)
3951	64	29	64	30	combine all Romanovsky et al. references [ Sabine Baumann, Germany]	Editorial - copy edit to be completed prior to publication.
54903	64	30	64	31	Suggest: Recent (2017-2018) permafrost temperatures in the upper 20-30 m, where seasonal variation is minimal, were the highest observed at most sites. [ Nancy Hamzawi, Canada]	Taken into account in revisions made to text.
57927	64	33	64	33	I would use 0.5 +/- 0.1 degrees to denote the average trend per decade, and reference to Romanovsky et al. (2018), BAMS State of the Climate 2017. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - Range in rates based on regional averages has been provided. Note reviewer is referring to an older report rather than the 2019 report which was used.
54905	64	33	64	33	Change to: Increases in temperature [ Nancy Hamzawi, Canada]	Taken into account in revisions made to text.
23755	64	33	64	33	maybe change 'greater' to 'larger' [ Annett Bartsch, Austria]	Taken into account in revised text.
71157	64	33			what is "colder permafrost", i.e. this must be defined in the glossary. Maybe you mean cold permafrost, but even this is not properly defined in literature. The Canadian Standard Association is currently working on new guidelines that will define a clear temperature range for cold and warm permafrost for the first time. But these documents won't be available for another 1 - 2 years. [ Lukas Arenson, Canada]	Noted - "Colder" is used as a relative term. Revisions made to text to refer to warmer permafrost at temperatures higher than -2°C.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57929	64	34	64	34	I am unsure where the average value of 0.17C for sub-Arctic regions comes from. Taken from Romanovsky et al. (2018), I use Central Mackenzie Valley, Southern Foothills of Brooks Range, Interior Alaska and Southern Norway to obtain a value of 0.19C per decade. I think this is nearer the correct value. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - Reviewer refers to older report and 2019 report was used. Also in Romanovsky, ranges are often given for groups of sites whereas the average value here is based on the data for individual sites.
54907	64	34	64	34	Change "warm" to "warmer" [ Nancy Hamzawi, Canada]	Taken into account in revised text.
57931	64	34	64	36	The value range between 0.4 and 0.9C is just for the Canadian Arctic and does not include e.g. Russian Arctic, which would mean the range should be between 0.1 and 0.9. Either make this clear, or use the 0.1-0.9 range. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - The text has been revised and this statement has been removed to focus on larger scale trends rather than focus on a specific area.
71159	64	34			what is "warm permafrost", i.e. this must be defined in the glossary as this is not properly defined in literature. The Canadian Standard Association is currently working on new guidelines that will define a clear temperature range for cold and warm permafrost for the first time. But these documents won't be available for another 1 - 2 years. [ Lukas Arenson, Canada]	Noted - Warm and cold are used as relative terms. Revisions made to text to refer to warmer permafrost at temperatures higher than -2°C.
54909	64	35	64	35	Change to "permafrost temperatures have" [ Nancy Hamzawi, Canada]	Noted - Revisions have been made to paragraph.
127073	64	35	64	36	How is 'high Arctic Canada' defined? [ Trigg Talley, United States of America]	Noted - Revisions have been made to paragraph and this sentence has been removed.
54911	64	36	64	36	Wording is a bit confusing ("almost twice that..."). I assume this means that the increase since 1978 in high Arctic Canada is half the increase for the rest of the Arctic? Consider clarifying the language a bit. [ Nancy Hamzawi, Canada]	Noted -Revisions have been made to paragraph and this sentence has been removed.
23753	64	36	64	36	something seems to be missing for a complete sentence (around '...', almost twice ...'). Maybe add '... what corresponds to ...' [ Annett Bartsch, Austria]	Noted -Revisions have been made to paragraph and this sentence has been removed.
54913	64	41	64	41	Consider adding the depth range in the parenthetical remarks [ Nancy Hamzawi, Canada]	Accepted - text revised.
57933	64	54	64	54	To be absolutely precise, the value of 0.3 stated here is actually 0.26 +/- 0.12C and the reference should be Noetzli et al. (2018). Whilst it is fine to state the value of 0.3, an uncertainty of 0.1 should also be stated. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted - The value quoted by reviewer is not given in Noetzli et al (2018 BAMS SoC) for all mountain permafrost for last 2-3 decades. The reviewer may be quoting a value for all global permafrost for 2007-16. The value in the text is based on a number of references (including more recent State of Climate Report, Noetzli et al 2019) for 20 to 30 year period and we state "up to 0.3" which implies a range rather than giving average.
57935	64	56	64	56	The Liu et al. (2017a) referenced is not provided in the bibliography and so I can't comment on its accuracy. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial- reference list has been corrected.
116003	64		64		for 2.3.2.4 please see my earlier remark on past polar warming (not just past warm climates). [ Valerie Masson-Delmotte, France]	Rejected; Unclear comment. No further action taken, but in response to other comments, changes were made to this paragraph.
99671	65	1	65	1	please correct "(less than 10 years)"to "(most are less than 10 years)" [ Goncalo Vieira, Portugal]	Taken into account in revised text.
12595	65	1	67	41	An assessment on the acceleration of ocean warming will be of great interest (or in another word, change of warming rate over time). SROCC did so briefly, which places a good ground to AR6. [ Lijing Cheng, China]	Taken into account. The rates of ocean heating are now better assessed but we avoid the use of the term acceleration.
12597	65	1	67	41	It will be helpful to visualize the warming rate change over time in Fig. 2.25, for example, calculate the OHC rate (0-2000m) from 1955-1970, 1971-1985, 1986-2005, 2006-2018. SROCC did the calculation for several time windows, AR6 can do further, taking the advantages more data available. [ Lijing Cheng, China]	Taken into account in figure revisions.
12599	65	1	67	41	As far as I understand, AR6 chapter-2 will provide a final estimate for OHC time series and rate, to be used in Earth's energy imbalance estimate and sea level budget estimate. An introduction to the method and justification of doing so is needed. A super-ensemble mean among datasets will be a balanced way (i.e. in recent GCOS review paper). An alternative way is a selection of several good estimates based on literature. [ Lijing Cheng, China]	Taken into account in the revisions to this section text and creation of the table.
30363	65	1			reference to Noetzli et al 2018 is incorrect (dates are confused and doi as an extra space); reference for 2019 is missing [ Gilles Delaygue, France]	Editorial - copy edit to be completed prior to publication. Note that Noetzli (2019), more recent State of Climate report is correct.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79307	65	1			It is possible to mention that Permafrost in Antarctica warmed by $0.37 \pm 0.10$ °C (Biskaborn et al 2019). [ Carla Mora, Portugal]	Noted - Data is limited for deeper temperatures as stated in Noetzli et al and trends are less evident.
54919	65	3	65	11	The ALT paragraph could be strengthened with a short addition describing the discrepancy between active layer measurements and the amount of actual permafrost thaw when accounting for subsidence. I believe this was hinted at in the last report, and there have been recent studies highlighting that permafrost thaw is masked by ALT measurements when ground surface subsidence is not accounted for: e.g. Shiklomanov et al. 2013, <a href="https://doi.org/10.1002/2013GL058295">https://doi.org/10.1002/2013GL058295</a> ; Streletskiy et al. 2017, <a href="https://onlinelibrary.wiley.com/doi/abs/10.1002/ppp.1918">https://onlinelibrary.wiley.com/doi/abs/10.1002/ppp.1918</a> ; O'Neill et al. 2019, <a href="https://ascelibrary.org/doi/10.1061/9780784482599.074">https://ascelibrary.org/doi/10.1061/9780784482599.074</a> ). [ Nancy Hamzawi, Canada]	Noted - Process understanding is covered in ch9 in more detail. Revision made to text to mention subsidence.
71161	65	3			what are "deeper permafrost temperatures"? Do you mean deeper at depth, or colder? [ Lukas Arenson, Canada]	Noted -revisions have been made to paragraph
54915	65	5	65	6	Consider adding O'Neill et al. 2019, which includes updated data analysis of ALT, thaw penetration, and subsidence from the northern region of the thaw tube network analyzed in Duchesne et al. Long-Term Permafrost Degradation and Thermokarst Subsidence in the Mackenzie Delta Area Indicated by Thaw Tube Measurements <a href="https://ascelibrary.org/doi/10.1061/9780784482599.074">https://ascelibrary.org/doi/10.1061/9780784482599.074</a> [ Nancy Hamzawi, Canada]	Noted - Paragraph has been revised and additional references have been considered in preparation of FGD and included if appropriate.
4229	65	13	65	19	It propose to mentioned here that 1) there are regionally clear indications of acceleration of landscape changes i (Liljedahl et al. , 2016; Borge et al., 2017; Lewkowicz and Way, 2019) and 2) landscape changes, in particular abrupt thaw, cause a proportionally large part of carbon and greenhouse gas emissions from thawing permafrost (Turetsky et al., 2020, Nature Geoscience, 13, 138-143; Abbott and Jones, 2015, Global change biology 12, 4570-4587) [ Jacobus (Ko) van Huissteden, Netherlands]	Noted - Detailed discussion of process and implications of thaw are beyond scope of Ch 2 assessment, which focusses more on evidence of change in permafrost conditions. Ch 9 considers process in more detail. Additional recent references considered in preparation of FGD and included if appropriate.
57937	65	14	65	17	I think it is also pertinent to mention that ground ice melting has also led to widespread damage of infrastructure due to land subsidence and that these regions actively evolving on inter and intra-annual timescales [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Reject - Out of scope - impact is beyond scope of ch 2 assessment
90377	65	15	65	17	reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Editorial - copy edit to be completed prior to publication.
54917	65	15	65	17	It may be worth here also adding references from recent studies that have observed more gradual subsidence that isn't as apparent as e.g., slope failure, ice-wedge gully, lake expansion (e.g. Shiklomanov et al. 2013, <a href="https://doi.org/10.1002/2013GL058295">https://doi.org/10.1002/2013GL058295</a> ; O'Neill et al. 2019, <a href="https://ascelibrary.org/doi/10.1061/9780784482599.074">https://ascelibrary.org/doi/10.1061/9780784482599.074</a> ) The reference list is heavily focused on rapid or easily perceived changes, though widespread gradual subsidence is also important and has recently been reported on. [ Nancy Hamzawi, Canada]	Taken into account in revised text. Note that Ch 9 provides more detailed discussion of process.
73619	65	15	65	17	References should be in chronological order. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
23757	65	15	65	17	maybe reorder references by publication date (but not sure what IPCC rules are for that) [ Annett Bartsch, Austria]	Editorial - copy edit to be completed prior to publication.
35535	65	15	65	17	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial - copy edit to be completed prior to publication.
73981	65	17	65	18	One more time I just wonder why Boreal regions without permafrost are not considered when speaking about possible risks: in such region the hazard due to destabilisation of rock mass in mountainous areas can increase due to climate change and variability without permafros. [ Elena Kozlovskaya, Finland]	Rejected - out of scope - assessment of risk is beyond scope of ch 2 assessment. This section deals specifically with indicators of permafrost change.
71163	65	17	65	19	With regards to destabilization of rock glaciers, there is some debate / confusion because this term is used differently by different professions and research fields. As such it must be used carefully. The authors are referred to the following IPA document that illustrates the issue: IPA-RG (2020). Towards standard guidelines for inventorying rock glaciers: Baseline concepts (Version 4.0). , eds. R. Delaloye and T. Echelard Longyearbyen, Svalbard: International Permafrost Association (IPA) Action Group Rock glacier inventories and kinematics Available at: <a href="https://bigweb.unifr.ch/Science/Geosciences/Geomorphology/Pub/Website/IPA/Guidelines/V4/200117_Baseline_Concepts_Inventorying_Rock_Glaciers_V4.pdf">https://bigweb.unifr.ch/Science/Geosciences/Geomorphology/Pub/Website/IPA/Guidelines/V4/200117_Baseline_Concepts_Inventorying_Rock_Glaciers_V4.pdf</a> . [ Lukas Arenson, Canada]	Noted - The intention here is to provide evidence of permafrost change as indicated in the papers cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24135	65	19	65	19	A brief statement should be added pointing to indications that the frequency and volume of large rock ice avalanches from relatively warm permafrost in rock walls are increasing. References: (1) Coe, J.E., Bessette-Kirton, E.K., Geertsema, M. 2018. Increasing rock-avalanche size and mobility in Glacier Bay National Park and Preserve, Alaska detected from 1984 to 2016 Landsat imagery. Landslides 15, 393-407. doi:10.1007/s10346-017-0879-7 (2) Haeberli, W., Schaub, Y. Huggel, C., 2017. Increasing risks related to landslides from degrading permafrost into new lakes in de-glaciating mountain ranges. Geomorphology 293, 405-417. <a href="http://dx.doi.org/10.1016/j.geomorph.2016.02.009">http://dx.doi.org/10.1016/j.geomorph.2016.02.009</a> [ Wilfried Haeberli, Switzerland]	Noted - The intention here is to provide evidence of permafrost change rather than detailed discussion of process or impacts of landslides which is beyond scope of Ch 2 assessment.
66409	65	21	65	24	This summary statement neglects to discuss changes to active layer and thermokarst features entirely, or shallow permafrost temperature dynamics shown in figure 2.24, all of which are very relevant to understanding permafrost feedback processes, perhaps more so than the features that are discussed here. Suggest refocusing somewhat on these dynamics. [ Charles Koven, United States of America]	Noted - The summary does refer to the increase in temperatures in the upper 30 m which is shown in Fig 2.24. The summary also mentions thaw occurring since LIA so not ignoring thermokarst. Note Ch 2 assesses selected indicators of change/ECVs, not all aspects of permafrost. Temperature at depth of minimal seasonal variability is an accepted ECV for assessment of long-term change(filter out short-term variation).
73621	65	23	65	23	Insert space between 30 and m. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - revision made.
73623	65	23	65	23	Change 'programs' to 'programmes' for consistency with British English and also to avoid confusion with computing processes [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copy edit to be completed prior to publication.
85017	65	27	67	18	No comments [ Katrine Husum, Norway]	Noted.
31499	65	31	65	33	Marine related extremes are assessed in chap 9 and Xbox 9.1 [ Jean-Baptiste SALLEE, France]	Editorial. Text revised.
71873	65	34			As I understand, The Cheng et al. estimates also rely on CMIP models for a first guess, as well as the spatial pattern. What does this mean for the independence of these "observational" estimates and thus their value in independently evaluating the CMIP models? [ John Church, Australia]	Accepted. All estimates considered for assessment are illustrated in figure but assessed changes do not include Cheng et al and other hybrid estimates when direct observations are available (i.e. independent of CMIP or other models).
34853	65	36	66	24	The SOD claims that recent Ocean Heating is unprecedented over recent millennia. Please see general comment #5 above. [ Jim O'Brien, Ireland]	Unactionable. Comment #5 could not be located.
12541	65	36	67	17	The section did a good job in gathering OHC information together in figure/table, which is a basis for a thorough assessment. The general comment is: this section could be improved by better addressing the following three questions: (1) what is the state of knowledge in AR5/SROCC? (2) What has been improved since AR5/SROCC? (3) How did these improvements in ocean temperature/heat content estimate lead to better estimates of OHC (better knowledge of global ocean warming) in literature and in AR6? The SOD did properly address the first question in the first paragraph of this section, yet was less developed in discussing the recent progresses and how these progresses support better OHC estimate in AR6. I suggest to strengthen these aspects. [ Lijing Cheng, China]	Not applicable. There are standard procedures for the assessment in AR6 and scopes across chapters. Part of comments are within scope of chapters 1 and 9, not chapter 2.
12543	65	38	65	45	SROCC has increased the confidence level for 700-2000m ocean warming to "virtually certain" from AR5 (which is "likely"), accounting for the recent progress in estimating 700-2000m ocean warming. It is worthy to account for the update in SROCC. [ Lijing Cheng, China]	Rejected. Increased confidence levels for 700-2000 m in SROCC relied only on one estimate and this estimate being a hybrid estimate (initial guess and covariance from CMIP models) prior to Argo era.
35039	65	43	65	45	these quoted are of sea level rise DUE TO thermal expansion. Suggest reword "SROCC reported global mean contributions to sea level rise attributable to thermal expansion of 0.89±0.05 ....." [ W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39581	65	47	66	42	Wunsch, C., Heimbach, P., 2014, Bidecadal thermal changes in the abyssal ocean. J. Phys. Oceanogr. 44, 2013, estimate the heat content down to abyssal depths to approximately 4E22 J in 19 years, for a net heating of 0.2 W/m2. Figure 10 of Laloyaux et al (2018) doi: 10.1029/2018.MS001273, shows that the ocean heat content seems to follow a 60-70 year cycle, possibly related to Atlantic Multidecadal Oscillation. CO2 heats ocean water only marginally because the penetration depth of the energy radiated at the wavelength of vibration of 15 micrometer is only 0.01 millimeter, hence the OHC change is likely mainly natural, not anthropogenic. [ François Gervais, France]	Noted. Attribution to natural/anthropogenic factors is out of the scope of chapter 2. See chapters 3 and 9.
42891	65	47			For completeness of the palaeo evidence going forward in time, you should+134 include the evidence about OHC in the last interglacial (Shackleton, S., et al. (2020), Global ocean heat content in the Last Interglacial, Nature Geoscience, 13(1), 77-81, doi:10.1038/s41561-019-0498-0). Shows a brief period of wamer than present in early LIG, and similar to present across the LIG plateau. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Suggested literature reviewed.
57939	65	49	65	49	The value of 0.3C kyr-1 does not have a reference and I also cannot trace the origin of the value in the previous papers. Could you comment on how this was calculated or provide a suitable reference? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text revised.
98749	65	53	65	53	Noble gases can't imply anything. Maybe: Changes in noble gas imply... [ Meredith Parish, United States of America]	Editorial. Text revised.
83077	65		65		Section 2.3.3.1. I think it would be good to summarise the approximate % contributions by layer for OHC and ThSL for the recent periods, i.e. 1971-2018 and 2006-2018, (see Table 7.1) and reflect on these numbers compared to previous IPCC assessments. Table 7.1 suggests that the sub-700 m layer has a bigger share of the warming signal in the more recent period, which is a point perhaps worth bringing our here as well? [ Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised.
12619	66	1	66	50	We see year-to-year fluctuations in OHC time series, a short explanation will help: and since 2005, Argo-based observation start to resolve inter-annual variations on global OHC (Roemmich et al. 2011, Cheng et al. 2019). (1) Roemmich, D., and Gilson, J. ( 2011), The global ocean imprint of ENSO, Geophys. Res. Lett., 38, L13606, doi:10.1029/2011GL047992. (2)Cheng L., K. E. Trenberth, J. T. Fasullo, M. Mayer, M. Balmaseda, J. Zhu, 2019: Evolution of ocean heat content related to ENSO. Journal of Climate, 32, 3529–3556, https://doi.org/10.1175/JCLI-D-18-0607.1 [ Lijing Cheng, China]	Noted. Definitely, interannual variations are present in the time series after 2005/6 (Argo period) and they are likely resolved by ARGO. However, CH2 is concentrated on global trend estimates. In this context interannual variability is considered as a factor potentially masking trend estimates and preventing their accurate estimation (especially for trends based on short time series (as in case of 2006+). In the final version of FGD this is addressed with the following manner: "Cheng et al. (2020), von Schuckmann et al. (2020) and Johnson et al. (2020) have further confirmed that the central estimates of rates of OHC change in the upper 2000 m depths have increased after 1993 and particularly since 2010 (Section 3.5.1.3, Figures 2.26, 3.26), although uncertainties are large (Table 2.7)."
90379	66	1			close space between - and 2.2 [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
73625	66	2	66	2	Insert space between 100 and m. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
57941	66	2	66	3	The temperature change trend of -1.0 +/- 7C pertains to the northeastern section of the Atlantic, so generalising to the entire Atlantic may not be appropriate. I suggest saying 'in parts of the Atlantic'. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Text revised.
90381	66	2			space between 1000 and m; this is a general issue on this page [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
4631	66	5	66	8	This statement not reflected in Table 2.7. At which depth do we observe heat loss? [ Andries Kruger, South Africa]	Editorial. Text revised.
71883	66	6	66	8	But recent evidence is that the abyssal ocean is warming (Purkey et al.) [ John Church, Australia]	Accepted. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57943	66	7	66	7	I suggest change first part of second clause to: ", implying that since 1750 CE there has been deep ocean heat loss that partially offsets...". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Text revised.
12613	66	10	66	12	The justification for the choice of Zanna et al. 2019 is only one sentence, and with a very subjective wording "novel". I think a comprehensive assessments of recent progress is needed including both the advantages and disadvantages. I also oppose to used it as observations to evaluate models later. The reasons : (1) Zanna et al. is not based on observations of ocean subsurface temperature which are OHC observations, instead, it is based on SST. (2).It is a reconstruction based on a single physical process: it assumes subsurface changes are due to vertical transportation from surface, and this vertical transportation does not change over time. This assumption is apparently over-simplified, which neglects many other important processes (changes in ocean circulation etc.). This assumption works well over very long time scale, so it was used by Zanna et al. to reconstruct OHC since late-1800s. So the long-term change can be used for some sort, but not for shorter periods less than ~50 years. (3). The vertical heat transport information used in Zanna et al reconstruction comes from an ocean reanalysis product, ECCO, which spans from 1990s to present. The reanalyses did poor job in representing OHC changes, see Palmer et al. 2018 Climate Dynamics paper. ECCO is one of the worst products representing OHC changes compared with real observations, which indicates that this reanalysis data have huge bias in the ocean subsurface. (4). The method of Zanna et al is based on an additional assumption for the driver of OHC: OHC is driven by SST. However this is again an oversimplified assumption. We know local SST change is driven by air-sea heat flux and ocean processes including vertical/horizontal advection and mixing in the mixed layer. However, local OHC change is a balance of heat flux and heat convergence/divergence within in an ocean volume. Their controlling processes has fundamental difference, so SST drives OHC framework is a over-simplified one. (5) Given all these issues above, it is more proper to label Zanna et al. as a reconstruction and used it as an additional source of information, rather than as "observation". And it should not be used to benchmark models, especially models have more or less complete physical processes, so it is not an apple-to-apple comparison (in chapter-9). (6). An assessment of Zanna et al. time series is emergently needed by AR6 author group across chapters: LAs should seriously consider: is this estimate represents the state of knowledge for ocean warming rate? Is this reconstruction superior than observational estimates given some recent new developments (Ishii et al. 2017, Cheng et al. 2017, update of Levitus et al. 2012 and other Argo-based products)? A simple	Not applicable. All estimates considered for assessment are illustrated in the figure but assessed changes are only based on hybrid estimates (Zanna et al.) when direct observations are not available for periods and depth layers in question. This approach is the same for paleo estimates.
30365	66	10	66	12	Two comments here. 1. A reconstruction very similar in terms of technic, as been proposed by Gebbie & Huybers (2019), cited just above L5-8: Why is this study not cited here? 2. If i correctly understand the results of Gebbie & Huybers, the results of Zanna et al used in Fig 2.25 + Table 2.7 should be biased high, because their simulation was initialised in 1870. This initialisation imposes cooler temperature in the deep Pacific, which prevents it from cooling over the 19 & 20th centuries. I am not an expert in this field, I'm just wondering why the Gebbie & Huybers study is cited elsewhere but not in this paragraph concerning reconstruction of OHC. [ Gilles Delaygue, France]	Accepted. The whole text related to the reconstruction is re-written, Gebbie & Huybers (2019) paper (along with Scheen and Stocker (2020)) is cited in the following context: "A combined study of model and observational data further confirmed these results, treating temperature as a passive tracer (Gebbie & Huybers, 2019) and addressing the role of circulation dynamics (Scheen and Stocker, 2020).
7147	66	10	66	12	the starting date is 1871 [ Nicolas Kolodziejczyk, France]	Editorial. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57713	66	10	66	24	I would move this paragraph up in the text (right after line 45) because it makes more sense to talk about what has been corrected from AR5 immediately after the recap of AR5 findings. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The structure of subsections in CH2 follows two major priorities - (i) going from deep past to the present and (ii) indicating progress in data, methods, etc. compared to AR5. It was a common agreement that it is (i) an external loop and (ii) an internal one. This justifies placing paleo estimates para first and progress in estimates based on data for the last decade after AR5 afterwards. Given the relatively short paragraph on paleo, we do believe that the section flow is better understood. Moreover, methodological part logically precedes estimates provided for the last decades in the next para.
12603	66	10	66	50	Limitations of Argo-based observations since 2005 could also be discussed: the month-to-month or even year-to-year variability still has quite some uncertainty, this is responsible for the difference in various products after 2005 (Trenberth et al. 2016). Reference:Trenberth K. E.*, K., J. T. Fasullo, K. von Schuckmann, L. Cheng, 2016: Insights into Earth's Energy Imbalance from Multiple Sources. Journal of Climate, 29(20), 7495-7505, <a href="https://doi.org/10.1175/JCLI-D-16-0339.1">https://doi.org/10.1175/JCLI-D-16-0339.1</a> ; [ Lijing Cheng, China]	Accepted. See also comment #12619. Given the size limitations, uncertainty issue in the final version of FGD is addressed, as it is given in our response to comment # 12619.
127075	66	10	67	5	By far the most comprehensive and validated OHC product is that of Cheng et al (2017) which is in the references and Figure 2.25, but not in the text. Moreover there are important updates as follows: Cheng, L., J. Abraham, Z. Hausfather, K. E. Trenberth, 2019: How fast are the oceans warming? Observational records of ocean heat content show that ocean warming is accelerating, Science, 363, 128-129. doi: 10.1126/science.aav7619; Cheng, L., J. Zhu, J. Abraham, K. E. Trenberth, J. T. Fasullo, B. Zhang, F. Yu, L. Wan, X. Chen, and X. Song, 2019: 2018 continues record global ocean warming. Adv. Atmos. Sci., 36(3), 249-252, <a href="https://doi.org/10.1007/s00376-019-8276-x">https://doi.org/10.1007/s00376-019-8276-x</a> ; and Cheng, L., J. P. Abraham, J. Zhu, K. E. Trenberth, J. Fasullo, T. Boyer, R. Locarnini, B. Zhang, F. Yu, L. Wan, X. Chen, X. Song, Y. Liu, and M. E. Mann. 2020: Record-setting ocean warmth continued in 2019. Adv. Atmos. Sci., 37, 137-142, <a href="https://doi.org/10.1007/s00376-020-9283-7">https://doi.org/10.1007/s00376-020-9283-7</a> . [ Trigg Talley, United States of America]	Not applicable. All estimates considered for assessment are illustrated in the figure but assessed changes do not include Cheng et al and other hybrid estimates when direct observations (i.e. independent of CMIP or other models) are available.
57945	66	12	66	12	I think Figure 2.25 should also be referenced here, [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Text revised.
108113	66	13	66	13	Instead of the term "bias-corrections" I suggest to use the term "bias adjustments", which is explained in Chapter 10 Section 10.3.1.4.2 and used in Chapter 2, 8, 10 and 12. Probably in the case of instrumental measurements, the term "bias correction" is more appropriate. [ Claas Teichmann, Germany]	Editorial. Text revised.
35537	66	13	66	16	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial. Text revised.
12545	66	14	66	16	This is a bit over-simplified and a more detailed discussion will help to guide the audience to better understand the OHC assessment results in AR6 (i.e. to clarify what progresses have been made and then how they support an improved ocean heat content estimates). [ Lijing Cheng, China]	Accepted. In the final version of FGD we explicitly list with references all aspects of improved accuracy of OHC changes, then Table 2.7 provides estimates themselves with Table caption providing technical information on data sources and methods. Given the limitations of space, this coverage is considered to be sufficient.
90383	66	14	66	16	reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12617	66	14	66	16	It is more than "better understanding of sources of uncertainty", but also accounting for these knowledge and improve the estimates (so the best knowledge has been shifted). For example, better understanding of instrumental error in XBT data leads to new community-agreed correction method significantly improve the OHC estimate (Cheng et al. 2014; 2016); better understanding of the bias in traditional gap-filling methods (Durack et al. 2014) leads to new or updated gap-filling techniques (Cheng et al. 2017, 2019; Ishii et al. 2017). These significantly improve the OHC estimate, leading to the convergence of OHC estimates. These aspects should be developed here. References: (1) Cheng L., John Abraham, Gustavo Goni, Timothy Boyer, Susan Wijffels, Rebecca Cowley, Viktor Gouretski, Franco Reseghetti, Shoichi Kizu, Shenfu Dong, Francis Bringas, Marlos Goes, Loïc Houpert, Janet Sprintall, Jiang Zhu, 2016: XBT Science: Assessment of Instrumental Biases and Errors, Bulletin of the American Meteorological Society, 97(6), 924-933, <a href="https://doi.org/10.1175/BAMS-D-15-00031.1">https://doi.org/10.1175/BAMS-D-15-00031.1</a> . (2) Cheng L., J. Abraham, Z. Hausfather, K. E. Trenberth, 2019: How fast are the oceans warming? Science, 363, 128-129. <a href="https://doi.org/10.1126/science.aav7619">https://doi.org/10.1126/science.aav7619</a> . (3) Durack, P.J., P.J. Gleckler, F.W. Landerer, and K.E. Taylor. 2014. Quantifying underestimates of long-term upper-ocean warming. Nature Climate Change 4(11):999–1,005, <a href="https://doi.org/10.1038/nclimate2389">https://doi.org/10.1038/nclimate2389</a> . [ Lijing Cheng, China]	Accepted. See also comments ##12603, 12545. In the final version of FGD we use more accurate and explicit wording addressing different aspects of improved uncertainty estimates. Some of suggested references along with additional references are included into the final FGD version.
73627	66	14	66	16	References should be in chronological order [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
71871	66	15			"partially offsets" [ John Church, Australia]	Accepted. Text revised.
12547	66	16	66	16	It is not clear what "these new and updated in situ based analyses" stand for? It seems to me that none of the references in previous sentence give new or updated analyses. [ Lijing Cheng, China]	Accepted. Text revised.
73629	66	16	66	16	Remove hyphen from in-situ. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. One style used across chapter for consistency.
12549	66	17	66	18	This is first analyzed and mentioned in Cheng et al. 2019 Science paper and then assessed/confirmed in SROCC. [ Lijing Cheng, China]	Noted. Cheng et al. 2019 is only one of the analyses and a hybrid estimate that relies on CMIP models for initial guess and covariance.
12551	66	17	66	18	In Fig.2.25 and the table, there are combined estimates for 0-2000m OHC change by summing up a 0-700m OHC time series (Domingues) and a 700-2000m time series (Levitus or Ishii), that is Domingues+Levitus and Domingues+Ishii labelled in Fig.2.25 and table. The use of these combined estimates should be justified, because there are many coherent analyses providing OHC time series from surface continuously to 2000m (Ishii, Cheng, Levitus), why not using these estimates rather than sum up two different estimates? I recommend not putting more weights over these combined estimates in AR6, because they are physically inconsistent and do not represent the best knowledge: (1) the two separate time series Domingues and Levitus/Ishii are from two groups, based on a set of completely different techniques including bias-correction, gap-filling, data processing methods etc. (2). The time scales are different due to different representation of climate variability in their gap-filling methods (i.e. Levitus/Ishii time series has much stronger year-to-year variation than Domingues time series, which is much smoother), so the representation of the underlying physical processes are different and not comparable. (3). It is difficult to give a reasonable uncertainty estimate, because each time series has their own methods for error bar estimates and they are different and physically inconsistent. [ Lijing Cheng, China]	Rejected. Combination is consistent with AR5 approach. Only trend estimates are used below 700 m (not timeseries). Assessed changes do not include Cheng et al and other hybrid estimates when direct observations (i.e. truly independent of CMIP or other models) are available to avoid circularity in detection and attribution (chapter 3) and is also consistent with the approach used for paleo estimates across the full report.
12553	66	17	66	18	Why we have "higher rate" needs a brief discussion, to help audience understand the underlying reason (some discussions were provided in Cheng et al. 2019 Science and also in Meyssignac et al. 2019 Front.Mar.Sci.) [ Lijing Cheng, China]	Accepted. In the final version of the FGD this part of the text was considerably revised. In the paragraph preceding the summary, we very briefly mention the tendencies and their range emphasizing "an improved consistency among available estimates of OHC rates in the upper 2000 m since 2006". Further information is now provided in Table 2.7 and Fig 2.25.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12609	66	17	66	18	A potential good way to use Zanna et al. together with other estimates for a final OHC time series is: take an ensemble approach: (1) pre-1960, use Zanna et al. and its uncertainty range. (1) 1960-2005, use ensemble of Cheng et al. 2017, Ishii et al. 2017, Levitus et al. 2012, take their ensemble mean for final estimate and standard deviation as uncertainty range, this is done for 0-2000m. If LAS want to add Domingues+Levitus as another ensemble member, it is fine as it is used in AR5 and Cheng et al. 2019 used this and result in similar OHC changes compared with other 0-2000m estimates. (3). Use all available data products after 2005 except Zanna et al., calculate ensemble mean and standard deviation as central estimate and error bar.. This approach provides a balanced IPCC-stype assessment, and fully takes account of the progress made since AR5. [ Lijing Cheng, China]	Rejected. An ensemble mean would average out random errors but not systematic errors, although useful for uncertainty estimates. It does not make sense to average together, for example, estimates that statistically account for data gaps with estimates that tend to revert to zero anomaly. Please see chapter 3, AR5 assessment. Assessed changes do not include hybrid estimates when direct observations (i.e. independent of CMIP or other models) are available to avoid circularity in detection and attribution (chapter 3), in agreement with the approach used for paleo estimates across the full report.
12611	66	17	66	18	What about the GCOS assessment on earth energy budget? von Schuckmann, K., Cheng, L., Palmer, M. D., Tassone, C., Aich, V., Adusumilli, S., Beltrami, H., Boyer, T., Cuesta-Valero, F. J., Desbruyères, D., Domingues, C., García-García, A., Gentile, P., Gilson, J., Gorfer, M., Haimberger, L., Ishii, M., Johnson, G. C., Killik, R., King, B. A., Kirchengast, G., Kolodziejczyk, N., Lyman, J., Marzeion, B., Mayer, M., Monier, M., Monselesan, D. P., Purkey, S., Roemmich, D., Schweiger, A., Seneviratne, S. I., Shepherd, A., Slater, D. A., Steiner, A. K., Straneo, F., Timmermans, M.-L., and Wijffels, S. E.: Heat stored in the Earth system: Where does the energy go? The GCOS Earth heat inventory team, Earth Syst. Sci. Data Discuss., <a href="https://doi.org/10.5194/essd-2019-255">https://doi.org/10.5194/essd-2019-255</a> , in review, 2020 [ Lijing Cheng, China]	Accepted. Suggested literature considered.
57947	66	17	66	18	It is difficult to interpret Figure 2.25 because of the changes in panel size and y axis. If it is possible to make all figure panels the same size, this would greatly enhance the interpretation of this figure. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Figure was revised.
105679	66	18	66	18	"...improved consistency among rates in the upper 2000 m since 2006.." which is due to the more comprehensive measurement coverage of the Argo Program - it makes sense to spell this out to a reader [ Paul Durack, United States of America]	Editorial. Text revised.
73631	66	19	66	19	Space required between numbers and units (2000 m - 6000 m). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
43095	66	19			Read "the deep ocean (2000 m-6000 m) from repeated " rather than "the deep ocean (2000m-6000m) from repeated " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
73633	66	21	66	21	delete ( before Purkey. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
43097	66	21			Read " (updated after Purkey and Johnson (2010))" rather than " (updated after (Purkey and Johnson (2010))" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
83069	66	23	66	24	It would be good to be a bit more quantitative here about what is meant by "depths of decreased in situ data availability". I think we are talking about depths below the upper few hundred metres - or you could even specify below 700 m or some other choice taken directly from Palmer et al (2017). You might also state explicitly that before the mid-2000s (and Argo) there is very limited sampling below 700 m to constrain ocean reanalyses. Note that Palmer (2017) provides a recent/concise review of these issues in the context of estimating Earth's energy imbalance. <a href="https://link.springer.com/article/10.1007/s40641-016-0053-7">https://link.springer.com/article/10.1007/s40641-016-0053-7</a> [ Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The chapter must cover a broad range of topics within a stipulated page limit. It is therefore necessary to consider issues in a condensed form and a balanced manner that precludes significant expansion of this section.
19743	66	27	66	42	While the left part of figure 2.25 is easy to understand, the right part is not. Should not the OHC over 0-2000m depth be the sum of 0-700m and 700-2000m contributions? It is not. Maybe there is a problem with the scale for the 0-2000m subplot. [ philippe waldteufel, France]	Editorial. Figure revised.
12555	66	29	66	31	Zanna et al. relied on both SST and ECCO ocean reanalysis based on MITGCM, also based on some simplified assumptions on physical processes. [ Lijing Cheng, China]	Accepted. Information on this is now mentioned in the caption for Table 2.7 and is also expanded in the FAIR data table.
57949	66	29	66	40	I think Figure 2.25 should be adapted. It takes a while to work out why the y axis on the right hand panels have different values to the left hand panels. If the dotted lines are trends, this should also be made clear. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Figure revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30367	66	29			Legend has to be completed: 1. data shown are 'changes in OHC', not OHC itself; 2. the Zanna et al changes have been calculated wrt year 1870; 3. all other data have been adjusted to the Zanna et al ones at the end of the period (2018?), which is a purely ad hoc adjustment. [ Gilles Delaygue, France]	Accepted. Text revised.
73635	66	30	66	30	These categories don't make sense. A value of 700 could appear in either category. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial, accepted.
113643	66	32	66	32	I can't see "Meysignac et al., 2019" dataset in the Figure. Is it combined with another dataset? [ Agnieszka Kowalczyk, Poland]	Editorial. Text revised.
73637	66	32	66	40	The formatting of the references needs tidying up, and some of the blocks need to be put in chronological order. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
29909	66	32	66	40	Add "Ishii" before "(Ishii et al., 2017;". There several similar omissions and unbalanced parentheses in the rest of the lines of this legend, please check. [ Hernan Edgardo Sala, Argentina]	Editorial. Text revised.
90385	66	33	66	34	reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
13241	66	35	66	35	Missing or extra () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
13243	66	36	66	36	Missing or extra () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
90387	66	36	66	38	merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
43099	66	36		38	Read " to infill coverage gaps (Purkey and Johnson, 2010; von Schuckmann and Le Traon, 2011; Desbruyères et al., 2017; Kolodziejczyk and Prigent-Mazella, 2017; Purkey and Johnson, 2010; von Schuckmann and Le Traon, 2011);" rather than " to infill coverage gaps); (Purkey and Johnson, 2010; von Schuckmann and Le Traon, 2011) (Desbruyères et al., 2017; Kolodziejczyk and Prigent-Mazella, 2017; Purkey and Johnson, 2010; von Schuckmann and Le Traon, 2011);" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
7149	66	37	66	37	Please add the proper peer reviewed publication describing the ISAS15 global OHC change which is : Kolodziejczyk, N., Llovel, W., & Portela, E. (2019). Interannual variability of upper ocean water masses as inferred from Argo Array. Journal of Geophysical Research: Oceans, 124. doi :10.1029/2018JC014866. (Their Figure 1). Note that Kolodziejczyk, Prigent-Mazella, and Gaillard (2017) refers to the ISAS15 data set doi. [ Nicolas Kolodziejczyk, France]	Editorial. Text revised.
35539	66	37	66	38	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial. Text revised.
57951	66	47	66	49	The caption for this table should state the uncertainties are quoted in square brackets. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Text revised.
12557	66	47	67	4	There seems to be 14 estimates rather than 13? [ Lijing Cheng, China]	Editorial. Text revised.
30369	66	47			Table 2.7 >2000 m, last two rows: if there is a reason for the trends being equal over both periods then a note should be added with this reason; i think 'Temperature data' instead of 'Subsurface temperature data' is clearer. [ Gilles Delaygue, France]	Editorial. This suggestion has been taken into account.
73639	66	49	66	49	Change reference to Santer et al. (2008). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
26633	66	50	66	50	Table 2.7 / 2nd row :why using only Domingues et al. 2008 for the depth range 0-700 and the periods 1971-2018 and 1993-2018. Many other products are available in the literature that cover the same period with a comparable quality [ Eric Brun, France]	Rejected. References for many other products were not provided. Combination is consistent with AR5 approach. To the best of our knowledge, there are no other direct 0-700 m estimates that consider statistical infilling in data sparse regions. Cheng et al. rely on CMIP models initial guess and covariance.
73641	66	50	67	1	The 700 m categories in the table don't make sense (see earlier comment). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised.
83071	66		66		I think the work of Roemmich et al to estimate OHC change since the HMS Challenger expedition should be touched upon somewhere in the discussion (these results are included in the Gebbie and Huybers, 2019, study) as a strand of evidence for ocean warming for the period 1870 to 1971. <a href="https://www.nature.com/articles/nclimate1461">https://www.nature.com/articles/nclimate1461</a> [ Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. However, Roemmich's paper is not new advances since SROCC.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83073	66		66		Table 2.7. I just wanted to flag that we should check the consistency between this table and the results presented in Chapter 7 and cross-chapter box 9.2 on the energy and sea-level budgets. Those budgets are premised on a difference in OHC and sea level for two specific years: 1971 and 2018. This simplifies the mathematics considerably, since we don't really need to worry about autocorrelation in the timeseries, and may result in different estimates of the rate of change. [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Revised with cross-chapter coordination.
83075	66		66		Table 2.7. There may be an argument for making this table even more comprehensive, in terms of the number of estimates included. If that is the case, it might make sense to focus on the graphical representation (Figure 2.25) and move Table 2.7, along with details on the different analyses/estimates, to the Observations Annex? [Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figures and tables revised.
90391	66				Table 2.7 delete (in front of Domingues - 3rd line [Jeannine-Marie St-Jacques, Canada])	Editorial. Text revised.
29865	67	1	67	1	Please, check the use of the parentheses throughout the "Source" column of the Table 2.7. [Hernan Edgardo Sala, Argentina]	Editorial. Text revised.
90395	67	1			space needed in vonSchuckmann; should be von Schuckmann; LeTraon should be Le Traon [Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
12559	67	9	67	9	700-2000m warming is "very likely" here, which is a tune-down from SROCC assessment (virtually certain), but no explanation. This should be discussed [Lijing Cheng, China]	Noted. Increased confidence levels for 700-2000 m in SROCC relied only on one estimate and this estimate being a hybrid estimate (initial guess and covariance from CMIP models) prior to Argo era.
12607	67	9	67	9	The assessment of ocean heat content starts from 1971, I recommend CLAs/LAs reconsider this time selection. Reason: (1) There are many data (Levitus, Ishii, Cheng) and studies showing that we have reliable OHC time series back to 1955-1960. (2) Temperature is much better observed than many other ocean parameters, i.e. salinity, oxygen, pH. However, salinity assessment starts from 1950, why should temperature start from 1971?? (3) The consistency among Levitus/Ishii/Cheng data since 1958 support the reliable estimate of OHC since late-1950s. On this basis, I recommend a time period from 1960-2018 used here and in Table 2.7 and in all AR6 OHC assessments. [Lijing Cheng, China]	Rejected. Reference periods are in agreement with AR5 and aligned across relevant AR6 chapters.
73643	67	9	67	9	Insert space between 700 and m. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
12561	67	9	67	17	It is not clear to me how these assessments were made. [Lijing Cheng, China]	Accepted. Text revised for clarity.
12563	67	9	67	17	Is there an universal definition on "intermediate ocean" in AR6? [Lijing Cheng, China]	Noted. No. Text revised to be explicit.
69167	67	9	67	17	Inserting "In summary" at the top of the paragraph, like the other sections in 2.3.3. is suggested. [Kaoru Magosaki, Japan]	Editorial. Noted.
127077	67	9	67	17	Check the uncertainty and likelihood statements here. Surely the confidence is higher down to 2000 m and borne out by sea level rise and closure also. [Trigg Talley, United States of America]	Rejected. There is no major advance since AR5/SROCC in terms of confidence levels for 700-2000 m (e.g., larger number of data recovered, data quality or gridding method) prior to the Argo era. SROCC relied only on one hybrid estimate (initial guess and covariance from CMIP models).
73645	67	10	67	10	Insert space between 2000 and m. [Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
35041	67	11	67	11	Suggest replace "Intermediate ocean warming" with "Intermediate level global warming." [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
35043	67	14	67	15	It would be helpful to include a brief reference here to the likely causes of deep ocean heat loss. [W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised for clarity.
21529	67	17	67	22	References to sections in chapter 2 need to be updated following section ordering changes made therein. [Peter Thorne, Ireland]	Editorial. Text revised.
34855	67	20	68	36	It seems to be only speculation in the SOD that Ocean pH is at its lowest in 2m years. Please see rebuttal comment #5 above. [Jim O'Brien, Ireland]	Rejected. The other part of the comment the reviewer is referring to cannot be found. However, the figure and assessment text has been revised, which also replies to this comment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12565	67	20	68	50	This section should highlight the progress since AR5, please state more explicitly what is the new knowledge in understanding the salinity changes related to climate change: i.e. do we have more in situ data? do we have better data products? do we know more about "salinity gets saltier, fresh gets fresher" change? What happens for salinity change during Argo-period, when we have more data? [ Lijing Cheng, China]	Noted. The chapter must cover a broad range of topics within a stipulated page limit. It is therefore necessary to consider issues in a condensed form and a balanced manner that precludes significant expansion of this section. Part of the comments belongs to chapter 1's scope.
12587	67	22	68	49	"acceleration of hydrological cycle" is often used here, but please consider to use "intensification of hydrological cycle". [ Lijing Cheng, China]	Editorial. Text revised.
71621	67	23	67	25	This part only includes upper ocean topics. For deep ocean, Kobayashi (2018) clarified abyssal freshening. It should be mentioned. :Taiyo Kobayashi. Rapid volume reduction in Antarctic Bottom Water off the Adelie/George V Land coast observed by deep floats. DEEP-SEA RESEARCH PART I-OCEANOGRAPHIC RESEARCH PAPERS, 2018, 140, p. 95-117, doi: 10.1016/j.dsr.2018.07.014 [ Shuhei Masuda, Japan]	Noted. Chapter 2's scope is large scale changes not regional/local.
30371	67	23			'low salinity mostly in the tropics': i think it necessary to be more specific here; maybe equatorial regions? [ Gilles Delaygue, France]	Accepted. Text revised for clarity.
12567	67	25	67	25	Can AR6 provide a better estimate for this number? [ Lijing Cheng, China]	Noted. Figure/values were a placeholder. Text and figure revised with updated values.
12569	67	25	67	25	Is this from AR5? Is this surface change? [ Lijing Cheng, China]	Noted. Correct. No changes requested or made.
37869	67	25	67	25	Increased by 0.13 (0.08 to 0.17)' : I wonder where 0.13 comes from. Maybe the change in the latitudinal difference between two years (0.17 - 0.08 = 0.09) would be the right number? [ Junhee Lee, Republic of Korea]	Noted. 0.13 is the central estimate. This is standard IPCC practice.
30373	67	25			'PSS-78' > 'in Practical Salinity Scale 1978 (PSS78)' (add reference to Lewis & Fofonoff 1979 or Fofonoff 1986?) [ Gilles Delaygue, France]	Accepted. Text revised.
57953	67	29	68	1	I agree that the salinity contrast has become larger in recent decades, But I think it is more complex than this and actually many high latitude regions, particularly in the southern hemisphere, show a more patchy response. So, I would state that the subtropical Pacific shows the most pronounced salinity increase, whilst the Arctic Ocean is freshening. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Text revised to highlight that this chapter is only concerned with large scale changes.
7155	67	30	67	30	I m surprised that 'submitted' paper can be cited ? [ Nicolas Kolodziejczyk, France]	Accepted. In the final FGD version the reference to the accepted paper is used.
90397	67	30	67	31	reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
73647	67	30	67	31	References should be in chronological order, with the 'submitted' one last. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
35541	67	30	67	31	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial. Text revised.
26635	67		67		Table 2.7 refers very old products whose updates have not been published. Some of these products have deficiencies that have been published in the recent literature and there is no proof that these deficiencies have been corrected for in the updates of these old products. The IPCC reports are assessments rather than reviews and as such they are supposed to make scientifically based judgements and selections on the products and research results that are presented. I feel here the authors only gathered all products available in the literature. The authors could consider making a critical judgement on the products and end up with a selection of state of the art products [ Eric Brun, France]	Rejected. While the figure lists all estimates considered, the table makes a critical judgement on the estimates and end up with a selection of state of the art products to be used in the assessment, as expected by the reviewer and in agreement with the IPCC mandate.
26637	67		67		Table 2.7 / raw 4 : why using only Levitus et al. 2012 for the depth range 700-2000 and the periods 1971-2018 and 1993-2018. Other products are available in the literature that cover the same period with a comparable quality [ Eric Brun, France]	Rejected. All direct observational estimates were assessed. Other products that the reviewer might be referring might be models with data assimilation. References not provided by the reviewer.
13245	67		67		Tsble must be completed as it is missing cells [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
90393	67				Table 2.7 (updated from (Purkey and Johnson, 2010) should be (updated from Purkey and Johnson, 2010) 5 instances; last line is mangled [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
12571	68	1	68	3	It is stated here "four datasets", but Fig.2.26, only one data shown. Update? And what is the criteria for the data selection? [ Lijing Cheng, China]	Noted. Figure caption revised.
30375	68	2			'Durack and Wijffels, 2010a and 2010b' have the same reference [ Gilles Delaygue, France]	Editorial. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105681	68	5	68	6	"..strengthening the inter-basin contrast (Durack et al 2018)" This is an incorrect citation, rather Durack 2015 doi: 10.5670/oceanog.2015.03 may be a more suitable reference which includes the near-surface and global zonal mean subsurface trend plots across 3-4 observational products. This citation confusion is also a problem for P68 L10-11 [ Paul Durack, United States of America]	Editorial. Text revised.
105683	68	6	68	6	"An updated observational record (1896 to 2013) further.." A key point to note about the Friedman et al 2017 analysis is this considers a multi-centennial period, over which the great salinity anomaly and other events have occurred. So therefore shows a robust, coherent and persistent change in salinity properties over their 117 years of analysis [ Paul Durack, United States of America]	Accepted. In the revised version of the FGD, the discussion of Friedman results is provided as follows; "There are indications that the subpolar freshening and subtropical salinification of the Atlantic ocean may extend back to at least 1896 (Friedman et al., 2017)."
12573	68	6	68	7	"updated record 1896-2013", why not 2018? [ Lijing Cheng, China]	Taken into account. Timeseries stops in 2013 and not 2018.
30379	68	6			'changes with subpolar' [ Gilles Delaygue, France]	Editorial. Text revised.
30381	68	7			'low-latitude' > 'subtropical' [ Gilles Delaygue, France]	Editorial. Text revised.
12575	68	9	68	9	Observation shows that subsurface ocean changes are not mirror those at surface (Durack et al. 2010; Cheng et al. 2020 submitted) [ Lijing Cheng, China]	Noted. Revised text now clarifies that subsurface changes are along ventilation pathways (from surface).
35045	68	9	68	15	This paragraph talks about the subsurface ocean behaviour mirroring that at the surface. This statement needs qualifying since it only applies to the upper ocean. [ W John Gould, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text revised for clarity.
12581	68	9	68	30	The two paragraphs can be better framed: starting from why subsurface or integrated salinity are important (it is an integral of surface anomalies, which can store climate-related signals compared with surface changes; signal to noise is higher for integrated metrics than surface changes), then discussing the uncertainty in data and progress since AR5 (do we have better data/techniques to deal with the issues compared with AR5), and then introduce the new knowledges we have now in AR6 based on recent literature. [ Lijing Cheng, China]	Accepted. In the final FGD version this part of the text was revised. The assessment flow goes along Figure 2.26 pointing to major uncertainty sources and data collection methods. Extensive discussion of these issues is however avoided given space limitations.
35545	68	10	68	10	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Noted; published sources are used.
90399	68	10	68	11	mangled citation list and formatting [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
73649	68	10	68	11	References should be in chronological order, with the 'submitted' one last and Durack changed to Durack et al. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
105685	68	10	68	11	The Durack et al 2018 and Rupp et al 2013 references are misplaced. Durack 2015 may have been the citation that was intended, no suggestion for Rupp et al 2013 (which is focused on N. Hemi snow cover). I would query the Rupp et al 2013 reference on P68 L18 as well [ Paul Durack, United States of America]	Editorial. Text revised.
35543	68	10	68	11	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial. Text revised.
71875	68	10			Suggest add Helm et al GRL [ John Church, Australia]	Rejected. No new publication/results since SROCC.
57955	68	13	68	15	Freshwater runoff from the Greenland Ice Sheet represents a major source of change in the North Atlantic and could be mentioned here due to its potentially significant impact on ocean circulation. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Scope is large scale and not regional salinity changes. For the circulation assessment, this was not considered as chapter 2 is focused on the observed change, and not the drivers.
12577	68	14	68	14	It is not simply implied by observing system limitations, it is also strongly dependent on the reliability of gap-filling method and other techniques. [ Lijing Cheng, China]	Editorial. Limitations in observing systems imply how those gaps are infilled. Text was revised for clarity.
13247	68	15	68	15	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
35547	68	15	68	15	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Noted, published sources are used.
90401	68	15	68	18	merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
43101	68	15			Read " (Bindoff et al., 2019; Cheng et al., submitted). " rather than " (Bindoff et al., 2019)(Cheng et al., submitted). " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
37871	68	17	68	19	It is unclear whether the link between salinity change and hydrological cycle is strengthened, or salinity change is strengthened. [ Junhee Lee, Republic of Korea]	Taken into account. Clarified in edits.
127079	68	17	68	36	"Amplification" is a better term than "intensification" or "acceleration" of the water cycle. As seen through salinity, it's directly related to the "wet get wetter and dry get drier" precipitation changes, thus should be included. Figure 2.26 should include the subsurface salinity of Cheng et al. (submitted). [ Trigg Talley, United States of America]	Editorial. Text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43103	68	17		18	Read " (Bindoff, 2013; Rupp et al., 2013; Durack et al., 2014; Zika et al., 2015; Cheng et al., submitted)," rather than " (Bindoff, 2013; Rupp et al., 2013; Durack et al., 2014; Zika et al., 2015) (Cheng et al., submitted)," [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
13249	68	18	68	18	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
57957	68	18	68	19	The reference to Section 2.3.1.3.5 seems a bit tenuous given that only data from 1980 is presented. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Intent of comment not obvious so no changes made.
57959	68	18	68	19	The qualitative statement 'strengthened' is subjective and could mean anything from 'correlated together' to 'close relationship in certain places'. I think explicitly stating that evaporation trends correlate with those of salinity is better and a reference to Figure 2.15 as well. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Text revised for clarity.
81203	68	19	68	20	As mentioned earlier, paired measurement of salinity and d18O of sea surface water could provide better information about the hydrological cycle. [ Supriyo Chakraborty, India]	Taken into account. The paleo salinity content has been completely revised.
12579	68	19	68	21	This sentence seems belong to the lines1-7, it is about surface change. But before and after this sentence, it is all about integrated change. [ Lijing Cheng, China]	Accepted. Text revised for clarity.
31497	68	19	68	23	This is inconsistent with chap 3. Chap 3 assesses this is due to E-P with high confidence. Chap 3 use model simulation which you cannot do here. For consistency across the report I suggest you take chap 3 assessment (and discuss with them if you disagree), and just report their assessment here. You can even say something like « From observation it is unclear blabla, but additional evidence from idealised and climate numerical simulation provide high confidence that blabla (Chap 3) ». [ Jean-Baptiste SALLEE, France]	Rejected. Cannot use model simulations to ascertain confidence levels for observed changes.
105687	68	19	68	23	"...unclear.. Reported rates of salinity .. Since 1980s/90s are realistic in reflecting an enhancement.. Or whether they potentially result from change in sampling methodology" This statement is unsupported by literature. It is a reasonable point to make about the RATE of change, but not the processes that are driving the changes. All studies that have assessed the long-term record of salinity change in observations report the coherent basin-scale salinity changes for the near-surface and subsurface ocean. Model studies, both free running (e.g. CMIP3/5/6) and idealized (E-P and other forcings) represent these same basin-scale changes. While the poor historical coverage of salinity (and temperature) observations do naturally lead to questions about the absolute rates of change, and what can be attributed to a forced response vs internal unforced variability, however, there is no current study(ies) which support the current statement quoted above [ Paul Durack, United States of America]	Accepted. Text revised.
73651	68	23	68	23	Insert space between 700 and m. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
105689	68	23	68	29	A couple of issues to highlight in this text block. Durack et al 2014 is an incorrect citation (ocean warming 1970-present, not salinity). The Vinogradova and Ponte 2017 (and previous publications) use an ocean reanalysis product, and just like other ocean reanalyses, these are very sensitive to a step change with Argo data coming online 2003-2006. In addition, this analysis considers a shorter sub-20-year timescale (1993-2010) which is strongly impacted by end points, particularly ENSO variability. This study (Vinogradova and Ponte 2017) concludes that water cycle amplification has occurred, but cautions the direct use of near-surface salinity in isolation, and rather advocates for a volume-averaged approach instead to account for variability effects [ Paul Durack, United States of America]	Accepted. Text revised.
73653	68	24	68	24	Insert space between 2000 and m. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
73655	68	25	68	25	remove )( and replace with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
13251	68	25	68	25	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
35549	68	25	68	25	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Rejected. Published sources are cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
12583	68	25	68	29	I suppose this sentence is to say there are interannual/decadal scale natural variabilities in salinity changes, so need to be careful to interpret short-term changes for example after 1993. That is fine, but the sentence needs modify to better convey this information. And there are lots of studies about inter-annual/decadal scale cahnges other than Vinogradova and Ponte 2017. A thorough assessment will be helpful. [ Lijing Cheng, China]	Accepted. Text revised for clarity.
90403	68	25			merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
43105	68	25			Read "(Durack et al., 2014; Cheng et al., submitted)" rather than "(Durack et al., 2014) (Cheng et al., submitted)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
41555	68	28	68	36	Apparent contradiction between lines 28-29 and 35-36? [ Laurent Labeyrie, France]	Accepted. Text revised for clarity.
105691	68	31	68	36	For Ch3 we have assessed "extremely likely" that human influence has contributed to both near-surface and subsurface oceanic salinity changes since the mid-20th century, with "high confidence" that salty gets saltier and fresh gets fresher. There is no obvious inconsistency with the final assessment paragraph, but would note with the comments above, particularly P68 L23-29 your assessment summary and supporting text appear inconsistent [ Paul Durack, United States of America]	Accepted. Text revised for clarity.
31501	68	33	68	33	it is disturbing that subsurface salinity change is very likely (chap 2) but the attribution to this change is extremely likely (chap 3). This was an issue up elevated in the TS and SPM. It does not look good. Need a discussion on that point with Chap 3 & 9 [ Jean-Baptiste SALLEE, France]	Rejected. Cannot use model simulations to ascertain confidence levels for observed changes.
57961	68	33	68	33	I don't think virtually certain is relevant here. I agree that the subtropics have become more saline, but there is much more variation at high latitudes. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Chapter 2's scope is to focus on large scale changes not regional/local.
90405	68	34			Oceans [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
12589	68	35	68	36	I suppose the authors want to say "the increase in the difference between high-s and low-s rergions are linked to an acceleration of the global hydrological cycle"? [ Lijing Cheng, China]	Accepted. Text revised for clarity.
12591	68	35	68	36	If the increase of salinity-contrast links to the acceleration of water cycle, what happens if the salinity-contrast increase is accelerating?? So, I recommend to change "acceleration" to "intensification". [ Lijing Cheng, China]	Accepted. Text revised for clarity.
12593	68	41	68	41	EN4 and DW10 were used. Please check Good et al. 2013 for the limitation of EN4 in long-term trend analyses, and also Cheng et al. 2020 provided some comparison among different datasets, this will help for the assessment of data products. [ Lijing Cheng, China]	Accepted. Suggested literature considered.
12585	68	41	68	49	It is almost the same to AR5 (except adding several years, but essentially the plot will be almost idencial to AR5 plot, so it seems to me that AR6 repeats the AR5 knowledge here). Some new knowledge can be highlighted here, for example (1) time series of salinity-contrast, showing the time evolution of "salty gets saltier and fresh gets fresher" since 1960 (2) The integrated salinity trends (0-700m or 0-2000m), which was not possible in AR5. [ Lijing Cheng, China]	Noted. Values/figure were place holder. Both revised with updated estimates.
30377	68	42			'Durack and Wijffels, 2010a and 2010b' have the same reference [ Gilles Delaygue, France]	Editorial. Text revised.
29867	68	43	68	43	Unbalanced parentheses in "analysis period 1950–2012)". [ Hernan Edgardo Sala, Argentina]	Editorial. Text revised.
13253	68	43	68	43	Missing or extra () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
113645	68	43	68	44	I would delete "Black contours show the associated climatological mean SSS for the analysis period." as it is written again at the end of the Figure caption (page 68 lines 46-48). [ Agnieszka Kowalczyk, Poland]	Editorial. Text revised.
30383	68	45			'All estimates are scaled to represent equivalent magnitude changes over a 50-year period': i do not understand the meaning of this sentence. The values should be the linear trend over 50 years. If it is, instead, the 1950 to 2008 differences scaled by 50/(2008-1950) (for a), this must be clearly explained. [ Gilles Delaygue, France]	Editorial. Text revised for clarity.
117283	68	54	70	29	Would it be possible to include an assessment of longer sea level change recostruction than last 2,500 yearrs, as shown in fig 2.27? Same time coverage than used for GHG would make it easier to compare. Or could be done in Ch09 likewise. [ Maisa Rojas, Chile]	Accepted. Figure panel (a) in Fig. 2.27 is now presenting time series for the last 800 ka with the text addressing all quantitatively estimated changes in GMSL for longer periods.
34857	68	54	70	29	The SOD claims that GMSL has risen faster in the last century than in the last 3 millennia. Please see rebuttal comment #6 above. [ Jim O'Brien, Ireland]	Accepted. Text revised for clarity.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37525	68	55	69	1	Please state if the IPCC audited any of the sea level data from tidal gauges. If that audit had been undertaken many of the uncertainties would have been identified (e.g. tidal gauges in rivers or at river mouths and influenced by river flow, neighbouring gauges in close proximity - just 1 or 2km - showing markedly different trends in sea level rise, the huge question of isostatic movement that GPS gauges only partly reveal because they are very rarely on the tide gauges) [ John McLean, Australia]	Noted. IPCC does not audit tide gauge observations but does assess published estimates based on tide gauges.
41557	69	2	69	21	Lines 2-21 need rewriting, with apparent contradictions and insufficient logic of presentation: Li 7-8 mean sea level (GMSL) during the last interglacial was, over several thousand years, between 5 and 10 m higher than 1985-2004 (medium confidence). Li 17-18 The contribution of ice-sheet loss during the LIG ranged between 3-11m (medium confidence). [ Laurent Labeyrie, France]	Accepted. Clarified assessment of LIG.
37093	69	3	69	4	Remove sentence unless it can be shown that the CMIP5 model accurately included all factors. (I draw your attention to text box 9.2 of AR5 in which 111 or 114 CMIP5 climate model runs predicted greater warming for the previous 15 years than the data from temperature records showed.) [ John McLean, Australia]	Rejected. Assessment is based on observations not models.
73657	69	4	69	5	Remove unit split across lines [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
7157	69	5	69	8	Please consider a new independent estimates of the ocean mass contribution to the GMSL based on the global ocean salt budget (over 2005-2015). This supports further support the exiting observation of mass increase. Llovel, W., S. Purkey, B. Messygnac, A. Vazquez, N. Kolodziejczyk and J. Bamber, 2019: Global ocean freshening and sea level rising since 2005, 9:17717 Nature Scientific Report.doi:10.1038/s41598-019-54239-2 [ Nicolas Kolodziejczyk, France]	Rejected. Mass change contribution to total sea level is not assessed in this subsection. Only total sea level.
30385	69	7			'global mean sea level (GMSL)' > use 'GMSL' (already defined) [ Gilles Delaygue, France]	Editorial. Text revised.
93379	69	10	69	10	Delete space before Miller [ Carles Pelejero, Spain]	Editorial. Text revised.
13255	69	10	69	10	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
35551	69	10	69	10	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Rejected. Published sources are cited.
57963	69	10	69	12	Reference to Morlighem et al. (2019) is incorrect, should be Morlighem et al. (2020) (doi: 10.1038/s41561-019-0510-8). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial. Text revised.
57965	69	10	69	12	It is unclear how the uncertainty estimates were derived the GMSL values. By adding uncertainties I obtain a value of +/- 1.03. Could you clarify or amend this uncertainty value? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. $\text{SQRT}(2^2 + 1.8^2) = 2.6$
90407	69	10			merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
43107	69	10			Read " (Cramer et al., 2011; Miller et al., in rev.). " rather than " (Cramer et al., 2011) ( Miller et al., in rev.). " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
30387	69	12	69	14	I do not think these explanations of assumptions are relevant, as incomplete and not precise enough to be of interest; I would keep the result of GMSL. What is the difference between 'OHC change' and 'thermosteric sea-level rise'? [ Gilles Delaygue, France]	Accepted. Removed OHC.
8909	69	14	69	14	65.6 + 7 = 72.6, but presumably the degree of rounding is different for different numbers, and 73 ± 3 would be more consistent [ Robert Kopp, United States of America]	Accepted. Rounded as recommended.
30391	69	15	69	17	This sentence should be simplified. 1. 'During the LIG': in this context of GMSL, periods have been considered with their max or min GMSL. 'During' refers instead to a period of few ka: is it the goal here to refer to changes in GMSL during the LIG? 2. 'so all but' is a very stylish and complicated expression to say 'the rest from polar ice sheets'. 3. '0.7+-0.6': why such a large uncertainty, from both components it should be +/-0.3 only? [ Gilles Delaygue, France]	Accepted. See comment #57671.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57671	69	15	69	18	<p>These two sentences could be combined and restructured, as the meaning appears unclear. The sentence states that "...~0.7 ±0.6 m (sea level rise) must have arisen from polar ice sheets (Dutton et al., 2015a)". However, study by Dutton et al. (2015b) suggests that the polar ice sheets contributed ~5–8 m of ice-equivalent sea level to the LIG sea level peak.</p> <p>According to Dutton et al. (2015a, b), the higher-than-present GMSL (+6 to 9 m, possibly up to 10 m, Rohling et al., 2017) can be explained by ocean thermal expansion and melting of mountain glaciers, which together contributed ~1 m to sea level rise, in addition to Greenland Ice Sheet melt (~0.6 to 3.5 m) and Antarctica Ice Sheet (AIS) mass loss. While evidence for AIS retreat are sparse, recent research from Turney et al. (2020) argues that Antarctica may have contributed several meters to global sea level during early last interglacial.</p> <p>Dutton, A., Carlson, A. E., Long, A., Milne, G. A., Clark, P. U., DeConto, R., ... &amp; Raymo, M. E. (2015a). Sea-level rise due to polar ice-sheet mass loss during past warm periods. <i>Science</i>, 349(6244), aaa4019.</p> <p>Dutton, A., Webster, J. M., Zwartz, D., Lambeck, K., &amp; Wohlfarth, B. (2015b). Tropical tales of polar ice: evidence of Last Interglacial polar ice sheet retreat recorded by fossil reefs of the granitic Seychelles islands. <i>Quaternary Science Reviews</i>, 107, 182-196.</p> <p>Rohling, E. J., Hibbert, F. D., Williams, F. H., Grant, K. M., Marino, G., Foster, G. L., ... &amp; Webster, J. M. (2017). Differences between the last two glacial maxima and implications for ice-sheet, δ18O, and sea-level reconstructions. <i>Quaternary Science Reviews</i>, 176, 1-28.</p> <p>Turney, C. S., Fogwill, C. J., Golledge, N. R., McKay, N. P., van Sebille, E., Jones, R. T., ... &amp; Ramsey, C. B. (2020). Early Last Interglacial ocean warming drove substantial ice mass loss from Antarctica. <i>Proceedings of the National Academy of Sciences</i>, 117(8), 3996-4006. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]</p>	Accepted. In the final version of FGD Dutton et al. (2015) is cited and relevant estimates of GMSL for the MIS-11 period are provided. The conclusion is made that "AR5 estimate of 6–13 m for MIS 11 remains the best available". Also more details on LIG budget along with discussion are moved to 9.6.2.
93381	69	15	69	21	<p>Regarding sea level during the LIG and the contribution of ice sheets, it would be good to include the recent reference: Turney et al (2020). Early Last Interglacial ocean warming drove substantial ice mass loss from Antarctica. <i>PNAS</i>, 117(8), 3996–4006. <a href="https://doi.org/10.1073/pnas.1902469117">https://doi.org/10.1073/pnas.1902469117</a> [ Carles Pelejero, Spain]</p>	Noted. Uncertainties associated with LIG budget are briefly mentioned with the reference to Capron et al., 2019. Further discussion on this issue is provided in section 9.6.2 (CH9).
8911	69	15	69	21	<p>Something got cut for the LIG that provided an assessment of total LIG GMSL change. "As assessed by SROCC, during the Last Interglacial (LIG, ~129-116 ka), it is virtually certain that GMSL exceeded current levels (high confidence), and reached a peak that was likely 6–9 m higher than today, but did not exceed 10 m (medium confidence) (Oppenheimer et al., 2019). "</p> <p>My impression is that chapter 2 was to assess global-scale metrics, and attribution of these global-scale changes to different process would be assessed by chapter 9. This text is somewhat redundant with 9.6.2. 9.6.2 also refers back to an assessment that is apparently not made in this section. See:</p> <p>"During the Last Interglacial (LIG, ~129-116 ka), GMST was very likely 1-2°C above pre-Industrial temperatures (Chapter 2). It is virtually certain GMSL was higher than today, likely by 5–9 m (Chapter 2). No more than 0.3 ± 0.1 m of this GMSL rise could have come from glaciers (Section 9.5.1), and no more than 0.4 ± 0.3 m from global mean thermal expansion (McKay et al., 2011), so all but 0.7 ± 0.3 m of the GMSL rise must be sourced from the polar ice sheets (Dutton et al., 2015a)."</p> <p>This is inconsistent with the statement of 3-11 m ice sheet contribution, which is attributed to 9.6.2, but is not present there.</p> <p>The citations on the multiple peaks during the LIG are selective and incomplete, missing key studies including Kopp et al. (2013) and Vyverbeg et al. (2018). [ Robert Kopp, United States of America]</p>	Accepted. Clarified assessment of LIG.
30389	69	15			<p>I think the total LIG GMSL is missing here, before detailing its different components. [ Gilles Delaygue, France]</p>	Accepted. Clarified assessment of LIG.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42893	69	16	69	17	A newer estimate of the rise from global mean thermal expansion was derived from the OHC in Shackleton et al 2020 for the LIG. 0.7+/-0.3 for the transient early LIG, and close to zero cf modern for the rest of the LIG. This replaces the McKay estimate. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Noted, see comments #30391 and #93381
35295	69	16	69	17	On the Dutton et al., 2015 citation: Cross check with chapter 9.6.2, citing the same reference but a different uncertainty. [ Alessio Rovere, Germany]	Accepted. Clarified assessment of LIG.
35297	69	18	69	18	<p>3-11m range: Section 9.6.2 never cites this value, and does not go in the details of maximum LIG sea level. For full disclosure, I am among the co-authors of two of the articles cited here. Please feel free to cross-check relevance.</p> <p>I would add this to clarify the context: Large uncertainties underpinning the LIG maximum sea level are mostly stemming from uncertainties in GIA corrections (Rohling et al., 2017;Dendy et al., 2017), field data accuracy (Hibbert et al., 2016, Rovere et al., 2016) and post-depositional tectonic or dynamic topography displacements (Austermann et al., 2017).</p> <p>Rohling, E.J., Hibbert, F.D., Williams, F.H., Grant, K.M., Marino, G., Foster, G.L., Hennekam, R., De Lange, G.J., Roberts, A.P., Yu, J. and Webster, J.M., 2017. Differences between the last two glacial maxima and implications for ice-sheet, <math>\delta^{18}O</math>, and sea-level reconstructions. Quaternary Science Reviews, 176, pp.1-28.</p> <p>Dendy, S., Austermann, J., Creveling, J.R. and Mitrovica, J.X., 2017. Sensitivity of Last Interglacial sea-level high stands to ice sheet configuration during Marine Isotope Stage 6. Quaternary Science Reviews, 171, pp.234-244.</p> <p>Hibbert, F.D., Rohling, E.J., Dutton, A., Williams, F.H., Chutcharavan, P.M., Zhao, C. and Tamisiea, M.E., 2016. Coral indicators of past sea-level change: A global repository of U-series dated benchmarks. Quaternary Science Reviews, 145, pp.1-56.</p> <p>Rovere, A., Raymo, M.E., Vacchi, M., Lorscheid, T., Stocchi, P., Gomez-Pujol, L., Harris, D.L., Casella, E., O'Leary, M.J. and Hearty, P.J., 2016. The analysis of Last Interglacial (MIS 5e) relative sea-level indicators: Reconstructing sea-level in a warmer world. Earth-Science Reviews, 159, pp.404-427.</p> <p>Austermann, J., Mitrovica, J.X., Huybers, P. and Rovere, A., 2017. Detection of a dynamic</p>	Accepted. Clarified assessment of LIG.
30395	69	18	69	19	what means 'locations' here, 'records'? [ Gilles Delaygue, France]	Taken into account. Changed to 'sites.'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35299	69	18	69	21	<p>I find this a bit too simplistic and not too well organized. Actually, many different patterns of sea level change were proposed for the LIG. In general, oscillations (described here) are surely one of the most discussed, as they would imply ice regrowth. But also sudden accelerations were proposed (and are currently debated, but not yet disproven in full), such as in O'Leary et al., 2013. Also I think reference to Chapter 9.6.2 should be added here. Also see Kopp et al. (2017) and Capron et al., 2019 on this matter.</p> <p>O'Leary, M., Hearty, P., Thompson, W. et al. Ice sheet collapse following a prolonged period of stable sea level during the last interglacial. <i>Nature Geosci</i> 6, 796–800 (2013).  <a href="https://doi.org/10.1038/ngeo1890">https://doi.org/10.1038/ngeo1890</a></p> <p>Kopp et al., 2017. <a href="http://www.pastglobalchanges.org/products/11513">http://www.pastglobalchanges.org/products/11513</a></p> <p>Capron et al., 2019 <a href="https://doi.org/10.1016/j.quascirev.2019.06.030">https://doi.org/10.1016/j.quascirev.2019.06.030</a></p> <p>Here is how I would rephrase this part.</p> <p>One open question regarding the Last Interglacial is whether sea level maintained a stable versus oscillatory pattern (Kopp et al., 2017; Capron et al., 2019). While there is evidence from some locations of high-to-low sea level swings (Bentley et al., 2014; Skrivaneck et al., 2018) or sudden accelerations in sea level change (O'Leary et al., 2013), other locations support the notion that Last Interglacial sea level was substantially stable (Pan et al., 2018, Polyak et al., 2018). Barlow et al., 2018 re-analyzed geological and stratigraphic evidence of ice-sheet regrowth during the Last Interglacial, and found no evidence of ice-sheet regrowth in this period. It was recently proposed that asynchronous minima in AIS and GrIS volume may explain evidence for multiple sea-level peaks (Chapter 9.6.2). [ Alessio Rovere, Germany]</p>	Accepted. Clarified discussion of multiple LIG peaks.
42923	69	18			Ch 9 says 5-9 m (page 88, line 47) not 3-11 m. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified assessment of LIG.
30393	69	18			'ranged between 3-11m': i do not have Chapter 9 to check, but such range seems huge to me. A more usual range is 6-9 m (e.g., Dutton et al 2018). [ Gilles Delaigue, France]	Accepted. Clarified assessment of LIG.
41559	69	19	69	21	"However, there is evidence from some locations of multiple local regional sea level peaks of ice-sheet regrowth interrupting the LIG sea level rise, whereas other locations do not support this" Where is science? [ Laurent Labeyrie, France]	Accepted. Clarified discussion of multiple LIG peaks.
30397	69	19			'local regional': keep one of them [ Gilles Delaigue, France]	Accepted.
43109	69	20		21	Read "(Pan et al., 2018b; Polyak et al., 2018; Barlow et al., 2018)." rather than "(Pan et al., 2018b; Polyak et al., 2018) (Barlow et al., 2018)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
73659	69	21	69	21	Remove )( and replace with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
13257	69	21	69	21	references can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
90409	69	21			merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
30399	69	21			Barlow et al., 2018 missing in the reference section [ Gilles Delaigue, France]	Added. Barlow, N. L., McClymont, E. L., Whitehouse, P. L., Stokes, C. R., Jamieson, S. S., Woodroffe, S. A., ... & Horrocks, J. R. (2018). Lack of evidence for a substantial sea-level fluctuation within the Last Interglacial. <i>Nature Geoscience</i> , 11(9), 627-634.
26639	69	23	69	23	This definition of the LGM (21-19 ka) is different of that given previously/above (i.e. 21 ka). It also differs from that given in Chapter 7, Table 11 or in Chapter 8 (8.2.2.1) as well (21 ka). It should be homogeneized throughout AR6. [ Eric Brun, France]	Taken into account. Removed dates.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127081	69	23	69	36	For consistency with the discussion of more recent sea level rise, express changes here as mm yr <sup>-1</sup> rather than m kyr <sup>-1</sup> . Yes, they're the same, and at a minimum authors should note they're the same so that readers can make the connection with recent numbers. [ Trigg Talley, United States of America]	Accepted.
57967	69	24	69	26	Both the Lambeck et al. (2014) and Yokoyama et al. (2018) study agree that the rate was 12 m kyr <sup>-1</sup> between 16.5 ka and 8.2 ka. Please amend this. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Removed Yokoyama et al. (2018) for discussion of rate over the bulk over the deglacial, which cites Lambeck et al. (2014) for this rate.
26641	69	25	69	25	The deglaciation of the southern margin of NH ice-sheets began well before. About 20-19 ka BP. See Carlson & Clark (2012, Reviews of Geophysics) for a thorough review. Carlson, A. E., and P. U. Clark (2012), Ice sheet sources of sea level rise and freshwater discharge during the last deglaciation, Rev. Geophys., 50, RG4007, doi:10.1029/2011RG000371 [ Eric Brun, France]	Taken into account. Clarified that we are talking about the main phase of the deglaciation.
57675	69	26	69	28	Study of Liu et al. (2016) is indeed valid for sea level history during meltwater pulse-1A, however, I failed to find there the direct estimates of the sea level rise, i.e., that "GMSL rose between 8 m and 15 m (Liu et al., 2016b)". If I am correct and have not overlooked this critical information, another reference is needed here. Suggestion for rephrasing: The fastest rise exceeded 40 m kyr <sup>-1</sup> and occurred between 14.6 and 14.3 ka, during which the sea level rose by ~14 to 18 meters in less than 500 years (Deschamps et al., 2012; Sanborn et al., 2017). Deschamps, P., Durand, N., Bard, E., Hamelin, B., Camoin, G., Thomas, A. L., ... & Yokoyama, Y. (2012). Ice-sheet collapse and sea-level rise at the Bølling warming 14,600 years ago. Nature, 483(7391), 559-564. Liu, X., Rendle-Bühning, R., & Henrich, R. (2016). Climate and sea-level controls on turbidity current activity on the Tanzanian upper slope during the last deglaciation and the Holocene. Quaternary Science Reviews, 133, 15-27. Sanborn, K. L., Webster, J. M., Yokoyama, Y., Dutton, A., Braga, J. C., Clague, D. A., ... & Hansen, J. R. (2017). New evidence of Hawaiian coral reef drowning in response to meltwater pulse-1A. Quaternary Science Reviews, 175, 60-72. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The suggested literature was reviewed and included if within scope of the assessment being performed by chapter 2.
57673	69	27	69	27	Is it correct that Liu et al. (2016b) refers to a dataset, not a paper? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Liu et al 2016 is now correctly cited in the bibliography.
30401	69	27			Liu et al., 2016 should refer to Liu et al 2016, Nat Geo, doi:10.1038/ngeo2616, and NOT the Liu et al 2016b in the reference [ Gilles Delaygue, France]	Accepted.
30403	69	28			'from 8.2 m' : 'from 8.2 ka' ? [ Gilles Delaygue, France]	Accepted.
35301	69	29	69	29	I think here is worth mentioning the systematic compilation of Holocene RSL data led by Khan et al., 2019 in QSR. It is the most up-to date reference for standardized Holocene sea level data [ Alessio Rovere, Germany]	Accepted. Khan joined the CH2 team as a CA at the stage of SOD-FGD transition. Khan et al. paper is cited. The Khan et al. database is a major new resource for evaluating RSL during the Holocene is extensively used in the assessment.
43111	69	31			Read " with decimetre scale " rather than " with deci-metre scale " [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
57969	69	32	69	34	The statement that the increase in GMSL is sustained is correction, although there is a slight change in ~1960 which Dangerdorf et al. (2019) relate to the intensification of the Southern Hemispheric westerlies and enhanced ocean heat uptake, [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Mechanisms are the domain of chapter 9 and not chapter 2.
5397	69	34			Both 1820 and 1860 are in the time period of no data. Why not just say evident since 1880? Too little is made of the important Fig. 2.27. [ Bryan Wear, United States of America]	Rejected. Statistical analyses of proxy and tide-gauge data support this timing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66471	69	36			<p>Comment on Table 9.3:</p> <p>My comment refers to Svalbard record in the Table 9.3 as this is my main area of interests. The values given in the Table 9.3. are from the SROCC report, where SLE was estimated based on mass balance assessments studies.</p> <p>Studies which were used for SROCC estimates are: Aas et al. (2016), Wouters et al. (2019) and Zemp et al. (2019). They are excellent studies and significantly contributed to our knowledge on response of glaciers to climate changes. However, either due to their global nature (thus generalisation/low resolution) or not a full view on MB process, in my opinion some of them do not present the full picture (or a detailed one) for Svalbard.</p> <p>Tidewater glaciers covers ~60% of glaciated area of Svalbard (Błaszczyk et al., 2009). Their frontal ablation (e.g. calving) may be a large contributor to the total MB changes in Svalbard. On the other hand, climatic mass balance models (CMB) do not take into account the dynamic processes of glaciers in their estimations – including calving. One of such studies is a study of Aas et al. (2016). This is an excellent study and describes many complex processes which are a sum of the CMB of Svalbard, however, it does not take into account dynamic processes (it is not a total MB). Błaszczyk et al. (2009) estimated mass loss due to calving on Svalbard as much as 6.75 +/- 1.7 Gt yr<sup>-1</sup> (2000-2006). This is the only study of calving flux in Svalbard and, even though calving depends on e.g. surging events or temperature of water to which a glacier terminates (and study should be updated), the given number is significant and should be considered in total MB assessments. This number is 3/4 (!) of CMB estimated by Aas et al. (2016) – which is -9 Gt yr<sup>-1</sup>. In addition, Błaszczyk et al. (2019) estimates the frontal ablation as 40% of the total annual mass loss for glaciers in Hornsund fiord (South Spitsbergen, Svalbard). Therefore, frontal ablation of glaciers in Svalbard should be considered (or: should not be neglected) in total MB assessments. Therefore, the estimated SLE in the SROCC table is probably underestimated.</p> <p>Wouters et al. (2019) on the other hand, is a global study of land ice masses (excluding Greenland and Antarctica) and their contribution to SLR. Unique and significant as it provides information on changes of mass in 17 glaciated regions based on gravimetry (GRACE). However, Svalbard is archipelago of ~60.000 km<sup>2</sup> total area (less than 60% is glaciated), horizontal resolution of GRACE is</p>	Sent to Chapter 9. Taken into account, text revised and a consensus estimate for Svalbard mass loss (Schuler et al., 2020) and new estimate from elevation changes (Hugonnet et al, in review) are included in table 9.3.
37095	69	41	69	50	<p>Yet again the IPCC is oblivious to the fact that the number of recording stations reporting data and the distribution of them has changed markedly over time. A simple examination of PSMSL tide gauge data reveals that the annual average number of reporting gauges in 1900 was 74, in 1950 was 260, in year 2000 was 671 and peaked at 781 in 2011. It defies logic that anyone would think that 74 gauges was enough to determine a global average. [ John McLean, Australia]</p>	Noted. Smaller number of observations are reflected in larger uncertainty.
113647	69	42	69	42	<p>"tide gauge reconstruction (Kopp et al., 2016; Kemp et al., 2018)" -- I can see Kemp (2018) dataset in the Figure, but can't see Kopp (2016) dataset. Is it combined with Kemp (2018) data (grey line)? [ Agnieszka Kowalczyk, Poland]</p>	Noted. Correct. Kopp et al 2016 has been superseded by Kemp et al. 2018.
30405	69	45	69	46	<p>'Vertical lines indicate changes in the resolution of the time axis which becomes increasingly resolved from left to right': not sure after 1970? (20 yrs betw. 2 ticks) [ Gilles Delaygue, France]</p>	Editorial. Figure revised.
73661	69	48	69	48	<p>Change 'is' to 'are'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]</p>	Editorial. Text revised.
30407	69	48			<p>'For clarity...all data have been vertically shifted': what does it mean? [ Gilles Delaygue, France]</p>	Editorial. Text revised for clarity.
73663	69	50	69	50	<p>Change 'is' to 'are'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]</p>	Editorial. Text revised.
26643	69	55	69	55	<p>What is meant by "continued" acceleration is unclear. If what is meant is that there is an acceleration over any small sub-period (of 10 to 30 year) within the 20th century then the data does not fully support this statement. All the data mentioned here are reconstructions that cannot reproduce precisely the interannual to decadal variability because of structural issues (see Calafat et al. 2014) or because of the tide gauge sampling ( see for example Christiansen et al. 2010) [ Eric Brun, France]</p>	Accepted. Text revised for clarity.
5399	69	55	69	56	<p>No where else in this document do you talk about accelerations. The units are confusing and unhelpful. It would be much better that the to say the rates increased from XXX in period X to YYY in period Y. Such rates could be inferred from Fig. 2.27, unlike the mentioned acceleration. [ Bryan Weare, United States of America]</p>	Accepted. Text revised for clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6555	69	55	70	24	Line 55 of page 69 refers to "acceleration of the GMSL rise", but line 23 of page 70 refers to acceleration of the "GMSL rate". Line 24 of page 70 refers to "acceleration rate". The use of the word acceleration needs to be tidied up. Cannot the text simply refer to "acceleration of GMSL"? At least the phrase "rate of GMSL" does not make sense. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised for clarity.
8913	69	56	69	56	0.004 ± 0.009 does not really support an acceleration.  Also, it is important to note that -- as Dangendorf et al 2019 highlight -- the acceleration began in the late 1960s; accordingly, measuring an acceleration from the beginning of the 20th century is a bit of an odd metric. [ Robert Kopp, United States of America]	Accepted. Text revised for clarity.
43113	69	56	70	1	Read " (Church and White, 2011; Olivieri and Spada, 2013; Jevrejeva et al., 2014; Wenzel and Schröter, 2014; Hay et al., 2015; Kemp et al., 2018; Marcos and Woodworth, 2017)." rather than "(Church and White, 2011; Olivieri and Spada, 2013; Jevrejeva et al., 2014; Wenzel and Schröter, 2014; Hay et al., 2015; Kemp et al., 2018) (Marcos and Woodworth, 2017) [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
116005	69		69		section 2.3.3.3, paleo information, I could not find a clear assessment of LIG sea level change (the partition is there but not the estimate in the paragraph, while the overall increase is reported in the summary statement). [ Valerie Masson-Delmotte, France]	Accepted. Added clearer assessment of LIG GMSL.
87377	70	1	70	1	The recent reconstruction of Ayache et al. (2018) do show substantial variation over the Holocene, in line with Thornalley et al. (2013) and do show a maximum in the early Holocene that might be useful to notice here. Ayache M. Swingedouw D., Mary Y., Eynaud F., Colin C (2018) AMOC variability over the Holocene: A new reconstruction based on multiple proxy-based SST records. Global and Planetary Changes 170, pp. 172-189. [ Didier Swingedouw, France]	Taken into account, and the publication is now considered in the assessment.
73665	70	1	70	1	Remove )( and replace with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
13259	70	1	70	1	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
9941	70	1	70	1	double brackets [ Olga Zolina, France]	Editorial. Text revised.
90411	70	1			merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
26645	70	2	70	2	In chapter 9 Dangendorf et al. 2019 is referenced as "Dangendorf et al. submitted" and has a slightly different title. Please check out for consistency [ Eric Brun, France]	Taken into account. Paper is correctly cited in Chapter 2. Paper in Chapter 9 refers to a different time period (since 1970s), so likely they are different papers.
79071	70	2	70	4	I think 'relies on' is too strong - the CMIP models are used to inform spatial variations in the reconstructions, but as it is written here the reader might infer that the CMIP models determine the entire thermosteric and dynamic contribution, which is(to my understanding) not the case. I think this description does not accurately reflect the methodology used by Dangendorf et al., and could be improved, especially since these estimates might be used elsewhere in the report too. [ Aimee Slangen, Netherlands]	Reject. Text revised for clarity
30409	70	2			'20th & 21st centuries' or 'since 1960' [ Gilles Delaygue, France]	Editorial. Text revised.
8915	70	3	70	3	This is not really accurate -- effectively, it uses a prior based on CMIP5, but reweights based on observations. It is a multi-model Kalman Smoother method, as is Hay et al. 2015 -- effectively Dangendorf et al 2019 extends Hay et al 2015 and adds in high-frequency variability [ Robert Kopp, United States of America]	Accepted. Text revised for clarity
71877	70	3			Is this correct as stated? [ John Church, Australia]	Accepted. Text revised for clarity.
108115	70	4	70	4	Instead of the term "bias-corrections" I suggest to use the term "bias adjustments", which is explained in Chapter 10 Section 10.3.1.4.2 and used in Chapter 2, 8, 10 and 12. Probably in the case of instrumental measurements, the term "bias correction" is more appropriate. [ Claas Teichmann, Germany]	Accepted. Text revised.
79075	70	4	70	4	cross reference for SL obs is 9.6.1 [ Aimee Slangen, Netherlands]	Editorial. Text revised.
71879	70	4		7	Should refer to Watson et al. (2014) here for their identification of the bias. [ John Church, Australia]	Accepted. Suggested literature considered.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43115	70	5			Read " (see for example WCRP Global Sea Level Budget Group (2018))" rather than " (see for example (WCRP Global Sea Level Budget Group, 2018))" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
43117	70	6		7	Read "for the 1993-2015 period" rather than "for the period 1993-2015" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
79073	70	7	70	9	As we're using the Dangendorf estimates throughout WG1, would it be worth expanding on the assessment here, and briefly explaining here why D19 is lower than assessed in SROCC? [ Aimee Slangen, Netherlands]	Rejected. Avoiding hybrid estimates when direct observations are available.
5401	70	7			Is time bias accounted for in F.2.27? Would there be a visible change? [ Bryan Weare, United States of America]	Noted. Not all estimates included. Figure was revised.
30411	70	7			'GMSL rising rates' [ Gilles Delaygue, France]	Editorial. Text revised.
26647	70	8	70	9	what do you mean by "are consistently lower" "albeit not statistically different". Do you mean that the mean or the median of Dangendorf et al. distribution is lower than the mean or the median of the SROCC range for all periods? Can you specify please? [ Eric Brun, France]	Accepted. Text revised for clarity.
26649	70	11	70	11	Literature supports more than only an agreement on linear rates between satellite products. The variability in satellite estimates of sea level is significant at all time scales from interannual to longer time scales and there is agreement on all time scales from interannual to longer time scales between satellite products on the estimate of this variability. This is also true for accelerations. See for example Ablain et al. 2019 [ Eric Brun, France]	Rejected. Scope is only observed changes not details about variability. Potential overlap with chp9's scope.
43119	70	12		13	Read "(Chen et al., 2017; Dieng et al., 2017; Nerem et al., 2018; Wang et al., 2019) " rather than "(Chen et al., 2017; Dieng et al., 2017; Nerem et al., 2018)(Wang et al., 2019)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
73667	70	13	70	13	Remove )( and replace with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
13261	70	13	70	13	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
30413	70	13	70	14	'is almost double that of the 1900-1990': the rate over this period is missing here [ Gilles Delaygue, France]	Accepted. Text revised for clarity
90413	70	13	70	16	merge the citations into one chronologically ordered list [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
30415	70	14	70	15	'with an acceleration of about 0.1 mm yr-2 over the satellite era': remove since detailed at L.18 [ Gilles Delaygue, France]	Editorial. Text revised.
43121	70	15		16	Read " (Nerem et al., 2018; WCRP Global Sea Level Budget Group, 2018; Wang et al., 2019)" rather than " (Nerem et al., 2018; WCRP Global Sea Level Budget Group, 2018)(Wang et al., 2019)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial. Text revised.
5403	70	15			Eliminate the use of "acceleration" [ Bryan Weare, United States of America]	Rejected. Assessment based on published literature.
73669	70	16	70	16	Remove )( and replace with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Text revised.
13263	70	16	70	16	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial. Text revised.
57971	70	17	70	18	I think the origin of the WCRP (2018) products needs to be stated i.e. these are altimeter products that have uncertainties related to them. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. However, altimeter products do not come with uncertainty. The reviewer is Referred to the respective website sources.
30417	70	18			'GMSL rising rates' [ Gilles Delaygue, France]	Editorial. Text revised.
69169	70	22	70	29	Inserting "In summary" at the top of the paragraph, like the other sections in 2.3.3. is suggested. [ Kaoru Magosaki, Japan]	Editorial. Text revised.
26651	70	23	70	23	It is not the "rate" of sea level that has "accelerated" but the "changes" in sea level that have "accelerated". The "acceleration" of sea level change only makes the "rate" of sea level change "increase" or "rise" but not "accelerate" [ Eric Brun, France]	Accepted.
26653	70	23	70	23	The sentence "has doubled" is not clear here and requires caution. What are you comparing the past 2-decade trend with? With the 20th century trend? With a 2-decade period at the beginning of the 20th century? In the first case people will complain you are not comparing apples with apples. In the second case the uncertainty in the sea level trend of the first decades of the 20th century probably does not allow you to state unambiguously for a doubling of the trend. [ Eric Brun, France]	Accepted. Text revised for clarity.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8917	70	24	70	24	No assessment has been made to support the statement that GMSL is higher now than in 6000 years, and this may well not be true. It does not appear to be true in Kemp et al. (2018). [ Robert Kopp, United States of America]	Taken into account; statement about GMSL during the mid-Holocene has been extensively revised.
57677	70	24	70	25	In my opinion, the sentence "GMSL is now higher than at least the last 6000 years and likely since the last interglacial" (see also executive summary, page 2-6, lines 23-24) needs support and/or rephrasing. Evaluation of uncertainty is missing in the first part of the sentence. Including information about GMSL changes during the last 6000 years can help to correctly define the level of uncertainty (likely or about as likely as not?). At present, literature overview (page 2-69, lines 23-36) mainly focuses on rates of sea level rise after the last glacial, but not on actual GMSL estimates, which is essential to support the aforementioned conclusion (page 2-70, lines 24-25 and executive summary, page 2-6, lines 23-24). While I failed to find robust estimations for GMSL during last 6000 years in the literature, evidence from Lambeck et al. (2014) could be added, which suggests that total global sea level rise since 6.7 kyr was ~4 m, of which ~3 m occurred in the interval 6.7-4.2 kyr BP with a further rise ≤1 m up to the time of onset of recent sea-level rise 100-150 years ago. The second part of the sentence „GMSL is now higher than at least the last 6000 years and likely since the last interglacial (lines 24-25) also needs support from literature (page 2-69, lines 23-36). In particular, information about the Holocene sea level highstand (~7000 years ago, Dougherty et al., 2019), which was possibly 1 to 1.5 m above present mean sea level (Murray-Wallace et al., 2014), is lacking. Overall, considerable debate surrounds both the peak level and timing of the Holocene highstand (Dutton et al., 2015; Dougherty et al., 2019). Lambeck, K., Rouby, H., Purcell, A., Sun, Y., & Sambridge, M. (2014). Sea level and global ice volumes from the Last Glacial Maximum to the Holocene. Proceedings of the National Academy of Sciences, 111(43), 15296-15303. Murray-Wallace CV, Woodroffe CD. Quaternary sea-level changes: a global perspective: Cambridge University Press; 2014. Dougherty, A. J., Thomas, Z. A., Fogwill, C., Hogg, A., Palmer, J., Rainsley, E., ... & Turney, C. (2019). Redating the earliest evidence of the mid-Holocene relative sea-level highstand in Australia and implications for global sea-level rise. PLoS one, 14(7). Dutton, A., Carlson, A. E., Long, A., Milne, G. A., Clark, P. U., DeConto, R., ... & Raymo, M. E. (2015). Sea-level rise due to polar ice-sheet mass loss during past warm periods. Science, 349(6244),	Taken into account; statement about paleo GMSL has been extensively revised.
127083	70	24	70	25	The text on page 69, lines 28-29, said that GMSL was rising until at least 4.2 kya. So if it was higher than at least the last 6000 years and the cited text on the previous page is correct, it was indeed higher than at any time since the last interglacial. So the 6000-year assessment here is inconsistent with the text on the preceding page. [ Trigg Talley, United States of America]	Taken into account; statement about paleo GMSL has been extensively revised.
30419	70	24			'than at least over the last' [ Gilles Delaygue, France]	Editorial. Text revised.
8919	70	27	70	29	No assessment has been made on which to base this 3-11 m very likely range for the LIG. Prior to the relevant text being moved over to ch 2, the SOD Advanced Draft of ch 9 made an assessment of a likely range off 5-9 m (medium confidence). [ Robert Kopp, United States of America]	Taken into account; 3-11 m was based on "very likely" range rather than "likely". Range now consistent with CH9.
30421	70	28			'a peak that was very likely 3 to 11 m': this range has only 'medium confidence' p.69L18 for the main component (ice sheets), how can it be 'very likely' here? [ Gilles Delaygue, France]	Taken into account; 3-11 m was based on "very likely" range rather than "likely". Range now consistent with CH9.
85019	70	32	72	49	No comments [ Katrine Husum, Norway]	Noted, comment not clear, and not applicable.
41561	70	34	71	53	Bravo, a real good quality synthesis, which proves this chapter could be written better, following the same philosophy of presentation, focused on science results, not history of publications. [ Laurent Labeyrie, France]	Thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69069	70	34	71	53	The heading of this section is misleading "Overturning circulation". This section is extensively address the Atlantic Meridional Overturning Circulation (AMOC). There are no cited reference for the Southern Ocean upper or lower cell and unless cited literature is added to support the conclusions all reference to the Southern Ocean upper and or lower overturning circulation should be removed and the section renamed. A suggested starting point for Southern Ocean is (1) Purkey, S. G., Johnson, G. C., Talley, L. D., Sloyan, B. M., Wijffels, S. E., Smethie, W., et al. (2019). Unabated bottom water warming and freshening in the South Pacific Ocean. <i>Journal of Geophysical Research: Oceans</i> , 124, 1778–1794. <a href="https://doi.org/10.1029/2018JC014775">https://doi.org/10.1029/2018JC014775</a> and (2) LD Talley, RA Feely, BM Sloyan, R Wanninkhof, MO Baringer, JL Bullister, CA Carlson, SC Doney, RA Fine, E Firing, N Gruber, DA Hansell, M Ishii, GC Johnson, K Katsumata, RM Key, M Kramp, C Langdon, AM Macdonald, JT Mathis, EL McDonagh, S Mecking, FJ Millero, CW Mordy, T Nakano, CL Sabine, WM Smethie, JH Swift, T Tanhua, AM Thurnherr, MJ Warner, J-Z Zhang, 2016. Changes in Ocean Heat, Carbon Content, and Ventilation: Review of the First Decade of Global Repeat Hydrography (GO-SHIP). <i>Annual Review of Marine Science</i> , 8, 19.1-19.31, 10.1146/annurev-marine-052915-100829 [Bernadette Sloyan, Australia]	Accepted. The title of the section has been changed to 'Atlantic Meridional Overturning Circulation'
7827	70	37	70	37	The statement that the SROCC has medium confidence in historical weakening seemingly contradicts the low confidence stated elsewhere in AR6 (ie P6 L26 and P71 L42 of chapter 2) [ Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, and revisions have been included accordingly, also due to other review comments.
30423	70	37			remove 'Conversely' (strictly, results are not opposed since the observational periods are not the same between AR5 & SROCC) [ Gilles Delaygue, France]	Accepted, and changed accordingly.
30029	70	39	70	40	the AMOC had weakened during instrumental era relative to 1850-1900. What is the instrumental era? 2004-2007? It is too short period. Please clarify. [ Yihui Ding, China]	Rejected. The first part of the sentence provides the information for the period, and the last part of the sentence specifies the importance of natural variability as the period is short.
73671	70	40	70	40	Change 'was' to 'were' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, thank you.
18129	70	46	70	55	A recent Nd isotope reconstruction suggests that the vertical and meridional structure of the Atlantic water mass distribution experienced only minor changes since the LGM ( <a href="https://doi.org/10.1130/G47628.1">https://doi.org/10.1130/G47628.1</a> ) [ Ersek Vasile, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The section does not address the water mass structure change of AMOC.
73673	70	48	70	48	References should be in chronological order [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and changed accordingly.
35553	70	48	70	48	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Accepted, and changed accordingly.
90415	70	48			reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	Accepted, and changed accordingly.
26655	70	50	70	50	Böhm et al. (2015, Nature) must be cited here. Böhm, E., Lippold, J., Gutjahr, M., Frank, M., Blaser, P., Antz, B., ... & Deininger, M. (2015). Strong and deep Atlantic meridional overturning circulation during the last glacial cycle. <i>Nature</i> , 517(7532), 73-76. [ Eric Brun, France]	Accepted, and reference added, thank you.
83445	70	50	70	50	I would be good to include here also a none US first authored reference like Böhm, E., Lippold, J., Gutjahr, M., Frank, M., Blaser, P., Antz, B., Fohlmeister, J., Frank, N., Andersen, M.B., Deininger, M., 2015. Strong and deep Atlantic meridional overturning circulation during the last glacial cycle. <i>Nature</i> 517, 73-76, doi: 10.1038/nature14059 [ Antje H. L. Voelker, Portugal]	Accepted, and reference added, thank you.
83447	70	53	70	53	Galaasen et al., 2014 could be updated to Galaasen, E.V., Ninnemann, U.S., Kessler, A., Irvani, N., Rosenthal, Y., Tjiputra, J., Bouttes, N., Roche, D.M., Kleiven, H.F., Hodell, D.A., 2020. Interglacial instability of North Atlantic Deep Water ventilation. <i>Science</i> 367, 1485, doi: 10.1126/science.aay6381. [ Antje H. L. Voelker, Portugal]	Accepted, and reference added, thank you.
80447	70	53	70	54	please also consider citing Galaasen et al., 2020 (Science) and Huang et al., 2020 (NComms) [ Samuel Jaccard, Switzerland]	Accepted, and reference added, thank you.
30425	70	54	71	3	It is not obvious to me why LSW formation and subpolar gyre strength are indicators of the AMOC and can be cited here. Also, I think records of 'centennial cold events' are not indicators of the AMOC but of climate and should not be cited here. [ Gilles Delaygue, France]	Rejected. Changes of LSW and SPG are well linked to AMOC change, see for example Thornalley et al 2018; Jackson et al. (2020) (both cited). Slight rewording is however proposed to clarify better. See also chapter 9, 9.2.3.1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5405	71	1	70	53	This is much too long given the low confidence in long term changes. [ Bryan Weare, United States of America]	Comment not clear as line numbers mis-match (from page 71 to 71..?)
93383	71	1	71	2	I believe the correct reference here is Moffa-Sánchez & Hall (2017). North Atlantic variability and its links to European climate over the last 3000 years. Nature Communications, 8, 9. <a href="https://doi.org/10.1038/s41467-017-01884-8">https://doi.org/10.1038/s41467-017-01884-8</a> [ Carles Pelejero, Spain]	Noted, and reference also added.
96231	71	5	71	14	There are observations since the 1940s in the interior of the Labrador Sea (surface to 2800m depth, see Yashayaev et al., 2016, J. Geophys. Res.Oceans, their Figure 4). The observations show large decadal variability of the deep convection in the Labrador Sea, which - in models - cause AMOC variability. However there is no long-term trend in these time series or it is obscured by the large (natural) decadal /multiannual variability. We suggest to add this here. This time series (Temp, Salinity, density 0-2800m depth, data quality controlled) located in a key region of the climate relevant ocean circulation (that might even be unique since it includes the deep ocean) is mostly ignored by climate modellers, and climate models with AMOC trends stick to compare model with observed SST variability. [ Nicole Wilke, Germany]	Taken into account. Additional text is added, and references, including the proposed one.
89869	71	6	71	7	In line with the previous comment, I suggest adding one reference: "Reconstructions based on instrumental observations suggest an overall weakening trend of AMOC through the 20th century superimposed with multidecadal variations (Ezer, 2013; McCarthy et al., 2015a; Rahmstorf et al., 2015; Caesar et al., 2018; Haustein et al., 2019)." [ Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. No clear indication which publication the reviewer is referring to, and the publication list online does not provide a paper on AMOC change based on reconstructions.
89307	71	10	71	14	<p>While it is clear that non-AMOC processes can of course influence SST and sea level, this is a bit like saying "natural factors can affect climate" and on this basis claiming we have "low confidence" in anthropogenic warming. It is too vague and simplistic an argument to make for IPCC. Regarding SST, what needs to be discussed is whether any non-AMOC change can explain the highly specific, centennial SST "fingerprint" (Caesar et al. 2018), with its geographically highly specific dipole pattern, which is found in the observational data and predicted by high-resolution models, as well as being physically well-understood. It is like with anthropogenic warming: we have the well-established dynamic theory, we have the models predicting this, we have a specific fingerprint, and (to my knowledge) we have no alternative explanation for this fingerprint pattern.</p> <p>The Josey et al. 2018 paper primarily discusses the short-term 2016 record warming hole as an atmosphere-driven phenomenon, but also notes that centennial trends are likely driven by ocean circulation. Also Josey et al. was published online in 2017, preceding the Caesar et al. fingerprint paper, and already for this reason does not provide any counter-argument whatsoever against the validity of the fingerprint as powerful evidence for a long-term AMOC decline.</p> <p>In addition we have all the proxy evidence consistent with a long-term large-scale AMOC decline discussed in my earlier comment to this chapter, which is completely ignored here. This section does not provide a balanced, impartial and thorough assessment of the evidence, but just ignores or dismisses it. One would have thought that if the AR6 now backtracks on the cited SROCC conclusion "that the AMOC had weakened during the instrumental era relative to 1850–1900 (medium confidence)" there would be good reasons, like some strong contrary evidence, but none is provided. [ Stefan Rahmstorf, Germany]</p>	Taken into account. Together with the comment for the ES, the text is now changed to 'medium confidence' for the AMOC slowdown. The text on SST-based uncertainties is slightly modified as well accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83451	71	14	71	16	There are newer references on the Agulhas leakage such as 1) Martinez-Mendez, G., Zahn, R., Hall, I.R., Peeters, F.J.C., Pena, L.D., Cacho, I., Negre, C., 2010. Contrasting multiproxy reconstructions of surface ocean hydrography in the Agulhas Corridor and implications for the Agulhas Leakage during the last 345,000 years. <i>Paleoceanography</i> 25, PA4227, doi: 10.1029/2009pa001879. 2) Marino, G., Zahn, R., Ziegler, M., Purcell, C., Knorr, G., Hall, I.R., Ziveri, P., Elderfield, H., 2013. Agulhas salt-leakage oscillations during abrupt climate changes of the Late Pleistocene. <i>Paleoceanography</i> 28, 599-606, doi: 10.1002/palo.20038. 3) Dyez, K.A., Zahn, R., Hall, I.R., 2014. Multicentennial Agulhas leakage variability and links to North Atlantic climate during the past 80,000 years. <i>Paleoceanography</i> 29, 1238-1248, doi: 10.1002/2014PA002698. [ Antje H. L. Voelker, Portugal]	Accepted. Thank you, references have been added. Note that this had been for page 72, not 71 as indicated.
83453	71	16	71	16	You could add a sentence regarding Gulf Stream variability based on Billups, K., Hudson, C., Kunz, H., Rew, I., 2016. Exploring Globorotalia truncatulinoides coiling ratios as a proxy for subtropical gyre dynamics in the northwestern Atlantic Ocean during late Pleistocene Ice Ages. <i>Paleoceanography</i> 31, 553-563, doi: 10.1002/2016PA002927. [ Antje H. L. Voelker, Portugal]	Taken into account, and the publication is considered for the assessment.
66473	71	16	71	18	A suggestion of studies to consider as a part of the Report: Increase in runoff due to decrease in refreezing of meltwater was estimated also for Svalbard, e.g. Østby et al. (2017) Van Pelt (2019). References: Østby, T. I., Schuler, T. V., Hagen, J. O., Hock, R., Kohler, J., and Reijmer, C. H. (2017). Diagnosing mass balance of glaciers in Svalbard. <i>The Cryosphere</i> , 11, 191–215. doi: 10.5194/tc-11-191-2017 Van Pelt, W. J. J., Pohjola, V., Pettersson, R., Marchenko, S., Kohler, J., Luks, B., et al. (2019). A long-term dataset of climatic mass balance, snow conditions, and runoff in Svalbard (1957-2018). <i>The Cryosphere</i> , 13, 2259–2280. doi: 10.5194/tc-13-2259-2019 [ Barbara Barzycka, Poland]	Not applicable: the comment appears to be linked to the wrong text.
127085	71	16	71	44	The trends in AMOC as seen through RAPID are questionable because of inhomogeneities in the record. Alternative reconstructions by Trenberth et al. (2017, 2019) do not show strong trends but rather are dominated by variability, especially from the NAO. Citations: Trenberth, K. E., and J. T. Fasullo, 2017: Atlantic meridional heat transports computed from balancing Earth's energy locally. <i>Geophys. Res. Lett.</i> , 44, 1919–1927, doi:10.1002/2016GL072475; Trenberth, K. E., Y. Zhang, J. T. Fasullo, and L. Cheng, 2019: Observation-Based Estimates of Global and Basin Ocean Meridional Heat Transport Time Series. <i>J. Climate</i> , 32, 4567-4583, <a href="https://doi.org/10.1175/JCLI-D-18-0872.1">https://doi.org/10.1175/JCLI-D-18-0872.1</a> [ Trigg Talley, United States of America]	Taken into account, and 2017 reference added.
127087	71	17	71	18	Given the large year-to-year variability and uncertainties, it seems odd to compare AMOC changes over 3-4 year time scales. What is the magnitude of the year-to-year variability? Also how large are the estimated differences compared to total AMOC transport in terms of percent change? [ Trigg Talley, United States of America]	Taken into account. According to other review comments, the text has been modified slightly, and percentage can be derived from given values in the sentence.
32895	71	17	71	18	should say "with multi-year averages from 2008-2012 and 2012-2017 being weaker than the period 2004-2008 by 2.9 Sv and 2.5 Sv respectively" (see Smeed et al., 2018) [ Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, thank you.
57973	71	18	71	20	If it is possible, I would like to see this data presented in a map showing point locations and graphs representing Sv trends. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Thank you for the comment, but due to space limitations no further figure can be added.
83449	71	18	71	20	You could include here also the 47°N evidence of "significant trend in the time period 1993–2018 of -0.60 Sv/year" from Rhein, M., Mertens, C., & Roessler, A. (2019). Observed transport decline at 47°N, western Atlantic. <i>Journal of Geophysical Research: Oceans</i> , 124. <a href="https://doi.org/10.1029/2019JC014993">https://doi.org/10.1029/2019JC014993</a> [ Antje H. L. Voelker, Portugal]	Accepted, and information has been added accordingly.
96233	71	26	71	26	Rhein et al., 2019 <i>J. Geophys. Ocean</i> quantified the circulation in the western subpolar NA at 47°N and showed that in the time period from 1993 - 2018 the interior circulation in the western subpolar basin declined significantly. This finding should be added here. [ Nicole Wilke, Germany]	Accepted, and a sentence has been added accordingly, thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66475	71	29	71	38	<p>A suggestion of studies to consider as a part of the Report: Another good example of glacier's retreat acceleration is a study of Błaszczyk et al. (2013) where fluctuations of glaciers' fronts terminating to Hornsund fiord (Svalbard) were analysed over 1899-2010 time span. One of the main findings is that - due to climate change - the retreat of glaciers in Hornsund in decade of 2001-2010 was three times higher than in first decades of the 20th century.</p> <p>The retreat of tidewater glaciers in Hornsund fiord continues and as a result, a new strait between Barents Sea and Greenland Sea is expected to be open in less than 45 years (Grabiec et al. 2018). The new strait will be a result of diminishing Hornbreen – Hambergbreen glacier system, which connects Sørkapp Land and Spitsbergen island and which bed is located below sea level. This will lead to changes in the ocean circulation patterns around Spitsbergen, weather and topoclimatic conditions, acceleration of tidewater glaciers' retreat and ecological shifts (Ziaja and Ostafin, 2014). Another interesting study from Svalbard archipelago focusing on glaciers retreat: Szafranec (2019) estimates the acceleration rate of retreat of land-based glaciers of southern and western Spitsbergen on 2.75 level (2000-2014 time span compared to 1936-2000).</p> <p>References:  Błaszczyk M., Jania J.A., Kolondra L. (2013). Fluctuations of tidewater glaciers in Hornsund Fjord (Southern Svalbard) since the beginning of the 20th century. Polish Polar Research, 34(4), 327-352, doi 10.2478/popore-2013-0024  Grabiec, M., Ignatiuk, D., Jania, J. A., Moskalik, M., Glowacki, P., Błaszczyk, M., Budzik, T. and Walczowski, W. (2018). Coast formation in an Arctic area due to glacier surge and retreat: The Hornbreen–Hambergbreen case from Spitsbergen. Earth Surface Processes and Landforms, 43, 387– 400. DOI: 10.1002/esp.4251  Szafranec, E. (2018) Deglaciation rate on southern and western Spitsbergen in the conditions of Arctic amplification. Polish Polar Research, 39(1), 77-98. DOI: 10.24425/118739  Ziaja, W., and Ostafin, K. (2014). Landscape-seascape dynamics in the isthmus between Sørkapp Land and the rest of Spitsbergen: will a new big Arctic Island form? AMBIO. doi: 10.1007/s13280-014- 0572-1. [ Barbara Barzycka, Poland]</p>	Not applicable: the comment appears to be linked to the wrong text.
57975	71	33	71	33	Instead of saying 'recently', state the year in which deep convection returned to the Labrador Sea i.e. 2012. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account, and according to the publication, the year 2015 has been pointed out as particular event, thank you.
96235	71	34	71	34	The review from Rhein et al., 2017 ( Phil Trans R Soc doi: 10.1098/rsta.2016.0321) should be added. The authors showed that after the onset of the deep convection, subpolar North Atlantic basin wide changes occur following the spreading pathways of LSW. [ Nicole Wilke, Germany]	Accepted, and reference is added, thank you.
102745	71	37	71	37	"overturing" should be "overturning" [ Philippe Tulkens, Belgium]	Accepted, thank you.
102747	71	38	71	38	"Antartic" should be "Antarctic" [ Philippe Tulkens, Belgium]	Accepted, thank you.
70259	71	38	71	40	Confidence statement is inserted with no presentation of evidence and comes with a referral to chapter 9. I believe that at least some information should be presented here about observed changes in the SO. If no new evidence is available maybe it should be stated here? [ Shayne McGregor, Australia]	Rejected. Due to space limitations, an agreement with chapter 9 is found to assure that information are available and cross-linked.
30427	71	38			'Antarctic' [ Gilles Delaygue, France]	Accepted, thank you.
80449	71	41	71	42	calibrated langage should be consistent with CH09 (e.g. p. 9-28/29) [ Samuel Jaccard, Switzerland]	Accepted, and revised accordingly.
32897	71	42	71	42	should say "direct observations show a weakening of the AMOC beginning around 2004" OR "around 2004-2008" [ Meric Srokosz, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and changed accordingly.
37679	71	42	71	46	No AMOC time series shown, which is understandable because of low confidence for longer and shortage for modern periods, but in light of the important projection message of weakening AMOC, one might want to see how the recent tendency of AMOC is represented with available data (and, perhaps, by models/reanalyses). [ Masahide Kimoto, Japan]	Rejected. CH2 is considering observed changes only, not projections. This assessment is performed in chapter 9.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70261	71	46	71	48	Confidence statement is inserted with no presentation of evidence and comes with a referral to chapter 9. I believe that at least some information should be presented here about observed changes in the SO. [ Shayne McGregor, Australia]	Rejected. This is part of cross-chapter coordination and agreed to not include repetition and support limited space constraints. However, SO MOC changes is now excluded from the summary paragraph.
57977	71	47	71	48	The Antarctic Bottom Water is not really discussed in this section so suggest remove. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The SO MOC info is now excluded from the summary paragraph.
116007	71		71		Coordination with ch 9 is needed on the FAQ related to AMOC (called "Gulf Stream"). [ Valerie Masson-Delmotte, France]	Taken into account, and both the chapter 2 assessment, and chapter 9 assessments went through major revisions due to also other review comments, and cross-chapter exchange have been established.
57747	72	1	72	48	This section is about changes in the poleward main currents and the circumpolar current. Thus, I believe there the Atlantic Water inflow in the Arctic should be mentioned. Atlantic Water (AW) is the main heat and salt source of the Arctic Ocean and plays a relevant role on keeping ice free part of the Nansen basin (e.g. Onarheim et al., 2014) and Barents Sea (e.g. Århus et al. 2019). The SROCC reports how the AW layer heat content continues to increase based on the observations from Polyakov et al. (2017). The main entrance of AW into the Arctic is Fram Strait. On the long time series observations at Fram Strait, Beszczynska-Möller et al. (2012) found increasing AW temperatures from 1997 to 2010 with a rate of 0.06°C per year, while Casanova-Masjoan et al. (2020 JGR-Oceans under review) found a lower trend in the AW diversion around Iceland (0.03°C per year) from observations between 1993 and 2017. The latest publication using the Fram Strait long-time series observations reports an AW warming of about 0.05°C per year (von Appen, 2019), and these latest results are supported with model outputs (Wang et al., 2020). Reconstructions of the August sea surface temperatures in a Svalbard fiord also revealed an increasing trend over 60 years (1952-2014; Guruvayoorappan et al. (2019)). Similarly, at the Barents Sea, Muilwijk et al (2018) reported that temperature has increased 1°C in the 20th Century while Lind and Ingvaldsen (2012) also observed a positive trend in temperatures over 40 years. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. This section also now includes an assessment of interbasin exchange, including also the exchanges Pacific / Arctic; Atlantic / Arctic.
13265	72	2	72	2	WBC must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Accepted, thank you .
70263	72	2	72	48	It seems odd that ACC is specifically mentioned at several stages and AR5 and SROCC assessments are presented, but no new information is presented, outside of an assesment summary from CH9. If this is to remian here, I believe some information should be presented? [ Shayne McGregor, Australia]	Taken into account. The text has been revised, and assessment from chapter 9 in the chapter 2 summary have been removed.
26657	72	12	72	12	Yet, there is clear evidence for a Kuroshio weakening that lasted several centuries to millenia during the mid-Holocene (Jian, Z., P. Wang, Y. Saito, J. Wang, U. Pflaumann, T. Oba & X. Cheng 2000 "Holocene variability of the Kuroshio Current in the Okinawa Trough, northwestern Pacific Ocean," Earth and Planetary Science Letters 184, pp. 305-319.) [ Eric Brun, France]	Taken into account. The sentence has changed from 'intensified' to 'varied' to refer to the evidence from the cited publications.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79873	72	12	72	41	<p>Comments on section '2.3.3.4.2 Western boundary currents (WBCs) and the Antarctic Circumpolar Current (ACC)'</p> <p>I suggest to rewrite the lines between 12-41. These three paragraphs can be sorted into two parts, i.e., the position changes of ocean circulations, the strength changes of ocean circulations.</p> <p>1. Satellite observations, climate simulations, and paleo-proxies all indicate a poleward shift of the ocean circulation pattern in a warmer climate. But the results from recent observations are not statistically significant due to strong nature variability and short temporal coverage of data.</p> <p>2. Based on SST, heat flux, and climate model simulations, it is proposed that the WBCs (except the Gulf Stream) are getting stronger under global warming. But in-suit observations, covering more than two decades, mostly do not support an intensification.</p> <p>Here is a preliminary draft:</p> <p>Satellite observations of SSH and SST indicate a consistent poleward shift of the large-scale ocean gyres in the past four decades (Yang et al. 2020), combining with a systematic poleward migration of WBCs and ACC (Frankignoul et al. 2001, Ridgway, 2007, Goni et al., 2011, Sokolov &amp; Rintoul, 2009, Wu et al. 2012, Yang et al 2016, Bisagni et al., 2017, Yang et al. 2020). The shift in ocean circulation shows dynamic consistency with the observational shift in the pattern of atmospheric circulation (Chen et al., 2008, Archer &amp; Caldeira, 2008, Yin, 2005, Norris et al., 2016, Scheff &amp; Frierson, 2012). Due to short temporal coverage and strong natural climate variability, these observed shifts are mostly not statistically significant, especially over the Northern Hemisphere. However, climate simulations (Saenko et al., 2005, Yang et al. 2020) and paleo proxies (Peeters et al. 2004, Bard &amp; Rickaby, 2009, Mokeddem, et al. 2014, Gallagher et al. 2015, Gray et al. 2020) suggest that the wind-driven ocean circulation pattern is very likely to shift towards higher latitudes in response to a warmer climate.</p>	Taken into account. Thank you for the information, and the text draft. Although the entire proposed revised text is not included, the text has been revised, and the results of the indirect study are considered now. Accordingly, the assessment has been changed to 'medium confidence'.
73675	72	14	72	14	Capital 'S' for 'south'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable anymore because text revised.
409	72	14	72	15	Do you mean the Agulhas leakage in the south Atlantic? Or the Agulhas Current in the SW Indian Ocean? [ Leticia Cotrim da Cunha, Brazil]	Taken into account, and the text has been modified for further clarification (in the South Atlantic removed).
26659	72	16	72	16	Recent references missing: Caley, T., F. J. C. Peeters, A. Biastoch, L. Rossignol, E. van Sebille, J. V. Durgadoo, B. Malaizé, J. Giraudeau, K. Arthur & R. Zahn (2014) Quantitative estimate of the paleo-Agulhas leakage. Geophysical Research Letters, 41. Caley, T., J. Giraudeau, B. Malaizé, L. Rossignol & C. Pierre (2012) Agulhas leakage as a key process in the modes of Quaternary climate changes. Proceedings of the National Academy of Sciences, 109, 6835-6839. [ Eric Brun, France]	Accepted. Thank you, and information and reference have been added accordingly.
57679	72	16	72	16	Recent study by Ballalai et al. (2019) may be worth citing here, as it presents evidence for Agulhas Leakage strengthening during early last interglacial, coeval with the resumption of convection in the Labrador and Nordic seas. Ballalai, J. M., Santos, T. P., Lessa, D. O., Venancio, I. M., Chiessi, C. M., Johnstone, H. J., ... & Albuquerque, A. L. S. (2019). Tracking spread of the Agulhas Leakage into the western South Atlantic and its northward transmission during the Last Interglacial. Paleoceanography and Paleoclimatology. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, and reference added, thank you.
102749	72	16	72	16	"exception" is repeated [ Philippe Tulkens, Belgium]	Accepted, thank you.
127089	72	16	72	16	The term "exception" is repeated. [ Trigg Talley, United States of America]	Accepted, thank you.
29869	72	16	72	16	Typo. [ Hernan Edgardo Sala, Argentina]	Editorial. Not clear, I assume the repeated exception.
69813	72	16	72	16	Delete one of the two "exception". [ Kaoru Kubota, Japan]	Accepted, thank you.
67767	72	16	72	16	Remove the second "exception" [ Magdalena Andres, United States of America]	Accepted, thank you.
9943	72	16	72	16	two times repeats "exception" [ Olga Zolina, France]	Accepted, thank you.
83947	72	16	72	16	exception is written twice [ Marco Tulio Cabral, Brazil]	Accepted, thank you.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23279	72	16	72	16	Delete "exception". [ Toshio Suga, Japan]	Accepted, thank you.
90417	72	16			exception repeted twice [ Jeannine-Marie St-Jacques, Canada]	Accepted, and removed, thank you.
41563	72	16			With the exception exception ... repetition [ Laurent Labeyrie, France]	Accepted, thank you.
30429	72	16			'exception exception' [ Gilles Delaygue, France]	Accepted, thank you.
30431	72	16			'exception of one location' > 'exception of one record'? [ Gilles Delaygue, France]	Accepted, thank you.
30435	72	20	72	21	'was much stronger on millennial time scales during the last interglacial and glacial intervals': not clear here whether it was stronger during the whole last interglacial, or during 'intervals' of the last interglacial. Idem for 'glacial intervals': does it refer to DO style events, or to the whole glacial period? [ Gilles Delaygue, France]	Taken into account, and text reads now also abrupt and rapid change.
30433	72	20			'was much stronger': than what? not clear here [ Gilles Delaygue, France]	Taken into account, and text reads now also abrupt and rapid change.
23867	72	24	72	25	Redundancy in '...based on SST or air-sea flux observations...' [ Branko Grisogono, Croatia]	Accepted, and second repetition removed, thank you.
57979	72	24	72	25	Why are the WBCs moving polewards? Is it due to processes at depth or near the surface, and what processes are driving the changes? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. Chapter 2 is not assessing the process of change, this is chapter 9.
29871	72	24	72	25	Please revise wording. [ Hernan Edgardo Sala, Argentina]	Accepted, and repetition removed, thank you.
67769	72	24	72	25	Maybe this statement about assessments of the poleward shift in the WBCs and their extensions should also refer to the more recent work (based on SST and altimetry) by Yang et al., 2020: Yang, H., Lohmann, G., Krebs-Kanzow, U., Ionita, M., Shi, X., Sidorenko, D., et al. (2020). Poleward shift of the major ocean gyres detected in a warming climate. Geophysical Research Letters, 47, e2019GL085868. <a href="https://doi.org/10.1029/2019GL085868">https://doi.org/10.1029/2019GL085868</a> (which appears to be a follow on to the 2016 paper on subtropical WBCs already referenced). [ Magdalena Andres, United States of America]	Accepted. The reference has been now included, thank you.
30437	72	25			remove 'based on SST or air-sea flux observations' (twice) [ Gilles Delaygue, France]	Accepted, thank you.
73677	72	27	72	27	Insert date for Collins reference [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and year added, thank you.
67771	72	28	72	28	Andres 2016 focuses on Gulf Stream position rather than transport. A reference that reports on long-term changes in Gulf Stream transport based on in situ measurements would be: Andres, M., K. Donohue, J. Toole, 2020. The Gulf Stream's Path and Time-averaged Velocity Structure and Transport at 68.5°W and 70.3°W, Deep Sea Research, 156, <a href="https://doi.org/10.1016/j.dsr.2019.103179">https://doi.org/10.1016/j.dsr.2019.103179</a> . [ Magdalena Andres, United States of America]	Accepted, and added, thank you.
73679	72	28	72	28	Insert date for Collins reference [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and done, thank you.
102751	72	29	72	29	"Currents" should be spelled with "c" [ Philippe Tulkens, Belgium]	Accepted, and done, thank you.
30439	72	30			'Consistent poleward migration of WBCs has also not been noted': this contradicts L.24, unless 'Previous assessments' L24 are AR5 (and SROCC?). Needs to be clarified. [ Gilles Delaygue, France]	Accepted, and removed.
6557	72	32	72	32	Some words are missing. Should "short direct observations" instead be "the short duration of the period of direct observations" or something similar? [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and changed accordingly, thank you.
29873	72	32	72	33	Evaluate adding "the detection of" in the following way: "...obscures the detection of any long-term changes". [ Hernan Edgardo Sala, Argentina]	Accepted, thank you, done.
90419	72	32			air sea-sea flux [ Jeannine-Marie St-Jacques, Canada]	Accepted. Wrong line / page number, but found later, and done, thank you.
90421	72	32			air-sea flux? [ Jeannine-Marie St-Jacques, Canada]	Accepted. Wrong line / page number, but found later, and done, thank you.
30441	72	33			'obscure' [ Gilles Delaygue, France]	Noted. Intent of comment not clear.
98665	72	35	72	35	Change "joint" to "link" [ Sonya Legg, United States of America]	Taken into account, and sentence changed (... Central for global climate...)
127091	72	35	72	35	The term "central joint" is unfamiliar and unnecessary. Delete "...is a central joint for global climate, and it...". [ Trigg Talley, United States of America]	Taken into account, and sentence changed (... Central for global climate...)



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57981	72	35	72	41	I find that this paragraph does not add a great deal to the description of changes. I would suggest shortening it and including with the previous section. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This paragraph is providing the linkage to the more detailed assessment provided in chapter 9 as part of cross-chapter agreements.
5407	72	35			"joint" should be something like "tenant" [ Bryan Weare, United States of America]	Taken into account, and sentence changed (.. Central for global climate..)
73681	72	37	72	37	Not clear what is meant by 'sea-sea' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This was an editorial error, and has been corrected.
102753	72	40	72	40	"in" should be "of"? [ Philippe Tulkens, Belgium]	Accepted.
30443	72	40			'no evidence in ACC transport change': in recent decades, or on which time scale? [ Gilles Delaygue, France]	Accepted, and 'in recent decades' added.
5409	72	43	72	44	There are no citations or plots which support this conclusion [ Bryan Weare, United States of America]	Rejected. There is no plot, but 2 paragraphs of text with many references above.
69171	72	43	72	48	Suggest inserting "In summary" at the top of the paragraph, like the other sections in 2.3.3.? Or how about make "2.3.3.4.3 Overall assessment of ocean circulation" like 2.3.1, combining P71L42-P71L53 with this paragraph. [ Kaoru Magosaki, Japan]	Accepted, and added, thank you.
102755	72	43	72	48	It is not entirely clear whether the statements in this section summarizes the abovementioned result (as in all other sections) or introduce new findings (in which case references are missing) [ Philippe Tulkens, Belgium]	Noted. As for all section, this is the summary paragraph of the above information, which is referenced.
98667	72	43	72	48	This paragraph contains some information about the WBC changes, which should go into the earlier paragraph on WBCs (ending p72 line 33). Then the 2nd sentence of this paragraph, on the ACC repeats information given in the previous paragraph, and should be deleted. [ Sonya Legg, United States of America]	Taken into account. The text has been revised, and the ACC repetition has been removed.
69071	72	45	72	45	Add to this sentence, " .... Of WBCs hinders the detection of changes and LACK OF OBSRVATIONAL TIMESERIES OF APPROPRIATE LENGTH." [ Bernadette Sloyan, Australia]	Accepted, and changed accordingly (slightly different wording, but same information).
30445	72	45			'detection of long term changes' (changes are detected at decadal time scale) [ Gilles Delaygue, France]	Accepted.
57627	72	46	72	48	The last sentence of this paragraph would benefit from some clarification of if this is a well agreed with statement or not [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, and confidence statement has been added.
30447	72	47	72	48	This meaning of this sentence is not clear to me, given the very large changes in some circulation systems (esp. N. Atl.) seen in archives. Also, what means 'variability in the mean circulation state': variability is defined as the departures around the mean. [ Gilles Delaygue, France]	Taken into account, and text has been revised accordingly.
127093	72	53	74	3	It is not clear why several paragraphs are devoted to pH but there is no mention of aragonite saturation, which is much more biologically relevant (and is not closely tied to pH at the geologic time scales discussed here). [ Trigg Talley, United States of America]	Rejected. This section is focussed on observed pH change only. Drivers and particularly aragonite saturation are assessed in chapter 5.
54461	72	55	74	3	2.3.4 Biosphere 2.3.4.1 Ocean pH. Suggestion: Try to mention "foraminifera" in the main text (as it was included in the Figure based on boron isotopes). Otherwise the paragraph/section seems to be more related with chemistry/biochemistry than with "biosphere". [ Maria del Pilar Bueno Rubial, Argentina]	Taken into account. The section is now included in the ocean section. For the comment on foraminifera: not precise, and thus Rejected .
112931	72		72		not to quibble, but ocean pH and oxygen are not biosphere indicators per se, but rather chemical and physical indicators, respectively. They have biospheric implications, but then again, so does temp. rainfall, etc. Maybe consider the name of this subsection or move to "ocean" and reserve this section for truly biospheric indicators like coral reef bleaching or the like. WRT the latter, Terry Hughes and Mark Eakin have recently compiled and published some timeseries of coral bleaching that are global in nature. Perhaps they can be referenced here, if not visualized somewhere in Figure 2.29? [ Kim Cobb, United States of America]	Taken into account, and the section has been moved to 'ocean'.
116009	72		72		please explain better the cause for different conclusion / AR5 and SROCC : literature not considered in earlier assessments? Role of multi decadal variations better characterized compared to trends from short records? [ Valerie Masson-Delmotte, France]	Taken into account. Due to several review comments and further evidence the confidence level has now changed and is not different anymore, so no further adjustments / explanations needed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
411	73	1	73	5	There could also be a cross chapter reference to chapter 5, especially section 5.2 [ Leticia Cotrim da Cunha, Brazil]	Accepted, and a link to chapter 5 has been added in this subsection.
18035	73	3	73	3	Why is the entire discussion of pH about the surface ocean? Are there no records of change at the sea floor or in deep water (which is most of the ocean)? [ Lisa Levin, United States of America]	Taken into account, and information added accordingly.
57629	73	4	73	5	Clarification regarding sentence meaning - if it is meant to imply that the decline in surface ocean pH assessed by SROCC has already breached/overexceeded the realm of natural variability for 95% of the global surface open ocean, then the sentence formation isn't helpful because of the use of the word 'emerged'. Emerged makes it seem like the decline in pH can be attributed to natural variability. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, and the wording 'emerged' has been changed to 'exceed'
21243	73	7	73	16	either mark the PETM era in Fig. 2.28A by shading (maybe at a different resolution as inset), or bestter in a new Figure, cpmparing contemporary pH decrease with PETM at same time scale. [ Michael Schmitt, Germany]	Taken into account, and PETM era highlighted in Fig. 28a.
57983	73	8	73	8	Recently there has been a paper published where the pH is mentioned directly for the paleo time scales: Harper et al. 2019: 'The Magnitude of Surface Ocean Acidification and Carbon Release During Eocene Thermal Maximum 2 (ETM-2) and the Paleocene-Eocene Thermal Maximum (PETM)'. That could be a good alternative for one of the already mentioned papers. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, and text and reference added, thank you.
6559	73	9	73	9	"transiently" can probably be removed; it seems superfluous. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and removed.
73683	73	9	73	9	Move 'transiently' to after 'fluctuated'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable anymore. Due to other review comments, the wording 'transiently' has been removed.
21187	73	10	73	16	The idea that large volcanic CO2 emissions triggered or set the stage for the PETM is gaining support in recent years. However, there is no general acceptance of this yet, so claiming "...the PETM, which involved a large pulse of volcanic CO2 released into the ocean-atmosphere system within 5-20 kyr (Kirtland Turner, 2018)" is too bold a statement. "may have" is currently more realistic. See also chapter 5 p. 44 on the PETM and FAQ 9.1 [ Robert Speijer, Belgium]	Accepted, and changed accordingly.
69815	73	11	73	11	Is there no methane source carbon input? How confident is this statement? [ Kaoru Kubota, Japan]	Taken into account, and text modified to reflect total carbon release.
2017	73	11	73	11	There is still debate as to the source of the CO2 at the PETM, so maybe just "CO2" rather than "volcanic CO2". [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and changed accordingly.
81013	73	12	73	12	The PETM perturbations of the ocean-system had substantial impact on the ocean biosphere as a result of warming, deoxygenation and pH decrease. This should be mentioned here. [ Jeffrey Philip OBBARD, Singapore]	Rejected. This chapter only assesses the changes, not their impacts.
73685	73	13	73	13	remove , before 'and'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and removed.
57985	73	15	73	16	In order to point out the difference between today and i.e. the PETM I would suggest to mention that the cumulative carbon mass may have been similar to today, however, the rate of pH change was different e.g. 'Despite similar cumulative mass of carbon between the PETM and today, the pH decrease was likely on order of magnitude slower than currently (Penman et al., 2014; Zeebe et al., 2016).' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, and changed accordingly.
96237	73	21	73	21	Table 2.28 there are two abbreviations for the same unit (Ma or Myr) in the text, please use only one. Since 'Myr' as well 'Ma' are not very often used units it may be worth to write the full name 'million years' instead of an abbreviation. [ Nicole Wilke, Germany]	Accepted.
43123	73	22		25	Read "band (Hönisch et al., 2009; Seki et al., 2010; Bartoli et al., 2011; Foster et al., 2012; Badger et al., 2013; Greenop et al., 2014; Martínez-Botí et al., 2015c; Anagnostou et al., 2016b; Chalk et al., 2017; Gutjahr et al., 2017; Sosdian et al., 2018)." rather than "band. Data include: (Hönisch et al., 2009; Seki et al., 2010; Martínez-Botí et al., 2015c; Anagnostou et al., 2016b; Chalk et al., 2017; Gutjahr et al., 2017; Sosdian et al., 2018) (Bartoli et al., 2011; Foster et al., 2012; Badger et al., 2013; Greenop et al., 2014)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Rejected. The current version clearly links to the data use, and with the proposed modification this would become unclear.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73687	73	23	73	29	References should be in chronological order. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
69817	73	24	73	24	Delete parenthesis between Sosdian et al., 2018 and Bartoli et al., 2011. [ Kaoru Kubota, Japan]	Accepted.
13267	73	24	73	24	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Accepted.
73689	73	28	73	28	Change in-situ to in situ. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
73691	73	29	73	29	Edit refrec to Keelign et al. (1976). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected, Keeling is correct.
13269	73	30	73	30	add a space after : [ Maria Amparo Martinez Arroyo, Mexico]	Accepted.
30449	73	31			I think it necessary here to clarify the difference between direct observations of pH (BATS, HOT?) and estimates based on CO2 concentration (GLOBAL product). [ Gilles Delaygue, France]	Accepted, and information has been added accordingly.
13271	73	34	73	34	Missing or extra () [ Maria Amparo Martinez Arroyo, Mexico]	Not applicable. Comment not clear, line 34 is an empty line. Comment appears misplaced.
100613	73	38	73	38	Note: I would restructure this paragraph to talk about magnitudes of changes first, then rates of change second. [ Matthew Kohn, United States of America]	Rejected. The current version already builds upon a similar organisation.
525	73	38	73	40	To state that "Surface ocean pH has gradually increased over the last 50 Myr" and then immediately follow it by "The available evidence suggests surface pH values as low as today have not been experienced in the last 2 million years" is jarring. It would be FAR better to appropriately transition between the increases suggested by the first sentence and the decreases suggested by the second sentence. This could be done by expanding the start of the second sentence from "The available evidence suggests" to "However, it has been decreasing in recent decades, and the available evidence suggests ...". [ Claire Parkinson, United States of America]	Taken into account, and the text now includes the wording 'however' for clarification, thank you.
93387	73	39	73	39	It should be made clear that this relates to global-mean surface pH. [ Carles Pelejero, Spain]	Accepted, and changed accordingly.
95907	73	39	73	48	On line 39: "today" is not informative term in this context given referencing with respect to last 2 million years. Use more relevant term e.g. current century should include ending year, e.g. ending 1900?. Notably, the recent decades are implied in the text starting Line 48. It is very useful to indicate explicitly some of these past decades e.g. 2009 -2018 or whichever are valid according to the sources of the evidence. [ Joseph Mutemi, Kenya]	Accepted, and changed accordingly.
9945	73	40	73	41	double brackets [ Olga Zolina, France]	Accepted.
13273	73	40	73	41	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Accepted.
99227	73	40			the cited papers would not conclude that the CO2 was not higher in the last 2 Ma but 2.7. This is important as impacts on other climate parts link to this. Figure 2.28 does not convey that message. Maybe running average might help to highlight the information needed to support the statement? [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, and text has been revised accordingly. However, a running average has not been applied to the figure because it does not address the issue at hand.
100615	73	41	73	41	Add: "and that possible levels by the end of the 21st century (c. 7.75) have not occurred for 16 Myr (Sosdian et al., 2018)." [ Matthew Kohn, United States of America]	Rejected because chapter 2 does not include projected change, only observed change.
29875	73	41	73	41	There is an unbalanced parenthesis and a missing space in "2018b)Cross-Chapter Box 2.4)." [ Hernan Edgardo Sala, Argentina]	Accepted.
73693	73	41	73	42	Insert space after ). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
90423	73	41			missing a ( [ Jeannine-Marie St-Jacques, Canada]	Accepted.
83949	73	42	73	43	Given that the first part of the phrase deals with rates, the brackets at the just befor the comma at: "pH changes over recent decades (0.12-0.16 pH units)" should mention a rate pH units ky-1, rather than just stating the pH units of change, or this information should be placed at the end of this phrase just before the references. [ Marco Tulio Cabral, Brazil]	Take into account, and change has been added.
100617	73	43	73	43	Add: "Middle Miocene variations were 1.5 to 2 times larger than observed in the Pleistocene, but occurred 10 or more times slower (Sosdian et al., 2018)" [ Matthew Kohn, United States of America]	Rejected. We only focus on those changes of magnitude relevant to recent changes, provided there is sufficient dating constraints to allow precise determinations of rate of change.
57989	73	43	73	44	The Hönlisch et al, 2009 reference should be within the brackets: (Hönlisch et al., 2009; Chalk et al., 2017; Shao et al., 2019). Proabaly an error in the citation manager. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted.
13275	73	43	73	44	references can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73695	73	43	73	44	Edit format of references and list chronologically [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
43125	73	43		44	Read "(Chalk et al., 2017; Shao et al., 2019; Hoenisch et al., 2009)." rather than "(Chalk et al., 2017; Shao et al., 2019) (Hoenisch et al., 2009)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
73697	73	48	73	48	Quantify or define 'several decades'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and information has been added accordingly.
69819	73	48	74	3	Better to show that there is OA hotspot, e.g., California Current Upwelling System? Rate of pH decrease differs regionally (Bates et al., 2014, Oceanography). [ Kaoru Kubota, Japan]	Rejected. Chapter 2 is focussed on global scale change, and regional change (e.g. hotspots) are tackled in chapter 5 / 9.
43127	73	48		49	Read "(IPCC, 2019b; Lauvset et al., 2015; Hurd et al., 2018)." rather than "(IPCC, 2019b) (Lauvset et al., 2015; Hurd et al., 2018)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Accepted.
18091	73	48			The first sentence of the paragraph talks about declining pH, while the second one goes into CO2 flux variability. While the two are indeed linked, it would be good to have an explicit sentence linking the effects of one on the other. [ Vlad Macovei, Germany]	Taken into account, and the text has been revised for improved clarity.
13277	73	49	73	49	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Accepted.
73699	73	49	73	49	Remove )( and replace with ; List references chronologically. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
83455	73	49	73	49	There is a study for the North Atlantic by Perez, F.F., Fontela, M., García-Ibáñez, M.I., Mercier, H., Velo, A., Lherminier, P., Zunino, P., de la Paz, M., Alonso-Pérez, F., Guallart, E.F., Padin, X.A., 2018. Meridional overturning circulation conveys fast acidification to the deep Atlantic Ocean. Nature 554, 515, doi: 10.1038/nature25493. that you could include here, even if more locally. I saw later that this study is cited in Chapter 5; so it could be skipped here in Chapter 2. [ Antje H. L. Voelker, Portugal]	Accepted. The reference is now cited, also in reply to another review comment to provide also information on the subsurface ocean.
93389	74	3	74	3	It is important to add "global-mean" before "surface pH", since locally and seasonally surface pH experiences large variability [ Carles Pelejero, Spain]	Accepted, and added at several places in the summary statement.
30451	74	6	74	39	'deoxygenation': this term must be explained here because its literal sense is far from obvious. Does it refer to an expansion in the extent of OMZ, or to the surface decrease in O2 saturation? Amplitudes are too contrasted to be designed with the common expression of 'deoxygenation'. [ Gilles Delaygue, France]	Rejected. This term is defined in the glossary, and will not be defined here in chapter 2.
10241	74	8	74	39	This section should take into account the IUCN report on deoxygenation of the ocean presented at the UNFCCC COP 25 - 'Ocean deoxygenation: Everyone's problem. Causes, impacts, consequences and solutions' Edited by D. Laffoley and J.M. Baxter. <a href="https://www.iucn.org/theme/marine-and-polar/our-work/climate-change-and-oceans/ocean-deoxygenation">https://www.iucn.org/theme/marine-and-polar/our-work/climate-change-and-oceans/ocean-deoxygenation</a> . [ Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and report is considered in the assessment.
41565	74	8	74	39	Why coastal waters anoxia events development are not evoked here? [ Laurent Labeyrie, France]	Taken into account. Although coastal deoxygenation is tackled in chapter 5, some information is also provided now in chapter 2 with a global/large scale perspective, which is the task of chapter 2.
18031	74	8	74	39	There does not appear to be an update since AR5 other than inclusion of paleo evidence. Even for paleo discussions the emphasis on anoxia seems misplaced since there can be major consequences of different levels of oxygen loss. What do modern studies say about mechanisms causing change beyond decadal oscillations? [ Lisa Levin, United States of America]	Accepted, and the section has been revised accordingly, including further assessment of available information on the recent changes.
3895	74	9	#REF!	10	"Chlorophyll-a, the major photosynthetic pigment contained in phytoplankton, is a commonly-used measure of phytoplankton concentration", well, not really...it is the concentration of Chl-a that is a proxy of phytoplankton biomass. At the very least, it cannot be the pigment that is a proxy, but some quantifiable feature of the pigment. [ David Schoeman, Australia]	Comment misplaced but taken into account in revisions to marine biosphere section.
3897	74	10	#REF!	11	This sentence comprising three clauses separated by semi-colons does not read at all well. [ David Schoeman, Australia]	Taken into account, and the sentence has been slightly modified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30453	74	15			I do not think that 'changes in deoxygenation' has any sense > 'changes in oxygen content' [ Gilles Delaygue, France]	Accepted and changed accordingly.
99229	74	16	74	17	The oxygen reduction across the PETM is still a topic of much research. Widespread deoxygenation is a strong statement, recent comprehensive model data synthesis suggest regional evidence for deoxygenation which given the patchy data is a fair assessment of the current knowledge Carmichael MJ, Inglis GN, Badger MPS, Naafs BDA, Behrooz L, Rimmelzwaal S, et al. Hydrological and associated biogeochemical consequences of rapid global warming during the Paleocene-Eocene Thermal Maximum. Global and Planetary Change. 2017;157:114-38. [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The sentence has been amended. Sulphur isotope data support a 10-20% increase in ocean anoxia compared to today. "Widespread" has been replaced by "large-scale".
99231	74	18	74	22	There is a misbalance in evidence and strength of statement here. There is little data for long time intervals in the Mesozoic and yes some of these have had anoxia. The Cenozoic is a relatively shorter time interval and the confidence displayed is less. As such I consider it important that they consider the strength of statement for the Mesozoic. Furthermore, it is important to stress in this context that ocean circulation and geography was different and as such this is not a strong comparisons as the drivers and the response were different as well. This raises the question how relevant the information of earlier time intervals was as it is also not covered in the own summary 2.3 of the section [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Fair comment. Our intention was to show that large-scale deoxygenation occurred during warm intervals of the past with consequences for the carbon cycle and marine ecosystems.
93391	74	19	74	21	Stramma and Schmidt, 2019 (not in the list, and I don't know of a paper by only these two authors in 2019) is not a correct reference for the assertion. Perhaps one that wraps Permian and Cretaceous could be: Song, H., Wignall, P., Song, H., Dai, X., & Chu, D. (2019). Seawater Temperature and Dissolved Oxygen over the Past 500 Million Years. Journal of Earth Science, 30. <a href="https://doi.org/10.1007/s12583-018-1002-2">https://doi.org/10.1007/s12583-018-1002-2</a> [ Carles Pelejero, Spain]	Taken into account. The reference is added in the list, and the other proposed reference is considered for the assessment now.
73701	74	20	74	20	Insert 'the' before 'Cretaceous'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted and changed accordingly.
80451	74	21	74	21	please double-check reference [ Samuel Jaccard, Switzerland]	Accepted, yes there was an error, the ref is from the IUCN report.
21189	74	21	74	22	"Although the Cenozoic Era is poorly documented..." is a very odd statement. The Cenozoic record is the best and most studied of all geological records. Perhaps it meant to state that global anoxia are not known from the Cenozoic? That would be correct. The paper by Wang et al. (2019 - unknown to me) stating this is not listed. There is another Wang et al. (2019) paper on ozone listed, though. [ Robert Speijer, Belgium]	Accepted, and sentence has been revised accordingly.
57991	74	22	74	22	Wrong citation source. The Wang et al., 2019: 'Twenty-Five Years of Lower Tropospheric Ozone Observations in Tropical East Asia: The Influence of Emissions and Weather Patterns' listed in the the reference section is wrong. Instead it should be Wang et al., 2016: 'A Cenozoic seawater redox record derived from 238U/235U in ferromanganese crusts'. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The ref is Wang 2019, but not provided correctly in the list, it is from the IUCN report, corrected now.
93395	74	22	74	22	Wang et al., 2019, not in the list. [ Carles Pelejero, Spain]	Accepted, and paper now added.
23467	74	22	74	25	There are two recent review papers on this (same conclusion)t: Anderson et al 2019, Global Biogeochem Cycles, <a href="https://doi.org/10.1029/2018GB006049">https://doi.org/10.1029/2018GB006049</a> ; Jacobel et al 2020, Quaternary Science Reviews, <a href="https://doi.org/10.1016/j.quascirev.2019.106065">https://doi.org/10.1016/j.quascirev.2019.106065</a> [ Jean Lynch-Stieglitz, United States of America]	Taken into account, and the papers are now included in the assessment.
83457	74	25	74	25	Add as additional reference: Tetard, M., Licari, L., Beaufort, L., 2017. Oxygen history off Baja California over the last 80 kyr: A new foraminiferal-based record. Paleoceanography 32, 246-264, doi: 10.1002/2016PA003034. [ Antje H. L. Voelker, Portugal]	Accepted, and reference added.
57993	74	25	74	27	Wrong paper is cited. Schmidt et al. 2017: 'Decline in global oceanic oxygen content during the past five decades' describes the evolution of the oxygen content over the past 50 years and not for the past several hundred million years. Unfortunately, I wasn't able to find the correct paper. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, and this is now corrected.
29877	74	26	74	26	Please, consider moving the brief explanation for anoxia "(oxygen depleted)" after "anoxic events" in the line 18 of the same page. [ Hernan Edgardo Sala, Argentina]	Accepted, and changed accordingly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73703	74	26	74	26	anoxia' is oxygen absence rather than depletion. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and changed to 'absence of dissolved oxygen'
80453	74	26	74	27	This first part of the sentence is odd. I would suggest removing it. [ Samuel Jaccard, Switzerland]	Accepted, and changed accordingly.
93397	74	27	74	27	I don't think Schmidt et al 2017 is a good reference here. [ Carles Pelejero, Spain]	Accepted, and reference changed.
52601	74	29	74	29	Estimation of SL for LIG given here is 3-11 m (in agreement with Figure 2.33). In Figure FAQ 1.3. Figure 1 a range 3-10 m is given and in Figure 1.3. the black dot and error bar seem to indicate 7.5 +/- 3. [ Gema Martínez-Méndez, Germany]	Comment misplaced but taken into account in revisions to the GMSL section
57995	74	31	74	31	Wrong citation of the Deutsch et al., 2015: 'Climate change tightens a metabolic constraint on marine habitats' paper. The Deutsch et al., 2015 paper is about metabolically viable habitats shifts and not about decadal oscillation associated with climate modes and multidecadal trends in oxygen and nutrients in the upper ocean. If the Deutsch et al., 2015 paper is canceled out, the citation for the sentence is correct. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted, and the reference has been removed, thank you.
73705	74	34	74	34	Change 'kilometer' to 'kilometre'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, and changed.
21191	74	36	74	39	Sentence is too complicated. Better split in two. It's also a strange to conclude (with low confidence) that there were parts of the ocean that remained well-oxygenated for several 100s of millions of years. Is that really relevant? I think it is much more relevant that many and/or large parts of the oceans were prone to anoxia on several occasions (the OAEs) in this time interval. The PETM was probably not a global OAE, but many marginal basins became anoxic, especially along the Tethys ocean (Gavrilov et al. 1997 - Lith. Miner. Resour.; Speijer & Wagner 2002 -GSA SP356; Dickson et al. 2014 - Paleocceanography). [ Robert Speijer, Belgium]	Taken into account, and text is revised.
93399	74	38	74	39	I'm not sure about the last part of this sentence "... And hence, there is low confidence...". Is this really important for the report? [ Carles Pelejero, Spain]	Rejected. Although modified due to other comments, the information is relevant.
18033	74	42	74	42	It is unclear how the marine ecosystems section fits into 2.3 (changes in large-scale climate). The entire discussion for marine ecosystems is about phenology. But to cover marine ecosystems properly it would be necessary to talk about how climate change is manifested in many other properties other than phenological ones. [ Lisa Levin, United States of America]	Accepted. Valid question about chapter structure. Sections have been reorganised in response.
2019	74	42	74	42	I am surprised that there is not a "paleo context" section for this section 2.3.4.3 Marine ecosystems, because the paleo can provide some important context here. Maybe Dani Schmidt, a WG2 CLA, could help with this? There is a paleo paragraph in the next section on terrestrial ecosystems, so why not here? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Relevant text added to cross-chapter box 2.1
100121	74	42	76	9	Would be good to cite recent studies on the detection of climate change signals in the global ocean color record (Dutkiewicz et al, 2019), which showed that changes in phytoplankton community structure are likely to show the most rapid climate-driven signals. And Henson et al (2018) identified phenological changes in phytoplankton blooms, showing that bloom timing generally shifts later at mid-latitudes and earlier at high and low latitudes by ~5 days per decade to 2100. Dutkiewicz, S., Hickman, A.E., Jahn, O. et al. Ocean colour signature of climate change. Nat Commun 10, 578 (2019). <a href="https://doi.org/10.1038/s41467-019-08457-x">https://doi.org/10.1038/s41467-019-08457-x</a> . Henson SA, Cole HS, Hopkins J, Martin AP, Yool A. Detection of climate change-driven trends in phytoplankton phenology. Glob Change Biol. 2018;24:e101– e111. <a href="https://doi.org/10.1111/gcb.13886">https://doi.org/10.1111/gcb.13886</a> . [ Steven Bograd, United States of America]	Rejected. This work is model-based. Chapter 2 focus is on observations.
83237	74	42	76	9	Section 2.3.4.3 Marine ecosystems - As far as I can see, there seems to be nothing here about change in polar marine ecosystems e.g., krill, whales, seals, polar bears, penguins etc. [ Robert Massom, Australia]	Accepted. New section 2.3.4.2.3 mentions seabirds. The limited scope of the section on biosphere is now stated clearly at the beginning of Section 2.3.4.2.
127095	74	42	76	9	This section really needs a makeover, it doesn't really assess changes. For example, page 75, line 28, says "the global trends in chlorophyll data are small, but are significant and more pronounced in some regions." What direction are the trends -- increasing, decreasing? Is this consistent with warming over the oceans? [ Trigg Talley, United States of America]	Accepted. Section has been rewritten substantially and the figures have been re-drawn.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105097	74	42	76	9	There are lost of evidence of changes in marine ecosystems in the past, this is even the basis for paleo-reconstructions in productivity and in climate. I think that the message that marine ecosystems do change with climate, and that we have a lot of evidence from marine cores, is an important one. Some studies focusing on changes in productivity in relation with changes in climate/atmosphere have been published, e.g. for the Arabian Sea and the Indian summer monsoon ( Le Mézo, P., Beaufort, L., Bopp, L., Braconnot, P., and Kageyama, M.: From monsoon to marine productivity in the Arabian Sea: insights from glacial and interglacial climates, Clim. Past, 13, 759–778, <a href="https://doi.org/10.5194/cp-13-759-2017">https://doi.org/10.5194/cp-13-759-2017</a> , 2017. ) but it is not the only one. [ Masa KAGEYAMA, France]	Accepted. Text included in cross-chapter box 2.1.
102757	74	42	76	10	A paragraph on the impacts of ocean warming and ocean acidification on coral reefs should be added [ Philippe Tulkens, Belgium]	Rejected. This material was dealt with in detail in SROCC and AR5. Furthermore, this chapter does not deal with attribution.
50689	74	44	75	7	This section may benefit from inclusion of discussion of the role of planktonic changes to carbon cycling, such as the observed changes recorded in Beaugrand et al 2010 (Beaugrand G., Edwards, M. and Legendre L. (2010) Marine biodiversity, ecosystem functioning, and carbon cycles. PNAS, 107, 10120–10124.) showing changes in dominance by smaller organisms that may influence how carbon flows. This could also be supported by reference to regional studies in the North Atlantic such as Harris et al 2014 or 2015 (full citations - Harris, V., Olhede, S. and Edwards, M. (2014) Multidecadal Atlantic climate variability and Community Responses in Ecological Datasets. Journal of Marine Science, 133, 144–153. Harris, V., Olhede, S.C. and Edwards, M. (2015) Multidecadal spatial reorganisation of plankton communities. Journal of Marine Science, 142, 16–24.,) which also demonstrate changes in plankton communities due to temperatures. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Role of phytoplankton in carbon cycle is dealt with in Chapter 5.
105141	74	44	75	19	Phenological shifts and especially tropic match/mismatch correspond to “impacts” on marine ecosystems structure and functioning; and they are covered AR6WGII Ch-3 Oceans and it would be useful include reference to that Chapter. It would also be good to try to minimise overlaps on the marine phenologies between WGI Ch2 and WGII Ch3. In AR6 WGII Ch3, the assessment of changes in phenology in marine ecosystems have been complemented with recent literature, and with comprehensive assessment across trophic levels for both observed and projected changes. Moreover, for info, the detailed outline for AR6 WGII Ch3 is specifically requesting assessment of phenological shifts as follows: 1) “assess changing trends in ocean and coastal ecosystems and the detection and attribution of climate change as driver in observed trends such as changes in abundance, phenology, distribution, community composition or combinations thereof”; and 2) assess “climate impacts on future key vital rates and phenological shifts” (reference - WGIIAR6_ScopingPaper-AnnotatedOutline.pdf). [ Marie-Fanny Racault, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Section was coordinated with WG II. Marie-Fanny Racault is part of discussion group.
3893	74	44	76	9	There seems to be significant overlap here with material scoped for WGII Chapter 3. Also, many of the papers cited here are more than two years old, and likely reflect material already assessed by AR5 WGII and/or SROCC Chapter 5. Consider citing AR5 Chapter 30 or SROCC Chapter 5, with confidence language? [ David Schoeman, Australia]	Taken into account. WG2 has been consulted.
3901	74	44	76	9	Why is the focus here on phenological changes when there is so much evidence for range shifts, behavioural alteration, etc? This needs some context? [ David Schoeman, Australia]	Accepted. Context provided for limited scope of section.
99233	74	44	76	9	While I recognise that biotic response is now part of the outline, I am strongly concerned about WGI making these assessments. I would suggest strongly shortening the section and referring to WGII where this assessment is happening. Several examples here were already discussed in AR5 WGII and are not novel. As a result the text is more a review of some literature and not an assessment. As such the summary on top of page 76 does not provide new information above what was stated in AR5 WGII and the text is too general to be useful. [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. WGII has been consulted.
7225	74	44			Consistency in citing a report. Several text cite only AR5, and some refer to AR5 WGII. [ Asaad Irawan, Indonesia]	Editorial

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83459	74	52	74	52	If you want to include a kelp study that looks at scenarios from the LGM, MH to IPCC future projections look at Assis, J., Araujo, M.B., Serrao, E.A., 2018. Projected climate changes threaten ancient refugia of kelp forests in the North Atlantic. Glob Chang Biol 24, e55-e66, doi: 10.1111/gcb.13818. [ Antje H. L. Voelker, Portugal]	Rejected. Paper referred to presents an ecological niche model and results. Models and future projections fall outside the scope of this chapter, which has an observational focus.
7227	74	52	74	54	All examples and research are conducted in the northern hemisphere, thus may inhibit a geographical bias. It is recommended to explore and investigate research and literature from other regions (tropical or southern hemisphere) [ Asaad Irawan, Indonesia]	Taken into account. We have given global examples using satellite data. SROCC includes many examples from the SH. A tropical ocean example (Indian Ocean) was already given, and an additional one on the Humboldt current region (Southern Hemisphere) has been added (Jackson et al. 2011).
67829	74	52	74	54	All examples and research are conducted in the northern hemisphere, thus may cause a geographical bias. It is recommended to include researches and literature from other regions (tropical or southern hemisphere) [ Ruandha Agung Sugardiman, Indonesia]	Accepted. In addition to global examples using satellite data, specific examples from tropics (Indian Ocean) and South Pacific have been added.
3899	74	52	75	41	There is very little confidence language here. Instead, it reads like a mix of illustrative examples, rather than an assessment. Having said that, undertaking such a huge assessment in two pages is next to impossible. This begs the question: why attempt it here when it is scoped in detail for WGII Chapter 3? One solution might be to flag in advance that this is just a series of examples, but that the main assessment happens in WGII? [ David Schoeman, Australia]	Accepted. Confidence language has been revised. WGII has been consulted.
50691	74	55	74	55	If suitable, there is a more recent publication that can be added that provides more recent outputs of plankton data from the North Atlantic (Edwards, M., Helaouet P., Alhaija, R.A., Batten S., Beaugrang G., Chiba S. et al. (2016) Global Marine Ecological Status Report: results from the global CPR survey 2014/2015. SAHFOS Technical Report, Plymouth, UK, 11, 1-30. ISSN 1744-0750.) [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference has been added.
57997	74	55	74	55	The paper by Edwards and Richardson (2004) is correctly cited, however, it is missing in the references. Edwards and Richardson (2004): Impact of climate change on marine pelagic phenology and trophic mismatch. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing reference has been added.
83461	74	55	75	2	The study of Jonkers, L., Hillebrand, H., Kucera, M., 2019. Global change drives modern plankton communities away from the pre-industrial state. Nature 570, 372-375, doi: 10.1038/s41586-019-1230-3. corroborates this and you could potentially include it. [ Antje H. L. Voelker, Portugal]	Accepted. Jonkers et al. (2019) has been added.
24075	74		75		Subsection 2.3.4.3 Marine ecosystems, is mainly limited to the report of chlorophyll as a major player in the ocean carbon cycle and, the links between phytoplankton an higher-order species is discussed. Since no other effect on other ecosystems are included in this section, and changes in chlorophyll serve as the sole indicator to climate change it would be of importance to justify the reason for this in the beginning of the section. That is, reason to why no other ecosystems is discussed and only chlorophyll and its links is addressed. [ Linn Berglund, Sweden]	Agreed. Text has been introduced to provide rationale for the material included.
102759	75	1	75	1	The statement that phenological changes can lead to trophic mismatches should be lifted to the SPM, as such mismatches may have far reaching consequences. [ Philippe Tulkens, Belgium]	Accepted. WGII has been consulted.
43129	75	1			Read "varied from each to other," rather than "varied from each other," [ Cyriaque Rufin Nguimalet, Central African Republic]	Rejected. Original text is more correct from a linguistic point of view.
105143	75	9	75	12	The definitions about Chlorophyll-a and main drivers of phytoplankton are provided in AR6 WGII Ch3 section Point of departure. Perhaps – a reference to section could be included and would help to minimise overlaps. [ Marie-Fanny Racault, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This text is being coordinated with WG II. Marie-Fanny Racault is part of the discussion group.
413	75	10	75	10	phytoplankton biomass instead of concentration [ Leticia Cotrim da Cunha, Brazil]	Accepted. Changed as requested.
73707	75	13	75	13	Insert , around 'for example'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
57999	75	14	75	14	The paper by Platt et al., 2003 is correctly cited, however, it is missing in the references. Platt et al., 2003: 'Spring algal bloom and larval fish survival' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing reference has been added.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83463	75	15	75	15	For Sardine in the Iberian upwelling system you could add Garrido, S., Silva, A., Marques, V., Figueiredo, I., Bryère, P., Mangin, A., Santos, A.M.P., 2017. Temperature and food-mediated variability of European Atlantic sardine recruitment. Progress In Oceanography 159, 267-275, doi: <a href="https://doi.org/10.1016/j.pocean.2017.10.006">https://doi.org/10.1016/j.pocean.2017.10.006</a> . [ Antje H. L. Voelker, Portugal]	Accepted.
58001	75	15	75	15	The paper by Koeller et al., 2009 and Ouellet et al. 2011 are correctly cited, however, they are missing in the references. Koeller et al., 2009: 'Basin-Scale Coherence in Phenology of Shrimps and Phytoplankton in the North Atlantic Ocean', Ouellet et al. 2011: 'Ocean surface characteristics influence recruitment variability of populations of northern shrimp ( <i>Pandalus borealis</i> ) in the Northwest Atlantic' [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing reference has been added.
58003	75	16	75	16	The paper by Kassl et al. 2018: 'Remotely Sensing the Biophysical Drivers of Sardinella aurita Variability in Ivorian Waters' is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing references have to be added.
58005	75	16	75	16	The paper by Trzcinski et al. 2013: 'Variation in ocean colour may help predict cod and haddock recruitment' is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing reference has been added.
73709	75	16	75	16	Insert genus name (at least) after 'cod'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Species is <i>Gadus morhua</i> . The paper also talks about haddock ( <i>Melanogrammus aeglefinus</i> ) larvae. Species names have been added for cod and haddock.
58007	75	17	75	17	The paper by Borstad et al. (2011) : 'Environmental control of the breeding success of rhinoceros auklets at Triangle Island, British Columbia' is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing reference has been added.
58009	75	22	75	22	The paper by Racault et al. 2012, 2016 and Sapiano et al. 2012 are missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing references have been added.
58011	75	22	75	22	The paper by Longhurst (1998) is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference changed to a later edition book and reference added.
58013	75	27	75	27	The paper by Mélin et al. (2017) is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing reference has been added.
5413	75	27	75	41	Much of this paragraph is impossible to infer from F 2.29. [ Bryan Weare, United States of America]	Taken into account, Figure 2.29 and the corresponding text have been modified.
58015	75	28	75	28	There has been a mix up with the sources. First of all Sathyendranath et al. 2019 is mentioned as the source of the sentence, however, the references stated Sathyendranath et al. 2018 - therefore a different date and second I'd say that's the wrong citation in general. I'd say it should be von Schuckmann et al., 2018, Copernicus Marine Service Ocean State Report, Journal of Operational Oceanography, 11:sup1, S1-S142, DOI: 10.1080/1755876X.2018.1489208 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References have been changed to the von Schuckmann papers.
73711	75	28	75	28	Remove ( before second Sathyendranath reference and move to before 2018, deleting , after al. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been edited (including change in reference).
30455	75	28	75	32	Clarification needed: the expression 'global trends' seems to correspond to regional trends ('in some regions') and not the globally averaged trend as it should ('global' means globally averaged). [ Gilles Delaygue, France]	Taken into account. Text has been modified to avoid confusion.
30457	75	28	75	32	Clarification needed: 'trends...are small, but are significant' seems to contradict further text 'this precludes any firm assessment of trends' (also cf. in the legend of Fig 2.29: 'trends are significant') [ Gilles Delaygue, France]	Taken into account. Text modified to improve clarity.
58019	75	30	75	30	The paper by Brewin et al. 2012; Racault et al. 2017 are missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing references have been added.
30459	75	31			what means IOD? [ Gilles Delaygue, France]	Accepted. Indian Ocean Dipole expanded and referred to Technical Annex 6.
58021	75	34	75	34	The paper by (Kulk et al. Submitted) is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Missing references were added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57749	75	34	75	38	Kulk et al submitted and Kulk et al. (2019, submitted) shall be the same paper but I can't find the reference in the list, so I couldn't check if it is already published. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Reference has been updated. Paper is now published.
527	75	35	75	37	It is important to indicate whether the primary production referred to in this sentence is still global and still just marine, as in the preceding sentence. [ Claire Parkinson, United States of America]	Taken into account. Global marine primary production. Clarification has been inserted.
58023	75	38	75	38	The paper by Kulk et al (2019, submitted) is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Missing references have been added.
13279	75	38	75	38	al missing . [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
73713	75	38	75	38	Reference formatting needs tidying up. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
58017	75	51	75	51	Wrong citation. Instead of Sathyendranath et al. (2019) it should be von Schuckmann et al., 2018, Copernicus Marine Service Ocean State Report, Journal of Operational Oceanography, 11:sup1, S1-S142, DOI: 10.1080/1755876X.2018.1489208 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
50693	75	53	75	53	"as in (Santer et al., 2008) applied to monthly data." Should read "as in Santer et al. (2008) applied to monthly data.". Bracket moved, comma removed. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Caption has been amended for clarity.
30461	75	53			'Only significant trends are shown.': given the very short period of time (20yrs), and the variability underlined in the text, it is necessary to clarify the level of significance [ Gilles Delaygue, France]	Accepted. P <0.05. Information added to Fig. 2.29.
3351	75		9	41	I consider the text very valuable, but I think it is important to expand ideas in this paragraph, in order to contribute more to the knowledge in the elements that are mentioned here, they are very valuable and I believe in these two paragraphs deserve to be deepened [ Eduardo Erazo Acosta, Colombia]	Accepted. Text has been expanded. Overlaps and gaps have been discussed with WGII.
15923	76	1	76	10	On the discussion of the marine ecosystems, some comment should be made of the increasing bleaching frequencies that are being experienced on coral reefs across the world. [ Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This material is covered in great detail in SROCC. Furthermore, it falls outside the limited scope of this section, which is now clarified at the beginning of section.
127097	76	3	76	9	This summary paragraph makes a number of quasi-assessment statements, with confidence attached. But it doesn't really assess the changes. It only says that things have changed. How have they changed? Are these changes consistent with a changing climate? [ Trigg Talley, United States of America]	Rejected. This chapter does not deal with attribution. We are merely reporting observed changes here.
3903	76	5	#REF!	#REF!	What is the difference between "poor" sampling and "undersampling"? [ David Schoeman, Australia]	Accepted. Changed as requested.
3905	76	6	#REF!	9	I don't see too much evidence provided for this statement in the preceeding text. I don't doubt that it is true, I just don't think that confidence statements can be made without a chain of evidence. At best, you have listed correlations between selected demographic characteristics of some species and chl-a concentration...and most of the species listed don't consider phytoplankton to be "food"? [ David Schoeman, Australia]	Accepted. Text has been edited to make it easier to follow the line of evidence. Members of WGII have been consulted.
81523	76	6	76	9	Suggest to revise "...Evidence is also available from different locations that there is a strong dependence of survival in higher organisms (fish, exploited invertebrates, birds) on the availability of food at vulnerable moments in their life cycle (high confidence) which is often a function of climate conditions.", as it is vague. [ Ee Ling Lee, Malaysia]	Accepted. Text has been revised.
73715	76	8	76	9	I wonder why this is rated at high confidence and not very high confidence, as the impact of food availability is well known. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Retained high confidence language, to be cautious.
102761	76	11	77	46	A paragraph on the weakening ecosystem functions (carbon sequestration and storage) of terrestrial ecosystems as a consequence of ecosystem degradation and loss should be added [ Philippe Tulkens, Belgium]	Rejected - attribution is outside the scope of the chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70817	76	12	77	46	Here or at another position, the many studies discussing the impact of past land use changes on terrestrial ecosystems could be mentioned, e.g. the 'controversies' around the claim by Ruddiman that land-use impacts were significant very early in human history (e.g. Ruddiman 2018 10.1177/0309133318783142 and many more authors that either confirm or contradict). [ Karlheinz Erb, Austria]	The suggested literature has been reviewed and included if within scope of the assessment being performed by chapter 2.
106501	76	12			Section 2.3.4.4 "terrestrial ecosystems" has large overlap with WGII ch2 "terrestrial and freshwater ecosystems" and with WGII CCB PALEO. Care should be taken to ensure consistency between WGs in messages and uncertainty assessments of those messages. [ camille parmesan, France]	Noted. There has been coordination with WGII
93043	76	12			There is considerable literature on terrestrial ecosystem shifts for the LIG: e.g., Hoogakker et al., CP, 2016; Tarasov et al., CP, 2013; CAPE, QSR, 2006. [ Bette Otto-Bliesner, United States of America]	Taken into account - text revised (reference added).
81637	76	14	76	44	The first sentence gives the impression that most of what will be covered here is changes in the recent decades, whereas the remainder of the text is mostly about paleo shifts in vegetation. Please clarify scope of the assessment at the beginning [ Sönke Zaehle, Germany]	Rejected - this sentence simply restates the AR5 conclusion, which indicates there have been changes in recent decades.
99235	76	14			similarly, the evidence in this section is in parts not novel compared to AR5 WGII as poleward, upslope and faster were already concluded there in the terrestrial ecosystem chapter raising concerns about differences in main findings between WGI in this section and WGII. It would be very beneficial if WGI would focus on the conditions which evoke change as covered in the section on growing season but refer to the impacts of these changes to WGII. [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Chapter 2 is charged with assessing a broad range of observational evidence on the changing state of the climate system, including the biosphere. Further consideration is provided in WGII report.
100619	76	23	76	23	Add: "During the Miocene Climatic Optimum, perennial ice in the Arctic Ocean and a Greenland ice cap may have been absent (Herold et al., 2008; Super et al., 2020) while at times the Antarctic ice cap was significantly reduced in volume (by 80%), perhaps even absent (Hansen et al., 2013; Frigola et al., 2018; Miller et al., 2020). [ Matthew Kohn, United States of America]	Rejected - the suggested changes do not document changes in species distributions, which are the focus of the section.
100621	76	23	76	23	Add: "Numerous vegetation-based climatic zones shifted to higher latitudes, including tropical (10-15° shift to the north and south), warm temperate (25° to north), and temperate (30° to the north, reaching almost 80° N latitude), while humid subtropical forest was present in Iceland, tundra approached 80° S lat. and shrubby trees were stabilized in Antarctica (Warny et al., 2009; Pound, 2012; Denk et al., 2013)." [ Matthew Kohn, United States of America]	Taken into account - text revised (key reviewer suggestions incorporated into paragraph).
100623	76	23	76	23	Add: "Records of local plant assemblages point to geographically heterogeneous responses to global warming, such that some areas warmed while others cooled, and some areas showed increases in precipitation while others showed decreases (Harris et al., 2020)" [ Matthew Kohn, United States of America]	Rejected - the suggested citation refers to conditions in central-eastern Idaho rather than global-scale changes in terrestrial ecosystems.
21245	76	23	76	24	"During the Pliocene, boreal forest extended to the Arctic coastline, with the northernmost extent of treeline being at least 5° closer the pole than at present (Figure 2.33)" Figure 2.33, however shows a value of 7° for change of the northernmost extent of treeline in Pliocene. The same with the present change 1-2° in the text, 1° in the Figure. [ Michael Schmitt, Germany]	Taken into account - text revised (latitude changes in text corrected to match values in the figure).
105099	76	23	76	31	LIG results could be included here. [ Masa KAGEYAMA, France]	Rejected -- comment is ambiguous and does not include actionable input.
9947	76	24	76	24	Probably wrong reference to Fig.2.33. [ Olga Zolina, France]	Rejected - figure reference is correct.
30463	76	24			'closer to the pole' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
52123	76	26	76	26	Southern hemisphere Pliocene vegetation trends discussed here: Sniderman, J.K., Woodhead, J.D., Hellstrom, J., Jordan, G.J., Drysdale, R.N., Tyler, J.J. and Porch, N., 2016. Pliocene reversal of late Neogene aridification. Proceedings of the National Academy of Sciences, 113(8), pp.1999-2004. AND Pliocene expansion of savannahs in Australia discussed here: Andrae, J.W., McInerney, F.A., Polissar, P.J., Sniderman, J.M.K., Howard, S., Hall, P.A. and Phelps, S.R., 2018. Initial expansion of C4 vegetation in Australia during the late Pliocene. Geophysical Research Letters, 45(10), pp.4831-4840. [ Kathryn Fitzsimmons, Germany]	Taken into account - text revised (references added).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5627	76	28	76	31	add "in most areas", at least. In North America, the treeline history is more complex. In the Mackenzie Delta region, treeline was further north than today ~11.5ka and retreated southward ~7ka; in central Canada, treeline was further north between ~6 - 4ka, then retreated south, and in northwestern Quebec, treeline position hasn't changed; here the treeline is a broad transtion zone and although the position hasn't changed, the forest tundra was more dense between 5-3ka. Although the histories are different, affectd by changing climates as deglaciation occurred, the transitons are synchronous (ref 3) and still explainable as a response to climate changes, but that the climate sequence was more complex. Also in the past 3ka, there is seen a further reduction in pollen production at all sites, even though treeline is not moving, due to neoglaciation. Treeline shows climate impacts not only through movement, but also through changes in the density of trees on the landscape, or pollen production, for example. References are: (1) (a book chapter, this would have been at peer-reviewed) Gajewski, K and GM MacDonald. 2004. Palynology of Arctic Lakes. Pp 89-116 in: R Pienitz, M Douglas and J Smol, eds. Long-term environmental change in Arctic and Antarctic Lakes. Kluwer. ;(2) a recent peer-reviewed paper on interpretation of trreline movemnts and climate: Gajewski, K. 2019. Environmental History of the Northwestern Québec treeline. Quaternary Sciences Reviews 206:29-43.; (3) a book chpter, but not reviewed, still the ideas from this are used in the other papers: Gajewski, K, A Viau and M Sawada. 2007. Millennial-scale climate variations in the Holocene – the terrestrial record. Pp 133-154 in: Kutzbach, G, Ed. Climate Variability and Change: Past, Present and Future. John E Kutzbach Symposium. Center for Climatic Research, University of Wisconsin. Madison. [ Konrad Gajewski, Canada]	Accepted - text revised.
18567	76	33	76	44	If you discuss treeline expansion during the Pliocene in the previous paragraph, it should be noted that high latitude locations, such as Beringia, experienced treeline expansion into the Arctic during the early Holocene as a result of insolation changes. See, for example, Kaufman et al., 2004 QSR. More specifically, e.g., Mann, D.H., Groves, P., Reanier, R.E. and Kunz, M.L., 2010. Floodplains, permafrost, cottonwood trees, and peat: What happened the last time climate warmed suddenly in arctic Alaska?. Quaternary Science Reviews, 29(27-28), pp.3812-3830. [ Miriam Jones, United States of America]	Rejected. Key references are already included.
127099	76	34	76	37	It's unclear from the wording what decreased. Do you mean "which both decreased"? [ Trigg Talley, United States of America]	Taken into account - text revised (phrase deleted).
90425	76	34			figures 2.29 cited then figure 2.33 before first citation of figs 2.30, 2.31, 2.32, reorder [ Jeannine-Marie St-Jacques, Canada]	Rejected. Figure 2.33 is principally associated with Section 2.3.5.
30465	76	36			'which decreased during the early to middle Holocene': are these rates of temperature change or of ecosystem? [ Gilles Delaygue, France]	Taken into account - combined with comment 127099.
83465	76	37	76	37	You could include as additional reference Naughton, F., Sanchez Goñi, M.F., Rodrigues, T., Salgueiro, E., Costas, S., Desprat, S., Duprat, J., Michel, E., Rossignol, L., Zaragosi, S., Voelker, A.H.L., Abrantes, F., 2016. Climate variability across the last deglaciation in NW Iberia and its margin. Quaternary International 414, 9-22, doi: 10.1016/j.quaint.2015.08.073. [ Antje H. L. Voelker, Portugal]	Rejected. Key references are already included.
5417	76	38			Except for maybe Europe it is hard to see on F.2.30 the "change have accelerated". On the scale shown it simply looks highly variable. [ Bryan Weare, United States of America]	Taken into account - text revised (figure and sentences discussing it have been deleted).
30467	76	40	76	41	Figure 2.30 stops at 1950, so that it is not clear what means 'recent' in 'recent rates'; readers have no clue how rates over the last millennia compare with rates over the last decades. [ Gilles Delaygue, France]	Taken into account - combined with comment 5417.
7499	76	40	76	41	The sentence "In Europe, recent rates of ecosystem change exceed any observed over the last 10 ka, while in North America rates are at or above any observed over the last 10 ka" could not be expressed better as "In Europe and North America recent rates of ecosystem change exceed any observed over the last 10 ka"? [ Alejandro Cearreta, Spain]	Taken into account - combined with comment 5417.
88381	76	42	76	43	Is there evidence of acceleration which is implied by statement - if not really the case then revision suggested: "... likely to continue thawing." [ Sharon Smith, Canada]	Rejected - the sentence does not discuss anything that might 'continue thawing' like snow or ice (comment appears to have the wrong page number).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116011	76		76		reference to SROCC for high mountain ecosystems is missing [ Valerie Masson-Delmotte, France]	Taken into account - text revised (introductory paragraph now includes a sentence on SROCC findings for high mountain ecosystems).
21247	77	6	77	24	I'm missing here a mention of the poleward shift of vector-borne infectious diseases due to a shift of mosquito populations. Eg. Ref. Joseph L. Servadio, Samantha R. Rosenthal, Lynn Carlson, Cici Bauer, Climate patterns and mosquito-borne disease outbreaks in South and Southeast Asia, Journal of Infection and Public Health, Volume 11, 2018, 566-571 and Colin J. Carlson, Ellen Bannon, Emily Mendenhall, Timothy Newfield, Shweta Bansal, Rapid range shifts in African Anopheles mosquitoes over the last century bioRxiv 673913; doi: <a href="https://doi.org/10.1101/673913">https://doi.org/10.1101/673913</a> [ Michael Schmitt, Germany]	Rejected. Key references are already included.
127101	77	6	77	46	Given the fact that humans have an enormous impact on terrestrial ecosystems, more attention needs to be paid to the role of land-use change. It's only mentioned a few times. The effects of changing climate cannot be separated from human impacts. [ Trigg Talley, United States of America]	Rejected - attribution and impacts are outside the scope of the chapter.
52127	77	6	77	46	See examples of biodiversity and range loss in Central Asia: Su, J., Aryal, A., Hegab, I.M., Shrestha, U.B., Coogan, S.C., Sathyakumar, S., Dalannast, M., Dou, Z., Suo, Y., Dabu, X. and Fu, H., 2018. Decreasing brown bear (Ursus arctos) habitat due to climate change in Central Asia and the Asian Highlands. Ecology and Evolution, 8(23), pp.11887-11899. ALSO Gozlan, R.E., Karimov, B.K., Zadererev, E., Kuznetsova, D. and Brucet, S., 2019. Status, trends, and future dynamics of freshwater ecosystems in Europe and Central Asia. Inland Waters, 9(1), pp.78-94. AND Zhang, G., Biradar, C.M., Xiao, X., Dong, J., Zhou, Y., Qin, Y., Zhang, Y., Liu, F., Ding, M. and Thomas, R.J., 2018. Exacerbated grassland degradation and desertification in Central Asia during 2000–2014. Ecological applications, 28(2), pp.442-456. [ Kathryn Fitzsimmons, Germany]	Rejected. Key references are already included.
73717	77	8	77	8	Remove , after Greenland. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
105537	77	14	77	24	This section summarizing recent observations of ecosystem change should be instructional to those trying to quantify earlier vegetation change (see prior comment on Figure 2.3). Many ecosystem processes, such as soil development and soil erosion, procede at different rates, and are not easily encapsulated in short-term statistics such as changes fossil pollen deposition. [ Kenneth Cole, United States of America]	Rejected -- processes are the purview of later chapters.
29879	77	20	77	20	Contemplate using "Poleward advance..." instead "Polar advance...". [ Hernan Edgardo Sala, Argentina]	Accepted - text revised.
30469	77	26			'climate zones': not clear wether 'climate' is here considered defined by meteorological or vegetation parameters [ Gilles Delaygue, France]	Rejected - climate zones are defined on both meteorological and vegetation parameters depending upon the analysis.
58025	77	33	77	33	The paper by Ceglar et al., 2019: 'Observed Northward Migration of Agro-Climate Zones in Europe Will Further Accelerate Under Climate Change' is missing in the references [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
58027	77	36	77	36	The source is wrongly listed Gibson-Reinemer et al., 2015. In line 16 & 17 page 77 it's Gibson-Reinemer and Rahel, 2015 even though it's the same source. Probably an error in the citation manager. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
109565	77	40	77	41	Greenness is and indicator of leaf biomass or LAI, and by extension fAPAR. fAPAR can also be measured from similar data. Greenness is not directly an indicator of photosynthetic activity, they are related but many other things also affect photosynthetic activity. It would be more accurate to say "(i.e., green leaf biomass, area, and fAPAR, which contribute to photosynthetic activity)" [ Anthony Walker, United States of America]	Taken into account - text revised (definition of greenness changed to 'green leaf area and/or mass').
30471	77	45	77	46	not clear where the comparison between the 20 and 21st centuries and the last millennia is discussed [ Gilles Delaygue, France]	Taken into account - text revised (sentence deleted).
34859	77	49	78	42	It is little appreciated that the increasing CO2 level is actually beneficial to the greening through photosynthesis. Please see general comment #16 above. [ Jim O'Brien, Ireland]	Noted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81047	77	49	78	42	Here global greenness is presented as an indicator of change in large scale climate (section 2.3). However, research shows that a good part of the greening trend is due to the intensification of agriculture which has nothing to do with climate change. Although the agreement was that the section doesn't talk about mechanisms, in this case there is a need to point out the issue, otherwise we are making the reader believe that the observed changes are all due to changes in climate. Likewise, also a secondary point here, the single most important driver is the elevated CO2 fertilization effect, globally, which is not a response to changes in climate either, but in this case closely related, so that it is ok. Other parts particularly high latitudes, warming is the main driver. Important to coordinate so the mechanisms are explained somewhere, here or there. Reference on the role of agricultural intensification playing a major role in greening are: The key paper is: Chen_2019_China and India lead in greening of the world through land-use management. NatureSust. See also Mueller et al 2014 Human Land-Use Practices affect Global Long-Term Trends in Photosynthetic Capacity. Remote Sensing. Also reference ch3 p.57 line 56 (section 3.6.1) where the mechanisms are discussed in more detail. [canadell pep, Australia]	Taken into account - text revised (sentence added on the consistency between greening and other factors, including the intensification of agriculture).
70815	77	49	78	42	An article is upcoming which might add to this passage: Winkler et al., "Slow-down of the greening trend in natural vegetation with further rise in atmospheric CO2" AGU advances. [Karlheinz Erb, Austria]	Taken into account - combined with comment 42121.
5629	77	49	78	42	Need to be careful here. Although earlier work showed large amounts of greening, more recent work is showing browning as well. For example: Ju, J and J Masek. 2016. The vegetation greenness trend in Canada and US Alaska from 1984-2012 Landsat data. Remote Sensing of Environment 176: 1-16, and others. The greening is greater than the browning, but still some areas are, in fact decreasing in production. As you note, it depends on which index you use to indicate greening. [Konrad Gajewski, Canada]	Taken into account - combined with comment 42121.
42121	77	49	78	45	Potentially relevant additional paper: Winkler et al. (submitted). According to Winkler et al. (submitted) the LAI3g product shows a recent slow-down of the greening trend and the emergence of browning clusters, particularly in tropical high productive ecosystems. The slow down in greening and the emergence of browning clusters are an important indicator of climate change / human impact. Slow-down of the greening trend in natural vegetation with further rise in atmospheric CO2, Winkler et al. submitted - <a href="https://www.essoar.org/doi/abs/10.1002/essoar.10503202.1">https://www.essoar.org/doi/abs/10.1002/essoar.10503202.1</a> [Julia Nabel, Germany]	Taken into account - text revised (reference added and discussion now notes slower changes in the past two decades and the emergence of browning).
109563	77	51	77	52	Odd phrasing. Seems more accurate to say that greening is a proxy for photosynthetic activity given that satellites actually measure spectra (i.e., colour). [Anthony Walker, United States of America]	Taken into account - combined with comment 109565.
127103	77	51	78	42	It could be worth mentioning that flux observations show a different picture. They do not show trends in productivity. [Trigg Talley, United States of America]	Taken into account - text revised (flux tower measurements now discussed).
43131	77	53		54	About this conclusion, it seems that the Africa realities are not taken into account due to the fact that deforestation around cities is ongoing. [Cyriaque Rufin Nguimalet, Central African Republic]	Rejected - Chapter assesses large-scale (i.e., global and hemispheric) rather than urban- and regional-scale changes.
116013	77		77		the assessment of browning trends is missing (cf SRCCL). SRCCL also provided insights on effects of local land and water management for greening trends in some tropical areas, they would need to be reported here too (check / ch 5 / ch 10 too). [Valerie Masson-Delmotte, France]	Taken into account - text revised (existing discussion of browning trends augmented with SRCCL insights).
109567	78	1	78	15	Worth citing Donohue et al., 2013 in here. This study demonstrates nicely an increase in vegetation cover (i.e. greenness) for a given precipitation amount in semi-arid ecosystems. Donohue, R. J., Roderick, M. L., McVicar, T. R., & Farquhar, G. D. (2013). Impact of CO2 fertilization on maximum foliage cover across the globe's warm, arid environments. Geophysical Research Letters, 40(12), 3031–3035. [Anthony Walker, United States of America]	Taken into account - combined with comment 81047.
58029	78	4	78	4	The paper by Liu et al., 2015b is missing in the references. [APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
5419	78	10			Something must be added to suggest that the trends are related to climate change and not increased agriculture, as implied by the strong positive trends over China, India and Brazil shown in F2.31. [Bryan Weare, United States of America]	Taken into account - combined with comment 81047.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
58031	78	13	78	15	In my opinion it's misleading if in brackets (such as parts of central Asia and the Congo basin) is mentioned and one of the sources at the end of the sentence (Yang et al., 2018a) is about browning in the Amazon region. I suggest to include the Amazon region within the brackets like (such as parts of central Asia, the Congo basin and parts of Amazonia). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - text revised (text now mentions Amazonia).
57631	78	15			Information on the timing of the browning would be useful in terms of understanding the context of these changes [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - combined with comment 42121.
52177	78	20	78	20	Words "are not" is repeated twice in the sentence "solar and wind farm are not are not assessed here..." [ Maritza Jadrijevic Girardi, Chile]	Not applicable, this sentence does not discuss solar and wind farms (comment appears to be for another chapter).
30473	78	20			'per decade': not consistent with the legend of Figure 2.31 stating in 'percent over 1998-2018' [ Gilles Delaygue, France]	Editorial; copyedit to be completed prior to publication.
529	78	21	78	21	In this caption to Figure 2.31, it would help to have "TIP C6" spelled out. [ Claire Parkinson, United States of America]	Rejected - this is the standard nomenclature for this product.
58033	78	33	78	33	The citation should be Pan et al., 2018a instead of Pan et al., 2018 since there are two Pan et al., 2018 papers. It's correctly listed in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
81043	78	35	78	36	The sentence here tries to address the issue, but the agricultural intensification not only might make but drivers a big part of the global trend, and therefore requires a specific mention. [ canadell pep, Australia]	Taken into account - combined with comment 81047.
19745	78	40	78	42	There is no doubt about this statement. Is there, on the other hand, a general agreement about this issue being part of the climate system? [ philippe waldteufel, France]	Taken into account - combined with comment 81047.
7229	78	41	78	42	Low confidence statement. This paragraph needs to be supported by compelling research and analytical data, as the magnitude of greening possibly is higher in certain places due to completed data and lower in several areas due to data insufficiency. [ Asaad Irawan, Indonesia]	Rejected - paragraph that precedes this summary already presents multiple reasons for low confidence.
67831	78	41	78	42	Low confidence statement. This paragraph needs to be supported by a stronger or more convincing research and analytical data, as the magnitude of greening is possibly higher in certain places due to availability of data, and lower in several areas due to data insufficiency. [ Ruandha Agung Sugardiman, Indonesia]	Rejected - paragraph that precedes this summary already presents multiple reasons for low confidence.
57635	78	42			I believe the statement about a lack of ground based calibration in the above paragraph should also be mentioned in the concluding statement as a reason for the low confidence [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - paragraph that precedes this summary already presents multiple reasons for low confidence.
42123	78	45	80	4	Potentially relevant additional paper: Buermann et al., 2018. According to Buermann et al. (2018) many northern ecosystems do not see a benefit of warmer springs (change in growing season) on growing-season ecosystem productivity due to negative compensations in later seasons caused by the accumulation of seasonal water deficits. Buermann, et al. (2018), <a href="https://doi.org/10.1038/s41586-018-0555-7">https://doi.org/10.1038/s41586-018-0555-7</a> [ Julia Nabel, Germany]	Rejected - attribution is outside the scope of the chapter.
98751	78	53	78	53	If you want a Holocene-length reconstruction of growing degree days in North America see Marsicek et al. (2018). <a href="https://doi.org/10.1038/nature25464">https://doi.org/10.1038/nature25464</a> [ Meredith Parish, United States of America]	Rejected - focus of the section is growing season length rather than growing degree days, which are somewhat different (but the reference was appreciated nonetheless).
57637	78	53	78	55	Lacks a pertinent reference. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - this is the introductory sentence to a paragraph that provides numerous citations for in situ analyses that document changes in the growing season.
57633	78				Figure 2.31 - caption states that grey denotes unvegetated land surfaces however Antarctica, Greenland and the Canadian High Arctic are shown in white despite being unvegetated [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted - figure revised.
67715	79	1	79	1	(Dunn,2019): Is this reference right? [ Hiroaki Kondo, Japan]	Editorial; copyedit to be completed prior to publication.
58035	79	1	79	1	The correct paper by Dunn, 2019 is missing in the references. In the references there is a Dunn et al., 2019 paper, however, that one is about another topic. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
52179	79	1	79	1	Poor quality on Figure 2.24. [ Maritza Jadrijevic Girardi, Chile]	Noted. Figure quality is improved throughout.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5421	79	8	79	9	At least one more sentence should be added discussion the recent lage shifts shown in F.2.32. [ Bryan Weare, United States of America]	Taken into account - combined with comment 30475.
30475	79	8	79	10	Same comments as for Figure 2.32: I am wondering whether picking 2 examples does not harm the section because 1. it suggests that there are very few series; and 2. the very strong interannual variability makes the trends hardly significant. Or maybe useful with a discussion on centennial variability. [ Gilles Delaygue, France]	Taken into account - text revised.
531	79	26	79	28	It should be clarified what type of data were used for the first part of the sentence (e.g., satellite data?), as otherwise the last part of the sentence ("in general agreement with ground-based data") lacks proper context. [ Claire Parkinson, United States of America]	Taken into account - text revised (sentence now clarifies that satellite-based records are consistent with surface observations).
58037	79	42	79	42	The paper by Forkel et al., 2016: 'Enhanced seasonal CO2 exchange caused by amplified plant productivity in northern ecosystems' is missing in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
127105	79	45	79	45	The increase of CO2 annual amplitude is consistent with lenghtened growing season, but does not ensure causality. Increased uptake during summer could be happening just due to warmer and wetter conditions, and not because the growing season is longer. Also, respiration needs to be considered as this is sensitive to soil temperature and moisture. [ Trigg Talley, United States of America]	Taken into account - text revised (discussion now mentions warmer and wetter conditions as other potential drivers).
127107	79	45	79	47	Wouldn't global greening also enhance the seasonal cycle of CO2? Is there evidence (that can be discussed) for whether global greening or lenghtened growing season has a larger impact on the CO2 seasonal cycle? [ Trigg Talley, United States of America]	Taken into account - text revised (discussion of changes in seasonal cycle of CO2 notes consistency with increased greening and longer growing season).
81205	79	45	79	53	A recent paper may be cited in this context: Chakraborty, Tiwari et al. Observations and Modeling of GHG Concentrations and Fluxes Over India; In: Assessment of Climate Change over the Indian Region; R. Krishnan, J. Sanjay, Chellappan Gnanaseelan, Milind Mujumdar, Ashwini Kulkarni, Supriyo Chakraborty Editors 2020 . <a href="https://doi.org/10.1007/978-981-15-4327-2">https://doi.org/10.1007/978-981-15-4327-2</a> [ Supriyo Chakraborty, India]	Taken into account - text revised (Indian sites now included in discussion).
373	79	47	79	49	units missing for changes in amplitude [ Wolfgang Obermeier, Germany]	Rejected - changes in the seasonal amplitude are expressed as percentages in the cited papers.
19747	79	55	80	4	Unlike the global greening case presented in subsection 2.3.4.5, there is here very little doubt that such changes participate to the evolution of the climate system. [ philippe waldteufel, France]	Rejected -- comment is ambiguous and does not include actionable input.
109569	80	2	80	3	"enhanced seasonal cycle in the atmospheric burden of CO2" seems overly wordy, suggest: "amplified seasonal cycle in atmospheric CO2" [ Anthony Walker, United States of America]	Accepted - text revised.
68049	80	3	80	4	"There are no widely available and reliable sources of proxy information that permit a longer-term context for global-scale growing season length changes" : nothing from the PalEON project ( <a href="http://www.paleonproject.org">www.paleonproject.org</a> )? [ Michael Evans, United States of America]	Taken into account - text revised (sentence deleted).
108079	80	7	80	44	Section 2.3.5 Synthesis of evidence for past changes: In my opinion, land use and/or land cover should be mentioned as one of the main indicator for monitoring of past changes. [ Asylbek Aidaraliev, Kyrgyzstan]	Rejected. In CH2 LULC is treated as a forcing agent under section 2.2
24387	80	16	80	16	To say that "Evidence is ubiquitous..." is an overstatement. Ubiquitous means everywhere and all the time. If evidence was truly ubiquitous, we would have complete and total knowledge and there would be no uncertainties. The word "ubiquitous" should be replaced with appropriate IPCC calibrated language. [ Owen Cooper, United States of America]	Taken into account. Text modified for clarity.
127109	80	16	80	16	"ubiquitous" is stretching it. [ Trigg Talley, United States of America]	See comment ID 24387
24389	80	16	80	25	There are many instances of awkward language and phrasing in this paragraph, requiring thorough copy-editing. [ Owen Cooper, United States of America]	Taken into account. Text modified for clarity.
81207	80	20	80	20	Possible re-wording required. [ Supriyo Chakraborty, India]	Taken into account. Text modified for clarity.
93401	80	20	80	20	"both more aspects" consider rewording [ Carles Pelejero, Spain]	Taken into account. Text modified for clarity.
6561	80	21	80	21	The period of reliable global instrumental observation is less than 150 years for most climate indicators. The sentence needs rewriting to say that global observation of climate indicators began 150 or so years ago, or something similar. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Sentence has been split in two to account for this and avoid this potential misinterpretation.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6563	80	23	80	23	"essentially" would be better than "truly" as satellites in polar orbit tend to miss the immediate vicinities of the poles. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
30477	80	27			'Assessing the long-term context of recent changes is key to understanding their importance.' with the STRONG caveat that no past situation can serve as a strict analogue to the current one. [ Gilles Delaygue, France]	Taken into account. Text has been clarified.
89417	80	33	80	34	"Similarly, several fast-varying components are collectively in states as likely as not unseen in millennia (e.g. AMOC, Arctic glaciers, GMSL)." – Should the AMOC be included in this statement? A recent publication based on 6 independent proxy data sets seems to suggest that the AMOC is likely in a state not seen in millennia (Rahmstorf et al. 2015, DOI:10.1038/nclimate2554). [ Ricarda Winkelmann, Germany]	Noted. The text here is meant to summarise the preceding sections and this reference has been considered there.
7793	80	33	80	34	What is the evidence that the AMOC is in a state as likely as not unseen in millennia? I don't know enough about the other processes to comment, but this seems strong given the discussion of the AMOC earlier (which I think is very good) [ Laura Jackson, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The AMOC assessment has been extensively revised and this has informed edits here.
89309	80	33	80	35	I am truly baffled by the double standards of evidence displayed here. So ice-sheet extent is "very likely in states unseen in centuries to millennia", but the AMOC only "as likely as not"? In my earlier comments I have listed 6 independent proxy data series that suggest consistently that the AMOC is weaker in modern times than any time in the last millennium, consistent with what models predict. Do you have six equally valid studies that suggest that it is not weaker? Why don't you discuss those then, and instead just dismiss the positive evidence with superficial truisms? Are you really sure you have far stronger evidence for the Antarctic Ice Sheet extent now being "very likely in states unseen in centuries to millennia"? What is it? I have not seen it in this chapter. [ Stefan Rahmstorf, Germany]	See comment 7793
108007	80	34	80	39	These unprecedented changes should be characterized as evidence of tipping points, via increased sensitivity to perturbation as tipping points are approached, as per "critical slowing down" in AR6 WG1 pg.56 line 38 [ Kelly Wanser, United States of America]	Rejected. Unprecedented changes are not synonymous with tipping points and other chapters are charged to assess these.
31503	80	35	80	39	Can we say this is highly unusual and highly marked, when wbove you say that current OHC rate of change is similar to what has been seen at the end of the Younger Dryas (medium confidence).  This is just a naïve question. I am just unsure how to handle that information of similar OHC rate for end of YD. [ Jean-Baptiste SALLEE, France]	Rejected. Highly unusual covers all aspects suggested by the reviewer here to our view.
93505	80	39		40	Remove ) (...in between 3rd and 4th references... [ Rahab KINYANJUI, Kenya]	Not applicable. Misplaced comment. Two words present in text identified and no citations.
116015	80		80		Confusing use of the term "components" and the term "state" here (level rather than state for a given variable, state for a component of the climate system = ocean?). I am also confused about references to "fast varying" and "slow varying" components or indicators. Please revisit the whole paragraph lines 27 to 39. [ Valerie Masson-Delmotte, France]	Taken into account. Edits applied in line with this comment.
100625	81	6	81	6	This same material at virtually the same level of specificity can be summarized for the MCO. While the Pliocene is a good example of higher than PI levels of pCO <sub>2</sub> , the MCO is a good example of higher than present levels of pCO <sub>2</sub> . [ Matthew Kohn, United States of America]	Noted; MCO added as a paleo reference period in CH2, including CCB2.1
100587	81	6	81	6	Note: the same kind of information can be gathered for the Miocene. I can't provide that within the current timeline, however. [ Matthew Kohn, United States of America]	Noted; MCO added as a paleo reference period in CH2, including CCB2.1
575	81	7	83	56	In Cross-Chapter Box 2.4, it would be interesting to include an item related to Atlantic Ocean variability and modes, as discussed by Pontes et al. (2020). They examined Atlantic SST variability during the Mid-Pliocene Warm Period (MPWP) results show all Atlantic modes had their frequencies shifted towards lower values. The Atlantic modes also had their spatial structure changed due to the altered meridional SST gradient. Full reference: Pontes, G.M.; Wainer, I.; Prado, L.F.; Brierley, C. (2020). Reduced Atlantic variability in the Mid-Pliocene. Climatic Change, doi: 10.1007/s10584-020-02675-9. [ Luciana Figueiredo Prado, Brazil]	Rejected; agreed that Atlantic variability is of interest; however, priority features of climate considered in the Cross-Chapter Box include multiple lines of evidence, not only model simulations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23869	81	15	81	31	Since the overall report is so epic, much talkative and self-repeatitive, why not to put general time-borders to various geological epoches, such as Pleistocene and more? [ Branko Grisogono, Croatia]	Taken into account; ages of geological epochs discussed in text are stated.
18323	81	15	83	56	Please define the Pliocene Epoch using a specific time period (e.g., xx-yyMa before present) in the first first sentence of this insert box. While the Pliocene climate may be in equilibrium with its atmospheric CO <sub>2</sub> , the current climate is not! That is, people are comparing an equilibrium climate with a transient climate that have similar CO <sub>2</sub> levels. Clearly, they are not comparable quantitatively, although our recent work (Huang et al. et al. 2020) suggests that the change patterns (normalized by the global-mean temperature change) are likely similar for the equilibrium and transient response to a CO <sub>2</sub> increase. Another problem for comparing today's climate with the Pliocene climate is that today's climate is only about 1K warmer than the preindustrial climate, while the middle Pliocene climate is 1.9-3.6K warmer than the preindustrial climate, even though today's CO <sub>2</sub> level has already exceeded the upper limit of the estimated CO <sub>2</sub> level for the middle Pliocene. In my opinion, these two major differences should prevent us from using the middle Pliocene as a true analog for today's climate or the model projected climate in the 21st century. One might argue that the final equilibrium warming of today's CO <sub>2</sub> level would lead to larger warming than observed. While that is true, it won't double the observed warming and we need to wait another 2000 years to get that remaining warming in order to be comparable to the Pliocene equilibrium climate. In any case, these major differences should caution any efforts to directly compare today's climate to that of the middle Pliocene. Relevant Ref.: Huang, D., A. Dai, and J. Zhu, 2020: Are the transient and equilibrium climate change patterns similar in response to increased CO <sub>2</sub> ? J. Climate, revised. [ Aiguo Dai, United States of America]	Taken into account; (1) "Pliocene" is defined in the Glossary and is shown graphically in CCB2.1, Figure 1. (2) Omitted the term "analogue". (3) Implications for climate sensitivity discussed in CH7.
30479	81	15			'Plio: more; cene: recent' (invert colon-semi colon) [ Gilles Delaygue, France]	Accepted, fixed
29881	81	16	81	16	Unbalanced parenthesis in "(e.g.,". [ Hernan Edgardo Sala, Argentina]	Editorial; copyedit to be completed prior to publication.
13281	81	17	81	17	Missing or extra () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
73721	81	17	81	17	Delete ( before Haywood and insert ( before 2016. Add second ) after end ) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73723	81	19	81	19	Delete , after cryosphere [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, fixed.
127111	81	25	81	28	The cited paper, Burke et al. (2019, PhilTransB), only mentions the Pliocene briefly in the introduction and doesn't contain an assessment of analogous climates. More generally, it's difficult to see how the MPWP, which is an equilibrium climate, can be at all analogous to a climate undergoing rapid change under RCPs 4.5 and 8.5. Do authors mean to make the more limited statement that radiative forcing was most analogous to the predicted RCP climates? [ Trigg Talley, United States of America]	Taken into account; (1) Omitted the term "analogue". (2) Omitted reference to Burke et al. (3) Implications for climate sensitivity discussed in CH7.
4725	81	26	81	28	"Introduction paragraph: the authors highlight the recent work using MPWP constrain on the future climate (Sun et al. 2013; Sun et al. 2018; Burke et al., 2019). In addition to surface climate, two more works also target this issue. Within the PlioMIP framework, tropical circulation and regional monsoon climate were investigated by performing a direct comparison between MPWP and RCP4.5 (Sun et al. 2013; Sun et al. 2018). References Sun Yong, Ramstein Gilles, Contoux Camille, and Zhou Tianjun, 2013: A comparative study of large-scale atmospheric circulation in the context of a future scenario (RCP4.5) and past warmth (mid-Pliocene). Clim. Past, 9, 1613-1627. Sun Yong, Ramstein Gilles, Li Laurent. Z X, Contoux Camille, Tan Ning, and Zhou Tianjun, 2018: Quantifying East Asian summer monsoon dynamics in the ECP4.5 scenario with reference to the mid-Piacenzian warm period. Geophysical Research Letters, 45, 12523-12533." [ Yong Sun, China]	Rejected; agreed that tropical circulation is of interest; however, priority features of climate considered in the Cross-Chapter Box include multiple lines of evidence, not only model simulations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
30483	81	27	81	28	'the most analogous to predicted climate under scenario RCP8.5 by 2030 and under RCP4.5 by 2040': as such this claim is both misleading and wrong, for several reasons (i give 3 below). This criticism applies as well to the Burke et al. (2018) paper, which to me has not been properly reviewed. I understand that it is not the role of IPCC editors to review published papers, however it is their role to screen the most relevant publications. One reason for which this claim is wrong is that Burke et al. only consider temperature and precipitation on the continents to define their 'climate analogue'. There is no way by which continental T & P could define 'global climate' in an IPCC AR. (The reason Burke et al did not use SSTs, for instance, is that SSTs are, indeed, completely different from ones expected in 2030 or 2040.) Another reason for which this claim is wrong is that MPWP is found the closest analogue to RCP8.5 in 2030 only with one model simulation (CCSM) among the 3 tested simulations (see their fig.2). The reason why this claim is misleading is that Burke et al. considered warm climatic scenarios of CCSM which won't happen in reality because the real climate has not been following these trajectories. MPWP is a very interesting analogue for the future climate and environment of the Earth in few millennia, not in 10 yrs. [ Gilles Delaygue, France]	Taken into account; (1) Omitted the term "analogue". (2) Omitted reference to Burke et al.
30481	81	28	81	28	Burke et al 2019 do not address Pliocene but LGM period: change the reference to Burke et al 2018 DOI:10.1073/pnas.1809600115 [ Gilles Delaygue, France]	Taken into account; omitted reference to Burke et al.
29541	81	28	81	28	The citation of Burke et al., 2019 is incorrectly inserted here. Burke et al., 2018 is the proper citation: Burke KD, Williams JW, Chandler MA, Haywood AM, Lunt DJ, Otto-Bliesner BL. 2018 Pliocene and Eocene provide best analogs for near-future climates. Proc. Natl. Acad. Sci. USA 115, 13 288–13 293. (doi:10.1073/pnas.1809600115) [ Kevin Burke, United States of America]	Taken into account; omitted reference to Burke et al.
43133	81	29		30	Read "(Haywood et al., 2013; McClymont et al., 2020)," rather than "(Haywood et al., 2013) (McClymont et al., 2020)," [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
4201	81	30	81	30	Reference has since been published so this can be updated. [ Emily Dearing Crampton Flood, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, fixed.
9949	81	30	81	30	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13283	81	30	81	30	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
2021	81	30	81	30	remove open and close brackets between two references. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73725	81	31	81	31	Move 'more confidently' to after 'assessed'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; section rewritten.
41567	81	31	83	56	good presentation of what makes similar climatically MPWP to RCP 8.5... But what do we know for that period which could have make it different, why it was warmer (briefly, what was before, and how forcing factors could have evolved..)? (no prior development of major ice sheets ice sheets, warmer deep ocean, low and high altitude albedo smaller? One again, this chapter must provide hard data and uncertainties, but also science and background for understanding processes. [ Laurent Labeyrie, France]	Taken into account; additional information about MPWP paleo reference period, including antecedent conditions, is given in CCB2.1
8921	81	37	81	37	Provide an associated uncertainty from the ensemble range? [ Robert Kopp, United States of America]	Accepted; updated to include ensemble range.
13285	81	39	81	39	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
2023	81	39	81	39	remove open and close brackets between three references. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73727	81	39	81	39	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
43135	81	39		40	Read "(Martinez-Boti et al., 2015b; Foley and Dowsett, 2019; McClymont et al., 2020). " rather than "(Martinez-Boti et al., 2015b; Foley and Dowsett, 2019) (McClymont et al., 2020). " [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
30485	81	39			Martinez-Boti et al 2015 a & b are the same reference [ Gilles Delaygue, France]	Accepted, fixed.
57765	81	40			"global average temperature" is unclear as the report trying to separate GMST and GSAT in the assessment. It should be specified in this context. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; changed to "global mean surface temperature".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42895	81	41			The scaling factor of 1.6 specifically for the Pliocene comes from Snyder et al 2016; Fischer et al only proposed we use the same scaling for other warm periods such as LIG. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; land-sea temperature contrast factor used to translate global SST to GMST has been re-calculated based on model simulations (Figure 3.1).
8923	81	42	81	42	(2-3°C) x 1.6 = 3.2-4.8°C [ Robert Kopp, United States of America]	Taken into account; the land-sea temperature contrast was re-evaluated in Chapter 3.
99237	81	47	81	50	The section here raises, very valid, needs for improvement of our understanding of paleo proxies for pH. The main section earlier though discussing pH and CO2 reconstructions, lacks this important reference to uncertainties in the conversion. It is important that the same caution is expressed in the text outside the box [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; second paragraph of section 2.3.3.5 cites multiple paleo pH studies and section 2.2.3.1 discusses accuracy of CO2 proxy indicators.
30487	81	47			'uncertainties ON the boron-isotopic composition of seawater' (otherwise it has no sense) [ Gilles Delaygue, France]	Accepted, as suggested.
58039	81	48	81	48	Something went wrong with citing the paper by Sosdian et al. 2018. First of all the date is missing and secondly 'Lear' does not belong to the source. It should be Sosdian et al., 2018. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
73729	81	48	81	49	References should be in chronological order. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30489	81	48			'the second carbonate system parameter': what is that? [ Gilles Delaygue, France]	Taken into account; omitted text.
8925	81	51	81	51	What is the uncertainty in the 385 ppm estimate? [ Robert Kopp, United States of America]	Accepted; added range of CO2 estimates for MPWP.
35555	81	54	81	54	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial; copyedit to be completed prior to publication.
116017	81		83		I think that this box could be shortened by at least half a page. Missing description of criteria to define what is an analogue and deviations from these criteria (geography, time scale of response, exact orbital configuration etc). Role of other aspects of atmospheric composition (CH4, dust, aerosols) not described. Sharpen the description of evidence (SST, LSAT, GSAT, GMST), use of proxy based information vs use of model results. Provide uncertainty ranges for estimates if possible. The text is a description of studies, rather than an assessment of what is known and what are the limitations. Please avoid using the term "demonstrates". The statements about aerosol cloud interactions are cryptic, please help the reader understand what is meant. Please also explain how Pliocene simulations differ from those of future climate or idealized long term equilibrium simulations with today's CO2 level (missing). It is hard to understand what is new compared to what was known at the time of AR5 which could be used as a starting point (new evidence, new simulations). What about CMIP6 here? (are model results described here based on CMIP5? what is the implication of models with a higher sensitivity for Pliocene model data comparisons?). The description of GMSL changes is hard to follow and reasons for differences in AR5, SROCC, here not clearly explained (linked to methods and results in the literature assessed). [ Valerie Masson-Delmotte, France]	Accepted: (1) Shortened box considerably by moving assessment of major indicators into main text of CH2 and calling out assessments in other section, where information about what was known at time of AR5 is given; (2) specified GSAT vs GMST and proxy vs model results; (3) omitted word, "demonstrates". Taken into account: (1) omitted use of term "analogue"; (2) role of aerosols and clouds in models, along with model-specific information is in CH7, now cited. Rejected as out of scope for this limited treatment: (1) explanation of Pliocene simulations; (2) role of other aspects of atmospheric composition.
26661	82	7	82	8	Please check the "high confidence" since many meridional computations are based on alkenone records situated in upwelling regions, and in the Fedorov 2013 Nature article they are all derived from alkenones in eastern boundary currents, which are subsequently subtracted to Mg/Ca records from elsewhere to compute meridional gradients. Yes, multi-proxy SST records in the Benguela system indicate that both SST proxies are far from recording the same temperature, probably because of more seasonally sporadic events of upwelling (see Leduc et al., 2014, G-cubed), challenging this "high confidence" assigned to reduced meridional temperature gradients. At least in eastern boundary upwelling regions, multiple proxies still need to be analyzed to validate such statement. [ Eric Brun, France]	Not applicable; temperature gradient assessed in CH7.
58041	82	9	82	9	The McClymont et al, 2020 should be within the first brackets of the other cited papers. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
35557	82	9	82	10	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial; copyedit to be completed prior to publication.
43137	82	9		10	Read "(e.g., Dowsett et al., 2019; Fedorov et al., 2015; Salzmänn et al., 2013; Tierney et al., 2019; McClymont et al., 2020)." rather than "(e.g., Dowsett et al., 2019; Fedorov et al., 2015; Salzmänn et al., 2013; Tierney et al., 2019) (McClymont et al., 2020)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73731	82	10	82	10	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
9951	82	10	82	10	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13287	82	10	82	10	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
2027	82	10	82	12	"A reduced meridional temperature gradient is also demonstrated through climate simulations using boundary conditions appropriate to the MPWP (Haywood et al., submitted)". I would consider removing this from here, and adding it to the next paragraph on model simulations. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; temperature gradient assessed in CH7.
73733	82	11	82	11	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
58043	82	11	82	11	The Haywood et al., submitted paper should be with the brackets of Zhu et al., 2019 e.g. (Zhu et al., 2019; Haywood et al., submitted). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
2025	82	11	82	11	Zhu et al is an eocene paper, not pliocene. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, omitted citation.
13289	82	11	82	11	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
35559	82	11	82	42	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Taken into account; only published sources are used.
43139	82	11		12	Read "(Zhu et al., 2019; Haywood et al., submitted, a)." rather than "(Zhu et al., 2019) (Haywood et al., submitted, a)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
30491	82	11			Zhu et al., 2019 report on PETM simulation, not MPWP [ Gilles Delaygue, France]	Accepted, omitted citation.
58045	82	12	82	13	The McClymont et al, 2020 should be within the first brackets of the other cited papers. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
43141	82	12		13	Read "(Bachem et al., 2016; Sánchez-Montes et al., 2019; McClymont et al., 2020)," rather than "(Bachem et al., 2016; Sánchez-Montes et al., 2019) (McClymont et al., 2020)," [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
73735	82	13	82	13	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
9953	82	13	82	13	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13291	82	13	82	13	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
5425	82	26	82	27	Box 2.4,F. 1a) does not show increase precipitation over much of the westerlies of either hemisphere. The enhanced tropical precipitation and diminished midlatitude values imply a strengthened Hadley. How is this reconciled in the citations? [ Bryan Weare, United States of America]	Noted; there is a clear positive precipitation anomaly in the mid-latitudes of both hemispheres, but it is maximized in the westerly's during the winter months. Added "winter" to text to clarify. The figure, however, shows MAP, not winter. Hadley Cell is weaker the PI in both PlioMIP1 and PlioMIP2.
73737	82	27	82	27	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
4727	82	27	82	27	"Meridional temperature gradients and sea ice paragraph: the existing work of Sun et al. (2013) on weakening of Hadley circulation during MPWP suggested to be involved the following sentences. The reduced meridional temperature gradient led to a simulated poleward shift in the position of the westerlies (Li et al., 2015a), with models predicting enhanced precipitation within the westerlies, and a weakening in the Hadley Circulation (Sun et al. 2013) (Haywood et al., submitted, a) (Corvec and Fletcher, 2017)). Reference Sun Yong, Ramstein Gilles, Contoux Camille, and Zhou Tianjun, 2013: A comparative study of large-scale atmospheric circulation in the context of a future scenario (RCP4.5) and past warmth (mid-Pliocene). Clim. Past, 9, 1613-1627." [ Yong Sun, China]	Taken into account; temperature gradient assessed in CH7.
9955	82	27	82	27	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13293	82	27	82	27	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
43143	82	27		28	Read "Hadley Circulation (Haywood et al., submitted, a; Corvec and Fletcher, 2017 v)." rather than "Hadley Circulation(Haywood et al., submitted, a) (Corvec and Fletcher, 2017))." [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
127113	82	28	82	30	This sentence is misplaced and belongs in the hydrological cycle section on page 83. [ Trigg Talley, United States of America]	Taken into account; text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23871	82	28	82	35	Again and again, a bad concoluted, self-oriented style, like "old boys network" - citing unpublished work, perhaps science. The whole 6th IPCC report is full of that. It makes me sad, as a scientist, and more... [ Branko Grisogono, Croatia]	Not applicable; out of place comment? line numbers cross random paragraphs.
23875	82	28	82	35	In the comment 21, it was meant to say, '...a bad convolution...' - I believe that this report should have been a much better work if not so many bureaucrats and administrative people have been involved in. Of course that 10-15 % of these sci. writers are excessively scillful and write each day 100-200 pages, but that is most often a deluted content. [ Branko Grisogono, Croatia]	Not applicable; out of place comment? line numbers cross random paragraphs.
73739	82	29	82	29	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30493	82	29			'multi-model mean INCREASE of 0.17 mm day <sup>-1</sup> ' (?) [ Gilles Delaygue, France]	Taken into account; text revised.
2029	82	32	82	34	Could make a reference to Chapter 7 where the Pliocene model latitudinal gradianet is illustrated in a figure. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; section in CH7 called out.
73741	82	35	82	35	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
9957	82	35	82	35	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13295	82	35	82	35	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
43145	82	35			Read "(Otto-Bliesner et al., 2017; Dowsett et al., 2019; Haywood et al., submitted, a)" rather than "(Otto-Bliesner et al., 2017; Dowsett et al., 2019) (Haywood et al., submitted, a)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
73743	82	42	82	42	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
9959	82	42	82	42	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13297	82	42	82	42	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
43147	82	42			Read "(Hopcroft, submitted; Kageyama et al., 2018)," rather than "(Hopcroft, submitted) (Kageyama et al., 2018)," [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
2031	82	45	83	2	Definitely check this for consistency with Chapter 7 which also has an assessment of pliocene Pacific tropical SST gradients. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; CH7 now called-out for the primary account of Pacific SST gradients
68051	82	45	83	9	I think this needs clarification for context, by results that inform on the mean state vs the interannual variability. Also: distinguish what simulations provide as testable hypotheses (what is expected when models are forced, and unforced), for simulation of ENSO, by models which are skillful with ENSO). If I am not mistaken, much of this discussion and the citations are informative primarily about the observed and simulated mean state, e.g. conditions averaged over longer than decades. Clarify. This summary should also include: historical period (19th and 20th century) estimates based on VOS data, e.g. Chen et al (2004); also annually resolved paleoclimatic reconstructions, e.g. Tierney et al (2015); and estimates over the Common Era (Steiger et al 2018). I think most of these do not show a change in the zonal T gradient; most do not show a warming in the eastern equatorial Pacific; for the paleoclimatic data, we are limited by sparse annually resolved data from the eastern equatorial Pacific, especially before the 19th century. Calibration and reproducibility of decadal variability in all of the data sources is difficult and not as well validated as for interannual variations, in which small amounts of change from decade to decade could explain what we observe (Evans et al 2001; Newman et al 2003; others). Also, it is thought that relative chronology uncertainty, e.g. that which accumulates from layer or band counting in annually resolved records, results in more uncertainty for detection of decadal variations (e.g. Comboul et al 2015, Hu et al 2017; Lawman et al 2020). The latter should be acknowledged here as a constraint on our ability to detect variance changes between decades, although as the review shows (section 2.4.2), there are independent sources of evidence for this, from observations, reconstructions and simulations. [ Michael Evans, United States of America]	Taken into account; the revised version distinguishes between the discussion of the mean state (tropical Pacific gradient) and ENSO sensu stricto. This box is focused on the Pliocene, so discussion of last millennium results is not included here.
73745	82	54	82	54	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
58047	82	54	82	54	The McClymont et al, 2020 should be within the first brackets of the other cited paper. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
9961	82	54	82	54	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13299	82	54	82	54	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
43149	82	54			Read "(e.g., Tierney et al., 2019; McClymont et al., 2020)" rather than "(e.g., Tierney et al., 2019) (McClymont et al., 2020)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
9223	83	2	83	3	Suggest adding the following highly relevant results: Changes in Pacific SSTs and SST gradients have resulted in variations of the Aleutian low, Pacific jet stream, and ENSO-related teleconnections, as well as their impacts on regional climates (Gan et al., 2017; Soulard et al., 2019; Yu and Lupo, 2019). References to add: Gan, B., and co-authors, 2017: On the response of the Aleutian low to greenhouse warming. J. Clim., 30, 3907– 3925. Souillard N., H. Lin, and B. Yu, 2019: The changing relationship between ENSO and its extratropical response patterns. Sci. Rep., doi:10.1038/s41598-019-42922-3. Yu B., and A. Lupo, 2019: Large-scale atmospheric circulation variability and its climate impacts. Atmosphere, 10, 329, doi:10.3390/atmos10060329. [ Bin Yu, Canada]	Rejected; these studies are all related to modern climatology, primarily based on models and nothing involving the Pliocene.
54921	83	2			Suggest adding the following relevant results:  Changes in Pacific SSTs and SST gradients have resulted in variations of the Aleutian low, Pacific jet stream, and ENSO-related teleconnections, as well as their impacts on regional climates (Gan et al., 2017; Soulard et al., 2019; Yu and Lupo, 2019).  References to add: Gan, B., and co-authors, 2017: On the response of the Aleutian low to greenhouse warming. J. Clim., 30, 3907– 3925. Souillard N., H. Lin, and B. Yu, 2019: The changing relationship between ENSO and its extratropical response patterns. Sci. Rep., doi:10.1038/s41598-019-42922-3. Yu B., and A. Lupo, 2019: Large-scale atmospheric circulation variability and its climate impacts. Atmosphere, 10, 329, doi:10.3390/atmos10060329. [ Nancy Hamzawi, Canada]	Rejected; these studies are all related to modern climatology, primarily based on models and nothing involving the Pliocene.
100627	83	5	83	5	Note: the primary reference to the permanent El Niño state would be either Cane and Molnar (2001; Nature) or Molnar and Cane (2002; Paleoceanography) [ Matthew Kohn, United States of America]	Accepted; added reference.
30495	83	6			'El Niño collapsed': sentence seems incomplete and at odd with above claim of a 'Permanent El Niño state'. Not clear what is meant here; maybe replace with 'El Niño variability collapsed' or 'ENSO collapsed' ? [ Gilles Delaygue, France]	Taken into account; clarified text.
4729	83	12	83	17	"Hydrological cycle paragraph: in addition to tropical and subtropical precipitation changes, the authors reviewed regional monsoon precipitation changes during MPWP. It seems the mechanism study of regional monsoon precipitation during MPWP is not involved. Sun et al. (2016) concluded the intensified Asian monsoon precipitation under thermodynamic control. In addition, the following sentence demonstrate the regional drying, but not involve subtropical drying to poleward shift of Hadley circulation. Higher lake levels in the southwestern United States suggest wetter conditions in this region during the Pliocene (Ibarra et al., 2018), which is inconsistent with zonal mean condition of subtropical drying in conjunction with poleward shift of Hadley circulation (Sun et al. 2013) References: Sun Yong, Zhou Tianjun, Ramstein Gilles, Contoux Camille, and Zhang zhongshi, 2016: Drivers and mechanisms for enhanced summer monsoon precipitation over East Asia during the mid-Pliocene in the IPSL-CM5A. Climate Dyn., 46,1437-1457. Sun Yong, Ramstein Gilles, Contoux Camille, and Zhou Tianjun, 2013: A comparative study of large-scale atmospheric circulation in the context of a future scenario (RCP4.5) and past warmth (mid-Pliocene). Clim. Past, 9, 1613-1627. " [ Yong Sun, China]	Taken into account; deleted statement about SW U.S. lake levels; Rejected; multi-model syntheses prioritized over single-model studies.
127115	83	14	83	16	It is not obvious why wetter conditions in the Southwest U.S. would be inconsistent with mean subtropical drying. Drying on average doesn't imply drying everywhere. [ Trigg Talley, United States of America]	Taken into account; deleted statement about SW U.S. lake levels.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127117	83	18	83	19	Should be "average simulated global tropical cyclone intensity..." [ Trigg Talley, United States of America]	Accepted; added "simulation".
57639	83	20			This section on the hydrological cycle would benefit from a summary and assessment of the confidence of the impacts on the hydrological cycle [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; added that precipitation rate was higher (high confidence) and monsoon systems were stronger (medium confidence).
35291	83	22	83	40	<p>The line of evidence provided by field data on geomorphic imprints of past sea levels is preactically ignored here. I put here a brief outline of the main works in this field, that might be useful to draft one or two lines to address this. For full disclosure, I authored some of these works, so please cross-check their relevance within the context.</p> <p>The issues of reconstructing precise MPWP sea level histories resides in three main problems: 1) finding well-preserved and precise RSL indicators; 2) date them with enough accuracy; 3) Correct them for post-depositional movements due to GIA, tectonics or dynamic topography (Raymo et al., 2011; Rovere et al., 2014). A sequence of works done in the East Coast of the US focuses on these problems. Rowley et al. (2013) and Rovere et al. (2015) mapped sea level indicators in the field and attempted GIA and dynamic topogrpahy modelling. Moucha and Ruetenik (2017) combined these data and models within a landscape evolution model to show that the wide scarp marking MPWP sea level in the US Atlantic Coastal Plain is most consistent with a paleo sea level 15m above present. This datum seems consistent with precise sea level indicators surveyed in the caves of Mallorca, Spain. After correction for GIA and post-depositional effects, Dumitru et al. (2020) obtained that these indicators mark sea level at 3.27 +/- 0.12 Ma at 16.2m (16th-84th percentiles of 5.6m-19.2m).</p> <p>Rowley, D.B., Forte, A.M., Moucha, R., Mitrovica, J.X., Simmons, N.A. and Grand, S.P., 2013. Dynamic topography change of the eastern United States since 3 million years ago. science, 340(6140), pp.1560-1563.</p> <p>Rovere, A., Raymo, M.E., Mitrovica, J.X., Hearty, P.J., O'Leary, M.J. and Inglis, J.D., 2014. The Mid-Pliocene sea-level conundrum: Glacial isostasy, eustasy and dynamic topography. Earth and Planetary Science Letters, 387, pp.27-33.</p> <p>Moucha, R. and Ruetenik, G.A., 2017. Interplay between dynamic topography and flexure along the US Atlantic passive margin: Insights from landscape evolution modeling. Global and Planetary Change, 149, pp.72-78.</p> <p>Rovere, A., Hearty, P.J., Austermann, J., Mitrovica, J.X., Gale, J., Moucha, R., Forte, A.M. and Raymo,</p>	Taken into account; expanded on text and assessment of MPWP global mean sea level; moved to main CH2 text.
30497	83	26			'Antarctic marine-based ice': not clear/too technical expression [ Gilles Delaygue, France]	Accepted, clarified.
73747	83	28	83	29	References should be in chronological order [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
30499	83	35	83	36	'SROCC had low confidence that GMSL did not exceed 25 m': not clear, does it imply that SROcC had high confidence that GMSL did exceed 25 m? [ Gilles Delaygue, France]	Accepted, clarified.
35293	83	39	83	40	See my description of field data above. Probably the lower end can be raised to 10m? [ Alessio Rovere, Germany]	Rejected; New estimate places 5.6 m lower bound as the 16th percentile.
73749	83	47	83	47	Insert '(MPWP)' after 'Period' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; as suggested.
105101	83	47	83	56	It is a pity that these figures do not show actual data. But maybe this will be the case for the next draft, since this is a place holder. [ Masa KAGEYAMA, France]	Accepted; proxy data now plotted for marine and terrestrial sites, and precipitation plotted for terrestrial sites.
73751	83	51	83	51	Change 'outies' to 'outlines'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; text omitted.
57641	83	56	83	57	Lacking confidence statement. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; number of models in agreement indicates confidence.
30501	83	56			'Antarctica' [ Gilles Delaygue, France]	Accepted, fixed.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112637	84	1	91	40	This section is missing an appraisal of the Quasi-biennial Oscillation: the most regular internal mode of interannual climate variability. The QBO dominates the tropical stratosphere and has well known links to the high latitudes (e.g. polar vortex and Northern Europe) and tropical troposphere (e.g. Madden-Julian Oscillation). Furthermore, the amplitude of this mode of natural variability has systematically changed over recent decades (Kawatani and Hamilton, 2013) and has shown recent fundamental change via disruptions in 2016 (Newman et al, 2016; Osprey et al, 2016) and in 2020 (Anstey et al, submitted). The fundamental change to this natural mode of variability is likely due to climate change and will pre-occupy the stratospheric research community for years to come. [ Scott Osprey, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. QBO is not included in chapter (result of x-chapter and intra-chapter discussions and decision).
67673	84	1	91	41	The QBO is a major mode of stratospheric variability, and is missing here. It has experienced 2 disruptions in recent times (one ongoing right now). It should be mentioned. It is mentioned in regards to stratospheric water vapour, so does play a role in one of the topics discussed in this chapter. It should also be discussed in modes of atmospheric variability. [ Karen Rosenlof, United States of America]	Rejected. QBO is not included in chapter (result of x-chapter and intra-chapter discussions and decision).
5427	84	6	91	39	Each sub section needs an introductory sentence or two describing the phenomenon and its importance. Often too much space is spent on things which are either relatively unimportant or lead to lower confidence conclusions. [ Bryan Weare, United States of America]	Rejected. Detailed definitions of the modes of variability is given in the Technical Annex. This is referred to in the general introduction to the modes, but space does not allow repetition for every mode.
84167	84	6	91	39	For each mode the Annex VI should be referenced. In that annex all the modes considered in the whole report are described (this would be useful also in perspective of interactive pdf). Also the terminology of the modes should be aligned with those used in the annex (as agreed at LAM3) [ Annalisa Cherchi, Italy]	Accepted. The discussion of each mode now refers to the Technical Annex on Modes of Variability.
127119	84	6	91	41	Would be helpful to have an overall synthesis of the assessments in Section 2.4 similar to what was done in 2.3. [ Trigg Talley, United States of America]	Rejected. The Executive Summary on modes of variability provides such a synthesis. Any similar section would be repetitive.
41569	84	8	84	13	What is known and not known (concepts and models) on variability and interactions between modes should be emphasized briefly, in particular in relation to blockades and extremes. I was surprised also not to find some elements on forcings of variability in atmospheric and oceanic dynamics, as the mountains of Nevada for the northern jet stream and European climates and southern caps for the circumpolar waters [ Laurent Labeyrie, France]	Taken into account. The gaps in knowledge are discussed in section 2.5. More detailed discussions on the modes of variability are provided in Technical Annex VI.
58049	84	20	84	21	Abbreviation for the NAM and NAO are mentioned in the header (2.4.1.1), therefore in line 20 only the abbreviation of the NAO needs to be mentioned. Otherwise it's inconsistent with line 21, where only the NAM is mentioned and not the full name. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The abbreviation for NAM and NAO was provided in the header of the section. For the rest of the section we referred to the acronyms.
535	84	20	84	22	"offset" does not seem to be the right word to use at the start of line 22, given the shift in time periods; perhaps it should be replaced by "reversed" or "halted" or another more appropriate word. [ Claire Parkinson, United States of America]	Taken into account. AR5 indicates that "Confidence is high that the increase in the NAO index from the 1950s to the 1990s has been largely offset by recent changes". However, according to your suggestion we used the word "reversed".
30503	84	21			'from the 1960s' [ Gilles Delaygue, France]	Rejected. AR5 indicates that "Confidence is high that the increase in the NAO index from the 1950s to the 1990s has been largely offset by recent changes".
70265	84	28	84	28	Maybe change wording to be more definitive, several indicates more than two, so it seems odd to be talking "the presence of a significant 1500-year periodicity" in what may only be two millennia of data. [ Shayne McGregor, Australia]	Taken into account. The sentence was changed according to your suggestion.
30505	84	28	84	29	'NAO variability': Darby et al 2012 do not address NAO variability; they analysed a sediment core located on the Alaskan Chukchi margin (Deser 2000 did show that AO=NAM and NAO have different variabilities, especially that the centers of actions in the N. Pacific and Atlantic are not correlated) [ Gilles Delaygue, France]	Noted. The reference was retained considered what is described in the Technical Annex IV, section AIV.2.1 regarding the similarities between the NAO, NAM and AO.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1955	84	31	84	31	Some reconstructions suggested a strong positive phase of the NAO during the MWP but this has been strongly moderated by more recent and better validated reconstructions (Ortega et al. 2015). [ Hugues Goosse, Belgium]	Taken into account. The sentence was changed according to your suggestion.
58051	84	31	84	31	Only in the Trouet et al. 2009 paper a strong positive NAO phase for the MWP is mentioned, therefore, in order to be consistent with the other two papers (Olsen et al. 2012, Faust et al. 2016) I'd suggest to use 'presistent positive phase' instead of 'strong positive phase'. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence was changed according to your suggestion.
1583	84	31	84	38	The strong positive NAO during the MWP from the Trouet et al (2009) doesn't make any climatological sense (unless the influence of the NAO completely changed from what it has done since instrumental records, so the 1700s). During the MWP, northwest Europe had many mild winters, but SW Greenland should have been very cold. Despite that a few Norse settled there. Winters would have been really cold, but maybe summers were warm, so they could survive the next winter. The interannual variability of the NAO can't have changed to constantly one phase in the MWP. AR6 should be an Assessment not a review of what's been done. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference was removed from the assessment performed in the section.
30507	84	31			'Reconstructions show a strong positive phase for the MWP': Ortega et al 2015 (cited here) do not find such a positive phase [ Gilles Delaygue, France]	Noted. The references for the sentence do not include the paper of Ortega et al. (2015): "Reconstructions show a strong positive phase for the MWP (Trouet et al., 2009; Olsen et al., 2012; Faust et al., 2016)." We added the word "Several" before reconstructions to clarify that not all the papers document this NAO phase. By the time of submitting the FGD, the sentence indicates no dominant NAO phase during 1000–1300 CE.
73753	84	35	84	35	Change 'centuries' to 'Centuries' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial - copyedit to be completed prior to publication.
30509	84	35			'constructed from the difference in sea level pressure between two sites': i think this sentence is too loose in relating SLP to NAO, for 2 reasons. 1. Those two sites must be located at or near the so called 'centers of action' (max. of SLP variability and max. of correlation), and 2. movement of these centers of action (esp. seasonally) is not captured by using fixed sites. Hence, i suggest to 1. complete the sentence, and 2. distinguish works with spatial extent which enables them to address - or not- the full NAO variability. For instance, Cornes et al 2013 recognize that their index captures only part of the traditional NAO index, due to the location of London and Paris. On the contrary, Cropper et al. work is based on 2 sites at the centers of action. Also, works based on reconstructing large scale SLP field should more reliably get the full NAO variability: this is the case for Mellado-Cano et al 2019 (geostrophic SLP), and for Delaygue et al 2019 (doi 10.1007/s00382-018-4506-7) based on combining multiple SLP series. [ Gilles Delaygue, France]	Taken into account. The sentence was modified according to your suggestion. However, we decided to add more details about the NAO reconstructions using SLP differences in the Technical Annex IV, section AIV.2.1.
73755	84	40	84	40	Delete 'season' (winter is a season). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was rephrased and now there is no need for deletion of the word season
37097	84	54	84	55	This sentence is deceptive. The increase in El Nino events came after mid-1976. This is easy to identify by simply aggregating the Troup SOI values in each month from January 1950 or even January 1961. The aggregated values are unimportant; it's the trend in those values that is. If one uses the Troup index (+ve for the La Nina side of absolutely neutral and -ve for th El Nino side), the trend is predominantly upwards from the start of data until June 976 and then after June 1976 it is downwards. This is so trivial that it shouldn't need a paper to point it out. [ John McLean, Australia]	Not applicable. The comment is not linked to the review performed in the section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57751	85	1	85	3	In Section 2.4.1.1 I missed seeing a sentence explaining the main consequences of the observed NAO behavior. This is well done in section 4.1.2 (page 85, lines 37-38) for the SAM. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The SAM has been shifted towards a more positive phase, which is unprecedented over the last several centuries. Therefore, we could infer persistent changes in the circulation patterns. On the other hand, the NAO was dominated by large interannual to multidecadal variabilities, shifting from positive to negative phases without a clear trend. In this sense, the consequences of the observed NAO behaviour depict the oscillatory characteristic of this mode of variability. This is why it was not included in the section.
80275	85	6	85	43	I don't see any mention of possible impact of Antarctic ozone depletion on SAM, although such impact has been documented and assessed (WMO, 2018). [ Sophie Godin-Beekmann, France]	Noted. Assessment of attribution is the purview of Chapter 3 and is not covered here.
99341	85	11	85	12	It is worth noting the error's (at 1 sigma) on the moraine ages are +/- 800 years for the older phase and +/- 160 for the onset of the second phase, meaning at 95% confidence the uncertainties on this are between 3000 and 600 years. There are much higher resolution lake records with significantly reduced uncertainties over this period (e.g. Moreno 2018, cited in Reynhout) that put a more precise timing on these two periods but also show much greater variability than just these two episodes. [ Simon Blockley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We included the findings of Moreno et al (2018) and the new evaluation from Hernández et al. (2020) to characterize the SAM variability across the Holocene.
57753	85	18	85	20	I think that at "lies outside the 5-95% range of the past 1 kyr" is not the same as what the paper states: "lie outside 5–95% range of pre-industrial natural variability" or "exceeds the 5–95% range of natural variability" because the past 1kyr includes industrial and preindustrial times together, while the percentage of the paper only refers to 1946–2005. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence was revised and modified according to your suggestion.
84169	85	29	85	29	"as well" to add after "implying" [ Annalisa Cherchi, Italy]	Rejected. In the context of the sentence there is no need to perform the suggested addition.
6565	85	31	85	31	There is no ERA-20CR analysis. Is ERA-20C what is being referred to? There is also CERA-20C. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The sentence was modified to describe "centennial reanalyses" instead of including the name of each of the products.
35901	85	33	85	33	Lee et al., 2018 --> Lee et al., 2019. [ Jiwoo Lee, United States of America]	Editorial - copyedit to be completed prior to publication. Reference year was corrected.
26663	85	46	85	46	Subsection 2.4.2 is a well balanced and comprehensive assessment [ Eric Brun, France]	Noted. Thanks for the compliment.
105103	85	46	88	24	It is a pity that changes in ENSO over more ancient periods are not covered, as they raise the concerns about changing variability related to changing background climate. Cf Brown et al., <a href="https://www.clim-past-discuss.net/cp-2019-155/cp-2019-155.pdf">https://www.clim-past-discuss.net/cp-2019-155/cp-2019-155.pdf</a> [ Masa KAGEYAMA, France]	Rejected. It is unclear what "ancient periods" are referred to here, but the choice of periods is driven by the overall choice of deep past periods for the chapter as a whole (discussed in earlier sections). Model studies such as the Brown et al paper cited here are outside the scope of this chapter but potentially within the scope of Chapter 3.
24049	85	46	88	24	I feel in this section the model results of paleo ENSO are severely underrepresented. I can only find very limited modelling studies that are included such as Karamperidou et al. (2015) and Tian et al. (2017), compared to all the other proxy data studies. I would like to recommend some recent modelling studies (summarized by Lu et al. 2018) that advance our understanding of the mechanisms of past ENSO changes and interpreting of reconstructions. Please see my following specific comments. ref: Lu, Z., Liu, Z., Zhu, J., & Cobb, K. M. (2018). A review of paleo El Niño-Southern Oscillation. Atmosphere, 9(4), 130. [ Lu Zhengyao, Sweden]	Rejected. Modelling studies are outside the scope of this chapter but potentially within the scope of Chapter 3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112933	85		86		This review of paleo-ENSO and its implications for detection of potential anthropogenic trends contains all the right pieces of the puzzle, but the presentation remains unclear in my opinion. In my mind, the paleo-periods must be clearly distinguished as regards the issue of any forced responses, and not blended with the question of whether current decades are significantly different than background variations arising from internal and/or previous natural variability more broadly. That is because these studies used datasets and methodologies that were designed for specific questions, and the detection of anthropogenic trends is indeed the highest bar because that signal is potentially only decades long at most. I have particular concern with the last paragraph of this section, which presents some smattering of datasets that purport to have variability "more closely comparable to mid-20th century behavior and onwards." This specifically undercuts any potential finding of anthropogenic influence that derives from a very robust set of datasets that point to just that. It is important to note that the Grothe et al 2019 dataset flags a 30-yr period in the 17th century that exhibits very high ENSO variance, but the last 50yrs are the highest period of variance in the 2000-yr-long record. In other words, the finding of high variance in individual records does not preclude the influence of GHG on ENSO, because the high nature of regional and temporal variability. You need LOTS of data from many regions. The signs do point to an influence on ENSO, and it might still be at low confidence, but sensitive stakeholders do deserve to know where the literature is pointing on this matter. This finding is even more impressive because it is one of the only emergent signals of ENSO's response to external forcing, the rest being somewhat more muddy at the moment. Happy to discuss and to step back into my role as CA here, which I abdicated last year when things in ch1 consumed me! [ Kim Cobb, United States of America]	Taken into account. A number of changes have been made to this section, including an explicit assessment finding that recent-period variability is above the Holocene average (whilst not necessarily beyond the range of Holocene variability) and a specific citation of the Grothe et al. finding comparing the recent period with the last millennium. The wording mentioned by the reviewer has also been amended to make it clearer that the cited papers are referring to various windows within the 3000-700 BP range. (We note that the Grothe et al paper has very limited data in the 3000-1000 BP window so findings in that period are not necessarily inconsistent with the overall results of the paper).
6567	86	7	86	8	The final sentence of this sentence should be stronger. As buoy data are available in significant numbers only from the late 1970s, and the Argo data from the mid-2000s, isn't the implication that any SST analysis prior to the 2000s, and especially the 1980s, must be treated with caution? [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph is dealing specifically with data set versions. However, the doubt as to how well the intensity of pre-1980s events is being resolved contributes to the relatively low confidence in the assessment of the significance of recent strong events in a longer-term context.
30511	86	7			'than analyses which do not': remove (means 'which do not resolve stronger events' but this is not the expected sense here) [ Gilles Delaygue, France]	Taken into account. The reference here is to analyses which do/do not include buoy and Argo data. Rewording has been carried out to make this clearer.
105751	86	10	86	12	Manucharyan and Fedorov (2014) compile earlier records, rather than generating new data. It would there seem more fitting to alter the sentence to use the reference as a citation at the end - rather than main subject of the sentence. [ Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This reference is to indicate the context of ENSO variability over a period of several million years (and to link to the box). The more detailed assessments which follow relate to the Holocene (or subsets of it), a much shorter period.
58053	86	14	86	14	Something went wrong with the reference manager. In the references there is only one paper by White et al., 2018, however, for the sentence White et al., 2018a is cited. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
73757	86	14	86	15	References should be in chronological order [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
24051	86	14	86	15	Some important modelling studies showing ENSO variability during the LGM can be included to support the argument. Zhu et al. (2017) provides the first isotope-enabled climate model simulation results; Liu et al. (2014) offers results from the first transient climate simulations. refs: Zhu, J., Liu, Z., Brady, E., Otto-Bliesner, B., Zhang, J., Noone, D. & Tabor, C. (2017). Reduced ENSO variability at the LGM revealed by an isotope-enabled Earth system model. Geophysical Research Letters, 44(13), 6984-6992. Liu, Z., Lu, Z., Wen, X., Otto-Bliesner, B. L., Timmermann, A. & Cobb, K. M. (2014). Evolution and forcing mechanisms of El Niño over the past 21,000 years. Nature, 515(7528), 550-553. [ Lu Zhengyao, Sweden]	Rejected. Modelling studies are outside the scope of this chapter but potentially within the scope of Chapter 3.
35561	86	14	86	18	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Editorial. Copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73759	86	17	86	18	References should be in chronological order [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
24053	86	17	86	18	Some important modelling studies that consistently show weaker ENSO variability during the MH can be included. Chen et al. (2019) is a systematic study using PMIP2/PMIP3 model results; Liu et al. (2014) and Lu et al. (2019) offer results from the first transient climate simulations; Pausata et al. (2016) takes into account the influence of remote forcing of land cover in North Africa.refs:Chen, L., Zheng, W., & Braconnot, P. (2019). Towards understanding the suppressed ENSO activity during mid-Holocene in PMIP2 and PMIP3 simulations. Climate dynamics, 53(1-2), 1095-1110. Liu, Z., Lu, Z., Wen, X., Otto-Bliesner, B. L., Timmermann, A. & Cobb, K. M. (2014). Evolution and forcing mechanisms of El Niño over the past 21,000 years. Nature, 515(7528), 550-553. Lu, Z., Liu, Z., Chen, G. & Guan, J. (2019). Prominent Precession Band Variance in ENSO Intensity Over the Last 300,000 Years. Geophysical Research Letters, 46(16), 9786-9795. Pausata, F. S., Zhang, Q., Muschitiello, F., Lu, Z., Chafik, L., Niedermeyer, E. M. & Liu, Z. (2017). Greening of the Sahara suppressed ENSO activity during the mid-Holocene. Nature communications, 8(1), 1-12. [ Lu Zhengyao, Sweden]	Rejected. Modelling studies are outside the scope of this chapter but potentially within the scope of Chapter 3.
58057	86	18	86	18	Something went wrong with the reference manager. There are two papers with the same name and date (McGregor et al., 2013a,b) listed in the references , however, in the sentence the source is only listed as McGregor et al, 2013 without the a or b version. It should be McGregor et al., 2013b. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. References have been corrected.
30513	86	20			'remote proxies' > 'remote locations/sites' (a proxy is a recorded parameter, so a 'remote proxy' has no sense to me) [ Gilles Delaygue, France]	Rejected. The fact that these are remotely located is clear from the context.
93507	86	20			remove "the" in the sentence "with the stronger decreases... [ Rahab KINYANJUI, Kenya]	Accepted. Text edited.
29961	86	21	86	23	It should be more explicit that Karamperidou et al. referred to changes in ENSO flavours (explored in this case in one climate model). It should be noted also that the only evidence so far of past changes in ENSO flavours during the Holocene came from the changing asymmetry of ENSO anomalies in Peru which indicated a CP dominated ENSO around 7ka while the rest of the Holocene was dominated by the EP mode (Carré et al., 2014). [ Matthieu Carré, France]	Taken into account. A reference to the Carre 2014 finding of high CP activity around 7 ka has been added to the paragraph on CP and EP events.
58061	86	27	86	27	There is no paper by Joanides, 2012a. Instead it should be Koutavas and Joanides, 2012. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference changed.
58063	86	27	86	27	There are four papers in the references by the name of Cheng et al, 2016 that are about different topics. Therefore they should be listed as Cheng et al., 2016a,b,c,d. Hence the citation for the sentence is partly wrong. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This reference is Chen et al, not Cheng et al. Initial has been added to distinguish from a different Chen.
58065	86	28	86	28	There is no paper by Koutavas and Thompson et al., 2017b listed in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. The correct reference is Thompson et al 2017.
71623	86	30	86	32	Important study by Masuda et al (2015) is implying the mechanism of ENSO modulations of multi-decadal timescale. At least, the reference should be included. :Masuda, S., J. P. Matthews, Y. Ishikawa, T. Mochizuki, Y. Tanaka, A. Awaji (2015), A new Approach to El Niño Prediction beyond the Spring Season, Scientific Reports, 5, 1-9, DOI: 10.1038/srep16782. [ Shuhei Masuda, Japan]	Rejected. The Masuda et al paper deals with the post-1960 period and is not relevant to the multi-centennial periods discussed in this section.
29963	86	30	86	39	Ledru et al., 2013 should not be cited in this section since these authors do not present a record of ENSO variability. ENSO changes are only speculatively proposed as the cause of a low frequency record. [ Matthieu Carré, France]	Accepted. The Ledru 2013 reference is not relevant to this part of the assessment and has been removed.
73761	86	31	86	31	Change 'find' to 'found' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The present tense is used consistently across this section.
73763	86	34	86	34	Change 'find' to 'found' [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The present tense is used consistently across this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5429	86	34			Fig. 2.34b is not consistent with this conclusion. Frame a) is ambiguous given the broad range of values of the different reconstructions for the 1600-1900 period. [ Bryan Weare, United States of America]	Rejected. Figure 2.34b is not relevant to this conclusion since it compares the most recent decades to the pre-instrumental period and Figure 2.34b does not cover the pre-instrumental period. The spread of results shown in Figure 2.34a is reflected in the confidence levels given in the assessment.
73765	86	39	86	39	Delete , in Thompson et al. reference. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Copyedit to be completed prior to publication.
17785	86	40	86	40	Issue with the units? 700 is years not kyrs, so this should be indicated. [ Raphael Neukom, Switzerland]	Accepted. Reworded as '700 to 1000 years'.
37099	86	46	86	52	Figure 2.34 (b) is deceptive. Why is one line for June-November (6 months) but another line on the graph for April-March (i.e. 12 months). The data should be shown across the same calendar months. [ John McLean, Australia]	Rejected. April-March covers the typical life cycle of an El Nino or La Nina event, but the SOI in December-March is a less reliable indicator of ENSO than are earlier values because of the superposition of short-term variability driven by the Madden-Julian Oscillation at Darwin and Tahiti during the southern hemisphere summer.
30515	86	46			No clear what is shown here: is it the variance of the running averaged Nino3.4 index, or the running averaged variance? [ Gilles Delaygue, France]	Noted. It is the 30-year running mean of the variance of the annual mean values, as stated in the caption.
30517	86	46			Some explanation may help clarify that the general increasing trend in the variance is not related to the varying time resolution of the records. [ Gilles Delaygue, France]	Rejected. The values are reported as annual means (as stated in the caption) so there is no change in time resolution.
29883	86	50	86	50	Add "SOI" after "Southern Oscillation Index". [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
37101	87	1	87	4	None of this discussion should be in terms of La Nina events and El Nino events because these are designated on the basis of crossing purely arbitrary thresholds based on a combination of index values and period of time. The indices are in fact continuous scales and a state that falls just short of the threshold is indistinguishable from a state that just crosses it. The discussion should be in terms of the index values. [ John McLean, Australia]	Rejected. This part of the discussion relates to the mean state on decadal timescales and event thresholds are not material to the statement.
84171	87	1	87	12	this does not seem different /new from what assessed in AR5 and reported at the beginning of the section [ Annalisa Cherchi, Italy]	Noted. The reviewer is correct that this underlies an assessment finding which does not substantially differ from that in AR5, despite some new evidence.
90811	87	3			Refer updated from National Centers for Environmental Information ( <a href="https://www.ncdc.noaa.gov/teleconnections/enso/indicators/sst/">https://www.ncdc.noaa.gov/teleconnections/enso/indicators/sst/</a> ) [ Vivien How, Malaysia]	Noted. It is unclear what the reviewer wants here. Links to data sets (such as ONI, which is covered in this link) are the domain of Annex I.
5431	87	4			This is not evident in Fig. 2.34b. [ Bryan Weare, United States of America]	Rejected. As stated in the caption for the figure, Figure 2.34b shows running variance for 30-year periods ending in the stated year, so the 1910-1950 period in the text corresponds to 1940-1950 in Figure 2.34b - whose values are clearly lower than more recent decades.
35563	87	5	87	20	Change & for "and" in bibliographic citations [ Carlos Antonio Poot Delgado, Mexico]	Editorial. Copyedit to be completed prior to publication.
70273	87	7	87	7	Can more observations be added (or one SST product be extended back to early 1900 period) to figure 2.34b to demonstrate the spread highlighted in this sentence. (for instance the SST data sets that feature in Fig. 2.35) [ Shayne McGregor, Australia]	Rejected. The limitations of pre-1950 SST data sets in resolving the intensity of events (discussed in the text) are likely to cause a substantial negative bias in SST variance in that period, and thus extending the time series backwards would be at high risk of being misleading.
37103	87	10	87	12	The cited source might say that but it's false. See comments above for page 84 lines 54 and 55. [ John McLean, Australia]	Rejected. No evidence is presented for the reviewer statement.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
6569	87	14	87	15	This sentence refers to anomalies. Are the anomalies with respect to a fixed climatological reference period? If so one would expect a larger Nino3.4 SST index for 2015-2016 than for 1997-1998 and 1982-1983 simply because of the underlying upward trend in SST. Or were temperatures de-trended first? I assume not, as the next sentence refers to detrended data. Either way, this could be written a bit more clearly. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This sentence refers to un-detrended data (implied by the use of detrended in the following sentence).
84173	87	14	87	37	these two paragraphs should be merged into a unique one, summarizing and merging old and new references [ Annalisa Cherchi, Italy]	Rejected. These two paragraphs cover clearly different subjects (the first is an assessment of ENSO intensity, the second discusses events in recent years and their implications).
90429	87	18			replace & by and [ Jeannine-Marie St-Jacques, Canada]	Editorial. Copyedit to be completed prior to publication.
57643	87	19	87	20	While it's stated that uncertainties in SST data reduce confidence in ONI estimates prior to 1950, there is no overall confidence measure stated for the claim. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. This lower confidence in pre-1950 values is considered to be a statement of fact.
29885	87	21	87	21	The acronym "SODA" is not defined in this chapter; please add its meaning. [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
127121	87	33	87	35	Perhaps because the chapter's author team does not include anyone from western South America, there's no mention of the fact that there was a very strong El Nino near the South American coast one year later, with devastating consequences ( <a href="https://en.wikipedia.org/wiki/2016--17_South_America_floods">https://en.wikipedia.org/wiki/2016--17_South_America_floods</a> ). [ Trigg Talley, United States of America]	Taken into account. The 2017 event is known to the authors and was included in earlier drafts but deleted for space reasons, as it is not material to any of the assessment findings in the section. However, recent research has raised the question of "coastal Nino" events such as 1925 and 2017 in increasing uncertainty in paleo-ENSO reconstructions which use South American proxies, and coastal Nino events are mentioned in that context in Annex VI.
58069	87	35	84	35	The abbreviation for 'Eastern Pacific' (EP) and 'Central Pacific' (CP) is used, however, the entire name is initially explained in line 40 and 41 (2-87). Therefore, the abbreviation should already be explained in line 35. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
127123	87	35	87	35	These acronyms are defined in the following paragraph instead of here. [ Trigg Talley, United States of America]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
57645	87	35	87	37	Lacks a pertinent reference. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The reference here is to findings reported in AR5, as stated at the start of the paragraph.
6571	87	35	87	37	If AR5 started its trends in 1979, why does AR6 start in 1980 not 1979? If there's a good reason, fine, but it looks a bit odd when set out in a sentence such as the one that spans these lines. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. 1980 has been used as a standard starting point across the chapter. The text referred to by the reviewer has been deleted.
30519	87	35			'EP from CP events': acronyms are defined in the following paragraph only [ Gilles Delaygue, France]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
70267	87	39	87	39	I believe that adding reference to the technical annex would be useful and suggest adding it here "ENSO events (See Technical Annex VI), in particular". I [ Shayne McGregor, Australia]	Accepted. Cross-reference added.
24011	87	41	87	42	Ciasto et al., 2015 'Teleconnections between Tropical Pacific SST Anomalies and Extratropical Southern Hemisphere Climate', worth adding to the references as it discusses the difference in extra-tropical SH teleconnections associated with tropical CP and EP SST anomalies and their relationship to the SAM. [ AMNA JRRAR, Jordan]	Accepted. Reference has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9225	87	42	87	42	Suggest adding one highly relevant reference as: ... (e.g. Ashok et al., 2007; Ratnam et al., 2014; Capotondi et al., 2015; Yu et al., 2015; Timmermann et al., 2018). Reference to add: Yu B., X. Zhang, H. Lin, and J.Y. Yu, 2015: Comparison of wintertime North American climate impacts associated with multiple ENSO indices. Atmos.-Ocean, 53:4, 426-445, doi: 10.1080/07055900.2015.1079697. [ Bin Yu, Canada]	Accepted. Reference has been added.
58071	87	42	87	42	There is no paper by Timmermann et al., 2018 listed in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference has been added.
37873	87	42	87	45	Yeh et al. (2009) and Lee and McPhaden (2010) should be added as those are pioneering work which pointed out that the CP-type El Nino is frequently occurred after 2000s in the observation. Yeh, S. W., Kug, J. S., Dewitte, B., Kwon, M. H., Kirtman, B. P., & Jin, F. F. (2009). El Niño in a changing climate. Nature, 461(7263), 511-514. Lee, T., & McPhaden, M. J. (2010). Increasing intensity of El Niño in the central-equatorial Pacific. Geophysical Research Letters, 37(14). [ Junhee Lee, Republic of Korea]	Rejected. The list of citations given is not intended to be exhaustive and focuses on post-AR5 literature.
54923	87	42			Suggest adding one highly relevant reference as:  ... (e.g. Ashok et al., 2007; Ratnam et al., 2014; Capotondi et al., 2015; Yu et al., 2015; Timmermann et al., 2018).  Reference to add: Yu B., X. Zhang, H. Lin, and J.Y. Yu, 2015: Comparison of wintertime North American climate impacts associated with multiple ENSO indices. Atmos.-Ocean, 53:4, 426-445, doi: 10.1080/07055900.2015.1079697. [ Nancy Hamzawi, Canada]	Accepted. Reference has been added.
30031	87	43	87	45	Please increase a recent study (Bin Wang et al., 2019). which has made a similar study, but statistically, nor definitely. [ Yihui Ding, China]	Taken into account. The SOD text referred to deals with the frequency of CP and EP events which is not discussed in the Wang et al paper. However, the Wang et al paper is relevant to discussion of decadal variability of ENSO teleconnections and is now cited in that section.
3511	87	52	88	9	The treatment here of long-term multi-decadal variations in monsoon teleconnections seems fair. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
17787	88	2	88	2	Dätwyler et al. (2020) should be cited here instead of Dätwyler et al. (2019). (See my comment below about page 207, line 31) [ Raphael Neukom, Switzerland]	Accepted. Reference corrected.
30681	88	5	88	6	Cite here relevant analysis of Simmonds, I., and P. Hope, 1997: Persistence characteristics of Australian rainfall anomalies. Int. J. Climatol., 17, 597-613, doi: 10.1002/(SICI)1097-0088(199705)17:6<597::AID-JOC173>3.0.CO;2-V. [ Ian Simmonds, Australia]	Rejected. This list is not intended to be exhaustive and focuses on post-AR5 literature.
58073	88	7	88	7	There is only one paper listed by Jin et al., 2016 in the references, therefore, it should be Jin et al., 2016 instead of Jin et al., 2016a. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Reference corrected.
3513	88	9	88	11	To the discussion here on the role of CP/EP ENSO in monsoon teleconnections you may consider the much earlier work of Krishna Kumar et al. (2006) who noted the strong influence of CP rather than EP El Nino on the Indian monsoon, as an explanation for observed variations in the teleconnection. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The four listed citations are considered to be sufficient for this point. Pre-AR5 references are not normally used where post-AR5 references are available.
29887	88	12	88	12	The acronym "AMO/V" has not been previously defined in this chapter; please add its meaning. [ Hernan Edgardo Sala, Argentina]	Taken into account. A standard policy has been applied to the definition of acronyms, which are defined at their first use in a section. An acronym list is also included in an annex to the report.
58075	88	12	88	12	There are two paper listed by Wang et al., 2014, hence it should be Wang et al., 2014b. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Initial has been added.
37105	88	18	88	24	Add to this summary paragraph. "While the consequences of different ENSO states - variations in sea level, ocean currents and wind patterns - the root cause of the different states is still unclear." [ John McLean, Australia]	Rejected. The proposed sentence makes no sense, and in any case attribution is the domain of Chapter 3.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57755	88	19	88	20	Where it says "both the amplitude and the frequency is", should be "are" and the same happens at the end of the sentence. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Text edited.
39925	88	27	88	27	Add a definition to the glossary for 'Indian ocean basin and dipole modes' [ TSU WGI, France]	"Indian ocean basin and dipole modes" are now included in the glossary.
70275	88	31	88	33	Ch2 of AR5 states "The Indian Ocean Basin Mode (IOBM) has a strong warming trend (significant at 1% since the middle of the 20th century)." See second last paragraph of Section 2.7.8. [ Shayne McGregor, Australia]	In AR6 the linear trend has been removed from IOBM index, and is based on the post 1950s era with generally improved observations.
26665	88	35	88	35	Here again the definition of the LGM is not consistent with that defined above. Sometimes, it is 21-19 ka; sometimes it is 21 ka (see Chapter 7 Table 11 for example); here it is 25-17 ka. Must be clarified / homogenised throughout the AR6 document. [ Eric Brun, France]	Taken into account. Homogeneous definitions are not used in the literature being assessed. Varying definitions are discussed in context of published literature assessed.
105753	88	35	88	37	It seems most strange to discuss the variability in the Indian Ocean at the LGM without mentioning the important finding of Thirumalai et al. (2019). This paper demonstrates that there was a brand new mode of variability in the Indian Ocean at the LGM, which does not operate today. I believe this is the only evidence for climate changes resulting in the creation of novel modes of climate variability. [Thirumalai, K., DiNezio, P.N., Tierney, J.E., Puy, M. and Mohtadi, M., 2019. An El Niño Mode in the Glacial Indian Ocean?. Paleoclimatology and Paleoclimatology, 34(8), pp.1316-1327.] [ Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The suggested references are now discussed.
45303	88	35	88	37	Thirumalai et al. (2019) used Individual Foram Analysis and suggested strong interannual variability that resembles IOD/ENSO in Eastern Indian Ocean during LGM doi:https://doi.org/10.1029/2019PA003669. [ Anson Cheung, United States of America]	Noted. Discussions of the ENSO/IOD connections during the LGM are now improved.
71633	88	35	88	52	There is an additional reference that would add to this section. Abram et al., (2020), Palaeoclimate perspectives on the Indian Ocean Dipole. This demonstrates there is recent trends toward more frequent and intense positive IOD events due to a shift toward a more positive IOD-like mean zonal SST gradient [ Jessica Hargreaves, Australia]	Accepted. The suggested reference (Abram et al., 2020) has been added.
58079	88	40	88	43	The reference for the sentence is correct (Zinke et al. 2014), however, in the references the wrong paper is listed. Instead of Zinke et al. 2014: Corals record long-term Leeuwin current variability including Ningaloo Niño/Niña since 1795 (Nature Communications), it should be Zinke et al. 2014: Seychelles coral record of changes in sea surface temperature bimodality in the western Indian Ocean from the Mid-Holocene to the present (Climate Dynamics). [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The discussions of Zinke et al's papers are now well-referenced.
3515	88	44	88	46	The statement here on monsoon strengthening and IOD tendencies is confusing. Does it pertain to both phases or a particular phase of the IOD? One would expect (in the western pole of the IOD) that stronger Asian monsoon circulation leads to enhanced coastal upwelling off Somalia, leading to a colder western pole or more IOD negative-like mean state. Meanwhile the sentence itself suggests wind anomalies in the eastern region will increase, also enhancing upwelling and cooling there, which would tend things towards an IOD-positive-like state. These things appear contradictory. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. In the FGD it is now stated clearly that reconstructions from fossil corals for the eastern Indian Ocean point to stronger negative IOD SST anomalies due to the enhanced upwelling and cooling driven by a stronger monsoon with enhanced anomalous easterly winds in the eastern Indian Ocean during the MH.
71631	88	51	88	51	The Abram et al., (2020) reference is now available, however is not listed in the references section [ Jessica Hargreaves, Australia]	Noted. The reference Abram et al (2020) is now provided.
58081	88	51	88	51	There is no paper by (Abram et al., submitted) listed in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. The reference Abram et al (2020) is now provided.
5433	89	11	89	12	The various curves in the IOD frame of F.2.35 agree quite well, contrary to this statement. [ Bryan Weare, United States of America]	Noted. The statement is now revised to indicate this agreement.
107021	89	26	89	36	Indices of the modes are not defined in the legend. Do their definition follow the ones defined in the Technical Annex? Consistency needs to be verified [ Christophe CASSOU, France]	Taken into account. The definition of the indices in Technical Annex VI is adopted here and is now referred to under this Figure.
57757	89	26	89	37	On the caption of Figure 2.35 the different SST datasets are numbered, Don't they need to be color coded to match with the legend instead? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The data sets are colour coded with matching legends.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
29889	89	29	89	29	Add "also referred as AZM" after "Atl3" (the lowermost plot of the Figure 2.35 has the label "AZM"). [ Hernan Edgardo Sala, Argentina]	Taken into account. AZM is now used consistently instead.
113649	89	29	89	29	I can't see "Atlantic Niño (Atl3)" in the Figure. Please double-check. [ Agnieszka Kowalczyk, Poland]	See comment 29889
5435	90	7			The bottom frame of F.2.35 shows no such weakening in this or any other time period. [ Bryan Weare, United States of America]	Noted. This statement is now revised.
58083	90	10	90	10	There is no paper by (Nnamchi et al., 2020) listed in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Nnamchi et al 2020 is now included in the references.
73767	90	14	90	14	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
70277	90	14	90	14	Consider merging "(Burmeister et al., 2016) (Foltz et al., 2012)" [ Shayne McGregor, Australia]	Editorial; copyedit to be completed prior to publication.
13301	90	14	90	14	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
43151	90	14			Read " (Foltz et al., 2012; Burmeister et al., 2016)" rather than " (Burmeister et al., 2016) (Foltz et al., 2012)" [ Cyriaque Rufin Nguimalet, Central African Republic]	Editorial; copyedit to be completed prior to publication.
102763	90	23	90	23	The section is named Pacific Decadal Variability (PDV) but the section mostly refers to the Pacific Decadal Oscillation (PDO). This should be changed, i.e., which naming convention to use (this ambiguity is repeated in the use of acronyms in the section - PDO and PDV is used interchangeably. [ Philippe Tulkens, Belgium]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
35905	90	23	91	5	PDV and PDO were used in mix, and full name for PDV was not explicitly defined although section title is for it. Maybe something like "... PDV (also known as PDO)" may help clarifying it in more explicit way. [ Jiwoo Lee, United States of America]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
18325	90	23	91	39	Again, I'm very surprised to see there is no mention of the ongoing debate on whether the observed AMO and IPO cycles since the late 19th century resulted purely from internal variability (IV) or partly from decadal variations in external forcing (such as anthropogenic and volcanic aerosols). Many recent studies (Hua et al. 2019; Qin et al. 2020a,b; and cited refs there) suggest that decadal variations in anthropogenic and volcanic aerosols over the North Atlantic happen to be roughly in phase with the IV-induced component of the observed AMO, and therefore enlarged the IV-induced AMO by about 40-50% (Qin et al. 2020b). This amplification of the AMO cycles by external forcing is consistent with smaller AMO amplitudes in preindustrial records (Qin et al. 2020a). Since the decadal aerosol variations will not continue to be in phase with the IV-induced AMO cycles, future AMO cycles will be affected. The role of the external forcing also affects our assessment of the temporal changes in the AMO. The effect of external forcing on AMO is also linked to the attribution of the AMM or AZM discussed in section 2.4.4 on pp. 89-90, as shown by Hua et al. (2019). In contrast to AMO, the observed IPO has resulted primarily from IV, although Pacific SSTs since the 1990s have been significantly influenced by external forcing (Hua et al. 2018). Relevant refs. (also see refs. cited within these papers): Hua, W., A. Dai, and M. Qin, 2018: Contributions of internal variability and external forcing to the recent Pacific decadal variations. Geophys. Res. Lett., 45, 7084–7092. <a href="https://doi.org/10.1029/2018GL079033">https://doi.org/10.1029/2018GL079033</a> . Hua, W., A. Dai, L. Zhou, M. Qin, and H. Chen, 2019: An externally-forced decadal rainfall seesaw pattern over the Sahel and southeast Amazon. Geophys. Res. Lett., 46, 923-932. <a href="https://doi.org/10.1029/2018GL081406">https://doi.org/10.1029/2018GL081406</a> . Qin, M., W. Hua, and A. Dai, 2020a: Aerosol-forced multi-decadal variations across all ocean basins in models and observations since 1920. Science Advances, accepted. Qin, M., A. Dai, and W. Hua, 2020b: Quantifying contributions of internal variability and external forcing to Atlantic multidecadal variability since 1870. Science Advances, submitted. [ Aiguo Dai, United States of America]	Noted. The role of external forcings in the modes of variability is the scope of Chapter 3 and is not covered here.
39095	90	23	91	39	Regarding PDO and AMO, Mann et al 2020 call into question if the can really be classified oscillations. The use of PDO/PDV and AMO/AMV is not consistent throughout chapter 2,3,and 4. Mann, Michael E., Byron A. Steinman, and Sonya K. Miller. "Absence of internal multidecadal and interdecadal oscillations in climate model simulations." Nature Communications 11.1 (2020): 1-9. [ Ola Kalen, Sweden]	Noted. The role of internal variability versus external forcings is still being debated in the literature considering several modes of variability. Moreover, Chapters 2, 3 and 4 provide a consistent assessment of PDV focusing on observations, historical model simulations and future projections, respectively.
5437	90	23			Why is there no PDO or PDV frame in F 2.35? This would seem essential for this section. [ Bryan Weare, United States of America]	Taken into account. Time series of PDV were included in the new Figure 2.38.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70279	90	25	90	25	Is "weakening" the right word? Because it actually transitions to a strong (absolute magnitude) negative value of the PDO? Maybe this is better described as a transition to negative values? [ Shayne McGregor, Australia]	Taken into account. The sentence was changed according to your suggestion.
11631	90	25	91	5	The text accurately describes the large uncertainties in reconstructions of PDV that extend beyond the instrumental record. However, there is a growing body of literature addressing multi-millennial scale shifts in North Pacific coupled ocean-atmosphere circulation, which overwhelmingly indicates that the patterns characteristic of PDV may vary/evolve on longer timescales. The most common theme that has emerged from the literature is an inferred shift from the middle Holocene, which most studies assert was characterized by dominantly PDO- ( weak Aleutian Low) conditions, to the late Holocene, which most studies find was characterized by PDO+ (strong Aleutian Low) conditions, as well as overall increased hydroclimate variability (Anderson et al., 2005, doi:10.1016/j.yqres.2005.03.005; Barron and Anderson, 2011, doi:10.1016/j.quaint.2010.02.026; Jones et al., 2014, doi:10.1016/j.quascirev.2013.12.025; Bailey et al., 2018, doi:10.1016/j.quascirev.2018.06.027; DesChamps et al., 2019, doi:10.1029/2018PA003485; Jones et al., 2019, doi:10.3389/feart.2019.00025; Broadman et al., submitted Dec 2019 to Quaternary Science Reviews). Current knowledge of Holocene PDV was also summarized in a recently submitted review paper (Hernandez et al., submitted Dec 2019 to Earth Science Reviews), and highlighted a shift in the periodicity of PDV at roughly this time. This topic was not addressed in AR5, and several studies have emerged since that strengthen and support claims made by relevant preexisting literature. A discussion of this shift and possible variations of North Pacific hydroclimate on multi-millennial timescales would both expand on the current description of PDO/PDV, and incorporate a larger contribution from the relevant paleoclimate literature. Additionally, this major, millennial-scale shift might indicate that variability in this mode is beyond the range captured by climate models. The number of studies showing a shift in North Pacific ocean-atmosphere circulation from the middle to the late Holocene suggest high confidence that the shift occurred. The agreement among most of these records that the shift was from PDO- to PDO+ conditions suggests medium confidence in the direction of the shift. [ Ellie Broadman, United States of America]	Taken into account. The description of PDV changes during the Holocene was updated based on the suggested literature.
18569	90	27	90	28	change to "The SROCC reported a variable to slightly positive shift in the PDO since the mid-1970s." [ Miriam Jones, United States of America]	Taken into account. The sentence was changed as "AR5 and SROCC reported a large shift of the PDO in the late 1970s, with a predominantly positive phase until the end of the 1990s, being mainly negative afterwards."
24417	90	30	90	30	Show the full name for PDV when it appears first time. Some explanation should be given for the difference of PDV, PDO, IPO. [ Zhou Botao, China]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
102765	90	30	90	30	PDV = PDO? [ Philippe Tulkens, Belgium]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
70281	90	30	90	30	Transition from "PDO" to "PDV" and PDV is not defined. This transition could be used to direct the reader to technical annex VI. [ Shayne McGregor, Australia]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
127125	90	30	90	30	Is PDV "Pacific decadal variability"? [ Trigg Talley, United States of America]	Accepted. Yes, this was clarified in the text.
80297	90	30	90	30	PDV has not been defined in the text [ Paola Arias, Colombia]	Accepted. PDV was defined.
29891	90	30	90	30	Please define "PDV". [ Hernan Edgardo Sala, Argentina]	Accepted. PDV was defined.
98755	90	30	90	30	Need to explain shift from PDO to PDV. [ Meredith Parish, United States of America]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
9965	90	30	90	30	De-abbreviate PDV as Pacific Decadal Variability [ Olga Zolina, France]	Accepted. PDV was defined.
13303	90	30	90	30	PDV must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Accepted. PDV was defined.
45305	90	30	90	31	marine sediments also: O'Mara et al. (2019) doi: https://doi.org/10.1029/2019GL084828. Additional reference for tree rings: MacDonald and Case (2003) doi:10.1029/2005GL022478, [ Anson Cheung, United States of America]	Taken into account. We included O'Mara et al. (2019). The additional reference for tree rings was replaced by a newer one (D'Arrigo and Ummenhofer, 2015).
30521	90	30			'a variety of proxy' [ Gilles Delaigue, France]	Editorial - copyedit to be completed prior to publication
30523	90	35		38	Over a period of time of 400 yrs, it is hardly possible to detect centennial or bicentennial periods. So both parts of the sentence (millenia vs. 'less than 400 yrs') do not seem to contradict themselves. [ Gilles Delaigue, France]	Editorial - copyedit to be completed prior to publication.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102767	90	43	90	43	The IPO index should be defined here. [ Philippe Tulkens, Belgium]	Accepted. IPO was defined.
70283	90	43	90	43	IPO is not defined. [ Shayne McGregor, Australia]	Accepted. IPO was defined.
80295	90	43	90	43	IPO has not been defined in the text [ Paola Arias, Colombia]	Accepted. IPO was defined.
29893	90	43	90	43	Please define "IPO". [ Hernan Edgardo Sala, Argentina]	Accepted. IPO was defined.
58085	90	43	90	43	The abbreviation of the Inter-decadal Pacific Oscillation (IPO) is mentioned but never the entire name. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. IPO was defined.
13305	90	43	90	43	IPO must be expanded acronym has not been used [ Maria Amparo Martinez Arroyo, Mexico]	Accepted. IPO was defined.
127127	90	43	91	5	The text begins characterizing the phases of PDO as cold and warm, but then switches to positive and negative without stating which corresponds to which. Be consistent or explain. [ Trigg Talley, United States of America]	Taken into account. The text was revised and modified to ensure consistency in the PDO description.
102769	90	50	90	50	PDV = PDO? [ Philippe Tulkens, Belgium]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
24419	90	55	90	55	Please show the time for "a recent switch" of PDO. [ Zhou Botao, China]	Accepted. The sentence was modified to include the year.
70285	90		90		There should be a clear pointer to the technical annex in the PDO section (actually, each mode discussed here should include a point to the technical annex for those that do not read the very brief intro paragraph to the mode subsection) [ Shayne McGregor, Australia]	Taken into account. A link to the technical annex was provided in the opening paragraph of Section 2.4.
102771	91	3	91	5	PDV = PDO? [ Philippe Tulkens, Belgium]	Accepted. A link between PDV and PDO was provided at the beginning of the section.
80299	91	10	91	10	AMO/V has not been defined [ Paola Arias, Colombia]	The definitions are contained in the technical annex on modes of variability.
41897	91	10	91	39	Our paper (Garry et al., in review, title: Climate models may underestimate pre-industrial North Atlantic Ocean summertime multidecadal variability) in second stage of review at Climate Dynamics is very relevant to this paragraph, as it compares the variability of north east Atlantic temperatures from a high resolution multi-centennial proxy to CMIP5 models, finding that CMIP5 models (and higher resolution shelf model) are likely to underestimate multidecadal variability. The paper also shows the relationship between the proxy site and the north Atlantic, inferring that multidecadal variability across the Atlantic is likely to be underestimated. Please get in touch with freya.garry@metoffice.gov.uk if you would like to see the draft manuscript. The abstract follows: "Multidecadal variability is a prominent feature of the North Atlantic ocean, but this variability is not well understood due to the relatively short nature and anthropogenic 'contamination' of instrumental records. This restricted knowledge of multidecadal variability limits our ability to evaluate climate prediction capability for regions around the North Atlantic. Multi-centennial annually resolved and absolutely dated marine oxygen isotope (d18Oshell) data from bivalves uniquely provide absolutely dated, high resolution insights into preindustrial summertime marine variability. The d18Oshell variability represents the combined influence of summertime seawater salinity and temperature. We compare preindustrial d18Oshell marine variability at a site west of Scotland (Tiree Passage, Hebridean Shelf) to the variability in summertime model data at the same location in a hierarchy of models, including CMIP5 global climate models and a high-resolution shelf sea model. On annual timescales, d18Oshell variability from the bivalves and that derived from instrumental observations are consistent with the range of model simulations, providing confidence in the proxy estimate. On multidecadal timescales the models generally underestimate pre-industrial multidecadal variability as recorded by the bivalves. Multidecadal variability in last millennium climate model simulations with external forcing (solar and volcanic) is only half that recorded in the bivalve record on 20-year timescales. Models and observations indicate that variability at the Tiree Passage is representative of wider change across parts of the North Atlantic. Our results therefore imply that models do not adequately represent the wider North Atlantic processes that result in Atlantic multidecadal variability." [ Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Garry et al. was not yet published at the time of assessment, moreover, consideration of model results is out of scope of CH2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41899	91	10	91	39	A nice addition to this paragraph could be to cite this paper which shows that marine variability can be reconstructed from high temporal resolution proxies, with networks of proxies used to skillfully reconstruct key components of North Atlantic Ocean variability with absolute dating precision, enabling construction of powerful baseline records of past North Atlantic variability into the unobserved preindustrial period <a href="https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018PA003366">https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018PA003366</a> Reynolds DR, Hall IR, Slater SM, Mette MJ, Wanamaker AD, Scourse JD, Garry FK, Halloran PR (2018) Isolating and Reconstructing Key Components of North Atlantic Ocean Variability From a Sclerochronological Spatial Network. <i>Paleoceanography and Paleoclimatology</i> , 33, 1086–1098. <a href="https://doi.org/10.1029/2018PA003366">https://doi.org/10.1029/2018PA003366</a> [ Freya Garry, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The suggested references have been discussed.
58087	91	16	91	16	The source of the sentence should be Wang et al., 2017b instead of Wang et al., 2017 because there are several sources. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted.
58089	91	16	91	16	The paper by Chylek et al., 2012: 'Greenland ice core evidence for spatial and temporal variability of the Atlantic Multidecadal Oscillation' is missing in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Chylek et al., 2012 is now included in the references.
58091	91	16	91	16	There is no paper listed in the references by Wang et al., 2017 that discusses uncertainties of the AMO on the base of corals. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. The sentence has been revised and uncertainties reference by the review paper of Hernández et al., 2020, rather than Wang et al., 2017.
43153	91	16		17	Read "(Kilbourne et al., 2014; Svendsen et al., 2014; Wang et al. 2017)." rather than "(Wang et al. 2017)(Kilbourne et al., 2014; Svendsen et al., 2014)." [ Cyriaque Rufin Nguimalet, Central African Republic]	Noted. The references are now correctly arranged.
73769	91	17	91	17	Replace )( with ; [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
102773	91	17	91	17	AMV = AMO/V? [ Philippe Tulkens, Belgium]	Editorial; copyedit to be completed prior to publication.
29895	91	17	91	17	Typo in "AMV". [ Hernan Edgardo Sala, Argentina]	Editorial; copyedit to be completed prior to publication.
109025	91	17	91	17	Review brackets for '2017'(Kilbourne' [ Belen Martrat, Spain]	Editorial; copyedit to be completed prior to publication.
9963	91	17	91	17	double brackets [ Olga Zolina, France]	Editorial; copyedit to be completed prior to publication.
13307	91	17	91	17	References can all be in the same () [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
73771	91	20	91	20	References should be in chronological order (no dates alphabetically at start or end of the sequence) [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
78405	91	20	91	20	Should the reference be Gray et al. 2004? If so, note that it only goes back to 1567 (not including the MCA). [ Hans W Linderholm, Sweden]	Taken into account in edits
89865	91	24	91	28	The problem here is, that Hausteine et al. (2019) does not quite show the same thing. Rather, they suggest a revised AMV index, which they call NAVI (North Atlantic Variability Index). Since it's also not mentioned in the Technical Annex VI, it is defined as average SST at 40-60°N and 15-50°W minus NH SSTs. It is deliberately not de-trended as it is meant to show the difference of the AMV/AMOC region compared to the rest of the global ocean (teasing out the signal associated with AMOC variations). The resulting time series is shown in Fig. 7a therein (in black). While NAVI still carries multidecadal variability with similar magnitude (note scale on the right-hand side of Fig 7a), it shows a steady decline since 1950, superimposed by accelerated cooling between 1960 to 1990 (arguably aerosols). In contrast to what the paragraph says, quote: "they all show warm periods [...]" but no overall sustained change during the instrumental period." ... Hausteine et al. (2019) do find a sustained decline in the NAVI index. [ Karsten Hausteine, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The definitions of Hausteine et al. (2019) are discussed in the technical annex on modes of variability.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89867	91	24	91	28	Continued: There are two ways to resolve that. Either to take the reference out, or to add a sentence which highlights that alternative AMV definitions have been proposed. It goes without saying, that the latter is the way I think the issue should be resolved. Thereby, their finding that NAVI differs markedly from the traditional AMV index definition should be highlighted. The technical description of the alternative/revised AMV index could go in the Technical Annex VI. In addition, as argued in the same paper, NAVI might be a suitable proxy for the change in the AMOC strength. Accordingly, Haustein et al. (2019) lends additional support to the hypothesis stated in Section 2.3.3.4.1. May I therefore suggest to add Haustein et al. (2019) also at page 2-71 line 6/7: "Reconstructions based on instrumental observations suggest an overall weakening trend of AMOC through the 20th century superimposed with multidecadal variations (Ezer, 2013; McCarthy et al., 2015a; Rahmstorf et al., 2015; Caesar et al., 2018)." [ Karsten Haustein, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The definitions of Haustein et al. (2019) are discussed in the technical annex on modes of variability.
73773	91	30	91	30	Delete 'The' and capitalise 'oceanic'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
58093	91	34	91	34	There is no paper by Robson et al., 2016 listed in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Editorial; copyedit to be completed prior to publication.
30525	91	37			'is by definition a multidecadal mode': not clear to me why AMO/V should be multidecadal 'by definition'? [ Gilles Delaygue, France]	Noted. All definitions are now contained in the technical annex on modes of variability.
127129	91	40	91	40	It would be nice to have an overall summary of modes of variability. This summary paragraph could summarize which ones are expected to be sensitive to climate change, whether change was observed, and how limited observations affected conclusions. [ Trigg Talley, United States of America]	Taken into account. The Executive Summary on modes of variability provides such a summary.
73775	91	42	91	42	For clarity, I suggest adding what is being assessed to the section title. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	See comment id 24077
102775	91	42	91	42	All other sections are equipped with a short leading paragraph. This is missing here. [ Philippe Tulkens, Belgium]	Noted. This is not required in this instance in our view.
19749	91	42	93	34	In many cases, it remains possible to alleviate the limitations listed here. One would like however to get a feeling for a priority order. Among eco-physiological rate parameters of marine organisms and the forcing due to aerosol-cloud interactions, for example, what is the most important? While certainly the reader may reach his/her own conclusions, opinions shared by the IPCC WG1 community, if any, would deserve to be listened to carefully. [ philippe waldteufel, France]	Noted. Section 2.5 has been completely rewritten following discussions across chapters.
34861	91	42	93	34	This section describing factors limiting the SOD assessment is very welcome, and seriously questions the degree of confidence used in many of the SOD conclusions. Please see general comment #15 above. [ Jim O'Brien, Ireland]	Noted. Comment above is unclear and no specific actions requested. We would note that the section does not undermine the preceding sections.
2957	91	42	93	34	The observed temperature in the Arctic region had the limitation ( Jianbin Huang, Xiangdong Zhang, Qiyi Zhang, Yanluan Lin, Mingju Hao, Yong Luo, Zongci Zhao, Yao Yao, Xin Chen, Lei Wang, Suping Nie, Yizhou Yin, Ying Xu and Jiansong Zhang, 2017, Recently amplified arctic warming has contributed to a continual global warming trend, Nature Climate Change, 10.1038/s41558-017-0009-5) [ Zong Ci Zhao, China]	Taken into account. The impact of under-sampling of the Arctic on global temperature estimates is discussed in the expanded box 2.3.
85021	91	42	95	6	No comments [ Katrine Husum, Norway]	Noted. No changes requested or made
7231	91	42			It is suggested to include a review of all of the "Low Confidence" statements within this Section. [ Asaad Irawan, Indonesia]	See response to 19749
67833	91	42			It is suggested to include a review of all of the "Low Confidence" statements within this Section. [ Ruandha Agung Sugardiman, Indonesia]	See response to 19749
73777	91	44	91	44	Change in-situ to in situ. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. We followed the style guide.
73779	91	46	91	46	Change in-situ to in situ. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. We followed the style guide.
73781	91	48	91	48	Change in-situ to in situ. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. We followed the style guide.
24077	91		93		Section 2.5 Limitations to the assessment, since this section is referring to limitation for the assessment of the entire chapter it could be more clearly reflected by adding: "Limitations to the assessment of climate systems" or similar [ Linn Berglund, Sweden]	Taken into account. Section has been completely redrafted based upon discussions across all chapters and with the bureau.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8927	92	8	92	9	"The oldest atmosphere sampled" excludes blue ice [ Robert Kopp, United States of America]	Taken into account. The blue ice data from Yan et al. (2019, Nature) do not define the concentration ranges during glacial-interglacial cycles because the stratigraphy is not continuous for the older part.
42897	92	9			poorly known or unknown? [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	See response to 19749
73783	92	14	92	14	Change in-situ to in situ. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. We followed the style guide.
6573	92	20	92	21	Please at least insert the word "direct" before "observational". Reanalyses assimilate observations. Even if they don't assimilate observations of marine air temperature, their analyses of the latter will be influenced not only by SST information but also by the assimilation of related observations such as of marine winds. Reanalyses provide observational evidence, though it is not by itself conclusive. Hence the need for direct observations of marine air temperatures. However, it should not be forgotten that if these observations come from large ships, marine air temperature will be evaluated by changing the height of the observations using boundary-layer modelling of the type employed also in reanalysis. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	See response to 19749
79045	92	20	92	25	If GSAT is needed going forward then the decline in air temperature measurements over the oceans needs to be halted or reversed. It's not simply a case of "further development", observations are needed. [ John Kennedy, France]	See response to 19749
4001	92	20	92	26	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. [ Guoyu Ren, China]	Noted. Urbanization is assessed in the main text for surface temperatures.
73785	92	21	92	21	Summaries should stand alone, please define GMST and GSAT. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. These have been defined earlier.
73787	92	23	92	23	Summaries should stand alone, please define SST and MAT. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. These have been defined earlier.
29897	92	23	92	23	"MAT" has not been defined in this chapter. [ Hernan Edgardo Sala, Argentina]	Noted. The term has been defined in prior text.
57767	92	23			Current marine air temperature measurements are made at different height based on different measurement platforms (ship, buoy, etc). The challenges caused by this varying measurement platform difference should also be acknowledged for the assessment. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Text revised to better reflect these issues but also text changed in cross-chapter box 2.3 and section 2.3.1 where much of this is very apposite.
73789	92	25	92	25	Summaries should stand alone, please define ESMs [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
73791	92	27	92	27	Summaries should stand alone, please define CE. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
537	92	27	92	28	This sentence ("Paleo estimates of global deep past temperatures prior to the CE tend to be based upon marine SST proxies ...") seems to be referring to a period in the distant past, far more distant than simply "prior to the Christian Era" would suggest. If "CE" refers to something other than the Christian Era, then that should be clarified. (There are too many acronyms in this chapter, making it very difficult for the reader.) Alternatively, maybe "global deep past temperatures" was meant to be "global deep-ocean past temperatures", in which case it would be important to change "deep" to "deep-ocean". [ Claire Parkinson, United States of America]	See response to 19749
4203	92	27	92	31	In contrast to abundant SST records there is very little known about terrestrial temps in deeper time due to (1) lack of continuous dated archives on land, and (2) few quantitative terrestrial proxies. This is probably worth mentioning as it represents a gap in the knowledge. [ Emily Dearing Crampton Flood, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; GMST estimates for pre-Holocene periods are based primarily on SST for reasons stated by reviewer. These are scaled to GMST using the land-sea relation in Figure 3.2b.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100629	92	30	92	30	Note: I think everything is poorly understood at some level. I would write here "...configured, leads to ..." [ Matthew Kohn, United States of America]	See response to 19749
83245	92	42	92	50	This paragraph should also include something about challenges and gaps relating to glaciers, ice sheets and terrestrial snow (and possibly also icebergs). [ Robert Massom, Australia]	See response to 19749
83239	92	43	92	45	Change "...there exists limited observational data on sea-ice thickness and snow thickness.....and Southern Ocean for winter conditions, and for Arctic permafrost conditions: to "...limited observational data exists on sea-ice thickness and snow-cover thickness for both the Arctic Ocean and Southern Ocean, and for Arctic and Antarctic permafrost conditions." [ Robert Massom, Australia]	See response to 19749
83241	92	43	92	45	Comment should be made here of the particular challenges to deriving accurate long-term sea ice thickness information over large scale scales around Antarctica using satellite radar and laser altimeters, and the crucial need for dedicated calibration and validation. [ Robert Massom, Australia]	See response to 19749
73793	92	49	92	49	Change 'behavior' to 'behaviour'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication so FAQ conforms to uniform style.
71625	92	52	93	12	Discussion in this part seem not to include the issues on accuracy of observations. It is better to contain some simple descriptions. In particular, ocean salinity observation has substantial problem stems from ambiguity in conductivity sensing. Most recently, Uchida et al. (2019) show a possibility of future salinity monitor.: Uchida, H., Kayukawa, Y. & Maeda, Y. Ultra high-resolution seawater density sensor based on a refractive index measurement using the spectroscopic interference method. Sci Rep 9, 15482 (2019). <a href="https://doi.org/10.1038/s41598-019-52020-z">https://doi.org/10.1038/s41598-019-52020-z</a> [ Shuhei Masuda, Japan]	See response to 19749
73795	92	53	92	53	Insert space between 2000 and m. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. Followed style guidance.
58095	93	1	93	1	The paper by Gouretski et al., 2000: 'Systematic errors as the cause for an apparent deep water property variability: global analysis of the WOCE and historical hydrographic data' is missing in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	See response to 19749
58097	93	1	93	1	The paper by Durack et al., 2016: 'Keeping the lights on for global ocean salinity observation' is missing in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	See response to 19749
35565	93	2	93	2	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Not applicable. Context unclear.
58099	93	3	93	3	The paper by Cessi et al., 2019: 'The Global Overturning Circulation' is missing in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	See response to 19749
73797	93	5	93	5	References should be in chronological order. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. IPCC guidelines were followed.
35567	93	5	93	5	Bibliographic citations in chronological order [ Carlos Antonio Poot Delgado, Mexico]	Accepted. IPCC guidelines were followed.
35569	93	7	93	7	Use published sources [ Carlos Antonio Poot Delgado, Mexico]	Noted. Use of references is consistent with IPCC guidance
58101	93	7	93	7	The paper by Gloege et al., 2020 is missing in the references. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Reference not used in chapter FGD.
102777	93	14	93	16	Use of the word understanding should be preceded by "our" or "scientific" [ Philippe Tulkens, Belgium]	See response to 19749
83243	93	14	93	22	What about major gaps in understanding of polar diversity and ecosystems e.g., in sea ice-covered regions? [ Robert Massom, Australia]	See response to 19749
99239	93	14	93	22	the assessment of climate change impacts on marine ecosystem is clearly the focus of WGII raising concerns of the basis of this assessment here lacking authors who have been selected to cover this skill set. [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	See response to 19749
73799	93	27	93	27	Delete , after 'America'. It is not required in this context. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	See response to 19749
30033	93	29	93	34	For multi-decadal SST changes at global and basin-wide scales, there is a coordinated inter-decadal variability, particularly between AMO and PDO. Please see (1) Zhang Z.Q. et al., 2018, J. Climate, 31: 5485-55006;(2) Li. Y., Y.H. Ding, W.J., 2017, Adv. Atmos. Science 34(7), 833-846; (3) Kosaka, 2018, Nature Geoscience, 11, 12-13 [ Yihui Ding, China]	See response to 19749



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
40133	94	0			FAQ2.1 is very nicely written and structured. [ TSU WGI, France]	Noted.
39657	94	0			- I would just slightly adapt the text to match the figure a bit better by making a new paragraph (or italicise) the point about the reversal of a long-term trend [ TSU WGI, France]	Accepted; separated the reversal point into a new paragraph.
32659	94	1	94	20	Discuss how the lack of reliable data globally has affected the analysis [ sadegh zeyaeyan, Iran]	Rejected; evidence behind the assessment is explained within the main text.
32989	94	1	94	20	Discuss how the lack of reliable data globally has affected the analysis [ Sahar Tajbakhsh Mosalman, Iran]	Rejected; evidence behind the assessment is explained within the main text.
2033	94	3	94	3	My understadning is that these FAQs should be able to stand alone separate from the report as a whole...as such I think that the time periods in this FAQ should be defend each time they appear. And in general some of the more technical language simplified. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; omitted "Pleistocene"; defined "Holocene" as beginning 12,000 years ago.
98357	94	3	95	5	The reasons for the differences in current warming, could have been listed as numbered or bulleted points for easier readability. [ Feba Francis, India]	Taken into account; reasons are featured as bulleted points in FAQ 2.1, Figure 1.
539	94	5	94	5	"over thousands of years" should be "over millions of years" or, better yet, "over millions (even billions) of years". [ Claire Parkinson, United States of America]	Accepted; replaced "thousands of years" with "always".
1239	94	5	94	5	"Earth's climate has changed naturally over thousands of years". It could equally say millions of years or billions of years. Perhaps say that the earth's climate has always been changing. The fact that it has always changed indicates that it's sensitive to varied conditions. Furthermore, we can monitor all physical mechanisms that affect the atmosphere and oceans to day, thanks to satellites, and can eliminate those natural causes that in the past were responsible for the variations in the climate. [ Rasmus Benestad, Norway]	Accepted; replaced "thousands of years" with "always".
73801	94	15	94	15	Ignore this, no comments. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Noted.
37107	94	18	94	19	This statement is an unproven claim. It is unclear whether the MWP, Roman Warm Period and Minoan Warm Periods were warmer or cooler. [ John McLean, Australia]	Taken into account; evidence for global mean temperature history is assessed in CH2 text.
541	94	18	94	20	On line 18, I recommend changing "very long time" to "long time"; and at the end of the sentence on line 20, I recommend adding "(and, on a much deeper time scale, for the last 55 million years)". [ Claire Parkinson, United States of America]	Taken into account; omitted "very"; rather than extending further back, text now places the cooling as "following the last major ice age".
98355	94	18	94	24	For the question of 'How is the current warming any different?', The response 'It's been a very long time since it's been this warm' sounds contradictory. The other reasons explained after this particular one are clear and understandable. Also, the time 125,000 years, raises a question of whether the warming is a different one or just an eccentricity forcing by the orbital cycles. [ Feba Francis, India]	Taken into account; moved this response to the end for better context rather than starting with this response; FAQ refers to FAQ 3.3 for information on the cause of the recent warming.
37109	94	19	94	23	We don't have to look back 125,000 years or anything like it. Alley 2004 provides data from the GISP2 project that indicates that the MWP, RWP and Minoan WP were all warmer than today, what's more the data indicates that temperatures for the last 800 years have been the longest cool period in the last 10,000 years. Plots from Alley's data can be found at multiple locations including <a href="http://mclean.ch/climate/Ice_cores.html">http://mclean.ch/climate/Ice_cores.html</a> . One might argue that GISP2 temperatures only applied to the northern hemisphere or to Greenland but previous IPCC reports have had no trouble asserting that just a few tree-ring proxies for temperature from the Yamal Peninsular applied to at least all of the northern hemisphere (or was it to the Earth as a whole?) [ John McLean, Australia]	Rejected; evidence for global mean temperature history is based on hundreds of sites, including the summit of the Greenland ice sheet, but not exclusively Greenland.
102779	94	20	94	20	Global average temperature = global average surface temperature [ Philippe Tulkens, Belgium]	Accepted; added "surface".
109751	94	20	94	20	I think it is fair to point out that this long-term cooling trend was punctuated by multi-centennial relatively warmer periods. [ Charpentier Ljungqvist Fredrik, Sweden]	Accepted; added, "punctuated by relatively warmer decades and centuries".
89311	94	20	94	23	"There is ongoing scientific discussion about whether the world is warmer now", true, but weak statement. If you look at this debate and the evidence pro and con, don't you think it is at least "likely" that it is warmer now? As IPCC authors you have the task to assess the evidence, not just say "there is a debate". Figure 1 Box 2.1, for example, suggests that even considering the error bar on the mid-holocene, it is warmer now. Likewise if you use the Marcott reconstruction. By the way the caption there refers to the Kaufman data as "multi-model" - shouldn't that read "multi-method" as in the paper title? [ Stefan Rahmstorf, Germany]	Accepted; replaced "still discussion" with "more likely than not" to match CH2 conclusion; corrected Cross-Chapter Box.1 Figure 1 caption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57769	94	20		33	"global average temperature" is unclear as the report trying to separate GMST and GSAT in the assessment. It should be specified in this context. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; added "surface".
14887	94	22	94	22	Section 2.3.3.3. indicates a significance uncertainty in the sea level chage between LIG and 'now' that is not reflected in this FAQ. Should it be? [ Marie-France Loutre, Switzerland]	Accepted; added range of values for global sea level, matching CH2 text.
52603	94	22	94	22	Here it is said "sea level was around 8 m higher", I suggest to say "sea level was around 7 m higher" to be consistent with the 7+/-4 m estimate. [ Gema Martínez-Méndez, Germany]	Taken into account; value now matches CH2 text.
42899	94	22			where does 8 m come from? Just because this is a FAQ desn't mean you should jump on a high value (3-11 is what you say earlier which is not 8) [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; added range of values for global sea level, matching CH2 text.
71881	94	22			Where does this figure of 8 m come from? What is the justification? [ John Church, Australia]	Accepted; added range of values for global sea level, matching CH2 text.
37111	94	26	94	36	Again, the GISP2 data disputes what is stated in this paragraph. [ John McLean, Australia]	Rejected; evidence for global mean temperature history is based on hundreds of sites, including the summit of the Greenland ice sheet, but not exclusively Greenland.
39757	94	27	94	27	"ice ages" -> This usage isn't consistent with the geologic definition for 'ice ages' (we're currently in an ice age) [ TSU WGI, France]	Accepted; omitted "ice ages".
14881	94	31	94	32	It would be usetul to provide rate of changes for 'rapid events' (for example, during an Heinrich Event or the Younger Dryas. [ Marie-France Loutre, Switzerland]	Taken into account; rate quoted for the transition includes post Younger Dryas warming.
127131	94	32	94	32	The "nearly 1°C" is inconsistent with page 37 of this chapter, which stated the rise as 1.06 or 1.10°C, depending on the metric. [ Trigg Talley, United States of America]	Taken into account; warming value is now consistent with CH2 assessed value, albeit simplified with qualifiers for purposes of FAQ.
6575	94	32	94	32	"Earth" could be changed to "the atmosphere at the surface of the Earth". [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; simplified terms are used for purposes of FAQ.
6577	94	32	94	32	Is it really "nearly 1°C"? The three estimates of the temperature increase from 1850-1900 to 2009-2018 reported in Table 2.4 are all over 1°C, with an average of 1.06°C for GMST, suggesting an increase of around 1.1°C for GSAT. And today we are in 2020, so one can add on another 0.1°C or so. So shouldn't "nearly 1°C" in fact be "around 1.2°C"? Although if one regards the targets of the Paris Agreement as applying to warming estimates available at the time of the Agreement one could reduce this perhaps to 1.1°C, replacing "1850-1900" by something like "the pre-industrial level on which the Paris Agreement was based". Note also that Chapter 3, page 3-4 line 18 uses 1.0-1.3°C for 2010-2019 relative to 1850-1900. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; warming value is now consistent with CH2 assessed value, albeit simplified with qualifiers for purposes of FAQ.
37113	94	32	94	32	More false claims about supposedly average *global* temperatures during 1850-1900 when the data was from less than 50% of the globe (and sometimes as little as 14%). This ignores ignores the lack of homogeneity in the distribution of the coverage and how it was heavily biased towards European data in the SH and a shipping route down through the South Atlantic and then eastwards to teh general region of Indonesia. [ John McLean, Australia]	Taken into account; uncertainties arising from the sparse coverage in the 1850-1900 period are incorporated into the uncertainties quoted in the assessment.
39741	94	32			"Earth has warmed nearly 1°C since 1850–1900" doesn't contrast with 1.1C in FAQ1.4? [ TSU WGI, France]	Taken into account; warming value is now consistent with CH2 assessed value, albeit simplified with qualifiers for purposes of FAQ.
89313	94	32			"Earth has warmed nearly 1°C since 1850–1900". What? It has warmed about 1.2 °C since then, if you smooth out inter-annual variability. Have a look at Fig. 2.11. [ Stefan Rahmstorf, Germany]	Taken into account; warming value is now consistent with CH2 assessed value, albeit simplified with qualifiers for purposes of FAQ.
73803	94	35	94	35	Replace 'years. But for' with 'years, but for' (better English). [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication so FAQ conforms to uniform style.
73805	94	36	94	36	Lack of clarity, do you mean any 50-year period over the last 150 years, or another time span? Please clarify. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; time span is stated as, "for the past 2000 years".
109753	94	36	94	36	Maybe add something like "expect for recovery after volcanic cooling" as the rate of increase may have been stronger after the mid-6th century cooling (some data indicates that this could have been the case). [ Charpentier Ljungqvist Fredrik, Sweden]	Rejected; evidence assessed in CH2 indicates that rate of warming over last 50 years was higher than any other 50-year period of the past 2000 years.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17807	94	38	94	43	Maybe I missed it, but I have not seen a paleo reference in the text to the statement that it is now warming almost everywhere. For the common Era, Neukom et al. 2019 (doi: 10.1038/s41586-019-1401-2) could be used. I am not sure if literature for earlier warm periods exist... [ Raphael Neukom, Switzerland]	Accepted; added statement about uniformity of recent warming to main CH2 text.
109755	94	42	94	42	Maybe add a "likely" or something similar considering the large uncertainties in available data for the first millennium CE limiting the robustness of coherency studies.Maybe add a "likley" or something similar considering the large uncertainties in available data for the first millenium CE limiting the robustness of coherency studies. [ Charpentier Ljungqvist Fredrik, Sweden]	Taken into account; while likelihood statement is not included in FAQ, the confidence level is included in CH2 main text.
5439	94	42			"consistent" should be "uniform" [ Bryan Weare, United States of America]	Accepted; as suggested.
127133	94	45	94	45	Suggest an italicized header for this paragraph: "It's warming for different reasons." [ Trigg Talley, United States of America]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
5441	94	45	94	50	This pargraph need work. For instance: "Temperature fluctuations ...that cause temperature to change" "unusualness" [ Bryan Weare, United States of America]	Taken into account; paragraph extensively cut.
17791	94	45	94	50	The point about the causes of warming is the key argument, also for climate policy, so I suggest to make this very clear . [ Raphael Neukom, Switzerland]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
17789	94	45	94	52	Include "titles" for the last two paragraphs/distinctions in FAQ2.1, similar to the previous paragraphs/distinctions. For example: "The causes are different" as heading for the paragraph starting in line 45 and "Impact on societies and ecosystems are different" or "societies and ecosystems are in a different state now" for the paragraph starting in line 51 [ Raphael Neukom, Switzerland]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
40527	94	45		50	maybe slightly rephrase this paragraph to make it clearer that the unusualness of the warming (cf 4 characteristics previously mentioned) are due to the fact that warming is caused by human activities. i.e. reverse cause and consequences: currently it reads : human cause = uniqueness, while given the structure of the text, it might be better to human activities --> uniqueness [ TSU WGI, France]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
37115	94	47	94	50	These two sentences are false. There are other reasons why the Earth might warm, there is huge uncertainty about the accuracy of historical temperature data and no evidence whatsoever has been shown in this current chapter nor in chapter 1, nor even in chapter 3 FAQ 3.3 where the so-called evidence amounts to "models say so" (but models never provide evidence unless they have been validated) and the rate of warming is now different, which is nothing more than trying to claim that a correlation proves cause. As has been the case in past IPCC reports, you have no solid, irrefutable and consistent evidence to support your spurious claims. [ John McLean, Australia]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
18571	94	52	94	52	add "than" between 'fewer' and 'about' [ Miriam Jones, United States of America]	Taken into account; paragraph extensively cut.
15925	94	52	94	55	This sentence would be strengthened by acknowledging that UN predictions are for the population to expand further to 10.5 billion by mid century. [ Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
50695	94	52	95	5	This part of the answer is missing a couple of elements. SRCC and IPBES last year were clear that there is considerable human impact on ecosystems beyond climate change, but this currently reads a little like those impacts are purely driven by population growth and human development. However, SRCC also showed that impacts on land resources and ecosystems are a product of development pathways (e.g. risks are much lower under SSP1). I think this paragraph could benefit from a little nuance to make that clear. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
102781	94	52	95	5	These are key messages and should be incorporated into the SPM [ Philippe Tulkens, Belgium]	not applicable; comment belongs in SPM, probably WGII content.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39865	94	52	95	5	"there is at least one other major difference ...never before experience" maybe slightly rephrase this paragraph to focus more on the fact that unique global warming has unprecedented consequences [ TSU WGI, France]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
102783	94	53	94	53	"about" should be changed to "than" [ Philippe Tulkens, Belgium]	Taken into account; paragraph extensively cut.
71635	94		96		Is there a reason that the start of paragraphs are in italics? It is not consistent through all paragraphs, and the second FAQ. [ Jessica Hargreaves, Australia]	Rejected; italic font retained to connote headings.
71639	94		96		The FAQ section is great, I think this is a nice addition to this chapter and will be useful for science communication purposes. The narrative of the piece of writing is well established and aimed at the correct audience. The figures could be refined slightly, making the text bigger etc. Currently I think the arrows and boxes are a good choice, just needs to be easier to read. [ Jessica Hargreaves, Australia]	Noted.
23877	95	2	95	3	Please avoid slang such as "...it's..." [ Branko Grisogono, Croatia]	Editorial; copyedit to be completed prior to publication so FAQ conforms to uniform style.
18573	95	2	95	4	"wreck coastal communities" seems colloquial and subsequent clauses seem a bit disjointed. Suggested edit. "Warming is causing sea level rise, which impacts coastal communities, leads to habitat loss that exacerbates species extinction, and shifts water resources that threaten food and water security." [ Miriam Jones, United States of America]	Taken into account; paragraph extensively cut.
17089	95	2	95	4	I suggest these changes: Warming is causing sea level rise, which can wreck coastal communities; and it's shifting water resources that threatens food and water security, affecting habitats and exacerbating species extinction. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; paragraph extensively cut.
5443	95	2	95	5	Very awkward [ Bryan Weare, United States of America]	Taken into account; paragraph extensively cut.
127135	95	3	95	3	Suggest revising to, "which can exacerbate species extinction" since there will be different types of species responses to the rate and magnitude of warming. [ Trigg Talley, United States of America]	Taken into account; paragraph extensively cut.
105755	95	10	95	10	This FAQ highlights 3 point, but the figure has 5 images. Should these not self-consistent? [ Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; figure has 4 images that match the headings of FAQ.
40135	96	0			FAQ2.2 is very nicely written and structured but the text could be simplified here and there (e.g. permafrost (L35) or phenological metrics (L44) may sound a bit jargony to a lay audience) [ TSU WGI, France]	Taken into account - text revised (brief parenthetical descriptions of each term now included).
127137	96	1	96	1	Regarding the FAQ on evidence of climate change, suggest putting more emphasis on the global oceans. The large heat capacity of the ocean makes it a useful thermometer for the temperature of the planet (see Chen et al., 2017, <a href="https://eos.org/opinions/taking-the-pulse-of-the-planet">https://eos.org/opinions/taking-the-pulse-of-the-planet</a> ). Also, it has a major effect on other aspects of the climate systems, such as Antarctic ice sheet melting, thermal expansion, heat transport and currents, and extreme weather. [ Trigg Talley, United States of America]	Taken into account - text revised (section modified such that the global oceans receive more equitable discussion relative to the atmosphere, cryosphere, and biosphere).
5447	96	1	96	48	This entire important section needs to be tightened considerably. Is there evidence for "rad changes for ALL aspects of our global climate"? Lines 13-14 are not parallel. There is scant evidence in F.2.14c for "rising precipitation" as stated in line 21. The cryosphere paragraph is better at being specific about the time periods being referred to. The text and Fig. 1 are sometimes inconsistent. For instance the text refers to NH snow, but the figure says just snow. The text says nothing about snowpack mentioned in the figure. The text combines glaciers and ice sheets, but the figure combine sea ice and ice sheets. I am uncertain whether the climate link of "global greening" has been sufficiently establish to highlight it here. [ Bryan Weare, United States of America]	Taken into account - text revised (dropped reference to "all" aspects of global climate, specified time periods for atmospheric changes, ensured consistency between text and figure).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68053	96	1	96	55	An important piece of the evidence cited here is that there is a plausible working hypothesis which is grossly consistent with the evidence. That is to say, we can model, on a variety of temporal and spatial scales, and for a variety of diagnostics, the response to anthropogenic and natural forcings, and we can estimate the unforced variability. Those predictions do not allow us to reject the hypothesis that climate change is a response to a combination of natural and anthropogenic forcing; that warmer periods are moister and colder are drier; that past high CO2 intervals are more ice free and warmer; that there is Arctic amplification arising from positive feedbacks associated with ice; that there is substantial interannual variability in 'warmer' and 'colder' states which to first order sits atop the mean changes. I realize this is the subject of later chapters, but I think this is an important link and underlies the use of reanalysis, data assimilation, statistical modeling, bias correction, and other uses of models that underlie many analyses reviewed in this chapter. In other words, the assessment of 'is the climate changing' is framed by scientific hypothesis in the form of models. [ Michael Evans, United States of America]	Rejected - the focus of the FAQ is observational rather than model-based evidence.
4207	96	2	96	48	I am suggesting to indentify causes and effects of each meteorological parameter. For example the rise in surface temperature of the earth is the concentration of GHG emissions. The rise in surface temperature leads to mean sea level rise and more water vapor in the atmosphere [ Girmaw Bogale, Ethiopia]	Rejected - attribution is outside the scope of the chapter.
17091	96	3	96	3	This sentence is not coherent to the previous claim in Chap.2, FAQ 2.1, Page 95, line 9, which state that "the temperature of the planet's surface is a key indicator of its overall climate state". Therefore, I suggest these change: The evidence for climate change rests on more than just increasing surface temperatures and Evidence abounds from a broad range of indicators. Those indicators that collectively lead to the inescapable conclusion that we are witnessing rapid changes to all aspects of our global climate do exist. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the temperature of the planet's surface is a key indicator of the overall state of the climate. FAQ2.2 then goes on to discuss a broader suite of indicators.
96239	96	5	96	6	The issue of "detection and attribution" is covered in principle within different FAQs, but the terms are not mentioned explicitly. In order to include the term "detection" in FAQ 2.2, please re-phrase as "We are seeing Changes are detected in the atmosphere, oceans,... " [ Nicole Wilke, Germany]	Rejected - detection and attribution are outside the scope of the chapter.
81015	96	7	96	7	Reference is made to warming 'high in the atmosphere' which seems contradictory to evidence that the stratosphere has or maybe cooling. Perhaps modify this sentence to avoid ambiguity. [ Jeffrey Philip OBBARD, Singapore]	Taken into account - text revised (phrase deleted).
543	96	10	96	10	Since there were others well before the 16th century who could just as readily be labeled "scientists", and since it might be questionable to label some of the 16th century data collectors as "scientists", it would be better to replace "From the earliest scientists taking observations" by "From individuals taking observations". [ Claire Parkinson, United States of America]	Rejected - some scientists were taking observations in the 16th century.
81285	96	10	96	48	I suggest that as an argument for climate change you mention that ALL climate subsystems have increased in energy storage which is consistent to an energy imbalance from increased atmospheric concentrations of GHGs. You do mention the ocean which is the largest component, but perhapd including heat storage increases at all climate system reservoirs leaves out the possibility of doubt that based on "the possibility that others reservoirs cooiled". This also the case in the Figure FAQ2.2 There only Ocean heat content is mentioned. You should include Land, Atmosphere, Cryosphere, heat increases The latest reference is: von Schuckmann, K., Cheng, L., Palmer, M. D., Tassone, C., Aich, V., Adusumilli, S., Beltrami, H., Boyer, T., Cuesta-Valero, F. J., Desbruyères, D., Domingues, C., García-García, A., Gentine, P., Gilson, J., Gorfer, M., Haimberger, L., Ishii, M., Johnson, G. C., Killik, R., King, B. A., Kirchengast, G., Kolodziejczyk, N., Lyman, J., Marzeion, B., Mayer, M., Monier, M., Monselesan, D. P., Purkey, S., Roemmich, D., Schweiger, A., Seneviratne, S. I., Shepherd, A., Slater, D. A., Steiner, A. K., Straneo, F., Timmermans, M.-L., and Wijffels, S. E.: Heat stored in the Earth system: Where does the energy go? The GCOS Earth heat inventory team, Earth Syst. Sci. Data Discuss., <a href="https://doi.org/10.5194/essd-2019-255">https://doi.org/10.5194/essd-2019-255</a> , in review, 2020. [ Hugo Beltrami, Canada]	Rejected. Key references are already included.

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57771	96	17			"Global mean land and ocean surface temperature" need to be rephrased to identify whether it is for surface temperature or surface air temperature. One suggested change is "Global mean surface temperature and global mean air temperature over land and ocean" to be consistent throughout the report. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - text revised (now specified as global mean surface temperature).
96241	96	17			The issue of "detection and attribution" is covered in principle within different FAQs, but the terms are not mentioned explicitly. In order to include the term "detection" in FAQ 2.2, please insert "... that has warmed since the Industrial Revolution. The process of identifying such a change is referred to as "detection"." [ Nicole Wilke, Germany]	Rejected - detection and attribution are outside the scope of the chapter.
73807	96	20	96	20	Capital 'T' for 'troposphere'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the 'T' in 'troposphere' is not capitalized by convention.
17093	96	20	96	21	I suggest these changes: with associated increases in atmospheric water vapor and evidences of rising and/ or decreasing precipitation trend in many regions. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - chapter assesses large-scale (i.e., global and hemispheric) rather than regional-scale changes.
73809	96	21	96	21	Change 'vapor' to 'vapour'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
6579	96	25	96	25	The sentence that starts this line should at least have "been" before "transmitted", although changing "transmitted to" to "reached" or "has occurred down to" are other options. [ Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
83247	96	32	96	37	Add here that on the Antarctica, major ice shelves that had been in place for millenia catastrophically disintegrated in a matter of weeks (e.g., Larsen B in 2002). These ice shelf disintegration events (Larsen A and B and Wilkins) since the 1990s were largely unanticipated and shocking in their abruptness, rapidity and scale. [ Robert Massom, Australia]	Rejected - individual extreme events are outside the scope of the chapter.
73983	96	32	96	37	As soon as you define Cryosphere as the portion of the Earth where water is seasonally or continuously present as snow and ice, it would be necessary to consider processes in Boreal regions without permafrost for completeness and for better understanding climate change consequences. These regions experience pressure on their biodiversity, forestry etc. , but the new report ignores these problems. See also previous comments. [ Elena Kozlovskaya, Finland]	Rejected -- processes and regional changes are the purview of later chapters.
83249	96	33	96	34	Change "There have been decreases in Arctic sea ice area and thickness since the mid-1970s" to "There have been decreases in Arctic sea ice area and thickness and major changes in regional and seasonal sea-ice coverage around Antarctica since the mid-1970s". It's very important to acknowledge the major change and variability in Antarctica as well as in the Arctic regarding sea ice. [ Robert Massom, Australia]	Taken into account - text revised (sentence rewritten per reviewer suggestion).
57647	96	37			A mention of ice shelf collapse particularly in Antarctica could be useful as a further example of significant change in the cryosphere [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected - extreme events are outside the scope of the chapter.
83251	96	39	96	44	Add something about change to the polar ecosystems e.g., the effect of loss of sea ice on polar bears, walrus, seals etc, and change in penguin populations etc. around Antarctica. [ Robert Massom, Australia]	Rejected - attribution is outside the scope of the chapter.
96243	96	39	96	44	Please indicate the references to underpin these statements. [ Nicole Wilke, Germany]	Rejected - references are not included in the FAQs.
40793	96	46		48	should you specify in the conclusion that the warming is caused by human emissions of GHG? [ TSU WGI, France]	Rejected - attribution is outside the scope of the chapter.
83253	98	1	152	37	The format of the Reference List is inconsistent e.g., the use of capitals throughout the titles of some papers, but not others. [ Robert Massom, Australia]	Editorial; copyedit to be completed prior to publication.
69821	98	1	152	37	Too many typos, repetition, unnecessary symbols in the reference list. [ Kaoru Kubota, Japan]	Editorial; copyedit to be completed prior to publication.
13309	100	19	100	19	Misuse of } { [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
13311	100	22	100	22	Misuse of } { [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
13313	100	28	100	28	Misuse of } { [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.
95835	101	7	101	7	This paper has just been submitted as didn't apparently undergo any kind of reviewing. Is it ethical to consider such a paper here while many other peer-reviewed papers could be cited here but cannot by lack of space, and isn't there a risk to put some pressure on the referees of the paper to accept it because of its citation in IPCC AR6 (what would not be ethical) ? [ Christine Bingen, Belgium]	Rejected. Papers are selected based on relevance, not space considerations.
13315	101	38	101	38	Misuse of } { [ Maria Amparo Martinez Arroyo, Mexico]	Editorial; copyedit to be completed prior to publication.

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29751	103	13	103	13	Delete "PAGES 2k Consortium" between the brackets, use simply "(2019)". [ Hernan Edgardo Sala, Argentina]	Noted. The reference has been amended accordingly
93403	103	28	103	28	The right reference here is Burke et al., 2018: Burke, K. D., Williams, J. W., Chandler, M. A., Haywood, A. M., Lunt, D. J., & Otto-Bliesner, B. L. (2018). Pliocene and Eocene provide best analogs for near-future climates. Proceedings of the National Academy of Sciences, 115(52), 13288 LP – 13293. <a href="https://doi.org/10.1073/pnas.1809600115">https://doi.org/10.1073/pnas.1809600115</a> . There is a Burke et al 2019 in the list of references, but on another topic. [ Carles Pelejero, Spain]	Noted. The reference has been amended accordingly.
23785	105	18	105	20	Likely this sentence defining Ch10 needs to be revised slightly. The aspect of "an assessment of observed changes at regional scales" implies that all regions could be covered. Instead, this summary sentence must give the impression that Ch10 uses worked examples to illustrate how regional climate information can be assembled. I suggest liaising with Ch10 to craft this statement. [ Andrew Turner, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Unable to locate sentence in text. Incorrect page / line numbers, but nonetheless sentence quoted is not present. Misplaced comment?
17793	107	25	107	25	This is a duplicate reference (same as the next one). Remove. [ Raphael Neukom, Switzerland]	Noted. Has been removed.
17795	107	31	107	31	Publication year of this paper is 2020, not 2017 [ Raphael Neukom, Switzerland]	Noted. Has been corrected.
3955	109	12	109	16	no {} [ Sabine Baumann, Germany]	Editorial; copyedit to be completed prior to publication.
3957	109	40	109	44	no {} [ Sabine Baumann, Germany]	Editorial; copyedit to be completed prior to publication.
67717	109	53	109	56	Are these two references the same or not? Same range of pages for both papers is strange. [ Hiroaki Kondo, Japan]	Noted. Has been corrected.
8681	109	53	109	56	Dunn et al 2017a and 2017b are the same paper (also Ch2 p45/l47 and p46/l6) [ Robert Dunn, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Has been corrected.
3959	110	19	110	20	unedited reference [ Sabine Baumann, Germany]	Noted. Has been corrected.
93377	110	19	110	23	Repeated reference, Dyez et al. [ Carles Pelejero, Spain]	Editorial; copyedit to be completed prior to publication.
3961	110	29	110	29	no {} [ Sabine Baumann, Germany]	Editorial; copyedit to be completed prior to publication.
3963	111	36	111	36	no {} [ Sabine Baumann, Germany]	Editorial; copyedit to be completed prior to publication.
3965	112	14	112	15	no {} [ Sabine Baumann, Germany]	Editorial; copyedit to be completed prior to publication.
109015	112	30	112	30	reference Frajka-Williams, E., and Co-Authors is not complete or wrong ... please remove the status 'submitted' and update 'https://www.frontiersin.org/articles/10.3389/fmars.2019.00260/full' [ Belen Martrat, Spain]	Noted. Has been corrected.
3967	112	56	112	61	unedited reference and no {} [ Sabine Baumann, Germany]	Editorial; copyedit to be completed prior to publication.
109013	117	38	117	38	Change 'Hernandez, 2019 ...' to 'Hernandez et al., 2020' and complete the reference... to include the 18 authors of this study [ Belen Martrat, Spain]	Noted. Has been corrected.
74313	118	31	118	38	Duplicated reference, and neither is current: Hollis, C. J., Dunkley Jones, T., Anagnostou, E., Bijl, P. K., Cramwinckel, M. J., Cui, Y., Dickens, G. R., Edgar, K. M., Eley, Y., Evans, D., Foster, G. L., Frieling, J., Inglis, G. N., Kennedy, E. M., Kozdon, R., Lauretano, V., Lear, C. H., Littler, K., Lourens, L., Meckler, A. N., Naafs, B. D. A., Pälike, H., Pancost, R. D., Pearson, P. N., Röhl, U., Royer, D. L., Salzmann, U., Schubert, B. A., Seebeck, H., Sluijs, A., Speijer, R. P., Stassen, P., Tierney, J., Tripathi, A., Wade, B., Westerhold, T., Witkowski, C., Zachos, J. C., Zhang, Y. G., Huber, M., and Lunt, D. J., 2019, The DeepMIP contribution to PMIP4: methodologies for selection, compilation and analysis of latest Paleocene and early Eocene climate proxy data, incorporating version 0.1 of the DeepMIP database: Geosci. Model Dev., v. 12, no. 7, p. 3149-3206. [ Christopher Hollis, New Zealand]	Noted. Has been corrected.
93373	118	31	118	38	Repeated reference, Hollis et al., and update reference since it has been already published. <a href="https://doi.org/10.5194/gmd-12-3149-2019">https://doi.org/10.5194/gmd-12-3149-2019</a> [ Carles Pelejero, Spain]	Noted. Has been corrected.
21185	118	31	118	38	The two citations of Hollis et al. 2019 are identical. In the main text 2019, 2019a and 2019b are given. These are all the same. [ Robert Speijer, Belgium]	Noted. Has been corrected.
2007	118	31	118	38	Hollis et al reference appears twice. [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Has been corrected.
3969	118	48	118	48	unedited reference [ Sabine Baumann, Germany]	Noted. Has been corrected.
26667	121	38	121	43	These 3 references seem to refer to only one article, which has been published in between. [ Eric Brun, France]	Noted. Has been corrected.
109007	121	40	121	43	reference Kaufman et al., submitted Sci Data: duplicated, please remove one and update [ Belen Martrat, Spain]	Noted. Has been corrected.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21237	121	42	121	43	replace by the proper reference: "Kaufman, D., McKay, N., Routson, C. et al. A global database of Holocene paleotemperature records. <i>Sci Data</i> 7, 115 (2020). <a href="https://doi.org/10.1038/s41597-020-0445-3">https://doi.org/10.1038/s41597-020-0445-3</a> " [ Michael Schmitt, Germany]	Noted. Has been corrected.
58077	121	44	121	46	The date is missing for the paper by Kayano et al. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Has been corrected.
79031	122	11	122	16	Kennedy et al. 2019 is cited twice. [ John Kennedy, France]	Editorial; copyedit to be completed prior to publication.
95829	122	57	122	57	This paper is has not been accepted, and the acceptance should be a request before being cited in the assessment report. [ Christine Bingen, Belgium]	Accepted.
7153	123	30	123	30	Please replace F.G. by F. Gaillard in the authors list. [ Nicolas Kolodziejczyk, France]	Noted. Has been corrected.
58055	123	42	123	45	Something went wrong with the reference manager. The paper by Koutavas and Joanides, 2012 is listed twice. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Has been corrected.
115543	124	55	124	55	The WMO report has been published now [ Rolf Müller, Germany]	Noted with thanks.
26669	125	5	125	6	We suggest a more appropriate reference : "La2010: a new orbital solution for the long-term motion of the Earth, J Laskar, A Fienga, M Gastineau, H Manche - <i>Astronomy &amp; Astrophysics</i> , 2011 etc.) [ Eric Brun, France]	Rejected. Unable to place comment in context.
58067	125	23	125	28	Something went wrong with the reference manager. The paper by Ledru et al. (2013) is listed twice. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Has been corrected.
35903	125	37	125	39	Reference info updated to: Lee, J., Sperber, K. R., Gleckler, P. J., Bonfils, C. J. W., and Taylor, K. E. (2019). Quantifying the agreement between observed and simulated extratropical modes of interannual variability. <i>Clim. Dyn.</i> 52, 4057–4089. doi:10.1007/s00382-018-4355-4. [ Jiwoo Lee, United States of America]	Noted. Has been corrected.
98757	126	44	126	44	Should Lisiecki and Raymo (2005) be cited if it's used in a figure? [ Meredith Parish, United States of America]	Accepted. Has been corrected.
3971	126	59	126	59	wrong letter in name, maybe ö or ä [ Sabine Baumann, Germany]	Noted. Has been corrected.
1987	128	55	128	59	Martinez-Boti reference appears twice [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Has been corrected.
93527	130	12	130	17	McLandress et al 2015a and 2015b are the same references! [ Michaela Hegglin, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Has been corrected.
30527	130	43			the correct DOI is 10.1175/JCLI-D-19-0135.1 [ Gilles Delaygue, France]	Noted. Has been corrected.
3973	134	1	134	3	no {} [ Sabine Baumann, Germany]	Editorial; copyedit to be completed prior to publication.
67371	135	6	135	7	Between these two lines, add this reference: Peings, Y., Cattiaux, J., Vavrus, S. and Magnusdottir, G. (2017). Late Twenty-First-Century Changes in the Midlatitude Atmospheric Circulation in the CESM Large Ensemble. <i>Journal of Climate</i> , 30(15), 5943–5960. [ Martin Stendel, Denmark]	Noted. Has been corrected.
67373	135	6	135	7	Between these two lines, add this reference: Peings, Y., Cattiaux, J., Vavrus, S. J. and Magnusdottir, G. (2018). Projected squeezing of the wintertime North-Atlantic jet. <i>Environmental Research Letters: Env. Res. Lett.</i> , 13(7), 074016. [ Martin Stendel, Denmark]	Noted. Has been corrected.
2919	135	23	135	30	Are the simulations CMIP5? [ Zong Ci Zhao, China]	Not applicable. Misplaced comment. This is an observations chapter.
109009	138	50	138	55	reference Routson et al., <i>Nature</i> : duplicated [ Belen Martrat, Spain]	Noted. Has been corrected.
3975	141	52	141	52	authors' names should not be in capital letters [ Sabine Baumann, Germany]	Noted. Has been corrected.
10243	142	61	143	4	Duplicate Sosdian et al (2018) references. [ Chris Vivian, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Has been corrected.
57987	142	61	143	4	Something went wrong with listing the paper by Sosdian et al., 2018. It is listed twice in the reference list, with the first one containing the page number and the second one containing the doi. Probably an error in the citation manager. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Has been corrected.
93385	142	61	143	4	Repeated reference, Sosdian et al [ Carles Pelejero, Spain]	Noted. Has been corrected.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21587	144	1	144	1	A huge amount of data going off the y-axis is really bad. Chapter 2 assessed several data products and found considerable uncertainty. Why is this only considering one? Suggest expand the axis-range and bring in several or ideally all the observational products assessed in chapter 2 and make sure you are using the same versions. Overall figure also needs a title so it is self describing. [ Peter Thorne, Ireland]	Misplaced comment, should be for Chapter 3. Response from Chapter 3 LA, Marcelo Barreiro: Accepted. Figure will be redrawn taking the suggestions into consideration. (Response replicated here for completeness of record.)
58059	145	3	145	8	Something went wrong with the reference manager. The paper by Thompson et al., 2017 is listed twice. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Noted. Has been corrected.
93393	147	57	147	57	Ref repeated three times [ Carles Pelejero, Spain]	Noted. Has been corrected.
457	153	1	153	5	The Quick Guide on the right of Figure 2.1 is confusing in its incompleteness. It would help to simply change "Quick Guide" to "Quick Guide to Selected Key Topics". [ Claire Parkinson, United States of America]	See response to 116021
4601	153	1	153	8	Section 2.3 covers changes in the large-scale climate. The subsequent sub-sections do not reflect changes in the climate only. Recommend that Atmosphere be in a separate section than the remaining 3 sub-sections [ Andries Kruger, South Africa]	See response to 116021
57649	153	1			Figure 2.1: in the 'Quick Guide' column to the right, move the number of the subsection above its title, e.g. '2.2.3' comes before 'well-mixed greenhouse gases'. Also align this whole column with the bottom part of the 'Cross-Chapter boxes' for a more coherent layout. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	See response to 116021
57651	153	1			Figure 2.1: according to the Table of content, section 2.2 is called 'changes in climate drivers, section 2.3.4 is called 'synthesis of evidence for past changes, section 2.5 is called 'limitations to assessment'. Change names accordingly in the figure. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	See response to 116021
57653	153	1			Figure 2.1: for consistency, I'd suggest you have all the subsections for section 2.2 and 2.4 written out in the figure, similarly to section 2.3. Alternatively, have only one image per section on the right, and the name of the section along with its subsections aligned on the left. This way the layout might be clearer and more coherent. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	See response to 116021
116021	153		153		an example for land biosphere is relevant too (eg greening). [ Valerie Masson-Delmotte, France]	Noted. TSU have decided to redraw all figures of chapter roadmaps which is ongoing at the time of finalisation of these comment responses.
42125	154	1	154	1	include legend with colours for future scenarios [ Julia Nabel, Germany]	Taken into account; figure extensively revised.
42127	154	1	154	1	update to Raymo et al., 2018 in figure [ Julia Nabel, Germany]	Taken into account; Raymo dataset no longer used.
1585	154	1	154	29	The various timescales are clearer, but it's the words that do this, it's not the 24 hour clock. My main point here is the ranges on the projections. The uncertainty ranges for the earlier epochs are given, but for the million year timescale these appear small, compared to the future at year 2300. Uncertainties in decades vs uncertainties for millennia. Might be better if the diagram only showed the last millennium. 5-60 millennia ago aren't compatible due to the continents moving. [ Philip Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; added "characteristic uncertainty" error bars for each timescale. Blow-up of global temperature over the past 1000 years is in Figure 2.11a and in Figure 3.2c.
8929	154	1	154	30	Absence of uncertainty in the paleo record is not defensible. [ Robert Kopp, United States of America]	Taken into account; added "characteristic uncertainty" error bars for each timescale.
36343	154	1	154	34	Whatever else that must be cut to save space, keep this figure! It summarizes the chapter magnificently, containing in one graphic a wealth of information (in contrast to the silly "visual abstract"). You will probably need to explain a bit more, e.g. how did you convert Antarctic- and Greenland-averaged temperatures to GMST? [ Curt Covey, United States of America]	Taken into account; polar ice sheet data no longer used.
30529	154	1			CC box 2.1 Figure 1: 1. an IPCC report should only contain realistic information, hence figuring an analogy with a 24h day must be abandoned (although an interesting concept). 2. reference to Lisiecki & Raymo in the graph should be changed to Raymo et al (2018) given in the legend. [ Gilles Delaygue, France]	Taken into account; Cenozoic clock omitted; Lisiecki & Raymo dataset no longer used.
15219	154	1			Will the final Figure 1 have uncertainty shading (e.g. for Holocene temperatures)? It's not absolutely necessary. However, otherwise, the graph implies that 2019 temperatures are certainly greater than any time since the LIG, which though possible, is not very high confidence in the text. [ Simon Donner, Canada]	Taken into account; added "characteristic uncertainty" error bars for each timescale.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100631	154	2	154	2	Note: Add bar for MCO 16.9 to 14.7 Ma [ Matthew Kohn, United States of America]	Accepted; Miocene climate optimum added as a paleoclimate reference period in Cross-Chapter Box 2.1.
29535	154	2	154	2	Arrows along the top of the figure, for Medieval Warm Period and Little Ice Age should not extend into the Holocene box. [ Kevin Burke, United States of America]	Taken into account; figure extensively revised.
98759	154	2	154	2	Should be Lisiecki and Raymo, not Lisiecki and Raymon. I also think it was published in 2005, even though it's referred to as LR04. [ Meredith Parish, United States of America]	Taken into account; figure extensively revised.
26671	154	2	154	3	Figure 1 : 2019 should be displayed in vertical position (to avoid the possibility that the red line / error bar correspond to 2019...) [ Eric Brun, France]	Taken into account; figure extensively revised.
69823	154	2	154	3	Typo in the fig. 1 (not Raymon (2004), but Raymo (2004) [ Kaoru Kubota, Japan]	Taken into account; figure extensively revised.
99745	154	2	154	4	Typo in reference in figure panel "Pliocene" - should be Lisiecki and Raymo (2004) [ Kira Rehfeld, Germany]	Taken into account; figure extensively revised.
127139	154	2			It's Lisiecki and Raymo (2005). There's a typo and wrong year in the figure. [ Trigg Talley, United States of America]	Taken into account; figure extensively revised.
26675	154	6	154	6	Figure 1 : We suggest to display changes in GSAT instead of changes in GMST in order to ensure the coherence with the general approach of Volume 1. [ Eric Brun, France]	Rejected; proxy data attest to changes in GMST rather than GSAT.
89319	154	6			Box 2.1, Figure 1: this figure is a good overview e.g. for students (and I use a similar one in teaching) but too complex for a wider audience, e.g. with the different time scales and different types of data. No newspaper would print this graph. An important key message is lost, namely that we are witnessing an extremely rapid warming which has stopped short a natural slow cooling trend. What is needed - also for the SPM - is a much simpler graph like this one: <a href="https://twitter.com/rahmstorf/status/1267699267881271296/photo/1">https://twitter.com/rahmstorf/status/1267699267881271296/photo/1</a> Which could of course include the new Kaufman data in addition to or instead of the Marcott data. [ Stefan Rahmstorf, Germany]	Taken into account; figure extensively revised to reduce complexity. Figure similar to reviewer's suggestion is included in Figure 2.11. Figure TS.1 also conveys this message.
26677	154	8	154	8	We suggest to use same units/abbreviations than in Box 2.1, Table 1 [ Eric Brun, France]	Taken into account; figure extensively revised.
127141	154	17	154	18	In a comment to Section 2.3.1.1, it was noted that many of the best estimates in this figure are actually not assessed in the text. Also, plus or minus 2 standard deviations is appropriate for a statistical estimate but not an expert judgment, which should be expressed as a "very likely" range or something similar. [ Trigg Talley, United States of America]	Taken into account; switched from 2 SD to 5% to 95% (very likely range), with more emphasis on confidence levels, but retaining quantitative ranges. Direct comparison between benthic-isotope-inferred temperature and assessed temperatures based on multiple proxies is now done in Figure 1.5.
42903	154	18	154	23	I can live with it but I am curious why you used Lisiecki and Raymo, which has nothing to do with surface temperature, and GRIP/EPICA which is hardly a basis for a global average, rather than Snyder and then Shakun which both at least try to reconstruct a global average surface temperature. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; neither Lisiecki & Raymo, nor the polar dataset are used in extensively revised version.
26673	154	19	154	19	Figure 1: Lisiecki and Raymon 2004 should be referred in legend as Raymo et al., 2018 [ Eric Brun, France]	Taken into account; figure extensively revised.
26679	154	19	154	19	It is shown in figure as Lisiecki and Raymo 2004 and not Raymo et al., 2018. [ Eric Brun, France]	Taken into account; figure extensively revised.
29533	154	19	154	19	Citation of Raymo et al., 2018 is incorrect. This data is from: Lisiecki LE, Raymo ME (2005) A Pliocene-Pleistocene stack of 57 globally distributed benthic $\delta^{18}O$ records. Paleoclimatology 20:PA1003. [ Kevin Burke, United States of America]	Taken into account; figure extensively revised.
127143	154	19	154	19	The Raymo citation in the caption does not match the Raymo citation in the figure. [ Trigg Talley, United States of America]	Taken into account; figure extensively revised.
127145	154	21	154	21	Do authors mean "transformed to global temperature"? [ Trigg Talley, United States of America]	Accepted; added "global".
17797	154	23	154	23	Publication year for Kaufmann et al. is wrong (9999) [ Raphael Neukom, Switzerland]	Editorial; copyedit to be completed prior to publication.
127147	154	23	154	23	Just a note that there are three Kaufman et al. references in the works cited list and none of them have dates attached to them. [ Trigg Talley, United States of America]	Editorial; copyedit to be completed prior to publication.
69825	154	23	154	23	Should be Kaufman et al., "submitted"? , instead of 9999 [ Kaoru Kubota, Japan]	Editorial; copyedit to be completed prior to publication.
109011	154	23	154	23	Kaufman et al 9999; please correct [ Belen Martrat, Spain]	Editorial; copyedit to be completed prior to publication.
83955	154	23	154	23	please correct (Kaufmann et al., 9999), [ Marco Tulio Cabral, Brazil]	Editorial; copyedit to be completed prior to publication.
113651	154	23	154	23	"Kaufman et al., 9999" -- please correct the year of the publication [ Agnieszka Kowalczyk, Poland]	Editorial; copyedit to be completed prior to publication.
42129	154	25	154	26	why are the grey lines between ~1960 and 2019 thinner? [ Julia Nabel, Germany]	Taken into account; figure extensively revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
8931	154	26	154	28	The 24-hour clock is a cute addition, but needs to be more clearly explained [ Robert Kopp, United States of America]	Taken into account; figure extensively revised; Cenozoic clock no longer included.
42131	154	27	154	27	"begin at 24:00:00 hours at 66 Ma and end at 00:00:00 hours at 2300" This is nice but could have one sentence explanation, i.e. that the timeline is represented as a day for illustrational purposes? Maybe consider to add more timesteps at e.g. ~1 Mio., 300 Thousand and 10 Thousand? [ Julia Nabel, Germany]	Taken into account; figure extensively revised; Cenozoic clock no longer included.
29543	154	29	154	29	Please include citation in figure legend attributing the original publication. E.g. Figure design adapted from Burke et al., 2018. Full Citation: Burke KD, Williams JW, Chandler MA, Haywood AM, Lunt DJ, Otto-Bliesner BL. 2018 Pliocene and Eocene provide best analogs for near-future climates. Proc. Natl. Acad. Sci. USA 115, 13 288–13 293. (doi:10.1073/pnas.1809600115) [ Kevin Burke, United States of America]	Taken into account; figure extensively revised.
116023	154		154		Very nice figure. Please consider other estimates of past global temperature (last deglaciation, glacial interglacial periods) in addition to ice core based data (to coordinate / ch 1 too). The size of the panels make recent warming look extremely compressed. Please consider other approaches, having in mind what is the key message from the figure. Maybe a different y axis starting from the Pliocene? More recent scaling of Greenland and Antarctic records to temperature than cited here exist. It could be good to communicate on this figure on the main drivers of these past changes too. [ Valerie Masson-Delmotte, France]	Taken into account; (1) polar ice sheet data no longer used; (2) GMST reconstruction here matches CH1; (3) one key message is how recent and projected temperature compare with Cenozoic temperature; therefore, double y-axis would complicate this message; (4) main drivers are presented in CCB2.1 Table 1
15173	154				Will the final Figure 1 have uncertainty shading (e.g. for Holocene temperatures)? It's not absolutely necessary. However, otherwise, the graph implies that 2019 temperatures are certainly greater than any time since the LIG, which though possible, is not very high confidence in the text. [ Simon Donner, Canada]	Taken into account; added "characteristic uncertainty" error bars for each timescale
17813	155	1	155	10	It is difficult to align the temperature events and the volcanic events with the subpanels in this order. It would be much more effective if the top row were subpanels a, b and the bottom row were c, d so that the time axes were aligned vertically. [ Baylor Fox-Kemper, United States of America]	Reject. The reviewer has a good point, but it would come at the expense of not being able to show how the lower panels relate to the upper ones (with the dashed lines). It also might give the (wrong) impression that the two time series might be related by some means.
10549	155	1	155	20	The differences between this plot and the solar and volcanic forcings in Figure 2.10 need commenting on. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The clarification is added to the FAIR data table explanations.
45307	155	2	190	2	I would make sure all the time series plots have x-axis label on there. It's missing in many figures and can be misleading without any context. [ Anson Cheung, United States of America]	Reject. The IPCC guideline is not to label x-axes that clearly are time series.
79231	155	3	155	21	Figure 2.2d: It looks like there was a relatively large volcanic eruption in the 1850s in the CMIP5 dataset which disappeared in the CMIP6 dataset. Can the authors provide some details why that is the case? [ Martin Stolpe, Switzerland]	Noted. The newer dataset (that relies on ice core data) is an improvement over the former one (that only had volcanological data). Unfortunately space limitation does not permit to discuss details of the evolution in the Chapter 2 text.
10543	155	6	155	20	The recommended CMIP5 TSI has been offset in panel b to be similar to the recommended CMIP6 data. That is fine, but it needs explaining. i.e., CMIP5 was around 1365Wm-2 ( <a href="https://solarisheppa.geomar.de/cmip5">https://solarisheppa.geomar.de/cmip5</a> ), not the 1361Wm-2 recommended to be used in CMIP6. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Explanation added.
10545	155	6	155	20	It is fine to show CMIP6 recommended values, but those are a few years old now. Are there any more up to date dataset that should also be shown in this chapter, that may differ somewhat? [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The volcanic forcing is the most up-to-date data, and for TSI the data are updated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10547	155	6	155	20	Showing volcanic changes as forcing, (RF or ERF?), is confusing when referring to CMIP5/CMIP6. Volcanoes were not (almost universally) applied to historical model simulations as changes in radiative forcing, but rather as changes in simulated stratospheric aerosol. So the diagnosed forcing across models are actually somewhat different. Also they don't always use the same source aerosol dataset. Some will use SO2 emissions, others concentrations of aerosol. I don't believe a specific volcanic dataset was a formal CMIP5 recommendation. Given how specific this is to individual models, I think this is an example of a possible odd place to have this plot (Chapter 7 seems more natural to discuss model forcings). [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It is the ERF inferred from the SAOD that is reported here, not an ERF based on model results (which are discussed in Chapter 3).
15853	155	6	155	20	The figure 2.2 title could be shortened and be more concise: a lot of acronyms made it difficult to follow. Maybe include legend in the figure with color and the corresponding acronyms? [ Fei Luo, Netherlands]	Noted. However, the number of acronyms is appropriate for the figure context.
79151	155	10	155	11	"including the contribution from 200-400 nm": I find this very confusing and misleading. TSI, by definition, is an integral over the entire spectral range. Thus the variability in TSI includes the variability at 200-400 nm by definition. We do not write explicitly "including 500-700" or "1000-3000 nm range". When this is stated, the reader might think some other reconstructions do not include this, which is not correct. [ Natalie Krivova, Germany]	Accepted. the statement is revised and moved to the FAIR data table.
116025	155		155		Please use a RF scale for TSI too. [ Valerie Masson-Delmotte, France]	Accepted.
109197	156	0	156	18	Figure 2.3 (and probably others) would really benefit from a visual legend/key, especially because some of the timeseries data look very similar to the imposed statistical trends. [ Steph Courtney, United States of America]	Taken into account - a clearer legend has been added.
42133	156	1	156	1	for illustrative purposes it would be nice to also have the grey "homo sapiens" bar as in Fig.1 here in panel (c) [ Julia Nabel, Germany]	Rejected - The aim of this figure was to present the CO2 data from the last 450 million years in a clear fashion. Additional labelling was considered to clutter the figure and make it harder to see the CO2 data.
30723	156	1	156	2	In Figure 2.3 trace c and b, the Data of Cui et al might be added to fill the gaps especially in trace c: Ying Cui, Brian A. Schubert, A. Hope Jahren; A 23 m.y. record of low atmospheric CO2. Geology doi: <a href="https://doi.org/10.1130/G47681.1">https://doi.org/10.1130/G47681.1</a> [ Michael Schmitt, Germany]	Rejected - given that recent studies have cast doubt on the accuracy of the D13C method (e.g. Lomax et al. 2019 GCA, <a href="https://doi.org/10.1016/j.gca.2018.12.026">https://doi.org/10.1016/j.gca.2018.12.026</a> ) we have focused on the results from the marine-based proxies d13C of alkenones, d13C of phytane and d11B of foraminifera.
45309	156	2	156	2	I think it would be easier to have a legend on the side to indicate what each line/marker represent. Currently, it's a bit confusing to see labels indicating some proxies (but not all) on the figures and can only find a more detailed explanation in the figure caption. [ Anson Cheung, United States of America]	Taken into account - a clearer legend has been added.
100633	156	3	156	3	Note: The values calculated by Pagani et al. for low pCO2 through the middle Cenozoic have largely been abandoned (especially Pagani et al., 2005). SST's were often dramatically underestimated, which reduces calculated pCO2 - See Super et al. (2018). Also, recent work (e.g., Stoll et al., 2019; Badger et al., 2019; Zhang et al., 2019) indicates that pCO2 could have been underestimated by a factor of ~2-3. [ Matthew Kohn, United States of America]	Taken into account. We now only plot the data from Stoll et al. (2019) for the last 23 Ma from the Alkenone proxy.
100635	156	3	156	3	Note: I think panel b should be reevaluated because many of these pCO2 estimates are known to be inaccurate. I recommend coordinating with Bärbel Hönlisch (Columbia University), who is leading the paleo-pCO2 working group on this. There will be a paper submitted later this summer on pruning the questionable estimates and assigning realistic uncertainties. [ Matthew Kohn, United States of America]	Taken into account - see comment #100633. No publication has emerged from the paleo-CO2 working group in time for the IPCC deadline. However, we note that the data plotted in panel (b) are often the same datasets compiled by that working group (albeit restricted to the marine proxies - the reasons for this are now detailed in the text accompanying this figure).
100637	156	3	156	3	Note: Revisit primary sources and update everything. [ Matthew Kohn, United States of America]	Taken into account. Relevant data are checked and updated. See response to the other comments.
99225	156	3			The largest part of the text focusses on the more recent part of the geological record raising questions of the policy relevance of the top panel of the figure [ Daniela Schmidt, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - we agree that the majority of the text concerns panel b and c, but we wish to include panel a as it sets the scene for rest of the discussion.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
100639	156	11	156	11	Note: include leaf gas exchange and stomatal frequency estimates [ Matthew Kohn, United States of America]	Rejected. Stomatal frequency estimates are included in panel a. Leaf gas exchange estimates are not included in the compilation of Foster et al. (2017) and as now discussed in the text we rely more on marine-based proxies to define the Cenozoic trend in CO <sub>2</sub> given their greater data density and smaller age uncertainty.
83467	156	19	156	28	right column panel with CO <sub>2</sub> data: keep in mind that color blind people cannot distinguish between red and green. [ Antje H. L. Voelker, Portugal]	Rejected. We use different three groups of colours to distinguish CO <sub>2</sub> , CH <sub>4</sub> and N <sub>2</sub> O data sets. It is difficult to avoid to use both green and red colours.
116027	156		156		Could the drivers of past changes in CO <sub>2</sub> concentration be also mentioned somewhere? (not covered in ch 5 I think). [ Valerie Masson-Delmotte, France]	Noted. In the updated SOD, CH <sub>5</sub> deals with deep past records and carbon emission rate.
109199	157	0	157	40	There are a lot of figures where the dotted lines connecting broader time series to narrower ones below is cluttered and confusing, but it definitely doesn't work in Fig 2.4. Consider instead giving distinct boxes and titles to b) and c), and if you want to show the relationship on a) there could be a color-coded bar/dot below the corresponding sections of the x axis. Additionally, the color-coding of axis labels in b) and c) is more confusing since there are multiple colors of data shown - not sure what the fix is since the color-coding is useful in a), but in b) it makes it looks like the WAIS data set isn't CO <sub>2</sub> -- maybe just make dataset colors more standard and label them spatially rather than by color [ Steph Courtney, United States of America]	Taken into account. The panel (a) and (b) are combined. The (c) is separately presented. The colours are now more consistent within the figure 2.4.
42135	157	1	157	1	I find the choices, label and numbering of the different x-axis confusing. All x-axis indicate "years before 1950", however, the CO <sub>2</sub> values (as well as the y-axis) in panel (a) indicate 20XX values around year zero / end of x-axis. Also the "x 10 <sup>3</sup> " is only added to the first timepoint of the x-axis in panel (a) and (b) [ Julia Nabel, Germany]	Taken into account. Now the x-axis labels in (a) and (b) are "thousands of years before 2000", and in (c) "Year (CE)"
42137	157	1	157	1	panel (c): colour choices could better distinguish the different GHGs, particularly the WAIS Divide colour for CO <sub>2</sub> might be changed to orange as in (b) and e.g. a yellow or dark red could be used for EDML [ Julia Nabel, Germany]	Accepted. The colours are changed so that they better be discerned.
42139	157	1	157	1	for better readability it would be nice if the second y-axis (CH <sub>4</sub> ) could have a small offset such that the lines at the end of the timespan do not overlap with the axis, particularly in panel (a) [ Julia Nabel, Germany]	Taken into account. The y-axis is relocated so that it has a small offset
45311	157	1	157	1	The time axis of subplot b is confusing. My understanding is that this subplot emphasizes deglaciation. However, only the oldest x axis tick mark indicates 20000 years before 1950 whereas the other x tick marks seem to suggest few 10 years before 1950. I would make sure it's clear that all the tick marks are in thousand years. [ Anson Cheung, United States of America]	Taken into account. Now the x-axis label is "thousands of years before 2000" which is consistent with that in chapter 5.
17815	157	1	157	10	The lines connecting the N <sub>2</sub> O record in the upper panel connect across gaps in the data, it seems. That should be avoided. Generally, using lines to connect intermittent data is maybe better avoided, and a scatter plot might be better? This approach is used in the lower panels, where the data is more dense, but I think it might be even more critical when the data is less dense. [ Baylor Fox-Kemper, United States of America]	Taken into account. The lines that connect N <sub>2</sub> O gaps are removed.
127149	157	1	157	40	In Figure 2.4, panel b, rather than having one of the tick mark labels say 20 x 10 <sup>3</sup> , express the axis label as "Thousands of years before 1950". [ Trigg Talley, United States of America]	Taken into account. The panel (b) becomes to the inset of panel (a) with the x-axis label "thousands of years before 2000".
127151	157	1	157	40	[PRECISION] In Figure 2.4, panel c, consider replacing or adding an axis for years CE. Also, since panel a goes to nearly the present-day, panel c ought to extend to near the present-day too. [ Trigg Talley, United States of America]	Taken into account. Now the x-axis label is "Year (CE)" only. The recent concentrations are also indicated in the graph.
5333	157	1	157	53	A simple global temperature reference curve would be very useful for one or more of these panels. In panel (c) the CO <sub>2</sub> should be in some range of red/orange like the other panels, not the green of CH <sub>4</sub> . [ Bryan Weare, United States of America]	Rejected. The comparison of global temperature with greenhouse gas concentrations are not the main topic of Chapter 2. Thus we present them separately. In (c) the colours are changed so that they are red, orange, pink and reddish brown.
15221	157	1			Figure 2.4, panel (a). The N <sub>2</sub> O time series has a straight line from ~300kya to ~240 kya and again from ~240 kya to ~220 kya. These are presumably gaps in the record and should be blank? Other gaps are left blank. [ Simon Donner, Canada]	Taken into account. The lines that connect N <sub>2</sub> O gaps are removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
42905	157	4	157	13	Part a of figure needs redrafting - the modern vertical lines all run into each other (for CO <sub>2</sub> , CH <sub>4</sub> , N <sub>2</sub> O) so it's hard to see what the modern values are. And the palaeo data are too squashed on the y axis to see the glacial variability properly. It needs a redesign. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The concentrations in 2019 are added in the figure.
26681	157	38	157	38	The x-axis in these figures is questionable, since the 'x10 <sup>3</sup> ' (for thousands of years) are 'lost' in the left part of the figure. A common age scale in kilo-years (even for the lower right panel) should be more explicit. [ Eric Brun, France]	Taken into account. Now the x-axis label is "thousands of years before 2000" which is consistent with that in chapter 5.
116029	157		157		Th figure needs work to show data points + correct for lab differences for ice core measurements (as done in papers of Malte Meinshausen for defining boundary conditions for PMIP simulations). Please think of what is the key message of the figure (not the analytical noise but the signal extracted). [ Valerie Masson-Delmotte, France]	Taken into account. To figure out offsets among different ice core records, we made CO <sub>2</sub> and N <sub>2</sub> O composite data sets for figure 2.4(b) which was figure 2.4 (c) in the previous manuscript version.
15175	157				Figure 2.4, panel (a). The N <sub>2</sub> O time series has a straight line from ~300kya to ~240 kya and again from ~240 kya to ~220 kya. These are presumably gaps in the record and should be blank? Other gaps are left blank. [ Simon Donner, Canada]	Taken into account. The lines that connect N <sub>2</sub> O gaps are removed.
29899	159	3	159	4	Consider adding in the Figure 2.6 a vertical line signaling the year of Montreal Protocol as an important time reference and for visual aid also. [ Hernan Edgardo Sala, Argentina]	Rejected. The suggestion would complicate the presentation of the figure.
81339	159	9	159	9	Why does the PFC category in this figure not include other compounds such as c-C <sub>4</sub> F <sub>8</sub> , c-C <sub>4</sub> F <sub>8</sub> O, SF <sub>5</sub> CF <sub>3</sub> , n-C <sub>4</sub> F <sub>10</sub> , n-C <sub>5</sub> F <sub>12</sub> , n-C <sub>6</sub> F <sub>14</sub> , i-C <sub>6</sub> F <sub>14</sub> , and n-C <sub>7</sub> F <sub>16</sub> ? This is puzzling as a) most of these compounds were included in AR5, b) the combined radiative forcing from these is much larger than that of C <sub>3</sub> F <sub>8</sub> (which is included), and c) recent (apart from one) publications provided atmospheric records for many of them (Muehle et al., 2019; Sturges et al., 2012; Vollmer et al., 2019; Droste et al., 2020, all ACP). HFC-43-10mee is missing from the HFC list. Also, please correct the formulas of CH <sub>2</sub> Cl <sub>2</sub> and CHCl <sub>3</sub> . [ Johannes Laube, Germany]	Noted. The figure displays mainly components with RF>0.001 Wm <sup>-2</sup> and refers to Annex 3 for further components.
67663	160	7	160	7	annual mean total ozone is a proxy for stratospheric ozone, not a good proxy. And, it is a proxy for the stratospheric column, not values at specific levels, which actually matter for radiative forcing considerations. [ Karen Rosenlof, United States of America]	Accepted: we have modified the sentences into "a good proxy for vertically integrated stratospheric ozone". We retain that this is a good proxy.
87659	160	7	160	7	The caption says 2018 but the plots only go up to 2014. I really hope these can be updated because the extra years will make things look somewhat different, especially in Antarctica. [ Matthew Tully, Australia]	Rejected. The values are going to 2018.
87657	160	9	160	11	Should you also express a Dobson Unit in SI units? ie 0.4462 mmol/m <sup>2</sup> [ Matthew Tully, Australia]	Rejected. We refer to the glossary for a full explanation on the SI equivalent to Dobson unit.
90283	160	10			insert space between 1mm to 1 mm [ Jeannine-Marie St-Jacques, Canada]	Accepted. Space added.
127153	160		160	14	The text is very hard to read on these panels. [ Trigg Talley, United States of America]	Accepted the graphical quality and font size were improved.
90281	160				The grey axes and grey tics in figure 2.7 does not work - too hard to see. All other figures have black lines and tics for their axes and tics. Change. Also figure seems of poorer resolution than the others. [ Jeannine-Marie St-Jacques, Canada]	Accepted the graphical quality and font size were improved.
5339	161	1	161	44	The dramatic differences between the "high elevation" and "aircraft" trends need to be addressed, especially given both are judged to be highly significant. [ Bryan Weare, United States of America]	Rejected. We do not discuss in detail in the text the difference between high-altitude and aircraft trends, however this issue was specifically addressed by Cooper et al. 2020. They showed that the aircraft data are representative of the lower free troposphere, whereas, the high elevation observations are impacted by the boundary layer and therefore have a very different footprint. The decreasing ozone trends at the high elevation sites in the middle of Europe and in the middle of North America are impacted by regional scale decreases of ozone in the boundary layer, whereas the aircraft positive trends in the lower free-troposphere reflect the positive trends also observed in the mid- and upper troposphere.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87661	161	1	161	49	In the middle panel - Figure 2.8 b - the points are all from aircraft with one exception, that of Hilo ozonesondes. Why would you include one station in the whole world? Global ozonesonde sites are represented in the third panel. I suggest removing Hilo (unless you want to include all the other sites). [ Matthew Tully, Australia]	Rejected. Figure 2.8b does not show all published ozone trends in the mid-and upper troposphere (see review by Cooper et al. 2020) because some results are reported for different layers of the atmosphere, or for different time spans, or because they are based on seasonal data rather than annual data. The trend in the mid-troposphere above Hilo, Hawaii (Chang et al., 2020) was calculated using the same method as the trends based on IAGOS data (Gaudel et al., 2020). Therefore, to show as much data as possible we chose to include the methodologically consistent trend from Hilo. This is explained now in the caption.
477	161	18	161	21	In Figure 2.8, the color coding gives two colors for each of the first three rows of p values. Either the caption should explain the reason for the two colors or, if there is no necessary reason, the figure should be changed to have only one color for each of the p value divisions. [ Claire Parkinson, United States of America]	Accepted. The colours indicating p-values are revised to an IPCC-approved monochromatic scale, with no difference between p-values for positive or negative trends. Most of the studies reported by Figure 2.8 accounted for autocorrelation.
127155	161		161	49	The colors used in this figure are hard to interpret intuitively as levels of statistical significance. Suggest using a tool like ColorBrewer to pick a better gradient of colors. Also, is autocorrelation accounted for when calculating these p-values? [ Trigg Talley, United States of America]	Accepted. The colours indicating p-values are revised to an IPCC-approved monochromatic scale, with no difference between p-values for positive or negative trends. Most of the studies reported by Figure 2.8 accounted for autocorrelation.
58277	162	1	162	1	Figure no 2.09 please check the dates in text and the date in the title of this figure as it is contradictory e.g page 2-26 line 13 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted. Dates checked and made consistent.
58259	162	7	162	32	Nowhere in the caption, nor in the main text, the relation between panels a-b) and c-d) has been explicitly mentioned. This would help the interpretation of the figure as a whole. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted and explanation added.
113093	162	23	162	23	Spectroradiometer' [ Diego Miralles, Belgium]	Editorial; copyedit to be completed prior to publication.
113095	162	25	162	35	Three inconsistent formats for presenting time ranges in which a few lines ' – ', '–', ' '. [ Diego Miralles, Belgium]	Editorial; copyedit to be completed prior to publication.
10551	162	27	162	29	How are the trends deduced to be significant? Not many of the boxes are significant... one would expect a certain number to appear to be significant even if no signal was present. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. An explanation is added.
35943	162	29	162	31	Figure 2.9: Very nice to superimpose AERONET trends! [ Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thanks for the nice words!
4633	163	1	163	1	Shouldn't the line at 2000 in the "Rate of change" block be extended back to 1950? [ Andries Kruger, South Africa]	Taken into account. Figure updated.
99449	163	1	163	1	Figure 2.10. Part of the inset figure in the lower right corner (with the title 'Rate of change of anthropogenic ERF') is covered by white rectangle masking part. Found around years 1950-2000 along the x-axis, and 0.2 Wm <sup>-2</sup> at the y-axis. [ Johan Friberg, Sweden]	Taken into account. Figure updated.
17799	163	1	163	1	The rate of change in anthropogenic ERF is not visible between 1950 and 1995. [ Raphael Neukom, Switzerland]	Taken into account. Figure updated.
42143	163	1	163	1	Maybe add uncertainty range for total forcing [ Julia Nabel, Germany]	Taken into account. The reader is now referred to the extra uncertainty analysis in Chapter 7.
42145	163	1	163	1	Without further explanation the small plot could look a bit arbitrary and not objective due to specific selected timespans [ Julia Nabel, Germany]	Taken into account. A clarification is added.
58279	163	1	163	1	Figure 2.10 Rate of change of anthropogenic ERF is shown as an insert graph but there is not data from 1950 to 200 is the data missing or its not available also I recommend to use line graph here also which can give better information [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure updated.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
21235	163	1	163	1	In the inset of Figure 2.10 the line between 1950 and 2000 at 0.2 Wm-2 is missing [ Michael Schmitt, Germany]	Taken into account. Figure updated.
29901	163	1	163	2	Check the inset of Figure 2.10 (see Rate of change of anthropogenic ERF, 1950-2000). It seems to be a data gap there (?). [ Hernan Edgardo Sala, Argentina]	Taken into account. Figure updated.
18391	163	1	163	3	Yellow colors for solar and aerosols are too similar and may be confusing. Would make sense to change one of them in order to increase the contrast. [ Olga Solomina, Russian Federation]	Taken into account. Figure updated.
10553	163	1	163	7	I assume that within the little panel that there is an invisible line with 0Wm-2/decade between 1950 and 2000. This should be made visible. [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure updated.
109201	163	1	163	15	Figure 2.10 is also important so I'll be nit-picky -- the visual key is good but solar and aerosol are too similar to tell apart. Either make more distinct, or move key to align with final values of each forcing, especially since the colors are muddled by uncertainty ranges on many of the lines. Some other small errors I assume will be corrected such as anthropogenic typo and 1950-2000 lower-right trend not showing up. I also think the lower-right rate of change box could be shown better, either with finer temporal resolution, more space or explanation given to it, etc. [ Steph Courtney, United States of America]	Taken into account. Figure updated.
112675	163	1	163	17	The total ERF line in Figure 2.10 does not visualise the 5 to 95% uncertainty range of the total forcing. This is important information, because it is a crucial uncertainty in climate science. Either the complete line of Total ERF could visualise the uncertainty through the same shading method used for the individual components or the uncertainty can be given at the end of the total line. Table 7.8 gives the following estimates for the Total ERF: 2.53 [1.56 to 3.32], which could be used here. [ Leon Simons, Netherlands]	Taken into account.
112677	163	1	163	17	In the small plot in the lower right of figure 2.10, the rate of change for the period 1950-2000 is missing. [ Leon Simons, Netherlands]	Taken into account. Figure updated.
30531	163	1			Figure 2.10. In the legend (top left) 'Other anthropogenic'. In the bottom right graph of changing rate, the bar over the 1950-2000 period is hidden (by the legend, i assume). [ Gilles Delaigue, France]	Taken into account. Figure updated.
30533	163	1			Figure 2.10. Since ERF are calculated wrt year 1750, i guess the volcanic forcing should be zero in 1750, which is not -exactly- the case. [ Gilles Delaigue, France]	Noted. The explanation is added to the corresponding FAIR data table notes.
30535	163	1			Figure 2.10. i suggest an additional plot showing the total (or anthropogenic) ERF curve and its uncertainty: i think it'd be very valuable to realize when this ERF was strictly positive or could have been negative, especially wrt the surface temperature record. [ Gilles Delaigue, France]	Taken into account. The reader is now referred to the extra uncertainty analysis in Chapter 7.
15223	163	1			Figure 2.10 - note that the 1950-2000 ERF rate line did not reproduce, or is in error [ Simon Donner, Canada]	Taken into account. Figure updated.
81341	163	2	163	2	The legend in this figure should say "other anthropogenic". [ Johannes Laube, Germany]	Taken into account. Figure updated.
127157	163	15	163	17	While a drafting error has somewhat mangled the small plot in the lower right of Figure 2.10, even in its perfect state, it would show remarkably little information. Better would be running 20-year changes in total anthropogenic ERF, estimated either by simply 20-year differences or by a linear fit to overlapping 20-year segments. [ Trigg Talley, United States of America]	Accepted. The way of performing the analysis is revised.
127159	163		163	23	The 1950-2000 period is missing in the sub-plot showing the linear trends (only one short black line in the late 1990s). Perhaps it's masked by the plot title? [ Trigg Talley, United States of America]	Taken into account. Figure updated.
127161	163		163	23	Change the color of the linear trend lines in the small sub-plot. Either that or move the small plot up a little bit more into the main figure area. It's easy for someone to miss the first lines for 1850-1900 since it coincides with the main plot x-axis. [ Trigg Talley, United States of America]	Taken into account. Figure updated.
116031	163		163		please show on this figure the total anthropogenic and the total forcing. For the lower panel on rates of changes, it would be much better to have a separate panel, with a better visual representation (incl. uncertainties) + a decomposition of causes for increased rate of change (for recent decades, stable or declining negative effect of aerosols + increasing CO2). [ Valerie Masson-Delmotte, France]	Taken into account. The total ERF is shown, the total anthropogenic not for clarity. Unfortunately space did not permit to expand on the rate of change analysis.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24391	163		163		Figure 2.10: The box in the lower right hand corner should have a horizontal bar that spans 1950 to 2000, but it only appears as a very short bar above 2000. [ Owen Cooper, United States of America]	Taken into account. Figure updated.
24393	163		163		Figure 2.10: Figure 2.2(d) shows that volcanic radiative forcing is either zero or negative, as expected, because the volcanic aerosol reflects sunlight. However, Figure 2.10 seems to indicate that in the absence of strong volcanic eruptions, there is always a slight positive radiative forcing of approximately +0.2 W m <sup>-2</sup> . Is this an error? Should the maximum value be zero rather than 0.2? [ Owen Cooper, United States of America]	Accepted. The volcanic forcing is corrected.
26063	163		163		In figure 2.10, the small plot (bottom right) shows for the period 1950-2000 a very short line not coinciding with the corresponding years on the x-axis, possibly hidden by the label "ERF" [ Don Alfonso Pino Maeso, Spain]	Taken into account. Figure updated.
15177	163				Figure 2.10 - note that the 1950-2000 ERF rate line did not reproduce, or is in error [ Simon Donner, Canada]	Taken into account. Figure updated.
90295	163				spelling of "other anthropogenic" in legend for figure 2.10 [ Jeannine-Marie St-Jacques, Canada]	Accepted.
90297	163				inset in figure 2.10 mangled. Line for 1950-2000 covered up mostly by subtitle [ Jeannine-Marie St-Jacques, Canada]	Taken into account. Figure updated.
58205	164	0	164	0	For Fig.2.11: would putting 2.11c to the right of 2.11a allow easier visual comparison of temperature profiles? 2.11d could then go below 2.11c. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account; figure extensively revised.
7409	164	0	164	0	The quality of the ifgure is poor. Please check that it is a vector format. [ Jeremy PANTHOU, France]	Taken into account; figure extensively revised.
42147	164	1	164	1	select differred colours for last glacial and mid Holocene bars to prevent overlap with red and gold colours selected for the temperature records [ Julia Nabel, Germany]	Taken into account; figure extensively revised.
42149	164	1	164	1	Redundant °C in panel B [ Julia Nabel, Germany]	Taken into account; figure extensively revised.
4525	164	1	164	1	Figure 2.11: 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. Furthermore the new Neukom et al. 2019 and PAGES2k 2019 temperature series should not be used as its database has been critized to be parly flawed and incomplete. See discussion in: Lüning et al. (2019): 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. Paleocceanography 32 (11): 1219-1235, doi: 10.1002/2017PA003237, Lüning et al. 2019: The Medieval Climate Anomaly in the Mediterranean region. Paleocceanography and Paleoclimatology, 34 (10): 1625-1649, doi: 10.1029/2019PA003734, The Medieval Climate Anomaly in Oceania. Environmental Reviews, doi: 10.1139/er-2019-0012, Lüning, S., M. Galka, F. Vahrenholt (2019): The Medieval Climate Anomaly in Antarctica. Palaeogeogr., Palaeoclimatol., Palaeoecol., 532, doi: 10.1016/j.palaeo.2019.109251. I strongly advise against returning to the hockey stick era with its unsupportable claim of lack of pre-industrial climate change. [ Sebastian Luening, Switzerland]	Rejected; CH2 remit includes placing recent climate changes in a long-term context. CH2 does not "claim lack of pre-industrial climate change". Only one of the cited papers mentions the PAGES 2019 reconstruction, but with no evidence that it is flawed.
21241	164	1	164	1	The error bars of the blue dot (Last glacial maximum) in Figure 2.11A do not correspond to the value of -6 \pm 1.5°C but imply an upper limit of +2°C [ Michael Schmitt, Germany]	Taken into account; figure extensively revised.
18393	164	1	164	3	Last glacial maximum temperature marked at the upper panel looks confusing. [ Olga Solomina, Russian Federation]	Taken into account; figure extensively revised.
10555	164	1	164	9	An axis is needed on right hand side on panels b and c. That is where all the interesting climate change is happening! [ Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; figure extensively revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
70179	164	1	164	19	Reading this section it is unclear to me where the mid-Holocene error estimate of 0.3°C in Figure 2.11 comes from. At first glance it seems to conflict with the error indicated in the text: 0.5°C (0.1, 1.7). Is this simply due to the translation of the 5-95 percentile range to $\pm 2$ SD intervals? It does refer to the mid-Holocene estimate of Kaufman et al. (submitted a), right? Perhaps it would be an option to add some clarification or a reference to Kaufman et al. (submitted a) here, to make this clearer. "best estimate" is a bit unclear considering that many studies are cited in 2.3.1.1.2. [ Oliver Heiri, Switzerland]	Taken into account; figure extensively revised.
112899	164	1	165	30	Figure 2.11 and Cross-Chapter Box 2.3, Figure 1, depict the HadCRUTv5 (Morice et al., submitted). Yet, HadCRUT5.0 should not be used anywhere in the AR6 report because it is not published nor discussed, analyzed or studied by the scientific community yet. HadCRUT5.0 record varies significantly from HadCRUT4.6 for the period from 2000 to 2020 by showing a significant warming that is not seen in several other climatic records including in the HadCRUT4.6. The adoption of the unchecked HadCRUT5.0 record in AR6 questions the credibility of the IPCC. In fact, this record appears to have been chosen because it contradicts the HadCRUT4.6 during the last 20 years by not showing the temperature standstill from 2000 to 2015. Yet, the scientific community has not checked or studied the properties of this record. Please substitute the HadCRUT5.0 record with the HadCRUT4.6 everywhere in the report. In fact, there are several open issues with this records such as the possibility that the land record is severely affected by Urban Heat Island effects; for example see Scafetta, N., Shenghui, O., 2019. Detection of UHI bias in China climate network using Tmin and Tmax surface temperature divergence. Global and Planetary Change, 181, 102989. [ Nicola Scafetta, Italy]	Rejected. The use of papers submitted by December 2019 is in accordance with IPCC requirements. Only papers accepted by January 2021 are included in FGD. The text already discusses local UHI effects in China with a number of citations.
42151	164	7	164	8	move (red line) behind "three instrumental-based datasets"; which three of the five and why not all five? [ Julia Nabel, Germany]	Taken into account; figure extensively revised.
127163	164	8	164	8	Since these are best estimates, the bars in Figure 2.11 should represent a confidence range (such as "very likely") rather than something that represents a statistical sampling such as standard deviation. In this context, 2SD would literally correspond to the SD calculated from a sample of individual scientists' assessments. [ Trigg Talley, United States of America]	Accepted, as suggested.
70177	164	9	164	10	In Figure 2.11, caption it says (line 9-10): "Large circles are best estimates (bars are $\pm 2$ SD) for mid-Holocene, last glacial maximum, and last interglacial period based on the assessment in section 2.3.1.1.1". However, 2.3.1.1.1 deals with "deep time". 2.3.1.1.2 deals with the postglacial. So the reference should actually be to sections 2.3.1.1.1. AND 2.3.1.1.2? [ Oliver Heiri, Switzerland]	Accepted, as suggested.
26065	164		164		Figure 2.11 A. The figure is somehow misleading. Perhaps, it could be improved either with two clearly differentiated parts or better with the lower part zooming from x-axis around year 2000 and moving the text in-between possibly to the right (as in the bottom graph) [ Don Alfonso Pino Maeso, Spain]	Taken into account; figure extensively revised.
58215	165	0	165	0	For Cross-Chapter Box 2.3 Figure 1 (right panel): It's difficult to distinguish Implied GSAT (black) from GMST Projections (dark blue). Perhaps change colour of one of these lines? [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This figure is no longer being used.
58281	165	1	165	1	Cross box 2.3 Figure 1 as both the data from GSAT and GMST show almost similar trend up-to 2000 its difficult to differentiate I think using more bright colors can give some information in differentiating them. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Not applicable. This figure is no longer being used.
99407	165	1	165	30	The right panel of Box 2.3, Figure 1 is not easily comprehensible. It would help a lot if the terminology used in the figure is clearly defined in the text of Box 2.3, namely "hybrid" and "implied GSAT", which don't seem to be used in the text at all. Although the term "definition gap" is used in the text (p. 36, line 33-34), I don't find it obvious which two quantities this is a gap between. [ Herman Fuglestad, Norway]	Not applicable. This figure is no longer being used.
5353	165	1	165	30	Some mention must be made of the extreme variability in the 1990-present values, which is not seen in the 75 year period before or the projections afterwards. [ Bryan Weare, United States of America]	Not applicable. This figure is no longer being used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
50697	165	1	165	32	The left hand side of cross chapter box 2.3 figure 1 is very clear, but the right hand side does not obviously communicate its key message. It is difficult to know which line to follow as there are multiple overlapping lines in the 2015-2018 period - it would be helpful to have an obviously defined switchover date from observations to projections. The natural variability in the data shown also makes it difficult to get across the concept - perhaps a more conceptual diagram of the definition gap, making it clear how it arises from the switchover between data sets would be helpful, and this could be done for two different switchover dates, thereby illustrating how the definition gap grows. If such a conceptual diagram were shown, the difference in rate of change of temperature could be artificially emphasised to make this point easier to get across. [ Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This figure is no longer being used.
99409	165	23	165	25	Using "surface air temperature" here adds to the confusion discussed in my comment #4, regarding the air temperature definitions on p. 35. [ Herman Fuglestad, Norway]	Not applicable. This figure is no longer being used.
58283	166	1	166	1	Fig 2.12 is possible to start the figure (b) from 5 or 7.5 as there is no data from 0 to 5 [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected. The 0km lower bound is retained for consistency with the other two panels.
112901	166	1	166	26	Figure 2.12 shows trends in the tropospheric temperature at various heights. The period analyzed is from 2002 to 2018. Yet this period is very questionable because a linear trend above the equator would be strongly biased by the strong ElNino effect between 2015 and 2016. Please, show the time series of the data that are fit, at least at 10km to evaluate the possible ELNino bias in the estimated trend. [ Nicola Scafetta, Italy]	Rejected. While the potential influence of the 2015-16 El Nino is noted, there were also El Nino events early in the period (2002-03, 2006-07).
72181	166	1	166	34	Trends in temperature at various atmospheric heights for 2002-2018, from the radiosonde data sets and radio occultation datasets differ significantly close to tropopause both in global data and in the tropics, but this discrepancy is poorly described in the text [ Joanna Wibig, Poland]	Taken into account. New text has been added to note the results of the Steiner et al 2020 paper that trends from the latest generation of radiosondes largely matched the RO results (implying that the difference in Figure 2.12 largely arises from as-yet-unresolved inhomogeneities in the larger radiosonde data sets).
52815	166	1	166	35	Fig. 2.12 is the only tropospheric trend diagram I see and it clearly misrepresents the evidence. This period is short (17 years) with a major warm ENSO near the end - misleading the readers on the actual tropospheric situation (i.e. an example of "cherry-picking" described above). It also uses only radiosondes which have been influenced by the warm shift in tropospheric temperatures due the software change in Vaisala RS92 radiosondes after 2009 as noted in Christy et al. 2018 (see above comment #1 for details). The current diagram would be shown to be misleading under normal cross-examination and critical review if it remains. There are other depictions of the tropospheric profile of trends from 1979 that are far more relevant to the topic discussed here, e.g. Christy and McNider 2017 or similar profile depictions in the BAMS State of the Climate reports. [ John christy, United States of America]	Accepted. A panel has been added to show 1980-2019 trends for those data sets where this period is available.
30537	166	1			Figure 2.12a: modify title to 'Near global' (polar vortices are not included) [ Gilles Delaygue, France]	Accepted. Title amended.
7411	167	0	167	0	Please check the unit of figure 2.13b). Please check : to me the relative humidity trend is in % per decade rather than in g/kg per decade. [ Jeremy PANTHOU, France]	Accepted. The relative humidity unit was changed.
30539	167	1			Figure 2.13b: i think the unit for relative humidity is not correct. [ Gilles Delaygue, France]	Accepted. The relative humidity unit was changed.
72183	167	2	167	3	Trend in relative humidity shouldn't be in g kg-1 per decade [ Joanna Wibig, Poland]	Accepted. The relative humidity unit was changed.
90813	167	5			High resolution scientific map/figure to be updated [ Vivien How, Malaysia]	Noted. Original resolution is higher than made available for SOD review.
98015	168	1	168	1	Poor graphics construction for panels a and b of Fig. 2.14. A different scale should be used rather than being tied to the one used for 1980-2018 trends. Over that shorter time period the trends are likely dominated by natural variability anyway, so no need to tie the 1901-2018 trend to the same color scale. Also the tiny gridboxes in panels a and b are hard to see. Could the results be presented at 5x5 degree scale? For a look at how a better scaled version of these would appear, see Fig. 3 of Knutson and Zeng (2018). Ref: Knutson, T.R. and F. Zeng, 2018: Model Assessment of Observed Precipitation Trends over Land Regions: Detectable Human Influences and Possible Low Bias in Model Trends. J. Climate, 31, 4617–4637, <a href="https://doi.org/10.1175/JCLI-D-17-0672.1">https://doi.org/10.1175/JCLI-D-17-0672.1</a> . [ Thomas Knutson, United States of America]	Taken into account. The new Figure 2.15 was improved for FGD considering new colorbars and different spatial resolution.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17817	168	1	168	10	I'm not sure if this is the stippling affecting my color perception, the underlying data actually disagreeing, or something else, but the disagreement among the products on the right column--even where stippled (e.g., central Africa)--is hard to understand in this figure. Is the intention here to illustrate agreement--in which case this figure doesn't serve--or disagreement in which case showing upper panels as a residual/difference from GPCP might be more direct? Or, it may be that the stippling is affecting my color perception right where the significant signals lie, which again should be rethought (e.g., by only shading where significance is high, rather than using stippling, or using much smaller points to stipple, or using white stippling to indicate lack of significance rather than dark stippling to indicate significance). [ Baylor Fox-Kemper, United States of America]	Taken into account. The new Figure 2.15 was improved for FGD considering new colorbars, different spatial resolution and an improved stippling.
5359	168	1	168	56	Clearly these observational data do not proceed beyond 2020 as is implied in c). Are these global values only for land? What is the meaning of global means otherwise? [ Bryan Weare, United States of America]	Noted. The panel c had an error in the labelling of the x-axis. This was corrected for the FGD. The global values in panel c) are only for land. This was clarified in the panel title.
45313	168	1	169	1	I don't think Red-Blue colorbar is best to display precipitation trend. Red-Blue is more associated with temperature, and other colormaps such as the one in Figure 2.13 might be more appropriate in the precipitation context. [ Anson Cheung, United States of America]	Taken into account. We changed the colorbars of the new Figures 2.15 and 2.16.
17095	168	24	168	43	Figure 2.14.c presents precipitation anomaly in lines. I wish the lines can be differentiated for the colour blind people. May be authors can draw it using several dash line types or symbols. The 2D spatial colour figure is OK as the colour blind people still able to recognize the spreading pattern. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The new Figure 2.15 was improved for FGD considering new colorbars.
113097	168	45	168	45	Capitalize 'Spatial'. [ Diego Miralles, Belgium]	Editorial - copyedit to be completed prior to publication.
495	168	54	168	54	"(e) and (f)" should be "(d) and (e)". [ Claire Parkinson, United States of America]	Editorial - copyedit to be completed prior to publication.
90331	168	54			I think that you are missing a ")" somewhere. Please double check. [ Jeannine-Marie St-Jacques, Canada]	Editorial - copyedit to be completed prior to publication.
127165	168		168	56	Year labels on x-axis in panel c) are misaligned. [ Trigg Talley, United States of America]	Noted. The panel c had an error in the labelling of the x-axis. This was corrected for the FGD.
24395	168		168		Figure 2.14: Panel c is supposed to show products based on observations through 2018, but the x-axis shows that data are being reported from 1890 to 2040. Are forecasts included after 2018, or is this just an error in the labelling of the x-axis? [ Owen Cooper, United States of America]	Noted. The panel c had an error in the labelling of the x-axis. This was corrected for the FGD.
17819	169	1	169	10	Smaller dots for stippling would help with color distortion effects. [ Baylor Fox-Kemper, United States of America]	Taken into account. The new Figure 2.16 was improved for FGD considering new stippling.
5361	169	1	169	37	I strongly question the inclusion of the two ERA20C analyses, which are basically extensions of surface data, even for this period of excellent observations. There is clearly an artificial jump in the late 1990s. These just confuse any interpretation. [ Bryan Weare, United States of America]	Taken into account. We aim to include all the available information for the assessment. The time series of the new Figure 2.16 were improved for the FGD.
17097	169	20	169	28	Figure 2.15.b,c,d present precipitation - evapotranspiration time series in lines. I wish the lines can be differentiated for the colour blind people. May be authors can draw it using several dash line types or symbols. The 2D spatial colour figure is OK as the colour blind people still able to recognize the spreading pattern. [ Santosa Sandy Putra, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The new Figure 2.16 was improved for FGD considering new colorbars.
90335	169	35			insert comma in "al., 2019) CFSR (Saha et al., 2010)", ie "al., 2019), CFSR (Saha et al., 2010)," [ Jeannine-Marie St-Jacques, Canada]	Editorial - copyedit to be completed prior to publication.
42153	170	1	170	1	figure and panel headline (NH) are overlapping [ Julia Nabel, Germany]	Taken into account. The new Figure 2.17 was improved for the FGD.
29907	170	1	170	1	Modify "NH" label position (Figure 2.16, top right). [ Hernan Edgardo Sala, Argentina]	Taken into account. The new Figure 2.17 was improved for the FGD.
113653	170	1	170	1	The figure title covered the "NH" label in the upper right panel and it's unreadable because of that. [ Agnieszka Kowalczyk, Poland]	Taken into account. The new Figure 2.17 was improved for the FGD.
127167	170	1	170	2	In Figure 2.16, horizontal reference lines need to be added so that any trends can be perceived. [ Trigg Talley, United States of America]	Taken into account. The new Figure 2.17 was improved for the FGD.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9933	170	1	170	15	Fig.2.16 "NH" is overlapping with the title of the figure [ Olga Zolina, France]	Taken into account. The new Figure 2.17 was improved for the FGD.
127169	170	9	170	9	In Figure 2.16, what does "weakened overturning peak value" mean? At least provide a citation for the definition. [ Trigg Talley, United States of America]	Taken into account. Details on data sources and processing are available in the associated FAIR data table (Table SM2.Figure 2.17).
90337	170				figure 2.16 - magled title, I think there's an "NH" underneath it. [ Jeannine-Marie St-Jacques, Canada]	Taken into account. The new Figure 2.17 was improved for the FGD.
42155	172	1	172	1	what does panel b show? what is the difference between panel a and b? why February and not March in b? [ Julia Nabel, Germany]	Noted; panel a shows Arctic sea ice area, and panel b shows Antarctic sea ice area. This information is given both in the figure caption and the vertical axes. Months are selected according to when currently maximum and minimum annual SIA occur.
507	172	1	172	12	With the numbers on the x-axis only going to 2013, it would be nice to have the caption provide the end year of the plots. [ Claire Parkinson, United States of America]	Accepted; The horizontal axes are modified, and time series are updated until 2019/20 (according to data availability).
30541	172	1			Figure 2.18: i think it would help to repeat years on the top of panel a. [ Gilles Delaygue, France]	Taken into account; Years are now added to the upper panel horizontal axis.
83255	172	2	172	12	Figure 2.18b - The x-axis is unusually marked - change the increments to 5 years, and run from 1950 to 2000. [ Robert Massom, Australia]	Accepted; The figure is now revised with labels every 10 and tick marks every 5 years.
83257	172	2	172	12	Figure 2.18 - the 2 figure parts don't fully capture the interannual variability because the maxima and minima are on the same y-axis scales. To heighten and emphasize the patterns, please consider condensing separate y-axis scales for each of the Mimimum and Maximum plots on both the Arctic and Antarctic parts e.g., for the Arctic part, have an expanded minumum axis on the right and ranging from 2 to 8 only, and above this have the Maximum y-axis on the left and ranging from 12-16 only. Do a similar thing for the Antarctic plots also (Figure 2.18b). [ Robert Massom, Australia]	Accepted; The figure is now revised, and the vertical axis is now split in each panel, in order to ease visualizing the changes and variations over time.
83203	172	4	172	11	Figure 2.18 is confusing, given the wide range in the SIA magnitude across the difference algorithms/sea ice datasets e.g., in September for Antarctica. Also, why is the trend given for the NASA Team algorithm, and not say the Comiso Bootstrap algorithm? Which one is deemed to be the most accurate, reliable and robust, and does this vary between the Arctic and the Antarctic? [ Robert Massom, Australia]	Taken into account; for more clarity, Fig. 2.18 (now 2.20) is now revised, with split vertical axes, decadal means added, and trend lines removed.
113655	172	7	172	7	It should probably read "Bootstrap from NOAA CDR 3.0" as it is in line 10 on the same page. [ Agnieszka Kowalczyk, Poland]	Noted; The figure caption was revised and the respective part is not included anymore.
90345	172	7			replace the from by a comma and close up the space [ Jeannine-Marie St-Jacques, Canada]	Noted; The figure caption was revised and the respective part is not included anymore.
90347	172	7			delete the space in front of the period [ Jeannine-Marie St-Jacques, Canada]	Noted; The figure caption was revised and the respective part is not included anymore.
57693	172				Figure 2.18: Since this paragraph only deals with Arctic sea ice, if the figure is not moved further along the text (in 2.3.2.2), I would remove the b) panel. I would suggest removing it rather than moving the entire figure, since the data it conveys ( that there is no declining trend in Antarctic sea ice) does not need a figure to be fully understood. As for the linear trend fit (dashed line), I think it doesn't really add a lot to the figure it could falsely convey the idea that the trend starts when the Nasa Team data series begins, while in the text it is correctly stated that there is no significant trend before 1990s. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; Fig. 2.18b (now 2.20b) is kept since it illustrates the changes of Antarctic SIA and several places in the text information is connected to this, where the figure supports understanding. The figure is now revised and updated with data until 2019/20 (according to data availability). The trend lines were removed and replaced by decadal means.
5377	173	1	173	45	The mentioned regression analysis is clearly not linear. Exactly what is it? There seems like no justification for the peak near 1980 for Oct-Nov. [ Bryan Weare, United States of America]	Noted; In the figure caption, it was and is expressed that the submarine-based data are means for autumn and winter. The figure is now revised, and the legend is changed to clearer state which lines and patterns represent what in the figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
57701	173				Figure 2.19: This image is very difficult to read. The information on the considered area is important: maybe it should be moved to a separate map, showing both the area covered by submarine transects and for satellite measurements. Also, instead of the vertical bars used to represent the estimated uncertainty, it would be better to also use the shadings as to armonize the figure between the two different data analyzed, even if they do not represent the same thing. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Accepted; The figure is now revised, and the representation of uncertainty for submarine- and satellite-based data is harmonized. Information on the geographical area for the satellite-based data is added to the figure caption.
4527	174	1	174	1	Fig. 2.20 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 autumn 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, Switzerland]	Noted - SCE for other seasons is mentioned in text and figure in ch 9 (ref made to ch 9) shows SCE for all months. Data for pre-satellite period only available for 2 months which is reason April was used to show longer term trend in spring.
52821	174	3	174	11	Figure 2.20 depicts April snow cover as supporting a loss of NH snow cover. To be open and transparent the authors must also show fall snow cover (which is increasing in some datasets) or winter which has no real trend. This is an example that under critical review may be shown to be a biased way of viewing the observations. Please include the other months=, not simple the one that supports a particular point of view. The dismissal of the NOAA dataset by Robinson is unprofessional and does not address deficiencies NOAA finds with the datasets of Brown and the others. Though on a much smaller scale, Christy (2012 J Hydrometeorology, updated through 2019) finds no significant trends in snowfall in the Sierra of California nor in the Cascades of Oregon and Washington beginning in 1890. [ John Christy, United States of America]	Noted - SCE for other seasons is mentioned in text and figure in ch 9 (ref made to ch 9) shows SCE for all months. Data for pre-satellite period only available for 2 months which is reason April was used to show longer term trend in spring. NOAA has not been dismissed but only indicating that results are not found with other products. Section revised to indicate greater uncertainty in autumn trends. (Note NOAA data was utilized in development of multi-product time-series used in figure - See Mudryk et al. 2020).
54925	174		174		Figure 2.20: Can shading be used to illustrate the spread around the multi-dataset mean? This would be consistent with how the observational-era trends were shown in SROCC and there is value in showing how uncertainty around the mean has evolved from decade to decade. [ Nancy Hamzawi, Canada]	Accepted - Figure revised.
96245	175	0			Fig. 2.21: We find panel (a) a bit problematic, since no reference is given on the number of glaciers reported. I.e., it would be more useful to give the percentage of advancing glaciers of all glaciers that are reported. Alternatively, if the number of glaciers reported does not change over time, please give this number in the caption. [ Nicole Wilke, Germany]	Noted; the figure is revised now, and the figure caption includes the information that figure 2.23a (earlier 2.21a) shows numbers of a finite selection of surveyed mountain glaciers. More information is given in subsection 2.3.2.3.
127171	175	1	175	19	In Figure 2.21, better than the number of glaciers advancing would be the percentage of glaciers advancing. [ Trigg Talley, United States of America]	Noted; Fig. 2.23a (earlier 2.21a) shows the number of a finite selection of surveyed mountain glaciers, as it is also now stated in the figure caption. Therefore it is now chosen to show a percentage.
17821	175	1	175	20	The right panel units are hard to grasp, as it isn't clear whether the area considered is changing in the "per square meter". This could be clarified in the caption, or the right-hand cumulative axis could be in total tonnes lost rather than tonnes per square meter. Keeping the blue bars in tonnes per square meter would illustrate the rate at the same time. [ Baylor Fox-Kemper, United States of America]	Noted; figure 2.21b (now 2.23b) is now revised, showing global glacier mass change, with only one vertical axis remaining, with the unit Gt per yr. This is also mentioned in the revised figure caption.
519	175	1	175	21	It would be very helpful if, in addition to the plot of the "Number of glaciers advancing through the last 2 ka", Figure 2.21 would also include the corresponding plot of the number of glaciers retreating through the last 2 ka. (Note that the summary statement on p. 61, lines 26-27, for the section including Figure 2.21 states that "The number retreating is highly anomalous in the context of the last 2000 years", adding to the appropriateness of having Figure 2.21 include a plot of the number of glaciers retreating.) [ Claire Parkinson, United States of America]	Noted; the (available) and selected data shown in Fig. 2.23a (earlier 2.21a) illustrate a part of the changes over the last 2000 years. Additional information can be found in the respective subsection 2.3.2.3. The wording on retreat and glacier mass loss in this subsection is now revised, according to available information.
5383	175	1	175	27	In frame a) isn't it also necessary to know how many glaciers are receding? In frame b) a much more useful quantity would be total mass loss or equivalent sea level change, rather than per unit area. [ Bryan Weare, United States of America]	Accepted; In Fig. 2.23a (earlier 2.21a) available information for the last 2000 years is shown. Both panels are now revised, and in Fig. 2.23b mass change per time is now shown.
3977	175	1	175	27	Figure 2.21 b: are the global numbers area weighted? Additional to taking only one mean value per area? [ Sabine Baumann, Germany]	Noted; figure 2.21b (now 2.23b) is now revised, showing global glacier mass change. This is specifically mentioned in the revised figure caption.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16035	175	2	175	15	in the caption of Fig. 2.21 a), it is written that "The total number of mountain glacier advances reported in a recent global compilation of mountain glacier fluctuations during the past 2 ka, summarized in 50-year intervals", it is not mentioned if the total number of glaciers in the world is considered. I believe that it is not the case and it could be relevant to mention it and to give the number of glaciers in the analysed sample or to express it in percentage [ Christian Vincent, France]	Noted; Fig. 2.23a (earlier 2.21a) shows the number of a finite selection of surveyed mountain glaciers, as it is also now stated in the figure caption. More information on this is given in the respective subsection 2.3.2.3.
66463	175	3			Comment to Figure 2.21b: in the description of the Figure both [t m-1] and [m w.e.] are present. In the paragraph, where the Figure is described (starting from page 60, line 47) only unit of [m w.e.] is used. Therefore, I would suggest either adding [m w.e.] units to the y-axis labels or replacing "tonnes per square metre" by "metre water equivalent" (i.e. the unit used in the text). [ Barbara Barzycka, Poland]	Noted; Figure 2.21b (now 2.23b) is now revised, showing global glacier mass change, and the unit has changed to Gt per yr. This is also mentioned in the revised figure caption.
66465	175	3			Comment to Figure 2.21b: I would suggest changing of the labels' colours according to the symbology of the graph, i.e. labels (numbers) of left hand axis in blue, right-hand in red - might be easier to reference presented data to the correct axis. [ Barbara Barzycka, Poland]	Noted; Fig. 2.23b (earlier 2.21b) is now revised, showing global glacier mass change, and only one axis is included in the panel.
24397	175		175		Figure 2.21: In panel b, the y-axis on the left says "tonnes" while the y-axis on the right says "tones". [ Owen Cooper, United States of America]	Accepted; The figure is now revised, showing global glacier mass change, and unit is different and appears only once.
24399	175		175		Figure 2.21: In panel b, why does the red line start at a value of 5 rather than a value of zero? Is it referenced to a particular year, such as 1970, where it has a value of zero? Please specify the reference year. [ Owen Cooper, United States of America]	Noted; The figure is now revised, showing global glacier mass change, and the red line (accumulated mass change) as shown in the SOD is not shown anymore.
18117	175		175		the reference year of the accumulated mass change is unclear. There is a positive accumulated mass balance for the first years but the annual mass balances are negative. Maybe the origin of the right y-axis is wrong? [ Thorsten Seehaus, Germany]	Noted; Fig. 2.23b (earlier 2.21b) is now revised, showing global glacier mass change, and the red line (accumulated mass change) as shown in the SOD is not shown anymore.
57703	175				Figure 2.21a: I don't think that the number of advancing glaciers the most important data to represent from the previous section. I think the most important information that needs to be stressed is that the rate of retreat of most mountain glaciers is unprecedented. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Rejected; the (available) and selected data shown illustrate a part of the changes over the last 2000 years. Additional information can be found in the respective subsection 2.3.2.3.
90357	175				figure 2.21b misspelling of tonnes on R-hand axis [ Jeannine-Marie St-Jacques, Canada]	Accepted; The figure is now revised, and units are different and appear only once.
127173	176	1	176	7	In Figure 2.22, show Greenland and Antarctica on the same scale so that the relative importance of ice loss in each location can be perceived. [ Trigg Talley, United States of America]	See response to 116033
127175	176	1	176	7	In Figure 2.22, consider adding uncertainty bands to the ice margins in the figure. [ Trigg Talley, United States of America]	See response to 116033
17823	176	1	176	20	We need to ensure that these figures remain consistent with those in Chp 9, Fig. 9.19 [ Baylor Fox-Kemper, United States of America]	See response to 116033
90363	176	11			reorder citations chronologically in increasing order [ Jeannine-Marie St-Jacques, Canada]	See response to 116033
116033	176		176		missing representation of uncertainty + confidence in the figure. [ Valerie Masson-Delmotte, France]	Not Applicable - Figure removed from chapter 2 and appears in chapter 9 in FGD (reference made to ch 9 in text)
42907	176				I do not understand the basis for the Lig reconstruction of Antarctica at all. I assume it's from Goelzer et al 2016, but this is a modelling study based on an EMIC, and has no basis in evidence at all. This is highly misleading, and should not be used. If it is, it's essential to label it as a modelling study with no evidential support. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	See response to 116033
5389	177	1	177	7	This figure is inadequate to illustrate such an important topic. There should also be a frame showing the year to year change. In addition the y axis should also have an equivalent sea level estimate. [ Bryan Weare, United States of America]	Noted - Figure shows ice sheet mass over time and conveys loss in fairly standard way. (equivalent sea level estimates have been provided in text rather than figure).
83259	177	5	177	5	Change "Greenlandic" to "Greenland" [ Robert Massom, Australia]	Accepted - revision made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116035	177		177		The figure could convey the outcome of analyses of rates of changes. To be done with the same approach for eg. ERF, this, and GMSL. [ Valerie Masson-Delmotte, France]	Noted - unclear on comment - Goal of figure is to show the ice mass over time and the figure conveys loss of mass. Rates of change are included in text.
5423	178	1	178	14	This summary figure needs substantial revision to be clearer. The date column needs to agree better with the accompanying text on p. 80. The last two rows should be eliminated since they are not discussed in the text and refer to very different earth. This would allow a rescaling of some of the colors so that the top half have fewer white boxes. An additional column showing rate of change of surface temperature should be added. The three rate columns should be put on the far right. As it is the CO2 column looks contrary to the others, which is clearly because it is for rates. Why are there no values for the glacial extent column, given there are color codes for magnitudes? [ Bryan Weare, United States of America]	Taken into account; figure extensively revised; added statement about "relative extent" of glaciers. EECO and PETM retained because they are discussed in the text.
116037	178		178		The figure could convey the outcome of analyses of rates of changes. [ Valerie Masson-Delmotte, France]	Noted - unclear on comment - Intent of figure is to show permafrost temperature over time and is a similar approach used in other assessments. Long-term rates of change are discussed in text.
7151	179	1	179	1	Figure 2.25, right pannel : to make the figure more readable, it is suggested to plot thinner curves. [ Nicolas Kolodziejczyk, France]	Editorial. Figure revised.
113657	179	1	179	1	ISAS-15 dataset is not described in the Figure caption. [ Agnieszka Kowalczyk, Poland]	Editorial. Text revised.
12615	179	1	179	15	uncertainty range should be given. [ Lijing Cheng, China]	Accepted. Figure revised.
5393	179	1	179	16	Why don't the vertical scales for the left and right plots match up? For instance for the full depth it looks like the current values should be about 500 not 100. I assume the reference is different, but this should be fixed. [ Bryan Weare, United States of America]	Accepted. Vertical offsets were used for plotting purposes. Figure revised for clarity.
12601	179	1	179	16	Zanna et al. gives much much lower warming rate from 2005 to 2018 than other datasets (also from 1990s to present), how much can we trust zanna et al? I saw Zanna et al. time series has been used extensively in AR6 chapters, so this underestimation is an alarm to understand the advantages/limitations of Zanna's reconstruction. At least from this figure, Zanna estimate is not useful in OHC estimate for any time windows less than 30 years (physically becuae, for shorter time scales, heaving and heat redistribution are more important than heat transport by mean flow, see recent Zika et al. 2020 paper). [ Lijing Cheng, China]	Noted. All estimates were considered in figure but not selected for the assessment. Hybrid estimates were not used when direct observations were available.
83261	179	5	179	16	The caption for Figure 2.25 is unclear and confusing as written. [ Robert Massom, Australia]	Editorial. Figure caption revised. Greater use is made of the FAIR data tables throughout chapter.
113659	179	8	179	8	I can't see "Meyssignac et al., 2019" dataset in the Figure. Is it combined with another dataset? [ Agnieszka Kowalczyk, Poland]	Editorial. Figure revised.
116039	179		179		Could not this figure be combined with the one on past CO2 changes for the same time periods? [ Valerie Masson-Delmotte, France]	Noted. This is more an activity for upstream synthesis aspects.
83079	179		179		Figure 2.25. I wonder if the paleo evidence could be represented somehow in this figure? I find the top-left panel (i.e. full-depth OHC change for the period 1870 to present) a little confusing, because it doesn't seem to reflect the uncertainty among products shown for the 0-700m layer. Perhaps all the 0-700 m layer products could be translated into full-depth estimates, as has been done for the two Domingues+ estimates? [ Matthew Palmer, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is done in a figure in chapter 9.
83959	179				Following other Figures presented in the Report, panels should be identified by letters, otherwise it is very hard to understand it. [ Marco Tulio Cabral, Brazil]	Editorial. Figure revised.
17825	180	1	180	20	I thought Chp 2 was not planning to show sections. Chp. 9 does not show salinity change any more, so please do! [ Baylor Fox-Kemper, United States of America]	Accepted. Changes agreed with chapter 9.
5395	180	1	180	38	One cannot identify the climatological maxes and mins, so that it is hard to verify comments on pg 68, I2-3. [ Bryan Weare, United States of America]	Rejected. Not clear to what the comment refers.
30543	180	1			Figure 2.26: what means 'EN4' on b and d? Obvioulsy this does not refer to Durack (2015). [ Gilles Delaygue, France]	Editorial. Figure revised.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
521	180	2	180	38	Figure 2.26 is a great figure, and it will be nice to get the indicated updates. If feasible, the figure could be even more informative if instead of giving DW10 and EN4 versions, the best quality of the two versions were chosen, eliminating the other one and replacing the two dropped parts of the figure by Atlantic and Pacific versions of the c,d cross sections. I.e., it would still be a four-part figure but using only one source and including the near-surface salinity map (part a) and three cross sections. [ Claire Parkinson, United States of America]	Noted. Figure revised.
54451	180	30	180	30	[PLACEHOLDER – FIGURE TIME PERIODS WILL BE UPDATED IN THE FGD]. Replace? [ Maria del Pilar Bueno Rubial, Argentina]	Noted. Figure updated.
113661	180	32	180	33	I would delete "Black contours show the associated climatological mean SSS for the analysis period." as it is written again at the end of the Figure caption (page 180 lines 35-37). [ Agnieszka Kowalczyk, Poland]	Editorial. Figure caption revised.
90389	180	32			missing an "(" [ Jeannine-Marie St-Jacques, Canada]	Editorial. Text revised.
112903	181	1	42		Figure 2.27 shows sea level records. The longest show record is from Kemp et al (2018). Yet this paper shows a sea level comparable to the present about 700-800 year BC: their figure 9-11. Please, report the entire record not a truncated one in 250 BC. [ Nicola Scafetta, Italy]	Rejected. In the final version of the FGD we show 2500 years from Kemp et al. At the same time we provide 800 ka reconstruction of sea-level from ice core oxygen isotopes. We found this arrangement to be most appropriate for the context of the assessment.
127177	181	1	181	26	In Figure 2.27, the label for year -500 is offset from its tick mark. [ Trigg Talley, United States of America]	Accepted. Figure revised.
127179	181	1	181	26	In Figure 2.27, the figure would be much clearer and impactful if, rather than having a changing time scale, the left graph was broken into two separate graphs: -750 to 2020 and 1800 to 2020. The same amount of space is required either way. [ Trigg Talley, United States of America]	Accepted. Figure revised.
30545	181	1			Figure 2.27: i find that using so different time scales is detrimental to the scope of the graph, especially for the rising rate. Would it be possible to divide the figure in two over 2 different periods: even if the rate changes by a factor two this would be better than by a factor 10. The inset over 1993-2018 is, conversely, not very usefull. [ Gilles Delaygue, France]	Accepted. Figure revised.
30547	181	1			Figure 2.27: I think the time scale is the same before and after 1970 (20 yrs betw. 2 ticks), contrary to the meaning of the vertical line. [ Gilles Delaygue, France]	Accepted. Figure revised.
100641	181	30	181	30	Note: I like this figure a lot, but I don't see immediately just how rapid recent sealevel rise has been. Can a panel with constant X-scaling be added? Otherwise I worry that a casual reader might think that previous rise has been faster, just because the scales are different. [ Matthew Kohn, United States of America]	Accepted. Figure revised.
52825	181	30	181	39	Figure 2.27 records only sea level from a minimal point after the mid-holocene high-stand. The appropriate image here is to include the data that indicate sea level was a bit higher in the mid-holocene as part of natural variations. To miss this point is to offer accusations of bias in the material presented that the IPCC does not need. [ John christy, United States of America]	Taken into account. The final version of the FGD shows a 800 ka reconstruction of sea-level (fig 28a). As noted in the revised main text there is considerable ambiguity whether MH GMSL was higher or lower than today with most evidence suggesting lower. Given the complexities this is best addressed via the assessment text.
89315	181	30			Fig. 2.27: As a person giving many public talks, I am disappointed to find a sea-level figure which I couldn't show in a public lecture because of the uneven x-axis. This is OK for scientists but not good for public communication, unless you have a lot of time explaining it. It would be better to have one figure for the past 2500 years with linear axis, and another one zooming in to the instrumental period. [ Stefan Rahmstorf, Germany]	Accepted. Figure revised.
113663	181	31	181	31	"tide gauge reconstruction (Kopp et al., 2016; Kemp et al., 2018)" -- I can see Kemp (2018) dataset in the Figure, but can't see Kopp (2016) dataset. Is it combined with Kemp (2018) data (grey line)? [ Agnieszka Kowalczyk, Poland]	Accepted. Kemp et al 2018 supersedes Kopp et al. 2016, as it uses updates of the same basic methodology and data set.
523	181	34	181	35	This sentence states that the vertical lines in Figure 2.27 "indicate changes in the resolution of the time axis", a statement that is quite correct for the leftmost two vertical lines, at 1750 and 1850, but doesn't appear valid for the rightmost vertical line, at 1970. I suggest leaving the sentence as is but removing the vertical line at 1970, which is confusing in face of the sentence about the meaning of the vertical lines. [ Claire Parkinson, United States of America]	Accepted. Figure caption revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127181	181	34	181	35	[PRECISION] In Figure 2.27, contrary to the text, the last vertical line does not correspond to a change in the resolution of the time axis. [ Trigg Talley, United States of America]	Accepted. Figure caption revised.
83961	181				Based on the Figure caption and Legend, it is not possible to identify what is the source used for the proxy data. [ Marco Tulio Cabral, Brazil]	Accepted. Clarified.
5411	182	1	182	16	The top frame is of little value given the unexplained shift over the gap. The discussion in the text on pg. 73 refers to the 5-20kyr time period which is not evident at all. Is the "global" curve in the lower frame really global or low-latitude as described by the caption. [ Bryan Weare, United States of America]	Taken into account, and the figure caption has been revised, as well as the figure.
69827	182	1	182	17	For better visualization, better to show ice-core CO2 based estimation in Fig. 2.28(b)? [ Kaoru Kubota, Japan]	Taken into account, and the figure went through several revisions, which also includes a split of the time periods for better visualisation.
57759	182	1			I would suggest to change the scale bar of 0.25 on the right to 0.2 which can directly match the break on the left of the figure. Otherwise, it could create confusion for some readers. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Although scale change is challenging, the figure now includes several information to avoid confusion for the reader.
127183	182	14	182	14	In Figure 2.28, should "at" be "from"? [ Trigg Talley, United States of America]	Not relevant anymore as text had been revised due to other review comments.
7413	183	0	183	0	The colormap seems to be conter-intuitive : blue for decrease in GGP, red for increase. I would prefer green for GPP increase, brown for decrease. Like in figure 2.31 [ Jeremy PANTHOU, France]	Accepted. Figure 2.29 has been revised and updated for clarity.
127185	183	1	183	2	In Figure 2.29, don't know what's plotted in panel b, but it's not chlorophyll trends, which should have both positive and negative values and would not be plotted on a log scale. [ Trigg Talley, United States of America]	Taken into account. Fig. 2.29 has been revised and updated for clarity.
5415	183	1	183	13	The color scales of a) and b) are very difficult to interpret given the log scales. Would not b) be more useful as a percent as in frame c)? [ Bryan Weare, United States of America]	Accepted. Figure 2.29 has been revised and updated for clarity. Changes are now shown as % changes.
30549	183	1			Figure 2.29: fig 29b only shows positive values, which means that only positive trends have been measured? This seems to contradict fig 29c with (some) negative trends. [ Gilles Delaygue, France]	Accepted. Figure 2.29 has been revised and updated for clarity.
57761	183	1			The second figure indicates it is the trend of global ocean chlorophyll-a. However, the unit of the figure scale is change of absolute value. The difference need to be reconciled since trend should be defined as change per time unit. A suggestion would be change the "trend" to "change". [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account. Figure 2.29 has been revised and updated for clarity.
83263	183	2	183	13	Figure 2.29 doesn't capture or do justice to the polar regions due to the map projection used e.g., high productivity associated with sea ice seasonal melt etc. Could this also include 2 additional maps looking down on both poles. This will better highlight what appear to major and important trends occurring around Antarctica and also in the Arctic. Such polar trends are of wide-ranging significance. [ Robert Massom, Australia]	Rejected. Issue of limited space. Also retaining global focus (remit of the chapter) and avoiding regional focus (out of scope).
105535	184	1	184	14	Figure 2.3. While this variable can be easily quantified, It understates both the pace and magnitude of the vegetation shifts precipitated by the Younger Dryas, Early Holocene warming, and latest Holocene perturbations. This statistic is essentially the shortest-term noise in the records, and is seriously blurred by errors in age measurements between sediment cores. It is blind to slower extreme directional changes. It is as if one attempted to measure gains and losses of the Standard and Poors 500 Index by graphing it as the value of stock market volatility. Stock market volatility is an easily accessed statistic, but if you were to attempt to use it to quantify change over the past 20 years, you would see very high values during early 2009 and March, 2020. Periods of slow but extreme directional and significant change such as between 2009 and 2019, would not show up at all. I don't today have a solution, but quantifying these extreme but slow directional changes in ecosystems is a continuing problem since they resist averaging between different regions with different histories. [ Kenneth Cole, United States of America]	Taken into account - combined with comment 5417.
30551	184	1			Figure 2.30: information stops at 1950, i.e., more or less just before the acceleration of man impact on its environment, which is the motivation of the UNFCCC and so of IPCC AR. I find this graph very deceptive without any clue on the 'recent' (see text) rates. [ Gilles Delaygue, France]	Taken into account - combined with comment 5417.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64693	184	5	184	13	Fig 2.30 Would it be possible to extend this figure to the to the present (I.E 2012 or later)? To provide a view of the possible change of rate in the recent year. I see a potential caveat in this figure. Over the Holocene and in the recent period vegetation changes are a mixture of natural vegetation changes and land use (agriculture, deforestation). Could the figure include indications of the part of the changes that could be due to land use? [ Pascale Braconnot, France]	Taken into account - combined with comment 5417.
116041	184		184		What is the key message from this figure, is it needed? Same remark for the corresponding text, in fact. I do not understand what is the take home message. [ Valerie Masson-Delmotte, France]	Taken into account - combined with comment 5417.
57763	185	1			The caption in the figure states that "Grey depicts unvegetated land surface areas such as ice caps and barren deserts". However, the unvegetated regions are indentified as white in the figure which creates confusion with the no change category (0 change). This need to be corrected. Also, the region with statistically significant changes should be marked. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - combined with comment 57633.
102785	185	3	185	14	Figure 2.33. Interesting to see how much missing data there is for sea level rate of change as opposed to the other indicators. An explanation of the data gaps would be useful. The blocks could have percentages in them where applicable, +/- % from preindustrial baseline; color is interesting at a quick glance but numbers are better. [ Philippe Tulkens, Belgium]	Rejected - this figure does not depict sea level rate (comment appears to be for another figure).
66469	185	3			According to page 68, line 43, the potential sea level equivalent for glaciers is 324+/-84 mm, not 0.5 m as stated in Figure 9.1a (probably the SLE numbers at the Figure will be corrected at the final stage of the Report?... ) I would like to suggest generalising "mountain glaciers" to "glaciers" on the Figure 9.1a. [ Barbara Barzycka, Poland]	Rejected - this figure does not depict sea level rate (comment appears to be for another figure).
57655	185	31	185	32	Probably trivial, but the map doesn't have any grey regions, though the text mentions it. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - combined with comment 57633.
3659	185		185		The color bar of Figure 2.31 is confusing. Each color scheme should be between two different numbers. [ Jiafu Mao, United States of America]	Taken into account - combined with comment 57633.
116043	185		185		Please consider a visual depiction of global greening and browning trends. [ Valerie Masson-Delmotte, France]	Taken into account - combined with comment 57633.
17801	186	1	186	1	include units of x-axis (year CE). [ Raphael Neukom, Switzerland]	Rejected - year on x-axis is self-evident.
127187	186	1	186	2	In Figure 2.32, center the moving averages, which are smoothed versions of the original data and should not be offset. The present figure underplays recent changes by time-lagging them. [ Trigg Talley, United States of America]	Taken into account - figure revised.
72189	186	1	186	2	it seems that moving averages are marked in the last year of the averaging period and should be in the middle [ Joanna Wibig, Poland]	Taken into account - combined with comment 127187.
30553	186	1			Figure 2.32: This figure is more about an 'iconic' graph than a scientific proof backing the conclusions. I have 3 comments. 1. The corresponding section give conclusions on general trends, whereas the figure shows two very local series (iconic as among the longest ones): i am not sure there serve the conclusions. 2. There is a very strong interannual variability, which makes the last century trend hardly significant. The legend should clarify the significance of these trends. 3. Adjust the time scales to show the same period for both series. [ Gilles Delaygue, France]	Taken into account - combined with comment 30475.
116045	186		186		Is the key message related to the unusual recent trend? If yes this could be communicated visually too. [ Valerie Masson-Delmotte, France]	Taken into account - combined with comment 30475.
17803	187	1	187	1	using the minus sign ("-") to indicate a range is very confusing in this context. Suggest to replace by "to". E.g. change "-2-2" by "-2 to 2". I even suggest to include a "+" sign to positive values, to be clear, e.g. ("-2 to +2"), particularly as + signs are used in the cases where the uncertainty range is provided. Be consistent! [ Raphael Neukom, Switzerland]	Accepted; changed dashes to "to".
8933	187	1	187	14	Coloring scheme for CO2 rate of change is unclear -- PETM is not the lowest value. [ Robert Kopp, United States of America]	Taken into account; figure extensively revised.
30555	187	1			Figure 2.33: CO2 rate of change for the current period (top case) seems too high (194 ppm/100yrs) [ Gilles Delaygue, France]	Rejected; rate stated correctly.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15225	187	1			Figure 2.33 is terrific. The colour bar should be referenced more specifically in the legend, just so readers are 100% clear on how the colours for each box were defined. [ Simon Donner, Canada]	Accepted; revised label on colour bar.
112407	187	3	187	3	The top of the figure with CO <sub>2</sub> , temperature etc symbols is partially cut off. The rice symbol in the last deglacial transition looks like a volcano at the first glance. [ Feng Ran, United States of America]	Accepted; as suggested.
100643	187	3	187	3	Add: Miocene climatic optimum (16.9-14.7 Ma) 400 – 600 -2 – 2 8.7±2.3 7±3 50±5 (error is approximate) [ Matthew Kohn, United States of America]	Rejected; Miocene is not included in Figure 2.33 because most large-scale indicators have not been assessed. However, Miocene is included as a paleoclimate reference period in CCB2.1.
100645	187	3	187	3	Note: Estimates for temperature for the MCO are relative to modern, not PI. Here, I've added 1.1°, reflective of GMST in 2014 (study of Goldner et al., 2014). If necessary, this can be refined later (but it should be correct to ±0.1 °C, which is well within the uncertainty listed. [ Matthew Kohn, United States of America]	Rejected; Miocene is not included in Figure 2.33 because most large-scale indicators have not been assessed. However, Miocene is included as a paleoclimate reference period in CCB2.1.
83963	187	8	187	9	please insert the underlined information: "Refer to Cross-Chapter Box 2.1 for description of the climate state of the Reference Periods.", as Cross-Chapter Box 1.2. also deals with reference periods and is mentioned in the text of Cross-Chapter Box 2.1. [ Marco Tulio Cabral, Brazil]	Accepted; added call out to Cross-Chapter Box 1.2 for recent reference periods.
116047	187		187		nice figure. I suggest to be very cautious about rates of changes per century for past records with low resolution (it should be indicated "insufficient data"). I suggest to change the labels of the colors to : "warmer - higher sea level - less ice / colder -lower sea level - more ice" to reflect what is shown (rather than "inverse axis"). Please provide an estimate of the level of confidence for reported changes, with related pictograms (maybe like the dots in the ember diagrams of SROCC). Check consistency with ch 1 esp. FAQ1.3 [ Valerie Masson-Delmotte, France]	Taken into account; figure extensively revised. Rates of change restricted to three intervals; revised label on colour bar; added "very likely range" designation.
24401	187		187		I don't understand the color-scale for the glacial extent column. I understand why the color-scale was inverted, but white is supposed to correspond to pre-industrial levels. Instead, the pre-industrial box is colored a pale red, while white is found in the Last Deglacial Transition. [ Owen Cooper, United States of America]	Accepted, as suggested.
7491	187				In the same way, "Modern Era" should not be in capital letters in Figure 2.33 (Chapter 2, page 187) as it is not a geological term. [ Alejandro Cearreta, Spain]	Accepted, as suggested.
15179	187				Figure 2.33 is terrific. The colour bar should be referenced more specifically in the legend, just so readers are 100% clear on how the colours for each box were defined. [ Simon Donner, Canada]	Accepted; revised label on colour bar.
42909	187				I remain unconvinced by the error bar on the LIG temperature difference. If you've changed it elsewhere then it needs changing here too. [ Eric Wolff, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account; changed to "very likely" range rather than ±2SD. This range (1C) is equivalent to AR5 range because current understanding has not changed substantially since AR5. 1°C range is also larger than reported in SROCC.
127189	188	1	188	32	In Cross-Chapter Box 2.4, Figure 1, panel c, include 0 (gray) on the color bar. [ Trigg Talley, United States of America]	Accepted; added grey = 0 to bar.
127191	188	1	188	32	In Cross-Chapter Box 2.4, Figure 1, panel c, plot Greenland and Antarctica on the same spatial scale so that the relative importance of changes in ice coverage can be more easily perceived. [ Trigg Talley, United States of America]	Rejected; Greenland would be difficult to see at small scale.
533	188	17	188	44	Box 2.4 Figure 1c maps the "Number of models in agreement that ice was present". It is important to let the reader know (e.g., in the caption) how many models were included altogether. Likely the answer is 10 for the Antarctic and 8 for Greenland, but without a statement to that effect it is not clear. [ Claire Parkinson, United States of America]	Accepted; added (n = # models) to figure.
2035	188	35	188	35	For the "number of models agreeing ice was present", it needs to say how many models there were in total (I assume 8, but it doesn't say!). [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; added (n = # models) to figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2037	188	35	188	35	This figure really focusses on model output, but the box has somewhat more of a focus on the proxies....can proxies be brought in more into the Figure? [ Daniel Lunt, United Kingdom (of Great Britain and Northern Ireland)]	Accepted; added proxy data to maps in (a)
29911	188	36	188	36	Consider adding "anomaly" in the following way: "(a) Simulated surface air temperature and precipitation rate anomaly from..." [ Hernan Edgardo Sala, Argentina]	Accepted; added "anomaly".
1847	188	36			Cross-Chapter Box 2.4, Figure 1c. Use the same color scale for both Greenland and Antarctica, so they can be compared. [ Alan Robock, United States of America]	Taken into account; colour scale is the same when considered as a proportion of models (red = 100%), which facilitates comparison.
90427	188	39			outlines not outlies [ Jeannine-Marie St-Jacques, Canada]	Accepted, as suggested.
116049	188		188		missing information from proxy records, description of model used (CMIP5? CMIP6?). I do not find the lower left panel useful (what is the message?). Any observational constraint on ice sheets beyond counting model agreement? [ Valerie Masson-Delmotte, France]	Accepted; (1) added proxy data to maps; (2) added the purpose of the ice-sheet model figure (to show where models more consistently show the presence/absence of ice on Greenland and Antarctica); (3) added that the ice sheet simulations are driven by AR5 models; climate models are CMIP6 plus non-CMIP6 models; (4) geological evidence for ice sheet extent in CH2 text; (5) added modern biome map to illustrate the difference between Pliocene and modern distributions.
76813	189	1	189	38	Data here needs to be checked and updated. Tierney et al 2015 didn't reconstruct a Nino3.4 SST index (they did an east Pacific reconstruction but the reconstruction statistics weren't good for this region). Missing references/newer reconstructions: Datwyler et al., 2019, International Journal of Climatology (including an assessment of when different reconstructions of Nino34 agree and dont agree, 10.1002/joc.5983), Grothe et al., 2019, GRL, Dee et al., 2020, Science. [ Nerilie Abram, Australia]	Accepted. Data sets in figure updated.
52827	189	31	189	37	Fig. 2.34 neglects the work of researchers who show significant variability over longer time peirods (e.g. Tudhope et al. 2001, Moy et al 2002 and others). The readers need to see the longer timer series of this index. Also, with the current figure, I reader cannot tell the sense of invidicual reconstructions, some of which are obviously more trustworthy than others. The scatter is not convincing as to a trend. [ John christy, United States of America]	Rejected. The purpose of the figure is to support assessment findings back to 1400 (P88 L19), making earlier data (to the extent that it is available) not material to that assessment.
64695	189	31	189	37	Figure 2.34. This figure shoul also include the reconstruction for the Holocene. An important new features in the last years is the compilation of all the available records over the Pacific that allow to identify changes and diversity in the characteristics of ENSO variability with time. A reference for this (and associated database) is Emile-Geay, J., Cobb, K. M., Carre, M., Braconnot, P., Leloup, J., Zhou, Y., Harrison, S. P., Corregge, T., McGregor, H. V., Collins, M., Driscoll, R., Elliot, M., Schneider, B., and Tudhope, A.: Links between tropical Pacific seasonal, interannual and orbital variability during the Holocene, Nature Geoscience, 9, 168+, 2016. This reference also discusses the relationship one can extract from coral and shell records on the relationship between changes in seasonality and changes in interannual variability. It is thus an important step from data on the establishment of a relationship that was only discussed from theoretical or modeling point of views. [ Pascale Braconnot, France]	Rejected. Whilst the Holocene data are important and are described in the text, there is a limit to the number of figures which can be presented. The Holocene data are also not in a form which is compatible with the way the data in the figure are displayed.
112935	189		189		I like this figure, and it really resonates with my comment above. There is a signal here, esp given the difficulty of compiling these datasets over recent centuries - their convergence in part reflects shared underlying datasets, and so you might consider adding in some key single proxy recrods. But for the most part, I think the figure is effective. I would recommned providing shading or envelopes for the 95% distribution fo the data bc clearly there is a pile of overlapping data that constrains the mean, and the overall spread of the data and the current visualiation makes the outliers more visually influential. [ Kim Cobb, United States of America]	Accepted. Figure restructured along the lines suggested.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
76811	190	1	190	16	It isn't appropriate to use these instrumental SST products for the tropical Indian Ocean and IOD prior to 1958 due to the lack of input data from this region. This has been discussed in many papers over many years. [ Nerilie Abram, Australia]	Noted. The figure shows various SST indices using multiple data sets that available to the climate science community at the time of assessment. The figure is therefore necessary to show the extent to which the different data sets agree or disagree during different periods.
127193	190	1	190	16	In Figure 2.35, definitions (citations) for the various climate indices are needed. In particular, there's no consensus on how to define AMOV, and it's not possible to interpret the plot without knowing the definition. [ Trigg Talley, United States of America]	Accepted. Definitions are included in the glossary, and in the technical annex on modes of variability.
72191	190	1	190	16	AZM is the same as Atl3? [ Joanna Wibig, Poland]	Noted. AZM is now used consistently.
113665	190	4	190	4	The label "AZM" of the vertical axis of the panel at the very bottom is not explained in the Figure caption. Either "Atlantic Zonal Mode" should be included in the caption OR the label should read "Atl3" instead. [ Agnieszka Kowalczyk, Poland]	Noted. AZM is now used consistently.
71637	190		190		The colour scheme for Figure 2.35 is particularly hard to look at and distinguish between the various datasets. [ Jessica Hargreaves, Australia]	Noted. The difficulty in distinguishing different data sets implies agreement among the data sets.
17805	191	1	191	1	Include a pictogram for the cause! Also the pictograms are not consistent with the paragraphs on page 94. On page 94, the key headings are "long time", "rapid" and "everywhere". Make sure a paragraph with the header "reversed long term cooling" is included on page 94 or remove the pictogram in the figure. [ Raphael Neukom, Switzerland]	Taken into account; text headings and figure icons are now aligned.
5445	191	1	191	4	There is little value to this figure and its heading are not those of the text on pg. 94 [ Bryan Weare, United States of America]	Taken into account; text headings and figure icons are now aligned
83965	191	4	191	4	As this Figure does not actually represent the observational evidence, but it pin points the observational evidence, change the caption to "Infographic of the main points of observational evidence for the unusualness of recent warming." [ Marco Tulio Cabral, Brazil]	Taken into account; omitted "observational".
545	192	1	192	1	In FAQ 2.2, Figure 1, it seems that an up arrow for sea surface temperature should be added to the water block. [ Claire Parkinson, United States of America]	Taken into account - figure revised.
2931	192	1	192	8	Air: atmospheric circulaiton did not mention clearly. [ Zong Ci Zhao, China]	Rejected - comment is ambiguous and does not contain actionable information.
127195	192	1	192	8	In FAQ 2.2, Figure 1, consider adding black outlines to those arrows in which the magnitude of the changes are assessed to be likely unusual in at least the past 2000 years, say. [ Trigg Talley, United States of America]	Rejected - figure focuses on changes documented by the instrumental record, primarily over the past several decades.
8935	192	1	192	8	surface specific humidity is up but relative humidity decreasing over land. Weird to pair ice sheets with sea ice rather than glaciers -- also trend is not identified for Antarctic sea ice [ Robert Kopp, United States of America]	Taken into account - figure revised.
57773	192	1			As this report tries to identify the different between SAT and MAT, SST would be better to listed in the "Water" category. [ APECS, MRI, PAGES ECN, PYRN and YESS ECS group review, Canada]	Taken into account - combined with comment 545.
98359	192				In the Figure 1 of FAQ 2.2, for land, the growing season length is shown to be increasing. A statement of 'changing of growing season length' would be more accurate. Growing seasons are not uniform across all crops or regions. [ Feba Francis, India]	Rejected - a central conclusion of the biosphere section of the chapter is that growing season length has increased.
73719		30		30	No capital required for 'West'. [ Burt Peter, United Kingdom (of Great Britain and Northern Ireland)]	Editorial; copyedit to be completed prior to publication.
115977					For sections assessing temperature change from paleo records and paleo simulations, please check systematically what is reported in the literature and in the assessment (GMST, GSAT, SST). [ Valerie Masson-Delmotte, France]	Taken into account; GMST, GSAT or SST checked
115999					Question on elevation dependent warming as assessed in SROCC ch 2 : it is not mentioned here but is relevant for high mountains, could it be added? [ Valerie Masson-Delmotte, France]	Rejected. We agree this is important. However, this comment pertains to a regional aspect and thus should be covered by the regional chapters instead of this global chapter charged with an assessment at the very largest scales.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41781					General comment: I recognize the improvement w.r.t FOD chapter, presenting a good balance between completeness and length. Achieving a coherent chapter is difficult, given the large number of datasets and of topics covered. As before, I think that the executive summary and introductory sections are particular relevant in summarizing the key points of the chapter and framing within the whole AR6 report. In this respect I have to say that there has been a significant improvement between FOD and SOD. [ Isabel Trigo, Portugal]	Noted with thanks
30013					This chapter is very well-written, with well-condensed scientific evidences and achievements as well as the judgments of scientific uncertainty. The large amount of new scientific results made in the past 5 years are well-organized into this chapter of the global climate system. Especially, for each components, a short, but well-balanced conclusion is provided. I like this writing style. Figures in this chapter are well designed, especial Fig.2.1; Box 2.1, Fig.1; Fig.2.3; Fig.2.4; Fig.2.10 ; Figures.11 ; Figures.27 ; Figures.33; Box 2.4, Fig.1; By my judgment, this chapter only needs a minor revision. [ Yihui Ding, China]	Noted with thanks
90431					Look - this chapter needs a copyeditor to go over the mangled citation formats and people not leaving a space between numbers and their units. This is a mess. I give up on these problems as they are so widespread. I can't correct anything else at this rate. [ Jeannine-Marie St-Jacques, Canada]	Accepted. Citation formats and editorial elements are addressed in conjunction with TSU at final copyedit phase.
90433					This chapter is extremely well-written from a scientific viewpoint. It was a pleasure to read. Its arguments are well-constructed and persuasive. The authors are clearly very knowledgeable about their subjects. I really didn't find any faults in the science. My only complaints are that it really needs to be gone over by a scientific copy editor before release. The numerous typographic errors do detract from the presentation badly. [ Jeannine-Marie St-Jacques, Canada]	Accepted and thanks. Citation formats and editorial elements are addressed in conjunction with TSU at final copyedit phase.
93509					Check inconsistency in the use of the term "timeseries versus "time series" [ Rahab KINYANJUI, Kenya]	Editorial. Addressed in copy edits.
35927					Alternative/Complementary records for historical volcanic events are provided by Schneider, L., Smerdon, J. E., Pretis, F., Hartl-Meier, C., & Esper, J. (2017). A new archive of large volcanic events over the past millennium derived from reconstructed summer temperatures. Environmental Research Letters, 12(9), 094005. [ Felix Pretis, Canada]	Rejected; the suggested paper addresses volcanic impacts on NH summer temperature, which is the remit of CH3 detection and attribution.
35929					Empirical literature on paleo-climate are missing from the report, such as: Kaufmann, R. K., & Juselius, K. (2013). Testing hypotheses about glacial cycles against the observational record. Paleoclimatology, 28(1), 175-184. Kaufmann, R. K., & Juselius, K. (2016). Testing competing forms of the Milankovitch hypothesis: A multivariate approach. Paleoclimatology, 31(2), 286-297. [ Felix Pretis, Canada]	Rejected; the suggested papers address attribution which is out of scope for CH2
29311					very good work [ Zangari del Balzo Gianluigi, Italy]	Noted with thanks.
72089					Cross-Chapter Box 2.1, Figure 1: The projections are not labeled or named in the caption. Therefore, I'm not sure which color belongs to which scenario projection. This can be easily solved by changing the caption from:  Temperature projections for 2081-2100 are multi-model means (with $\pm$ 2SD) based on SSP1-2.6, SSP2-4.5, and SSP5-8.5 scenarios ....  to something like this: (not sure which color belongs to which projection)  Temperature projections for 2081-2100 are multi-model means (with $\pm$ 2SD) based on SSP1-2.6 (dark blue), SSP2-4.5 (light blue) and SSP5-8.5 (red) scenarios .... [ Elke Zeller, Republic of Korea]	Accepted; as suggested.
72091					Figure 2.4 (c) both the CO2 and N2O figures are very busy, and it's hard to separate the different data, especially for the N2O. Removing the error bars might help or choose a different kind of graph.  It might be an idea to plot the temp difference from Cross-Chapter Box 2.1, Figure 1 in the plot (a) to have a frame of reference for some more novel people. [ Elke Zeller, Republic of Korea]	Accepted. Both CO2 and N2O graphs modified to improve legibility.

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90267					Cross-Chapter Box 2.1, Figure 1 in the caption the squares should be Greek deltas [ Jeannine-Marie St-Jacques, Canada]	Editorial; copyedit to be completed prior to publication.
72093					Figure 2.11: the 2.5th and 97.5th percentiles lines are confusing and might be better represented by using a shaded are similar to what is done in figure 2.27 [ Elke Zeller, Republic of Korea]	Taken into account; figure extensively revised.
115877					FAQ2.1 when refering to the Holocene, please also include a description of the pattern (different / orbital forcing / global warming). What about stating that no natural driver or aspect of natural variability can explain the characteristics of recent climate change (not just warming, heat accumulation, etc)? The last paragraph seems disconnected from the question and WGI perspective. [ Valerie Masson-Delmotte, France]	Rejected; final two paragraphs now extensively cut to retain focus on unique content in this FAQ (evidence for unusual recent warming) and to call out other FAQs for related information.
115879					FAQ 2.2 : 4th paragraph, (has contributed to GMSL), together with the loss of land ice. Explain phenology for a non specialist in the FAQ. Check the figure text (acidification rather than acidity, near surface permafrost temperature extent or thaw?, sea ice only in the arctic, greening area larger then browning area (there is also browning, SRCCL),. [ Valerie Masson-Delmotte, France]	Taken into account - combined with comment 115805.
19373					Chapter 2 was the best of what I read. It was generally comprehensible, and it was well organized and illustrated. [ Steve Colman, United States of America]	Noted with thanks.
5313					'methane' and 'CH4' are used interchangeably throughout chapter, suggest using CH4 after defining on first appearance [ Sheel Bansal, United States of America]	Editorial. Addressed in copy edits.
112077					Would be good to coordinate with the Interactive Atlas so the information that is shown in the Interactive Atlas (IA) is aligned (and potentially allow reproducing) some of the Chapter 2 figures, in particular 2.11 (temp) and 2.14 (precip). The IA SOD version includes a preliminary configuration of observational datasets for both temperature (only for land, since air/ocean variables are treated separately) and precipitation. Further variables would be included and coordination (both methdological trends/deltas, masking and datasets) will be seek with Ch2 for designing the final draft. [ jose manuel gutierrez, Spain]	Taken into account. Better collaboration with the Interactive Atlas has occurred in finalisation.
5331					Overall, in this as other chapters there is too much jargon and too many acronyms making the chapter unnecessarily difficult to read. Often those, which do not define a highly specific variable, can be eliminated and replaced by more common terms. In general, acronyms should be redefined in each new section, that is more than a few pages from their last use. Many of those associated with geological time periods by using year spans. One does not want to continually refer back to the C-C Box 2.1 Table 1. I have not specifically pointed out the scores of examples. [ Bryan Weare, United States of America]	Taken into account. Where practical acronyms are spelt out. However, there is an unavoidable need to use acronyms in several places for consistency. The final report will include an annex with acronyms, to aid readability.
127197					There is no temperature "target" stated in the Paris Agreement. Reformulate to match the actual agreement text or describe as temperature goals to be consistent with the framing from other chapters. [ Trigg Talley, United States of America]	Taken into account. The Paris Agreement is no longer referred to explicitly in the revised cross-chapter box 2.3 although there is discussion of 1.5 and 2 C global warming levels.



Comment ID	From Page	From Line	To Page	To Line	Comment	Response
127199					There are some notable omissions, such as discussion of the water vapor feedback and discussion of changes in tropical cyclones and their environments. Readability is hampered by excessive use of acronyms, and in some cases inconsistent nomenclature. There is a question about how statistical significance is used in light of recent papers by the statistical community. Regarding water vapor feedback, despite the rationale for the ERF framework, some discussion and explanation is warranted concerning the radiative role of water vapor and the water vapor feedback. It is well known that water vapor is a dominant greenhouse gas in Earth's atmosphere. There is also high confidence that its concentration is increasing with warming, as discussed elsewhere in the chapter. Perhaps this could be included in Section 2.2.5, which discusses changes in short-lived, radiative important gases. Stratospheric water vapor changes are discussed, but not tropospheric water vapor changes. A quantitative description of changes in water vapor concentration of the type documented in Section 2.3.1.3 would help to clarify the situation for readers. Alternately, a discussion or review of the ERF framework would be helpful. The water vapor feedback, and the fact that water vapor concentrations are increasing, are key to describing the most important changes in the climate system. In Section 2.3.1.3.2, the increase in water vapor concentration is discussed: There is high confidence that this powerful greenhouse gas is increasing. However, it could be discussed in a radiative context in Section 2.3.1.3.3. In section 3.3.2.2, it is stated that water vapor is the most important natural greenhouse gas and that it is expected to increase with warming. On page TS-80, lines 33-36, the water vapor feedback is also discussed. It seems that these should be cross-referenced here, to improve accessibility for readers. Regarding Tropical Cyclones, while other chapters cover this topic, in this chapter, the history and recent changes in the number, intensity, and impacts of these important systems are not discussed but they could be. There is discussion of changes in blocking, jet streams, the polar vortex, and extratropical storm tracks, but not tropical storm tracks. Given the large societal impact of these systems, and the abundance of research on this topic, it is important to represent it in this chapter. Finally, regarding Statistical Significance, over the past several years, the scientific and statistical research communities have discouraged dichotomous distinctions between a given change or effect being "significant" or not (e.g., Amrhein et al. 2019). In fact, some have called for a complete ban on the use of the term	Taken into account. Water vapour feedback (ch.7) and tropical cyclones (ch. 11) are the purview of other chapters. However, tropical cyclones are mentioned implicitly in conjunction with consideration of Hadley Cell changes. Acronym use has been revised. The assessment of ERF is undertaken in chapter 7 and clearly cross-linked. The text has been reviewed for appropriate use of confidence language.
127201					Despite what it says on the top of page 2-8, this chapter repeats a lot of material that is also covered in later chapters (e.g., the discussion of long-lived and short-lived radiative forcing gases and particles discussed on pages 2-17 through 2-27 are also covered in later chapters, such as Chapters 5 and 6. Seems like the chapter could be greatly reduced in size without losing much content from the report as a whole. [ Trigg Talley, United States of America]	Taken into account. Further efforts have been made to reduce redundancy but the scoped structure of the overall report means it is inevitably a matrix problem and that some overlaps are unavoidable. Hand-offs are improved in the FGD wherever possible.
127203					The chapter is too long. The authors should look for ways to reduce. It is supposed to be an assessment not a review, which means not every reference needs to be discussed. [ Trigg Talley, United States of America]	Rejected. Chapter is actually shorter than requested (according to the allocated page limit) and already cites only a subset of the literature while performing an assessment.
115945					Congratulations for a well advanced SOD, and particularly for advancing the systematic integration of insights from paleoclimate information with those from modern observations. The summary statements at the end of each section are very helpful. I suggest to better highlight changes compared to AR5 and AR6 special reports in the executive summary statements (with a focus on what differs and why). This is particularly the case for ERF and for observed warming. [ Valerie Masson-Delmotte, France]	Taken into account in revisions to the ES and the summary statements in each section.
115947					There are several occurrences where there is an assessment of acceleration. There is a need to introduce a definition with a common method to diagnose an acceleration in datasets, and apply it consistently. Some aspects refer to a change in the mean rate of increase (but for GMSL, an acceleration is diagnosed). To coordinate with Ch1 + glossary. [ Valerie Masson-Delmotte, France]	Taken into account. We have made much less use of the term in revisions accounting for this and inter-chapter discussions on the matter.
93677					The lack of Indigenous knowledge (IK) in AR6 WG1 represents a data limitation, as Indigenous Knowledge could constitute a distinct line of evidence on paleoclimate reference periods. [ Bridget Doyle, Canada]	Taken into account; added statement to concluding section to point out this gap in CH2

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115949					The ES assessment of GMSL being unprecedented in the last 6000 years is not supported by the underlying assessment, please check. [ Valerie Masson-Delmotte, France]	Taken into account. In the FGD a more complete paleo series is assessed and the assessment finding is re-evaluated in this context and thus better supported.
93679					Tsleil-Waututh Nation can provide a case study on using myriad lines of evidence, including but not limited to: traditional knowledge, monitoring programs, seafloor observatories and paleoarchaeological data to develop a model of pre-contact ecological and climate conditions [ Bridget Doyle, Canada]	Taken into account in revisions to Section 2.5 where the potential role of indigenous knowledge is now flagged.
115951					The ES assessment of shifts in climate zones is not fully supported by the underlying assessment in section 2.3.4. Is it possible to define climate zones (text, glossary) and also to define somewhere what is meant by the "state of the climate system" (ch 1, glossary?). [ Valerie Masson-Delmotte, France]	Taken into account - text revised to better support the assessment finding.