Comment ID	From Page	From Line	To Page	To Line	Comment	Response
107629	0	0	0	0	Congratulations to the chapter 7 team for this excellent and interesting SOD. [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you
51299	0		0		GENERAL COMMENT ON CHAPTER - this is a really clearly written chapter, thank you. Throughout the climate sensitivity section, multiple references are made to the WRCP work on ECS and TCR, i.e. Sherwood et al. A significant amount of weight is evidently placed on this work and yet the methodological basis of this research and why it is such an advance is not really articulated. It is just referred to in generalities like "bayesian methodology". It would be helpful if a short consolidated overview of this paper is provided please. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The revised manuscript now more clearly refers to the related literature. We thank you for the positive comment.
19461	0				Comments of the Russian Federation: According to the IPCC AR5 WGI, the estimate of direct radiation forcing of black carbon is 0.40 W / m2 (Chapter 7, p. 617 and Fig. 7. 18). This is a significant amount as compared to the total anthropogenic forcing (about 2.5 W/m2). However, in the IPCC AR6 WG1 the direct radiation forcing of black carbon amounts to be no more than 0.2 W/m2 (Chapter 7, Fig. 7. 10). It would be useful to see in the text the comments on such a decrease. [Sergey Semenov , Russian Federation]	Taken into account. Per-species are assessed in Ch. 6 and the figure in question has been moved to that chapter, so this comment is no longer relevant to Ch. 7.
37135	0				The introduction to this chapter should provide a table that lists all GHG (including water vapour), showing for each the ppmv in air. If it also shows the GWP for each gas then an explanation of how the GWP was determined is required. (This is all part of being comprehensive, open and transparent.) (PS. I eventually found a table with this data on page 33, but the information is required at the start of the chapter, so either add a table or refer readers to table 7.5.) [John McLean, Australia]	Rejected: This information appears at appropriate places within the chapter
71955	0				Thisis a very important, if somewhat complex chapter. The authors should be congratulated on such a through assessment. This is not my area of expertise but it was pleasing to see the (long) attempt to narrow uncertainties regarding climate senistivities. However, I remain a little sceptical that the uncertainties have been reduced so significantly for the very likely range. In the Key Table (7.13), I do not see any justification why the upper end of the likely range would be less than the upper end of the process understanding likely range - there is no evidence in this table for a smaller range. Also, this table seems to ignore some information such as climate variabiity. For example, it seemed to downplay (ignore) the role of natural variability in changing alpha (Page 82, line 6-7) and in the paleo section, my reading of their table would have given a larger range, particularly at the top end, then presented in Table 7.13. [John Church, Australia]	Taken into account. The rationale for assessing the very likely range is given in Section 7.5.5, and we have added an improved explanation of why the upper likely range is lower than that from process understanding.
14869	0				Each chapter is supposed to include paleo-evidence however, there is really very few about (remote) past in this chapter.This should be included to meet the requirements. [Marie-France Loutre, Switzerland]	Rejected - this Chapter contains a substantial amount of paleo information, in particular Sections 7.4.3, 7.4.4, and 7.5
67863	0				The IPCC methodology for measuring the earth's energy budget needs to be improved. It is estimated that the surface energy budget has a big difference due to weak observations. This is necessary because the surface energy budget (both land and sea) can accommodate the anthropogenic CO2 emissions associated with burning fossil fuels and geothermal systems that have recently been developed to replace fossil fuels and are considered as green energy. As is generally known, the use of fossil fuels (for transportation and industry) is one of the main drivers that caused the increase of imbalance of the earth's energy. A better methodology in measuring the surface energy budget is expected to be able to determine whether or not there is a plan to reduce the use of fossil fuels by countries in the world, and whether the plan has been carried out. This can be a tool to monitor and evaluate activities or policies, especially in terms of developing green energy and green economy. Thus, a mathematical model may be needed to include CO2 concentrations in the atmosphere due to economic activity and all heat fluxes in the atmosphere. [Ruandha Agung Sugardiman, Indonesia]	Noted. We think there is some confusion here around the use of the term "energy budget" across IPCC working groups - perhaps the reviewer is referring to the WGIII definition that deals with anthropogenic energy consumption/production?. We will improve the clarity of our presentation/wording to make it clearer that we are talking about Earth's radiative energy budget - i.e., we seek to evaluate whether the difference between radiative forcing of the climate system and the Earth's radiative response is consistent with the observed changes in total heat content change (which we refer to as "total earth system warming").
67865	0				In this chapter it was mentioned the use of Top Atmospheric method. It is better to calculate the surface energy budget (land and sea) to be able to cover anthropogenic activities. Improved methodology is thus expected. [Ruandha Agung Sugardiman, Indonesia]	Noted. Please see our response to the previous comment from this reviewer.
5149	0				This chapter is an impressive intellectual achievement. I remember discussions years ago when the distinctions between instantaneous forcings and adjustments, how best to define transient sensitivity, what pattern effects were, and other topics were still being worked out. This chapter summarizes these topics with great clarity and rigor, something not possible ten or even five years ago. I must repeat I am impressed by the clarity and rigor of the definitions of the various climate terms. The discussion of the process constraints on climate sensitivity is balanced and appropriate. [Daniel Murphy, United States of America]	Noted. Thank you!

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Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132385	0				This chapter is not providing a lot of background on the effects of land use forcing, although they are substantial on regional scale and in particular for extremes and low-emissions scenarios: a) Lejeune, Q., et al. 2018, Nature Climate Change: "Historical deforestation locally increased the intensity of hot days in northern mid-latitudes". https://doi.org/10.1038/s41558-018-0131-z ; b) Hirsch, A. L., M. Wilhelm, E. L. Davin, W. Thiery, and S. I. Seneviratne (2017), Can climate-effective land management reduce regional warming?, J. Geophys. Res. Atmos., 122, doi:10.1002/2016JD026125.; c) Seneviratne, S.I., R. Wartenburger, B.P. Guillod, A.L. Hirsch, M.M. Vogel, V. Brovkin, D.P. van Vuuren, N. Schaller, L. Boysen, K.V. Calvin, J. Doelman, P. Greve, P. Havlik, F. Humpenöder, T. Krisztin, D. Mitchell, A. Popp, K. Riahi, J. Rogelj, C.-F. Schleussner, J. Sillmann, E. Stehfest, 2018: Climate extremes, land-climate feedbacks and land-use forcing at 1.5°C. Phil. Trans. R. Soc. A. 376. [Sonia Seneviratne, Switzerland]	Rejected: This chapter does not address regional-scale forcing
71717	0				The Executive Summary has a problem. ERF is mentioned all through this summary but it is a new term; there is a significant distinction between it and the RF used all through Chapter 5; Chapter 6 has different values for ERF in Table 6.4 than given in this chapter with no clear reason why; ERF is not defined in this chapter until Box 7.1 on Page 11; and it then has to be clarified further in section 7.3 starting on page 23. While the introduction to the Executive Summary has a short reference to ERF this does not mention that it is an advance on the AR5 or give any indication of what it covers that is not in Chapter 5. Either the introduction should be expanded or there could be a long footnote at the beginning of the Executive Summary saying why ERF is not the same as RF. While Box 7.1 helps, Figure 1 is not enough. The distinction between a direct RF and ERF needs to be introduced more clearly, and not just for CO_2, e.g. it could summarise why the CH_4 ERF is less than its RF. Further confusion is raised by Fig 7.10 which refers just to 'radiative forcing' but its base value is the ERF and the figure then shows additional indirect effects due to atmospheric chemistry and clouds. Clarification in the terminology should deal with all three significant components of radiative forcing and use a consistent terminology. For example, when just the Etminan et al formulae are used it has been called instantaneous radiative forcing, or I would suggest direct radiative forcing is clearer, then the term effective radiative forcing could be used as it is now, but when additional indirect effects are included as shown in Fig 7.10 it should be referred to as total ERFs, which is the terminology used in Thornhill et al (2020). Consistent use of this terminology across chapters 5, 6 and 7 would make them clearer. [Martin Manning, New Zealand]	Taken into account. Terminology and ES text has been clarified as suggested, ERF is based off AR5
132395	0				Recent publications have shown the fact that land use forcing can affect extremes differently than means, e.g. increases in albedo or evapotranspiration leading to a stronger cooling of hot extremes compared to mean temperature. This might be a relevant topic to be addressed in chapter 7. Refs: 1) Davin, E.L. et al. 2014, PNAS (www.pnas.org/cgi/doi/10.1073/pnas.1317323111); 2) Thiery et al. 2017: J. Geophys. Res. Atmos., 122, doi:10.1002/2016JD025740; 3) Seneviratne, S.I., R. Wartenburger, B.P. Guillod, A.L. Hirsch, M.M. Vogel, V. Brovkin, D.P. van Vuuren, N. Schaller, L. Boysen, K.V. Calvin, J. Doelman, P. Greve, P. Havlik, F. Humpenöder, T. Krisztin, D. Mitchell, A. Popp, K. Riahi, J. Rogelj, C.-F. Schleussner, J. Sillmann, E. Stehfest, 2018: Climate extremes, land-climate feedbacks and land-use forcing at 1.5°C. Phil. Trans. R. Soc. A. 376.; 4) Lejeune, Q., et al. 2018, Nature Climate Change: "Historical deforestation locally increased the intensity of hot days in northern mid-latitudes". https://doi.org/10.1038/s41558-018-0131-z [Sonia Seneviratne, Switzerland]	Rejected: This chapter does not address climate extremes.
132405	0				This chapter does not put the biophysical effects of land use in perspective with its CO_2 effects. However, this is highly relevant when discussing the potential of afforestation in limiting global warming, in particular in low-emissions scenarios (BECCS). I strongly suggest that the authors make an effort to provide a more in-depth assessment on this topic, maybe on ca. 1 page. Some relevant publications on this topic include e.g.: Betts, R.A., et al. 2007: "Biogeophysical effects of land use on climate: Model simulations of radiative forcing and large-scale temperature change", Agr. For. Met., doi:10.1016/j.agrformet.2006.08.021; Lejeune et al. 2018, Nature Climate Change: "Historical deforestation locally increased the intensity of hot days in northern mid-latitudes". https://doi.org/10.1038/s41558-018-0131-z"; Windisch et al., in review (I can provide a copy of this article to the chapter 7 authors). [Sonia Seneviratne, Switzerland]	Rejected: This chapter does not address mitigation methods.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79937	0				Chapter 7 leaves out metrics with timescales shorter than 50 years as does all the accompanying text. Such metrics should be included (e.g. GWP20, GTP10/20) as they are used extensively for analysis of consistency with long-term temperature targets, but also for life-cycle analyses, for carbon-equivalent footprints of nations/companies/etc., for analysis of the rate of change in the near-term (which is also part of agreements under the UNFCCC), and by policy-makers who have developed near-term climate mitigation plans. Figures in chapter 12 (e.g., 12.4, 12.5, 12.6) do consider projected changes for 2041–2060 so near-term climate metrics are, in addition, useful for interpreting these. Chapter 7 metrics should include shorter-term metrics to help policymakers discuss these near-term impacts. Including climate metrics with timescales shorter than 50 years would be consistent with climate metrics reported in the AR5 and AR4 Working Group I reports. AR5 Table 8.A.1 includes GWP values at 20, 50, and 100-year time horizons for GWP and GTP. AR4 Table 2.14 reports GWP of greenhouse gases at 20, 100 and 500 year time horizons. The WG1 authors do not provide a rationale for removing the short-term metrics, only indirectly discussing the benefits of comparing a step-change in short-lived forcing with a pulse change of long-lived gases. There would be enormous implications, policy and financial, of switching to a metric such as CGTP that would enormously increase the value of SLCF removals in the short-term but eliminate their value in the long term, thereby radically changing financial incentives. These could be discussed in WGIII, but WGI report should not simply eliminate the prior short-term metrics without consideration of the implications as outlined above. [Valentin Foltescu, India]	Taken into account. We have now included GWP20 in the assessment.
22145	0				This chapter has restored my faith (which was rapidly waning) that it is possible to draft coherent chapters that are easy to read despite containing scientifically dense information. Thank you. [Peter Thorne, Ireland]	Noted. Thank you
114563	0				Ch7 is well written, has a good structure and is in good shape. Sometimes a bit heavy to read, but this material is complex and needs a thorough treatment. The chapter contains many fidnings of high importance for WGI and also for WGIII. [Jan Fuglestvedt, Norway]	Noted. Thank you!
22163	0				There is a degree of repetition between the physical feedback component (7.4) and the ERF (7.3). I wonder whether it would make more narrative sense and also save some words to introduce feedbacks first and then ERF (switch 7.3 and 7.4)? [Peter Thorne, Ireland]	Taken into account. Switching the order of 7.3 and 7.4 would break the current logic of the chapter, so this has not been adopted. Nevertheless, repetition between the two subsections has been reduced and cross-referencing increased.
39595	0				While the coordinating lead author, P. Forster, is self-cited in the chapter not less than 32 times, prominent scientists like J. Christy, S. Schneider, R. Pielke Sr, and many others who reported important conclusions in the field of this chapter (notrickszone.com/50-papers-low-sensitivity) are not cited and their findings are ignored. As a result, the chapter attaches too much weight to highest values of climate sensitivity and ignores lowest values. This expert reviewer considers that this choice decredibilizes the entire chapter. The observation of (i) +0.4°C since 1945 (Figure 2.11B), beginning of the acceleration of CO2 emission, (ii) the "pause" since 1993 in the low stratosphere, (iii) the absence of evolution of UAH MSU tropical temperature at 200-300 hPa which does not validate the hot spot predicted by models, (iv) the low ocean heat content after Wunsch and Heimat (2014) and (v) his cyclical-like behavior shown in Fig. 10 of Laloyaux et al (2018) doi: 10.1029/2018MS001273, all these observations do not substantiate high climate sensitivity. Please consider major revision towards a more balanced chapter. [François Gervais, France]	Noted. This comment was already made, with exactly the same words, for the FOD. Articles were evaluated to the extent that they are relevant to this chapter. The list mentioned includes many articles whose results are misinterpreted by the authors of the list, as these articles do not demonstrate a low sensitivity of the climate to CO2
22189	0				It feels to me like the state dependency of the feedback parameter assessed in 7.4.3 even though only medium confidence is of great potential policy relevance and due consideration should be given to its elevation to the ES so it can be integrated into the TS and maybe even the SPM? [Peter Thorne, Ireland]	Taken into account. A statement on the state dependence of feedbacks has been added to an ES point.
22215	0				Thankyou also for being one of the few chapters to consider that the graphics may be used as standalone items. Nevertheless in a few places it would be good to consider font size / legibility and adding self describing figure titles so they can truly be used as standalone. [Peter Thorne, Ireland]	Taken into account. Figures were revised for the final draft.
52213	0				Are uncertainties denoted with a +/- symbol meant to imply that the distribution of uncertainty is symetric? If so, please state this. If not, please explicity state the values for the appropriate uncertainty bounds and use the +/- symbol only for cases where uncertainty is implied to be symetric. [Gregory Garner, United States of America]	Taken into account. Yes, this is clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37117	0				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'. 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 (CO2, CH4 and N2O) to name just a few. 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 CO2 has negligible effect. Also, IPCC TAR (2001) says on page 145, in a discussion of the radiative forcing of N2O, '[t]his RF is affected by atmospheric CH4 levels due to overlapping absorptions.' These were the only instances that I could find that even slightly talk about bandwidths and overlapping absorption. 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 atmsophere, it also means that the warming these other GHGs cause is negligible and there is no good reason why any gas other than CO2, which has a small bandwidth that does not overlaps with H2O, should ever be mentioned in an IPCC report. A reference that shows this simply is https://commons.wikimedia.org/wiki/File:Atmospheric_Transmission.png 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. Also, remove all discussion of GHGs whose action is negligible either by being present in such small quantities or by having their absorption bandwidth overlapping with gases whose concentration is much greater. [John McLean, Australia]	Noted. Water vapour is usually treated as a feedback rather than a forcing. There are papers on the GWP of water vapour, but we have not included these because of space constraints and the significance of the other forcing agents. IPCC has recognized the complete state-of-the-art-knowledge of water vapor in the climate system. See section 7.4.
36863	0				The IPCC authors don't know the difference between globally averaged near surface air temperature (your GSAT), which is impossible to calculate because there are insufficient temperature-measuring locations and there were even fewer in the past, and globally averaged near surface air temperature anomalies. (The misuse of GSAT occurs about 30 times in this chapter.) [John McLean, Australia]	Taken into account. GSAT definition has been clarified
31679	1	1	1	1	Really impressive chapter that has moved on significantly since the FOD. Nice work. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you
81385	1	1	1	1	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]	Taken into account: This has been addressed in the specific comments.
89169	1	1	1	36	Congratulations to the team for an excellent SOD. The chapter covers several chapters from AR5 on core knowledge on perturbations to the climate system and not unexpectedly has length of many pages. The length will probably increase after the review process. I encourage the Authors to consider if some material can be moved to the supplementary. [Gunnar Myhre, Norway]	Taken into account. Chapter length with be shortened for FGD
98651	1	1	206	8	sincere congratulations for assembling a very informative chapter for the next IPCC report ! [Michael Schulz, Norway]	Noted. Thank you!
77283	1	1	206	8	The Executive Summary needs to be expanded on to better explain the content of the chapter. Overall the chapter is quite technical and would benefit from an introductory paragraph to the different sections to explain the content. In particular Effective Radiative Forcing (ERF) should be explained in the executive summary as understanding this gives a context to the chapter as a whole. It is well explained in the opening sentence of the relevant paragraph (7.3). [Emer Griffin, Ireland]	Accepted. We have added a brief introductory paragraph for each topic of the ES.
77285	1	1	206	12	This is perhaps the most important chapter in the WG1 report as it updates our knowledge of how humans influence the Earth's energy budget/balance, by how much, and the climate systems responses. It should be written in an accessible and clear manner and fit into the AR6 narrative the central issue of how humans are changing the Earth's energy balance. It is well structured and written but could be clearer for the non specialists reader. [Emer Griffin, Ireland]	Accepted. We have simplified and clarified as much as possible.
77287	1	1	206	12	Scientific terminology should be avoided or explained in the context of a consistent narrative about the Earth's energy balance. Comments provided here are designed to assist in this. [Emer Griffin, Ireland]	Noted. We agree and thank you

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77289	1	1	206	12	As pointed out in other comments the term energy budget, energy balance, energy imbalance and perturbations of these constructs are used in the AR6. A consistent wording which is not deeply technical should be used to articulate this narrative. This can be provided here. Inclusion of Units can assist in this. [Emer Griffin, Ireland]	Accepted. Text has been revised for consistency across the chapter and wider AR6 report.
77291	1	1	206	12	The material in this chapter could be better addressed in the SPM and in Chapter 1, this includes section A of the SPM in which a clear not technical outline of the Energy budget/balance issue can be provided and how it has recently been changed by human activities can make this material more accessible. [Emer Griffin, Ireland]	Taken into account. Wording is made less technical in line with your comments and passed to SPM team for consideration
77293	1	1	206	12	The material in this chapter could be better addressed in the SPM and in Chapter 1, this includes in section A of the SPM in which a clear not technical outline of the Energy balance and how it has recently been changed by human activities can make this material more accessible. [Emer Griffin, Ireland]	Taken into account. Wording is made less technical in line with your comments and passed to SPM team for consideration
18007	1	1	206	55	IPCC chapters have a tendency to read like review papers in which every publication and numerical result is mentioned. They are meant to be assessments leading to significant, policy relevant statements. I keep looking for bullet points that are supported by an efficient scientific argument. Do you plan to bring some of these probability statements out of the text as bullet points that can be elevated to the TS or SPM? [Dennis Hartmann, United States of America]	Taken into account. Probability statements are reworked for clarity
18021	1	1	206	55	This chapter contains a wealth of important material, but its density and length can make it hard to find the important things. I think the authors should decide which of their conclusions are important enough to highlight and bring tthese out in highlighted statements. Then greatly edit the document to eliminate the text that is not required to support those conclusions. This can also help identifying things that should be brought forward to the TS and SP. Also, it seemed like the text was repetitive in places, describing the same concepts or qualifications multiple times. [Dennis Hartmann, United States of America]	Accepted. We have shortened the text and made it less repetitive.
10713	1	1			Whole chapter: As someone who has followed the evolution of IPCC assessments over the years I have noted with some bemusement the continuing changing definitions and details of the techniques used to estimate forcings and sensitivity. A more cynical person than I might think the changes in the language is used to hinder non experts in the field commenting or critiquing on methods and approaches! Effective radiative forcing is a case in point. The definition in this chapter seems to have subtly changed to what has been used in previous IPCC assessments and studies. I make a plea to the authors to 1) try to be as consistent as possible to previous assessments and only re-define things where absolutely necessary; 2) use a term consistently within the report ;3) create new terms rather than re-defining a previously used term. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. ERF is broadly consistent with AR5 though and the nuances reflect the literature - especially Sherwood et al. 2015
10715	1	1			Whole chapter:Is every given value of "effective radiative forcing", ERF, in this report 'adjusted' to attempt to account for land temperature adjustments? In this chapter I have come across numerous examples of quoted ERF values from studies being used which have not had this adjustment applied. I strongly recommend that it is clearly indicated when the adjustment has been applied and when it has not. I fear that currently there is a pick and mix attitude to using ERF values from different studies, which is very difficult to follow or interpret. If an amended version of ERF is going to be adapted - to get an "approximate measure of ERF" (Page 12:5) - then, and I can't believe I am going to write this, yet another term should be introduced to distinguish it from standard ERF. e.g., "Adjusted Effective radiative forcing" as defined in Richardson et al (2019) [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: It has been made clear throughout whether the land surface adjustment has been taken into account..
104893	1		223		Congratulations on a very well-structured chapter and for clearly establishing the advances/innovations since AR5. [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you!

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68875	1				Paleoclimate information has been successfully distributed across the WG1 report, as envisaged by the scoping documents. The Paleo BOG has now developed key messages to consolidate and convey the most policy-relevant paleoclimate content, and to advance it to the summary documents (TS & SPM). The Paleo BOG looks to CH7 to include critical information needed to address three of the key messages from paleoclimate and to include the outcome of the assessment in its Executive Summary, including: (1) What are the prominent large-scale, recurrent spatial patterns associated with past global changes (e.g., polar amplification, tropical Pacific gradients)? (2) What are the long-term effects of sustained warming across the Earth system? And (3) What do past climate states indicate about equilibrium climate sensitivity? [Darrell Kaufman, United States of America]	Noted - we address these three points in the Chapter.
81387	2	9	2	9	“TOA” is not defined yet. In addition, a list of acronyms would help, especially since other chapters already have one. [Johannes Laube, Germany]	Accepted. TOA is now defined.
102123	3	40			Change "Process-based estimates" to "Estimates based on process understanding" (to parallelied the different section heading) [Maria Rugenstein, Germany]	Accepted
102125	4				Subsections of 7.5, from 7.5.5 onward are hard to follow and not really intuative *from just reading the table of content* [Maria Rugenstein, Germany]	Taken into account. sub headings are reworked
31543	5	1	5	1	I really appreciated reading this chapter. Congratulation to the authors [Jean-Baptiste SALLEE, France]	Noted. Thank you!
17415	5	1	8	12	Land use and land use change effects should be more highlighted in "Executive Summary". [Mostafa Jafari, Iran]	Accepted. We have added statements on the ERF associated with land use change to the ES.
33041	5	1	8	12	land use and land use change effects should be more highlighted in Executive summery [Sahar Tajbakhsh Mosalman, Iran]	Accepted. We have added statements on the ERF associated with land use change to the ES.
32711	5	1	8	12	land use and land use change effects should be more highlighted in Executive summery [sadegh zeyaeyan, Iran]	Accepted. We have added statements on the ERF associated with land use change to the ES.
102127	5	1	8	12	If the audience are not scientists in the field, this has to be mae much more readable. One can see that different sections and even subsection are written by different authors. The structure is different for different subsections (sometimes AR5 is mentioned first, sometimes new results, sometimes definitions). I suggest to have the same structure for every subsection/paragraph: 1. Define what it's about, 2. Numbers, 3. Why/background/arguments 4. why this is different from CMIP5. [Maria Rugenstein, Germany]	Taken into account. We have now streamlined the sections such that they all follow roughly the same structure.
128811	5	1	8	13	[CONFIDENCE] More generally, the usage of confidence statements seems to be inconsistent across the Executive Summary as a whole, with confidence statements seemingly arbitrarily associated with the bold main findings sometimes, while at other times only associated with one aspect of the supporting text. Greater consistency in that usage would improve the Executive Summary. [Trigg Talley, United States of America]	Noted. Confidence statements are at end of paragraph if the same in whole paragraph, otherwise each sentence or group has its own.
106321	5	1	8	13	Excellent job in integrating advances in energy balance, RF and climate sensitivity in some of the most important messages that will come out of the report. I also commend the clarity of the ES. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Noted. thank you
77295	5	1	8	13	The executive summary is long, detailed and technical, Consider options to reduce the length to address key findings and provide material that could be used in the SPM. E.g. on energy budget/balance forcing , sensitivity and rates of change, contribution of main GHgs to temperature increments and masking of this by aerosols. [Emer Griffin, Ireland]	Taken into account. Some technical details are needed but text is reduced and simplified where possible
111351	5	1	119	11	General comment: I appreciate the systematic treatment of different species throughout this chapter. The assessment of ERF and confidence in species abundance is easy to follow. Very nice. This approach could flow back to ch6 which could benefit from slightly more discussion of detail. [Tami Bond, United States of America]	Noted. Chapter 6 has been made aware of your comment, thank you
111353	5	1	119	11	General comment: The effect of 1750 pre-industrial reference is always buried in the widely-shared forcing figures. Knowledge about the reference is discussed in this chapter-- however, this method of doing business is not ever questioned here. If the pre-industrial reference is in question, are there presentations with more confidence (eg. response to an increment of today's emissions) that could be considered? This may be a question for future AR, only. [Tami Bond, United States of America]	Taken into account. Further details are added discussing reference periods and reference made to Chapter 1
37153	5	3	5	3	Be balanced. Mention natural variations in the Earth's energy budget too. [John McLean, Australia]	Taken into account. Natural variations in Earth's energy budget are discussed in Chapter 7, including in section 7.2 and 7.5
31681	5	3	5	3	"emissions of aerosols and their precursors" [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
30631	5	3	5	4	It is inaccurate to say increases in aerosol emissions. Should be increases in emissions of aerosols and aerosol precursors. [Hong Liao, China]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77301	5	3	5	4	It is not just aerosol emissions, it is also precursor species, perhaps just refer to other other emissions to the atmosphere as considered in Chapter 6 [Emer Griffin, Ireland]	Accepted.
77297	5	3	5	16	In essence the Earth's energy balance/budget has remained relatively stable for centuries to millennia but that balance has been substantially changed by large scale emissions of GHGs and other species which have altered this balance. This type of explanation would open up this chapter for non specialist readers. [Emer Griffin, Ireland]	.Taken into account. Some of this sentiment now included to aid clarity
77299	5	3	5	16	The energy balance is measured in W/m2 and changes to this balance which are termed here Effective Radiative Forcing are also calculated as W/m2. [Emer Griffin, Ireland]	.Taken into account. ERF is discussed in next section but introduced at start and bullets changed
77303	5	4	5	4	Can the energy balance be used? Budget has wider means including in the AR6, and in other areas. [Emer Griffin, Ireland]	Accepted. We agree, balance used as much as possible in revision
77305	5	4	5	4	Put in units for Energy balance budget and ERF [Emer Griffin, Ireland]	.Taken into account. Added where appropriate
77307	5	4	5	4	EFR could be more usefully explained here as changes to the energy balance this would be clearer for the non specialist [Emer Griffin, Ireland]	.Taken into account. Not quite true - but text clarified
77309	5	4	5	4	It may be clearer to explain the response of earth's climate system is referred to as climate sensitivity and two types are considered i.e. transient and equilibrium. Then address the feedbacks. [Emer Griffin, Ireland]	Accepted.
83741	5	5	5	5	"Climate feedbacks that help understand" is awkward language. Suggest changing to "The response of the climate system to a given forcing is partitioned into climate feedbacks associated with physical processes" [Marvel Kate, United States of America]	.Taken into account. We agree, text reworded inline with suggestion
132379	5	5	5	7	Consider the role of regional feedbacks in the forced responses, which can be assessed using the regional climate sensitvity (Seneviratne and Hauser, 2020, Earth's Future). Note that the contribution of inter-model spread in RCS (regional climate sensitivity) to the regional spread in temperature extremes was found to be larger than that of the inter-model spread in GCS (global climate sensitivity or transient climate response). Some of the inter-model spread can be explained by decadal variability, and is thus not forced, but still the forced component is of substantial magnitude compared to the effect of GCS (see report in pre-LAM meeting in GMST session). Reference: Seneviratne, S.I., and M. Hauser, 2020, Earth's Future: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019EF001474 [Sonia Seneviratne, Switzerland]	Rejected. This is not relevant here, this section and bullet addresses global feedbacks
102129	5	5	5	7	Simplyfy sentence and grammar [Maria Rugenstein, Germany]	Accepted.
20061	5	5	5	11	"response to" on lines 5-6 is correct as well as on line 11; "response of" on line 8 and "response from" on line 9 are believed not to be. Besides, the sentence on lines 7-9 implies that carbon dioxide is not a gas. What is the reader to think? [philippe waldteufel, France]	Accepted. Wording has been changed to "response to" throughout. Text has been revised to reflect the fact that CO2 is indeed a gas.
81755	5	9	5	12	There is no mention on the link of chapter 2 for this specific chapter, but there should be given as chapter 2 is assessing the observed changes. [Karina von Schuckmann, France]	Accepted. Connections to Ch. 2 are now explicitly stated.
27073	5	9	5	12	There is no mention on the link with chapter 2 for this specific chapter, but there should be given as chapter 2 is assessing the observed changes. [Eric Brun, France]	Accepted. Connections to Ch. 2 are now explicitly stated.
83105	5	10	5	10	Ch6 i also using the ECS estimates to simulate contributions to future warming by SLCFs [Terje Berntsen, Norway]	Taken into account. We now refer also to Ch. 6 in this context.
114565	5	10	5	16	This desciption of relation to other chapters is useful [Jan Fuglestvedt, Norway]	Noted. Thank you!
117261	5	15	5	15	just note that in Chapter 1 and SPM the exact definition of very likely is (90–100%) probability. And 90% uncertainty interval as very likely range. [Maisa Rojas, Chile]	Accepted. Agree, reworded
103597	5	15	5	16	Convoluted sentence, separate "very likely" from the 5%-95% interval [Philippe Tulkens, Belgium]	Taken into account. Range now dropped
96687	5	15	5	16	On uncertainty expressed as 5% to 95%: As this is the general rule for AR6, this sentence can be omitted. [Nicole Wilke, Germany]	Accepted. Agree, changed as suggested
46059	5	15	5	16	Remove "Uncertainty is expressed (...) noted.". As mentioned in Chapter 1, this applies to the whole WG1 report. [Twan van Noije, Netherlands]	Accepted.
77311	5	19	5	21	An additional bullet which links the Earth's energy budget to the energy balance and how its is changed, and how these are quantified using the standard units would enhance the clarity of this chapter. [Emer Griffin, Ireland]	Accepted.
34905	5	19	5	27	Detailed Comments by SOD Chapter – Chapter 7: The SOD implies that the earth system warming is a matter of concern. Please see general comment #5 above. [Jim O'Brien, Ireland]	Noted. We want to imply it's a reason for concern - no action needed
77321	5	19	5	55	Consider reworking and merging the material in these bullets for clarity e.g. 1 Energy/ERF are robust and reliable indicators of climate change + include main numbers, 2 Outline changes and rates or energy uptake etc (3) identify the factors which control these e.g. GHG and aerosols. 4 Introduce the concept of climate sensitivity and how this translates to observations of ECVs [Emer Griffin, Ireland]	Accepted. The ES has been restructured in this way, including brief introductory paragraphs for each subtopic.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18023	5	19	8	13	It appears that these take home points are not repeated in the following text. Ideally, in my mind, the text would be organized to lead up to these conclusions, and at the end of this supporting discourse, the statement would be made in the text verbatim to what is carried forward to the summary. Trying to do this will illustrate the long and mazelike structure of some parts of the document, which should be reorgainzed to lead, without too many side excursions, to these points. [Dennis Hartmann, United States of America]	Accepted. We have streamlined the chapter to clarify links from ES bullets to the relevant text.
27075	5	21	5	21	We recommand to use the upper case "E" for Earth . Its an editorial comment, but it goes much further than this. An example: at UN level, there is a strong push that Ocean should be with upper case letter - this is more than an grammatical issue, and IPCC should not go a step backward in moving to Earth in lower case. [Eric Brun, France]	Accepted.
130529	5	21	5	21	unit for 144+-24 over 2006-2018? [Panmao Zhai, China]	Taken into account. Unit has been added.
128813	5	21	5	22	Shouldn't the bolded statement have a confidence determination (in parentheses) associated with it, as with the subsequent findings? [Trigg Talley, United States of America]	Accepted. Confidence statement has been added.
84831	5	21	5	22	" heat energy" is not correct . Use either "thermal energy" or energy stored in the atmosphere…. [Jayaraman Srinivasan, India]	Accepted. Text has been revised accordingly.
96689	5	21	5	22	Please rephrase since "change in" is included in "increased by …". We suggest the following modification: "Total earth system warming, i.e., the total additional heat energy of the atmosphere, land, ice and ocean, increased by 406 +/- 84 Zeta Joules over 1971-2018 and by 144 +/- 24 over 2006-2018. " Please consider including this paragraph into the SPM, in particular the last sentence. [Nicole Wilke, Germany]	Taken into account. Text has been revised, and material has also been included in the SPM.
15979	5	21	5	23	It is critical to recognize that the majority of the trapped heat (~70%) in the ocean is concentrated in the upper layers of the ocean, which is the most bio-diverse region of the ocean and also the part of the ocean that interfaces directly with ice sheets and subsea permafrost. This critical issue should be stated in these opening statements. See DOI: 10.1038/NCLIMATE2915 [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Upper ocean detail added
132381	5	21	5	27	An article currently in review and led by Pierre Gentine provides a revised estimate of land heat storage that is higher than in previous publications. I can share this article with the chapter 7 authors. [Sonia Seneviratne, Switzerland]	Noted.
128815	5	21	5	27	This applies also to 7.2.2. The borehole measurements in Chapter 3 of AR5 were assessed to give ground warming of "The 1950-2000 estimate of land warming, 6 TW,..." which is about 9 ZJ for 1971 to 2018, not the 5% or over 20 ZJ, given here. The paper justifying this is submitted and not available. Reviewers best estimate is more like 15 ZJ: authors' value is too large and not justifiable from anything published. See also Hansen et al 2011. [Trigg Talley, United States of America]	Taken into account. The reference has been made clear in section 7.2.2.2.
96691	5	21	5	27	We appreciate the use of global heat energy uptake as a metric for Total Earth System Warming. Please make the numbers more relatable to laypersons by comparing them to the total energy received by our planet from the sun in a certain period, e.g. one day (or a similar comparison). [Nicole Wilke, Germany]	Taken into account. See revised section 7.2.2.2.
104895	5	21	5	27	Not clear to which period doubled is relative. Missing a crosslink with Chapter 2 OHC section. [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The link to Chapter 2 is made from section 7.2 and Cross-Chapter Box 9.1
38609	5	21	5	27	"Total earth system warming is not explained in the glossary. It is unclear, whether the given numbers refers to a year or the time span mentioned. If so, it would be helpful to compare the yearly heat change. [Aribert Peters, Germany]	Taken into account. The text has bee revised to improve clarity.
65405	5	21	5	31	The statement that the earth's energy imbalance is 0.81±0.14 W/m2 is labeled as "high confidence". Why give a confidence level when you have the uncertainty? That doesn't make any sense. I note that on line 22, they give energy increase of the climate system in ZJ, but there is no confidence level here. That makes sense since there are error bars on the numbers. The error bars should give us confidence. [Andrew Dessler, United States of America]	Confidence statement is needed here as elements of the para have medium confidence
77313	5	21	5	36	This is key material but could be more clearly communicated. The main message would appear to be that the Energy Budget/Radiative Forcing or Effective Radiative Forcing is the most robust measure of climate change. This is a key message for policy which could be stated more clearly. [Emer Griffin, Ireland]	Taken into account. Reworded for clarity as suggested in intro paragraph
77315	5	21	5	36	While it may not follow the structure of the chapter it may be clearer to start with the energy balance/imbalance material as this is the driver of climate change and then move to the total earth systems warming. [Emer Griffin, Ireland]	Rejected. The preference is to structure the ES in the same way as the chapter itself, beginning with the total Earth system warming as an robust indicator of warming, and thereafter discuss the imbalance and its causes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77317	5	21	5	36	The use of term energy and heat are interchangeable here. Most non specialist readers will link heat to temperature increase rather than the wider issues of change of phase. Consider using energy, warming heat in a clearer manner for the non specialist. [Emer Griffin, Ireland]	Accepted. We now consistently use "energy" as opposed to "heat".
77319	5	21	5	36	A variation of the text from the opening of section 7.3 i..e line 39-41 page 23 could be used here . i.e. Changes in the Earth's energy balance are the fundamental driver of climate change change. These changes are quantified as ERF are a more reliable indicator of GCC than .... [Emer Griffin, Ireland]	Accepted. Agree, parts are added to a new bullet
28845	5	21			Earth heating is more accurate than warming since melting of ice takes up heat without increasing temperature. How about "Total Earth heating, spread across atmosphere, land, ice and ocean, increased by…."? (also next bullet) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have adopted the AR5 terminology of "global energy inventory".
52777	5	22	5	22	144+- (units are missing) over 2006-2018 [Monika Sikand, United States of America]	Taken into account. Units have been added.
39597	5	22	5	23	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, contradicting the > 90 %. 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 micrometers is only 0.01 millimeter, hence the OHC change is likely mainly natural, not anthropogenic. [François Gervais, France]	Noted. The Wunsch and Heimbach study would have been assessed as part of the recent SROCC report, from which the AR6 assessment builds and does not re-assess the older literature. The paper is known to be an outlier in terms of ocean heating estimates. The ocean heat content assessment is presented in chapter 2 and we refer you there for further information.
128817	5	23	5	23	There is new evidence that the contribution (in %) of ocean heat uptake (OHU) to total Earth system warming is slightly smaller than 90% (von Schuckmann et al., 2020), and that the contributions of upper, deep ocean, and the warming of other components such as of land and the cryosphere change over time. Recommend to review and potentially include findings of this paper in the present assessment. [Trigg Talley, United States of America]	Taken into account. The AR6 assessment includes the results of von Schuckmann et al (2020).
81757	5	23	5	23	There is a new publication which is currently under review - but matched the IPCC deadline, which provides new results, i.e. Ocean update of about 89% for a specific period. That means, it is variable - depending on period assessed, datasets used, uncertainties levels applied, etc - , and it should thus state: about 90% and not > 90%. https://www.earth-syst-sci-data-discuss.net/essd-2019-255/ [Karina von Schuckmann, France]	Taken into account. The AR6 assessment includes the results of von Schuckmann et al (2020).
27077	5	23	5	23	There is a new publication which is currently under review - but matches the IPCC deadline, which provides new results, i.e. Ocean heat uptake of about 89% for a specific period. That means, it is variable - depending on period assessed, datasets used, uncertainties levels applied, etc - , and it should thus state: about 90% and not > 90%. The corresponding paper is here (a LA is co-author of this paper, and the paper had been send to the TSU after submission on the 31st of December 2019): https://www.earth-syst-sci-data-discuss.net/essd-2019-255/ [Eric Brun, France]	Taken into account. The AR6 assessment includes the results of von Schuckmann et al (2020).
72147	5	23	5	24	In the summation of heat uptake per component, I suggest to add the inland water component. This could be done as follows: "… less than 1% in heating of the atmosphere and an even less than 0.1% by inland waters." (see previous comments in section 7.2.2.2 and Vanderkelen et al., 2020 doi: 10.1029/2020GL087867) [Inne Vanderkelen, Belgium]	Taken into account. It is too detailed to add inland water ways to the figure but this is now clarified in the text
81759	5	24	5	24	See previous comment. The paper reveals 2%, and thus this should be changed to about 1%. : https://www.earth-syst-sci-data-discuss.net/essd-2019-255/ [Karina von Schuckmann, France]	Taken into account. The AR6 assessment includes the results of von Schuckmann et al (2020).
27081	5	24	5	24	The following paper reveals 2%, and thus this should be changed to about 1%. https://www.earth-syst-sci-data-discuss.net/essd-2019-255/ [Eric Brun, France]	Taken into account. The AR6 assessment includes the results of von Schuckmann et al (2020).
27079	5	24	5	25	This is an important, but also strong statement, and we would recommend a modification: To state the importance of the Earth energy imbalance (and or the Earth heat inventory) as a robust indicator, but maybe not to say which one is bad (GSAT). Moreover, this strong statements needs an uncertainty level from the assessment outcome, otherwise it shoudl be removed. Moreover, suggest to mive this message to the paragraph below because its both, the imbalance, and the heat budget (ocean heat storage) which are used in literature as recommended global warming indicator. [Eric Brun, France]	Taken into account. We have slightly toned the statement down a no longer say that GSAT is bad
20063	5	24	5	26	This sentence is unpleasant because one should not try to take a stock (the cumulated earth system warning) as indicator of a rate. It becomes pleasant if "of the rate" on line 25 is deleted. [philippe waldteufel, France]	Taken into account. The rate is now used as suggested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81761	5	24	5	26	This is an important, but also strong statement, and I would recommend a modification: To state the importance of the Earth energy imbalance (and or the Earth heat inventory) as a robust indicator, but maybe not to say which one is bad (GSAT). Moreover, this strong statements needs an uncertainty level from the assessment outcome, otherwise it should be removed. Moreover, suggest to move this message to the paragraph below because its both, the imbalance, and the heat budget (ocean heat storage) which are used in literature as recommended [Karina von Schuckmann, France]	Taken into account. We have slightly toned the statement down a no longer say that GSAT is bad
19397	5	24	5	26	"indicator of global climate change? The heat uptake has greater signal-to-noise ratio than the surface temperasture, but I am not sure that it has a better ratio than the integral of the surface temperasture. [Isaac Held, United States of America]	Noted. We want to compare to GSAT here
86795	5	26	5	27	Please consider to put the following sentence in bold (as a headline statement): "The rate of earth system warming has roughly doubled since the 1970s.". [Oyvind Christophersen, Norway]	Accepted.
18643	5	26	5	27	After 1970.........Relative to what period? [Govindasamy Bala, India]	Taken into account. Clarification has been added.
10717	5	26			And "forced variability", as high frequency forcing factors, e.g. following explosive volcanic eruptions, are damped down in measures of total Earth system warming. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Exact wording has been added.
81763	5	29	5	29	I am surprised about this precise value: Isn't it the IPCC task to provide a range from what had been obtained from literature ? In AR5 for example, a range of 0.5- 1 W/m2 has been proposed - thus a range of published values. Moreover, I am not convinced that it makes sense to provide value precision until the second decimal - uncertainty is just too high (example: use 0.5 instaed of 0.54 for example, thus user rounded value, uncertianty 0.1 instead of 0.11) [Karina von Schuckmann, France]	Noted. For consistency with past IPCC reports and across the chapter, we retain precision in W m-2 to 2 decimal places.
20389	5	29	5	29	The 0,54 W/m2 figure deserves to be compared to the balanced energy budget, that is about 340 W/m2. This critical 0,0016 ratio should be known by (and made known to) anybody interested in anthropogenic climate change… [philippe waldteufel, France]	Rejected. The balanced energy budget has 0Wm-2 imbalance - the 340Wm-2 is the entire energy received by the sun and not a relevant quantity to compare to.
27083	5	29	5	29	The precise value of 0.54 +/- 0.11 is surprising : Isn't it the IPCC task to provide a range from what had been obtained from literature ? In AR5 for example, a range of 0.5- 1 W/m2 has been proposed - thus a range of published values. Moreover, we are not convinced that it makes sense to provide value precision until the second decimal - uncertainty is just too high (example: use 0.5 instaed of 0.54 for example, thus user rounded value, uncertianty 0.1 instead of 0.11) [Eric Brun, France]	Noted. For consistency with past IPCC reports and across the chapter, we retain precision in W m-2 to 2 decimal places.
99051	5	29	5	29	I do not understand the convention being used on capitalization of "Earth" when referring to the planet, especially so that one can leave "earth" to indicates what is happening to the soils, etc. Given the warming described includes atmospheric warming, I would urge that "earth system warming" be changed to "Earth system warming", so indicating the planet, just as "Earth" is capitalized in referring to "Earth's energy balance" and not the energy balance of surface soils. Lines 21, 24, and 26 also use "earth system" when they are really referring to planetary warming and so they too should read "Earth system". The planet that we live on merits capitalization in all situations by those in our communities, despite the traditin in some literary and media communities to use lower case, perhaps done as an implicit way of insulting Indigenous people's focus on references to the Earth, Moon, and Sun in their stories that are more a way of organzing and passing along empirical wisdom than a religion in the western-world sense of the word. [Michael MacCracken, United States of America]	Accepted.
86797	5	29	5	31	Is the Earth's energy imbalance of 0.54 +/-0.11 W m-2 the average for the period 1971-2018 or the change from 1971 to 2018? Please consider formulating this differently so that it is more apparent for the readers. [Oyvind Christophersen, Norway]	Taken into account. We have reworded for clarity
77329	5	29	5	31	The overlap of the periods for which data are provided does not assist in clarity. Could a different approach be used? [Emer Griffin, Ireland]	Noted. Standard periods are used across the AR6 (see, e.g. Ch2 and Ch9) for reporting the rates of change, for consistency.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
128819	5	29	5	31	[CONFIDENCE] The cited uncertainties on the EEI, pertinent to any confidence statement that might be made, are not credible. A main finding of the 2018 international workshop whose report is cited but not acknowledged (line 37, p. 16 Meyssignac et al., 2019) was "To date, only the method employing ocean in situ data (and potentially also the method based on re-analyses, but a robust and comprehensive uncertainty estimate is not yet available) enables to estimate the EEI with the required accuracy of 0.3 Wm-2 on decadal time scale." A key conclusion of that study was a call for a community assessment on EEI uncertainty given the current lack of definition. Such an assessment is ongoing under the auspicies of the WCRP. The same community also noted the cited "uncertainty" of order ±0.1 Wm-2 for the in situ observations does not represent total uncertainty that must include sampling and representativeness errors which are considered significant but not yet fully quantified. Since the total uncertainty is not yet available, and since the cited uncertainties are unrealistic being incomplete, a lower confidence than is stated is needed given the stated change in EEI over the time period noted (perhaps of the same order as the uncertainty). [Trigg Talley, United States of America]	Taken into account. The revised assessment of EEI draws upon underlying assessments made in other chapters of the AR6 report for consistency. In addition, the revised ocean heat content change assessment makes use of the more holistic approach described by Palmer et al (2021). This method accounts for both structural and internal/parametric (including sampling) uncertainty resulting in substantially larger uncertainties then using either a single product or simply taking the ensemble spread over number of products. This approach reflects the current state of knowledge of observational uncertainties and represents a more comprehensive treatment compared to previous IPCC reports .
39599	5	29	5	31	Please cite and discuss Delgado-Bonal et al, 2020. Nature Sci. Rep. 10, 922 who consider natural rather anthropogenic causes of earth energy balance. [François Gervais, France]	Taken into account. Paper has been cited as evidence for the importance of cloud and surface albedo feedbacks.
65001	5	29	5	31	The bold sentence is the very same ES conclusion as the first one (bold sentence lines 21-22), isn't it? Why this redundancy? Is the key message here not rather the increased confidence thanks to the closure of the energy budget? [Johannes Quaas, Germany]	Accepted. Agree, reordered as suggested
128821	5	29	5	36	Also 7.2.2. The value for total warming of 0.81 W m-2 is too low. In fact this is the value one gets for the ocean alone. Please see Trenberth, K. E., J. T. Fasullo, K. von Schuckmann and L. Cheng, 2016: Insights into Earth's energy imbalance from multiple sources. J. Climate, 29, 7495-7505. http://dx.doi.org/10.1175/JCLI-D-16-0339.1. and Trenberth, K. E., and Y. Zhang, 2019: Observed inter-hemispheric meridional heat transports and the role of the Indonesian ThroughFlow in the Pacific Ocean. J. Climate, 32, 8523-8536, https://journals.ametsoc.org/doi/pdf/10.1175/JCLI-D-19-0465.1. The latter uses ORAS5 which looks good but only after 2005. [Trigg Talley, United States of America]	Noted. The assessed value for EEI and uncertainty range for 2006-2018 of 0.79 +/- 0.27 is consistent with the Trenberth et al (2016) estimate and uncertainty range for 2005-2014 of 0.9 +/- 0.3.
34907	5	29	5	36	The corresponding rise in GMSL is modest (implying max 25cm rise by 2100, and hence no climate emergency), again questioning whether mitigation is at all to be recommended compared to prudent longer-term adaptation. Please see general comments #6 and #14 above. [Jim O'Brien, Ireland]	Noted. No mitigation statement is made here
20065	5	30	5	30	This sentence is highly ambiguous unless a comma is added following "2018". [philippe waldteufel, France]	Accepted. Comma has been added.
102131	5	30			"for the period 2006-2018 expressed" -- relative to an unperturbed state. "Increasing to *overall* 0.81 +- 0/14Wm2 for the period 2006-2018 relative to xxx xxx" (important as the paragraph above mentiones what happens within that later period [Maria Rugenstein, Germany]	Taken into account. We have been clearer on dates
128823	5	31	5	31	[CONFIDENCE] Sea level budget closure is attained only within large uncertainties associated with the individual components of the sea level budget, such as ocean mass change derived from GRACE data. Uncertainty in OHU from satellite measurements and required geophysical corrections is close to 50%; the budget is therefore hardly closed. Even the central value of OHU varies considerably depending on GRACE solution used and whether geophysical corrections are known and applied correctly. The satellite approach is still not mature enough to estimate OHU reliably (e.g., Meyssignac et al., 2019). Therefore closure of the sea level budget is not a good argument for high confidence. [Trigg Talley, United States of America]	Taken into account. Closure of the sea level budget based upon a consistent set of observations increases confidence in the assessment of changes in ocean heat content and the global energy inventory. For example, were the estimates of ocean heat content change to be substantially lower, the corresponding global sea level budget would no longer be closed. We refer you to Chapter 9 for more details on the assessment of the sea level budget and our responses to your other comments on this topic.
117263	5	32	5	32	Should "ipcc" say "AR5 ??? [Maisa Rojas, Chile]	Accepted. This should have been AR5.
83743	5	32	5	32	"since IPCC" does this mean sinceAR5? [Marvel Kate, United States of America]	Accepted. This should have been AR5.
84833	5	32	5	32	replace " since IPCC" with "since IPCC AR5 is on account of .." [Jayaraman Srinivasan, India]	Accepted. This should have been AR5.
107621	5	32	5	32	assume this should say IPCC AR5? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This should have been AR5.
71035	5	32	5	32	since IPCC -> since AR5 (?) [Yu Kosaka, Japan]	Accepted. This should have been AR5.
17325	5	32	5	32	AR5 missing [David Neubauer, Switzerland]	Accepted. This should have been AR5.
103599	5	32	5	32	Its not IPCC which closed the sea level budget -> its several scientific projects have reported closure of sea level budget, confirming IPCC estimates [Philippe Tulkens, Belgium]	Accepted. Clarification has been made as suggested.
96693	5	32	5	32	….since IPCC AR5…. ? [Nicole Wilke, Germany]	Accepted. This should have been AR5.
27085	5	32	5	32	"AR5" is missing before "IPCC" [Eric Brun, France]	Accepted. This should have been AR5.
69597	5	32	5	32	since IPCC' - you mean, since AR5? [Nicholas Golledge, New Zealand]	Accepted. This should have been AR5.
46061	5	32	5	32	Change "IPCC" to "the AR5". [Twan van Noije, Netherlands]	Accepted. This should have been AR5.
17937	5	32	32	5	. . . since IPCC . . . Did you mean since IPCC AR-5? [Dennis Hartmann, United States of America]	Accepted. This should have been AR5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71957	5	32			Something is wrong here - what does since IPCC mean? [John Church, Australia]	Accepted. This should have been AR5.
28847	5	32			IPCC --> AR5? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
102133	5	32			IPCC --> AR5 [Maria Rugenstein, Germany]	Accepted. This should have been AR5.
27087	5	33	5	33	Is there a specific task of this chapter to provide impacts of a changing Energy budget ? If yes, then the list should be complete (e.g. cryosphere loss, atmospheric warming, ocean warming, sea level rise, ...), if not this should be removed here in the Executive Summary. [Eric Brun, France]	Taken into account. We think it is important to state that the earth system will continue to gain heat even under strong mitigation of greenhouse gas emissions, and at least point out what the first-order implications are (to provide policy-relevant information).
107623	5	34	5	35	presumably there is high confidence SLR will continue beyond 2100 if we do not follow a strong mitigation scenario, in which case the formulation of the sentence isn't clear. Shouldn't the medium confidence be directly attached to a strong mitigation scenario rather than that being an additional clause? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been revised accordingly and confidence is given as high.
46063	5	34	5	35	This seems an understatement: global sea level will continue to rise on a multi-millennial timescale even when anthropogenic CO2 emissions cease completely during the coming decades to centuries. See e.g. Van Breedam, J., Goelzer, H., and Huybrechts, P.: Semi-equilibrated global sea-level change projections for the next 10 000 years, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-20, in review, 2020. [Twan van Noije, Netherlands]	Taken into account. The purpose of the text is to make some causal linkage to the EEI remaining positive, with a focus on the coming centuries (i.e. those where emissions scenarios are specified). We have included links to the relevant sections in Chapter 9.
96695	5	35	5	35	Please explain that the magnitude of the SLR nevertheless depends on the strength of the GHG reductions / the mitigation pathway. [Nicole Wilke, Germany]	Taken into account. We have included links to Chapter 9, where the scenario dependence is highlighted.
34649	5	38	5	39	The first sentence of this key message is very vague. Could some of the content from the third sentence (e.g., dimming from the 1950s-1980s) be placed in the opening sentence instead? [Russell Vose, United States of America]	Accepted. Bullet point has been reworded for clarity.
77323	5	38	5	40	The planetary heat uptake accounts for the entire energy added to or removed from the climate system. It arguably provides a more fundamental measure of global warming than global mean surface temperature, which is influenced by other decadal processes internal to the climate at the air-sea interface. [Emer Griffin, Ireland]	Taken into account. We have reworded along the lines of your suggestion
77325	5	38	5	40	This is very obscure and there is clearer text on this in the chapter. The message is that changes to atmospheric composition can alter the energy balance in a manner that leads to either warming or cooling at a global level [Emer Griffin, Ireland]	.Taken into account. Text clarified but not quite the same concept as ERF
65003	5	38	5	40	I think this ES statement could be much stronger. The two messages of real interest in this paragraph are that (ok, with medium confidence only) surface solar radiation is evidence for aerosol trends, and downward thermal evidence for GHG increases. So why not rather the bold first sentence: “Multidecadal trends in surface solar radaition between the 1950s and 1980s (decreasing, “dimming”) and thereafter (increasing, “brightening”) are consistent with aerosol emission trends (medium confidence), and multidecadal increasing trends in surface downward terrestrial radiation are consistent with increasing atmospheric greenhouse gas concentrations (medium confidence). “ and then in plein text. These trends are neither… [Johannes Quaas, Germany]	Taken into account. We revised this statement but avoided the risk of overinterpretation. The literature is not yet settled enough to unambiguously attribute the decadal changes in surface solar radiation solely to changes in aerosols. The contributions from other factors, such as unforced internal variability of the climate system, cannot be entirely ruled out.
107625	5	39	5	40	"These trends are neither a local phenomenon nor a measurement artefact." This sentence seems redundant. The first sentence essentially clarifies those points, i.e. "widespread locations" implies non-local and "occurred" implies this actually happened and is not an artefact. The way this is written reads a bit defensive and I don't think it is needed. [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, we revised this executive summary statement but still wanted to emphasize the fact that the trends are not spurious and are representative for a larger area.
46065	5	43	5	43	Better to change "aerosol forcing" to "aerosol loads" or "aerosol concentrations" since scattering and absorption by aerosols have opposite effects in (TOA) forcing but work in the same direction for dimming and brightening. [Twan van Noije, Netherlands]	Accepted.
46067	5	44	5	44	Please clarify that the assessment of "medium confidence" is based on direct observations only, and that the confidence level will be higher if other lines of evidence (consistency with other observables, physical reasoning, results from global models) are taken into account. [Twan van Noije, Netherlands]	Accepted. Confidence statement has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99053	5	44	5	47	This statement seems inherently contrary to physics as stated here. Theoretically, there is no way that these fluxes could not have increased. Now, observationally, measuring this is clearly difficult to do: having enough instruments out to actually assemble a globally integrated value, and doing so in a single or small number of locations is difficult due to the variability of the weather. I am assuming that this is why there is a statement of only medium confidence--and if this is the case this needs to be explained. To correct the situation, move the last six words of the sentence to the start of the sentence, so setting the stage rather than allow the reader to have to think about why there is only medium confidence in a result based on pure physics. [Michael MacCracken, United States of America]	Accepted. Confidence statement has been revised.
128825	5	46	5	46	With "other energy fluxes" are the authors referring to latent and sensible heat fluxes? [Trigg Talley, United States of America]	Accepted. Clarification has been made.
83107	5	46	5	47	Does the "low confidence" also applies to the LW_up part of the surface energy budget? Also relevant for section 7.2.2. [Terje Berntsen, Norway]	Accepted. changes in surface LW_up are better known than with "low confidence", even though they are not measured on an worldwide basis, since they predominantly depend on the surface temperature changes which are well known. We changed the sentence to "The downward and upward thermal radiation at the surface has increased in recent decades, in line with increased greenhouse gas concentrations and associated surface and atmospheric warming and moistening (medium confidence)"
28849	5	46			warming and moistening [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
102135	5	46			Unclear at this piont what "other energy flux changes" are referring to [Maria Rugenstein, Germany]	Accepted. Clarification has been made.
96697	5	52	5	52	Also a general point for this chapter and in particular the ES: The achievements of AR5 should not be the centre of executive summary statements. The headline statement should be primarily a collection of the most important and up-to-date assessment results. Only, secondarily it is interesting to state any major changes compared to AR5. [Nicole Wilke, Germany]	Rejected. The chapter is indeed focusing on updates since AR5.
77327	5	52	5	54	This could be shortened to ERF is a scientifically robust measure of changes to the Earth's energy balance. [Emer Griffin, Ireland]	.Taken into account. We revise using some of the suggested wording: robust
107627	5	52	6	29	All the ERF bullets just say high confidence at the end of what are sometimes long paragraphs with multiple points. It would be helpful if the confidence language could be more integrated with the key statements in the bullets and subbullet paragraphs, as is done in Earth's energy balance section of the ES. To give just one specific example, on P5 :54-55 it says climate models' ERF s (for CO2 - should be specified) like with 11% of the best estimate (high confidence). Presumably this is a fact because we can diagnose the models perfectly, in which case does the high confidence relate to the best estimate of the CO2 ERF of 4 Wm-2? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Confidence language has been refined
20067	5	53	5	53	Can one improve the accuracy of the "the ERF for a doubling" statement? The ERF induced by? Created by? Generated by?... [philippe waldteufel, France]	Taken into account. Text has been clarified.
81765	5	53	5	54	Sentence not clear [Karina von Schuckmann, France]	Taken into account. Text has been clarified.
65715	5	53	5	54	Suggest changing to: "The ERF for a doubling of carbon dioxide since the preindustrial era is ..." [Kushla Munro, Australia]	Taken into account. Text has been clarified.
27089	5	53	5	54	The sentence s unclear [Eric Brun, France]	Taken into account. Text has been clarified.
89171	5	54	5	55	The CMIP6 models have a smaller range on ERF due to 2xCO2 than CMIP5, but not necessarily that radiative transfer is the main reason. I can not find in the chapter a discussion or documentation that any of the CMIP models have improved their radiation scheme. [Gunnar Myhre, Norway]	Not applicable: This sentence has been removed.
99055	5	54	5	55	There needs to be an explanation for how the "assessed best estimate" is determined if not from models or the statement seems like an unclosed loop. Is the best estimate from paleoclimatic analysis, what the value would be for the best fit to observed changes in the past, etc., and in that the "assessed best estimate" must have an uncertainty, is the clmate models' representation within the bounds of the assessed best estimate. Also, the phrase "climate models radiative transfer reprsentation" does not make much sense unless all models have the same representation, and that seems very unlikely to be the case, so is this referring to the mean of some models? This might be cleared up by perhaps saying "The radiative transfer representations of climate models have improved since AR5, and the mean values of their ERFs lies within 11% of the best estimate, which itself has an uncertainty of xx%" [Michael MacCracken, United States of America]	Not applicable: This sentence has been removed.
20391	5	55	5	55	Anybody will wonder whether the mentioned 4 W/m2 is the so called assessed best estimate, and further wonder how the 11% figure is related to the +-0.5 W/m2 bracket mentioned just before. [philippe waldteufel, France]	Not applicable: This sentence has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31683	5	55	5	55	Not clear if the "they have ERFs" refers specifically to the doubling of CO2 mentioned in the previous sentence, or the overall historical change in ERF. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This sentence has been removed.
102137	5	55			"of the assessed bast estimate" --> estimate from what? Is that referring to the 4Wm2? But this comes from the same models? Unclear to me what is meant here. [Maria Rugenstein, Germany]	Not applicable: This sentence has been removed.
39601	6	1	6	26	Based on infrared spectra of the atmosphere, http://dx.doi.org/10.1155/2013/503727 concludes to a radiative forcing of 2.6 W/m2 at doubled CO2 concentration. This finding, as well as infrared spectra which are missing in the entire report should be mentioned and discussed because they contradict the highest values of both ECS and TCR. [François Gervais, France]	Rejected. The study cited is not a comprehensive radiative transfer model accounting for the necessary atmospheric processes.
15525	6	3	6	3	Re: 2.53 W m-2(1.58 to 3.34 Wm-2). The likely range shown in the main text is [1.56 to 3.32] (P.49-50, Table 7.8). Please check and revise as appropriate. [SAI MING LEE, China]	Taken into account: These tables have been revised and checked for consistency.
65717	6	3	6	3	Suggest changing first sentence to: "The net anthropogenic ERF over the industrial era (1750-2018) was 2.53 W m-2". [Kushla Munro, Australia]	Accepted.
40843	6	3	6	4	SPM B1.5 says The total effective radiative forcing (ERF) from increases in greenhouse gases from 1750 to 2018 is 3.63 W m-2 (3.27 to 3.97 Wm-2), 15% greater than the 2011 estimate in AR5 due to increases in atmospheric concentrations since 2011 and revisions to forcing estimates. Check the consistency [TSU WGI, France]	Taken into account: The halocarbon ERFs have been revised.
77331	6	3	6	7	Is this the current ERF (in 2018) relative to 1750? Or the average over that period? [Emer Griffin, Ireland]	Taken into account. Bullet point has been reworded for clarity.
77333	6	3	6	7	The message seems to be that increased understanding and changes to the atmospheric composition, in particular increases in atmospheric CO2 concentrations, means that the ERF in 2018 is 2.53W/m2 which is an 11% increase over the AR5 estimate. Is this correct? [Emer Griffin, Ireland]	Noted: This comment has been noted. No suggestions made.
22115	6	3	6	7	Might a reader look at this number and compare to the numbers given in p.5 ln 29-36 and conclude that something doesn't add up? Is some care required here to explain the difference? Is the first a rate relative to a modern baseline whereas the second is relative to an older baseline? Is there another explanation? Is such an explanation necessary? [Peter Thorne, Ireland]	Rejected: The "energy imbalance" includes radiative responses to the warming, whereas the ERF explicitly excludes any warming responses.
114569	6	3	6	7	If possible, you may consdier splitting the effect of changes in conc and revised forcing eff [Jan Fuglestvedt, Norway]	Taken into account: The ES statements have been revised to make it clearer how much is due to concentration increases.
65719	6	3	6	7	Suggest rephrasing to break down the 11% revision to that portion due the upward revision of radiative efficiency, and that due to the continuing addition of radiatively-active gases since 2011. The general public or media may misinterpret this as stating 'human activity has added the entire 11%'. Suggest similar clarification is needed for revised aerosol forcing. [Kushla Munro, Australia]	Taken into account: The ES statements have been revised to make it clearer how much is due to concentration increases.
16141	6	3	6	9	There seem to be one too many significant figures in these numbers given the uncertainty. [Steven Sherwood, Australia]	Accepted. We have reduced the significant figures.
102149	6	3	6	19	To me this is hard to follow [Maria Rugenstein, Germany]	Taken into account. Bullet point has been reworded for clarity.
20393	6	3	6	29	The dutiful reader will check that 3,63 (line 9) minus 1,1 (line 21) equals 2,53 (line 3). So far so good. Hence the impact of chemical adjustments following CH4 increase is included in the 1.1 W/m2 aerosol contribution. Provided this interpretation is the correct one, all is well. [philippe waldteufel, France]	Taken into account: More details have been given of the different components
89173	6	4	6	6	Useful to add the contribution from increase in the WMGHG concentration [Gunnar Myhre, Norway]	Taken into account: The ES statements have been revised to make it clearer how much is due to concentration increases.
77335	6	4	6	7	The material on CO2, CH4 and N20 can be included here with the message that gains on CFC have been offset by replacement gases. Estimates of aerosol cooling could also be included. [Emer Griffin, Ireland]	Rejected. Folding all new ERF estimates into a single ES bullet point would not improve the readability of the ES.
77337	6	4	6	7	terms like shortwave forcing are quite obscure. Could warming be used? [Emer Griffin, Ireland]	Rejected. There is no mention of shortwave forcing on the lines referred to.
102139	6	4			"11% increase" because of the timeframe or other things/reference frames etc. [Maria Rugenstein, Germany]	Taken into account: The ES statements have been revised to make it clearer how much is due to concentration increases.
102141	6	6			"15%" 11? [Maria Rugenstein, Germany]	Noted: The 15% refers to GHG-only, the 11% to GHG plus aerosols
102143	6	6			"offset" for what? [Maria Rugenstein, Germany]	Taken into account: The ES statements have been revised to make it clearer how much is due to concentration increases.
77339	6	9	6	11	It should be clear that + ERF is warming and that the EFR data are current ERF values; the wording could imply an average over the industrial era. [Emer Griffin, Ireland]	Taken into account. Text has been clarified in this respect.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
55063	6	9	6	13	Two comments: 1. We would like to see the ERF for CO2 added to this paragraph (in addition to simply stating that CO2 contributes the largest part of this forcing from well-mixed GHGs); 2. Is this conclusion about an increase in estimated shortwave forcing from methane more important to highlight in the excutive summary than the conclusion of an overall upwards revision to the methane ERF relative to the AR5? [Nancy Hamzawi, Canada]	Accepted. Agree with both points - upward revision is now highlighted
66795	6	9	6	13	Include further breakdown of what contributes the most; WMGHGs contribute the most and CO2 the largest impact, but what is the breakdown in percentages of CO2 and CH4 (and the others) to the total? [Kristin Campbell, United States of America]	Taken into account: This breakdown has been added
69885	6	9	6	13	While well-mixed GHGs may produced the largest contribution to ERF, the more useful insight is that the non-CO2 SLCPs can avoid more warming going forward well past 2050. Include further breakdown of what contributes the most; WMGHGs contribute the most and CO2 the largest impact, explaining breakdown in percentages of CO2 and CH4 (and the others) to the total. [Gabrielle Dreyfus, United States of America]	Rejected. This bullet point refers to historical GHG ERF, and breaks it down into contributions from different constituents. Generally, the importance of SLCPs is Ch. 6 territory, and projections are covered in Ch. 4.
68351	6	9	6	13	While well-mixed GHGs may produced the largest contribution to ERF, the more useful insight is that the non-CO2 SLCPs can avoid more warming going forward well past 2050. Include further breakdown of what contributes the most; WMGHGs contribute the most and CO2 the largest impact, explaining breakdown in percentages of CO2 and CH4 (and the others) to the total. [Durwood Zaelke, United States of America]	Rejected. This bullet point refers to historical GHG ERF, and breaks it down into contributions from different constituents. Generally, the importance of SLCPs is Ch. 6 territory, and projections are covered in Ch. 4.
31685	6	10	6	11	Not clear if SWV refers just to methane oxidation or includes other processes related to the adjustments. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been clarified that this comes from oxidation of methane
65005	6	11	6	12	It would be beneficial to note the share of CO2 in the forcing, and the share of CH4, too (since these are just two numbers, so with little extra space would quantify the statement). [Johannes Quaas, Germany]	Taken into account: This addition has been considered in the ES revision
77341	6	11	6	13	This could mention the relative contributions of the main GHGs rather than mention CO2 and shortwave forcing by methane. [Emer Griffin, Ireland]	Taken into account: This addition has been considered in the ES revision
102145	6	11			"largest part of this" --> quantify [Maria Rugenstein, Germany]	Accepted. Agree, CO2 effect quantified
65721	6	12	6	12	Suggest clarification since this is a crucial point of communication with a high risk of being misreported in the media. Suggest changing to: "There has also been an increase in the estimated shortwave forcing from methane." Explain here to what extent this is due to a revised estimate of the radiative efficiency of methane versus increased methane concentration in the atmosphere. Section 7.3.2 notes "historical ERF estimate from CH4 is revised upwards from 0.48 ± 0.10 W m-2 in AR5 to 0.54 ± 0.11 W m-2" . Suggest this revision be cited in the Executive Summary as well as in the SPM. [Kushla Munro, Australia]	Accepted: This has been reworded.
31687	6	12	6	12	This is ambiguous. You dont, I think, mean it has increased, because methane concentrations have increased, but it has increased because it had been previously neglected.? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been reworded.
114567	6	12	6	13	But how strong basis is there for giving this confidence level for methane SW forcing? [Jan Fuglestvedt, Norway]	Rejected: This confidence level has been justified in the main text
102147	6	12			"There has alsoo been" --> precsion! AR6 estimates (?) [Maria Rugenstein, Germany]	Accepted. Quantification has been added.
77343	6	15	6	16	The concept of reactive well mixed GHGs is quite new, and can be confused with very reactive gases. The term chemical adjustments is quite obscure. [Emer Griffin, Ireland]	Not applicable: This paragraph has been removed.
77345	6	15	6	16	Could this be stated the decomposition of a number of well mixed GHGs(CH4, N2O) and certain halocarbons causes changes to aerosol and ozone levels in the atmosphere. [Emer Griffin, Ireland]	Not applicable: This paragraph has been removed.
77347	6	16	6	19	The fact that halocarbons cause upper atmospheric ozone loss which is a key policy issue should be mentioned here as well as indicating that this may mean their climate impacts could be zero. [Emer Griffin, Ireland]	Not applicable: This paragraph has been removed.
15981	6	17	6	17	The forcing of methane, quoted as 0.45 W/m2 should be put in context with CO2 forcing. Using equations in AR5, then methane contributes 22% of the total forcing from CO2, and this proportion is likely to increase. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This paragraph has been removed.
100449	6	17	6	19	This range from 0.0-0.16 W m-2 for net ERF due to halocarbons seems too small - please see my comment to page 51 (Section 7.3.5) [Øivind Hodnebrog, Norway]	Not applicable: This paragraph has been removed.
102151	6	17			"contribution" to what? [Maria Rugenstein, Germany]	Not applicable: This paragraph has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31689	6	18	6	18	Ambiguous. Are you referring to all halocarbons or just ODSs? The sentence wording is not great "the direct ERF due to their effect on ozone" can be read in two ways. I don't think you mean the direct ERF is due to the ozone effect. As I note later, the nomenclature for halocarbons/halogens, is often a bit confusing throughout the chapter. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This paragraph has been removed.
99057	6	18	6	18	This needs to say "stratospheric ozone depletion" [Michael MacCracken, United States of America]	Not applicable: This paragraph has been removed.
68353	6	21	6	29	Add that there are aerosols, specifically black carbon and brown carbon, that add warming (and a significant amount, according to Bond et al 2013) as part of this calculation; also the impact of BC deposition on snow/ice surfaces. The goal should be to ensure that reductions of black and brown carbon occur faster than reductions of the cooling sulfates. Qian Y., et al. (2014) Light-absorbing Particles in Snow and Ice: Measurement and Modeling of Climatic and Hydrological impact, ADVANCES IN ATMOSPHERIC SCIENCES 32:64–91; Arctic Monitoring and Assessment Programme (AMAP) (2017) ADAPTATION ACTIONS FOR A CHANGING ARCTIC: PERSPECTIVES FROM THE BARENTS AREA; International Energy Agency (IEA) (2016) WORLD ENERGY OUTLOOK SPECIAL REPORT: ENERGY AND AIR POLLUTION; World Bank & International Cryosphere Climate Initiative (2013) ON THIN ICE: HOW CUTTING POLLUTION CAN SLOW WARMING AND SAVE LIVES; Ramanthan V. & Xu Y. (2010) The Copenhagen Accord for limiting global warming: Criteria, constraints, and available avenues, Proc. Nat'l. Acad. Sci. 107(18):8055–8062. [Durwood Zaelke, United States of America]	Rejected. The per-species aerosol forcings are assessed in Ch. 6. Ch. 7 assesses the overall aerosol forcing, which is robustly negative. Further, the Bond et al study is now somewhat outdated, with multiple studies showing that the positive forcing estimate was strongly exaggerated.
68355	6	21	6	29	While not GHGs, black and brown carbon aerosols also are important climate forcers and comes from some similar sources that should be considered part of this discussion. While organic carbon is reflective, the warming effect of black and brown carbon components overall amplify warming. Black carbon is a powerful climate-warming aerosol that directly warms the atmosphere by absorbing solar radiation and indirectly by darkening snow and ice surfaces. Nearly 90% of black carbon emissions come from residential solid fuels, diesel engines, and residential coal; the rest of the emissions come from aviation, shipping, and flaring. Reducing black carbon is especially beneficial for the Arctic because black carbon not only warms the atmosphere but also facilitates additional warming. Once black carbon is deposited on the snow and ice, it reduces the reflectivity (albedo) and absorbs extra solar radiation, which leads to further melting than pristine snow and ice. Since 1890, black carbon has contributed about 0.5–1.4 ºC of warming to the Arctic. Bond T. C., et al. (2013) Bounding the role of black carbon in the climate system: A scientific assessment, J. GEOPHYSICAL RESEARCH–ATMOSPHERES 118(11):5380–5552; Myhre G., et al. (2013) CHAPTER 8: ANTHROPOGENIC AND NATURAL RADIATIVE FORCING, 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, Table 8.A.6; Qian Y., et al. (2014) Light-absorbing Particles in Snow and Ice: Measurement and Modeling of Climatic and Hydrological impact, ADVANCES IN ATMOSPHERIC SCIENCES 32:64–91; Arctic Monitoring and Assessment Programme (AMAP) (2017) ADAPTATION ACTIONS FOR A CHANGING ARCTIC: PERSPECTIVES FROM THE BARENTS AREA; International Energy Agency (IEA) (2016) WORLD ENERGY OUTLOOK SPECIAL REPORT: ENERGY AND AIR POLLUTION; World Bank & International Cryosphere Climate Initiative (2013) ON THIN ICE: HOW CUTTING POLLUTION CAN SLOW WARMING AND SAVE LIVES.; Shindell D. & Faluvegi G. (2009) Climate response to regional radiative forcing during the twentieth century, Nature Geoscience 2:294–300; Feng Y., et al. (2013) Brown carbon: a significant atmospheric absorber of solar radiation?, ATMOS. CHEM. PHYSICS 13:8607–8621. [Durwood Zaelke, United States of America]	Rejected. The per-species aerosol forcings are assessed in Ch. 6. Ch. 7 assesses the overall aerosol forcing, which is robustly negative. Further, the Bond et al study is now somewhat outdated, with multiple studies showing that the positive forcing estimate was strongly exaggerated.
77349	6	21	6	29	This is quite obscure can the concept that aerosols reflect sunlight back to space and also cause certain clouds to do as well resulting in a cooling effect be stated. Also -ERF should be highlighted as cooling. [Emer Griffin, Ireland]	Accepted. The bullet point has been reworded for clarity as suggested.
77351	6	21	6	29	Could aerosol impacts on clouds and cloud cover and direct impacts aerosol be used? [Emer Griffin, Ireland]	Accepted. The bullet point has been reworded for clarity as suggested.
34651	6	21	6	29	I think you can drop the ERFaci and ERFari acronyms from this key message and just use the phrases "aerosol-cloud interactions" and "aerosol-radiation interactions." [Russell Vose, United States of America]	Rejected. The preference is to keep this acronyms in the ES to make it less wordy.
84835	6	21	6	29	There should be some comment on the role of Black carbon aerosols [Jayaraman Srinivasan, India]	Rejected. The per-species aerosol forcings are assessed in Ch. 6. Ch. 7 assesses the overall aerosol forcing, which is robustly negative.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69841	6	21	6	29	Differentiate between warming and cooling aerosols, as different mitigation measures will affect their relative loading, and if cooling aerosols are removed more quickly than warming aerosols, there will be a net warming effect. Ramanthan V. & Xu Y. (2010) The Copenhagen Accord for limiting global warming: Criteria, constraints, and available avenues, Proc. Nat'l. Acad. Sci. 107(18):8055–8062. Consider also the 0.5 to 1.4°C warming in the Arctic due to black carbon and radiative forcing estimates in Bond T. C., et al. (2013) Bounding the role of black carbon in the climate system: A scientific assessment, J. GEOPHYSICAL RESEARCH–ATMOSPHERES 118(11):5380–5552 [Gabrielle Dreyfus, United States of America]	Rejected. The per-species aerosol forcings are assessed in Ch. 6. Ch. 7 assesses the overall aerosol forcing, which is robustly negative. Further, the Bond et al study is now somewhat outdated, with multiple studies showing that the positive forcing estimate was strongly exaggerated.
66797	6	21	6	29	Add that there are aerosols, specifically black carbon and brown carbon, that add warming (and a significant amount, according to Bond et al 2013) as part of this calculation. Also black carbon directly warms the atmosphere by absorbing solar radiation and indirectly by darkening snow and ice surfaces. The goal should be to ensure that reductions of black and brown carbon—in addition to mitigation of other SLCPs that may arise from similar sources—occur faster than reductions of the cooling sulfates. While organic carbon is reflective, the warming effect of black and brown carbon components overall amplify warming. Nearly 90% of black carbon emissions come from residential solid fuels, diesel engines, and residential coal; the rest of the emissions come from aviation, shipping, and flaring. Reducing black carbon is especially beneficial for the Arctic because black carbon not only warms the atmosphere but also facilitates additional warming. Once black carbon is deposited on the snow and ice, it reduces the reflectivity (albedo) and absorbs extra solar radiation, which leads to further melting than pristine snow and ice. Since 1890, black carbon has contributed about 0.5–1.4 ºC of warming to the Arctic. Bond T. C., et al. (2013) Bounding the role of black carbon in the climate system: A scientific assessment, J. GEOPHYSICAL RESEARCH–ATMOSPHERES 118(11):5380–5552; Qian Y., et al. (2014) Light-absorbing Particles in Snow and Ice: Measurement and Modeling of Climatic and Hydrological impact, ADVANCES IN ATMOSPHERIC SCIENCES 32:64–91; Arctic Monitoring and Assessment Programme (AMAP) (2017) ADAPTATION ACTIONS FOR A CHANGING ARCTIC: PERSPECTIVES FROM THE BARENTS AREA; International Energy Agency (IEA) (2016) WORLD ENERGY OUTLOOK SPECIAL REPORT: ENERGY AND AIR POLLUTION; World Bank & International Cryosphere Climate Initiative (2013) ON THIN ICE: HOW CUTTING POLLUTION CAN SLOW WARMING AND SAVE LIVES. Myhre G., et al. (2013) CHAPTER 8: ANTHROPOGENIC AND NATURAL RADIATIVE FORCING, 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, Table 8.A.6; Shindell D. & Faluvegi G. (2009) Climate response to regional radiative forcing during the twentieth century, Nature Geoscience 2:294–300; Feng Y., et al. (2013) Brown carbon: a significant atmospheric absorber of solar radiation?, ATMOS. CHEM. PHYSICS 13:8607–8621. [Kristin Campbell, United States of America]	Rejected. The per-species aerosol forcings are assessed in Ch. 6. Ch. 7 assesses the overall aerosol forcing, which is robustly negative. Further, the Bond et al study is now somewhat outdated, with multiple studies showing that the positive forcing estimate was strongly exaggerated.
102153	6	23			"with the remainder due to aerosol-radiation interation" i.e. delete "the forcing assiciated with [Maria Rugenstein, Germany]	Accepted
16143	6	24			I would not consider it a "marked" reduction; the range in AR5 was -1.9 to -0.1, so the span has only been reduced by 10%. It is a reduction. Moreover this reduction is contradictory to the findings of a detailed WCRP report just last year which expanded the AR5 range. This begs the question of how meaningful small changes to this range are and whether they deserve to be highlighted. [Steven Sherwood, Australia]	Accepted. Sentence has been reworded. Note that the WCRP report took a different approach and did not have access to some of the new papers on the topic.
22117	6	27	6	27	in contrast to AR5. Compared to AR5 is a bit of a mouthful and could perhaps be rephrased to avoid this. [Peter Thorne, Ireland]	Accepted. Bullet point has been reworded.
31691	6	28	6	29	Do you mean there is a high confidence in the doubling (I think we are certain that the present ERFaci estimate is double the AR5 estimate!) or high confidence that the real forcing is in the given range? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Bullet point has been reworded for clarity.
86799	6	32	6	32	Please consider to begin this paragraph with a text about what kind of climate feedbacks that are assessed and their contributions in the order of magnitude. [Oyvind Christophersen, Norway]	Taken into account. Introduction improved
83375	6	32	7	26	I was sur[rised not to see discussion of the important ice-albedo feedback mechanism vis a vis sea ice loss, and the role of sea ice and its snow cover in the surface energy budget. [Robert Massom, Australia]	Taken into account. Text added to and cross references added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68877	6	32			Climate Feedbacks: Evidence from paleoclimate observations and models provide independent evidence to support the finding that climate sensitivity is non-linear, likely increasing with temperature. An ES statement is needed to support the paleo key message about the long-term effects of warming and to advance this key message to the TS to support the findings there. This information is covered in section 7.4.3 in reference to state-dependence of feedbacks, and in section 7.4.2.6 in reference to long-term feedbacks associated with ice sheets. [Darrell Kaufman, United States of America]	Accepted - The E.S. now includes a statement about non-linearity of feedbacks, and a statement about long-timescale warming associated with ice sheets.
96699	6	34	6	34	The headline statement should not start with repeating an AR5 finding, but with the new assessment result. E.g. "Net cloud feedback amplifies global warming (i.e. positive feedback) (high confidence)". Then, add the new quantitative result central value and range. Then, highlight and explain new achievements since AR5. [Nicole Wilke, Germany]	Accepted.
36865	6	34	6	36	More cloud means higher temperatures? Who do you think you are kidding? [John McLean, Australia]	Rejected. There is no evidence that cloud amount is increasing.
128827	6	34	6	36	[CONFIDENCE] Not convinced of the marked progress stated here. One factor in the strengthening the high confidence revolves around the role of high (tropical) clouds that are conveyed to the reader as understood with high confidence. This is a problematic over simplification of reality and there is more uncertainty than acknowledged. [Trigg Talley, United States of America]	Taken into account. We emphasized that the improved understanding of the main low-cloud feedback, which had been a primary source of uncertainty in the cloud feedback before AR5, increased the level of confidence. It is correct that the high cloud amount feedback still contain uncertainty, but the assessed sign is negative, so it does not affect the high confidence of the positive feedback (we did not stress that the number is assessed with high confidence).
99059	6	34	6	36	This expression of the finding has a lot of jargon that it would be helpful to the reader to explain. I would urge making the first sentence more readily understandable and extractable as a quote, and lightly edit the second one, saying: "AR5 concluded with medium confidence that the net effect of changes in cloud amount, type, and distribution would be to amplify the warming caused by the increasing concentrations of greenhouse gases. Major advances in the understanding of cloud processes now allow this finding, generally referred to as 'net cloud feedback', to be stated with high confidence and with a halving of the uncertainty range." [Michael MacCracken, United States of America]	Taken into account. The ES statement has been carefully revised and it is now written in a more plain language than in SOD.
107633	6	34	6	42	Is the high confidence in positive net cloud feedback at odds with the very likely range spanning 0? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We added explanation of why the negative value can be ruled out.
71039	6	34	6	42	I am afraid that the high confidence assessment that the net cloud feedback is positive but the very likely range includes negative values may be confusing. [Yu Kosaka, Japan]	Taken into account. We have deleted the very likely range to avoid confusion.
69599	6	34	6	42	perhaps explain v briefly what the 'cloud feedback' is? [Nicholas Golledge, New Zealand]	Taken into account. Rephrased.
90241	6	34	47	44	A key gap in the section on aerosol forcing is that there is little discussion regarding the uncertainty of preindustrial aerosol levels. This issue has been recognized for some time, especially in regard to cloud albedo forcing, which is nonlinear with respect to the aerosol burden. For example, Schmidt et al. (2012) showed that the effect of volcanic aerosol on cloud albedo forcing results in -1.0 Wm-2 cooling in a pristine environment, but only about half that in the polluted present-day environment, when more aerosols are available to compete for the available water vapor. Carslaw et al. (2013) also emphasized the “large contribution” of natural aerosols in the calculation of aerosol forcing. These authors performed a suite of sensitivity simulations and found that 45% of the variance of aerosol forcing in their simulations arose from uncertainties in the natural emissions of aerosols or aerosol precursors. Such emissions included those of volcanic sulfur dioxide, marine dimethylsulfide, biogenic volatile organic carbon, biomass burning and sea spray. Carslaw, K. S. et al. (2013), Large contribution of natural aerosols to uncertainty in indirect forcing, Nature, 503(7474), 67–71, doi:10.1038/nature12674. Schmidt, A., K. S. Carslaw, G. W. Mann, A. Rap, K. J. Pringle, D. V. Spracklen, M. Wilson, and P. M. Forster (2012), Importance of tropospheric volcanic aerosol for indirect radiative forcing of climate, Atmos. Chem. Phys., 12(16), 7321–7339, doi:10.5194/acp-12-7321-2012. [Loretta Mickley, United States of America]	Taken into account. A brief discussion about pre-industrial aerosol levels as an important source of uncertainty for aerosol ERF has now been added to section 7.3, citing some of the suggested papers . However, this is not viewed as central information for the ES.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
90243	6	34	47	44	More on uncertainty in preindustrial aerosol levels…. One recent study has revisited estimates of aerosol forcing since the preindustrial era in light of the observed levels of black carbon (BC) preserved in ice cores and charcoal records. This study relied on a global fire model that represents the human influence on fire by accounting for the effects of managed burning carried out by agricultural and pastoral societies as well as the effects of land use change, including landscape fragmentation (Pfeiffer et al., 2013). Using the emissions from this fire model, Hamilton et al. (2018) determined that the global cloud albedo forcing since the preindustrial era could be 35% less negative than that estimated using fire emissions recommended for AR6 (van Marle et al., 2017). Pfeiffer, M., A. Spessa, and J. O. Kaplan (2013), A model for global biomass burning in preindustrial time: LPJ-LMfire (v1.0), Geosci. Model Dev., 6(3), 643–685, doi:10.5194/gmd-6-643-2013. Hamilton, D.S., et al. (2018), Reassessment of pre-industrial fire emissions strongly affects anthropogenic aerosol forcing, Nature Communications, 9, van Marle, M. J. E. et al. (2017), Historic global biomass burning emissions for CMIP6 (BB4CMIP) based on merging satellite observations with proxies and fire models (1750–2015), Geosci. Model Dev., 10(9), 3329–3357, doi:10.5194/gmd-10-3329-2017. [Loretta Mickley, United States of America]	Taken into account. A brief discussion about pre-industrial aerosol levels as an important source of uncertainty for aerosol ERF has now been added to section 7.3, citing some of the suggested papers . However, this is not viewed as central information for the ES.
90245	6	34	47	44	More on uncertainty in preindustrial aerosol levels… Liu et al. (in review) presented evidence from records of 14 Antarctic ice cores and one central Andean ice core that suggest that historical fire activity in the Southern Hemisphere (SH) significantly exceeded present-day levels. These authors further showed that using the improved biomass burning emissions from Pfeiffer et al. (2013) led to aerosol forcing (direct radiative forcing + cloud albedo forcing) in the SH of -0.35 Wm-2, or about 40% less negative than that calculated with the AR6 recommended fire emissions (van Marle et al., 2017). This study suggests that the cooling effect of increasing aerosols from anthropogenic sources in the SH over the last century has largely been compensated by the decreasing trend in SH fire emissions. Liu, P., J. O. Kaplan, L. J. Mickley, Y. Li, N. J. Chellman, M. M. Arienzo, J. K. Kodros, J. R. Pierce, M. Sigl, J. Freitag, R. Mulvaney, M. A. J. Curran, and J. R. McConnell (in review), Improved estimates of preindustrial biomass burning reduce the magnitude of aerosol climate forcing in the Southern Hemisphere. This paper was first submitted to Nature Geoscience on 31 December 2019. The paper has now been revised and submitted to Scientific Advances. [Loretta Mickley, United States of America]	Taken into account. A brief discussion about pre-industrial aerosol levels as an important source of uncertainty for aerosol ERF has now been added to section 7.3, citing some of the suggested papers . However, this is not viewed as central information for the ES.
102155	6	34			This paragraph starts with AR5, others don't [Maria Rugenstein, Germany]	Taken into account. Paragraph has been reworded.
71037	6	35	6	35	leads -> lead [Yu Kosaka, Japan]	Accepted.
46069	6	35	6	36	I am confused about the statement that there is high confidence that the net cloud feedback is positive. Shouldn't the statement rather be that there is high confidence that the net cloud feedback is likely positive? [Twan van Noije, Netherlands]	Rejected. In the IPCC language, the confidence level and likely statement are not used altogether.
41487	6	36	6	36	mention high latitude cloud phase feedbacks as well? [Andrew Gettelman, United States of America]	Rejected. The ES statement should be short enough, so we could not mention individual cloud feedbacks here except for the low-cloud feedback that has been assessed with a higher confidence than AR5.
4647	6	36	6	36	"leads to a ... and halved its uncertainty range" should be changed to "leads to a ... and a halving of its uncertainty range" [Balasubramanya Nadiga, United States of America]	Accepted.
22119	6	37	6	37	GCMs or ESMs? Other chapters are generally using ESMs. [Peter Thorne, Ireland]	.Taken into account. ESMs used for consistency
99061	6	39	6	40	What is the difference between "total cloud feedback" and "net cloud feedback" and then "cloud feedback"? Using three different terms for what I think is the same thing is going to be confusing. [Michael MacCracken, United States of America]	Taken into account. We have used a common word of 'net cloud feedback'.
27091	6	40	6	40	is the range given here a likely or a very likely range? [Eric Brun, France]	Taken into account. This is now specified.
9697	6	40	6	40	is range likely or very likely? [Olivier Boucher, France]	Taken into account. This is now specified.
107631	6	40	6	42	Does the high confidence relate to the statement that CMIP6 has a more positive median cloud feedback? Presumably that is just a fact, as we can diagnose the models perfectly, so does not warrant a confidence statement [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. No, the level of confidence was not derived from a simple fact that the CMIP6 models have a more positive median feedback. We have revised the text to make it clearer.
67773	6	40	7	1	"ECS that is substantially higher than has been traditionally inferred from warming over the historical record (high confidence)." How can there be 'high confidence' when papers such as Tokarska2020 show the new models do not agree with paleo data? [Stephen Gaalema, United States of America]	Rejected. The reviewer seems to refer to a paper that does not deal with paleo climate evidence, so it is unclear what is meant. Nevertheless, the summary bullet point in Chapter 7 is relative to earlier energy-balance estimates of ECS based on historical warming which did not account for pattern effects, e.g. Otto et al. 2013.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
102157	6	40			"CMIP5 and CMIP6" --> other paragraphs don't make that distinction [Maria Rugenstein, Germany]	Rejected. This is viewed as particularly relevant information for this bullet point, given the change in feedback/ECS from CMIP5 to CMIP6.
77353	6	44	6	44	Projected to become rather than will become. [Emer Griffin, Ireland]	Accepted. Text has been revised accordingly.
84837	6	44	6	44	This comment is not clear. Are you talking about total feedback? [Jayaraman Srinivasan, India]	Taken into account. Text clarified
96701	6	44	6	44	"Less negative" radiative feedback is not "more amplifying", but rather "less dampening" (or similar). Please reword accordingly. [Nicole Wilke, Germany]	Taken into account. Text clarified
99065	6	44	6	44	What are the "radiative feedbacks" being referred to (and on line 54)? A bit of explanation is needed so not just really into the details will be able to read this summary. [Michael MacCracken, United States of America]	Taken into account. Report clarified
15397	6	44	6	46	In this context, TCR is more policy-relevant than ECS and should be elaborated considering the pattern effect of ocean heat uptake on TCR. [Junichi Tsutsui, Japan]	Rejected. The summary bullet point is devoted to a new finding regarding ECS, so there is no point in elaborating TCR.
10719	6	44			"...will become..." - sounds very confident! Is there no uncertainty about this? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised accordingly.
99063	6	45	6	45	The reader will want to be reminded what ECS is? [Michael MacCracken, United States of America]	Accepted. The acronym is now spelled out.
68879	6	51	6	53	I could not find information in the chapter text that explains how paleoclimate reconstructions support the conclusion about these future ocean warming patterns. Please strengthen this point so that it addresses the paleo key message about the prominent large-scale recurrent spatial patterns associated with past global warming, which is needed to support the findings in the TS. [Darrell Kaufman, United States of America]	Accepted - paleo data is used to inform polar amplification and Pacific zonal gradients.
107635	6	55	6	55	why specify that you cannot quantify a likely range? Why not just say "insufficient evidence to quantify the magnitude of those projected feedback changes"? It seems odd to use IPCC uncertainty language in this way [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Statement was kept in order to explain why a likely range cannot be quantified.
116593	6		6		The ES relates climate feedbacks to projections and models. Is it possible to report evidence for cloud feedbacks during the past decades? [Valerie Masson-Delmotte, France]	Accepted. Observational evidence for feedbacks have been further emphasized in the ES.
36867	7	3	7	4	Multiple independent papers, using different methods, have shown otherwise. [John McLean, Australia]	Rejected - we make a detailed assessment of the relevant literature to reach this conclusion - see text in Section 7.5.
77355	7	3	7	4	This is a key finding but it can be expressed more clearly for the non specialist reader including by 1 explaining what ECS is, 2 linking it to the doubling of CO2 calculation on page 5 3. pointing to current ERF values and implications for GHG emissions. [Emer Griffin, Ireland]	Rejected. This is beyond the scope of chapter 7.
68881	7	3	7	12	The section on paleoclimate evidence for estimating ESC is very strong in CH7 and the agreement with other lines of evidence is powerful. The topic is important enough that the estimated value based on each of the independent lines of evidence should be featured in the ES, and possibly in the TS as well, as was done for AR5. This is needed to address one of the paleo key messages, which is devoted to ECS: What do past climate states indicate about equilibrium climate sensitivity? [Darrell Kaufman, United States of America]	Rejected - due to space constraints we did not include the paleo-only estimates of ECS in the E.S., but they do figure prominently in the chapter, e.g. Table 7.13.
55065	7	3	7	12	Line 4 says multiple lines of evidence give a very likely range of ECS between 2 and 5°C. Line 10 says emergent constraint evidence and paleo evidence help rule out ECS values above 5°C. These two statements seem contradictory since the first statements certainly allows for possible values above 5°C. [Nancy Hamzawi, Canada]	Taken into account. The statement was revised.
71751	7	3	7	12	It is very good here to see an attempt to constrain the long-standing likely range of ECS (1.5-4.5K). However the quoted likely range here (2.5-4K), with a best estimate of 3K, is bizarrely assymetric. I was expecting to see something more like 2-4K, which is much more defensible in my view. Emergent constraints based-on interannual variability (Cox et al., 2018) suggest an ECS likely range of 2.8+/-0.6K, with a very likely range (5-95%) of 1.8-3.8K, which is consistent with very-likely ranges based-on the global warming over the last 50 years in CMIP5 (Jimeniz & Mauritzen, 2019) of 1.7-4.1K and CMIP6 (Nijsse et al., in press) of 1.5-4.0K. These emergent constraints based-on the global temperature record suffer much less from the long-tail problems of energy-balance apporaches. As a result the case for reducing the upper likely range (from 4.5K) is actually stronger than the case for increasing the lower likely range (from 1.5K). [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The assessment is based on more than just these emergent constraints, and there are concerns that some of these are over-confident in bounding the upper end of ECS, given uncertainty in the strength of pattern-effects and model's ability to represent these. It is generally more difficult to constrain the upper bound than the lower bound, hence the asymmetry.
34909	7	3	7	26	The SOD estimates of ECS and TCR need to be viewed in the context that these are not based on CMIP6 models and may even be based only on group-think. Please see general comment #3 above. [Jim O'Brien, Ireland]	Noted. The comment does not provide concrete suggestions, and furthermore the SOD assessment of ECS and TCR are neither based on raw output from CMIP5 nor CMIP6 models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111719	7	3	7	26	Overall these paragraphs represent a very significant advance in understanding from AR5, in an area that has been challenging to make progress on through the previous ARs. Apart from my specific comment on p7l9-p7l12, I think these paragraphs (and the supporting detail in the chapter) provide one of the headline advances of AR6. One additional advance that I would suggest bringing forward into the ES here (and indeed to the SPM) is the insight that the historical record unfortunately does not provide a good constraint on the upper range of ECS. This was kind-of known before but there is significant new insight, as discussed in section 7.4.4.3 and 7.5.2. To add a little to this, we have recently shown for one high ECS model that the rapid warming simulated in recent decades in the HIST runs is more likely explained by the well-known error in the pattern effect over recent decades (seen in many models, e.g. Gregory et al Cl Dyn 2019), than by the overall higher ECS of this model (M. Andrews et al JAMES 2020 https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019MS001995). I have heard that similar analysis is in progress for other CMIP6 models and may be submitted soon. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This ES point has been revised.
679	7	5	7	5	text correction needed: change "since IPCC" to "since IPCC AR5" [Bruce Wielicki, United States of America]	Accepted
84839	7	9	7	9	"ECS value below 1.5 is ruled out" will lead to confusion since IPCC has indicated that warming should be below 1.5 [Jayaraman Srinivasan, India]	Rejected. There is no contradiction between the two.
20069	7	9	7	12	To which statement does the "high confidence " statement apply? [philippe waldteufel, France]	Taken into account. Text has been clarified in this respect.
111717	7	9	7	12	This is an important sentence but I found it a bit confusing. Are you ruling out ECS>5C or not? (at say the 5% level). I also think 'challenging' is the wrong word here. It suggests the authors want to rule it out, rather than a neutral assessment of the evidence. I think it would be simpler to say simply 'ECS>5C cannot be ruled out'. Overall I think it's important to improve both the clarity and precision of this sentence. [Richard Wood, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement was revised.
96703	7	9	7	12	Please revise if it is really possible to "rule out" ECS > 5°C. Also emergent constraints are a debatable method and depend on specific conditions and assumptions, e.g. since they cannot account for longer term climate feedbacks or even tipping points. All it allows are probabilities, and actually not for ECS, but only for TCR. Paleo evidence has also limitations due to different geophysical conditions compared to the current state of the planet. [Nicole Wilke, Germany]	Taken into account. The statement was revised.
9699	7	10	7	12	Looks like a subjective statement to me. They cannot rule out …. Saying it is challenging implies you have an a priori hypothesis that they should be ruled out. [Olivier Boucher, France]	Taken into account. The sentence was revised
71959	7	10			Suggest "rule out" is too strong here. [John Church, Australia]	Taken into account. The sentence was revised
99067	7	11	7	11	Might it help to add a phrase after "ECS" to the effect: "such as has occurred on Venus" to indicate what is being referred to and that this is not just an idel statement but has happened in our solar system? [Michael MacCracken, United States of America]	Rejected. We do not consider the example of Venus as helpful.
102159	7	12			Difference to AR5 (?) [Maria Rugenstein, Germany]	Noted. The reviewer comment is unclear, however it is assumed that it refers to asymmetry in AR5 uncertainty statements regarding ECS. Also AR5 statements were asymmetric.
77357	7	14	7	17	This is a key finding but it can be expressed more clearly for the non specialist reader including by explaining what TCR is and why it is different from ECS in an accessible manner. [Emer Griffin, Ireland]	Rejected. We see no reason to elaborate at this point in the text.
77359	7	14	7	17	The term reduce biases is fine for a technical audience but a more user orientated statement would be useful here. [Emer Griffin, Ireland]	Accepted. Paragraph has been reworded.
9859	7	14	7	17	"high level of agreement" is assessment language -- it does not need a confidence statement but rather is part of the argument for high confidence [Robert Kopp, United States of America]	Accepted. Paragraph has been reworded.
99069	7	14	7	17	I would think that the TCR has to depend on the rate of warming approaching a doubling--unless the TCR is defined as the value reached when adjusting to a 1%/year increase in forcing or some similar standard. If indeed a standard forcing pattern that is used, then that needs to be stated making clear that if one approaches the doubling at a slower pace, the TCR minght well be higher, which is sort of saying that one does not get the full expected value by going to a slower rate of warming. I would add a line explaining what TCR is to this finding to more clearly explain for one not working closely in the field as the definition in the opening paragraph of this chapter is just not sufficient. [Michael MacCracken, United States of America]	Noted. Whereas the reviewer is correct that the rate of forcing applied affects the warming by the time of doubling, TCR is a well-defined metric in that it is with respect to a 1 percent per year increase in CO2.
17939	7	15	7	15	Should you explain what TCR is here. Do I need to go to the Glossary for that? TCR is not in the Glossary, although TCRE is. I guess this is all treated in Box 7.1. [Dennis Hartmann, United States of America]	Taken into account. TCR was in the glossary, however, we have requested an update of the glossary text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17941	7	16	7	16	The document makes heavy use of the 'There is .. Sentence construction. You could have said. Different lines of evidence show a high level of agreement . . . Does the high confidence apply to the level of agreement or to the statement about TCR, ambiguous. [Dennis Hartmann, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
34653	7	19	7	19	Seems like a simpler and clearer way to say this is, "Overall, CMIP6 models have higher ECS and TCS values than CMIP5 models…" That's how it reads in the last paragraph of 7.5.6. [Russell Vose, United States of America]	Accepted.
22121	7	19	7	19	distribution of seems an odd phraseology here. I think this would be clearer if these two words were dropped? If not it should probably be clearer what is meant given that no similar qualifier is applied to the CMIP5 models in the same sentence. [Peter Thorne, Ireland]	Accepted. The two words were deleted.
31693	7	19	7	19	"higher average" - Seems strange not to be quantitative - how much higher? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We do not find it relevant to be quantitative at this point in the text.
46071	7	19	7	19	It would be better to use "median" instead of "average". [Twan van Noije, Netherlands]	Not applicable. The word was deleted.
17943	7	19	7	20	This sentence is poorly constructed. Are you trying to say that the ECS and TCR in CMIP6 are generally higher across the odel ensemble than either CMIP5 or the assessment you are making in AR6? [Dennis Hartmann, United States of America]	Taken into account. The sentence was revised.
107637	7	19	7	20	Again, why assign high confidence to a statement that is based purely on model output that is known perfectly. Isn't this a fact? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The confidence statement was removed.
106325	7	19	7	26	This is an essential and well-formulated message for the ES. Please keep it through to the final draft. The last sentence can also be formulated more positively and succinctly: "The CMIP6 models with the highest ECS and TCRs values hence provide useful insights into high-risk, but low-probability futures." [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The last sentence was revised.
93681	7	19			"The distribution of models … has" [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
20071	7	20	7	20	What does "this Report" mean? From the way the sentence begins on expect it to be AR5 but in English "this report" usually means "the present one"… [philippe waldteufel, France]	Rejected. That "this report" refers to AR6 is deemed clear.
102161	7	20			Shouldn't a statement about the difference between CMIP5 and CMIP6 be made with very high confidence? Otherwise it looks as if you don't understand your own tools? [Maria Rugenstein, Germany]	Taken into account. No confidence statement is made as this is considered a fact.
41489	7	21	7	21	I agree that extra-tropical feedbacks are perhaps the major reason, but there are other regions that light up in different models to have increased ECS. So should there be a modifier here? ' can mostly be traced' or similar? [Andrew Gettelman, United States of America]	Accepted. We have added 'in some models' to indicate that this is not always the case.
102163	7	21			"can be traced to changes" --> e.g. Wyser et al. 2020 GMD says sth different. The recent talk by Mark Zelinka also sounded less certain than this statement. Soften "for most models" or for xx out of yy models [Maria Rugenstein, Germany]	Accepted. We have added 'in some models' to indicate that this is not always the case.
102165	7	23	7	24	"The ranges of ECS …" --> I do not understand this sentence [Maria Rugenstein, Germany]	Accepted. The text was revised.
17945	7	23	7	24	What does it mean to span the assessed very likely ranges? Unclear. Can you use plainer language? [Dennis Hartmann, United States of America]	Accepted. The text was revised.
46073	7	23	7	24	This statement depends on the way ECS is calculated from the CMIP6 simulations. Here it is implicitly assumed that ECS is calculated using linear regression of the first 150 years of the simulations. Some models however have continued the abrupt-4xCO2 simulation(s) beyond 150 years. Not restricting the regression to the first 150 years will result in higher ECS estimates that are more representative of the models' actual ECS. This may push the high-end of the CMIP6 range beyond the very likely range assessed here. Note that the difference can be quite substantial, as is for instance the case for CESM2 (see also the paper Rugenstein et al. (2019) quoted in this chapter). I suggest to add here that this statement is based on the assumption that ECS is calculated using 150 simulation years, and in the appropriate section add some analysis of the regression bias, including also results from the longer simulations. [Twan van Noije, Netherlands]	Rejected. The paper by Rugenstein et al. (2020) also looks at the bias introduced by using 4xCO2 instead of 2xCO2 and it is found to be of similar magnitude. There is no reason to elaborate on this in the summary bullet point.
10721	7	23	7	26	This is disappointing example of circular reasoning. e.g., physics based climate models are used to contribute to the assessment of ERF, which is used in much simpler models in the assessment of ECS. One cannot then use these results to weight down models that contributed to (even if indirectly) to the ECS assessment! [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement regarding assigning of probability was deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
128829	7	24	7	24	[ENSEMBLES] Only future scenarios provide insight into future climate. Don't understand how low confidence models would provide insight into anything else except their deficiencies. In fact, would it not be wiser to exclude such simulations from assessments or at least introduce some sort of weighting to diminish their impact on derived statistics? [Trigg Talley, United States of America]	Taken into account. Text has been revised to clarify the role of CMIP6 models in the assessment of future warming.
27093	7	24	7	24	Could you indicate : Which models? How many? How high ECS and TCR? How low is the probability? [Eric Brun, France]	Rejected. Numbers for specific models are given in table 7.SM.4, but this type of information is not appropriate for the Executive Summary.
67901	7	24	7	25	" The CMIP6 models with the highest ECS values are assigned low probability, but are nevertheless useful as they provide insights into high-risk, low-probability futures." So low probability ECS and TCR values are what is supporting the 'high confidence' ECS values described on page 7-6 line 40? [Stephen Gaalema, United States of America]	Not applicable. The statement on page 6 line 40 regards cloud feedback, not ECS or TCR.
9701	7	24	7	25	Which models? How many? How high ECS and TCR? How low is the probability? [Olivier Boucher, France]	Rejected. Numbers for specific models are given in table 7.SM.4, but this type of information is not appropriate for the Executive Summary.
71753	7	24	7	26	I suggest replacing the last line with: "The CMIP6 models with the highest ECS and TCR values tend to overestimate global warming over the last 50 years, and are therefore down-weighted in our assessment of these metrics. However, these models remain highly useful as they provide insights into high-risk, low-probability futures, and also help to define emergent constraints." [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement was revised.
10847	7	24	7	26	Where was the assigning of probabilities done? According to 7.5.6 (page 105:52-53) "it is problematic and not obviously constructive to provide weights for, or rule out, individual CMIP6 model ensemble members based solely on their ECS and TCR values." [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement regarding assigning of probability was deleted.
34655	7	24	7	26	I think the last sentence of this key message should be deleted. There is only one sentence in section 7.5.6 that directly addresses this point (page 106, lines 6-9). And it's stated much better there than in this key message. [Russell Vose, United States of America]	Taken into account. The statement was revised close to that suggested.
117265	7	25	7	25	Note that in chapter 1 and SPM we refer to "low-likelihood, high-impact" events. [Maisa Rojas, Chile]	Accepted, suggested wording adopted
31695	7	25	7	25	Maybe it is clear in 7.5.6 but it seems there are (at least) two distinct reasons for high-risk futures - one is that conventional feedbacks are underestimated (which is the implication of the CMIP6 models) and the other is the effect of processes not included in current generations of models. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	.Taken into account.. Wording changed and clarified
22123	7	26	7	26	Should this additionally cite the chapter 4 section(s) where this exploration of low probability high impact outcomes is further detailed? [Peter Thorne, Ireland]	Accepted Chapter 4 now referenced
128831	7	29	7	55	This is wrong. Water vapor is the strongest Greenhouse Gas. See Table 2 on page 6 (350) in A. E. Galashev and O. R. Rakhmanova Emissivity of the Main Greenhouse Gases. Institute of Industrial Ecology, Ural Branch, Russian Academy of Sciences, Yekaterinburg, Russia, ISSN 1990 7931, Russian Journal of Physical Chemistry B, 2013, Vol. 7, No. 3, pp. 346-353. © Pleiades Publishing, Ltd., 2013. Original Russian Text © A.E. Galashev, O.R. Rakhmanova, 2013, published in Khimicheskaya Fizika, 2013, Vol. 32, No. 6, pp. 88-96. [Trigg Talley, United States of America]	Rejected. Water vapour as the strongest GHG is not disputed, the cited text covers other material however, so it is not clear what is "wrong"
36869	7	31	7	31	Utter claptrap! Probably based on CIMP5 climate models that according to IPCC AR5 exaggerated warming. [John McLean, Australia]	Rejected. The evidence is detailed in 7.3.5, from many lines of evidence
65407	7	31	7	31	In the SPM, they describe the warming as "an established fact" but here they describe it as unequivocal. This should be made consistent — I support the unequivocal terminology in order to make it consistent with the statement about warming. [Andrew Dessler, United States of America]	.Taken into account, wording revised
10723	7	31	7	34	Very surprised to see this circular reasoning here. The estimated "GSAT" rise, uses ECS which uses the observed temperature record as an important constraint (7.5.5). It is thus clearly unsurprising the simple model can matches observed temperature trends. The ERF has been assessed by using, in greater and lesser degrees, by the use of climate models also used in attribution studies. This text must be amended to remove any claim that the simple model trends are independent of the observational record or the use of climate models. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, Chapter carefully avoids this circular reasoning as much as possible. Historical evidence does not place a strong constraint on ECS. And forcing estimates also largely independent of models. The arguments are explained in detail in Section 7.3
77361	7	31	7	35	Can this be shortened and simplified? [Emer Griffin, Ireland]	Taken into account. The details are largely needed, but text has been reworded for clarity
77363	7	31	7	35	Does this mean that the global temperature has increased by 1.1C since 1750? [Emer Griffin, Ireland]	Taken into account. This is the human forced trend - it happens to be similar to the observed trend. Text reworded

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114571	7	31	7	35	This sentence is a bit uncelar; espcially the part about "little knowledge". Can you consider reformuating it? [Jan Fuglestvedt, Norway]	Taken into account. Sentence reworded but it's a complex argument
71041	7	31	7	39	Chapter 3 assesses the attributable warming for GSAT change for 2010-2019 relative to 1850-1900, whereas here the warming for 1750-2018 is assessed. Assessment of a common period will be useful. [Yu Kosaka, Japan]	Accepted. Periods are made consistent with Chapter 3
103601	7	31	9	31	The year 1750 is a convention, so statement has to be softened to indicate a large interval (who knows what the solar constant was at that point in time). It is suggested to make a comment that 1750 is selected (somewhat arbitrarily) as the beginning of the industrial era. [Philippe Tulkens, Belgium]	Accepted. Good point, this is now added
16145	7	32			I find "give an estimate" confusing, I think what you mean to say is "imply" or "predict"--also I assume this claim refers to a range rather than an exact value so should probably say "range" somewhere [Steven Sherwood, Australia]	Accepted. Agree, suggestion is adopted
9861	7	33	7	33	"more-or-less" is awkwardly informal [Robert Kopp, United States of America]	Taken into account. This is hard to be exact, sentence worded though for simplicity
96705	7	33	7	33	Please explain what is meant by "assumes little knowledge". How does this relate to emergent constraints? [Nicole Wilke, Germany]	Taken into account. This is hard to be exact, sentence worded though for simplicity
96707	7	33	7	33	We suggest to replace "more-or-less" by something better quantified, e.g. "largely" (independent). [Nicole Wilke, Germany]	Taken into account. This is hard to be exact, sentence worded though for simplicity
31697	7	33	7	33	Is it more or is it less? Not very clear English in my view. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This is hard to be exact, sentence worded though for simplicity
102167	7	33			"more or less independent" --> mäh, more precision would be nice. [Maria Rugenstein, Germany]	Taken into account. This is hard to be exact, sentence worded though for simplicity
3571	7	34	7	35	It is explained that "For the period 1750-2018, this human forced trend is 1.1 °C (0.4 to 1.9 °C range) (high confidence)". However, in FOD (p.5 lines 26-27) text says "It is unequivocal that human activity has had a warming effect on the planet since 1750. Human induced surface temperature rise for the period 1750-2017 is 1.1 °C [0.9 to 1.3 °C 5% to 95% range]. Are these ranges consistent each other? If not, why range is so different? Also, the above figure in SOD chapter 5 1.1 °C (0.4 to 1.9 °C range) is different from SOD chapter 5, i.e. 1.1°C (0.9–1.3°C, likely range) between the 1850–1900 and 2010–2019 periods (p. 85 lines 49-52). Please clarify. [Mitsutsune Yamaguchi, Japan]	Taken into account. The chapters are making different/independent estimates of the same thing. This is clarified
17947	7	35	7	35	How can you have high confidence when the range is so large from half to twice the stated central value? [Dennis Hartmann, United States of America]	Taken into account. High confidence is in the warming
107639	7	35	7	35	Replace trend with increase or warming, the units are C not C per decade [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. agree, reworded as suggested
66995	7	35	7	35	In my view this range brings confusion with respect to those assessed in CH3 about human-induced warming. Is there any added value? ANT-induced warming of .4°C or 1.9°C seems quite inconsistent with historical observations. It even suggests that the considered ranges for ERF, ECS and TCR in Ch 7 could be further narrowed by historical observations. [Aurélien Ribes, France]	Taken into account. It is our view that it adds an independent line of evidence for the historical trends. However, the point is taken about further information
77367	7	35	7	37	This sentence should be reworked for clarity, perhaps used to sentences so the issues of the last 20 years are clearer. [Emer Griffin, Ireland]	Taken into account. The paragraph has been reworded for clarity
77365	7	35	7	39	Does warming mean the observed increase in the global temperature? [Emer Griffin, Ireland]	Accepted. Yes, this is now clarified and GSAT used
65007	7	36	7	36	That aerosol forcing with "high confidence" was constant in the last 20 years is in some contradiction with my conclusions in Chapter 2 that aerosol concentrations declined (I have said medium confidence for that decline, which would imply relative positive forcing). Chapter 6 is a bit vague but also has a decline in SO2 in its ES. We have to reconcile these statements. [Johannes Quaas, Germany]	Taken into account. This statement has now been made consistent across chapters.
46075	7	36	7	36	I assume "increasing trend" is meant to be "upward trend". [Twan van Noije, Netherlands]	Yes. Accepted.
28851	7	36			relatively constant as a global mean but with varying spatial distribution? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Paragraph has been reworded.
31699	7	41	7	41	Are you referring to the future here? I could read "this century's" to refer to 2000 to 2020 or 2000 to 2100 [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This ES point has been revised to clarify this.
16147	7	41	7	43	This sentence is unintelligible [Steven Sherwood, Australia]	Taken into account. This ES point has been revised.
77369	7	41	7	43	What does continued CO2 emissions mean here? E.g. in terms of scale? [Emer Griffin, Ireland]	Taken into account. This ES point has been revised to clarify this.
77371	7	41	7	43	Consider using two sentences to distinguish between the implications for sources of uncertainties. [Emer Griffin, Ireland]	Taken into account. This ES point has been revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99071	7	41	7	43	This sentence needs to open with the phrase, "Other than the variation in warming due to the emissions scenario itself," This is to make very clear that the major cause of the different outcomes is the amount of emissions, and then that variations around this are mainly a result of cloud feedbacks. I would note that it is not as iff the cloud feedback with vary randomly over its range for different scenarios--the feedback would move all of the scenarios up or down by about the same amount so there would not be a crossing such that a lower emissions scenario would lead to much more warming than a high emissions scenario--emissions are the most important controlling factor. [Michael MacCracken, United States of America]	Taken into account. This ES point has been revised to clarify this.
28853	7	41			can the cloud feedback contribution be quatified approximately? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This ES point has been revised.
98635	7	43	7	44	"OHU is a minor souce of uncertainty in centennial waming" - I am not so sure based on recent NorESM vs CESM analysis , effects of convection shutdown on Antarctic (and Arctic) and associated heat storage in deeper ocean water masses. It might be true for the recent history, but might not hold in the coming decades and centuries future [Michael Schulz, Norway]	Taken into account. This ES point and associated text has been revised.
128833	7	44	7	44	[CONFIDENCE] "Global ocean heat uptake is a relatively minor source of uncertainty in centennial warming." Uncertainty in OHU from in-situ observations is often quoted to be on the order of ±0.1 Wm-2. Such assessments often provide trend uncertainty as their only measure of uncertainty, neglecting sampling uncertainty and other sources of error that would increase OHU uncertainty to at least 0.2 Wm-2. OHU from satellite approaches come with larger but probably more realistic uncertainties, and the error analysis is more rigorous (Meyssignac et al., 2019 and references therein). [Trigg Talley, United States of America]	Taken into account. The findings supporting this ES point come from climate model projections rather than recent observations. This has been clarified.
17951	7	45	7	45	Time scales longer than what? What defines longer time scales? [Dennis Hartmann, United States of America]	Taken into account. The timescale has been clarified and this ES point has been rewritten.
128835	7	47	8	4	Surely this misses the most important difference between the Arctic and Antarctic which is that the latter is land and high and covered in bright ice! Also 7.2.2 [Trigg Talley, United States of America]	Taken into account. This ES point and supporting text has been rewritten to discuss these additional factors and the timescales over which they apply.
19399	7	47	8	4	Why highlight Arctic polar amplification in this Chapter, as opposed to many other aspects of regional change? [Isaac Held, United States of America]	Noted. Polar amplification is addressed in this Chapter because of the recent advances in understanding the phenomenon in terms of radiative feedbacks, heat transport, and other aspects of Earth's energy budget.
17953	7	48	7	48	This is ambigious. What is 'ocean heat uptake between the poles'? Suggest moving between the poles to after asymmetries. [Dennis Hartmann, United States of America]	Taken into account. This has been revised.
34657	7	48	7	48	Readability could be improved by replacing the phrase "a combination of assymetries" with the word "differences." [Russell Vose, United States of America]	Taken into account. This has been revised.
102169	7	48			"between the ploes" --> between polar regions or high latitudes [Maria Rugenstein, Germany]	Accepted.
99073	7	49	7	49	Need to change "poles" to "polar regions" [Michael MacCracken, United States of America]	Accepted
31701	7	49	7	49	"poles" = "Polar regions" [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
28855	7	49			can an approximate time scale be placed on "eventually" e.g. multi-century? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This paragraph has been modified to clarify the timescales over which the Southern Ocean is expected to warm. Given the open questions regarding Southern Ocean and Antarctic warming, the assessment has been modified to high confidence that Antarctic amplification will emerge as the Southern Ocean surface warms on centennial timescales, but low confidence regarding whether the feature will emerge during the 21st century.
69601	7	53	7	53	slower' -> 'more slowly' [Nicholas Golledge, New Zealand]	Accepted
68883	7	54	8	1	I believe that this is the only mention of paleo polar amplification in any chapter ES. This metric is important to support the paleo key message about prominent recurring patterns and as a target for paleo data-model comparison. The treatment in section 7.4.4.1.2 needs to be expanded by quantifying the magnitude of polar amplification (as was done in AR5) and including missing paleo reference periods and evidence from land. As an alternative, this topic could be subdivided between CH2 (proxy temperatures), CH3 (comparison with models), and CH7 (understanding of processes). [Darrell Kaufman, United States of America]	Accepted - land and SSTs now added, and quantified in the Figure.
28857	7	54			just a comment: it seems surprising to me that the slower Antarctic warming is dominated by ocean upwelling since the possibility for albedo feedback is much smaller and the contrasting permanent ice verses more transient ocean ice [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. While several factors play a role, models suggest that it is ocean upwelling (heat uptake) and asymmetries in the lapse rate feedback that dominate the difference in transient warming (Goosse et al. 2018). The ice albedo feedback is actually similar between the poles poleward of 60 degrees latitude. This ES point has been rewritten to discuss these additional factors and to clarify the timescales over which they apply.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17949	7	62	7	67	I was not able to review the entire 206 pages, but I found the cloud section here to be well balanced and infomrative. [Dennis Hartmann, United States of America]	Noted. Thank you!
116595	7		7		what does "eventually" mean here and how consistent is it with findings from Ch 3 and Ch 4 related to confidence in projections for Antarctic change? There are still open questions related to the ability of models to capture recent Antarctic warm phases (early Holocene, LIG) (possibly related to the representation of sea ice, see Holloway et al, Nature, 2016 and related publications. For Arctic warming, there is also literature based on recent observations stressing the role of liquid clouds in enhancing Greenland surface melt and surface Arctic warming, is this relevant here too (see Kay et al, 2016 for a review). [Valerie Masson-Delmotte, France]	Taken into account. This paragraph has been modified to clarify the timescales over which the Southern Ocean is expected to warm. Given the open questions regarding Southern Ocean and Antarctic warming, the assessment has been modified to high confidence that Antarctic amplification will emerge as the Southern Ocean surface warms on centennial timescales, but low confidence regarding whether the feature will emerge during the 21st century.
102171	8	1			Why only high confidence? [Maria Rugenstein, Germany]	Taken into account. The assessment of high confidence was arrived at based on robust agreement across multiple lines of evidence including the projections shown in Chapter 4. This is the highest level of confidence used in Chapter 7.
20395	8	2	8	4	Along with the Antarctic amplification, the summary might mention the similar issue of the east west SST gradient (see Page 80 L52-Page 81 L2): while present observations do not agree with model predictions, the report states that these predictions will ultimately be validated. [philippe waldteufel, France]	Taken into account. This ES point and associated text has been revised to discuss east west SST gradients.
31545	8	3	8	4	Could that sentence be clearer, sorry maybe my english, but I find it confusing: Do we have some evidence it will emerge this century but have low confidence ? Or we have low confidence in when it will emerge, with no evidence on whether that will emerge this century or later [Jean-Baptiste SALLEE, France]	Taken into account. This ES point has been revised.
17955	8	4	8	4	what is 'the feature' I think you should say 'Antarctic amplification' to be more clear. [Dennis Hartmann, United States of America]	Taken into account. This ES point has been revised accordingly.
65409	8	6	8	6	"Specifying short and long-lived …" seems like a weird high-level conclusion. Is this really important enough to be here? Seems like it's in the weeds, to be honest. [Andrew Dessler, United States of America]	Taken into account, text reworded and section heavily revised
51369	8	6	8	6	Does the separation have to be by lifetime (short vs long)? Suggest this instead highlights the general case where quantification of surface warming is more accurate when individual forcers/gases are separated in the calculation. At the moment, it looks like the IPCC is recommending that there be two baskets based only on lifetime. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded and section heavily revised
51371	8	6	8	6	Could you be more specific about what "improves" means? What is the magnitude of this improvement and how much of a difference does it make? This is important as it helps policy makers weigh up the benefits of a improved quantification of surface warming against other considerations. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded and section heavily revised
112029	8	6	8	8	Do you mean specifying SLCF concentrations? As opposed to expressing concentrations in CO2eq? This sentence is a bit confusing. [Cynthia Randles, United States of America]	Taken into account, text reworded and section heavily revised
127	8	6	8	10	The first two sentences in the Executive Summary are very similar to the 'summary' at the end of section 7.6.3. in my comments on p 116, l 14 - 18, I raise concerns about the confidence in these statements. They do not seem appropriate as high-level findings, until those confidence statements are consistent with the relevant guidance and better supported. [Harald Winkler, South Africa]	Taken into account, text reworded to better support confidence language
68357	8	6	8	12	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account GWP-20 added to tables and Figure and discussed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68359	8	6	8	12	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account GWP-20 added to tables and Figure and discussed
68361	8	6	8	12	For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. [Durwood Zaelke, United States of America]	Taken into account GWP-20 added to tables and Figure and discussed
15399	8	6	8	12	Issues and improvements of emission metrics are directly relevant for policymakers but seems omitted in SPM. What is described here is worth of being stated in SPM. [Junichi Tsutsui, Japan]	Taken into account and now added in SPM
77373	8	6	8	12	This is a key message for policy and should be included in the SPM. [Emer Griffin, Ireland]	Accepted, these are now added
106327	8	6	8	12	This is a correct, balanced and succinct ES message on emission metrics. The section in the underlying chapter, however, contains several statements that are too generalizing and can be misinterpreted easily when used out of context. In some cases, the chapter text also speaks to issues that are outside the mandate, scope and expertise of WG1, which should be given careful consideration during the revisions and in several cases removed, particularly because IPCC cannot be seen as being policy prescriptive or favouring a specific emission metric based on value judgments. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded and section heavily revised
73905	8	6	8	12	The summary does not seem to accurately summarize the main changes and main messages compared of the underlying chapter. The main changes are a re-evaluation of effective radiative efficiency, the linkages of metrics not only to temperature increase, but also on SLR and other carbon cycle responses, a consistent calculation of direct climate-carbon feedback in the presented metrics, a clearer presentation of the fossil correction for CH4 and other hydrocarbons, the discussion of new metrics such as GWP*. Does the approach to specify short and long-lived GHG separately in emission scenarios only improves the quantification of surfacing warming, or also all other climate impacts, such as SLR? If it is not correct for all other impacts, the highlighted sentence should be deleted as it would give a recommendation which has to be set in the proper contaxt which may not be the case here, or it should be mentioned that this is not the same for other impacts. This summary paragraph should also focus on the appropriateness of metrics related to the objectives of the Paris Agreement as this is the key mesage for policy makers and this is discussed in the underlying chapter. How emission scenarios can be improved, seem to be less important as a key message outside the internal WGI discussion. It would be very useful to add an explanation of how this recommendation on using metrics for emission scenarios has been implemented throughout all WGs of the AR6 as this seems to be the key area of application. [Anke Herold, Germany]	Accepted, these are now added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103603	8	6	8	12	This statement seems out of place in an exec summary - it relates to a technicality on how to improve estimates of climate response (a nobrainer that use a empirical correlation done until recently between short-term and long-term greenhouse gases don't make much sense). [Philippe Tulkens, Belgium]	Taken into account, text reworded
66799	8	6	8	12	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account GWP-20 added to tables and Figure and discussed
66801	8	6	8	12	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account GWP-20 added to tables and Figure and discussed
46077	8	6	8	12	Can this section be generalized to include aerosols and short-lived reactive non-greenhouse gases? [Twan van Noije, Netherlands]	Taken into account but the literature discusses methane
69887	8	6	8	12	"it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice. Physical science can only form a subset of the inputs to those choices." I would argue that the authors of chapter 7 by pushing GWP* and eliminating established metrics are doing the exact opposite. This isn't to say that scientists can't introduce GWP* as another approach, but it is inconsistent to "force" this metric on policymakers. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Gabrielle Dreyfus, United States of America]	Taken into account GWP-20 added to tables and Figure and discussed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51373	8	7	8	8	Suggest that more clarity is provided on which approaches use aggregated emissions and which don't. As written, it could give the impression that IAMs, for example, use aggregated emissions pathways when they do actually provide individual GHG emissions which are then used to calculate surface warming. An aggregated emissions approach might be taken in a more simple back-of-the-envelope calculations, such as adding up NDCs, where individual GHG emissions are not available. Clarity on this would help improve understanding of where choice of metric has a impact that makes a material difference. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded and section heavily revised
40041	8	8	8	8	could assign a high confidence level after this bullet [TSU WGI, France]	Accepted
106329	8	8	8	10	It might be useful to specify that this increased equivalence is valid only in the context of cumulative emissions. When considered in a single year (as often is the case with emissions targets) the equivalence might not be increased or even be decreased. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded and section heavily revised
20397	8	8	8	10	Sentence difficult to understand. What is "equivalence"? What is "more equivalence"? More than what? [philippe waldteufel, France]	Taken into account, text reworded and section heavily revised
112593	8	8	8	10	"comparing" and "more equivalence" is a bit mealy-mouthed. Also, not clear if Wigley (1998)'s Forcing Equivalent Index can still be called "new". Suggested rewording: "Metrics of so-called "CO2-warming-equivalent emissions", that relate a pulse emission of a fixed quantity of a very long-lived greenhouse gas such as CO2 with a permanently-sustained change in the emission rate of a short-lived greenhouse gas such as methane, provide a more accurate indication of the impact of emissions on global mean temperature change than conventional "CO2-equivalent emissions"." [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded and section heavily revised
18281	8	9	8	9	Better to exemplify "short-lived gases". [Yugo Kanaya, Japan]	Accepted
114573	8	9	8	9	"more" --> "improved" (?) [Jan Fuglestvedt, Norway]	Accepted
84841	8	9	8	10	"more equivalence in surface temperature response " is confusing. Jargon should be avoided in executive summary [Jayaraman Srinivasan, India]	Taken into account, text reworded and section heavily revised, including the ES bullet
31703	8	10	8	10	This is a bit picky, but CO2 was and is still 1. You could say AGWP and AGTP, or refer to non-CO2 gases? Also, are they larger than the tentative carbon-cycle values presented in AR5, which some people seem to favour? I guess we see later. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded
24055	9	1	9	1	The use of the word "innovations" in the title: 7.1 Introduction, conceptual framework and innovations since IPCC AR5, could with benefit be changed to advances to better reflect the content of the section and connect to the text within the section [Linn Berglund, Sweden]	Accepted. Agree, reworded as suggested
77375	9	3	9	52	See earlier points about linking the energy budget balance ERF etc in an accessible narrative at the start of this chapter. As well as focusing on these components a short description of how they fit together would be useful and not just illustrated in a figure. [Emer Griffin, Ireland]	Taken into account. This is now addressed in a slight rewording at the start
40651	9	5	9	5	Please review and revise as necesary the existing glossary definition for 'Climate sensitivity' (note that itt uses GMST rather than GSAT): "The change in the annual global mean surface temperature (GMST) in response to a change in the atmospheric carbon dioxide (CO2) concentration or other radiative forcing." [TSU WGI, France]	Taken into account. Glossary revised
116597	9	10	9	10	The statement refers to decadal timescales, but there is also evidence of response to shorter volcanic perturbations. [Valerie Masson-Delmotte, France]	Accepted. Agree, reworded
77377	9	10	9	11	It would be useful to explain that TOA is the key energy boundary for our climate systems. The budget change would be clearer than perturbed. [Emer Griffin, Ireland]	Accepted. Agree reworded
16149	9	10	9	21	What is the purpose of this paragraph? I would assume the outline of the report was given in e.g. Chapter 1. I guess the purpose is to relate this chapter to others so as to clarify what can be found where. But if so that should be stated at the beginning, and the only other chapters that need be discussed are those where there is potential for confusion or overlap (so for example, not the last three chapters I would say). [Steven Sherwood, Australia]	Accepted. Agree, reworded as suggested
77379	9	11	9	11	For the non specialist the changes to the energy budget occurs when it gains or loses energy rather than heat. Heat as a form of energy could be explained. [Emer Griffin, Ireland]	Accepted. Agree, reworded as suggested
12123	9	11			Switch "warming" and "cooling" to match with "gains or loses" [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, reworded as suggested
102173	9	14	9	15	"the process understand developed within this chapter" --> develeped in papers which are cited in this chapter? [Maria Rugenstein, Germany]	Accepted. Agree, reworded to say since AR5

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20073	9	17	9	21	This may be true; regional repartitions however do not appear in the plan of the chapter; It is not necessary to overstress the universal nature of this chapter 7, which ought to be apparent to every reader. Besides, the relevance of what is reported in this chapter for regional issues is explicitly indicated in the paragraph page 10 lines 39-46 [philippe waldteufel, France]	Accepted. Agree, this has now been removed
77381	9	18	9	18	Perhaps use quantification of changes rather than measures which have a wider meaning climate policy. [Emer Griffin, Ireland]	Taken into account. reworded
102175	9	19			the global (?) otherwise the statement is trivial. [Maria Rugenstein, Germany]	Accepted. agree, reworded
117267	9	23	9	28	For easier readability, it would be helpful to include the chapter number and name of the community-led assessments [Maisa Rojas, Chile]	Accepted. Topic of community assessments added
23879	9	27	9	27	As in most of chapters, a bad habit continues by citing unpublished work. This comment is basically a repetition of that to all chapters. [Branko Grisogono, Croatia]	Noted. Sherwood et al. now published
103605	9	27	9	27	Update or delete reference to submitted paper [Philippe Tulkens, Belgium]	Noted. Sherwood et al. now published
104897	9	27	9	37	Is only unforced variability? Is there any contribution from natural forced variability at those timescales? [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This sentence refers to unforced variability so is correct as is
102177	9	28			delete "when assessing specific details" [Maria Rugenstein, Germany]	Accepted. agree
77383	9	31	9	31	Is the word feedback needed here? Could a more accessible term be used for this? [Emer Griffin, Ireland]	Noted. Feedbacks is the right technical word in this context
27095	9	33	9	33	There are 3 ways / method to obtain the described climate metric here: the one addressed here - where however a specification is needed, i.e. to precise that this is the Earth energy budget at the Earth surface; then the radiation balance (or imbalance) at the top-of-the-atmosphere; and the Earth heat inventory, i.e. on how (and how much) heat is stored in the Earth system - they all should come up to a similar value. Practically however, the the 'integral of the Earth budget at the surface is not the 'metric' of global change discussed in literature: it is either the metric at the top-of-the-atmosphere, or the Earth heat inventory. The reason for this are: limitations of the observing system - the method proposed here as 'metric' contains on todays' capabilities a uncertainty of 10-20 W/m2 to capture a vlaue of 0.5-1 W/m2 - this is -at todays capabilities - not a practical way. A long text here, but fundamental why this wording needs to be adopted accordingly in connection wiht the climate change metric argument. [Eric Brun, France]	Taken into account. Point taken, this is now addressed in the revision
10725	9	35	9	38	Forced variability is also a contributing factor, as high frequency forcing factors, e.g. following explosive volcanic eruptions, are damped down in measures of total Earth system warming, but not in surface temperatures. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Wording now changed to clarify variability point.
81767	9	36	9	36	There are several wordings / therminology used here, but a precise definition at the very top of this chapter is missing: Earth energy budget (is this surface only?); Earth system warming: is this the Earth heat inventory (i.e. heat storage in the ocean, atmosphere, cryosphere, land) ? [Karina von Schuckmann, France]	Taken into account. This is now added at start as suggested
27099	9	36	9	36	There are several wordings used here to deal with warming, but a precise definition at the very top of this chapter is missing: Earth energy budget (is this surface only?); Earth system warming: is this the Earth heat inventory (i.e. heat storage in the ocean, atmosphere, cryosphere, land) ? [Eric Brun, France]	Taken into account. This sentence refers to unforced variability so is correct as is. The extra detail is not necessary here
81769	9	36	9	38	According to my comment at ES level: I will further go through the text, but if this is the only part where this is assessed, then I understand why there is no uncertainty statement provided. But there is much more literature on this topic which should be taken into account, such as for example: Cheng et al., 2019 (https://eos.org/opinions/taking-the-pulse-of-the-planet); Palmer et al., 2017 (https://link.springer.com/article/10.1007/s40641-016-0053-7); Trenberth et al., 2016 (ref in this chapter-; Meyssignac et al., 2019 (doi: 10.3389/fmars.2019.00432); Hansen et al., 2017 (https://doi.org/10.5194/esd-8-577-2017); Dieng et al., 2017 (doi: 10.1002/joc.4996) [Karina von Schuckmann, France]	Taken into account. Thank you - this has now been expanded on as suggested. But in section 7.2, this now refers forward to that discussion
27097	9	36	9	38	This is a very important statement, and needed, but it would need to be properly assessed, and accompanied by an uncertainty statement. To do that, there is much more literature on this topic which should be taken into account, such as for example: Cheng et al., 2019 (https://eos.org/opinions/taking-the-pulse-of-the-planet); Palmer et al., 2017 (https://link.springer.com/article/10.1007/s40641-016-0053-7); Trenberth et al., 2016 (ref in this chapter-; Meyssignac et al., 2019 (doi: 10.3389/fmars.2019.00432); Hansen et al., 2017 (https://doi.org/10.5194/esd-8-577-2017); Dieng et al., 2017 (doi: 10.1002/joc.4996) [Eric Brun, France]	Taken into account. Thank you - this has now been expanded on as suggested. But in section 7.2, this now refers forward to that discussion

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128837	9	37	9	38	The main refernce here should also be Cheng et al., 2018: Cheng, L., K. E. Trenberth, J. Fasullo, J. Abraham, T. Boyer, K. von Schuckmann, J. Zhu, 2018: Taking the pulse of the planet. Earth and Space Science News, Eos, 99, 14-16. Doi: 10.1029/2017EO081839. which analyzes the signal to noise ratio for OHC vs sea level rise vs GMST. [Trigg Talley, United States of America]	Taken into account. Thank you - this has now been expanded on as suggested. But in section 7.2, this now refers forward to that discussion
102179	9	43			This is only partly what Section 7.5 is about, maybe refer to othe subsection? [Maria Rugenstein, Germany]	Taken into account. Other sub sections now referenced
51375	9	44	9	45	Suggest a clearer reference to WGIII here: "and WGIII will provide further information on metrics, their use, and other policy goals beyond the temperature goal" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, now added
114575	9	45	9	45	It is good that you mention to link to WGIII. We need to follow up with closer contact here; commenting on each others drafts, cross WG discussions etc. [Jan Fuglestvedt, Norway]	Noted. No reply needed
46079	10	1	10	1	Change "IPCC AR5" to "the AR5". Also in the caption to Figure 7.1. [Twan van Noije, Netherlands]	Accepted. Agree
673	10	3	10	3	Very good summary in Figure 7.1 [Bruce Wielicki, United States of America]	Noted. thank you
31705	10	3	10	3	Note typo in top box ("wether") and "fossil" should be "fossil fuel" in the RF box, I feel. Maybe some could misinterpret why contrails and volcanoes have been singled out here. A 5-year-old dinosaur enthuisiast might also be concerned about the dinosaur because, as I understand it later, none of the paleo estimates come from the age of the dinosaurs. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. typo corrected
31729	10	3	10	3	Perhaps worth referring to the Ramaswamy review (10.1175/AMSMONOGRAPHS-D-19-0001.1) for some of the historical background to the development of some of the forcing concepts? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. agree, now cited
69889	10	9	10	25	Policymakers should have access to multiple metrics, including metrics that allow for a two-basket approach and recognize the near-term impacts of SLCP (such as GWP20 and GTP20). In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Gabrielle Dreyfus, United States of America]	Taken into account. GWP20 is now discussed in Section 7.6
68363	10	9	10	25	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account. GWP20 is now discussed in Section 7.6

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68365	10	9	10	25	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account. GWP20 is now discussed in Section 7.6
68367	10	9	10	25	For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. [Durwood Zaelke, United States of America]	Taken into account. GWP20 is now discussed in Section 7.6
114577	10	9	10	25	This para contains useful clarifications. [Jan Fuglestvedt, Norway]	Noted, thank you
66803	10	9	10	25	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. GWP20 is now discussed in Section 7.6

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66805	10	9	10	25	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. GWP20 is now discussed in Section 7.6
20075	10	10	10	11	Is this sentence really necessary? [philippe waldteufel, France]	Accepted. Agree, deleted
40865	10	10	10	15	Suggest to define 'climate metrics' and 'emission metrics' in the glossary [TSU WGI, France]	Taken into account. Emission metrics added to glossary
101	10	11	10	12	Why are some metrics mentioned here and not others? Other climate metrics than ECS and TCR are explained in Box 7.1 and other chapters, e.g. TCRE in ch 5. On emission metrics, the most commonly used with GWP, not GTP - why is the latter the only example? [Harald Winkler, South Africa]	Rejected. We don't provide a full list for readability
77385	10	12	10	12	Perhaps include mention of GWP here as well as it is more commonly used in policy. [Emer Griffin, Ireland]	Accepted. Agree, changed
51377	10	12	10	12	Better to give Global Warming Potential as an example here as it's the one that is used in climate policy around the world and will be familiar to readers. Also, the rest of the paragraph focuses on radiative forcing so makes sense to highlight GWP. Suggest GWP replaces GTP or is added in addition. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, changed
31707	10	15	10	15	I am not sure how Figure 7.2 shows how emission metrics fits in. To my mind you would need an extra box that includes mitigation options/decisions that then feeds back into the emissions (or energy economy) box. The metrics also act as a short cut between emissions and some of the intermediate steps (even though they are implicit in the metric formulation - e.g. the GTP goes staight from emissions to temperature, without explicitly considering forcing. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure now deleted
36871	10	17	10	19	Metrics do NOT evaluate anything. They might describe, but they don't evaluate. [John McLean, Australia]	Noted. They can evaluate when given a value. No change necessary
77387	10	27	10	27	Are TCR and ECR metrics or estimates? [Emer Griffin, Ireland]	Noted. They can be both. -we choose the term climate metrics
77389	10	27	10	27	Could the word theoretical be used rather than idealized? [Emer Griffin, Ireland]	Accepted. Word changed as suggested
36873	10	30	10	31	Would you not agree that the impact of various climate forcing agents varies over time, with some factors changing almost instantly and others taking far longer? Does it not follow that when one factor takes a long period to react, the faster reacting factors might have already changed in response to a some factor and not necessarily the original factor? On these grounds your TCR is hand-waving of no merit whatsoever. [John McLean, Australia]	Taken into account. TCR has a clear definition related to 1% per year increase in CO2
10727	10	31	10	35	The metrics help explain global surface temperature variation in models. ECS, and TCR are not particularly helpful for explaining model variations in precipitation, stratospheric temperatures, atmospheric circulation ... That they refer to 'global surface temperature' should be noted. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, text added
46081	10	34	10	34	Can this statement be generalized beyond CMIP5? [Twan van Noije, Netherlands]	Taken into account. Yes, text generalized
36875	10	39	10	41	How many more times do you need to be told that correlation does not prove causation and that the output of models depends on the data and algorithms put into them? If the data put into models is questionable (and much of the historial temperature data is) or the algorithms are incomplete (certainly true) then the output of models is worthless for anything other than testing the sensivity of the models. [John McLean, Australia]	Accepted. Agree that correlation was not a good word choice, wording now changed to make a more explicit connection

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46083	10	40	10	40	Try to be consistent across chapters in using "global climate model", "coupled general circulation model", and "Earth system model". Here the acronym "GCM" is re-introduced, whereas in other chapter the same class of models are simply referred to as "global climate models". [Twan van Noije, Netherlands]	Accepted. We revise to use ESM as much as possible
107641	10	41	10	41	Also point to chapter 11 here as well as ch 4 [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agreed, Chapter 11 now cited
10729	10	41	10	41	"A substantial fraction" is very vague. One person's substantial fraction is anothers small effect. Quantify this statement in some way. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is by way of intro and hard to be specific without a lot of detail, so not changed
19519	10	44	10	44	after Climate change add " variability" [Hamideh Dalaei, Iran]	Rejected. Addition is not needed here
20399	10	46	10	46	Yet, chapter 4 does not deal with regional issues [philippe waldteufel, France]	Accepted Chapter 11 now cited
30557	10	47			Figure 2.2 instead of 2.1 [Gilles Delaygue, France]	Accepted. reference changed
30559	10	47			replace 'radiogenic' by 'cosmogenic' (neither 14C nor 10Be are radiogenic) [Gilles Delaygue, France]	Accepted. agreed
114579	10	49	10	55	useful figure. But check consitency with Ch1, WGII (e.g. Brian O'Neill) and WGIII (e.g. Elmar Kriegler) authors. [Jan Fuglestvedt, Norway]	Not applicable. Figure deleted.
36877	10	51	10	51	Figure 7.2 is not so much conceptual as an unproven supposition that any human influence on temperature warrants concern. [John McLean, Australia]	Not applicable. Figure deleted.
18347	11	3	12	53	Box 7.1: Please note that a climate feedback is traditionally defined as any process that either amplifies or damps the initial response of the global-mean surface temperature (Ts) to a perturbation in external forcing. Based on this original definition of climate feedbacks, Hansen et al. (1984) and other studies have used the ratio of the Ts change with the feedback to the Ts change without the feedback as the feedback paratemer to quantify a feedback. As you can see, only Ts is involved in this conventional definition of climate feedbacks, TOA forcing is not involved. I understand that many papers since the late 1990s have focused on the changes in TOA net radiative fluxes to quantify a feedback. Since for a transient climate, the change in Ts depends more than TOA net flux (i.e, the surface energy balance is not the same as the TOA energy blanance), the definition based on TOA flux does not exactly follow the original definition of climate feedbacks based on Ts changes. At the least, this Box should acknowledge the earlier work by Hansen et al. and others, and recognize that the different definitions of the climate feedback or climate feedback parameter exist in the literature (e.g., Hansen et al. 1984; Roe 2009, etc.). Refs cited: Hansen J, Lacis A, Rind D, Russell G, Stone P, Fung I, Ruedy R, Lerner J (1984) Climate sensitivity: analysis of feedback mechanisms. Clim Process Clim Sensit (AGU Geophysical Monograph Series 29) 5(29):130–163. Roe, G. (2009) Feedbacks, Timescales, and Seeing Red. Annu. Rev. Earth Planet. Sci. 37:93-115. [Aiguo Dai, United States of America]	Taken into account. This is a general introduction, Section 7.4 now referred to for specifics on method
18349	11	3	12	53	Box 7.1: eq. 7.1: Please note that non-zero dN, dT and alpha can exist due to natural variations even when dF=0, and this complicates the estimates of these terms purely in response to external forcing (dF) under increasing CO2, as shown in Dai et al. (2020). That is, in a 2XCO2 or 4XCO2 coupled model run, the dN, dT, and alpha can result from both internal variaiblity and response to the CO2 forcing, and on short (decedal to centennial) time scales, ther variations and changes may be dominated by internal variaiblity, and the alpha due to internal variaibility may differ greatly from that due to exernal forcing (Dai et al. 2020). For estimating ECS from a relatively short 4XCO2 or 2XCO2 run of a few hundred years, it is the alpha resulting from the resonse to external forcing that matters, not the alpha on decadal to centennial time scales that often results from internal variability and varies a lot. The alpha resulting from the long-term response to CO2 forcing may actually be fairly stable based analyses of multi-milennial simulations done by Dai et al. (2020). This provides a basis for reliably estimating the ECS from relatively short 4XCO2 runs, as shown by Dai et al. (2020). Ref. cited: . Dai, A., D. Huang, B.E.J. Rose, J. Zhu and X. Tian, 2020: Improved methods for estimating equilibrium climate sensitivity from transient warming simulations. Climate Dynamics, DOI :10.1007/s00382-020-05242-1. https://link.springer.com/article/10.1007/s00382-020-05242-1 [Aiguo Dai, United States of America]	Taken into account. This is a general introduction, Section 7.4 now referred to for specifics on method
40099	11	7	11	7	Effective radiative forcing is currently only mentioned within the definition for 'Radiative forcing': "The radiative forcing once rapid adjustments are accounted for is termed the effective radiative forcing." Consider adding a separate definition. [TSU WGI, France]	Accepted. Glossary edited

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23881	11	7	11	45	Explain why you have to write twice ERF, though, in different letters? What is the key difference? If unimportant, then please try to shorten the Chapter and the whole Report. [Branko Grisogono, Croatia]	Noted. Different forcing estimates are used in Section 7.3
36879	11	13	11	15	The ERF was false in AR5 and repeating it here doesn't make it more credible. You CANNOT logically mix top-of-atmosphere values with an "effective radiative forcing" (which varies in direction and strength with altitude) with near-surface temperature. [John McLean, Australia]	Rejected. ERF is also a top of atmosphere measure, so the concern is not clear
36881	11	13	11	15	ERF assumes instant equilibrium within the climate system, which is nonsense. The climate system is always trying to reach equilibrium but the forcings continually change (e.g. the sun rises), with some of these changes taking months or even years. Doesn't the hottest part of the day typically occur AFTER peak insolation? Don't the warmest and coldest months of the year occur about six weeks after the respective solstices? A time delay is important andd you've ignored it. [John McLean, Australia]	Rejected. ERF makes no requirement for equilibrium and response time delays are included
10935	11	13	11	42	Box 7.1 would be a good place to introduce the ocean heat uptake efficacy, a parameter of the two-box model now used for emulation, as an alternative to the ambiguous term "Effective ECS". OHU efficacy is the ratio of the slopes of the yellow and accurate gray lines in panel b. The addition of an efficacy factor to equation 7.1 allows it to account for the characteristic kinked trajectory of the model state (blue dots): epsilon*del-N = del-F + alpha*del-T (alternate eqn. 7.1). This equation can be rearranged to show that 1) epsilon = [del-F / ECS] / [N / (ECS-del-T)] ← ratio of slopes of yellow and accurate gray lines in panel b, and 2) epsilon = [(ECS-del-T) / N] / [ECS / del-F] ← ratio of temp. sensitivities: N-sensitivity / del-F-sensitivity, qualifying epsilon as an efficacy. Defining the ocean heat uptake *efficiency* as kappa = N/del-T, allows us to write the alternative eqn. 7.1 as: N = del-F + [alpha - (epsilon-1)*kappa)] * del-T. Alternate eqn. 7.1 written this way has the same form as the standard eqn. 7.1 but with a variable alpha factor that becomes less damping as OHU, and hence kappa, decline. In other words, ocean heat uptake efficacy gives a theory using constant parameters for the variation of alpha in the standard eqn. 7.1 over the equilibration. See Winton et al (2010) for the first two interpretations of OHU efficacy and Held et al (2010) for the variable alpha interpretation. [Michael Winton, United States of America]	Taken into account. Too complex here but added later in chapter and in appendix
114581	11	16	11	16	insert "ERF" before "perturbation" to link better to line 21 [Jan Fuglestvedt, Norway]	Taken into account. The sentence reads "...efective radiative forcing perturbation…"
65411	11	16	12	41	Picking alpha as the feedback parameter is not the choice I would have made. I think most people use lambda. My suggestion is to explicitly say in the paragraph describing it (starting on line 8, page 12) you say this is conventionally written as lambda, so people reading this will not be confused. [Andrew Dessler, United States of America]	Taken into account. Convention and sign varies in the literature, so we retain alpha
20401	11	21	11	22	Would it be wrong to define ERF as the TOA energy budget change assuming there is no feedback from GSAT? [philippe waldteufel, France]	Taken into account. This is what we do - reworded for clarity
87945	11	23	11	26	It is disconcerting that in this chapter you have arbitrarily changed to a new measurement product for global surface temperature, namely using air temperature rather than the customary combined air-SST products. If it is now the IPCC view that air/SST products should never be used you had better get the other chapter teams to rewrite their sections, otherwise it reads like you have cherry-picked the available temperature products to bump up the ECS range. Also, having decided to use surface air temperature, why confine your data to the surface? The chapter makes little or no use of tropospheric temperature products, sonde or satellite, which are associated with low TCS estimates: [Christy, J.R. and McNider, R.T. (2017). Satellite bulk tropospheric temperatures as a metric for climate sensitivity. Asia-Pacific Journal of Atmospheric Science 53(4) 511-518 DOI:10.1007/s13143-017-0070-z https://link.springer.com/article/10.1007/s13143-017-0070-z] [Ross McKitrick, Canada]	Rejected. Using GSAT was a collective decision - GMST and GSAT are assessed to the same, so there is no bumping up of estimates
24057	11	25	12	25	BOX 7.1: Forcing, feedbacks and climate sensitivity framework. The defined parameters are presented in italic and the abbreviation non-italic apart from the "ECS". All could be changed to italic or all to non-italic to maintan a consistent structure [Linn Berglund, Sweden]	Editorial. italics added
128839	11	30	11	30	AR6 uses a general definition for climate feedbacks (alpha) and ECS is adopted. What Earth system processes are included here? Provide a list of these processes. [Trigg Talley, United States of America]	Accepted. Agree, example processes now added
102181	11	30			give examples for "many Earth system processes" --> this is important as this is new. Maybe also summarize what that change of definition does to ECS and how "backward compatible" ranges are then. [Maria Rugenstein, Germany]	Accepted. Agree, processes now added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36883	11	35	11	35	The CO2 concentration in preindustrial times is unknown. One thousand years ago is preindustrial, just as one million and one billion years are. Ice cores from one location or even two locations do not give you a global average. [John McLean, Australia]	Rejected. It is known reasonably well from ice cores and other proxies
72163	11	45	12	6	It remains unclear how ERF is estimated from GCMs. Also the associated figure 1 of box 7.1 does not help here. Regression methods are widely used, but is there a standard how many years are left out at the beginning? [Anna von der Heydt, Netherlands]	Taken into account. This presents the concepts, the details are addressed in section 7.3
10731	11	45	12	6	The difference between "Effective Radiative Forcing" and "Adjusted Effective Radiative Forcing" (Richardson et al 2019) should be clearly indicated by the use of the latter term where necessary. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We think that introducing a new term is unnecessary here
23883	11	45	12	53	Several repetitions, redundancy, appear in Box 7.1 and elsewhere. [Branko Grisogono, Croatia]	Taken into account. Text has been shortened
36885	11	47	11	49	I think you'll find that downward radiative flux isn't measured at the top of the atmosphere - how could it be? - and what's measured is the upward flux and that this is subtracted from an assumed value of what the flux would be if 100% was emitted upwards. [John McLean, Australia]	Taken into account. Downward is just the sign convention
37555	11	48			Adjustments here are described by enumerating state variables that might change. Would it not suffice to say that adjustmets are changes in temperature (emission) and opacity? [Robert Pincus, United States of America]	Taken into account. We think the text as is better for a general audience
31797	11	49	11	49	"these adjustments" - can something be said concerning the extent to which these adjustments are additive in the case of multiple forcings acting at the same time? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, text clarified
31709	11	49	11	49	Should this be "unrelated to any GSAT change" rather than "prior to"? In a temporal senses, these fast processes overlap to some extent with GSAT changes, as is stated a few lines later (line 55) [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Good idea - text reworded
10733	11	50	11	53	A new acronym is introduced here, "SARF". Are the authors aware of previous uses of this in recent climate research studies? I found "surface aerosol radiative forcing", "solar aerosol radiative forcing", and "snow-albedo radiative forcing" being used in various papers. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Acronym is needed for clarity
89175	11	52	11	52	The following is from TAR Chapter 6: IPCC (1990, 1992, 1994) and the Second Assessment Report (IPCC, 1996) (hereafter SAR) used the following definition for the radiative forcing of the climate system: “The radiative forcing of the surface-troposphere system due to the perturbation in or the introduction of an agent (say, a change in greenhouse gas concentrations) is the change in net (down minus up) irradiance (solar plus long-wave; in Wm-2) at the tropopause AFTER allowing for stratospheric temperatures to readjust to radiative equilibrium, but with surface and tropospheric temperatures and state held fixed at the unperturbed values”. This definition has been used in the radiative forcing chapters in AR4 and AR5. Why define SARF which is actually the same as RF in previous IPCC assessment reports? The chapter has followed AR5 definitions of IRF and ERF and therefore no conflict in using RF as an acronym. Introducing SARF is confusing and I strongly recommend the authors to reconsider and change to RF. [Gunnar Myhre, Norway]	Noted. RF is used generally in this report to either mean SARF or ERF, so we prefer to change acronyms despite the precedent
116599	11		12		No cited literature in the box, framing only? [Valerie Masson-Delmotte, France]	Taken into account. Some literature is cited at start but generally this is framing
46085	12	2	12	2	Change "sea-ice" to "sea-ice concentration". [Twan van Noije, Netherlands]	Accepted. Agree
40649	12	8	12	9	Please review and revise as necesary existing glossary definition for 'Climate feedback parameter' (note that itt uses GMST rather than GSAT): "A way to quantify the radiative response of the climate system to a global mean surface temperature change induced by a radiative forcing. It varies as the inverse of the effective climate sensitivity. Formally, the Climate Feedback Parameter (α; units: W m-2 °C-1) is defined as: α = (ΔQ - ΔF)/ΔT, where Q is the global mean radiative forcing, T is the global mean air surface temperature, F is the heat flux into the ocean and Δ represents a change with respect to an unperturbed climate." [TSU WGI, France]	Taken into account. glossary edited
90709	12	10	12	11	This equation needs to state changes in x (climate variable) that are due to changes in SAT.—most likely the partial derivative of x with respect to SAT. As written, any climate variable that impacts TOA radiation is included in the feedback parameter including those components that are designated as forcing (i.e. CO2 and stratospheric adjustment). [Aaron Donohoe, United States of America]	Accepted. Agree, text added
83109	12	12	12	13	This requirement that feedbacks that change atm CO2 concentartions can not be included in alfa, isn't that just because of how our MIP experiments (4xCO2) are set up for calculation alfa. It is nothing fundamental about this. If we had instead perturbed an HFC component to give apprx. the same forcing we would have avoided this problem. [Terje Berntsen, Norway]	Taken into account. Agree, however, it is needed in the chapter framework so text retained

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83745	12	14	12	19	Thank you for including this clarification- this is something that often gets confused in the literature and even though it's very basic and easy to infer from the mathematical definition, I feel it's helpful to include here. [Marvel Kate, United States of America]	Noted. Thank you
64531	12	19	12	19	"A change in variable x" should be "A change in process x". [Peter Caldwell, United States of America]	Accepted. Agree
72165	12	25	12	41	See comment on ERF above. The same holds for ECS. Please specify how ECS is determined in practice. [Anna von der Heydt, Netherlands]	Taken into account. This is done in section 7.5, text clarified
111117	12	29	12	29	This would be a good place to state ECS = F_{co2}/ alpha – unless there is some reason it doesn't in this framework. This statement hasn't appeared yet and the discussion following talks about both ECS and alpha without having made the connection between the two. [Aaron Donohoe, United States of America]	Accepted. Good idea, added here
111119	12	30	12	30	. A statement on whether the carbon feedback is included in alpha would be useful here. The discussion alluded to carbon feedbacks being excluded from alpha (page 12, line 13) but it remains a mystery how the chapter deals with carbon feedbacks. [Aaron Donohoe, United States of America]	Accepted. Agree, added here as well for clarity
68885	12	30	12	32	Please add the definition of ESS to the Glossary. ESS is not "assessed in Section 7.4.2.6" as implied. [Darrell Kaufman, United States of America]	Taken into account. ESS is not really used here, so sentence deleted
40653	12	43	12	45	Please review and revise as necesary the existing glossary definition for 'Climate sensitivity' (note that itt uses GMST rather than GSAT): "The change in the global mean surface temperature, averaged over a 20-year period, centred at the time of atmospheric carbon dioxide (CO2) doubling, in a climate model simulation in which CO2 increases at 1% yr-1 from pre-industrial. It is a measure of the strength of climate feedbacks and the timescale of ocean heat uptake." [TSU WGI, France]	Taken into account. glossary revised
88933	12	43	12	46	Flynn & Mauritsen use 'surface temperature', not 'surface air temperature'. It would be useful to specify the details of TCR method more compeltely somewhere in the chapter and particulary for Table 7.A.2. e.g. ESMValTool and Flynn & Mauritsen remove the linear fit of the pre-industrial control years corresponding to the years of the 1% simulation [Julie Arblaster, Australia]	Taken into account. This is referenced in Section 7.5 -now cited here
24059	12	47	12	47	The abbreviation "TCRE" should be written in bold [Linn Berglund, Sweden]	Accepted. agreed
40655	12	47	12	51	Please review and revise as necesary the existing glossary definition for 'TCRE' (note that itt uses GMST rather than GSAT): "The transient global average surface temperature change per unit cumulative carbon dioxide (CO2) emissions, usually 1000 GtC. TCRE combines both information on the airborne fraction of cumulative CO2 emissions (the fraction of the total CO2 emitted that remains in the atmosphere, which is determined by carbon cycle processes) and on the transient climate response (TCR)." [TSU WGI, France]	Taken into account. glossary edited
36887	12	48	12	48	TCRE is bogus because it's based on uncertain and unknowable preindustrial (what??? you don't say what it is) when it's simply impossible to know those preindustrial levels with any acccuracy. [John McLean, Australia]	Rejected. Not supported by literature
31711	12	52	12	53	TCRE and GWP - I don't understand this sentence, and didn't when I got to Section 7.6 [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sentence deleted
104907	13	1	15	56	In many parts, the text reads more like a literature review than an assessment. Please also check for any accidental mixing in terms of global integral & mean diagnostics. [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been checked to address these aspects.
81771	13	3	13	3	not clear what this means, and what 'relevance' stands for [Karina von Schuckmann, France]	Noted. The text has been retained to highlight that we focus our attention on the major flows of energy that are relevant for determining present and future climate - the reference to Figure 7.2 makes this explicit.
27101	13	3	13	3	It is not clear what this means and what 'relevance' stands for [Eric Brun, France]	Noted. The text has been retained to highlight that we focus our attention on the major flows of energy that are relevant for determining present and future climate - the reference to Figure 7.2 makes this explicit.
104899	13	9	13	9	Need for consistent use: either "internal" or "unforced". (internal seems to be used more frequently across the energy budget section). [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The two terms are equivalent and used interchangeably in the scientific literature, which this assessment report reflects, so we retain both.
104901	13	10	13	14	Has Earth experienced any sustained energy imbalance (over multidecadal or longer periods) prior to antropogenic forcing? Any paleo evidence? [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Internal variability in EEI is discussed in this paragraph in the context of climate model simulations. The observation-based assessment in Ch7 focusses on the instrumental record. The paleo evidence for changes in ocean heat content, which dominates changes in Earth's energy inventory is discussed in Chapter 2 in section 2.3.3. We did not duplicate that material in Chapter 7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27103	13	14	13	17	There is confusion in this chapter on the use of thermoniologies, i.e. between the use of 'Earth energy budget', and the use for 'the surface energy budget'. IN ly point of view, the use of 'Earth energy budget' includes also the 'surface energy budget'. This needs to be clarified at the very top of the report, and particularly clarified within the chapter team to assure a coherent use when LAs are writing up their specific sections assigned. This is a very sensitive and important detail because in some cases this can lead to physically wrong statements and confusion. [Eric Brun, France]	Accepted. Text has been revised for consistency across the chapter and wider AR6 report.
81773	13	14	13	18	There is confusion in this chapter on the use of thermoniologies, i.e. between the use of 'Earth energy budget', and the use for 'the surface energy budget'. In my point of view, the use of 'Earth energy budget' includes also the 'surface energy budget'. This needs to be clarified at the very top of the report, and particularly clarified within the chapter team to assure a coherent use when LAs are writing up their specific sections assigned. This is a very sensitive and important detail because in some cases this can lead to physically wrong statements, see also my comment above, and confusion. [Karina von Schuckmann, France]	Accepted. Text has been revised for consistency across the chapter and wider AR6 report.
27105	13	15	13	15	We recommand to use 'through' insted of "within" to encompass the physical change [Eric Brun, France]	Noted. We respectfully disagree with this suggestion. The point is that the energy flows *within* the climate system are a separate issue from the radiative energy transfer in/out of the system at the top-of-atmosphere.
53121	13	15	13	19	I noticed only three instances of "water cycle" in the whole CH7 which is somehow consistent with the highlighted sentence and the apparent mainstream paradigm of a relatively "passive" water cycle strongly constrained by the energy cycle. This is not exactly what I understood from Previdi and Liepert (2012) for instance. Could CH7 consider to temper this paradigm and also quote papers (e.g., Webb et al., 2018) highlighting that the water cycle is coupled to the surface energy budget? [Hervé Douville, France]	Accepted. Agree, wording changed and Chapter 8 cited
81775	13	17	13	18	recommend to better say 'and is tightly linked to the global water cycle'; Another point: there is also a link to the global carbon cycle, this should be mentioned as well (Resplandy, L., Keeling, R.F., Eddebbar, Y. et al. Quantification of ocean heat uptake from changes in atmospheric O2 and CO2 composition. Sci Rep 9, 20244 (2019). https://doi.org/10.1038/ [Karina von Schuckmann, France]	Noted. We have retained the text stating that the surface energy budget is "a key driver of the global water cycle" as a more useful physical description.
27107	13	18	13	18	We recommend to say 'and is tightly linked to the global water cycle' instead of " and plays a key role as driver of the global water cycle" Another point: there is also a link to the global carbon cycle, this should be mentioned as well (Resplandy, L., Keeling, R.F., Eddebbar, Y. et al. Quantification of ocean heat uptake from changes in atmospheric O2 and CO2 composition. Sci Rep 9, 20244 (2019). https://doi.org/10.1038/s41598-019-56490-z) [Eric Brun, France]	Noted. We have retained the text stating that the surface energy budget is "a key driver of the global water cycle" as a more useful physical description.
22125	13	21	13	27	Most other chapters do not have such introductions. Particularly if overlength I'm not sure you'd lose much by removing this link text and similar within section link text elsewhere. [Peter Thorne, Ireland]	Accepted. The text has been removed.
104903	13	22	13	22	Please define present-day. How far back in time? [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text and figure caption have been revised to clarify that we are referring to the early 21st century.
128841	13	24	13	24	Where are ocean-land energy and moisture transports discussed? These are intricately linked with circulations, the hydrological cycle, and climate extremes. [Trigg Talley, United States of America]	Noted. Given word length constraints, this is beyond the scope of Chapter 7. The hydrological cycle and climate extremes are discussed in Chapter 8 and Chapter 11, respectively.
104905	13	30	13	30	Again, please be specific about period in years. [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We replaced "present-day" by "at the beginning of the 21st century".
64741	13	32	13	32	The reference period of time used for the estimate of the « present-day » energy balance should be precized. [Serge PLANTON, France]	Accepted, We replaced "present-day" by "at the beginning of the 21st century".
36889	13	32	13	52	Your energy budget is a fantasy that should have been abandoned years ago. It assumes instantaneuos equilibrium, which is not how the atmosphere and biosphere operate. At any time the system is trying to reach equilibrium but the inputs keep changing on every scale from seconds through to millenia and even longer. (Two very simple examples are daytime maximum temperatures and the warmest and coldest months, the daily maximum temperature typically being after peak insolation, which occurs arond noon, and the warmest and coldest months of the year occuring about six weeks after the respective solstices.) Further, your budget ignores issues such as the movement of equatorial heat towards the poles (which elsewhere you admit is what happens) and the oceans storing heat and later releasing it. [John McLean, Australia]	Noted. The display shows the long term global mean energy fluxes, not an instantaneous picture. Since it is a global mean representation, it does not resolve the meridional heat transports.
41491	13	34	13	35	maybe clarify that aerosol-cloud interactions is a forcing, to distinguish from feedbacks; this looks like equivalence. [Andrew Gettelman, United States of America]	Accepted, we replaced "caused by" by "forced by".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111121	13	37	13	37	Suggest replacing “clear sky energy budget” with “energy fluxes under clear sky conditions” since the clear sky energy budget is not closed. This could be stated explicitly with a sentence like – “Under clear-sky conditions, there is a net TOA radiative imbalance of +20W m^-2 suggesting that the Earth would have to warm substantially if there were no clouds”. [Aaron Donohoe, United States of America]	Taken into account, we added a comment to the caption of Figure 7.3 (Figure 7.2 in the final version) as follows: "Note that the cloud-free energy budget shown here is not the one that Earth would achieve in equilibrium when no clouds could form. It rather represents the global mean fluxes as determined solely by removing the clouds but otherwise retaining the entire atmospheric structure. Thus, the cloud-free TOA budget is not closed."
81777	13	39	13	39	space measurements are also surface measurements, wrong wording. better to use 'in-situ' measureements, and satellite mesaurements or equivalent [Karina von Schuckmann, France]	Taken into account, we reformulated this as "They have been derived by taking into account information contained in both in-situ and satellite radiation measurements taken under cloud-free conditions" to make this point clear. Note, however, that satellites can only measure top of atmosphere fluxes directly, surface fluxes in satellite products are derived fluxes.
27109	13	39	13	39	Since space measurements are also surface measurements in this context , we recommend to use 'in-situ' measureements, and satellite mesaurements or equivalent [Eric Brun, France]	Taken into account, we reformulated this as "They have been derived by taking into account information contained in both in-situ and satellite radiation measurements taken under cloud-free conditions" to make this point clear. Note, however, that satellites can only measure top of atmosphere fluxes directly, surface fluxes in satellite products are derived fluxes.
38349	13	43	13	43	This sentence reads that thermal outgoing radiation at the TOA is enhanced without clouds by nearly 30 Wm-2 (268 ± 3 W m-2 instead of 239± 3 W m-2 globally). But in Figure 7.3 (2) on page 178, thermal outgoing at the TOA is 267 Wm-2. According to Wild et al. (2015, 2019) cited in Figure 7.3, it is suggested that 268 should be changed to 267. [Yaming LIU, China]	Accepted, we changed 268 Wm-2 to 267 Wm-2.
130523	13	43	13	43	268±3 is inconsistent with Figure 7.3 as 267±3. [Panmao Zhai, China]	Accepted, we changed 268 Wm-2 to 267 Wm-2.
39591	13	47	13	55	In Figure 7.3, how could a flux of 342 W/m2 (all sky) or 314 W/m2 (clear sky) travel from a COLDER atmosphere to a HOTER soil if the numbers correspond to heat? [François Gervais, France]	Noted. Every object emits radiation, depending on temperature and optical properties. It is only the net radiative flux that points towards the colder object.
28859	13	49			Figure 7.3: an addition could be to include the value of the net radiative cooling of the atmosphere in the diagram which determines atmospheric stability [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this would be interesting, but graphically difficult to display so that it can be as intuitively understood as the other components.
103607	14	1	14	21	The discussion on why the space-based estimates of net TOA FLUX is not reliable could be deepened. Fitting a measured net flux to model data seems highly dubious. It is an important message to space agencies to improve estimates, and important caveat that models are behind current net flux estimates. [Philippe Tulkens, Belgium]	Noted. The text does not say that the measured net flux is fit to a model. Rather it says the satellite estimate is constrained by in-situ observations. This is clearly stated on lines 12-17 and described in detail in Loeb et al. (2018a).
111123	14	6	14	8	. I suggest removing sentence starting with “Since AR5 …”. The nature and motivation for the CERES EBAF product is better discussed in the next two sentences and as written this sentence implies that the accuracy of the direct measurements of TOA radiation have improved significantly since AR5 which (I think) is untrue (absolute calibration error still dominate the direct estimate of EEI). [Aaron Donohoe, United States of America]	Taken into account. We cannot entirely remove this sentence as the remaining text would no longer be coherent. But we reformulated the sentence to become more neutral: "Since the AR5, the CERES Energy Balance EBAF Ed4.0 product was released, which includes algorithm improvements and consistent input datasets throughout the record (Loeb et al., 2018)."
82855	14	6	14	12	The numbers referred to for CERES EBAF accuracy do not seem to agree with those given in Loeb et al. (2018), or in the online CERES data product summary (https://ceres.larc.nasa.gov/documents/DQ_summaries/CERES_EBAF_Ed4.1_DQS.pdf). It would be good to clarify how these numbers were produced and/or make a reference to their source. Refs: Loeb, N. G., Doelling, D. R., Wang, H. L., Su, W. Y., Nguyen, C., Corbett, J. G., et al. (2018a). Clouds and the Earth’s Radiant Energy System (CERES) Energy Balanced and Filled (EBAF) Top-of-Atmosphere (TOA) Edition-4.0 Data Product. J. Clim. 31, 895–918. DOI:10.1175/Jcli-D-17-0208.1 [Frida Bender, Sweden]	Noted, the numbers are not the same, since they have been adjusted to the 90% confidence level.
81779	14	9	14	12	this descritpion is one-sided, and does not rise the importance of high-precision temporal changes of the net flux. This needs ot be added [Karina von Schuckmann, France]	Noted, see response to review comment No. 27111
27111	14	9	14	12	this descritpion is one-sided, and does not rise the importance of high-precision temporal changes of the net flux. This needs to be added [Eric Brun, France]	Noted. In this section we discuss the absolute magnitudes of the fluxes. Changes in the net fluxes are discussed in Section 7.2.2.
37535	14	9			It is limited precision, not accuracy, of CERES fluxes that leads to the need for re-calibration of the EBAF data [Robert Pincus, United States of America]	Accepted, we replaced "accuracy" by "precision" as suggested.
64533	14	12	14	12	I don’t' understand what "one time" means. I think you mean time-invariant [Peter Caldwell, United States of America]	Accepted. "one time" has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111125	14	12	14	13	Suggest saying “one time adjustments have been made to the paramaters in the CERES retrieval algorithm”. As written, it sounds like the CERES record is only adjusted over the 2005-2015 period. [Aaron Donohoe, United States of America]	Taken into account. The text has been revised to read as follows: "Therefore, adjustments within the uncertainty ranges of the CERES reflected solar and emitted thermal TOA fluxes were applied to the entire EBAF record to ensure that the net TOA flux for July 2005–June 2015 was consistent with the estimated Earth’s energy imbalance for the same period based on ocean heat content (OHC) measurements and energy uptake estimates for the land, cryosphere and atmosphere (Johnson et al., 2016; Riser et al., 2016; Section 7.2.2.2)."
64535	14	14	17	24	On p14 L14 you say EEI is 0.71+/-0.1 from 2005-2015 and on p15 L34 you say EEI is 0.59+/-0.14 for 2000-2015. On p 17 L 24 you quote 0.81 +/-0.14 W/m2 for 2016-2018. Perhaps you shouldn't bother mentioning the first 2 of these to avoid confusion? In any case, you seem to have forgotten to use the acronym EEI by page 15. [Peter Caldwell, United States of America]	Accepted. The text has been revised accordingly.
81781	14	15	14	16	this is not correct. the anchoring of the ceres data is based on ocean heat storage only. thus this part should be removed [Karina von Schuckmann, France]	Rejected. The reviewer is not correct. The Johnson et al (2016) paper clearly states that the 0.71 Wm-2 EEI value includes both ocean and non-ocean heat storage contributions.
27113	14	15	14	16	" and energy uptake by the lithosphere, cryosphere and atmosphere" is not correct. the anchoring of the CERES data is based on ocean heat storage only. Thus this part should be removed [Eric Brun, France]	Rejected. The reviewer is not correct. The Johnson et al. (2016) paper clearly states that the 0.71 Wm-2 EEI value includes both ocean and non-ocean heat storage contributions.
128843	14	16	15	11	This material is missing more recent pubications which give different values. Trenberth, K. E., J. T. Fasullo, K. von Schuckmann and L. Cheng, 2016: Insights into Earth's energy imbalance from multiple sources. J. Climate, 29, 7495-7505. http://dx.doi.org/10.1175/JCLI-D-16-0339.1 shows that other OHC analyses are deficient and get trends wrong because they assume no anomalies where there is no data. They find a value of 0.8 W m-2 for the ocean. This is reinforced by Trenberth, K. E., and Y. Zhang, 2019: Observed inter-hemispheric meridional heat transports and the role of the Indonesian ThroughFlow in the Pacific Ocean. J. Climate, 32, 8523-8536, https://journals.ametsoc.org/doi/pdf/10.1175/JCLI-D-19-0465.1 which uses ORAS5 in recent times (post 2005). Moreover, the local energy budgets and surface fluxes are known a LOT better than given here: see Trenberth, K. E., and J. Fasullo, 2018: Applications of an updated atmospheric energetics formulation. J. Climate, 31, 6263-6279. doi:10.1175/JCLI -D-17-0838.1. and 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. J. Climate,32, 4567-4583, https://doi.org/10.1175/JCLI-D-18-0872.1 The uncertainties in total surface fluxes are much reduced in observations to the point that one can get reliable meridional ocean heat fluxes. [Trigg Talley, United States of America]	Taken into account. An in-depth discussion of the absolute values of the EEI is given in subsection 7.2.2.2 (as referenced in the present subsection). In the present subsection we merely state that the CERES-EBAF product fluxes are adjusted in absolute terms to match estimates obtained from OHC changes on a global mean basis. We removed the absolute value of EEI given in this subsection to avoid further discussion.
36891	14	17	14	17	The adjusting of climate models is a very suspect practice because it is extremely likely that multiple different adjustments could all produce the same result. Why should anyone believe that the adjustments that have been done are correct? [John McLean, Australia]	Taken into account. Adjustments to match the observed TOA fluxes are only done on a global annual mean basis, whereas the model physics has to account for the determination of the regional, seasonal, diurnal and long-term variations of the fluxes, which then can be rigorously validated to assess the quality of the models. Text has been clarified
111129	14	17	14	19	The statement that models are adjusted to match the observational global mean SW and LW fluxes appears untrue to me. From CMIP5, the inetr-model spread (1 standard deviation) in global mean reflected SW ath the TOA is 3.5 W m^-2 and that in global mean OLR is 3.0 W m^-2. The ensemble mean bias of 1.5 W m^-2 relative to the values reported in Figure 7.3 is 1.5 W m^-2 (more reflected than observed) [Aaron Donohoe, United States of America]	Taken into account. The statement has been revised. In the tuning process of a climate model, an aim is usually to achieve TOA balances in reasonable agreement with CERES-EBAF reference values on a global mean basis. This is not achieved by every modelling group to the same degree, thus an intermodel spread remains on a global mean basis.
128847	14	23	14	23	CERES fluxes are often assumed to be a perfect measurement of radiation flux at TOA. Fact is, a lot of data processing goes into deriving TOA fluxes from measured radiances. Recommend a sentence or two on the sources of uncertainty in TOA flux to make this point clear. [Trigg Talley, United States of America]	Noted. The uncertainties of the CERES TOA fluxes are discussed in the preceding paragraph.
81783	14	23	14	23	just to come back to my previous comments: the use of therminology. The 'surface energy budget' used here, and the 'TOA energy budget': clear terms, but other terms have been used before for this. Coherence is absolutely needed through the document [Karina von Schuckmann, France]	Noted. We checked the text for a coherent treatment of the terms.
83115	14	23	14	23	This is true for the components of the surface energy budget, but not for the total. Maybe good to be clear from the start of the paragraph. [Terje Berntsen, Norway]	Accepted. We revised this sentence to read "The radiation components of the surface energy budget are associated with substantially larger uncertainties than at the TOA."
27115	14	23	14	23	The 'surface energy budget' used here, and the 'TOA energy budget' are clear terms, but other terms have been used in the text before for this. Coherence is absolutely needed throughout the document [Eric Brun, France]	Noted. We checked the text for a coherent treatment of the terms.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
36893	14	23	14	39	This is all nonsense for reasons given above for p13 lines 32-52. [John McLean, Australia]	Noted, see response to review comment No 36889
128845	14	23	14	39	[CONFIDENCE] This is another example where progress is claimed since AR5 whereas in reality progress is modest at best. Furthermore, the one area that DID progress is not emphasized. Stephens et al. (2012; Nat Geosci., DOI: 10.1038/NGEO1580) was the first to provide an extensive error estimate on all fluxes and highlighted the great challenge in describing surface fluxes in particular. Errors on the latter have not changed significantly since that paper despite what is written in lines 41-55. Also comparison to AR5 version of Figure 7.3 reveal practically no difference. The statement about increased confidence "On a global mean basis, ..." (lines 26-28) is illusionary since the uncertainties haven't changed and remain large and these "convergences" referred to are more "adjustments" within the existing (large) range of uncertainty. The one real study that represents genuine progress since AR4 are the 2015 joint studies of L'Ecuyer et al. and Rodell et al. who more carefully and objectively analyzed all uncertainties and provided optimally and jointly adjusted radiation and water fluxes done in a coupled way. It would perhaps have been better to use the L'Ecuyer et al. figure in place of Figure 7-3. [Trigg Talley, United States of America]	Taken into account. Confidence in global mean surface radiation fluxes has increased, since several entirely independent approaches came to very similar quantitative estimates for these magnitudes. L'Ecuyer et al. essentially confirmed the magnitudes of the surface radiative fluxes given in Figure 7.3 (Figure 7.2 in the final version) and AR5 based on completely different, complementary methods. While the estimates in Figure 7.3 (Figure 7.2 in the final version) take into account the information contained in direct radiation measurements provided by the worldwide surface radiation networks, L'Ecuyer et al. rely on modelled satellite-derived estimates. Also Kato et al. obtain very similar estimates, based on possibly the most advanced satellite-derived surface radiation dataset not considered in the L'Ecuyer et al. study. Thus there are multiple lines of evidence for the magnitudes of the global mean surface radiation values as given in Figure 7.3 (Figure 7.2 in the final version). This is different to the literature available for AR5, where global mean surface radiation estimates for example by Stephens et al. (2012) and Trenberth et al. (2009) considerably differed. We reformulated the paragraph to take into account the reviewers' concerns.
20403	14	28	14	30	For downward solar, the value given here applies for TOA. Since it is the average solar constant, it is probably the best known quantity over the whole figure 7.3. The trickier problems are elsewhere… [philippe waldteufel, France]	Noted. The related sentence in the SOD reads: "Best estimates for downward solar and thermal radiation at Earth's surface are thus near 185 W m-2 and slightly above 340 W m-2, respectively." Thus, the 340 Wm-2 do not refer to the downward solar radiation at the TOA, but to the thermal radiation at Earth's surface, which accidently has a very similar magnitude and thus might have lead to the confusion.
104569	14	35	14	37	A flux-tower-based observation evidence should be included here. '... remains a challenge with currently available satellite-derived datasets ...' would be revised as '... remains a challenge with flux-tower-based observations (Wilson et al., 2002; Zhou and Wang, 2016) and currently available satellite-derived datasets ...'. Reference: Wilson K, Goldstein A, Falge E, et al. Energy balance closure at FLUXNET sites. Agricultural and Forest Meteorology, 2002, 113(1-4): 223-243. Zhou, C., and K. Wang, 2016: Biological and environmental controls on evaporative fractions at ameriflux sites. J. Appl. Meteorol. Climatol., 55, 145-161. [Chunlüe Zhou, United States of America]	Noted, this sentence refers to large scale regional budgets, not to individual station observations..
128849	14	35	14	39	Regional balances require much more than can be delivered by satellite data sets so it is more than just a "challenge." The reference of Christensen et al. (2016, BAMS, DOI:10.1175/BAMS-D-14-00273.1) is one of the early examples of a regional energy budget, in this case for the Arctic, and both highlights and underscores the elements of such a regional balance that go beyond satellite observations. [Trigg Talley, United States of America]	Taken into account. The reference to Christensen et al. has been incorporated as follows: "Nevertheless, attempts have been made to derive surface energy budgets over land and oceans (Wild et al., 2015), over the Arctic (Christensen et al., 2016a) and over individual continents and ocean basins (L'Ecuyer et al., 2015; Thomas et al., 2020)."
104571	14	53	14	53	Quantification on the uncertainties of latent and sensible heat fluxes over land and their potential causes would be added after '... between 10% to 20% (L'Ecuyer et al., 2015).': 'The uncertainties in latent and sensible heat fluxes in ERA-Interim are assesed to be 13 Wm-2 and 3 Wm-2 over North America, respectively (Zhou and Wang, 2016a). These uncertainties mainly stem from unrealistic land use/cover and meteorological conditions and imperfect flux parameterizations in reanalysis (Zhou and Wang, 2016a, b).' References: Zhou, C., and K. Wang, 2016a: Evaluation of surface fluxes in ERA-Interim using flux tower data. J. Clim., 29, 1573-1582. Zhou, C., and K. Wang, 2016b: Biological and environmental controls on evaporative fractions at ameriflux sites. J. Appl. Meteorol. Climatol., 55, 145-161. [Chunlüe Zhou, United States of America]	Noted, due to space constraints, we had to reduce this section for the final draft substantially, so there remained no space to go into more detail here.
83113	14	53	14	53	Larger uncertainties in polar regions. I suppose this is in relative terms. Given the larger areas and more incoming solar energy in the Tropics, the uncertainties in terms of absolute contributions to teh global energy bedget is probably larger for the Tropics. [Terje Berntsen, Norway]	Taken into account. The polar surface energy budget uncertainty is generally larger than in other regions. For example, as shown in Table 8 of Kato et al. (2018), root-mean-square differences between observed and computed monthly mean surface downward irradiances for both shortwave and longwave are larger than in other regions (non-polar ocean and land). However, the reviewer is correct that the contribution of the uncertainty in the polar energy budget to the global mean energy budget is small.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116601	14		14		Could the chapter also provide a few orders of magnitudes related to : the amount of perturbation to the energy budget related to anthropogenic heat (this is discussed in ch 10 for regional aspects, but orders of magnitude could be worth discussing here) (several papers provide databases of heat flux). Also, I am wondering if the order of magnitude of the annual increase in energy in the climate system could be compared to human primary energy use (as an element of comparison) (I had in mind that from 2018 to 2019 the ocean heat content has increased as much as 45 times the total amount of primary energy used in the world in average per year). [Valerie Masson-Delmotte, France]	Accepted. Thanks for this useful suggestion. We included a comparative statement to aid the interpretation of the heating rates.
89177	15	1	15	6	The discussion would benefit from this new publication on CMIP6 models: https://link.springer.com/article/10.1007%2Fs00382-020-05282-7. [Gunnar Myhre, Norway]	Taken into account. We considered the reference of Wild et al. (2020) as suggested by the reviewer.
128851	15	2	15	2	The large discrepancies at the surface are in part because the models' surface energy budget is usually not "tuned", while the TOA budget is. Even if one had better constraints at the surface, would "surface tuning" be an option at all? [Trigg Talley, United States of America]	Noted, yes this is correct. In recent years we got more trust also in the global estimates of the surface energy budget, therefore in future model tuning efforts, these reference estimates could be taken into account.
128853	15	5	15	5	What is the variance in surface fluxes according to CMIP6? Only CMIP5 results are given here. [Trigg Talley, United States of America]	Taken into account, we added additional information on the surface radiative fluxes as simulated in CMIP6. However, published values on CMIP6 surface radiative fluxes refer only to global means.
128855	15	8	15	11	[CONFIDENCE] How can the TOA fluxes have become more accurate since AR5 as the overall uncertainties on each component hasn't changed, and the changes or improvements to surface fluxes are practically negligible and remain much higher than would be useful to understand changes to Earth surface energy balance over time. The confidence level is really is not different that AR5. The methodological approach to closure of energy balance (e.g., L'Ecuyer et al., 2015) is an improvement because it's much less ad hoc than other methods including those upon which Figure 7.3 is based. [Trigg Talley, United States of America]	Noted. Since AR5 the TOA fluxes have been newly processed to result in the CERES EBAF version 4.0 dataset, which profits from algorithm improvements and consistent input datasets throughout the record, see Loeb et al. 2018 for details on the improvements. Confidence in the estimation of the global mean surface radiation budget has increased since completely independent approaches result in closely matching estimates, thus provide multiple lines of evidence for the quantitative magnitudes of the global mean surface radiation budget. Estimates published in the past were much less consistent. The data sources used in L'Ecuyer et al. 2015 (satellite-derived) and in Figure 7.3 (direct observations and models) are complementary but lead to very similar all-sky results. Clear-sky estimates have not been provided by L'Ecuyer et al. 2015.
128857	15	8			The agreement between models and the Allen et al. dataset during Pinatubo needs a reference. For CESM it is shown in Fasullo and Nerem 2016, (Fig. 2). Fasullo, J.T., and R. S. Nerem, 2016: Interannual Variability in Global Mean Sea Level Estimated from the CESM Large and Last Millennium Ensembles, Water, 8 (11), 491; doi:10.3390/w8110491. [Trigg Talley, United States of America]	Not applicable. It seems that a wrong line number has been given by the reviewer. It is not clear where the comment applies.
128859	15	9	15	10	The estimates may be converging, nevertheless accuracy is still too low to be able to close the energy balance, neither at TOA nor at the surface. This needs to be stated clearly. [Trigg Talley, United States of America]	Taken into account. On a global mean basis, the energy balance can be closed within the uncertainty ranges of the individual energy balance components. This does not apply for regional energy budgets. This has been further emphasized in the revision of the text.
81785	15	10	15	11	according to the assessement, this summary does not reflect the huge uncertainties / discrepancies for the surface flux budgets, and need to be added here. [Karina von Schuckmann, France]	Taken into account, theses uncertainties refer primarily to the regional energy budgets, while the consistency in the global estimates has improved in recent years.
27117	15	10	15	11	according to the assessement, this summary does not reflect the huge uncertainties / discrepancies for the surface flux budgets, and need to be added here. [Eric Brun, France]	Taken into account, theses uncertainties refer primarily to the regional energy budgets, while the consistency in the global estimates has improved in recent years.
128861	15	10			[PROGRESS] It would be helpful to list the mean values and uncertainties in the energy fluxes at the TOA and surface for AR5 and AR6 in a table. It would show clearly the updates and improvements. [Trigg Talley, United States of America]	Noted. The numbers in Figure 7.3 (upper panel) (Figure 7.2 in the final version) have not changed substantially enough compared to AR5 to justify an own Table. However, since AR5, the magnitudes given in Figure 7.3 (upper panel) (Figure 7.2 in the final version) have been confirmed by other studies based on completely independent and complementary approaches (L'Ecuyer et al. 2015, Kato et al. 2018). This is increasing the confidence in these magnitudes.
115197	15	14	15	40	No mention of trends or changes in the clar sky greenhouse effect. We have quantified the changes due to water vapor, atmospheric temperature, and surface temperature. Values are consistent across CERES EBAF TOA observations, ERA-Interim, and GFDL AM4. CERES shows 1.07 Wm^-2decade^-1. This is an important trend that should be acknowledged. Our experiments show 0.76 Wm^-2decade^-1 with greenhouse gases changing while 0.49 Wm^-2decade^-1 without greenhouse gases changing. TOA budget changes are mentioned so you could also mention these changes in the greenhouse effect. From: Raghuraman et al., 2019: Quantifying the drivers of the clear sky greenhouse effect, 2000-2016. [Shiv Priyam Raghuraman, United States of America]	Accepted. Thanks for this reference. We now mention the increasing clear-sky greenhouse effect with a reference to Raghuraman et al. 2019.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64537	15	16	16	1	You should combine Sect 7.2.2.1 and 7.2.2.2 because TOA energy imbalance is equivalent to Earth System Warming. A lot of Sect 7.2.2 is actually talking about TOA energy imbalance. [Peter Caldwell, United States of America]	Noted. We have considered this suggestion, but in the end we decided against it. The reviewer is correct that the two quantities are equivalent, but our means of observing them are very different, so it is helpful to have separate sections. We revised the text to better link the two sections.
111145	15	21	15	24	The stated relationship between EEI and El Nino seems to contradict Johson et al (During El Niño, Pacific Warm Pool expands, ocean gains more heat: ENSO, the ocean, & Earth's energy uptake, GRL, 10.1002/2016GL071767) which claims global heat content peaks in phase with El Nino implying a quadrature phase relationship between global net TOA imbalance and El Nino. My impression is that more recent analysis that includes the atmospheric column energy tendency finds that global heat content peaks 2 months prioir to El Nino events. [Aaron Donohoe, United States of America]	Noted. Loeb et al. (2018b) show lagged regressions between CERES TOA fluxes and nino 3.4 index. They conclude the following: "Thus, a major El Niño occurring at zero lag would tend to be preceded within a year or so by an uptake of heat into the system and followed by a release of heat out of the system. This pattern is mainly driven by outgoing LW radiation (Figure 4b), which shows negative anomalies prior to an El Niño event and even stronger positive anomalies a few months following an El Niño, when surface temperatures are larger."
13515	15	22	15	22	Change “La Nina” for “La Niña”. [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Changed as suggested.
128863	15	24	15	26	This statement is not correct. On the global mean, the changes/trend evident in CERES reflected solar reported by Loeb (2019) are entirely atmospheric based and there is no significant impact of polar ice change on global mean reflected flux in that record. Naturally, there are, however, regional impacts of course but these do not translate to any significant influence on the global mean being dominated by lower latitude changes especially associated with clouds. [Trigg Talley, United States of America]	Accepted. The statement has been removed
31713	15	27	15	27	ECHAM seems quite different to the other models shown here. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this is also not clear to the authors of the related paper.
71043	15	29	15	29	Sea ice concentration is also prescribed in addition to SST [Yu Kosaka, Japan]	Accepted. We added "sea ice concentration"
681	15	33	15	43	the statement that there is high confidence (line 43) that radiative imbalance is higher in the 2000s than 1990s is not supported by the uncertainty bounds: the mean differences are well within the 90% confidence bounds. Perhaps "medium confidence"? i.e. 0.27 +/- 0.38 vs 0.59 +/- 0.14. [Bruce Wielicki, United States of America]	Taken into account. the statement has been reformulated and the related discussion shortened due to space constraints.
13517	15	36	15	36	Add space between “simulations” and parenthesis [Maria Amparo Martinez Arroyo, Mexico]	Accepted. Adjusted as suggested.
38045	15	36	15	39	The argument seems to be ambiguous. This sentence should be expalined in comparison with Fig. 7.4a. [Junhee Lee, Republic of Korea]	Noted. The reconstructions discussed here go further back in time than displayed in Figure 7.4, which only covers the period with accurate direct broadband observations from the CERES-EBAF dataset.
93071	15	38	15	39	The value given in this publication is 2.93+-0.3Wm-2K-1, which is quite large; is this in accordance with the CERES record (Loeb et al 2018), which state that the record is dominated by internal variability of the climate system? Somehow you have to relate these results. [Claudia Stubenrauch, France]	Accepted, the magnitude is suspect as it would either imply that there is no water vapour feedback or a strong negative cloud feedback since the dOLR/dTs is close to a black body "no feedback" response in their estimate. This is inconsistent with other estimates/reanalyses/modelling and relies on an older, less well calibrated record. The sentence has therefore been removed.
28861	15	39			Some assessment of the realism of the magnitude of outgoing longwave radiation change in this assessment is required since it implies a negative feedback [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, see response to review comment No. 93071.
128865	15	41	15	43	[CONFIDENCE] Research has yet to determine the extent to which the TOA measurements actually track multi-annual changes in EEI derived from the different data sources reviewed by Meyssignac et al. [Trigg Talley, United States of America]	Rejected. This is not true. Papers by Loeb et al. (2012), Johnson et al. (2016) and Trenberth et al. (2014, 2016) did just that.
81787	15	41	15	43	According to my comment at ES level: this would be the place to add the range of estimates for both periods as obtained from the literature assessment, particulalry also to better quantify this statement, and/or to propose a value (or value range) of this changes between the 2000 onwards period and the 90s. [Karina von Schuckmann, France]	Noted, see response to review comment No. 27119.
27119	15	41	15	43	this would be the place to add the range of estimates for both periods as obtained from the literature assessment, particulalry also to better quantify this statement, and/or to propose a value (or value range) of this changes between the 2000 onwards period and the 90s. [Eric Brun, France]	Noted, the related discussion had to be largely reduced due to space constraints in the FGD.
128867	15	42	15	42	How can one be absolutely sure that the variations (amplitude and phase of variability) in CERES data reflect reality? What is the approach to validate this? The reconstruction is likely of much lower quality and requires gap filling. How can one trust the derived energy imbalance variability with such high confidence? (R. Allean's work on the deep-C net radiative flux data explains the gap filling process) [Trigg Talley, United States of America]	Noted. Because independent measurements on different satellites show the same amplitude and phase of variability. This has been published in Loeb et al. (2018b; see their Figure 3).
71045	15	42	15	43	There is overlap between the 1985-99 and 200-2015 EEI estimates. The high confidence assessment on the EEI increase still holds? [Yu Kosaka, Japan]	Noted, see response to review comment No 681

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83111	15	47	15	47	Is it possible to give an uncertainty range for the CERES data in fig 7.4? [Terje Berntsen, Norway]	Noted. The uncertainty of the monthly anomalies is < 0.2 Wm-2 for SW and LW and < 0.3 Wm-2 for net TOA flux. The reference for this is Table 2 of Loeb et al. (2018b). However, due to space constraints, we cannot go into these technicalities here.
36895	15	47	15	54	Figure 7.4: Anomalies from 12-month running means? In 2009 I was accused, along with my fellow authors, of mis-using 12 month running averages (essentially taking monthly spikes and flattening across 12-month periods). We were not permitted to respond publically to the criticisms levelled at our paper but here Loeb is taking this even further by deriving new values from those 12-month means. If we were wrong to use 12-month running means then Loeb is even more incorrect. [John McLean, Australia]	Noted. We do not understand what is "wrong" with applying a 12-month smoother to monthly data. The Loeb et al (2020) paper where the data in Figure 7.4 are taken from shows both monthly and 12-month mean results.
103609	15	49	15	49	dotted [Philippe Tulkens, Belgium]	Accepted. Changed as suggested.
27121	16	1	16	1	We have a concern with the term "total Earth system warming" in the title. Literature mostly uses 'Earth heat inventory' or equivalent. 'Earth system warming' is a wording which is used already earlier in this chapter, but had been not introduced or clarified before - and this can induce confusion. [Eric Brun, France]	Accepted. We have we have removed this term from Chapter 7 and the wider AR6 report.
117269	16	1	16	55	can you please clarify why the periods analysed start in 1971 and 2006? [Maisa Rojas, Chile]	Taken into account. The assessment periods were chosen across several chapter author teams based on observing system capability and consistency with previous IPCC assessment reports.
46087	16	3	16	4	In principle heat can also be stored in terrestrial vegetation. I believe this component should be mentioned. If it can be neglected, that should also be mentioned. [Twan van Noije, Netherlands]	Noted. We are not aware of any literature on which an assessment could be based.
27123	16	3	16	4	We have a concern with the term "total Earth system warming". Literature mostly uses 'Earth heat inventory' or equivalent. 'Earth system warming' is a wording which is used already earlier in this chapter, but had been not defined before - and this can induce confusion. [Eric Brun, France]	Accepted. We have we have removed this term from Chapter 7 and the wider AR6 report.
104909	16	5	16	10	Please include a reference for negligible geothermal heat flux. Please include reference on GCOS/vonSchuckmann et al. paper on energy inventory. [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The von Schuckmann et al paper is cited as part of the assessment. Since we are mainly concerned with characterising the energy imbalance relative to pre-industrial (see Box 7.2), the time-invariant geothermal heating can be neglected.
27125	16	6	16	8	It is not correct. its the rate of change of OHC and not the change itself which dominates the change in warming. [Eric Brun, France]	Taken into account. We now refer to the rate
14863	16	7	16	8	How much did the OHC cahnge and varied in the remote past compared to the present past? [Marie-France Loutre, Switzerland]	Noted. This is beyond the scope of the material presented in section 7.2, which focusses on the instrumental period. However, there is some discussion in Chapter 2 which deals with observed changes in the climate system.
81789	16	7	16	8	not correct. its the rate of change of OHC. [Karina von Schuckmann, France]	Taken into account. We now refer to the rate
112031	16	8	16	8	OHC is not defined [Cynthia Randles, United States of America]	Noted. OHC is defined in the preceding sub-section.
2681	16	8			define OHC [Bryan Weare, United States of America]	Noted. OHC is defined in the preceding sub-section.
46089	16	12	16	28	Can we make similar statements about the CMIP6 models? [Twan van Noije, Netherlands]	Not applicable. The paragraph has been removed. evaluation of CMIP6 models is presented in chapter 3.
36897	16	13	16	15	Here you go again, trying to imply that the notion of "is consistent with" is equivalent to proof, which is untrue. [John McLean, Australia]	Not applicable. The paragraph has been removed. evaluation of CMIP6 models is presented in chapter 3.
104911	16	13	16	22	Cheng et al. uses covariance from CMIP5 models to infill observational gaps, particularly in deeper layers and backwards in time. Thus, how validity is to compare CMIP5 simulations with Cheng et al. estimate? Is the same argument valid for Smith et al. 2015? (MOSORA) [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been removed.
83747	16	15	16	17	"the spatial patterns of historical climate change may not have evolved in 17 the same way as reality for many climate models." I feel it's necessary to differentiate here between the spatial patterns simulated by coupled climate models due to different (but plausible) representations of internal variability, and the evolution of the forced response. The former is not necessarily a concern for model evaluation against observations, but the latter would suggest a model-observation discrepancy. [Marvel Kate, United States of America]	Not applicable. The paragraph has been removed.
31715	16	17	16	17	"implies" - We know that the feedbacks and forcings do differ among models, so isn't it more than an implication? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been removed.
81791	16	17	16	18	Are these the only reasons ? No references given. What about the representation/process of the ocean ? [Karina von Schuckmann, France]	Not applicable. The paragraph has been removed.
83117	16	17	16	18	I don't quite agree this implications. If there is also a significant variability between the models in the ocean circulation, then equal forcings and feedbacks could give very different rseponse in OHC change [Terje Berntsen, Norway]	Not applicable. The paragraph has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27127	16	17	16	18	Are these the only reasons ? No references are given. What about the representation/process of the ocean ? [Eric Brun, France]	Not applicable. The paragraph has been removed.
104913	16	20	16	40	There are studies showing that changing ocean circulation is important for ocean heat uptake (see chapter 9). In this light, what is the assessment in terms of the representativeness of OHC reconstructions that assume time-invariant circulations? (i,e, passive ocean heat uptake). The lower degree of confidence is not explained. Overall, should this assessment about observed OHC estimate be referred to chapter 2? While chapter 7 would only make use of the OHC estimates? (to be consistent with what has been done for several observed estimates used in other sections of chapter 7). [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraphs have been removed. Please see Chapter 2 for OHC assessment.
81793	16	28	16	28	rate of change of OHC [Karina von Schuckmann, France]	Not applicable. The paragraph has been removed.
27129	16	28	16	28	"Change of OHC" should be replaced with "rate of change of OHC" [Eric Brun, France]	Not applicable. The paragraph has been removed.
36899	16	30	16	32	This conflicts with your notions of a two-dimensional instantaneously balanced energy budget back on page 13. Decide which claim is correct and remove the claim that's not. [John McLean, Australia]	Not applicable. The paragraph has been removed.
81795	16	32	16	32	There is a need to add a reference (or references) to see from which assessment this outcomes comes from. [Karina von Schuckmann, France]	Not applicable. The paragraph has been removed.
27131	16	32	16	34	There is a need to add a reference (or references) to see from which assessment this outcomes comes from. [Eric Brun, France]	Not applicable. The paragraph has been removed.
14859	16	33	16	33	much further back'. Please be more specific (100yr? 1kyr? 10kyr? 100kyr? More?). [Marie-France Loutre, Switzerland]	Taken into account. This text has been moved to Chapter 2.
81797	16	36	16	36	unprecise: this is not the case for the surface fluxes, only for the net flux at TOA, text should be revised accordingly [Karina von Schuckmann, France]	Taken into account. revised version of this text appears in the preceding section
27133	16	36	16	36	"comparisons of satellite radiative fluxes" is unprecise: this is not the case for the surface fluxes, only for the net flux at TOA, text should be revised accordingly [Eric Brun, France]	Taken into account. revised version of this text appears in the preceding section
38047	16	37	16	40	The authors may want to refer to Fig. 1 in Box 7.2. [Junhee Lee, Republic of Korea]	Not applicable. The paragraph has been removed.
22127	16	38	16	40	Given that this was the charge of chapter 2 shouldn't this repeat the assessment of chapter 2 and cross-reference. Otherwise this is asking readers to play a game of spot the difference here. [Peter Thorne, Ireland]	Accepted. The assessment summary statements have been moved to Chapter 2.
27135	16	38	16	40	This sentence is not well placed, and needs to be interlinked with the following senetce, as this information is chapter 2 assessment task, and the interlinkage needs to be added here. [Eric Brun, France]	Accepted. The assessment summary statements have been moved to Chapter 2.
27137	16	44	16	46	the link to chapter 2 is sufficient, and the method does not need to be repeated here - there is a danger to induce inconsistencies in case of modifications in chapter 2 which are not coordinated with this chapter; Already the list of publications given here does not reflect all the literature assessed in chapter 2 [Eric Brun, France]	Accepted. The assessment summary statements have been moved to Chapter 2.
81799	16	44	16	46	the link to chapter 2 is sufficient, and the method does not need to be repeated here - there is a dange to induce inconsistencies in case of modifications in chater 2 which are not coordinated with this chapter; Already the list of publications given here does not reflect all the literature assessed in chapter 2 [Karina von Schuckmann, France]	Accepted. The assessment summary statements have been moved to Chapter 2.
6691	16	46	16	49	Table 7.1 covers the period 1971-2018, but the microwave satellite data that are used in the calculation for the atmosphere come from a set of instruments of which the first was launched in October 1978. It should be explained how atmospheric energy was calculated for 1971-1978. A sophisticated calculation for the atmosphere is not needed here as the atmospheric component of Earth-system heating is small, but the rather crude calculation following what was done in AR5 could have been done using a reanalysis, as this has been shown in peer-reviewed post-AR5 literature to provide estimates of atmospheric energy that are of a similar order of magnitude to those calculated for AR5, and for which comparison of global trends and variability has been made with the trends and variability familiar from the GMST and GSAT datasets. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The atmospheric heating rates are based on Steiner et al (2020) and we refer the reviewer to that publication for further details.
36901	16	48	16	48	I think you'll find that Christy and Spencer were the first to publish about the lower tropospheric and stratospheric temperatures, not late-comers Mears and Wentz. [John McLean, Australia]	Noted.
72143	16	49	16	50	Next to borehole temperature profiles, the study by Gentine et al. also estimated heat fluxes into the land surface using ground heat flux plate data and land-surface simulations with deep soils. [Inne Vanderkelen, Belgium]	Noted. Adding these details are not necessary here

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28863	16	49			I could not find information in section 2.3.1.3 or the appendix in Chapter 7 showing how the calculation of atmospheric energy balance (or land surface) was computed. The references imply that they provide values but in some cases they only produce temperature trends e.g. Mears & Wentz. Can reanalysis estimates of atmospheric energy (including temperature, moisture but also kinetic energy) be used? A back of the envelope calculation using 0.15K/decade, 1020 J/kg/K heat capacity of moist air and ~7500 kg/m2 tropospheric mass suggests 0.004 Wm-2 with an extra 0.002 Wm-2 from evaporation of moisture (24 kgm-2 *1%/decade*L) while reported reanalysis estimates of 0.01 PW imply 0.02 Wm-2 (Liu et al. 2015; AR4 Figure 5.4). For the land surface, the paper of Gentine et al. is quoted but this provides a new higher value of 0.12-0.36 Wm-2 for the land which globally would be about 0.07 Wm-2 globally, double the value quoted. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The atmospheric heating rates are based on Steiner et al (2020) and we refer the reviewer to that publication for further details.
72199	16	50	16	50	In a recent study (doi: 10.1029/2020GL087867), the heat uptake by inland waters, including lakes, reservoirs and rivers is quantified based on a combination of lake models, hydrological models and Earth System models. Compared to the other components of the Earth system, this is a small value, yet we think this is a non-negligible component to include. The following sentence is a suggestion to include this study in the report: “Energy uptake by inland waters are estimated using lake models, hydrological models and Earth System models. (Vanderkelen et al., 2020)." [Inne Vanderkelen, Belgium]	Accepted. The reference has been added.
128869	17	1	17	1	[CONFIDENCE] In Table 7.1 and associate narrative, these uncertainties on the ocean component do not represent total uncertainty. [Trigg Talley, United States of America]	Taken into account. The treatment of uncertainties has been revised substantially following the approach described by Palmer et al (2021) which explicitly accounts for both structural and internal uncertainty.
104915	17	1	17	21	Need for coordination across chapters 2, 7 and 9 for global OHC contribution. [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. This has been coordinated
27139	17	3	17	3	products / methods / literature chosen for this table is not conistsent with those used in chapter 2. Moreover, a community work (submitted under IPCC time frame, and manuscript send to TSU) on these estimates has been not considered: ttps://doi.org/10.5194/essd-2019-255 [Eric Brun, France]	Taken into account. The results from that publication are included as part of our assessment.
20077	17	3	17	3	Table 7.1: Zetta or Zeta? [philippe waldteufel, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
36903	17	3	17	8	Using extrapolated values lacks integrity. It simply assumes that 2006 to 2015 conditions will continue, but that's not necessarily true. Mind you, we also haven't been told anything about the variability of the data and the reader is left to assume constant change. [John McLean, Australia]	Noted. Extrapolation was used only to provide approximate numbers during the report drafting process. The FGD does not use extrapolated values.
36905	17	3	17	8	I doubt very much that the coverage of temperature measurements below 700 metres was homogenous and widespread in 1971, in fact I doubt that coverage of any of the four listed factors were back then or for some years after. You've ignored the important matter of data coverage, much like IPCC reports do about HadCRUT4 temperature data.. [John McLean, Australia]	Noted. The data issues are discussed further in Chapter 2. The uncertainty due to limited and heterogeneous data coverage is explicitly accounted for in the methods used to generate the estimates of global ocean heating.
81801	17	3	17	9	According to my comment in the ES, I am wondering whether there is an attempt to consider also an 'assessment approach' for the time series, or at least for the numbers published, prviding a range of outcomes. Additionally, the time series / products chosen do not consider the variety of scientific products available over the world, and there is nos pecification given why these specific products had been considered only. There had also been a community effort been recently submitted (under IPCC timeline, which is currently under review, and which considers the international available product range. https://www.earth-syst-sci-data-discuss.net/essd-2019-255/ [Karina von Schuckmann, France]	Taken into account. The treatment of uncertainties has been revised substantially following the approach described by Palmer et al (2021) which explicitly accounts for both structural and internal uncertainty. This revised estimate uses an ensemble approach with a larger number of ocean heat content input data sets. Further details are available in chapter 2.
83119	17	6	17	6	Table 7.1 Is it really true that the relative uncertainty in the OHC change for the 700-2000m depths is much lower that for the other layers? [Terje Berntsen, Norway]	Taken into account. The treatment of uncertainties has been revised following the More comprehensive approach described by Palmer et al (2021) and values have increased accordingly.
22129	17	6	17	7	The percentages should not be reported with greater precision than the numbers they are based upon. Thus all percentages should also be shown with ranges here. [Peter Thorne, Ireland]	Taken into account. For brevity, we do not include uncertainty ranges on the percentages since they are there to give the reader an impression of the relative contributions.
72145	17	6	17	7	In Table 7.1 the share of Heat Gain by inland waters could be included. Based on the calculations in Vanderkelen et al. (doi: 10.1029/2020GL087867), these are 0.38 ± 0.28 ZJ for 1971-2018 and 0.12 ± 0.14 ZJ for 2006-2018, and are directly retrieved from the simulations. This results in a relative contribution of 0.09% of the total heat gain, which is of course, very small but non-negligible. [Inne Vanderkelen, Belgium]	Taken into account. See main text.
2683	17	6			bold the final line, since this is what is discussed in the text [Bryan Weare, United States of America]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71047	17	11	17	19	Rathar than the ocean heat absorption of ">90%", why don't you specifically say "about 92%"? [Yu Kosaka, Japan]	Taken into account. The numbers have been revised and we now use 91%
128871	17	12	17	12	[CONFIDENCE] Suggest the OHU uncertainty provided is too optimistic; further assessments are needed to improve the definition, let alone the magnitude, of uncertainty from different approaches. Not in favor of assigning high confidence to the single OHU values proved here. But there is high confidence that EEI is positive and likely within a certain range. The authors could make use of a larger variety of in-situ datasets available. Here, their analysis relies on only a few (Table 7.1). [Trigg Talley, United States of America]	Taken into account. The treatment of uncertainties has been revised substantially following the approach described by Palmer et al (2021) which explicitly accounts for both structural and internal uncertainty. This revised estimate uses an ensemble approach with a larger number of ocean heat content input data sets. Further details are available in chapter 2.
13519	17	13	17	13	Add space between “area” and parenthesis. [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
27141	17	16	17	16	taking into account the results of the paper: https://doi.org/10.5194/essd-2019-255, ">90%" shoudl be changed to 'about 90%' [Eric Brun, France]	Taken into account. The results from that publication are included as part of our assessment.
128873	17	17	17	17	[CONFIDENCE] Uncertainties in the satellite approach are too large to claim closure of the sea level budget. Further and more rigorous evaluations are needed. [Trigg Talley, United States of America]	Taken into account. The treatment of uncertainties has been revised substantially for key elements of the sea level budget following the approach described by Palmer et al (2021) which explicitly accounts for both structural and internal uncertainty. Further details on assessment of the sea level budget are available in Chapter 9.
36907	17	22	17	50	Your notions of a balanced instantaneous energy budget are laughable. The hottest part of the day is usually after the time of greatest insolation and the warmest and coldest times of the year for most latitudes are about six weeks after the respective solstice. Heat is being stored, mainly in the oceans but some in the ground surface, and then released later OR, in the case of delayed cold temperature, the oceans and land continue to cool. And then there's poleward transport of energy, which you discuss in section 7.2.2.4, apparently unaware of the contradiction between that and a balanced energy budget. [John McLean, Australia]	Noted. Here we do not discuss an instantaneous energy budget but rather the long term mean state.
83123	17	24	18	4	The introduction to this section hints at dimming/brightning is a forcing, while in the context here it is a combination of forcing and feedbacks (e.g. BVOC, wildfires, natural dust, sea salt etc.). Please clarify. [Terje Berntsen, Norway]	Taken into account. Dimming/brightening can be considered as a forcing to the extent that it is anthropogenically forced through air pollution. A discussion of the role of anthropogenic-induced versus natural variations is given in the subsequent text. We changed the wording from "impacted" to "interacted with" for a more balanced statement.
104541	17	28	17	28	Citation of Du et al., (2017) is not appropriate. Our that paper adopts daily maxmum and minimum temperatures. Instead, I suggest citing Zhou et al., (2018) that uses daily mean temperature from in-situ observations and twelve reanalyses to investigate the impact of changes in solar radiation on regional warming rates in the past decades. Reference: Zhou, C., Y. He, and K. Wang, 2018: On the suitability of current atmospheric reanalyses for regional warming studies over China. Atmos. Chem. Phys., 18, 8113-8136. [Chunlüe Zhou, United States of America]	Accepted. We added the reference of Zhou et al. (2018).
116603	17		17		Is it possible to be more explicit on changes from AR5 to AR6 on these aspects? [Valerie Masson-Delmotte, France]	Accepted. The text has been revised to include an explicit comparison with AR5.
14865	18	5	18	5	What is the evidence for dimming and/or subsequent brightening inthe (remote) past? [Marie-France Loutre, Switzerland]	Noted. Unfortunately we have no information on variations in surface solar radiation before measurements were made.
117271	18	5	18	50	I wonder why there isn't any figure accompanying the discussion of dimming and brightening? [Maisa Rojas, Chile]	Noted. This is primarily due to lack of space.
104581	18	11	18	11	It would be better to cite a recent study on variability of direct and diffuse solar radiation. Text could be added before 'Since AR5, ...': 'More specifically, He and Wang (2020) provided a picture that direct solar radiation decreased by -3.52 Wm-2 decade-1 whereas diffuse solar radiation increased by 0.84 Wm-2 decade-1 from 1958 to 1989 during the global dimming, and both slightly decreased thereafter over China.' Reference: He, Y., and Wang, K., Variability in direct and diffuse solar radiation across China from 1958 to 2017. Geophysical Research Letters, 2020, 47(1): e2019GL084570. [Chunlüe Zhou, United States of America]	Noted. The statement on page 18 line 11 in the SOD refers to the fact that dimming and brightening is not an artefact of inhomogeneous records, but remains evident also after careful data quality control and homogenization. A statement on diffuse/direct radiation changes does not fit here and there is severe space limitation which does not allow to go in detail into the different studies covering the diffuse and direct components.
128875	18	26	18	26	The surface downwelling shortwave radiation form CMSAF Meteosat data is mostly a modeling result and highly depends on its inputs, such as aerosol information. Most CMSAF SIS products use aerosol climatologies. Therefore the effect of aerosol on shortwave radiation at the surface is neglected, which might yield biased trends. [Trigg Talley, United States of America]	Accepted. Changes in surface solar radiation in these CMSAF products are primarily induced by changes in cloudiness since aerosols are specified as temporally invariant. We discuss the role of clouds and aerosol on surface solar radiation trends in subsequent paragraphs and refer there to Pfeilroth et al. as a study based on CMSAF products which argues that changes in cloudiness could have contributed to the surface solar radiation trends.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83121	18	26	18	26	Could you be more specific on what is the area menat by: the area in view of the geostationary satellite Meteosat? [Terje Berntsen, Norway]	Accepted. We added "which views Europe, Africa and adjacent oceans."
100901	18	29	18	31	Added to this, the decadal changes in surface solar radiation were also found in the zenith and maximum transmittance data (i.e. clear sky conditions) in Japan since 1930s (Fig. 13 of Tanaka et al. 2016, ACP, doi:10.5194/acp-16-13969-2016). [Katsumasa Tanaka, France]	Accepted. We refer here now also to Japan and added the reference of Tanaka et al. 2016.
111149	18	29	18	41	The contribution of shortwave absorption by atmospheric water vapor to surface diming should be discussed -- both in the context of historical observed and anticipated future changes. This feedback was estimated to be 1 W m^-2 per K (absorbed in the atmopshere whch would equate to changes in downwelliong solar) by Donohoe et al. (2014 -- Shortwave and longwave radiative contributions to global warming under increasing CO2) using the convolution of radiative kernels and CMIP5 specific humidity changes though I'm sure there are more thorough estimates. [Aaron Donohoe, United States of America]	Accepted. We added a (due to space constraints only brief) statement on the role of water vapour in this context. Several studies indicate that water vapour and other radiatively active gases in the atmosphere play only a minor role in the dimming and brightening trends, and related references have been added. Also the sensitivity of 1 W m-2 per K mentioned by the reviewer suggests that water vapour contributed considerably less than 1 Wm-2 over the past decades to the surface solar radiation trends, which is thus a minor contribution compared to the observed magnitudes of the trends.
20405	18	38	18	41	This is certainly true, but what about possible cloud effects not associated with aerosols? [philippe waldteufel, France]	Noted, only aerosol related changes in cloudiness depend on the pollution levels as stated here. Changes in cloudiness unrelated to aerosols are still possible.
128877	18	39	18	39	A discussion of possible water vapor effects on the near-IR portion of the shortwave spectrum is needed. Is there any literature on enhanced solar absorption due to water vapor and what does it suggest? [Trigg Talley, United States of America]	Noted, this issue has been addressed in response to review comment No. 111149
6693	18	43	18	43	Reanalyses may indeed in general not represent the full effects of dimming and brightening in the radiation calculations used in their background models, but they may recover much of the consequential changes in temperature through their assimilation of observational data. Moreover, the relatively new ERA5 reanalysis does include CMIP5-specified total solar irradiance and aerosols in its background model. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, we mention the beneficial impact of including time-dependent aerosol in reanalyses in the subsequent sentence: "The inclusion of assimilated aerosol optical depth inferred from satellite retrievals in the MERRA2 reanalysis (Buchard et al., 2017; Randles et al., 2017) helps to improve the accuracy of the simulated surface solar radiation changes in China (Feng and Wang, 2019).
98653	18	44	18	45	Moseid et al ACPD 2020 have updated the Storelvmo comparison of downwelling surface radiation with CMIP6 model output. There is also in the current model generation a dimming and brigthning trend which is not easily reconcilable with surface observations. [Michael Schulz, Norway]	Accepted. We added the reference of Moseid et al. ACP
77391	18	45	18	45	Is there a listing of the Emissions data used for these assessments? A reference should be provided. [Emer Griffin, Ireland]	Noted. For this general statement, we cannot list all references of emission data that are used in these various studies, as they are dependent on model generation, reanalysis type, and emission source.
112025	18	45	18	47	Here you should cite the actual MERRA-2 aerosol reanalysis papers (Randles et al. 2017; Buchard et al. 2017) rather than just another group’s analysis of this dataset. [Cynthia Randles, United States of America]	Accepted. The references Randles et al. 2017 and Buchard et al. 2017 have been added.
107643	18	47	18	50	are there any citations to support this point? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We added two related references.
28865	18	53			The warming and moistening of the atmosphere dominate over well mixed greenouse gas increases in determining increased downward thermal radiation, particularly at lower latitudes e.g. Allan 2009 J. Clim http://dx.doi.org/10.1175/2008JCLI2616.1; Ma et al. 2014 JGR https://doi.org/10.1002/2013JD021427 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. We updated the related statement and added "..and the warming and moistening of the atmosphere".
116605	18		18		Coordination with ch 2, 3, 6 on dimming / brightening and developing common clear messages (for TS) is needed. Aspects related to the "lack of natural variability" in models need to refer to the corresponding assessment of chapter 3 for coherency. [Valerie Masson-Delmotte, France]	Taken into account, these issues have been coordinated.
41493	19	3	19	3	This is an awkward jump from dimming and brightening to turbulent fluxes: maybe add a topic sentence 'Turbulent fluxes of latent and sensible heat are also an important part of the surface energy budget (Figure 7.3)' [Andrew Gettelman, United States of America]	Accepted. We added this statement as suggested.
128879	19	3	19	17	Total surface fluxes are best estimated by indirect means: see Trenberth, K. E., and J. Fasullo, 2018: Applications of an updated atmospheric energetics formulation. J. Climate, 31, 6263-6279. doi:10.1175/JCLI -D-17-0838.1. and 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. J. Climate,32, 4567--4583, https://doi.org/10.1175/JCLI-D-18-0872.1 The uncertainties in total surface fluxes are much reduced in observations to the point that we can get reliable meridional heat fluxes. [Trigg Talley, United States of America]	Noted. The discussion in this paragraph relates to the trends in the turbulent fluxes of sensible and latent heat.
128881	19	5	19	5	What is meant by "annual seasonality"? [Trigg Talley, United States of America]	Accepted, we removed "annual"
107645	19	5	19	5	annual seasonality? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, we removed "annual"

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104575	19	16	19	17	This sentense would be revised as: 'Meanwhile, there was also progress in developing evaporation fraction dataset (the ratio of latent heat flux to the sum of latent and sensible heat fluxes) (Zhou and Wang, 2016) and directly benchmarking the terrestrial sensible heat flux (Siemann et al., 2018).' Because evaporation fraction parameterization developed by Zhou and Wang (2016) has been applied to estimate regional and global turbulent heat fluxes by peers, such as Beringer et al., (2017) and Chen et al., (2018). Reference: Zhou, C., and K. Wang, 2016: Biological and environmental controls on evaporative fractions at ameriflux sites. J. Appl. Meteorol. Climatol., 55, 145-161. [Chunlüe Zhou, United States of America]	Noted. The intention is not to review new datasets, but the scientific results of high relevance for this chapter that come out of them. Also, this paragraph had to be shortened substantially for the final version due to space constraints.
2685	19	19	19	20	what is meant by detected at many more locations also in remote areas? What map supports this? [Bryan Weare, United States of America]	Noted. Supporting evidence is given in the second paragraph of this subsection and the references listed in there.
38049	19	19	19	25	The meaning of "multidecadal trends" is not well understood. Please clarify this. [Junhee Lee, Republic of Korea]	Noted. We do not find a better expression that describes the temporal scales the trends apply in concise form, and do not want to further expand on this due to space limitations.
683	19	21	19	21	"high confidence that these trends are of widespread nature, and not only a local phenomenon or a measurement artefact" seems to need more discussion. Local radiative fluxes (like precipitation) are highly variable while surface radiative flux measurements represent an area of about 5 by 5km in area. Since the satellite estimates (Kato et al. 2018:line 23 of text) did not show this globally, nor did models including aerosols, how consistent are the surface local decadal anomalies with satellite 100km grid boxes? This should be easy to determine and if consistent would support the argument. otherwise perhaps "medium confidence" [Bruce Wielicki, United States of America]	Taken into account, The 5 x 5 km scale applies for the representativeness of near-instantaneous radiation fields. Several studies show that surface solar radiation measurements on monthly and longer timescales can represent the radiation climatologies and trends on far larger scales up to several 100 km (e.g. Hakuba et al. 2014 JGR; Schwarz et al. 2017 JGR; Sanchez Lorenzo et al. 2015 JGR). We added a related statement on the representativeness of surface radiation measurements for the larger scale radiation climatologies and variations.
107647	19	21	19	22	"origins need further investigation" this sounds policy prescriptive and like a research recommendation [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, we replaced "the origins need further investigation" with "the origin of these trends is not fully understood," to sound less policy prescriptive
71049	19	22	19	23	"anthropogenic aerosols might have substantially contributed" The precending paragraphs do not say that the aerosols are of anthropogenic origin. [Yu Kosaka, Japan]	Taken into account. We expanded the related discussion in the fourth paragraph of Section 7.2.2.3, which now reads as follows: "For Europe and East Asia, modelling studies also point to aerosols as an important factor for dimming and brightening by comparing simulations that include/exclude variations in anthropogenic aerosol and aerosol precursor emissions (Golaz et al., 2013; Nabat et al., 2014; Persad et al., 2014; Folini and Wild, 2015; Turnock et al., 2015; Moseid et al., 2020). Moreover, decadal changes in surface solar radiation have often occurred in line with changes in anthropogenic aerosol emissions and associated aerosol optical depth (Streets et al., 2006; Wang and Yang, 2014; Storelvmo et al., 2016; Wild, 2016; Kinne, 2019)."
99	19	30	20	48	Box 7.2 is on the global energy budget, what is meant here is the atmspheric TOA budget. It might be useful to relate somewhere in the box to the global energy budget as in WGIII, which would relate to energy sources (notably coal, oil and gas) which have historically been used, and the implications of using emissions-intensive energy sources in future - a different meaning of 'global energy budget'. WGIII ch 3 and ch 6 may be relevant, and you might look at a recent report on the production gap - SEI, IISD, ODI, Climate Analytics, CICERO & UNEP 2019. The production gap: The discrepancy between countries' planned fossil fuel production and global production levels consistent with limiting warming to 1.5°C or 2°C. http://productiongap.org/ [Harald Winkler, South Africa]	Noted. These energy comments refer to fossil fuel production and are outside of the scope of this section
22133	19	30			I found this box overall very hard to follow. Some efforts to increase accessability to a non-domain expert would clearly be worthwhile. The figures are good but several aspects of the first figure in particular could be made more explicit and obvious by e.g. adding labels and brief text within the panels to aid reader interpretation. There is white space in most panels to do so. [Peter Thorne, Ireland]	Taken into account. The language has been simplified and the figures improved
27143	19	33	19	34	The mention of "the excess …" is not valid for a general definition of the global energy budget - this is only valid under conditions of a positive imbalance. The text needs to be revised accordingly. [Eric Brun, France]	Taken into account. Text has been made clearer that we refer to the imbalance
81803	19	33	19	34	this is not valid for a general definition for the global energy budget - this is only valid under conditions of a positive imbalance. The text needs to be revised accordingly. [Karina von Schuckmann, France]	Taken into account. Text has been made clearer that we refer to the imbalance
71051	19	39	19	39	radiative ERFs -> ERFs [Yu Kosaka, Japan]	Editorial edit accepted.
3521	19	40	19	42	This might be because the figure is not final, but the lower bound for ERF in the figure is >> 44, and the dotted line is > 825 [Joyce Penner, United States of America]	Noted. The figure has been revised substantially.
52779	19	41	19	41	space needed between 95% and range [Monika Sikand, United States of America]	editorial change made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23885	19	41	19	41	space missing after %... [Branko Grisogono, Croatia]	editorial change made
103611	19	41	19	41	space in front of "range" [Philippe Tulkens, Belgium]	editorial change made
22131	19	41	19	42	But panel b is ERF not total energetics (according to the caption) and from the figure I cannot for the life of me work out where this huge range comes from. Is the lower bound really so close to zero? Maybe I misunderstand the text / figure / caption in which case I would suggest some clarifications are required for those not immersed in this topic a chance to follow along. [Peter Thorne, Ireland]	Taken into account. The underlying assessment and the figure have been revised.
72153	19	41	19	42	The values given for ERF since 1971 seem not to correspond with Box 7.2 Figure 1 panel b, both for the estimate as for the uncertainty range. [Inne Vanderkelen, Belgium]	Taken into account. The numbers have been corrected and now agree
27145	19	42	19	42	Although the figure is indicated as 'placeholder' it is not clear where the values / time series are comming from, and which products, methods etc have been used. And how some of those products are coherent with other chapters (e.g. chapter 2). [Eric Brun, France]	Taken into account. The code and data for all plots will be made available as part of the final report.
46091	19	46	19	49	Please clarify if a correction is applied to account for the response of surface air temperatures over land in such simulations, and, if not, explain why this hasn't been done. [Twan van Noije, Netherlands]	Not applicable. The text and methods have been revised.
72155	19	52	19	52	The values given for radiative response seem not to correspond with Box 7.2 Figure 1 panel c, both for the estimate as for the uncertainty range. [Inne Vanderkelen, Belgium]	Taken into account. The assessment text and figure have been revised.
46093	19	54	20	1	These quoted numbers are not consistent with the graph in the figure. Is this because the figure hasn't been updated to CMIP6 yet? [Twan van Noije, Netherlands]	Taken into account. The assessment text and figure have been revised.
3523	19	54	20	1	Again, perhaps this is because the figures are not final but the numbers quoted here do not match those in Fig 1f [Joyce Penner, United States of America]	Taken into account. The assessment text and figure have been revised.
64539	19	55	19	55	A mean estimate of energy change being negative from 1971-2018 with bounds ranging from positive to negative 600 ZJ suggests to me that the methodology is useless. I suggest deleting this box, particularly since it depends on feedback analysis you haven't discussed yet. [Peter Caldwell, United States of America]	Taken into account. The assessment text and figure have been revised.
72157	19	55	149	55	The values and uncertainty ranges of Radiative Forcing + Response given in this line are not corresponding to the ones shown on Box 7.2 Figure 1 panel f. The values in the tekst are consistent, to each other, leading to a negative implied energy change. The values on the figure suggest a positive energy change. [Inne Vanderkelen, Belgium]	Taken into account. The assessment text and figure have been revised.
96709	20	1	20	1	Please explain what we learn from Box 7.2, Fig. 1f. It is confusing for non-scientists that the mean value of the purple bar is lower than the orange one. [Nicole Wilke, Germany]	Taken into account. The assessment text and figure have been revised.
3525	20	1	20	4	Because the figure caption terminology differs from that used here, I could not figure out what was being compared in Fig 1f. I would change the figure caption to match this phrasiology. [Joyce Penner, United States of America]	Taken into account. The assessment text and figure have been revised.
72159	20	2	20	3	Consistent with the previous comments, the Total Earth System Warming of 406 ZJ is not corresponding to the value in Box 7.2, Figure 1 panel f. [Inne Vanderkelen, Belgium]	Taken into account. The assessment text and figure have been revised.
128883	20	3	20	3	The authors mean "increase in ocean heat content" not "storage". [Trigg Talley, United States of America]	Taken into account. Ocean heat content use as corrected
27147	20	5	20	6	Depending on the sensisitvity of the definition of the term "Earth energy budget", this sentence cannot be applied to the surface part of the budget. Rewording is needed accordingly. [Eric Brun, France]	Taken into account. The term "Earth's energy inventory" is now used for the energy budget of the entire climate system, to distinguish from other energy budgets, like that of the surface.
81805	20	5	20	6	This shows again the sensisitvity of the use of the wording (see my previous comments): This sentence cannot be applied to the surface part of the budget. Rewording is needed accordingly. [Karina von Schuckmann, France]	Taken into account. The term "Earth's energy inventory" is now used for the energy budget of the entire climate system, to distinguish from other energy budgets, like that of the surface.
46095	20	10	20	11	Can this statement be generalized to the SSPs used in CMIP6? [Twan van Noije, Netherlands]	Not applicable. The paragraph has been removed. Related Executive Summary statements are made in Chapter 4 and Chapter 9.
34911	20	10	20	19	The SOD comment that, while total Earth system warming can continue for decades, GSAT can stabilise or even reduce under strong mitigation measures, even apart from internal variability. Can this be correct? Please see general comment #14 above. [Jim O'Brien, Ireland]	Not applicable. The paragraph has been removed. Related Executive Summary statements are made in Chapter 4 and Chapter 9.
28867	20	11			These estimates neglect enthalpy terms (Trenberth et al. 2018 J. Clim https://doi.org/10.1175/JCLI-D-17-0838.1; Mayer et al. 2017 J. Clim http://dx.doi.org/10.1175/JCLI-D-17-0137.1;) although the ocean heat transport estimates are highly dependent on the ocean heat content dataset used (Bryden et al. (2020) J. Clim https://doi.org/10.1175/JCLI-D-19-0323.1) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The paragraph has been removed.
71053	20	16	20	19	Please consider citing Cross-Chapter Box 3.1 or Chapter 3 Section 3.5.1.3 here. [Yu Kosaka, Japan]	Not applicable. The paragraph has been removed.
114583	20	21	20	37	useful figures [Jan Fuglestvedt, Norway]	Noted. Thanks.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20409	20	21	20	37	On figure B7.2-1f, the plotted total Earth Warming on 1971-2018 is visibly smaller than 400 ZJ, while the summary as well as table 7.1 quote 406 ZJ. Why? The period seems to be the same. [philippe waldteufel, France]	Taken into account. The assessment text and figure have been revised.
3527	20	28	20	31	Change to "Panel (f) shows the Earth Energy Budget assessed for the period 1971-2018, i.e. the consistency between the total earth system warming from an observation-based assessment and the implied heat storage from the effectve radiative forcing and the Earth warming due to the forcing and response." [Joyce Penner, United States of America]	Taken into account. The text has been clarified
28869	20	32			Around one third of CMIP5 models considered by Loeb et al. (2016) simulate the wrong sign of cross equatorial heat transport and northern minus southern tropical rainfall difference (see Fig. 7d of Loeb et al. 2016). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. The sub-section being referred to has been removed.
20407	20	37	21	8	The numerical values given here do not correspond to those shown on figure B7.2 [philippe waldteufel, France]	Not applicable. The sub-section has been removed.
27149	20	53	20	53	It is energy in the form of heat, which suggests to use 'heat' instead of 'energy'. Moroverover, the wording 'planetary heat transport' could be used instead of 'poleward' [Eric Brun, France]	Not applicable. This section has been removed due to space limitations.
16155	20	53	23	34	This section is a bit stuck in GCM-world. It does discuss GCM-obs agreement, noting some model biases that sound troubling, but without any useful assessment of their implications. Moreover the paleoclimate perspective is totally missing. Paleoclimate data (absence of sufficient polar amplification in particular) shown later in the chapter suggest that our models have systematic problems in simulating either poleward heat fluxes or meridional gradients in local radiative feedbacks. Also some of the noted biases relative to modern observations appear to be fairly serious, for example that model cross-equatorial heat transport is off by a factor of two (a model consensus on a wrong answer!?). Given these multiple, systematic model deficiencies I cannot agree with the final assessment of "high confidence" at the end of the section, which seems to be based on model consensus rather than actual understanding or verification of predictive skill. [Steven Sherwood, Australia]	Not applicable. This section has been removed due to space limitations.
19401	20	53	23	34	Why include this discussion of poleward energy transport in this chapter? The pattern effect does require seom discussion of spatial structure of warming, but with the timing of the Southern Ocean warming and of the Tropical Pattern being the key issues. This is an interesting topic and some of the cited work is important. But the chapter is very long. i donlt think that you can affrod to lose the focus on global mean metrics. [Isaac Held, United States of America]	Not applicable. Thank you for this suggestion. This section has been removed to maintain a focus on global quantities.
20079	20	55	21	5	One wonders about units. A W/m2 is certainly not an energy; a PW to express heat transport is problematic. [philippe waldteufel, France]	Not applicable. This section has been removed due to space limitations.
128885	20	55	22	56	This completely misses the main new developments in this topic. See Trenberth, K. E., and J. Fasullo, 2018: Applications of an updated atmospheric energetics formulation. J. Climate, 31, 6263-6279. doi:10.1175/JCLI -D-17-0838.1. and 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. J. Climate,32, 4567--4583, https://doi.org/10.1175/JCLI-D-18-0872.1 The uncertainties in total surface fluxes are much reduced in observations to the point that one can get reliable meridional heat fluxes. Moreover Trenberth, K. E., and Y. Zhang, 2019: Observed inter-hemispheric meridional heat transports and the role of the Indonesian ThroughFlow in the Pacific Ocean. J. Climate, 32, 8523-8536, https://journals.ametsoc.org/doi/pdf/10.1175/JCLI-D-19-0465.1 produce new estimates of interhemispheric transports that are rather different than previous estimates for the following reasons: (1) They use a new improved atmospheric transport that includes enthalpy of precipitation (Trenberth et al. 2018), (2) they include 2015-2016 and the huge El Niño event made major differences to all the numbers, (3) the OHC is much improved, (4) it includes the increased uptake of heat by the southern oceans vs the northern hemisphere, and (5) the time series show enormous natural variability mostly associated with ENSO. Total interhemispheric transports vary from -0.3 PW to +1.2 PW. The explanation of why there is a cross equatorial transport is less than convincing, and what is given is an association not a "reason". [Trigg Talley, United States of America]	Not applicable. This section has been removed due to space limitations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
82857	20	55	23	34	In discussing poleward energy transports and their changes, an even more complete picture could be given. For instance, discussing latitudinal structure of SW radiation and pointing at heat transport errors arising from cloud biases, there is literature investigating cloud biases contributing to interhemispheric symmetry in absorbed radiation, and identifying the roles of cloud fraction differences and cloud albedo differences in different latitude bands (Bender et al. 2017). Regarding cross-equatorial transport, Acosta-Navarro et al. (2017) investigate the difference in impact between aerosol and GHG emission changes. Refs: Bender, F. A.-M., Engström, A., Charlson, R. J. and Wood, R. (2017) Evaluation of hemispheric asymmetries in marine cloud radiative properties, J. Clim., 30, 4131--4147, DOI: 10.1175/JCLI-D-16-0263.1 Acosta Navarro, J. C., et al. (2017) Future response of temperature and precipitation to reduced aerosol emissions as compared with increased greenhouse gas concentrations, J. Clim., 30, 939—954, DOI: 10.1175/JCLI-D-16-0466.1 [Frida Bender, Sweden]	Not applicable. This section has been removed due to space limitations.
37523	20	57	21	3	The contrast in what is surely LWR is NOT due more (more than what?) to differences in outgoing thermal radiation; those differences are only a consequence of the cause, which might be as simple as a difference in cloud cover. [John McLean, Australia]	Not applicable. This section has been removed due to space limitations.
20411	20	57	21	3	Because the (warmer) continental areas are a larger fraction in the NH? [philippe waldteufel, France]	Not applicable. This section has been removed due to space limitations.
37537	21	6	21	14	Simply enumerating the results of a range of studies is less helpful than a synthesis or assessment [Robert Pincus, United States of America]	Not applicable. This section has been removed due to space limitations.
31717	21	12	21	21	0.08: I feel that this needs a comment, as it is so different to the other estimates [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This section has been removed due to space limitations.
128887	21	22	21	22	Very unspecific. What is meant by "tropical processes"? [Trigg Talley, United States of America]	Not applicable. This section has been removed due to space limitations.
37539	21	25	21	35	Does the paragraph imply that the double ITCA bias common to many GCMs has roots in errors in cross-equaitorial heat transport? Is this understanding new since AR5? [Robert Pincus, United States of America]	Not applicable. This section has been removed due to space limitations.
111131	21	27	21	32	It should be stated that the inter-model spread in net cross equatorial is large compared to the bias relative to observations. From Loeb et al. 2018-- "As a result, HTEQ from the CMIP5 multi-model mean is more than double the observed value. We note that there is sig-nificant variability amongst the individual CMIP5 models (Fig. 4a) (standard deviation of 0.33 PW)." Additionally, it should be stated that the direction of the corss equatorial AHT differs between climate models (Fig.4B of Loeb). In general, the text should emphasize that the inter-model spread in cross equatorial energy transports (and it's partioning between ocean and atmosphere) is large compared to any ensemble mean bias. [Aaron Donohoe, United States of America]	Not applicable. This section has been removed due to space limitations.
20081	21	29	21	29	Change "emit" to "emitting" [philippe waldteufel, France]	Not applicable. This section has been removed due to space limitations.
128889	21	37	21	46	Stephens et al. (2016, J. Climate, DOI: 10.1175/JCLI-D-15-0234.1) offers a deeper explanation on the maximum heat transport and thus is relevant to the discussion of this section. It describes the factors that determine the maximum heat transport in each hemisphere, being those processes that govern energy loss to space poleward of the latitude of zero net TOA flux (which is also the latitude at which the meridional heat transport is maximum). Changes to this maximum transport are similarly defined by changes to processes that influence this heat loss in this region being slightly different in the southern hemisphere compared to the northern hemisphere. In a changing climate, the processes touched on in reference to Figure 7.16 concerning feedbacks in Arctic warming are indeed quite relevant to this max heat transport. These comments are also relevant to the discussion on page 22, lines 28-49. [Trigg Talley, United States of America]	Not applicable. This section has been removed due to space limitations.
128891	21	38	21	48	Probably should not cite Trenberth and Stepaniak (2003) as it used ERBE fluxes which had systematic biases with latitude resulting from poor ADMs. Might instead use studies using CERES such as Fasullo, J.T. and K.E. Trenberth, 2008: Part II: The annual cycle of the energy budget: Meridional structures and poleward transports, J. Climate, 21, 10, 2314-2326., doi:10.1175/2007JCLI1936.1 On line 48, it is more accurate to say that the changes in transport and warming are coupled in that the changes in warming act to reduce the transports. [Trigg Talley, United States of America]	Not applicable. This section has been removed due to space limitations.
23887	21	44	22	2	There seems to be something missing fundametally. The new knowledge on tropical [mesoscale, etc.] dynamics, especially convection has increased during the last few years. That pertaines to MJO basic mode and more. Please consult papers by Z. Fuchs & D. Raymond, for instance. [Branko Grisogono, Croatia]	Not applicable. This section has been removed due to space limitations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23889	21	44	22	2	It is difficult to retype commmenst of my own, due to clumsiness of the system; in the above comment, for example, it should be written/meant 'pertains', and much more above, regarding the typing into this designated format. [Branko Grisogono, Croatia]	Not applicable. This section has been removed due to space limitations.
128893	21	46	21	46	More studies could be cited here, such as by Jennifer Kay or Jim Haywood. [Trigg Talley, United States of America]	Not applicable. This section has been removed due to space limitations.
71961	21	48	22	23	Irving et al. GRL (10.1029/2019GL082015) computes changes in heat transport for the CMIP5 models allowing for ocean heat storage, for historical, greenhouse gas and aerosol forcing. [John Church, Australia]	Not applicable. This section has been removed due to space limitations.
111133	21	52	21	53	Should state an "increase in implied ocean heat transport poleward" since the OHT as diagnosed (from the spatial integral of surface heat fluxes over the polar cap) can not distinguish between ocean heat transport divergence and ocean heat uptake. I suggest explicitly adressing this issue with a sentence like: "Implied ocean heat transport inlcude both the impact of changes in ocean circulation and ocean heat uptake and it is possible (likely) that some component of the increased poleward OHT is associated with preferential ocean heat uptake in the high latitudes (Armour et al. 2019 -- Fig 5A). It is unclear if the increased poleward AHT at the expense of decreased implied OHT persists at equilibrium timescales." [Aaron Donohoe, United States of America]	Not applicable. This section has been removed due to space limitations.
16151	22	28	22	49	Although this paragraph says there is better understanding, what is described sounds more like a diagnosis than a true explanation. It says the transport doesn't change because the latitudinal gradient in TOA flux doesn't change, but no explanation is given of the latter. So this paragraph does not give me any more confidence, it just invokes one uncertain model prediction to explain another. In line 43 the word "show" should be "predict". [Steven Sherwood, Australia]	Not applicable. This section has been removed due to space limitations.
36909	22	33	22	34	This non-sentence is nothing more than a wild assertion,, in fact the entire paragraph is just one assertion after another, based on CMIP5 climate models, which according to text box 9.2 of IPCC 5AR exaggerate warming. [John McLean, Australia]	Not applicable. This section has been removed due to space limitations.
111135	22	40	22	40	Suggest adding (after citation to Armour 2019) " in part because temperature changes are greatest in regions with weaker magnitude (negative) radiative feedbacks (i.e. the Arctic) [Aaron Donohoe, United States of America]	Not applicable. This section has been removed due to space limitations.
128895	22	43	22	43	Changes at TOA have been shown to result from warming in the Arctic (Hartman et al.) so this sentence needs to be reconsidered. [Trigg Talley, United States of America]	Not applicable. This section has been removed due to space limitations.
19327	22	51	22	54	Feldl et al. (2017b) showed the responses discussed in this paragraph in an aquaplanet GCM, namely the compensation between poleward latent heat and dry static energy transport, the decomposition into contributions from the meridional overturning circulation and eddies, and the increase in equatorward latent heat transport in the Hadley cell and poleward latent heat transport by the mid-latitude eddies. Feldl, N., Anderson, B. T., and Bordoni, S. (2017b). Atmospheric eddies mediate lapse rate feedback and Arctic amplification. J. Clim. doi:10.1175/JCLI-D-16-0706.1. [Nicole Feldl, United States of America]	Not applicable. This section has been removed due to space limitations.
36911	22	51	23	11	This is a continuation of the nonsense of the previous paragraph. "Models show...", "models show ...", "Models ... are able to replicate..." doesn't mean a thing unless you can prove those models are accurate. [John McLean, Australia]	Not applicable. This section has been removed due to space limitations.
16153	22	54			IMHO "show" should be "predict." Models only "show" something if we undersrtand why it must be so. This comment applies to many places in the text, which I will not enumerate. [Steven Sherwood, Australia]	Not applicable. This section has been removed due to space limitations.
6695	23	1	23	1	This sentence works for the lower troposphere. But it is less clear that it hold for the upper troposphere. The greater warming of high latitudes is most marked at low levels in the atmosphere. And warming is larger in the tropical upper troposphere than at low levels. Please see the paragraph at the foot of page 32 of Chapter 4 of the SOD, where it is stated that "... increases the meridional temperature gradient" in the upper troposphere. [Adrian Simmons, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. This section has been removed due to space limitations.
111141	23	6	23	6	Suggest adding (after increases is poleward dry-static energy in the Hadley cell): "associated with tropopause rising and lapse rate changes". [Aaron Donohoe, United States of America]	Not applicable. This section has been removed due to space limitations.
23891	23	6	23	11	There must be put limits/borders on such over-simplifying statements regarding diffusion as emulating, or even worse, simulating the heat [and more] transport on those planetary scales. Namely, it has been well known and established that it is baroclinic instability doing the key-dominant effect, NOT diffusion. Please consult textbooks such as Holton and more. [Branko Grisogono, Croatia]	Not applicable. This section has been removed due to space limitations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23893	23	13	23	23	Even worse, continuing on the former/above comment, the authors still try to convince us that a sort of macro-diffusion is the main process for transporting properties on the planetary scale. This is fundametally wrong. One thing is that "it might just look like" diffusion, but it does not mean it really is so. Please do not bring us 50-70 years back in basic understanding of main seasonal, macro-scale processes; in this way you would put down works of Rossby, Charney, Bjerkness, Hoskins, Palmer, and more. [Branko Grisogono, Croatia]	Not applicable. This section has been removed due to space limitations.
67685	23	14	23	15	is it a narrowing and shift of the ITCZ, or a narrowing and intensificiation? (ie, see https://link.springer.com/article/10.1007/s40641-018-0110-5) [Karen Rosenlof, United States of America]	Not applicable. This section has been removed due to space limitations.
67687	23	16	23	16	there are discrepancies between observed (or reanalysis) estimates of Hadley cell strength changes and climate models with increasing CO2 which should be acknowledged. (see https://www.nature.com/articles/s41561-019-0383-x which describes the issue, and claims that the reananalysis are wrong...this should probably be assesses somewhere in the report, although perhaps not in chapter 7) [Karen Rosenlof, United States of America]	Not applicable. This section has been removed due to space limitations.
128897	23	21			Add reference to Byrne and Schneider (2016). Byrne, M. P., and T. Schneider (2016), Narrowing of the ITCZ in a warmingclimate: Physical mechanisms, Geo-phys. Res. Lett., 43, 11,350-11,357, doi:10.1002/2016GL070396. [Trigg Talley, United States of America]	Not applicable. This section has been removed due to space limitations.
20413	23	32	23	34	Throughout this subsection 7.2.24, one wonders whether expressions such a "poleward atmospheric heat transport" apply to both hemispheres or preferentially to the NH. Indeed, the text begins by pointing out and discussing a large asymmetry among hemispheres. Concerning the specific passage here, one is given to understand that heat is transported from low latitudes to middle latitudes, and then stops there. What happens? [philippe waldteufel, France]	Not applicable. This section has been removed due to space limitations.
114585	23	37	27	24	It woudl be useful if you coudl clarify more about the relation to "efficacy" (that got some attention in WGI AR5 ch8). Efficacy is mentioned a few times, but could be introduced [Jan Fuglestvedt, Norway]	Taken into account: The relation to efficacy has been explained.
36925	23	37	27	43	This entire section needs a clear description of the atmospheric physics that pertains to what you are talking about. I know the atmospheric physics that applies but I doubt that all your readers will. The description will also show whether the IPCC authors understand the physics correctly, which I doubt. [John McLean, Australia]	Rejected: no specific suggestions made. The atmospheric physics is well covered in this section.
37125	23	39	23	39	Wrong. Effective radiative forcing is not a fundamental driver of anything; it is a composite, and therefore artificial, construct. [John McLean, Australia]	Taken into account: This has been made clear that this refers to the energy budget framework.
77393	23	39	23	39	This is well stated and could be used as akey message [Emer Griffin, Ireland]	Taken into account: Elements are now in the ES
10735	23	39			One could argue that Internal 'modes' of climate variability are more fundamental drivers of climate change than ERF. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been made clear that this refers to the energy budget framework.
10737	23	39			No. ERF is not the fundamental driver of climate change. It is fundamental in a forcing/feedbacks framework used to in the understanding and estimation of climate change. It is not, in itself, a fundamental part of the physical reality of climate, rather a "useful measure of the climate effects of a physical driver" (Page 23:51-52). It might be the authors are trying to use a phrase to describe what drives many long term changes in climate, but have backed themselves into a corner by defining such general terms like "Radiative forcing" as having specific meanings. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been made clear that this refers to the energy budget framework.
23895	23	41	23	41	Why not use the already introduced abbrevb. For TOA? Why to rewrite again the same thing for so many times? The whole report is written by people who love to write [maybe even to read their own words?], be epic, instead of being short and focused. This corresponds to my main concern about the whole IPCC 6th report - why so much text in such a way? Why there is no bulk short text with figures, followed by dedicated and concise details? [Branko Grisogono, Croatia]	Rejected: TOA is spelled out here for clarity. Later on it is abbreviated.
37543	23	41	23	41	"allowing the system to adjust" describes the mechanics of an ERF computation. Conceptually, ERF is the TOA flux change "after the system has adjusted to the perturbation" [Robert Pincus, United States of America]	Taken into account: This has been rephrased
36913	23	45	23	46	This is fantasy! TOA ERF wasn't measured in 1750, in fact not until just a few decades ago. [John McLean, Australia]	Rejected: ERF isn't measured, The whole section explains how ERF since 1750 is derived.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37547	23	49			In describing adjustments it might be worth noting that most are mediated by changes in circulation, either on large scales (e.g. doi:10.1073/pnas.1508268112) or small scales (doi:10.1002/jame.20019), with resultant impacts on clouds. This could be used to contrast ERF, which requires dyanmical model, from IRF and SARF, which can be computed with purely radiative models. [Robert Pincus, United States of America]	Taken into account: the differences in modelling ERF and IRF has been explained.
71055	23	51	23	51	AR5 report -> AR5 [Yu Kosaka, Japan]	Accepted: This change has been made.
16157	23	51	24	15	There is unnecessary duplication here with Box 7.1. Can you just use the box to fully explain the definitions and just refer here to the Box? [Steven Sherwood, Australia]	Taken into account: Duplication with the box has been reduced.
46097	23	54	23	55	The clause "arising from the forcing heating profile and effects of clouds" seems incomplete. I suggest to remove it. [Twan van Noije, Netherlands]	Rejected: This explanation is important to keep.
46099	23	55	23	55	It makes sense to refere to the "responses in the troposphere" as "adjustments". However, in other places, the same word is used to indicate the associated TOA flux responses, e.g. on page 24, line 27. Please avoid this inconsistency. [Twan van Noije, Netherlands]	Taken into account: This has been reworded to remove inconsistency.
37541	23				The chapter's focus on effective radiative forcing as the metric for quantifying the energetic impacts of anthropogenic changes accurately reflects changed understanding within the scientific community. The explanation at the start of section 7.3 and in Box 7.1 is quite clear. [Robert Pincus, United States of America]	Not applicable. This section has been removed due to space limitations.
36915	24	2	24	4	Increased cloud fraction below? As I showed in 2014, (fig7 & 8) of McLean (2014) "Late Twentieth-Century Warming and Variations in Cloud Cover", Atmospheric and Climate Sciences) low level cloud reduced from 1995 to 2009 (end of ISCCP data) with the decrease almost exactly taken up by the mid and upper level cloud. There was no increased cloud fraction below. [John McLean, Australia]	Rejected: This comment refers to total cloud changes where as the text refers to cloud adjustment processes, so this paper is not relevant
83749	24	4	24	4	Clouds' [Marvel Kate, United States of America]	Accepted: This change has been made.
83751	24	5	24	5	suggest clarifying that "rapid" chemical and biospheric responses (or, to avoid discussion of timescale, chem/bio responses that do not depend on GSAT) are included in ERF [Marvel Kate, United States of America]	Taken into account: This has been clarified.
10739	24	5	24	7	"Adjustments" are defined as being independent of surface temperature, but later (Page 28:16-18) it says "The different adjustment components comprising the ERF for 2×CO2 were broken down by Smith et al. (2018b) where the temperature adjustment was split into land-surface temperature and tropospheric temperature (Table 7.3)". This sounds like some adjustments are not independent of surface temperature. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been clarified
89179	24	5	24	9	I agree that the importance of the adjustments are their independence of surface temperature, but is there any adjustments that occur on a much longer timescale than change in sea surface temperature as a reason for just using adjustments? 'adjustments' seems probably as a very vague description of the process to the community slightly outside our radiative forcing community, not that rapid adjustment is perfect. However, rapid adjustment has at least been used in several publications since AR5. [Gunnar Myhre, Norway]	Noted: this was discussed at length in the LA team. We decided that the vaguer "adjustments" term is needed to match the new conceptual approach in Box 7.1. The text has been further refined to explain these concepts more carefully
46101	24	6	24	7	Remove "(magnitude or pattern)". [Twan van Noije, Netherlands]	Accepted: This has been removed.
77395	24	6	24	8	Can "adjustments" be more clearly defined? [Emer Griffin, Ireland]	Taken into account: The adjustments definition has been revisited
78059	24	7	24	7	I don't understand why "pattern" is mentioned - what does "globally averaged surface temperature pattern" mean? In view of the statement on p24, lines 9-11, I suppose that your definition of adjustment means any change in global mean net energy flux into the system which occurs with zero change to local surface temperature everywhere, both land and sea - is that right? If so, I suggest the statement here should be clarified. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: "pattern" has been removed.
37545	24	7	24	15	The explanation as to why adjustments are defined as changes independent of GSAT change, as opposed to by time scale, is delightfully clear and relfects community consensus (if not unanimity). [Robert Pincus, United States of America]	Noted: Thank you!
79203	24	8	24	9	… adjustments are characterized by their independence from surface temperature rather than by rapidity. [it's not quite obvious whether this corresponds to 'importance' in a meaningful way] [Michael Ponater, Germany]	Taken into account: "important" has been rephrased
79205	24	9	24	10	"This means …" [What is this sentence meant to clarify? It's not obvious to me. "global mean change" of what?] [Michael Ponater, Germany]	Taken into account. This has been clarified
27151	24	10	24	11	The sentence is unclear [Eric Brun, France]	Noted: No suggestions made
46103	24	12	24	12	Not all forcing agents can be expressed in terms of concentrations, e.g. land use. Please change. Also on page 24, line 33. [Twan van Noije, Netherlands]	Accepted: Surface albedo change has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37549	24	15			"They" = emitted gases? [Robert Pincus, United States of America]	Rejected: "they" immediately follows "precursor gases".
46105	24	17	24	19	Why not? It's true that in AR5 other definitions were used to approximate ERF, but nevertheless the standard definition of ERF was based on fixed SSTs. Moreover, the same can be said of the current assessment because it seems that in many locations (e.g. Chapter 6) no correction for the response of surface air temperatures over land is made. I have the impression that ERF and fixed-SST RF are used interchangeably in many places, also in this chapter. [Twan van Noije, Netherlands]	Taken into account: The comments on AR5 have been revised.
77397	24	19	24	20	A reference for this body of work is needed. [Emer Griffin, Ireland]	Taken into account: This has been clarified that this refers to the rest of this section.
37551	24	21			"delivering"? [Robert Pincus, United States of America]	Taken into account: This has been rephrased
37561	24	23			Note that fixed-SST experiments were also used earlier, e.g. doi: 10.1175/1520-0442(2001)014<2960:IAFQFA>2.0.CO;2 [Robert Pincus, United States of America]	Rejected: We do not intend a comprehensive historical discussion here
83753	24	27	24	27	"The ERF is the sum of the Instantaneous Radiative Forcing (IRF) plus the adjustments" is this true in all cases? What if there are nonlinear interactions between different forcing agents? Maybe clarify that the ERF *for a particular forcing agent* is iRF+adjustments? [Marvel Kate, United States of America]	Accepted: This has been clarified as recommended.
89181	24	27	24	49	It is mentioned that the fsst method can only be used for forcing larger than 0.1 Wm-2 which is important to include. In the same paragraph it should be mentioned that the regression can only be used for large perturbations. I am unsure whether it has been quantified how large perturbation is required and is likely dependent on climate driver, but at least for some of them 1 Wm-2 is insufficient. [Gunnar Myhre, Norway]	Accepted: The perturbation size needed for regression has been added.
37553	24	27	25	8	In reviewing the two main ways in which ERF is estimated for climate models, it would be worth noting that both regression and fixed-SST simulations are approximations computational approaches to a determining a conceptually-clear idea. (It would be worth a sentence explaining why only SSTs, and not land Ts, are fixed.) It should be noted, too, that these approaches typically don't give the same answer. For pedagogical purposes it might also be worth linking to the energy balance equation (box 7.1, equation 7.1) and explaining that regression estimates of ERF rely on the linearity of this equation while fixed-SST estimates attempt to set delta T to 0. [Robert Pincus, United States of America]	Accepted: The approximations to the conceptual approach has been clarified. And the equation and Box 7.1 has been linked to help improve the pedological element as asked
37557	24	27			"Theoretically" -> "in principle" or similar. [Robert Pincus, United States of America]	Accepted: This change has been made.
46107	24	29	24	30	Even when using comprehensive models it is not straightforward to estimate ERF, because the new definition requires an extra correction term to account for the response of surface air temperatures over land. Please make this explicit. [Twan van Noije, Netherlands]	Rejected: The correction term is fully explained.
36917	24	29	24	30	This has no credibility because IPCC AR5 found CMIP5 models to be flawed and exaggerate warming (see its text box 9.2) [John McLean, Australia]	Rejected: This is not relevant to the discussion here on tropospheric adjustments.
37559	24	29			A "compresensive climate model" is needed to a) compute adjustements due to changes in circulation, and b) to do so on a global basis. These are conceptually distinct and some work exists to look at adjustments using finer-scale models. Greater precision would be useful here. [Robert Pincus, United States of America]	Taken into account: The reasons for needing comprehensive climate models has been expanded.
72167	24	30	24	40	Again, it remains unclear how exactly ERF is estimated. Regression methods are widely used as described here, but there is no discussion whether or not there is a standard way of doing this. The so-called gregory plots show a change in slope most of the time (as can be seen also in Fig. 1 of Box 7.1), but where? Is this the same in all models? If not, how is it decided what is adjustment and what IRF? [Anna von der Heydt, Netherlands]	Rejected: This section (and box 7.1) describes regression methods and explains the issues with the non-constant slope.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15409	24	30	24	49	Tsutsui (2020, https://doi.org/10.1029/2019GL085844) has established an alternative way to estimate ERF as well as temperature response parameters. This takes an approach using an impulse response model to emulate time series of delta N and delta T, instead of regressing delta N onto delta T. Although estimated ERF and climate feedback parameters are not significantly different between the two approaches, the new one, termed emulator method, deals with a more general temperature response parameters, from which ECS and TCR are analytically derived. Whereas the fixed SST method is simple and useful for diagnosing forcing, the regression method, which requires a longer time integration with a full ocean model, enables to separate forcing and response. On top of that, the emulator method, which is a little more elaborate, enables explicitly dealing with transient ocean heat uptake. The impulse response model directly serves climate change mitigation studies, which means that the emulator method has an advantage in ensuring methodological consistency. It also directly represents the concept of ERF as a fundamental driver of climate change. [Junichi Tsutsui, Japan]	Rejected: Emulators are covered in cross-chapter box 7.1
46109	24	33	24	34	This method of calculating ERF is inconsistent with the statement made on page 24, lines 9 to 11, because changes in land or ocean surface temperature patterns which average out to zero in the global mean will be including in the ERF in this way, and will therefore be counted as adjustments. Please clear up this inconsistency. I have the impression that in line 10 "not included" should be "also included". [Twan van Noije, Netherlands]	Rejected: No, the exclusion of pattern changes is necessary to ensure consistency between fSST and regression approaches.
46111	24	38	24	40	It would be appropriate to add the reference to Winton (2010: https://doi.org/10.1175/2009JCLI3139.1) here. [Twan van Noije, Netherlands]	Rejected: This reference does not obviously add anything new to the discussion here.
36919	24	40	24	43	This has no credibility because IPCC AR5 found CMIP5 models to be flawed and exaggerate warming (see its text box 9.2) [John McLean, Australia]	Rejected: This discussion is about fixed-SSTs, not coupled models.
78061	24	40	24	43	Since land surface temperature change takes place (as discussed on line 51ff), I would say that climate feedback is "partially" rather than "approximately" removed in a Hansen experiment. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This change has been made.
46113	24	42	24	42	Change "sea-ice" to "sea-ice concentrations". [Twan van Noije, Netherlands]	Accepted: This change has been made.
37563	24	43	24	46	It is inaccurate to describe the noise in either fixed-SST integrations or regressons against abrupt climate change as "uncertainty". What's being referred to here is noise in an imperfect way of computing ERF. The true uncertainty is methodological. [Robert Pincus, United States of America]	Taken into account: This has been rephrased
36921	24	47	24	48	This looks like more "constraining" in order to get a desired output. [John McLean, Australia]	Rejected: No suggestion made
46115	24	47	24	49	Please mention that nudging winds means that adjustments associated with circulation responses will be suppressed (see the paper by Schmidt et al.), so the resulting ERF estimates will in principle be biased. [Twan van Noije, Netherlands]	Accepted: The effect of circulation has been mentioned.
46117	24	51	24	53	In fact, according to Equation (7.1) ΔT_land is not the "land surface temperature change" but the "change in near-surface air temperature over land". Please correct this in lines 51 and 54. [Twan van Noije, Netherlands]	Accepted: This has been changed.
685	24	51	24	55	Not clear why fixed land surface temperatures were used in model runs just like fixed SSTs. Some comment on why not, or why this is insignificant is needed [Bruce Wielicki, United States of America]	Taken into account. The reasons for not constraining land surface temperatures has been explained.
10741	24	51	25	44	Precomputed `Kernels' are mentioned a lot here (and elsewhere in chapter), but I worry that so little detail is given that they appear to a lay person to be 'black boxes' to provide various adjustments. More detail needs to be provided of where they come from and if they are model and/or forcing factor dependent. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The discussion of kernels has been revised
83755	24	52	24	54	I think it's important to clarify whether it's viewed as important that the kernel be derived from the same model that has been run with fixed SSTs (or at least using the same radiative scheme). I don't think it matters very much for the radiative response to delta T_land, but if it's viewed as acceptable to use a standard kernel for every model this should be stated. [Marvel Kate, United States of America]	Taken into account: The model dependence of kernels has been discussed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10743	24	52	24	55	Are the authors sure they are using the correct references here? I looked at the studies, and none seem totally appropriate. Tang et al (2019) referred to land temperatures in an approach that was used in Hansen et al (2005), but I could not see where they referred to radiative transfer model kernal approaches just for land temperatures. Richardson et al (2019) does talk about how ERF is "adjusted to take into account land surface temperature change using radiative kernels." (Section 2.3.4), but that is all that is said on the subject. Stjern et al (2017) does not appear to mention either `kernels' or land temperatures. Land temperatures don't seem to be mentioned in Smith et al (2018b). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: These references has been revised
23897	24	53	25	8	Kernel 'k', and 'alpha' are not well explained; what is their relation, etc.? [Branko Grisogono, Croatia]	Taken into account: This has been clarified that k/=alpha
89183	24	54	24	54	Kernel not amplied in Stjern et al. (2017) and can thus be removed as a reference here. [Gunnar Myhre, Norway]	Accepted: This has been removed.
107649	25	1	25	1	is there a citation for the 0.2 W m-2? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: We now cite Smith et al, 2020, Tang et al. 2019
46119	25	1	25	2	Besides changes in tropospheric temperatures and water vapour there are also other feedbacks that needs to be mentioned here, e.g. changes in temperature-dependent biogenic emissions over land. [Twan van Noije, Netherlands]	Taken into account: This has been added.
36923	25	1	25	54	Just more computer games with models. Lines 33 to 38 are the typical IPCC claims that can be summarised as "the models aren't very good but they are getting better". Delete the whole section and don't mention this subject again until the models can be shown to accurately encompass every factor. [John McLean, Australia]	Rejected: This section is a rigorous comparison of model IRFs and SARFs
10745	25	1			What is the source for the "0.2Wm-2" adjustment number? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: We now cite Smith et al, 2020, Tang et al. 2019
89185	25	4	25	5	I do not understand the argument of insufficient evidence for removing part of the tropospheric temperature and water vapour caused by land surface warming. The radiative effect of land surface albedo change in fsst simulations is clearly something to remove but neglected in the approach taken in the chapter. In most models the radiative effect of land surface temperature change and surface albedo change are rather similar in magnitude (but of opposite sign). Richardson et al (2019) (referred to in the chapter) showed that removing surface temperature, albedo, tropospheric temperature and water vapour was the approach giving an efficacy closest to unit which is important for ERF to be a useful approach comparing various climate drivers. My recommendation would be to state that removing albedo, tropospheric temperature and water vapour is a preference and most physical correct but more computational complicated and thus removing only the land surface temperature is shown to cover a large part of the needed correction. [Gunnar Myhre, Norway]	Taken into account: We have added more explanation on the reasoning to explain the choice of how and which adjustments are included in the ERF. WE did not make your suggested choice to be more consistent with the Box 7.1 concepts, this is now explained
10753	25	6	25	8	I strongly recommend using the term "Adjusted Effective Radiative Forcing" (Richardson et al 2019) to avoid confusion with the current definition of "Effective radiative forcing". [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Reject: It will be confusing to add yet another term.
10755	25	6	25	8	Have all the quoted "ERF" numbers in this chapter had this kernal approach applied to them? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: It has been clarified where the Ts correction is applied
46121	25	6	25	8	There are quite a few caveats related to calculation of the correction, and the kernel approach in particular. For instance, the kernel is model dependent and doesn't account for feedbacks related to unrepresented (Earth system) processes. Apparently, there are pros and cons related to the new definition of ERF. Conceptually the new definition seems the better choice, but it becomes ill defined because 1) the kernel approach only provides a first-order correction, and 2) the correction is not consistently applied across all forcing agents. The fixed-SST ERF on the other hand was well defined and can be directly obtained from model simulations. It would be instructive to add some discussion about this, and better justify why a new definition has been adopted despite all the difficulties it entails. [Twan van Noije, Netherlands]	Taken into account: The Ts correction has been explained further
72169	25	6	25	8	"…the kernel approach will be used…": What is the kernel approach? And where is it used? The text below does not explain this! [Anna von der Heydt, Netherlands]	Taken into account: The kernel approach has been explained further
10747	25	6	25	8	What is the adjustment? Is it "0.2Wm-2" (page 24:1) for all the forcing factors for all periods? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The Ts adjustment have been clarified.
10749	25	6	25	8	Is the kernel approach climate model dependent? What uncertainty are there in its use? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The dependence of kernel on model has been discussed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10751	25	6	25	8	Is the kernel approach forcing factor dependent? I would be surprised if not, given the different spatial properties of the different forcing factors, e.g. aerosols during late 20th century have more influence in northern hemisphere than southern hemisphere. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The kernel approach has been clarified - depends on pattern.
27153	25	8	25	8	Are the results symetrical if the estimate from climate simulatoins is done with a reference at pre-idustrial or a referent at 2xCO2 (then with CO2 reduction) should also be discussed. This is important for the understanding of the dependancy of some of the estimates to the mean state (and to model complexity). [Eric Brun, France]	Taken into account: Reference to the mean state has been made.
27155	25	8	25	8	Please precise how is done the estimation of the error bar on the ERF estimation. [Eric Brun, France]	Taken into account: The error bars have been discussed further
77399	25	10	25	10	Perhaps use clearest rather than cleanest which adds wider dimensions. [Emer Griffin, Ireland]	Rejected: Cleanest is the most appropriate word here.
31719	25	12	25	12	Figure 7.7? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been changed.
46123	25	15	25	15	I suggest changing "The individual adjustments" to something like "Neglecting interactions between adjustment processes, TOA flux changes due to individual adjustments". Note that the term "adjustments" is again used for the TOA flux changes due to adjustments, while in other places it refers to the atmospheric processes themselves, e.g. on page 23, line 53 to page 24, line 2. Please avoid this inconsistency. [Twan van Noije, Netherlands]	Taken into account: The adjustments have been made clearer
23899	25	15	25	15	What does refer to 'The individual adjustments…'? [Branko Grisogono, Croatia]	Taken into account: The kernels have been explained better.
37565	25	15			Invoking radiative kernels here might be confusing especially because these are not the same as the surface kernel page 24, line 55. Perhaps explain the idea, or at least harmonize with the use of the term above? [Robert Pincus, United States of America]	Taken into account: The kernels have been explained better.
37567	25	16			Will casual readers understand what "partial radiative perturbation techniques" are? It would be simple enough to expain in less than a sentence [Robert Pincus, United States of America]	Taken into account: This has been explained
1639	25	22	25	23	The climate IRFs depend also on the base state of the model. [Lazaros Oreopoulos, United States of America]	Accepted: This has been clarified
128899	25	22	25	23	The climate IRFs depend also on the base state of the model. [Trigg Talley, United States of America]	Accepted: This has been clarified
107651	25	22	25	44	will there be any RFMIP results on IRF in CMIP6 models? [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: There are no further IRF studies beyond those cited here
37569	25	22			The discussion of IRFs might be more tightly focused. IRFs produced by climate models will differ both because the parameterizations are in error, as is assessed by the Pincus 2016 paper and its antecedents, but also because IRF depends on model base state, including the distribution of clouds, temperature, and humidity, which varies across models. Kernel methods are unable to separate these two factors and so don't represent a "useful test of climate model radiative transfer codes." Results from CMIP6 are not yet available but the results of Pincus et al. 2016 don't really support the claim of "high confidence in [the accuracy of?] climate model respresentation of radiative forcing from greenhouse gases" More importantly it would be useful to motivate why one would look at IRFs in the context of this chapter. [Robert Pincus, United States of America]	Taken into account: We agree with these comments and have expanded the discussion of IRFs and how it relates to uncertainty in the ERF and in models. We have also expanded the motivation for looking at IRF
1641	25	33	25	34	One cannot compare climate models with line-by-line models because the latter are radiative transfer models, i.e., models of a different type. The correct way to phrase this is “The larger spread in IRF produced by the radiative transfer codes of climate models… compared to line-by-line models…”. [Lazaros Oreopoulos, United States of America]	Taken into account: This has been rephrased
128901	25	33	25	34	One cannot compare climate models with line-by-line models because the latter are radiative transfer models, i.e., models of a different type. The correct way to phrase this is "The larger spread in IRF produced by the radiative transfer codes of climate models, ... compared to line-by-line models, ..." [Trigg Talley, United States of America]	Taken into account: This has been rephrased
84857	25	33	25	36	Check grammatical error in the bracket in Line 34 and sentence formation in line 35 [Monika Sikand, United States of America]	Not applicable: This sentence has been changed.
23901	25	33	25	40	In fact, this is both and editorial comment that repeatedly appears. Different citations, yet unpublished, here to Smith, in various forms, put the level od the overall Report down. Although I read a hand-waiwing argument of the authors that most of those manuscripts have been almost published, etc., this is just not good enough in terms of top science. I cannot overstress this point that relates esentially to all chapters. [Branko Grisogono, Croatia]	Rejected: The citations follow the IPCC rules.
71057	25	34	25	34	"line-by-line models" firsr appears here and needs explanation. [Yu Kosaka, Japan]	Accepted: This has now been explained
31721	25	35	25	35	Since models have many other possible differences (vertical and horizontal resolution, height of top level, differences in cloudiness and water vapour, etc) , is it safe to attribute all the difference to the radiation code itself? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: mention of differences in meteorological base state now added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31723	25	39	25	39	"greenhouse": I don't know how specific results for CO2 can be generalised to all ghgs [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been rephrased
46125	25	41	25	43	Please clarify if this is a step back compared to the AR5, in other words whether or not this was also the case in the earlier phases of CMIP. [Twan van Noije, Netherlands]	Taken into account: This has been rephrased to emphasise the benefits of CMIP6
10757	25	47	26	2	Correct model names (see Smith et al 2018b) are needed in the top half of table 7.2. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: The correct model names have been used
37127	25	49	25	49	The only CO2 experiments that can ethically be performed with unvalidated climate models are those that explore the sensitivity of the models to the inclusion of CO2. Delete table and the whole section. [John McLean, Australia]	Rejected: This section does explore the sensitivity of the models to in the inclusion of CO2
46127	25	50	25	50	Remove "Earth system". [Twan van Noije, Netherlands]	Accepted: This change has been made.
46129	25	53	25	53	Do the "bracketed numbers" refer to the multi-model ranges given in the table? [Twan van Noije, Netherlands]	Not applicable the mention of brackets has been removed.
46131	25	53	25	53	Please clarify why it is mentioned that only a subset of models is included to calculate the multi-model means and 95% range, while all table entries are provided for all the models included in the table. [Twan van Noije, Netherlands]	Not applicable the mention of brackets has been removed.
71059	25	53	25	53	"brackedted numbers" are not found in the table. [Yu Kosaka, Japan]	Not applicable the mention of brackets has been removed.
100451	25	53	25	54	I cannot see any "bracketed numbers" [Øivind Hodnebrog, Norway]	Not applicable the mention of brackets has been removed.
22135	25	53	25	54	As far as I can see there are no bracketed numbers? [Peter Thorne, Ireland]	Not applicable the mention of brackets has been removed.
46133	26	7			Figure 7.6: The perturbations for which forcings are presented in this figure look like a rather arbitrary selection, both in terms of the forcing agents covered and the relative magnitude of the perturbations. This is acceptable in a research paper, but in an assessment report one expects a more comprehensive treatment, including results across a larger range of climate forcings. If the graph is meant for illustrative purposes only, this should be mentioned. The same applies to Figure 7.7. [Twan van Noije, Netherlands]	Accepted. To address this, we have extended the list of forcings to include two recently accepted paper (Hodnebrog et al and Marshall et al) that includes adjustments for N2O, CFC11, CFC12, tropospheric ozone and volcanic forcing.
31725	26	10	26	12	Sorry for the "stuck record" mode on this, but I remain concerned that the kernel definition of SARF, with its very crude tropopause definition, is not a good test of the performance of the "traditional" SARF calculations. Maybe indicate this by calling it kernel-SARF or kSARF? Perhaps some calculations have been made to compare it, but otherwise I am quite concerned about this, especially for forcings which lead to near-tropopause temperature adjustments [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: It has been clarified that this is not a definition of SARF, but an approximation to it.
10759	26	21	26	22	I thought ERFs were designed to give more consistent global temperature per unit forcing, e.g. Gregory et al (2004) rather than be a discovered property. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: This is what the text already says. There is no change suggested.
3529	26	21	26	23	This was shown in Rotstayn and Penner, J. Climate, 2001 - of course it was difficult to convince anyone of adopting this at the time, but I would appreciate at least being acknowledged now! (called "quasi forcing" at the time) [Joyce Penner, United States of America]	Accepted: This paper has been cited
83757	26	21	27	11	In the discussion of forcing efficacy, I think it's important to note the importance of timescale. There are methodological differences between studies that estimate the "inferred" ECS from transient single-forcing runs (eg Marvel et al 2016, Shindell 2014) and studies that abruptly impose a large forcing and allow the system to come into (quasi) equilibrium. It's fully possible for some of the confusion surrounding forcing efficacy studies to be related to real.physical differences between transient effects and the effects in (quasi) equilibrium. One way to elide this confusion might be to discuss this in terms of an SST pattern effect: if different forcing agents induce different patterns of warming/cooling, and these patterns are radiatively significant (ie, they trigger quantitatively different global feedbacks) then "forcing efficacy" is properly thought of as a pattern effect, mathematically equivalent to the pattern effect that renders "true" ECS greater than "inferred" ECS. [Marvel Kate, United States of America]	Taken into account: We now cover the pattern effect when discussing efficacy
15411	26	21	27	11	The temperature response is proportional to ERF divided by alpha, which means that the absolute values of the two parameters are arbitrary. The fixed-SST method cannot separate forcing and response by itself. Although this issue is discussed to some extent in 7.4.1.2 (page 57, lines 42-48), I think that the magnitude of ERF estimated with the fixed-SST method should be confirmed from its consistency with accumulated heat content. [Junichi Tsutsui, Japan]	Taken into account: Additional citations are added
89189	26	21	27	11	I feel it can be further empasized that having a forcing definition giving an efficacy close to 1 is crucial for compared various climate drivers and for metrics like GWP. [Gunnar Myhre, Norway]	Accepted: This has been emphasised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78063	26	22	26	22	It would be appropriate to cite Shine et al. (2003, 10.1029/2003GL018141), who fixed land surface temperature as well, and drew the same conclusion. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been cited
89187	26	23	26	23	It is incorrect that Richardson et al (2019) showed that the ERF definition applied in the chapter gave efficacy closest to unit (see my commnet above). [Gunnar Myhre, Norway]	Taken into account: We have revised Richardson discussion
89191	26	53	26	54	Very minor comment: Our paper (Myhre et al. 1998) did not use a LBL model so you want to make a small change to the sentence. [Gunnar Myhre, Norway]	Accepted: This has been revised
20415	27	2	27	2	What is climate sensitivity? It has never been defined before (while of course the equilibrium climate sensitivity has been). The definition is actually given incidentally, in the legend of figure 7.7. Please make matters simpler by indicating, when climate sensitivity is introduced, that it is the inverse of the feedback parameter. Added remark: it is unfortunate that equilibrium climate sensitivity and climate sensitivity are expressed in vastly different units. [philippe waldteufel, France]	Taken into account: This has been phrased as 1/alpha
31727	27	2	27	2	Calculated using the kernel method? This is rather important to know, in my view, as it is not pure SARF [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been clarified
10761	27	2	27	3	As far as I can tell "Climate sensitivity" has not been formally defined in this chapter yet. i.e. 1/alpha. Box 7.1 might be an appropriate place to do it. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The use of "climate sensitivity" has now been clarified.
10763	27	3	27	4	There is somewhat over-confident language here for the consistency of ERF and variability of SARF. According to Figure 7.7 ERF seems to be also fairly variable across the forcing factors. For instance Ozone is ~30% lower than 2xCO2, and if you exclude 10xBC the SARF values are close to are only slightly more variable than the equivalent ERF. At least use same way of expressing differences across the factors rather than two separate ways for ERF and SARF as is done currently. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: We have revised Richardson discussion
69603	27	5	27	5	indentical' delete 'n' [Nicholas Golledge, New Zealand]	accepted: This has been deleted
18907	27	5	27	6	In addition to papers cited, the following publsied paper on the efficacy of BC aerosolos is also good to cite: Modak, A., and G. Bala, 2019: Efficacy of black carbon aerosols: the role of shortwave cloud feedback, Environmental Research Letters, https://iopscience.iop.org/article/10.1088/1748-9326/ab21e7 [Govindasamy Bala, India]	Accepted: This has been cited
78065	27	9	27	9	Gregory et al. (2016, 10.1007/s00382-016-3055-1), Marvel et al. (2016, Nature Climate Change), Gregory et al. (2020, 10.1007/s00382-019-04991-y) and Ceppi and Gregory (2019, 10.1007/s00382-019-04825-x) show evidence of lower climate sensitivity to volcanic aerosol forcing than to CO2; Ceppi and Gregory likewise for solar forcing. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been cited
10765	27	10	27	11	"10% range" is very approximate! Figure 7.7 suggests it is closer to 20%. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Both figure 7.7, now 7.6 has been updated and the text revised
71963	27	13		24	Suggest compare the AR6 estimates to that used in the AR5 (and why the difference and the implications?) [John Church, Australia]	Taken into account: This has been compared with AR5
79207	27	20	27	20	"that physical climate feedback parameters" [this is not obvious and not necessarily true for all climate feedback parameters] [Michael Ponater, Germany]	Taken into account: This has been clarified to be alpha.
31731	27	24	27	24	This sentence is a bit ambiguous. I think you mean that ERF is not a suitable estimator of GLOBAL-MEAN surface response. I could read that it is referring to local temperature, and that could be applied to all ERFs [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been clarified to be global mean
10767	27	27	27	43	I suspect what is in this figure is limited to what experiments have been done, but is there another reason why volcanic forcing (stratospheric aerosols) have not been estimated? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure deleted.
20083	27	27	27	43	It is recommended that for temperature, in the legend of ordinates on figure 7.7, °C are used rather than K, as everywhere else in the chapter. [philippe waldteufel, France]	Accepted
41271	27	46	27	46	I personally think the Dufresne et al. (2020) paper has been significant in terms of clarifying the physics behind the greenhouse effect from both a qualitative and quantitative point of view. I think it is worthy of at least a quick mention in this section. https://doi.org/10.1175/JCLI-D-19-0193.1 [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: This section does not provide textbook explanations of the greenhouse effect.
46135	27	46	27	46	I would suggest to simplify the section title to "Greenhouse Gases". [Twan van Noije, Netherlands]	Accepted: This has been renamed
114587	27	46	27	46	It would be good if the role of changed concentrations vs updated radiative efficiencies could be more clear. [Jan Fuglestvedt, Norway]	Accepted: This distinction has been made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114589	27	46	27	46	I think you could stress a bit more the differcne between concentration based ERF and ERF attributed to emissions. [Jan Fuglestvedt, Norway]	Accepted: The concentration-based has been discussed
22137	27	52	27	52	It isn't clear to me how LBL models would ever be able to calculate ERF by construction and hence the text after the comma arguably should be modified accordingly. A LBL model would never be able to include the physical feedbacks responsible for moving SARF to ERF after all? [Peter Thorne, Ireland]	Taken into account: This has been reworded
37119	27	52	27	56	Yet again you ignore overlapping absorption bands and the fact that gases in low concentration will have negligible, if any, effect. Or do you think that that the chances of a photon landing on a molecule of methane (at 1.6ppmv of the air) is the same as the chances of the photon landing on water vapour (at15,000 ppmv of the air)? You even admit to an overlap of water vapour and nitrous oxide! [John McLean, Australia]	Rejected: The overlapping bands are fully accounted for in these calculations.
36927	27	53	28	2	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: The radiation calculations fully account for overlaps with water vapour.
22139	27	54	27	56	Shouldn't reference be made in addition to the work highlighting this shortwave effect or is this the reference given? If so it should be moved forward to later in the sentence? [Peter Thorne, Ireland]	Accepted: This has been reordered
79211	28	8	28	8	"Hence …" [a rather awkward sentence, change to something like: "Hence climate models alone are not sufficient to establish ERF best estimates for the WMGHGs" [Michael Ponater, Germany]	Taken into account: This has been revised
37571	28	8			As noted above, the Soden et al. 2018 results are less well-suited for attributing differences in IRFs to model error than are more direct measures. [Robert Pincus, United States of America]	Taken into account: This has been revised
46139	28	12	28	16	Please mention that Vial et al. used the fixed-SST definition of ERF. [Twan van Noije, Netherlands]	Accepted: This has been mentioned
46141	28	12	28	23	The results described in this paragraph are specific to CO2. It should be explain if and to what extent the results can be generalized to other forcings. If the paragraph applies only to CO2, why not move it to the "Carbon Dioxide" subsection? [Twan van Noije, Netherlands]	Taken into account: This section has been reordered
46137	28	15	28	15	Change "water vapour and clouds" to "water vapour, clouds, and surface albedo". [Twan van Noije, Netherlands]	Accepted: This has been added.
31733	28	18	28	18	This could cause real confusion compared to the older literature and earlier ARs, where SARF was normally computed at the tropopause. The TOA SARF is, of course, the same, but the perceived impact of strata T adjustment is quite different, and normally of the opposite sign (quite dramatically so for CO2). Maybe a footnote would be useful here? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The difference between TOA SARF and tropopause RF has been explained.
80035	28	20	28	20	It is unclear whether the tropospheric adjustment can be linearly added to the SARF formula provided by Etminan. The land surface may also interact in non-linear ways with the longwave flux changes introduced by the stratospheric adjustment. It would be good to specifically point out here that these interactions are neglected by such linear assumption. [Gabriel Chiodo, Switzerland]	Accepted: It has been clarified that it is assumed that the adjustments are additive
128903	28	25	28	30	[CONFIDENCE] It seems odd to support these points with only reference to two submitted papers. The fact that WMGHGs can have further influences on ozone and aerosols, thereby further affecting radiative forcing, has been well understood for a long time (and described in some detail in AR5). It would be useful to provide some of those additional background references here. [Trigg Talley, United States of America]	Accepted: AR5 has been referenced.
37121	28	26	28	26	I hope you are joking when you cite two papers that have no publication date and are designated as "submitted". [John McLean, Australia]	Rejected: The citations follow the IPCC rules.
46143	28	32	28	32	This statement applies to the whole report, and can therefore be removed here. [Twan van Noije, Netherlands]	Taken into account: This has been incorporated earlier.
22141	28	32	28	32	This text dangling here feels odd. There is surely a better way to incorporate this, perhaps earlier in the section than here? [Peter Thorne, Ireland]	Taken into account: This has been incorporated earlier.
71061	28	32	28	32	This apparently applies to other forcings, so should be given earlier in the section 7.3. [Yu Kosaka, Japan]	Taken into account: This has been incorporated earlier.
80037	28	33	28	33	It would be good to also mention this assumption for ozone (i.e., ERF = RF) elsewhere in the report, e.g. Chapter 6. The same also applies to the discussion of ozone-depleting substances. [Gabriel Chiodo, Switzerland]	Accepted. Text and clarifications have been added to chapter 6
37123	28	37	28	40	The units of measurement are required for all factors. For example, a percentage difference in C degrees is very different to the same percentage difference in K. [John McLean, Australia]	Rejected: The caption clearly states these are % of the CO2 SARF
37129	28	37	28	42	The only CO2 experiments that can ethically be performed with unvalidated climate models are those that explore the sensitivity of the models to the inclusion of CO2. Delete table and the whole section. [John McLean, Australia]	Rejected: This table does show the sensitivity of the models to the inclusion of CO2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46145	28	37			Table 7.3: I believe the column "Surface temperature response" refers to surface temperatures over land only. [Twan van Noije, Netherlands]	Taken into account: This has been clarified
79209	28	40	28	40	Table 7.3, heading 4th column, should read "stratospheric temperature adjustment" [Michael Ponater, Germany]	Accepted: This has been changed.
37131	29	3	30	10	The only CO2 experiments that can ethically be performed with unvalidated climate models are those that explore the sensitivity of the models to the inclusion of CO2. Delete table and the whole section. [John McLean, Australia]	Rejected: This table does show the sensitivity of the models to the inclusion of CO2.
687	29	6	28	10	Very good ERF method to combine LBL radiative calculations with climate model adjustments [Bruce Wielicki, United States of America]	Noted: Thank you!
22143	29	7	19	8	If this partly / mostly can be given quantitatively it should be for completeness. [Peter Thorne, Ireland]	Accepted: This quantification has been given.
46147	29	7	29	8	Please explain how much of this increase in ERF can be explained by the change in definition. Is it true that excluding the response of surface air temperatures over land explains most of the increase? [Twan van Noije, Netherlands]	Taken into account: This quantification has been given
77401	29	7	29	10	This is a significant change that should be clearer in the Exec summary and SPM. Similar detail on changes for N2O and methane should be provided including contributions of the increased atmospheric concentrations of these gases. [Emer Griffin, Ireland]	Accepted: This details has been added.
31735	29	8	29	8	"historical" - which years? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The AR5 vs AR6 comparison has been clarified.
10769	29	8	29	10	What period is being covered in these ERF estimates? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The period is 1750-2019, this is now added.
10771	29	8	29	10	Has the AR5 value been adjusted for land temperatures? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The AR5 vs AR6 comparison has been clarified.
10773	29	8	29	10	Is the AR5 value ERF? According to Myhre et al 2013b, "RF for CO2 alone is 1.82 (1.63 to 2.01) Wm-2", and their definition of "RF" is not effective radiative forcing (8.1 in Myhre et al 2013b, for 1750-2011) [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The AR5 vs AR6 comparison has been clarified.
10775	29	8	29	10	Is the AR5 uncertainty range correct? According to Myhre et al 2013b, "RF for CO2 alone is 1.82 (1.63 to 2.01) Wm-2" for 1750-2011 [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The AR5 vs AR6 comparison has been clarified.
33183	29	11	29	16	The ERF is now defined explicitly without reference to a timescale (e.g. Box 7.1 and Section 7.3.1), so for CO2 I think this has the consequence of bringing into play a "CO2 fertillization effect" adjustment too, which is not discussed. This is distinct from the physiological adjustment discussed on lines 10-12. For example, CO2 fertillization might be expected to expand the boreal forests, reducing surface albedo (a positive adjustment). This presumably occurs on a longer timescale than we typically consider ERF, so I'm not sure there is much/any literature on this explcitly as an ERF per se, but it is a consequence of this defintion of ERF that does not consider timescales. Maybe this needs to be discussed as an additional uncertainty not quantified? [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Discussion of the effect of CO2 on albedo has been added.
23311	29	12	29	12	A recent paper on the biophysical feedback can suport this argument: 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]	Accepted: This paper has been cited
46149	29	14	29	14	As surface temperatures are not allowed to respond, I assume these biophysical effects are mainly associated with changes in surface winds. An important class of emissions that are strongly wind speed dependent and could be mentioned are emissions from natural fires. Moreover, besides biophysical effects, also biochemical effects describing the effect of CO2 on biogenic emissions can influence the ERF of CO2, and it would be instructive to make this clear. I would therefore suggest to change "biophysical effects for instance on dust and biogenic emissions" to "biophysical effects for instance on dust, natural fires and biogenic emissions as well as biochemical effects on biogenic emissions", or a similar formulation. [Twan van Noije, Netherlands]	Taken into account: This sentence has been revised.
33185	30	1	30	9	It is not clear to me how the numbers in Table 7.4 have been arrived at, so a better explanation of the method is required to make this table traceable to the literature. All the caption says is that uncertainties are taken from model spread in Smith et al., but what about the actual best estimate numbers - how are they arrived at and how do they relate to Table 7.3? [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The derivation of this table has been explained
10777	30	1	30	12	I am not sure "Myhre et al 2013b" is the right reference for the AR5 value. I could find no mention of forcing for "2xCO2" forcing there. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The AR5 values has been clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
10779	30	1	30	12	I am not sure double co2 ERF/SARF was "assessed" formally in AR5. In Flato et al 2013 (Table 9.5), the multi CMIP5 model mean is given for 2xCO2 effective radiative forcing, 3.7+/-0.8 for sst method, and 3.4+/-0.8 for the regression method. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The AR5 values has been clarified
89193	30	4	30	9	The uncertainty for the cloud adjustment of 7% seems low [Gunnar Myhre, Norway]	Rejected: These are percentages of the SARF, not of the cloud adjustment itself.
37133	31	1	31	14	The absorption bandwidth in which methane (at 1.6ppmv of air) operates overlaps with the absorption bandwidth of water vapour (at 15,000ppmv). In ppmv terms, a doubling of methane is equivalent to an extra 1.6ppmv of water, which is an increase of just 0.011%. Methane's influence is so small as to be negligible so drop all discussion of it. [John McLean, Australia]	Rejected: The band overlaps are fully accounted for in these calculations.
22147	31	3	31	14	The chapter principally talks about negative adjustments then suddenly pops out an estimate more positive than AR5 which isn't therefore obvious. Is this because concentration increases overwhelm the change in radiative understanding or are there additional longwave effects that are not somehow disclosed fully? This passage of text requires redrafting such that the rationale for why the number has increased follows much more cleanly from the underlying text. [Peter Thorne, Ireland]	Taken into account: This has been rewritten to separate the contributions from concentration change and radiative efficiency
31737	31	4	31	5	I think you need to say that these SW representations have not been compared against more complex codes (at least in the published literature) and I remain concerned about the crude method of separating strat and trop T adjustments in the kernel method, when much of the SW heating is in the lower stratosphere. Perhaps there is something emerging from RFMIP? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been addressed
79213	31	5	31	5	"… adjustments are robustly acting as a negative forcing because …" [Suboptimal wording obscuring the otherwise strict forcing-adjustment-feedback-response definition used in the chapter. Better:] " … adjustments act to reduce the ERF because …" [Michael Ponater, Germany]	Taken into account: This has been reworded
31739	31	10	31	10	Here and elsewhere, I don't think "spectroscopic" is a good shorthand. From a climate point of view, the uncertainties in the underlying methane spectroscopy are rather small, and they are not the thing that keeps me awake at night. Maybe "methodological uncertainty" or "radiative modelling uncertainty", would be better, given the many fewer studies of methane SW forcing [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been rephrased
89195	31	13	31	13	When I read the increase in CH4 forcing I wondered how much was the increase in concentration, but see it is given later. To me the concentration increase for CH4 forcing is sufficiently important to mention already here. For N2O and Halocarbons the forcing increase are due to concentration so you need it only for CH4 [Gunnar Myhre, Norway]	Accepted: The concentrations and radiative efficiency effects has been separated.
77403	31	13	31	14	Detail on the contributions of the increased atmospheric concentrations of methane since the AR5 should be included as was done for CO2. [Emer Griffin, Ireland]	Accepted: The concentrations and radiative efficiency effects has been separated.
98447	31	13	31	14	The Chapter states that the historical ERF estimate from CH4 is revised upwards from 0.48 ± 0.10 W m-2 in AR5 to 0.54 ± 0.11 W m-2 in this assessment. Recognizing how damaging SLCFs can be over the short-term, in U.S., for the California Air Resources Board (Government Agency)'s Short-Lived Climate Forcer (SLCF) Reduction Strategy plan, 20-year GWPs are used to quantify emissions of SLCFs, as opposed to 100-year GWPs. For example, the current methane GWP for a time horizon of 20 years is 84 (from AR5) and lifetime of the 12 year, which combined with its large emissions, makes it an attractive target for near-term climate mitigation policies. Thus, the use of GWPs with a time horizon of 20 years better captures the importance of the SLCFs and gives a better perspective on the speed at which SLCF emission controls will impact the atmosphere relative to CO2 emission controls. We need to continue to use GWP-20 to implement the State's climate policy. AR6 has updated the GWPs for 100 and 500-year time horizons based on new chemistry and physics. It is important that they also do GWP-20 in parallel. [nehzat Motallebi, United States of America]	Taken into account: GWP20 added
32071	31	13			How does this tally with Chapter 6 page 39 line 13? It's a bit challenging to see how this section here reconciles with Table 6.4 and Thornhill et al in ACPD. Also in Table 6.4 are the two AR5 and AR6 methodologies directly comparable? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: References to chapter 6 have been added.
37137	31	17	31	24	N2O is only at about 0.32ppm, with its lowest absorption band at about 3.7microns, which overlaps with C2O and where there is very little LWRT energy, its middle absorption band around 7.8 microns where it overlaps with methane and water vapour and around 10.8 microns where it overlaps with CO2 (415ppmv) and water vapour (15,000ppmv). It is rather dishonest to imply that N2O has any bearing whatsoever on tlower tropospheric emperatures. [John McLean, Australia]	Rejected: The band overlaps are fully accounted for in these calculations.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
105539	31	19	31	24	N2O ERF and adjustments have been done with kernels by Thornhill et al (submitted) for 3 models. ERF. Is 0.27 +/- 0.05 W.m2. Contact Gill Thornhill or Ryan Kramer for specifics. Paper was submitted before 12/31/2019. [Ryan Kramer, United States of America]	Taken into account: The Thornhill study has been addressed
22149	31	19	31	24	Again, the reason for the upward revision doesn't follow cleanly from the text which would naively imply no change but with elevated range. Text requires alteration to justify more cleanly why the mean value has increased. [Peter Thorne, Ireland]	Taken into account: The implications of tropospheric adjustments on the ERF estimates have been better clarified. We now explain differences from AR5
20417	31	20	31	20	What does "without the physiological effects" mean? [philippe waldteufel, France]	Taken into account: this has been clarified
89247	31	21	31	21	(it is reasonable based on ongoing work.) [Gunnar Myhre, Norway]	Noted
89197	31	22	31	22	The uncertainty ranges referred to from order studies are mainly due to other factors than spectroscopic data. The same for halogenated species. [Gunnar Myhre, Norway]	Accepted: This has been reworded
31741	31	22	31	22	See my 31:10 comment on use of the word "spectroscopic" [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been reworded
77405	31	23	31	24	Detail on the contributions of the increased atmospheric concentrations of N2O since the AR5 should be included as was done for CO2. [Emer Griffin, Ireland]	Accepted: This has been added.
81389	31	27	31	27	Some coordination of the terminology with Chapters 2 and 6 would be advisable as various, partly overlapping terms are used (including WMGHGs, LLGHGs, synthetic GHGs, halocarbons, halogenated species, and even “halogens”). [Johannes Laube, Germany]	Taken into account: Terminology has been coordinated
83125	31	27	31	45	What about PFCs and SF6? [Terje Berntsen, Norway]	Rejected: These are listed in the table. There is no need to discuss every species in the text
37139	31	27	31	46	Halogenated species are measured in parts per trillion and are therefore totally irrelevant. And don't try to argue that their GWPs make them important when their GWPs were derived from artificial circumstances in which they are never naturally found. [John McLean, Australia]	Rejected: The ERFs for these species are listed and are shown to make a non-negligible contribution.
83027	31	27	31	46	The discussion of the RF due to halocarbons needs to be complemented with a quantification of the RF due to the ozone loss which they cause. I can't see this in the subsequent section that deals with stratospheric ozone changes (these are caused also by a variety of factors that generally offset the ODS impact). A new paper (https://www.essoar.org/doi/10.1002/essoar.10502742.1) by Morgenstern et al. (an early version was uploaded to the IPCC repository in 2019) discusses this and finds a larger absolute offset due to ozone depletion than the central estimate of AR5. There may also be a need to coordinate this with Ch6. [Olaf Morgenstern, New Zealand]	Taken into account: Reference to indirect chemical forcings has been added
100453	31	27	31	46	Please note that a follow-up review paper of the Hodnebrog et al. (2013) paper is currently under 2nd review in Reviews of Geophysics (Hodnebrog et al., submitted), and should be relevant here (and also for Section 7.6.2.5 and associated tables (Table 7.15 / Table 7.A.3)). The follow-up paper includes new radiative efficiency and GWP calculations for a large number of compounds, based on a vast number of experimental absorption spectra that were not included in the previous 2013 review nor in the WMO (2018) report. While the lifetimes were mainly taken from the WMO (2018), the method for calculating REs was improved as described in detail in Shine and Myhre (2020, The Spectral Nature of Stratospheric Temperature Adjustment and its Application to Halocarbon Radiative Forcing, Journal of Advances in Modeling Earth Systems, 12(3), e2019MS001951, doi: 10.1029/2019MS001951), e.g., by using a revised radiative efficiency curve (so-called "Pinnock curve") that accounts for stratospheric temperature adjustment (in contrast to earlier versions of the curve which give instantaneous radiative efficiency). [Øivind Hodnebrog, Norway]	Taken into account: This new paper has been taken into account
89201	31	30	31	30	Ozone reductions above ~25 km give a positive forcing (see ozone radiative kernels) so 'no evidence to support' seems a too strong statement to me. [Gunnar Myhre, Norway]	Taken into account: The stratospheric ozone section has been rewritten following Skeie et al.
31743	31	30	31	30	There is a significant update to the Hodnebrog paper in revision cycle at Reviews of Geophysics. It includes various methodological improvements over that the WMO REs. Sorry if the LA's were not made aware of this - it may be a "too many cooks" situation [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This new paper has been taken into account
46151	31	31	31	31	Box 6.1 is not the right reference here. [Twan van Noije, Netherlands]	Accepted: The reference has been updated.
46153	31	31	31	34	I find this very confusing. Many of the halogenated species are included as SLCFs (Chapter 6), so it makes absolutely no sense to consider them as WMGHGs. By changing the section title to "Greenhouse Gases" (see my comment above), there is no need to treat them as WMGHGs, because then the section also deals with short-lived greenhouse gases. [Twan van Noije, Netherlands]	Taken into account: These are referred to a Greenhouse Gases
37573	31	32			Although the CMIP6 protocol calls for vertically-varying concentrations of some greenhouse gases including halogenated species, a survey undertaken for the Radiative Forcing MIP suggests that not a single climate model will implement these, so the radiative efficiencies described here won't apply to climate model simulations [Robert Pincus, United States of America]	Accepted: This point has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37143	31	37	31	46	The glossary to AR6 defines the stratosphere as "extending from about 10 km (ranging from 9 km at high latitudes to 16 km in the tropics on average) to about 50 km altitude". (The term "tropics" usually refers to a band of latitudes - no definition in the glossary - and 50% of the Earth's surface is between 30N and 30S, so I suspect either that the definitiion should refer to the equator rather than the tropics or thet the stratophere starts closer to 12km, but no matter.) My point is that from about 10km HGH molecules radiate photons of energy in any direction and given that some go upwards where the air density is lower, they have a greater probability of escaping into space, what's more the probability increases with altitude. As with all GHGs up in the stratosphere, the radiation is increasingly into space rather than back towards the Earth's surface. What's more if they do radiate downwards there's a high probability that they will be absorbed by another GHG molecule that reradiates upwards them again. Further, at low levels in the atmosphere the absorption wavelength of many anthropogenic GHGs overlap with the bandwidth of water vapour with its far greater ppmv but in the stratosphere, where GHGs radiate IR into space water vapour is no longer present. More of these GHGs means more radiation into space. [John McLean, Australia]	Noted: No suggestions made. None of these comments affects the text here.
100455	31	38	31	40	The uncertainties of 13% and 23% are the total radiative forcing uncertainty, while the spectroscopic uncertainty is one of several factors contributing to these values. Also, the above-mentioned paper contains updated estimates of radiative forcing uncertainties for halocarbon species. [Øivind Hodnebrog, Norway]	Accepted: The discussion of uncertainties has been revised
31745	31	39	31	39	See my 31:10 comment on use of the word "spectroscopic" - the Hodnebrog paper details the many non-spectroscopic components that contribute to these uncertainty estimates (and our newer paper (see 31:30) includes some additional ones, including the systematic neglect of shortwave absorption for the halocarbons). [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been revised to "radiative modelling uncertainties".
98449	31	43	31	46	The chapter states that the ERF from HFCs (which will be controlled under the Kigali Amendment to the Montreal Protocol) has increased by 0.012 ± 0.03 W m-2,and indicates that the concentration changes mean that the total ERF from halogenated species has increased since AR5 from 0.360 ± 0.036 W m-2 to 0.376 ± 0.058 W m-2. However, Chapter 7, Table 7.15 and Table 7.A.3 leave out metrics with timescales shorter than 50 years as does all the accompanying text for SLCF including HFCs. In the U.S., California Air Resources Board (CARB), the 100-year and 20-year GWP limits are both critical to the development of CARB's HFC regulation, both the enacted regulation backstopping the federal Significant New Alternative Policy (SNAP) prohibitions and proposed GWP limits for refrigeration and air conditioning. In addition, our regulations focus on the emissions impact of SLCFs so it is most likely that CARB's analysis would change with this new CGTP metric which incorporates emissions of longer-lived GHGs. [nehzat Motallebi, United States of America]	Taken into account: GWP20 added to the supplement
37141	31	45	31	45	Why do you cite Myhre et al (2013b) when it makes no mention of "SARF"? [John McLean, Australia]	Rejected: AR5 used the terminology RF for SARF
100457	31	49	32	34	The newly accepted Skeie et al. paper (in npj Climate and Atmospheric Science) should be highly relevant here. It provides historical radiative forcing estimates for ozone based on 10 CMIP6 models, and rapid adjustments due to ozone have also been estimated. [Øivind Hodnebrog, Norway]	Taken into account. This paper has been cited
89199	31	49	32	34	Skeie et al. (2020) is recently accepted and useful for discussion of adjustment, results from CMIP6 with atmospheric chemistry and trend since 2014. [Gunnar Myhre, Norway]	Taken into account: This paper has been cited
10781	31	49			Sub-section 7.3.2.5 should be moved out of section 7.3.2. Ozone is not a well-mixed greenhouse gas. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This section has been renamed
128905	31	51	32	6	This paragraph points specifically to two recent papers that provide model estimates of ozone ERF, MacIntosh et al. (2016) and Xie et al. (2016). While the paragraph spends time describing some findings from MacIntosh et al., it does not provide any information about findings from Xie et al. Adding some description of the relevant results from that paper would be helpful. [Trigg Talley, United States of America]	Taken into account: Xie has been discussed
35853	31	51	32	34	A paper accepted in principle in npj Climate: Skeie et al. "Historical total ozone radiative forcing derived from CMIP6 simulations" is relevant for this section. This study use ozone fields from CMIP6 historical simulations and radiative kernels. The focus is on total ozone RF, as the split between stratospheric ozone and tropospheric ozone is not easy. Tropospheric ozone precursors also affect ozone in the stratosphere, and ozone depleting substances also affect ozone in the troposphere. Results for strat O3 RF, trop O3 RF as well as RF due to ozone precursors based on separate simualtions are also presented in addition to ERF estimates based on kernels. The forcing is stronger compared to IPCC AR5 and Checa-Garcia et al., 2018 due to updated emission inventory for CMIP6. [Ragnhild Skeie, Norway]	Taken into account: This paper has been cited

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
37527	31	51	32	34	Unless the models used to estimate the SARF and ERF of ozone have been validated then there is no basis for this section, in which case it should be removed. [John McLean, Australia]	Rejected: The text explains why models are used here.
105541	32	8	32	8	adjustments to ozone for present day relative to pre industrial concentrations have been quantified by Skeie et al. (submiited) using 3 PDRMIP models. Paper was submitted before 12/31/2019 [Ryan Kramer, United States of America]	Taken into account: The ozone section has been revised.
46155	32	8	32	9	For ozone, SARF is used as an approximation for ERF. Wouldn't it be possible to account for adjustments in an approximate way? For instance, for tropospheric ozone, assuming a similar relative correction between 0 to 10% as for CO2. [Twan van Noije, Netherlands]	Taking into account: The ozone section has been revised.
37575	32	8			Simply enumerating the results of a range of studies is less helpful than a synthesis or assessment [Robert Pincus, United States of America]	Taken into account: The ozone section has been revised.
31747	32	22	32	22	Are ozone trends referred to elsewhere in the report? I think this refers to the global mean? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Chapter 6 has been referred to
31749	32	32	32	32	Checa Garcia do indicate a distinctly different time variation of strat ozone forcing, compared to AR5, with positive forcing (likely due to "tropospheric" precursors), up to about 1970. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This section has been revised following Skeie
79215	32	39	32	46	There is no example given for forcing agents impacting on tropical tropopause temperature. Old examples could be CFC changes (Forster and Joshi, Climatic Change 2005) or ozone changes near the tropopause (Stuber et al., Climate Dynamics, 2005), yet more recent examples might be found, especially related to near tropopause warming from various CFC or HCFC agents [Michael Ponater, Germany]	Taken into account: Examples have been provided
23903	32	51	32	53	Is it possible that to the WMGHG ERF the effect of CO2 in only about 7.7 %? This appears way too small; is there perhaps a typo? Otherwise, I comment on that must be in order. [Branko Grisogono, Croatia]	Taken into account: This has been rephrased to clarify
23905	32	51	32	53	The last sentence: '…a comment on… - TNX to the clumsy xlxs framefork…. While not willing to retype again… [Branko Grisogono, Croatia]	Taken into account: This has been rephrased to clarify
37145	32	51	37	51	The ERF in 1750 is unknown and unknowable because measurements were not made back then. All that you have are estimates, not only for 1750 but for almost every year since then. [John McLean, Australia]	Rejected: The text explains how ERFs are derived from radiative transfer calculations.
77407	32	52	32	54	The synthesis contains details not included in the main text. This could be used in the Exec Summary and SPM [Emer Griffin, Ireland]	Taken into account: This has been addressed and ES revised
31751	32	53	32	53	I am not sure that "halocarbons" is a good short-hand, and it is not consistenly applied in the chapter. "halogenated compounds" perhaps? We have had trouble coming up with a catch-all short-hand characterisation of the zoo of molecules that are in this category, but halogens feels too short. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Halogenated Species used.
128907	32	55	33	1	It would be helpful to indicate where (later) in the document the topic of the chemical impacts of WMGHGs on ozone and aerosols is discussed. [Trigg Talley, United States of America]	Taken into account: Reference to the emission based forcing has been added.
128909	33	7	34	3	Hoepfner et al. (2012) notes the following conclusion, "This work challenges a common perception on the negligible role of O2 and N2 as natural greenhouse gases in the Earth's atmosphere compared to species like CH4 or N2O. It is in fact the large abundance of oxygen and nitrogen which compensates for their only weak interaction with infrared radiation through collision-induced absorption bands. We have shown that for hypothetic atmospheres consisting of only single gases the natural greenhouse effect of O2 and N2 together would be larger than that of CH4 by a factor of around 1.3." As such, IPCC WGI might want to consider assessing the findings of this paper in terms of the positive radiative forcing effects of O2 and N2. Admittedly, the paper talks about a hypothetical single gas atmosphere, but still, since Hoepfner et al. (2012) was initially published, there may have been several papers citing it over the past 8 years, and those papers may or may not have been supportive of the findings in that paper, and so suggest that an evaluation of its findings in that context may be worthwhile given that one typically notes that oxygen and nitrogen, unlike greenhouse gases, are transparent to incoming sunlight. Therefore, some statement evaluating Hoepfner et al. (2012) in that context would be worthwhile with respect to inquiries from people who stumble across this paper and believe that O2 and N2 may play an outsized role in radiative forcing. Citation: Hoepfner, M., M. Milz, S. Buehler, J. Orphal, and G. Stiller, 2012: The natural greenhouse effect of atmospheric oxygen (O2) and nitrogen (N2). Geophys. Res. Lett., 39, L10706, doi:10.1029/2012GL051409. [Trigg Talley, United States of America]	Rejected: This section addresses gases that have changed since 1750.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31753	33	9	33	9	In the body of the table, it is not clear enough that the penultimate section (CFCs… halogens) is the sum of other individual molecules higher up the table, and then that "halogens" is the sum of them all. I have come across a few instances where the similar AR5 table has been misinterpreted by people summing the entire table [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account this table has been reformatted
37147	33	9	33	16	For a clear comparison of the GHGs, all of the concentrations should be given in the same units (either ppb or ppt) [John McLean, Australia]	Rejected: It is more concise to use different units.
37149	33	9	33	16	Water vapour should be included in the table because it too is a greenhouse gas. [John McLean, Australia]	Rejected: Water vapour is discussed in 7.3.4.1
71719	33	9	33	16	(Table 7.5) Most of the values shown under "with respect to 1850" are the same as those under "with respect to 1750". I would suggest dropping one of these, preferably "with respect to 1850" as 1750 has been accepted previously as a pre-industrial reference point. Also, there is a lot of overlap between this table and Table 7.8 given in the summary in Section 7.3.5.2, so it would be better to merge the two tables and put it in section 7.3.2.7. The table in the summary section 7.3.5.2 could be more useful if it clarified the different types of radiative forcing metric by having separate columns for the "instantaneous/direct RF", to the left of ERF, and for "Total ERF", to the right [Martin Manning, New Zealand]	Rejected: The 1850 values are useful for energy budget assessments.
81391	33	9	33	16	This table and the underlying calculations need coordinating with the updated Chapter 2 as some compounds have been removed from the table (and others added) and discussion in Section 2.2 to which the reader is referred to here. When comparing with Chapter 2, it is not clear what has been included in the ERF calculations for halocarbons. What about: CH2Cl2? CHCl3? CH3Cl? CH3Br? HFC-227ea? HFC-43-10mee? C3F8? c-C4F8? In addition, and as pointed out in my comments on Chapter 2, currently missing are many minor halocarbons (e.g. CFC-113a, -114a, HCFC-124, -133a, -31, c-C4F8O, SF5CF3, n-C4F10, n-C5F12, n-C6F14, i-C6F14, and n-C7F16) that, in sum, have a much larger radiative forcing effect than some of the compounds listed here. I urge the authors to either consistently exclude compounds with ERFs below a certain limit or to consider a fairer representation of the peer-reviewed literature here. [Johannes Laube, Germany]	Taken into account: This table has been aligned with Chapter 2.
18283	33	9	34	3	Table 7.5. Were "halogens" defined? Were previous "halocarbons" renamed to "halogens", as they include SF6 etc? Note that Table 7.8 uses "halogens" and Figure 7.10 uses "halocarbons". [Yugo Kanaya, Japan]	Taken into account: Halogenated Species used.
46159	33	9			Table 7.5: Please change "WMGHGs" to "GHGs" (reflecting that many of the halogenated species are SLCFs), and for completeness include tropospheric and stratospheric ozone, and stratospheric water vapour in the table. [Twan van Noije, Netherlands]	Taken into account: Halogenated species used.
46161	33	13	33	13	Correct "RF" to "ERF". [Twan van Noije, Netherlands]	Accepted: This has been corrected.
32073	33	16			Would be helpful to add 2019 concentration values [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: 2019 added
32075	33	16			For methane I'm having trouble comparing this ERF value with Chapter 6 Table 6.4 and the Chap 6 cited reference Thornhill et al (ACPD under discussion). There seem to be many different values or different +/- errors here. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been clarified
46157	33	52	33	52	Change "time period" to "reference year". [Twan van Noije, Netherlands]	Accepted: This has been changed.
38051	34	6	34	6	The authors may want to refer to "Aerosol-radiation-cloud interactions". [Junhee Lee, Republic of Korea]	Rejected. The ERF is discussed separately for aerosol-radiation and aerosol-cloud interactions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79789	34	6			The report should highlight that anthropogenic aerosols can affect the radiative forcing by natural aerosols. There are strong evidences that the chemical influence of air pollution on aeolian dust contributes to the aerosol cooling through the direct radiative effect (Lelieveld et al., 2019; Klingmueller et al., 2019) and weaker the cooling by aerosols through the indirect radiative effect (Karydis et al., 2017; Klingmueller et al., 2020). The dust aging by anthropogenic pollution has multiple consequences, such as increased solar radiation scattering from hygroscopic particle growth and decreased lifetime from more efficient rainout. Furthermore, since dust particles are globally abundant and relatively large in size, suppress the cloud formation from the smaller anthropogenic particles, reducing the cloud water and hence the reflection form solar radiation. To account for these effects, the CTMs and CCMs should simulate the particle chemistry and thermodynamics of crustal ions that currently are not included in the IPCC climate models. Karydis, V. A., Tsimpidi, A. P., Bacer, S., Pozzer, A., Nenes, A., and Lelieveld, J.: Global impact of mineral dust on cloud droplet number concentration, Atmospheric Chemistry and Physics, 17, 5601-5621, 10.5194/acp-17-5601-2017, 2017 Klingmuller, K., Lelieveld, J., Karydis, V. A., and Stenchikov, G. L.: Direct radiative effect of dust-pollution interactions, Atmospheric Chemistry and Physics, 19, 7397-7408, 10.5194/acp-19-7397-2019, 2019. Klingmuller, K., Karydis, V. A., Bacer, S., Stenchikov, G. L. , and Lelieveld, J.: Weaker cooling by aerosols due to dust-pollution interactions, Atmospheric Chemistry and Physics Discussions, acp-2020-531, 2020. Lelieveld, J., Klingmüller, K., Pozzer, A., Burnett, R. T., Haines, A., and Ramanathan, V.: Effects of fossil fuel and total anthropogenic emission removal on public health and climate, Proceedings of the National Academy of Sciences, 116, 7192-7197, 10.1073/pnas.1819989116, 2019. [Alexandra Tsimpidi, Germany]	Taken into account. This is mentioned as a source of uncertainty, and for further details the reader is referred to Ch. 6.
37577	34	6			The framing of aerosol forcing uses four components: IRFari, the direct impact on the radiation budget of aerosols; ERFari, which includes non-cloud (?) adjustments; IRFaci, i.e. changes to the radiation budget from the instantaneous cloud response to aerosols (i.e. smaller drops and brighter clouds); ERFaci, which include adjustments to cloudiness (here parameterized as "cloud fraction" and liquid water path). A strict interpretation of adjustments -- "changes in state that affect the radiation budget in the absence of surface temperature change -- would imply that all changes to clouds, including the brightening due to increased drop concentrations, would be considered adjustments, eliminating the need for a distinction between ari and aci. Recognizing the historical roots of this distinction the more elaborate decomposition should be justified by explaining how it adds understanding. [Robert Pincus, United States of America]	Taken into account. Exactly which adjustments are included in each of the ERF terms has now been clarified.
37151	34	8	34	8	Anthropogenic activity is not the only cause of aerosols. As with the previous section of this chapter, your focus on anthropogenic issues is contrary to the title of this chapter, "The Earth's energy budget, climate feedbacks and climate sensitivity", which says nothing about any cause. [John McLean, Australia]	Rejected. Anthropogenic activity is the primary source of aerosol changes since pre-industrial times (which is what is relevant in this context). Natural aerosol changes driven by climate change are considered feedbacks, and therefore discussed in Section 7.4.
30635	34	8	34	9	Again, it is inaccurate to say increases in aerosol emissions. Should be increases in emissions of aerosols and aerosol precursors. [Hong Liao, China]	Accepted. Text has been revised accordingly.
77409	34	8	34	10	Most aerosol materials are formed from precursor gases. This is not apparent from text which refers to aerosol emissions which are primary aerosols. [Emer Griffin, Ireland]	Accepted. Text has been revised to reflect the fact that both direct emissions of aerosols and their precursors are relevant here.
77411	34	8	34	10	The use of IRF and ERF is quite obscure; could cloud and non cloud effects be used for clarity? [Emer Griffin, Ireland]	Rejected. The adjustments are primarily, but not only, operating through clouds. The section also needs to be consistent with the definitions of adjustments introduced in Box 7.1.
98655	34	8	35	34	While the aerosol chapters mentions chapter 6 - I still feel there is not a perfect handover done. In particular an assessment of overall global observed aerosol AODs and trends is not dine using any surface observations. Global aerosol optical properties and recent trends compared between models and observations, as done in the AeroCom papers by Gliss et al and Mortier et al (along with other AOD and aerosol optical trend papers) seem to fall in between the two chapters. What is the observed global mean aerosol trend in AOD, SSA, CCN? Are models consistent with the observed trends? What implications for the undestanding of the aerosol forcing history? Are we sure we have a clear representative picture of the trends since 1950 ? While dimming and brightning is discussed more broadly in the chapter, other aerosol network observations fall a bit short. [Michael Schulz, Norway]	Taken into account. A better handover between Ch. 6 and Ch. 7 has been ensured, and the requested material has been added to Ch. 6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
46163	34	9	34	9	Please change "aerosol emissions" to "emissions of aerosols and aerosol precursor gases". [Twan van Noije, Netherlands]	Accepted.
35945	34	9	34	10	Could also point to Chapter 2 Figure 2.9 [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
79791	34	10			Nitrate and ammonium are also worth mentioning here. In contrast to the IPCC AR5, recent model intercomparison studies have shown that the radiative forcing induced by aerosol nitrate is significant (Myhre et al., 2017); especially when models take into account the interactions with mineral dust. Nitrate formation on dust particles control most of its global budget and determine its distribution between fine and coarse size aerosols (Bian et al., 2017). This also emphasizes the need for all CTMs and CCMs to include nitrate in their suit of aerosol components. Bian, H. S., Chin, M., Hauglustaine, D. A., Schulz, M., Myhre, G., Bauer, S. E., Lund, M. T., Karydis, V. A., Kucsera, T. L., Pan, X. H., Pozzer, A., Skeie, R. B., Steenrod, S. D., Sudo, K., Tsigaridis, K., Tsimpidi, A. P., and Tsyro, S. G.: Investigation of global particulate nitrate from the AeroCom phase III experiment, Atmospheric Chemistry and Physics, 17, 12911-12940, 10.5194/acp-17-12911-2017, 2017. Myhre, G., Aas, W., Cherian, R., Collins, W., Faluvegi, G., Flanner, M., Forster, P., Hodnebrog, Ø., Klimont, Z., Lund, M. T., Mülmenstädt, J., Lund Myhre, C., Olivié, D., Prather, M., Quaas, J., Samset, B. H., Schnell, J. L., Schulz, M., Shindell, D., Skeie, R. B., Takemura, T., and Tsyro, S.: Multi-model simulations of aerosol and ozone radiative forcing due to anthropogenic emission changes during the period 1990–2015, Atmos. Chem. Phys., 17, 2709-2720, 10.5194/acp-17-2709-2017, 2017. [Alexandra Tsimpidi, Germany]	Noted. This is covered in Chapter 6
71063	34	19	34	19	According to P24L8-9, "rapid" shold be removed here. The same applies throughout Section 7.3.3. [Yu Kosaka, Japan]	Accepted.
46165	34	21	34	22	"smaller but more numerous cloud droplets": this is not necessarily the case; it will depend on the sign of the aerosol changes associated with the forcing. Please generalize the formulation. [Twan van Noije, Netherlands]	Accepted. Text has been revised accordingly.
95869	34	23	34	25	These lines seem to imply that CCN changes do not affect ice crystal numbers but the evidence suggests otherwise (Koren et al., 2010; Dagan et al., ACP, 2020). [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been revised to reflect that CCN changes can also change ice crystal number.
18941	34	23	34	25	The hygroscopic growth of aerosols appear to also have a large impact on aerosol-radiation interaction as shown in very recent paper: Krishnamohan, KS, G. Bala, Long Cao, Lei Duan and Ken Caldiera, 2020: The climatic effects of hygroscopic growth of sulfate aerosols in the stratosphere, Earth's Future, https://doi.org/10.1029/2019EF001326 [Govindasamy Bala, India]	Noted
18943	34	23	34	25	The heating caused by aerosols in the stratosphere could also have a significant effect on the effective radiatiative forcing as demonstrated recently by this paper: Krishnamohan, KS, G. Bala, Long Cao, Lei Duan and Ken Caldiera, 2019: Climate System Response to Stratospheric Sulfate Aerosols: Sensitivity to Altitude of Aerosol Layer, Earth System Dynamics, https://doi.org/10.5194/esd-10-885-2019 [Govindasamy Bala, India]	Noted
65723	34	33	34	33	Suggest clarification of the phrase: " present-day is equivalent to 2010s". [Kushla Munro, Australia]	Accepted. Clarification has been added.
46167	34	33	34	34	I have the impression no attempt is made in this section to remove the effects of the response of surface air temperatures over land. In that case, it should be mentioned that the ERF estimates presented in this section are really fixed-SST forcings. [Twan van Noije, Netherlands]	Accepted. This has now been addressed explicitly.
35947	34	33	34	34	It would be good to clarify how the conversion to 2018 has been done. Scaling by emissions? [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarification has been added.
40659	34	37	34	37	Please review and revise as necesary the existing glossary definitions for 'Aerosol-radiation interaction' and its subterms('ERFari', 'RFari', 'ERFari+aci'). Consider adding a definition for IRFari. [TSU WGI, France]	Accepted.
89217	34	37	37	7	Previous IPCC assessments have provided IRFari estimates for various aerosol components. I think it is valuable even this is becoming more complicated from model simulations due to advanced aerosol schemes. Due to length constraints is this something for a short discussion in a supplementary? [Gunnar Myhre, Norway]	Taken into account. The assessment of forcing associated with individual aerosol species is given in Chapter 6.
37155	34	44	34	44	What is it that you are trying to claim this is evidence of? [John McLean, Australia]	Noted. The section presents evidence for the magnitude of the ERF due to aerosol-cloud interactions, as is stated right above.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89203	34	44	35	34	I've not looked at all these studies, but some of them insufficiently take into account that anthropogenic aerosols are more absorbing than current total aerosol abundance. This is simply because BC has increased more during the industrial era than scattering aerosols. Many (probably all if not combined with models) satellite derived approaches therefore provides a too negative IRFari. [Gunnar Myhre, Norway]	Taken into account. There is a discussion in the section of how updates to anthropogenic absorption affect estimates. Further, the overall assessment of ERFari combines observation-based and satellite-based lines of evidence, so does not rely exclusively on the evidence presented in this section.
37579	34	44			The lines of evidence here are relevant because they are global, not because they were obtained from a particular class of observing platforms [Robert Pincus, United States of America]	Accepted. The subsection title has been revised to "observation-based lines of evidence"
17327	34	45	34	45	define REari upon first use [David Neubauer, Switzerland]	Not applicable. The text in question has been removed.
41495	34	45	34	46	What is REari? I don't think it is defined. I don't think you mean ERFari? If so, why would that be easier to define than IRFari, if IRFari is a component of ERFari? [Andrew Gettelman, United States of America]	Not applicable. The text in question has been removed.
128911	34	45	34	51	Where is REari defined? [Trigg Talley, United States of America]	Not applicable. The text in question has been removed.
37157	34	45	35	26	This whole section is based on estimates. If you can't prove that those estimates are reasonably accurate then the section is mere speculation. [John McLean, Australia]	Noted. The section is based on a large body of literature based on models and observations. These combined support the overall assessments.
16159	34	45			Suggest "The total effect of natural and anthropogenic aerosols" for clarity. I took "total" to mean ari+aci. [Steven Sherwood, Australia]	Not applicable. The text in question has been removed.
46169	34	47	34	50	Please mention the year or period for which these estimates have been obtained. The estimates from Lacagnina et al. are for 2006. [Twan van Noije, Netherlands]	Not applicable. The text in question has been removed.
16161	34	49	34	51	Ths doesn't add up. The planet is 75% ocean, so to get from ~ –5 ocean to –2 total, the land value would have to be +7! [Steven Sherwood, Australia]	Not applicable. The text in question has been removed.
77413	34	51	34	51	Is this correct? Surely the radiative properties are independent of the underlying surface? [Emer Griffin, Ireland]	Not applicable. The text in question has been removed.
41497	34	53	34	54	Might be good to note that IRFari has both positive (Absorption) and negative (Scattering) components. [Andrew Gettelman, United States of America]	Accepted. The absorption AOD is mentioned here, and contributions to forcing from individual aerosol species are presented in Chapter 6.
16163	34	53	35	26	This text states that the new values are scattered around the AR5 value of –0.35, but all the values given are more negative than −0.35. This needs to be clarified—if there are other values they should be given, perhaps in a table? [Steven Sherwood, Australia]	Taken into account. Ma et al. (2014) showed a all-sky IRFari of -0.3 W m-2, less negative than -0.35 W m-2. Note also that the IRFari assessment based on observation-based lines of evidence is more negative at -0.4Wm-2.
37581	35	1	35	14	Simply enumerating the results of a range of studies is less helpful than a synthesis or assessment [Robert Pincus, United States of America]	Accepted, The text has been revised and now emphasizes assessment more.
35949	35	4	35	5	Suggest replacing "Rémy et al. (2018) applied the methods of Bellouin et al. (2013b)" with a single reference to Bellouin et al. (2020) https://doi.org/10.5194/essd-2019-251 -- that paper is more relevant for that statement. Same comment for reference to Remy et al. on lines 16 and 18. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Sentence was kept in order to facilitate tracing of estimates to the original methodological paper.
22151	35	12	35	14	Unless I misread the paragraph they are not scattered evenly (as would be implied by a naïve reader) around the AR5 estimate with more being substantively lower (more negative) than higher (less negative)? [Peter Thorne, Ireland]	Rejected. The text does not say evenly scattered, so it is not incorrect as is. Note also that the assessment is different from the AR5 central estimate, so the exact wording here is not critical to the assessment.
46171	35	22	35	25	Please clarify how this narrower range has been obtained. [Twan van Noije, Netherlands]	Accepted. The sentence prior to the assessment states what has improved to allow for a slightly narrower range.
37159	35	29	36	44	Models NEVER provide evidence unless it can be shown that the models are accurate in every regard. Climate models are not accurate, so this section is dishonest. [John McLean, Australia]	Rejected. All studies come with uncertainty, whether observational- or modelling-based. They still represent different lines of evidence supporting the assessment, as they do throughout the report and for a wide range of assessments.
46173	35	31	35	34	Please note that this distinction can be made for models that use a double call to the radiation scheme. This effectively provides both total (fixed-SST) ERF and ERFari (including semi-direct effects). [Twan van Noije, Netherlands]	Not applicable. This text has been removed.
41499	35	33	35	33	Are you including semi-driect in IRFari? Maybe state this here. [Andrew Gettelman, United States of America]	Noted. IRF doesn't include adjustments like the semi-direct effect, as stated in the introduction paragraph to section 7.3.3.
16167	35	36	36	16	This section should introduce/clarify why we are looking at models for IRFari when it can be observed (the previous text explains that we need models for ERFari, but doesn't mention IRFari). [Steven Sherwood, Australia]	Taken into account. Because also the observational lines of evidence rely on assumptions and come with uncertainties, it is relevant to bring in modelling evidence for IRFari as well. This rationale is explained in the introduction to section 7.3.3.
23907	35	36	36	16	Why twice the same sub-title: 'Model-based estimates of IRFari'? [Branko Grisogono, Croatia]	Accepted. The section has been restructured and rewritten such that this is no longer an issue.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79793	35	40	35	41	While this is true for the total SO2 emitted worldwide, the SO2 emission trends show high variability in different regions. The decline of SO2 emissions has continued during the last two decades over Europe and North America. On the other hand, SO2 emissions over Asia have increased drastically after 2000. After Beijing summer Olympics, SO2 emissions have dropped over East Asia while soaring in South Asia making India the world largest SO2 emitter currently (Li et al., 2017). Overall, it is worth mentioning that regional SO2 emission trends since 2007 have been so drastic that inventories and scenarios tend to underestimate them. Li, C., McLinden, C., Fioletov, V., Krotkov, N., Carn, S., Joiner, J., Streets, D., He, H., Ren, X., Li, Z., and Dickerson, R. R.: India Is Overtaking China as the World's Largest Emitter of Anthropogenic Sulfur Dioxide, Scientific Reports, 7, 14304, 10.1038/s41598-017-14639-8, 2017. [Alexandra Tsimpidi, Germany]	Noted. Regional patterns of forcing are covered in Chapter 6.
89205	35	43	35	43	Paulot et al showed that the model results compared well to satellite derived clear sky IRFari from 2001 to 2015, therefore I suggest to modify the sentence to increase the importance of that study. [Gunnar Myhre, Norway]	Taken into account. We have chosen to emphasize your (Myhre et al.) paper, because it is a multi-model study, but have revised the text to explain how it is supported by the single-model study of Paulot et al.
77415	35	44	35	45	This is interesting, but no quantification is provided. How is this reflected in data and figures on ERF? [Emer Griffin, Ireland]	Accepted. Forcings for individual aerosol species are given in Chapter 6.
77417	35	44	35	45	References for statements on organic carbon should be provided. [Emer Griffin, Ireland]	Accepted. The reference regarding changes to organic carbon forcing changes was already given above (Lund et al, 2018)
30633	35	55	36	9	Please be clear about what aerosol species were considered when forcing values were presented. I assume forcings of all aerosol species are presented here. Did the studies cited consider nitrate aerosol? [Hong Liao, China]	Accepted. Forcings associated with different species and the extent to which they are included in models are assessed in Chapter 6.
37583	35	55	36	9	Simply enumerating the results of a range of studies is less helpful than a synthesis or assessment [Robert Pincus, United States of America]	Taken into account, assessment of ERF based on CMIP6 experiments and expert judgement should be given.
2687	36	1	36	8	spell out CEDS, AOD [Bryan Weare, United States of America]	Accepted. CEDS is spelled out and defined in Chapter 6.
128913	36	1	36	9	Although all CMIP6 aerosol-climate models take the CEDS dataset for anthropogenic emissions, model treatments of precursor gas emissions for secondary aerosols (e.g., SOA and sulphate) and/or the gas-to-particle conversion are believed to be very different. Wang et al. (2020) show that a change in the SOA treatment in the E3SM model causes a difference of 0.15 (out of 0.5 W/m2) in RFari. This uncertainty is certainly worth noting here. Reference: Wang, H., Easter, R. C., Zhang, R., Ma, P.L., Singh, B., Zhang, K., et al. (2020). Aerosols in the E3SM Version 1: New developments and their impacts on radiative forcing. Journal of Advances in Modeling Earth Systems, 12, e2019MS001851. https://doi.org/10.1029/2019MS001851 [Trigg Talley, United States of America]	Taken into account. Uncertainties related to precursor gases are discussed in Chapter 6 and also in 7.3.3.2.2.
16165	36	1			Please define CEDS or else don't use it [Steven Sherwood, Australia]	Accepted. CEDS has been defined in Chapter 6.
79795	36	2	36	3	Current CTMs and CCMs simulate only POA or some of them include just one SOA surrogate species and assign to it some generic properties. However, beside the number and mass concentrations of OA, both its physical and chemical states determine its ability to absorb and scatter the solar radiation and its cloud condensation nuclei efficiency. The report should emphasize that the current bulk representation of OA by the CTMs and CCMs hinder their ability to provide accurate climate assessments (Carslaw et al., 2013) and should highlight the need to recast the modelling approach for representing the OA formation and physicochemical evolution. Carslaw, K. S., Lee, L. A., Reddington, C. L., Pringle, K. J., Rap, A., Forster, P. M., Mann, G. W., Spracklen, D. V., Woodhouse, M. T., Regayre, L. A., and Pierce, J. R.: Large contribution of natural aerosols to uncertainty in indirect forcing, Nature, 503, 67-+, 10.1038/nature12674, 2013. [Alexandra Tsimpidi, Germany]	Noted. Forcings associated with individual aerosol species are assessed in Chapter 6, as explicitly stated in the introduction to Section 7.3.3
71065	36	11	36	11	RFari -> IRFari [Yu Kosaka, Japan]	Accepted.
16169	36	11	36	14	I am not expert on this but it seems implausible to me that we are twice as confident in model predictions of aerosol forcing as we are in satellite observations, given the enormous complexity of aerosol proceses. Could we be confusing model consensus with certainty here? Values are quoted from only two GCMs (why only these?), and then a citation to Bellouin et al. but without saying anything about what (other) model values were given therein. This seems way to casual/flimsy to justify such a ocnfident assessed result. [Steven Sherwood, Australia]	Take into account. Satellite-based estimates also come with large uncertainty, and while it may seem surprising that there is smaller uncertainty range associated with model-based lines of evidence the range is indeed narrower based on model agreement, simply because of the narrower range spanned by model-based estimates. Since the overall assessment uses both lines of evidence, both uncertainty ranges ultimately feed into the assessed range.
77419	36	11	36	14	The material on black carbon should be clearer in terms of its ERF and presentation of this in figures. [Emer Griffin, Ireland]	Taken into account. As stated explicitly in the introduction to Section 7.3.3, forcings associated with individual species are assessed in Chapter 6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83759	36	12	36	12	"This represents a significant decrease" is confusing because -0.2 is an increase from -0.35. Maybe replace with "the currently assessed Rfari is less negative than AR5" or similar? Also, what is the difference between RF_ari and IRF_ari? [Marvel Kate, United States of America]	Accepted. Sentence has been rewritten.
5241	36	18	36	32	It would be good to note that the Samset and Myhre paper found that the adjustments are very dependent on the altitude of the black carbon. [Daniel Murphy, United States of America]	Not applicable. The sentence, including the reference to Samset and Myhre, has been removed.
89207	36	19	36	19	Incorrect to use 'principally', see Figure 7.6b [Gunnar Myhre, Norway]	Rejected. This statement is consistent with the assessed literature.
95871	36	21	36	24	This is interesting but there are questions if adjustments in 10xBC experiments are representative for PD/PI. This should at least be acknowledged. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Since evidence for adjustments come from studies also unrelated to the idealized PDRMIP simulations, and since they generally support the PDRMIP findings, the estimated adjustments are unlikely to be artefacts of highly idealized perturbations.
89209	36	21	36	28	All studies mentioned here, expect Smith et al. (2018b) quantified only the total adjustment. Smith et al. quanitified the various adjustements as given in Figure 7.6b. Put more emphasis on Smith et al. and maybe remove references to some of the other papers (I am co-author on several of them). [Gunnar Myhre, Norway]	Accepted. Smith et al. (2018) is indeed emphasized in the final assessment.
46175	36	24	36	26	Please clarify how offline radiative transfer calculation can provide information on adjustments. [Twan van Noije, Netherlands]	Not applicable. The text in question has been removed.
46177	36	27	36	32	The term "rapid adjustments" isn't consistent with the new definiton of ERF (see page, lines 5 to 9). Please explain why the adjustments considered in these study are "rapid adjustments", and note the inconsistency. [Twan van Noije, Netherlands]	Accepted. All adjustments are now referred to without any qualifiers.
13521	36	28	36	28	Add space between parenthesis and "also". [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
69605	36	28	36	28	insert space after Suzuki (2019) [Nicholas Golledge, New Zealand]	Accepted.
89211	36	29	36	32	It is correct that less BC in the middle and upper troposphere leads to weaker cloud adjustment and observations indicate that in this part of the atmosphere several models overestimate the BC concentration. However, Allen et al. use a method which is questionable and leads to very strong non-cloud rapid adjustment in many models. Since models which include double radiation calls provide IRF and ERF as direct output it is easy to quantify the total adjustment. Results from Allen et al. is in conflict to Smith et al (2018b) and it is unlikely that radiative kernel for temperature and water vapour are incorrect at that level. I encourage the authors to assess the study by Allen et al if included. [Gunnar Myhre, Norway]	Taken into account. While there was no space to go into this level of technical details in the text, the assessment clearly reflects that the assessment places more confidence in Smith et al. (2018) and supporting studies.
46179	36	34	36	42	Again, clarify if these are these ERF estimates are fixed-SST approximations or not. [Twan van Noije, Netherlands]	Accepted. The text now explicitly states that a correction to these estimates is needed to account for land surface cooling in order to make them consistent with the ERF definition.
52067	36	40	36	40	More model-based estimates are available in: Fiedler, S., Kinne, S., Huang, W. T. K., Räisänen, P., O'Donnell, D., Bellouin, N., Stier, P., Merikanto, J., van Noije, T., Makkonen, R., and Lohmann, U.: Anthropogenic aerosol forcing – insights from multiple estimates from aerosol-climate models with reduced complexity, Atmos. Chem. Phys., 19, 6821–6841, https://doi.org/10.5194/acp-19-6821-2019, 2019. [Fiedler Stephanie, Germany]	Rejected. The text already cited 6 supporting single-model studies, which was deemed sufficient.
95873	36	41	36	41	"Combining CMIP5 and CMIP6…"model based evidence is assessed to be…" It is not clear what this is now really based on. Only CMIP5/CMIP6? Including the individual modelling studies listed above? Including / excluding AeroCom? [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Text has been rewritten.
17329	36	42	36	49	Is the model-based ERFari -0.25 ± 0.25 Wm-2 or -0.25 ± 0.2 Wm-2 or? The same value for the uncertainty should be used in both lines [David Neubauer, Switzerland]	Accepted. Text has been revised and is now consistent in this respect.
46181	36	49	36	49	As indicated in line 42, the range shouldn't be -0.25 +- 0.2 but -0.25 +- 0.25 W/m2. [Twan van Noije, Netherlands]	Accepted.
16171	36	50			What "recent literature"? Also, I gather from this paragraph that Bellouin et al. did not include adjustments. Can you clarify? If this isn't the case, then why do you come in 0.05 lower than them? Is it new evidence or just different judgments? [Steven Sherwood, Australia]	Taken into account. The relationship between this assessment and Bellouin et al is now more explicit.
89213	36	52	36	52	An upper range of 0.0 Wm-2 for ERFari is not much supported by recent studies either from various observations or modelling studies. An estimate of 0.0 Wm-2 would need a strong IRFari from BC (or absorbing OA), but then the negative adjustments would be stronger than in the assessment. Additionally, the lower range seems strong in magnitude to me. Maybe consider a more narrow range? [Gunnar Myhre, Norway]	Taken into account. The justification for the range has been improved.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
35951	36	53	36	53	Slight confusion with numbers here: ERFari in Bellouin et al. (2020) doi:10.1029/2019RG000660 ranges from -0.71 to -0.14 W m-2 -- which is indeed consistent with your assessment of -0.3 +/- 0.3 W m-2. The numbers you give currently for Bellouin et al. (2020) are for Rfari. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thanks!
46183	36	53	53	53	Please mention that the given range estimated by Bellouin et al. is for IRFari. [Twan van Noije, Netherlands]	Taken into account. This should in fact have been ERFari - corrected now.
37161	37	3	37	7	The caption needs to say that these are estimates from unvalidated models. [John McLean, Australia]	Rejected. Models are validated against observations. See numerous model description papers.
83761	37	3	37	9	Optional, but it would be nice to have columns representing IRF and adjustments (ie, the components that sum to ERF) for both ARI and ACI in each model) [Marvel Kate, United States of America]	Taken into account. As the APRP method used to derive these estimates does not separate instantaneous forcing (ERFari or ERFaci) from adjustments, we do not do this here.
128915	37	3			In Table 7.6, double check if the positive ERFari in CESM2 is due to the use of a different approach in the forcing calculation. The number looks quite suspicious. There are many other CMIP6 models, of which the results are available by now. Their forcing estimates might not be included in source of this table (Smith et al., submitted), but some of them have been published (e.g., Golaz et al., 2019; Rasch et al., 2019). It is strongly recommended to include as many models as possible. References: Golaz, J.,ÄêC., Caldwell, P. M., Van Roekel, L. P., Petersen, M. R., Tang, Q., Wolfe, J. D., et al. (2019). The DOE E3SM coupled model version 1: Overview and evaluation at standard resolution. Journal of Advances in Modeling Earth Systems, 11, 2089-2129. https://doi.org/10.1029/2018MS001603 Rasch, P. J., et al. (2019). An overview of the atmospheric component of the Energy Exascale Earth System Model. Journal of Advances in Modeling Earth Systems, 11, 2377-2411. https://doi.org/10.1029/2019MS001629 [Trigg Talley, United States of America]	Taken into account. Thank you for the comment. CESM2 was indeed incorrect, as the model version supplied incorrectly included ozone changes as well as aerosol changes. The correct number is in Smith et al 2020b and the FGD. As (1) there is no separation into ERFari and ERFaci in the E3SM papers and (2) the "present-day" period is mid-2000s rather than 2014 we will not include this model in the table. The single-model study of Golaz et al will be referenced in the text and is within the range of model estimates of aerosol forcing.
89215	37	12	37	12	The authors are probable aware of Gryspeerdt et al. 2020 and the section would benefit including this reference. [Gunnar Myhre, Norway]	Taken into account. Gryspeerdt et al. (2020) is now referenced in the text, thanks.
95877	37	12	37	12	I was missing acknowledgment and a brief discussion of potential aerosol effects on convective clouds and deep convection. True, we cannot attribute forcings but there is evidence that aerosol perturbations also cause radiative perturbations - which are currently not included in the current generation of CMIP6 models. Dagan et al, ACP, 2020 shows some of these but there is also work by Fan et al etc. This is separate from the invigoration debate... [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The discussion about aerosol effects on deep convection has been expanded slightly, and it has been acknowledged as an important source of uncertainty.
40657	37	12	37	12	Please review and revise as necesary the existing glossary definitions for 'Aerosol-cloud interaction' and its subterms ('ERFaci', RFaci'). Consider adding a definition for IRFaci. [TSU WGI, France]	Accepted. Glossary definitions has been reviewed and revised.
37585	37	12			The description of cloud-aerosol interactions is compact and accurate, but why is this material separate from discussions of adjustments? [Robert Pincus, United States of America]	Taken into account. The cloud albedo effect is not an adjustment, and therefore needs to be discussed separately. The adjustments due to aerosol scattering/absorption are also separate from for example cloud lifetime effects, which would be adjustments to aerosol-cloud interactions, so the current separation of these topics is needed.
78717	37	14	37	15	Change "cloud condensation nuclei (CCN)" to only "CCN", was defined above! [Heike Wex, Germany]	Accepted. Changes made.
46185	37	15	37	15	Consistent with the use of CCN and LWP, I would abbreviate cloud droplet number concentration as CDNC and use N_d only in formulas. [Twan van Noije, Netherlands]	Accepted.
77421	37	15	37	15	Surprised that the 1977 publication by Twomey is not included here. [Emer Griffin, Ireland]	Accepted. The Twomey paper has been cited.
16691	37	15	37	18	the description “Increasing Nd while holding liquid water path (LWP, i.e., the vertically integrated cloud water) constant reduces cloud drop effective radius …”. In my opinion, using “liquid water content (LWC)” is more reasonable. [Chuanfeng Zhao, China]	Accepted.
3531	37	18	37	18	IRFari should be IRFaci here [Joyce Penner, United States of America]	Accepted. Thanks!
37587	37	18	37	21	The description of how clouds react to increased aerosol concentrations is conststent with hypotheses developed in the 1980s and 1990s and does not do justice to the far more nuanced current understanding. Indeed, all things being equal, smaller drops lead to less precipitation, and might be hypothsized to increase cloud water content or areal coverage, but most of the evidence presented later in the chapter shows that this simple view is incomplete. At the least this section should acknowledge that aerosol-cloud systems are often buffered, in the language of Stevens and Feingold 2009. [Robert Pincus, United States of America]	Accepted. Text has been modified to reflect the content of the remainder of the section.
46187	37	19	37	20	I find it a bit confusing that lifetime is a property of an individual cloud, while LWP and C_f are column properties. I would suggest to change the formulation to "Rain generally reduces cloud lifetime and thereby LWP and/or cloud fractional coverage (C_f)". [Twan van Noije, Netherlands]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
128917	37	20	37	25	It really needs to be recognized that the role of precipitation on ACI is profound, as for example illustrated in the studies of Golaz et al., 2013; Geophys.Res. Letters, 40(10), 2246-2251. https://doi.org/10.1002/grl.50232. and more recently Jing et al., 2019; J. Climate, 32(14), 4409-4430, https://doi.org/10.1175/JCLI-D-18-0789.1. The importance of precipitation is wholly understated in this chapter. This understatement has major implications for how one might assign "confidence" with respect to progress on both ACI and also on low cloud feedback. It is worth noting that the role of precipitation on ACI also depends on precipitation phase and the underlying processes associated with cold versus warm precipitation. This point was underscored in the study of Christensen et al. (2016; Geophys. Res. Lett., 41, 6970-6977, doi:10.1002/2014GL061320). [Trigg Talley, United States of America]	Taken into account. Aerosol effects on precipitation are addressed in Ch. 8, but are certainly relevant for the adjustments that influence ERFaci as well. This is now stressed in the chapter, primarily by referring to Ch. 8 material.
78719	37	22	37	23	In short, I suggest to change the sentence to: "However, atmospheric observations indicated that adding aerosols to non-precipitating clouds reduces LWP (Lebsock ...". This is because it would be good if it was mentioned if these studies are modelling studies or atmospheric observations. Also, I don't understand why it is said that it is an opposite effect on LWP - above it is dealt with the theoretical construct of keeping LWP constant and then of the case when precipitation occurs. This here is a different case. Hence this suggestion to alter the sentence. [Heike Wex, Germany]	Taken into account. Text has been reworded to address these concerns.
46189	37	24	37	25	Please also mention cloud top changes, as these are discussed later in the section. [Twan van Noije, Netherlands]	Accepted.
41501	37	27	37	29	We know that adding sulfate will change homogeneous nucleation of ice crystals: this is more certain than black carbon, and this effect has been modeled. Adding sulfate would tend to increase crystal numbers, with radiative effects. This is included in ERFaci in several climate models. I see this is stated in paragraph on L33 below: but this paragraph on L27 needs to be better integrated and clarified. Maybe remove the 'and influence ice crystal number in cirrus clouds' which is treated in the paragraph below. That would largely separate mixed phase from pure ice (cirrus). [Andrew Gettelman, United States of America]	Taken into account. Text has been reworded to address these concerns.
66587	37	28	37	30	Suggetion for using the most updated reference for black carbon IN negligibility - Black Carbon Particles Do Not Matter for Immersion Mode Ice Nucleation, Zamin A. Kanji et al. https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019GL086764 [Naruki Hiranuma, United States of America]	Accepted. Thanks!
9867	37	28	37	30	The description here should be updated. A very recent study (Zhao et al. 2019, Nature Geosciences, https://doi.org/10.1038/s41561-019-0389-4) showed polluted aerosols in Asia contain a considerable fraction of ice nucleating particles impacting ice particle radius in convective clouds by combining 11-year observations from multiple satellites and cloud-resolving model simulations. [Jiwen Fan, United States of America]	Accepted. The Zhao et al. study is now referenced.
78721	37	30	37	30	There are also atmospheric measurements on this, hence it could be added after "... Temprado, 2018)": "and likewise also in the polluted environment of Beijing (Chen et al., 2018)." ----- here the citation: Chen, J., Wu, Z., Augustin-Bauditz, S., Grawe, S., Hartmann, M., Pei, X., Liu, Z., Ji, D. and Wex, H. (2018). Ice nucleating particle concentrations unaffected by urban air pollution in Beijing, China, Atmos. Chem. Phys., 18, 3523–3539, doi:10.5194/acp-18-3523-2018. [Heike Wex, Germany]	Taken into account. These papers has now been cited to support the statement.
41503	37	31	37	31	You might clarify that effects of aerosols on cirrus clouds are included in several models (both sulfur and black carbon). Sulfur effects are a bit more constrained and certain than BC. This is different than 'cloud phase changes' you are declining to assess here. [Andrew Gettelman, United States of America]	Accepted.
17337	37	35	37	37	A recent study found that the ice nucleation ability of soot is enhanced by aging in the atmosphere: Mahrt et al., (2020), Aging induced changes in ice nucleation activity of combustion aerosol as determined by near edge X-ray absorption fine structure (NEXAFS) spectroscopy, Environ. Sci.: Processes Impacts, 2020,22, 895-907 [David Neubauer, Switzerland]	Taken into account. This study is now cited, along with several others, to inform the assessment of the ability of soot to act as INPs.
41505	37	36	37	36	Again, you discuss black carbon, but we know that sulfate will tend to increase ice crystal numbers by lowering homogeneous freezing thresholds. [Andrew Gettelman, United States of America]	Taken into account. The role of sulfate in ERFs via cirrus clouds is now discussed in greater depth.
52065	37		37		Table 7.6: MPI-ESM estimates are available from: Fiedler, S., Kinne, S., Huang, W. T. K., Räisänen, P., O'Donnell, D., Bellouin, N., Stier, P., Merikanto, J., van Noije, T., Makkonen, R., and Lohmann, U.: Anthropogenic aerosol forcing – insights from multiple estimates from aerosol-climate models with reduced complexity, Atmos. Chem. Phys., 19, 6821–6841, https://doi.org/10.5194/acp-19-6821-2019, 2019. [Fiedler Stephanie, Germany]	Taken into account. Thanks for the suggestion, but this model has not been included in the table on two counts: (1) there is no separation into ERFari and ERFaci in the paper; (2) the "present-day" period is mid-2000s rather than 2014. This and other single-model studies will be mentioned in the text as supporting the multi-model results.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
116607	37		37		It would be helpful to expand on the comparison between CMIP5 and CMIP6 and reasons for changes (and links between forcing diagnosed in CMIP6 models and model spread). The table 7.6 reports results on different periods could results be compared for the same periods. [Valerie Masson-Delmotte, France]	Noted. The reasons you suggest are discussed in Smith et al 2020 (the source of the CMIP6 results) so for brevity and because the results are actually not that different we do not expand on it here. Results cannot be compared for the same period for CMIP6 as CMIP5 because the experiment design represents a single year (e.g. we cannot diagnose year-2000 aerosol forcing in CMIP6 models).
37163	38	1	38	1	What does this purport to be evidence of? [John McLean, Australia]	Taken into account. We have made these subtitles more informative.
46191	38	1			Section 7.3.3.2.1: In this section aerosol-cloud interactions are described in terms of changes in LWP, Cf, and to some extent cloud top height. Are there no relevant observational studies that describe the interactions in terms of vertical profile changes, liquid and ice contents and cloud optical depth (see also page 62, line 54)? [Twan van Noije, Netherlands]	Taken into account. Unfortunately, the literature on such adjustments is very limited.
46193	38	3	38	4	Yes, but the AR6 definition of ERF is not consistent anymore with this framework. [Twan van Noije, Netherlands]	Taken into account. We have added this as a qualifier.
1643	38	4	38	5	“(ii) the literature assessing statistical relationships between aerosol- and cloud retrievals has grown”. Yet this literature (especially the most recent) does not receive proper mention in 7.3.3.2.1. A lot of the discussion is framed around LWP and Nd, quantities that are not directly retrievable from passive spectroradiometers that have global coverage (see separate comment). An example of an overlooked large-scale, large-volume study that falls in the category of investigating statistical relationships is Oreopoulos et al. (2020): A global survey of apparent aerosol-cloud interaction signals. Journal of Geophysical Research: Atmospheres, 125, e2019JD031287. https://doi.org/10.1029/2019JD031287. This study avoids the more uncertain LWP and Nd retrievals, uses two different AOD datasets, addresses multiple cloud classes (regimes) and finds that the independently-derived Cloud Radiative Effect (both SW and LW) from CERES almost always increases with AOD for all cloud regimes. [Lazaros Oreopoulos, United States of America]	Taken into account. This new paper, which was not available in time to be considered for the SOD, has now been cited and discussed.
128919	38	4	38	5	"... (ii) the literature assessing statistical relationships between aerosol-and cloud retrievals has grown, ..." Yet this literature (especially the most recent) does not receive proper mention in 7.3.3.2.1. A lot of the discussion is framed around LWP and Nd, quantities that are not directly retrievable from passive spectroradiometers that have global coverage. An example of an overlooked large-scale, large-volume study that falls in the category of investigating statistical relationships is Oreopoulos et al. (2020): A global survey of apparent aerosol-cloud interaction signals. Journal of Geophysical Research: Atmospheres, 125, e2019JD031287. https://doi.org/10.1029/2019JD031287. This study avoids the more uncertain LWP and Nd retrievals, uses two different AOD datasets, addresses multiple cloud classes (regimes) and finds that the independently-derived Cloud Radiative Effect (both SW and LW) from CERES almost always increases with AOD for all cloud regimes. [Trigg Talley, United States of America]	Taken into account. This new paper, which was not available in time to be considered for the SOD, has now been cited and discussed.
1645	38	8	38	8	Does the sub-heading need to add the word “anthropogenic”? This subsection vacillates between referring to cloud modifications by anthropogenic only and all aerosol. Quite a few results from volcanoes are being quoted and these certainly do not produce anthropogenic aerosols. Can one extent conclusions from aerosol-cloud interactions where the aerosol is natural (e.g., sea salt) to interactions where the aerosol has anthropogenic origin? The distinction of aerosol becomes more clearer only later in p. 7-40. There it is implied that one needs anthropogenic aerosol to estimate IRFaci and ERFaci. Under “Progress in satellite-based investigations of aerosol-cloud interactions” perhaps no mention at all should be made about IRFaci and ERFaci, just about statistical relationships, since the forcings have their own dedicated subsections. [Lazaros Oreopoulos, United States of America]	Taken into account. A clearer distinction between studies that inform how aerosols might affect clouds (and which could use volcanoes as analogues) and studies that yield ERF estimates (which only include anthropogenic aerosols) has now been made.
128921	38	8	38	8	Does the sub-heading need to add the word "anthropogenic"? This subsection vacillates between referring to cloud modifications by anthropogenic only and all aerosol. Quite a few results from volcanoes are being quoted and these certainly do not produce anthropogenic aerosols. Can one extend conclusions from aerosol-cloud interactions where the aerosol is natural (e.g., sea salt) to interactions where the aerosol has anthropogenic origin? The distinction of aerosol becomes more clearer only later, on page 7-40. There, it is implied that one needs anthropogenic aerosol to estimate IRFaci and ERFaci. Under "Progress in satellite-based investigations of aerosol-cloud interactions" perhaps no mention at all should be made about IRFaci and ERFaci, just about statistical relationships, since the forcings have their own dedicated subsections. [Trigg Talley, United States of America]	Taken into account. A clearer distinction between studies that inform how aerosols might affect clouds (and which could use volcanoes as analogues) and studies that yield ERF estimates (which only include anthropogenic aerosols) has now been made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1647	38	10	38	28	There is complete absence of discussion on why LWP and Nd are the preferred quantities (dominating this subsection) for framing the discussion aerosol-cloud interaction. Yes, they are the fundamental cloud physical properties affected by aerosol, but their retrievals are more uncertain than those for the optical properties cloud optical thickness and cloud effective radius. LWP comes from these two optical properties assuming either an invariant or a linearly-increasing LWC profile, both approximations. Nd retrievals are based on an adiabatic cloud model. Grosvenor et al. (2018) (cited) conducts an error analysis, the major findings of which should be quoted in this report. But the bottom line is that a passive spectroradiometer retrieves simultaneously cloud optical thickness and cloud effective radius using two channels and then two more quantities (LWP and Nd) are retrieved from these measurements. Four variables come out from two measurements! If there are multiple ways that cloud optical thickness, cloud effective radius, LWP and Nd are related, one cannot infer four quantities from two measurements. This major drawback of studies relying on LWP-Nd relationships should be mentioned. Given the above, I disagree that studies relying on droplet effective radius lead to "problematic results (lines 21-22). What ultimately matters for IRFaci and ERFaci is whether cloud optical depth, cloud fraction, and to a smaller extent cloud top height (or more accurately the effective cloud top height which controls the upward cloud longwave emission) are affected by (anthropogenic) aerosol. These are the quantities that regulate the cloud radiative effect and are derived directly from passive spectroradiometers. I understand that without inferring effects on LWP and Nd, there is limited physical understanding on processes taking place and model evaluation, but we can't escape the fact that the MODIS-like observations have limitations. [Lazaros Oreopoulos, United States of America]	Taken into account. The focus on LWP and droplet number mainly reflects what quantities are most frequently reported in the literature, when including observation-based and modelling-based studies. However, the fact that there are uncertainties associated with these retrievals is undisputable, and the section now stresses that more.
128923	38	10	38	28	There is complete absence of discussion on why LWP and Nd are the preferred quantities (dominating this subsection) for framing the discussion on aerosol-cloud interaction. Yes, they are the fundamental cloud physical properties affected by aerosol, but their retrievals are more uncertain than those for the optical properties (cloud optical thickness and cloud effective radius). LWP comes from these two optical properties assuming either an invariant or a linearly increasing LWC profile, both approximations. Nd retrievals are based on an adiabatic cloud model. Grosvenor et al. (2018) conducts an error analysis, the major findings of which should be quoted in this report. But the bottom line is that a passive spectroradiometer retrieves simultaneously cloud optical thickness and cloud effective radius using two channels and then two more quantities (LWP and Nd) are retrieved from these measurements. Four variables come out from two measurements! If there are multiple ways that cloud optical thickness, cloud effective radius, LWP and Nd are related, one cannot infer four quantities from two measurements. This major drawback of studies relying on LWP-Nd relationships should be mentioned. Given this, studies relying on droplet effective radius should not be cast as leading to "problematic results" (lines 21-22). What ultimately matters for IRFaci and ERFaci is whether cloud optical depth, cloud fraction, and to a smaller extent cloud top height (or more accurately the effective cloud top height which controls the upward cloud longwave emission) are affected by (anthropogenic) aerosol. These are the quantities that regulate the cloud radiative effect and are derived directly from passive spectroradiometers. Without inferring effects on LWP and Nd, there is limited physical understanding on processes taking place and model evaluation, but one cannot escape the fact that MODIS-like observations have limitations. [Trigg Talley, United States of America]	Taken into account. The focus on LWP and droplet number mainly reflects what quantities are most frequently reported in the literature, when including observation-based and modelling-based studies. However, the fact that there are uncertainties associated with these retrievals is undisputable, and the section now stresses that more.
17331	38	16	38	16	The anthropogenic fraction of AI (or aerosol in general) remains a considerable source of uncertainty. This needs to be mentioned explicitly [David Neubauer, Switzerland]	Accepted. This source of uncertainty has now been acknowledged.
22153	38	24	38	24	biased towards low values is ambiguous. Do you mean that the effect is underestimated or that the resulting estimates are too negative? Suggest to clarify. [Peter Thorne, Ireland]	Accepted. Ambiguity has been addressed.
46195	38	30	38	30	Please be more specific about the aerosol characteristics for which these relationships have been found. Do the relate to abundance, number concentration, optical depth and/or index? Also indicate this for the relationship mentioned on page 39, line 35. [Twan van Noije, Netherlands]	Taken into account. The relationship in question has been clarified in these instances.
95875	38	30	38	45	New work by Christensen et al., PNAS (accepted) using geostationary satellite data in a novel Lagrangian framework provides additional support for extended cloud persistence under polluted conditions. We will provide the accpted manuscript to the chapter authors. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paper is now cited.
17345	38	33	38	35	Also the processing of aerosol in clouds and precipiation can confound aerosol-cloud interactions (Neubauer et al., 2017). [David Neubauer, Switzerland]	Taken into account. This point is now made explicitly and Neubauer et al. (2017) cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31475	38	33	38	35	High temporal observation capability by the new geostationary satellite (e.g. Himawari-8 satellite) has been employed in the recent study to reduce the contamination of aerosol retrievals next to clouds, using the spatiotemporal differences between aerosol and cloud (Kikuchi et al. 2018). It is recommended that the authors address this to describe the future expected improvements in cloud fraction/LWP and aerosol relationship estimation. Kikuchi, M., Murakami, H., Suzuki, K., Nagao, T. M., and Higurashi, A., Improved Hourly Estimates of Aerosol Optical Thickness using Spatiotemporal Variability Derived from Himawari-8 Geostationary Satellite, IEEE Trans. Geosci. Remote Sens., 56, doi: 10.1109/TGRS.2018.2800060, 2018 [Maki Kikuchi, Japan]	Rejected. Potential future advances are not part of the assessment.
17333	38	35	38	39	Another method to avoid possible influencing factors such as relative humidity is the careful sampling done by Christensen et al. (2017). Thereby satellite pixels affected by water uptake in the humid environment adjacent to clouds are excluded. This approach reduces the dependence on derived (and therefore uncertain) quantities such as Nd (Grosvenor et al., 2018). [David Neubauer, Switzerland]	Taken into account. Both studies, Christensen et al. (2017) and Grosvenor et al. (2018) are cited in the section.
78723	38	37	38	38	Why "Another solution to this problem ..."? Better "Another approach to tackle this problem ..." [Heike Wex, Germany]	Accepted.
1649	38	41	38	41	"RETRIEVED Nd". [Lazaros Oreopoulos, United States of America]	Accepted.
46197	38	41	38	41	Please change "N_d" to "N_d retrieved from satellites" or "CDNC retrieved from satellites" (see my comment to page 37, line 15). [Twan van Noije, Netherlands]	Accepted.
128925	38	41	38	41	"RETRIEVED Nd". [Trigg Talley, United States of America]	Accepted.
65009	38	44	38	45	"larger" than what? I think the result is more or less the same than if using the regular Nd retrieval. [Johannes Quaas, Germany]	Taken into account. It has been clarified what "larger" is relative to.
2689	38	45			spell out Cf [Bryan Weare, United States of America]	Accepted.
1651	38	47	38	50	In view of how LWP and Nd are retrieved from MODIS, these finding should be treated as quite uncertain. [Lazaros Oreopoulos, United States of America]	Taken into account. The uncertainty is now explicitly acknowledged.
128927	38	47	38	50	Using MODIS (or analogous) observations to examine any relationship between Nd and LWP is somewhat meaningless. The reason is that with such measurements Nd and LWP are not independent and it's a matter of emphasis as to how you want to interpret given changes in reflected sunlight at vis and nir wavelengths either in an Nd-centric context or LWP context. This is described in Stephens et al. (2019, QJR Meteorol.Soc., DOI: 10.1002/qj.3589). While Nd is a property of clouds that directly links to aerosol, thus being central to the ACI problem, the fundamental process that governs Nd is precipitation (Wood et al., 2012; Journal of Geophysical Research, 117, D19210.https://doi.org/10.1029/2012JD018305). This serves to make the point again of the predominant role of precipitation on the cloud microphysics and on how clouds respond to the presence of aerosol. [Trigg Talley, United States of America]	Taken into account. Aerosol effects on precipitation are addressed in Ch. 8, but are certainly relevant for the adjustments that influence ERFaci as well. This is now stressed in the chapter, primarily by referring to Ch. 8 material.
128929	38	47	38	50	In view of how LWP and Nd are retrieved from MODIS, these finding should be treated as quite uncertain. [Trigg Talley, United States of America]	Taken into account. The uncertainty is now explicitly acknowledged.
35953	38	49	38	49	Rosenfeld has since published an erratum https://science.sciencemag.org/content/364/6446/eaay4194 that brings its estimate in line with the other studies cited. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The erratum is already cited.
46199	38	50	38	51	Aerosol emissions are not observed, so such "observed relationships" are only partly based on observations. [Twan van Noije, Netherlands]	Taken into account. We now write "observation-based".
20419	38	52	38	54	Should one understand that, according to this study, getting the ships to use oil-free energy sources would divide by 4 the ERF_aci for midlatitude stratocumulus? [philippe waldteufel, France]	Taken into account. No, this is not what one should understand. As already specified, this applies to the ERFaci associated with marine stratocumulus only. Further, the 3/4 contribution would apply to all aerosol sources. The sentence has been revised to avoid confusion.
41507	39	3	39	3	Mace and Abernathy 2016, also found higher cloud tops for Kilauea plume clouds. Mace, G. G., and A. C. Abernathy. "Observational Evidence for Aerosol Invigoration in Shallow Cumulus Downstream of Mount Kilauea." Geophysical Research Letters, January 1, 2016, 2016GL067830. https://doi.org/10.1002/2016GL067830 [Andrew Gettelman, United States of America]	Taken into account. This paper has now been cited in the somewhat expanded section on aerosol effects on cloud top.
46201	39	3	39	3	It could be mentioned earlier that changes in cloud top height can also contribute to ERFaci. [Twan van Noije, Netherlands]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1653	39	17	39	18	Reference to Koren et al. (2005) without a qualifier is misleading and may be irrelevant since the Koren study focuses on convective and high clouds. Up to this point in 7.3.3.2.1 only liquid clouds have been discussed (the entire discussion is framed in terms of Nd and LWP). [Lazaros Oreopoulos, United States of America]	Taken into account. The discussion about aerosol effects on deep convection has been expanded slightly, and the Koren et al. study has been folded into that.
128931	39	17	39	18	Reference to Koren et al. (2005) without a qualifier is misleading and may be irrelevant since the Koren study focuses on convective and high clouds. Up to this point in 7.3.3.2.1, only liquid clouds have been discussed (the entire discussion is framed in terms of Nd and LWP). [Trigg Talley, United States of America]	Taken into account. The discussion about aerosol effects on deep convection has been expanded slightly, and the Koren et al. study has been folded into that.
95879	39	17	39	23	I agree that a full discussion of convective invigoration goes beyond the chapter - but at the same time this paragraph seems to lack a bit of depth to do this topic justice. There exist many satellite based papers, many CRM studies, also as part of ACPC and a small number of GCM based studies (Nober et al., 2003, Lohmann 2008, Thayer-calder et al., 2012, Donner / et al. 20211 and recently Kipling et al., ACP, 2020 https://doi.org/10.5194/acp-20-4445-2020 - finding fairly small overall effects) [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The discussion about aerosol effects on deep convection has been expanded slightly (without adding excessive length to an already long chapter).
23909	39	18	39	23	Why spending so many lines on a small hypothesis? - So what? Please try to shorten the whole Report. [Branko Grisogono, Croatia]	Rejected. Spending 5 lines assessing a relationship that could have a major influence on ERFaci is not considered unnecessary.
16173	39	18			Nishant et al. 2017 (10.1002/2017GL073267) show via regional modeling that the more recent Koren et al. result (and likely others in subtropical regions) is due to meteorological covariation. I would suggest given this and other studies that a stronger statement can be made in this paragraph. [Steven Sherwood, Australia]	Taken into account. The suggested paper has been cited.
1655	39	25	39	27	“Identifying relationships between INP concentrations and cloud properties from satellites is intractable because the INPs generally represent a very small subset of the overall aerosol population at any given time or location.” This is a misleading statement. The main reason the problem is intractable is because we do not have any information (cannot retrieve) INPs. INP concentrations are simply unknown! [Lazaros Oreopoulos, United States of America]	Rejected. This is exactly what is stated.
128933	39	25	39	27	"Identifying relationships between INP concentrations and cloud properties from satellites is intractable because the INPs generally represent a very small subset of the overall aerosol population at any given time or location." This is a misleading statement. The main reason the problem is intractable is because no information exists (cannot retrieve INPs). INP concentrations are simply unknown! [Trigg Talley, United States of America]	Rejected. This is exactly what is stated.
2691	39	25			spell out INP [Bryan Weare, United States of America]	Accepted.
1657	39	30	39	31	“No global observational estimates of the ERFaci associated with mixed-phase clouds exist at present. For ice clouds, only few satellite studies have investigated responses to aerosol perturbations so far.” There are several problems here. Examining whether there are responses (what is referred to earlier as “statistical relationships between aerosol and cloud retrievals”) is one thing and estimating ERFaci, which requires using the anthropogenic fraction of aerosol loading, is another. One may seek or establish the statistical relationship, but not pursue a numerical value for ERFaci (the aforementioned Oreopoulos et al 2020 paper being an example). [Lazaros Oreopoulos, United States of America]	Taken into account. A clearer distinction between the two types of studies has been made.
128935	39	30	39	31	"No global observational estimates of the ERFaci associated with mixed-phase clouds exist at present. For ice clouds, only few satellite studies have investigated responses to aerosol perturbations so far." There are several problems here. Examining whether there are responses (what is referred to earlier as "statistical relationships between aerosol and cloud retrievals") is one thing and estimating ERFaci, which requires using the anthropogenic fraction of aerosol loading, is another. One may seek or establish the statistical relationship, but not pursue a numerical value for ERFaci (the aforementioned Oreopoulos et al. (2020) paper being an example). [Trigg Talley, United States of America]	Taken into account. A clearer distinction between the two types of studies has been made.
1659	39	32	39	33	Are satellite retrievals of ice crystal numbers even possible? [Lazaros Oreopoulos, United States of America]	Taken into account. Yes, but with considerable uncertainty. Examples are Mitchell et al. and Gryspeerdt et al. studies cited in the chapter.
128937	39	32	39	33	Are satellite retrievals of ice crystal numbers even possible? [Trigg Talley, United States of America]	Taken into account. Yes, but with considerable uncertainty. Examples are Mitchell et al. and Gryspeerdt et al. studies cited in the chapter.
46203	39	33	39	33	Please explain what is meant with "under strong dynamical forcing". [Twan van Noije, Netherlands]	Accepted. Clarification has been added.
1661	39	35	39	37	This statement contradicts the statement in lines 44-45 “There is no observational evidence at present for a significant response of ice clouds to aerosol perturbations.”. Having “no quantitative conclusions about IRFaci or ERFaci for ice clouds” is not equivalent to the statement in lines 44-45. [Lazaros Oreopoulos, United States of America]	Taken into account. The two statements are now consistent, reflecting the available literature.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
128939	39	35	39	37	This statement contradicts the statement in lines 44-45 that "There is no observational evidence at present for a significant response of ice clouds to aerosol perturbations." Having "no quantitative conclusions about IRFaci or ERFaci for ice clouds" is not equivalent to the statement in lines 44-45. [Trigg Talley, United States of America]	Taken into account. The two statements are now consistent, reflecting the available literature.
17341	39	35	39	38	The lack of studies investigating IRFaci and ERFaci of ice clouds from observations needs to be taken into account in the uncertainty of the satellite based estimates of IRFaci and ERFaci of all clouds presented in the following paragraphs. A lack of studies is not the same as certainty that the forcing from ice clouds is small. (See for example Section 7.4.2.4.2. and Table 7.9 where the lack of studies of the tropical high-cloud amount feedback leads to a large uncertainty of this feedback) [David Neubauer, Switzerland]	Accepted. This is a fair point. The uncertainty range for satellite-based studies has now been slightly expanded to reflect this.
1663	39	41	39	42	I don't agree with the "high confidence" assigned to Nd responses. I would feel more comfortable if the statement was about decrease in cloud droplet size which is more directly observed. [Lazaros Oreopoulos, United States of America]	Rejected. The two are largely equivalent, and no available study supports a lack of relationship or a negative relationship.
128941	39	41	39	42	[CONFIDENCE] "High confidence" should not be assigned to Nd responses. Iff the statement was about decrease in cloud droplet size, which is more directly observed, then maybe. [Trigg Talley, United States of America]	Rejected. The two are largely equivalent, and no available study supports a lack of relationship or a negative relationship.
3533	39	42	39	43	I don't believe an assessment of "high confidence" in this statement is warrented, given the discussion of both positive and negative impacts on LWP discussed above - or - are you saying if you apply an average of all results, you are highly confident there are no large changes to LWP? please clarify how you come to this assessment of "high confidence" [Joyce Penner, United States of America]	Accepted. This has been changed to "medium confidence".
128943	39	42	39	44	[CONFIDENCE] The discussion about LWP effects glosses over needed nuanced complexity. The high confidence stated as the LWP adjustments are far morec omplex than the discussion on page 39 acknowledges. For example, the Malvelle et al. study in fact shows that within the domain considered there were in fact large LWP change of alternating sign that when averaged over the region is indeed small. Similarly, the global response exhibits positive and negative sensitivities of LWP to aerosol that depend on cloud regime and that tends to produce a cancelled and smaller global effect as the Toll et al. reference noted, but this doesn't mean the adjustment or its global response are understood or that these adjustments are unimportant. Open cellular cloudiness, for example, tends to exhibit a strong positive LWP effect and more closed cellular cloudiness a negative to neutral effect, a point well illustrated in the Christensen ship track studies. Neither is it understood nor can models be shifted in open and closed cellular PBL cloud adequately enough to be confident about being able to represent their combined effects on LWP adjustments. [Trigg Talley, United States of America]	Accepted. This has been changed to "medium confidence", which better reflects the complexities discussed in the subsection.
37589	39	43			What is the evidence for even medium confidence that liquid cloud fraction increases with aerosol concentrations? This would seem to come from page 38, lines 38-45, but one might also take away from this paragraph that any signal is subtle and hard to observe. [Robert Pincus, United States of America]	Rejected. "Medium confidence" is not a very strong confidence statement, and the multiple studies that find a positive aerosol - cloud cover relationship support this.
128945	39	44	39	45	This statement is not true. There are studies that showed aerosols affect ice cloud optical thickness, cloud fraction and mass-weighted cloud top height and the effects are sensitive to aerosol type. Citations: Zhao, B., Y. Gu, K.-N. Liou, Y. Wang, X. Liu, L. Huang, J. H. Jiang and H. Su, Type-dependent responses of ice cloud properties to aerosols from satellite retrievals, Geophys. Res. Lett., 45, 3297-3306. doi:10.1002/2018GL077261, 2018. Jiang, J.H., H. Su, L. Huang, Y. Wang, S. Massie, B. Zhao, A. Omar, Z. Wang, Contrasting Effects on Deep Convective Clouds by Different Types of Aerosols, Nature Communications, 9, doi:10.1038/ s41467-018-06280-4, 2018. [Trigg Talley, United States of America]	Accepted. Statement has been changed to "limited evidence" and the suggested papers have been cited.
35955	40	1	40	4	Bellouin et al. (2020) https://doi.org/10.5194/essd-2019-251 is a better reference for the estimate currently cited as Remy et al. 2018. The estimate has been revised to -0.7 W m-2. Same comment for lines 13 and 19 of this page. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Remy et al. reference was kept. However, Bellouin et al referenced elsewhere

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64543	40	8	40	8	The transition between satellite-based sections talking about aerosol cloud interaction in general and IRFaci vs ERFaci separately was jarring/disorienting. The general ACI section provides estimated values, then the IRFaci section suddenly jumps back to the beginning of trying to understand aci. A transition sentence like "Now that overall ACI has been assessed, we turn to separating out the instantaneous and environmental-response-mediated components of ACI" would be helpful. [Peter Caldwell, United States of America]	Taken into account. Such a transition sentence has now been added.
1665	40	8	40	31	Is this just for liquid clouds? This is not clarified [Lazaros Oreopoulos, United States of America]	Taken into account. The statement has been clarified.
128947	40	8	40	31	Is this just for liquid clouds? This is not clarified. [Trigg Talley, United States of America]	Taken into account. The statement has been clarified.
65011	40	10	40	26	Using POLDER aerosol retrievals, Hasekamp et al. (Nature Comm 2019, doi 10.1038/s41467-019-13372-2) propose an IRFaci that is even stronger than the one of McCoy et al. (2017), at -1.14 Wm-2. In our review Quaas et al. ACPD 2020 (doi 10.5194/acp-2020-279) we list a couple of reasons to believe that such a strong Twomey effect may be more plausible than previous satellite-based results. [Johannes Quaas, Germany]	Accepted. The two new recent studies have been added to the discussion of IRFaci.
37591	40	13	40	22	Simply enumerating the results of a range of studies is less helpful than a synthesis or assessment [Robert Pincus, United States of America]	Taken into account. The text in fact attempts at assessment, while numbers are provided only in the table. The text has been revised to give it more of an assessment flavour.
112027	40	16	40	18	This is the incorrect reference. McCoy uses the MERRA-2 aerosol reanalysis. The correct references for the MERRA-2 aerosol reanalysis are Randles et al. 2017 and Buchard et al. 2017 [Cynthia Randles, United States of America]	Accepted.
46205	40	30	40	31	Please include the range estimated by Bellouin et al. in Table 7.7. I wouldn't call the two ranges (-1.1 to -0.1 versus -1.6 to -0.2 W/m2) "broadly consistent": with the new assessment, the probability of strong negative forcings is much reduced. [Twan van Noije, Netherlands]	Accepted. The relationship between this assessment and Bellouin et al. has been clarified.
35957	40	31	40	31	Note that the Bellouin et al. (2020) 10.1029/2019RG000660 lower estimate has been revised to -1.5 W m-2 during production. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Numbers have been revised accordingly.
16175	40	31			Is the comparative range from Bellouin based on satellite studies only, or is it their overall assessed range? If the former, why is yours weaker? If the latter, why are you making the comparison? [Steven Sherwood, Australia]	Taken into account. The comparison with Bellouin et al is made only in the overall ERFaci assessment.
46207	40	35	40	36	Again, please clarify if the adjustments estimated in these studies are consistent with the AR5 or AR6 definitions of ERF. [Twan van Noije, Netherlands]	Taken into account. A clarification has been made at the beginning of the section.
17335	40	37	40	38	Also Christensen et al. (2017) account for non-causal aerosol-cloud correlations, see previous comment [David Neubauer, Switzerland]	Accepted. Reference has been added.
1667	40	39	40	39	It is unclear what “augments ERFaci relative to IRFaci” means. ERFaci adds an additional negative forcing to the existing negative IRFaci? [Lazaros Oreopoulos, United States of America]	Taken into account. The statement has been clarified.
128949	40	39	40	39	It is unclear what "augments ERFaci relative to IRFaci" means. ERFaci adds an additional negative forcing to the existing negative IRFaci? [Trigg Talley, United States of America]	Taken into account. The statement has been clarified.
3535	41	3	41	3	The table refers to Grypeerdt et al. 2018b not 2018a [Joyce Penner, United States of America]	Accepted. The correct paper is now referred to.
1669	41	5	41	11	Perhaps in this summary it should be mentioned that there are two competing ERFaci contributions, the larger negative CF increase contribution and the smaller positive LWP decrease contribution (the latter contradicting the original Albrecht hypothesis). [Lazaros Oreopoulos, United States of America]	Taken into account. The suggested wording would not accurately capture the assessment, but the text has been clarified to state clearly that changes to LWP and cloud cover represent two semi-separate contributions to ERFaci.
128951	41	5	41	11	Perhaps in this summary it should be mentioned that there are two competing ERFaci contributions, the larger negative CF increase contribution and the smaller positive LWP decrease contribution (the latter contradicting the original Albrecht hypothesis). [Trigg Talley, United States of America]	Taken into account. The suggested wording would not accurately capture the assessment, but the text has been clarified to state clearly that changes to LWP and cloud cover represent two semi-separate contributions to ERFaci.
46209	41	8	41	10	Please reformulate to clarify that the estimated range is not based on evidence from satellite observations: as explained on page 40, additional information from models (e.g. on the anthropogenic fraction) is needed to estimate IRFaci and thus ERFaci. [Twan van Noije, Netherlands]	Accepted. The reliance on models also here is now acknowledged.
37165	41	13	43	1	Models NEVER provide evidence unless it can be shown that the models are accurate in every regard. Climate models are not accurate, so this section is dishonest. [John McLean, Australia]	Rejected. Model results serve as one of multiple valid lines of evidence throughout the report.
69607	41	27	41	27	insert space after -0.9 W m-2 [Nicholas Golledge, New Zealand]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
128953	41	31	41	33	If ERFaci results from the other models, including E3SM (Gloaz et al., 2019; Rasch et al., 2019), are not going to be analyzed and included in Table 7.6, they can probably be mentioned here. References: Golaz, J., C., Caldwell, P. M., Van Roekel, L. P., Petersen, M. R., Tang, Q., Wolfe, J. D., et al. (2019). The DOE E3SM coupled model version 1: Overview and evaluation at standard resolution. Journal of Advances in Modeling Earth Systems, 11, 2089-2129. https://doi.org/10.1029/2018MS001603 Rasch, P. J., et al. (2019). An overview of the atmospheric component of the Energy Exascale Earth System Model. Journal of Advances in Modeling Earth Systems, 11, 2377-2411. https://doi.org/10.1029/2019MS001629 [Trigg Talley, United States of America]	Accepted. References have been added.
3537	41	31	41	33	You should add Zhu et al, 2019 who find -1.67 W/m2 to the list of references here (Zhu, J., Penner, J. E., Yu, F., Sillman, S., Andreae, M., and Coe, H., 2019: Organic aerosol nucleation, climate and land use change: Decrease in radiative forcing, Nature Communications, 10, Article No. 423, https://www.nature.com/articles/s41467-019-08407-7) [Joyce Penner, United States of America]	Accepted. Reference has been added.
41509	41	35	41	35	I tripped over 'adjustment contribution'. Maybe 'adjustment contributions from LWP and cloud fraction' or 'cloud and LWP adjustment contributions' [Andrew Gettelman, United States of America]	Accepted. Clarification has been made as suggested.
3539	41	39	41	39	you need some reference to "Large-eddy-simulations also tend to suggest an exaggerated aerosol effect on cloud lifetime in GCMs" I would suggest: Zhou, C. and J. E. Penner, 2017: Why do GCMs overestimate the aerosol cloud lifetime effect? A case study comparing CAM5 and a CRM, Atmos. Chem. Phys., 17, 21–29, doi:10.5194/acp-17-21-2017, who made a direct comparison of a GCM and a high resolution model. [Joyce Penner, United States of America]	Accepted. Reference has been added.
79797	41	45	41	52	While anthropogenic aerosols are generally not considered as important INPs, they can affect the ice cloud formation indirectly by reducing the lifetime of mineral dust, which is the most abundant and efficient INP in the atmosphere. Furthermore, dust-pollution interactions can cool the atmosphere hindering the vertical water vapour transport and thus the formation of ice crystals. [Alexandra Tsimpidi, Germany]	Taken into account. Dust changes are generally not considered in the ERF estimates reported here, because the forcing-related dust change is currently not quantified. A sentence to this effect has been added.
78725	41	46	41	46	Concerning "While laboratory measurements": As mentioned above, first results on this exist for the real atmosphere, too, which might add to the significance of the laboratory work -> it could be changed here to: "While measurements in a polluted region (Chen et al., 2018) and laboratory measurements ..." (BTW: This is the same citation suggested on page 37.) ----- citation: Chen, J., Wu, Z., Augustin-Bauditz, S., Grawe, S., Hartmann, M., Pei, X., Liu, Z., Ji, D. and Wex, H. (2018). Ice nucleating particle concentrations unaffected by urban air pollution in Beijing, China, Atmos. Chem. Phys., 18, 3523–3539, doi:10.5194/acp-18-3523-2018. [Heike Wex, Germany]	Accepted. Reference has been added.
3541	41	48	41	50	Zhu and Penner (ACP, submitted) now find an estimate of -0.2 W/m2 when including changes in sulfate. [Joyce Penner, United States of America]	Accepted. The paper is now cited in the relevant discussion.
39007	41	48	41	52	A small negative net ERFaci due to black carbon could come from canceling out of large positive LW ERFaci and large negative SW ERFaci. It is important to state that the large positive LW ERFaci can cause heating of the atmosphere, leading to modifications in the large-scale atmospheric circulation and the hydrological cycle. For example, Oshima et al. (submitted) used the MRI-ESM2.0 model, one of the very few CMIP6 models including aerosol interactions with ice clouds, and found the potential importance of anthropogenic INP-induced high-level ice cloud modifications on longwave radiative heating of the atmosphere, despite of small negative net ERFaci of black carbon due to canceling each other out of both substantial positive LW ERFaci and negative SW ERFaci. [Seiji Yukimoto, Japan]	Taken into account. The suggested paper is now assessed.
3543	41	50	41	51	Storelvmo (2017) missed the publication by Zhou and Penner, JGR, 2017, which demonstrates that much of this sign of the cirrus effect is the result of how different models treat aerosols. The more complete/advanced aerosol models get negative forcing, which, I think, is the better result. [Joyce Penner, United States of America]	Accepted. The slight negative forcing has since the SOD been supported by additional studies consistent with Zhou and Penner (2017). The revised text reflects this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3545	41	55	42	1	Your estimat of forcing seems too small; i.e. it is in keeping with the AR5 and AR6 models, that do not have all the right physics/chemistry. You need to caveat this with at least low confidence as a result. Note that changes in mixed phase clouds can cause a positive forcing (see the satellite study of Christensen et al., JGR 2016 who deduce small but positive forcing in mixed phase and convective clouds, and the Yun and Penner 2012 model study for mixed phase clouds which has positive forcing (Christensen, M. W., Y.-C. Chen, and G. L. Stephens (2016), Aerosol indirect effect dictated by liquid clouds, J. Geophys. Res. Atmos., 121, 14,636–14,650, doi:10.1002/2016JD025245; Yun, Y. and J. E. Penner, 2012: Global model comparison of heterogeneous ice nucleation parameterizations in mixed-phase clouds, J. Geophys. Res., 117, D07203, DOI: 10.1029/2011JD016506.) [Joyce Penner, United States of America]	Taken into account. Some of the cited studies relied on outdated laboratory results, and are therefore less relevant. However, the confidence statement has generally been revised in line with what is suggested here.
37593	42	4			This section asserts that there is "increased confidence" in the characterization of ERFaci. This is hard to reconcile with the preceeding material - although the magnitude of observationally-based and modeling estimates of ERFaci are consistent, both are heavily influenced by cloud adjustments, with models adjusting cloud water content and observations suggesting a sensitivity to cloud cover (if I have read the chapter correctly). Are the authors arguing that the confidence arises from the magnitudes of the adjustments being similar, despite being arrived at by different paths? More evidence would be welcome. [Robert Pincus, United States of America]	Taken into account. Yes, the assessment is that agreement in the overall magnitude (and sign) of the adjustment increases confidence, even though the models produce more extensive clouds mainly in the vertical and observations suggest more extensive clouds mainly in the horizontal.
17339	42	5	42	6	The magnitude of the forcing is consistent but not necessarily the mechanisms. Cloud fraction adjustments for example could occur at spatial scales that are not resolved in global models (open-closed cell stratocumulus transition). [David Neubauer, Switzerland]	Taken into account. Yes, this is correct. Additional text has been added to clarify this.
3547	42	7	42	9	Just because you now have satellite and model estimates in agreement, I would very much hesitate to assume this is "high confidence" because you only evaluate studies that do not have all the correct physics/chemistry. If you added in and gave more weighting to more complete studies, they would not agree and so could not be "high confidence" [Joyce Penner, United States of America]	Taken into account. The confidence has been revised to "medium" to reflect this.
46221	42	9	42	9	"likely range" should be "very likely range". [Twan van Noije, Netherlands]	Accepted.
22155	42	9	42	11	This sentence is a hostage to fortune, at least as written. If you are going to retain it you should be explicit as to why your estimate is so much narrower than this recent assessment and probably this recent assessment should be better discussed in the preceding text. [Peter Thorne, Ireland]	Taken into account. The relationship between this assessment and Bellouin et al is now more explicit, and the differences between the uncertainty ranges have been justified.
46223	42	9	42	11	I don't think it has been convincingly argued why the assessed range is so much narrower than the range estimated by Bellouin et al. Please also note that their range was for fixed-SST ERFaci. [Twan van Noije, Netherlands]	Taken into account. The relationship between this assessment and Bellouin et al is now more explicit, and the differences between the uncertainty ranges have been justified.
31755	42	10	42	10	"consistent" Is it? The more negative limit is almost a factor of 2 different [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The uncertainty range has now been revised and the discussion of how it relates to that of Bellouin et al. has been expanded.
35959	42	10	42	11	The Bellouin et al. (2020) 10.1029/2019RG000660 lower estimate for ERFaci is -2.7 W m-2, not -3.1 W m-2 as indicated here. Probably a confusion with our lower estimate for total aerosol ERF. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thanks, the number has now been corrected.
114805	42	14	44	33	Some studies have found that the historical aerosol forcing is likely to be too strong is some CMIP6 models, and that lowering the aerosol forcing improves agreement with the historical record (eg. https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/2018MS001603, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019GL085806, https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/2019MS001995). How does this affect the assessment of likely aerosol ERF? [Andrea Dittus, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. These papers largely fall into the category of inverse estimates, so we have not explicitly be used as lines of evidence.
46225	42	14			Section 7.3.3.3. I assume this whole section is using the AR5 definition of ERF. Please clarify. [Twan van Noije, Netherlands]	Taken into account. This is now clarified at the beginning of the section.
46211	42	16	42	16	Please reformulate "models that simulate ERFaci". Models do not simulate ERFaci. They simulate the processes relevant to aerosol-cloud interactions which enable us to estimate ERFaci. Moreover, the model estimates derived from atmosphere-only simulations are fixed-SST values, so an additional correction would be needed to obtain the ERFaci consistent with the new definition. Please clarify this. [Twan van Noije, Netherlands]	Taken into account. Sentence has been reformulated.
46227	42	18	42	19	In order to claim consistency with the temperature record, one needs to make assumptions about forcing efficacies, and it is unclear how efficacies of aerosols are treated in these approaches. Please clarify. [Twan van Noije, Netherlands]	Taken into account. The ERF concept has largely made the efficacy concept superfluous.
83127	42	19	42	22	As written now it sounds like any tp-down estimates based on historical observations to give ECS and ERF for aerosols are circular. This is not correct as these can be estimated jointly in a Bayesian approach with quite wide priors. See e.g. Skeie et al., 2018. [Terje Berntsen, Norway]	Taken into account. While this is true, these studies nevertheless use a single observable to constrain two quantities. For transparency, these studies have therefore primarily been used to constrain TCR/ECS.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98877	42	22	42	24	A relevant publication can be found at https://www.gfdl.noaa.gov/wp-content/uploads/2020/06/SMH_rev.pdf. It provides an estimate of the total aerosol effective radiative forcing. The methodology is very different from the traditional AOGCM-based detection and attribution. An ealier version has been communicated to a number of lead authors. The paper is going through the last round of minor revisions, and will be accepted by Science Advances very soon. [Yi Ming, United States of America]	Noted. Thank you. The paper is now cited.
46213	42	22	42	36	Clarify if these estimates are fixed-SST values. [Twan van Noije, Netherlands]	Taken into account. Clarification has been added.
31757	42	25	42	25	"the first". Odd. Many earlier studies recognised that you can't constrain this forcing and ECS independently. The issue is even mentioned in FAR. See eg Wigley 1989 https://doi.org/10.1038/339365a0 and, I think, many Schlesinger papers from this era. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The Knutti study was in fact the first to infer an aerosol forcing estimate in this way.
46215	42	28	42	30	Clarify if the RFMIP estimate of ERFaci includes semi-direct effects or not. [Twan van Noije, Netherlands]	Taken into account. Clarification has been added.
46217	42	41	42	43	As this statement refers to both modelling and observational studies, I wouldn't put it in this section. [Twan van Noije, Netherlands]	Not applicable. The line numbering does not seem to correspond to the text the reviewer intended to refer to.
46219	42	55	43	1	Based on the numbers presented in this section, I don't see how this range comes about. Please clarify. [Twan van Noije, Netherlands]	Taken into account. The reviewer refers to page and line numbers that don't include any numbers, so it is not entirely clear what is meant here. However, the overall ERF range presented at the end of Section 7.3.3.3 directly reflects the estimates that were referenced above.
46229	42	55	43	1	Change "aersol emissions" to "global aerosol emissions". [Twan van Noije, Netherlands]	Accepted.
16177	42		44		The assessment of ERFaci seems too optimistic. As briefly pointed out in 7.3.3.4, GCMs mostly do not include any impacts of CCN on strongly convective clouds. I don't think most of the cited observational studies effectively do either. Yet this limitation seems to be ignored and no additional uncertainty accounted for, as far as I can tell. Perhaps this is why the assessment finds a narrower range than Bellouin et al. Agreement between model and obs-based estimates does not miitigate against this uncertainty if both exclude the same effect. There are quite a few studies claiming aerosol impacts on convective cloud and outflow properties (e.g. Sarangi et al. 10.1038/s41467-018-06015, Chakraborty et al. 10.1073/pnas.1601935113). While most of these do not give a forcing estimate, the possibility needs to be accounted for. One recent study giving a (very rough) forcing estimate is Nishant et al. (2019, https://doi.org/10.1038/s41612-019-0089-1). [Steven Sherwood, Australia]	Accepted. Potential contributions to aerosol ERF from aerosol interaction with deep convection are now more explicitly discussed, and the uncertainty range has been expanded to reflect that such effects have not been accounted for in most cited studies.
83129	43	12	43	12	total aerosol ERF? [Terje Berntsen, Norway]	Accepted. Yes!
31759	43	13	43	13	Doesn't this need a major caveat? BC and sulphate both have the same sign surface forcing but opposite sign ERFs, and so it is a rather weak constraint on the ERF [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Caveat has been added.
46231	43	15	43	15	Remove "relatively strong". [Twan van Noije, Netherlands]	Accepted.
35961	43	20	43	20	Note that this statement implies that anthropogenic aerosols are only moderately absorbing, since it would be possible to have a strong dimming and a positive TOA forcing if aerosols were strongly absorbing. But as noted in Bellouin et al. (2020) 10.1029/2019RG000660, where we develop a similar argument, there is no evidence for the strong absorption that would be required. [Nicolas Bellouin, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Caveat has been added.
10783	43	22	43	24	The authors emphasise at the start of section 7.3.3.3 that using an energy balance constraints for aerosol ERF would be circular. But it is not made clear how the authors would NOT use this information in the subsequent assessment? Were different authors involved in either assessment? Given the subjective component of an expert assessment it is hard to see how to avoid some influence either way. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Different authors drafted the two sections, and while the intention was for the two assessments to not influence each other, it is of course possible or even likely that authors were influenced by the other assessment.
31761	43	32	43	32	smaller magnitude [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
9869	43	38	43	41	I made some changes to the wording and also added a sentence about our current understanding, i.e., "(iii) Based on higher resolution models, doubt was raised regarding the ability of GCMs to represent the cloud adjustment component of ERFaci with fidelity, and particularly the way in which aerosol effects on warm-cloud processes (e.g., condensation and evaporation) were parameterized. In addition, aerosol effects on ERFaci through changing anvil cloud fraction and depth are difficult to represent with one-moment or two-moment cloud microphysics parameterizations (Fan et al. 2013)." The reference is "Fan, J., L. R. Leung, D. Rosendeld, Q. Chen, Z. Li, H. Yu, and J. Zhang (2013), Microphysical Effects Determine Macrophysical Response for Aerosol Impacts on Deep Convective Clouds, Proc. Natl. Acad. Sci. USA, 110, 48, doi:10.1073/pnas.1316830110". [Jiwen Fan, United States of America]	Taken into account. We have not adopted the exact wording, but have retained the essence of the suggestions.
46233	43	43	43	44	Here and elsewhere in the chapter, use "very likely range" instead of "5% to 95% (90%) confidence range", and check for correct use of "confidence" throughout the chaper. [Twan van Noije, Netherlands]	Accepted.
66589	43	47	43	50	Suggetion for using the most updated reference for black carbon IN negligibility - Black Carbon Particles Do Not Matter for Immersion Mode Ice Nucleation, Zamin A. Kanji et al. https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019GL086764 [Naruki Hiranuma, United States of America]	Accepted. Reference has been added.
46235	43	48	43	48	Remove "(argument (i) above)". [Twan van Noije, Netherlands]	Accepted.
95881	43	52	43	54	Some GCMs do include effects on convection (see above). This sentence mixes the invigoration discussion in Tao et al. with potential ice Twomey effects that are currently not represented nor currently assessed in satellte data focusing on warm clouds only. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The discussion of aerosol effects has been expanded, and the Twomey effect in ice clouds is discussed separately.
9871	43	52	43	54	The description should be updated with the most recent studies. Therefore, I would suggest to revise this sentence as "Likewise, very few GCMs incorporate aerosol effects on deep convective clouds and associated anvil clouds, and cloud-resolving modelling studies report different impacts on cloud radiative properties depending on cloud environmental conditions (Tao et al., 2012; Fan et al., 2016). Recent cloud-resolving modelling studies with spectral-bin cloud microphysics showed a remarkable aerosol invigoration of tropical convective clouds under very low aerosol background conditions, through both increased condensation by ultrafine aerosol particles (Fan et al., 2018) and expanded anvil clouds due to reduced droplet and ice particle sizes (Fan et al., 2013), suggesting a strong local effect on ERFaci. However, it is clear what the effect would be under the global background with large-scale adjustment." The references are: Fan et al. 2013 is the same as the reference provided in the comment above. The two other references are: Fan, J., Y. Wang, D. Rosenfeld, X. Liu (2016), Review of Aerosol-Cloud Interactions: Mechanisms, Significance and Challenges, J. Atmos. Sci., 73, 4221-4252. http://journals.ametsoc.org/doi/abs/10.1175/JAS-D-16-0037.1 Fan., J. , D. Rosenfeld, Y. Zhang, S.E. Giangrande, Z. Li, L.A.T. Machado, S.T. Martin, Y. Yang, J. Wang, P. Artaxo, H.M.J. Barbosa, R.C. Braga, J.M. Comstock, Z. Feng, W. Gao, H.B. Gomes, F. Mei, C. Pöhlker, M.L. Pöhlker, U. Pöschl, R.A.F. de Souza (2018). "Substantial Convection and Precipitation Enhancements by Ultrafine Aerosol Particles." Science, 359, pp. 411-418, DOI: 10.1126/science.aan8461. [Jiwen Fan, United States of America]	Taken into account. The discussion about aerosol effects on deep convection has been expanded along the lines suggested here, but the exact wording has not been adopted.
2693	44	4			what is argument (ii)? [Bryan Weare, United States of America]	Rejected. Argument (ii) is in the paragraph immediately preceding this one.
46237	44	15	44	18	Can it be explained how the various lines of evidence are weighted? [Twan van Noije, Netherlands]	Taken into account. The different lines of evidence are combined using expert judgement. This has now been clarified.
46239	44	20	44	21	The estimated range is given high confidence. I wonder if this is really justified given the evidence presented and the fact that the new range differs substantially from the AR5 range. [Twan van Noije, Netherlands]	Taken into account. The confidence has been revised to "medium" to reflect this.
3549	44	20	44	21	I think this is repetitious, but again the use of "high confidence" is not warrented [Joyce Penner, United States of America]	Taken into account. The confidence has been revised to "medium" to reflect this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65013	44	25	44	28	It was a very weak point in AR5 that the ERFaci estimate was not traceable, but came out of the blue, from “expert judgment”. Also Chapter 7 uses “expert judgment” solely in the context of the aerosol ERF. Is there no way to avoid this, i.e. to make the final assessment traceable? Can we for example say we trust specific lines of evidence most for specific values, or give some weight to some estimates and combine them? I accept this is in result not too different from expert judgment (and after all, the entire report is an assessment), but it is much better science, since it can be criticized, attacked, falsified. All this is not possible with expert judgment. [Johannes Quaas, Germany]	Taken into account. It is difficult to combine lines of evidence without using expert judgement. This is the same approach taken in Ch. 7.5 for TCR/ECS. However, the assessment should now be easier to trace back and better justified.
20421	44	25	44	33	Sincere congratulations for the way WG1 authors deal with this difficult aerosol ERF issue [philippe waldteufel, France]	Noted. Thank you!
84843	44	26	44	26	The value -1.1 w/m2 includes cloud but Table 7.8 shows aerosol-cloud interaction as -0.45 w/m2 ; Add a comment indicating that aerosol-cloud interaction is merged with aerosl-radiation in AR6 [Jayaraman Srinivasan, India]	Taken into account. This is an error in Table 7.8.
46241	44	27	44	27	How has the translation from 1850 to 1750 been made? The difference may be quite substantial because of changes in natural fires. Where possible, please indicate the period (i.e. both start and end year) for which the various ERF estimates presented in the section were obtained. [Twan van Noije, Netherlands]	Taken into account. The 1750 to 1850 translation has now been better explained.
84845	44	30	44	30	how did you get 3/4 ? [Jayaraman Srinivasan, India]	Taken into account. 3/4 comes from 0.9Wm-2/1.2Wm-2
96711	44	36	44	45	Please note in the caption to Figure 7.8 what is only very briefly explained in the previous paragraph on page 7-44, i.e. that the AR6 best estimate of Aerosol ERF is -1.1 W/m² because a correction of 0.1 W/m² is added to the CMIP6 best estimate for the years between 2014 and 2018. Otherwise it looks very strange that the satellite best estimate is -1.3, the model-based best estimate is -1.2, but the resulting AR6 best estimate of -1.1 is then even smaller than the average of the two lines of evidence. [Nicole Wilke, Germany]	Taken into account. Figure has change due to updated assessments. The years represented have been added.
31763	44	38	44	38	Include time period in caption? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
17417	45	3	45	51	Land use and land use change and its effects should be more highlighted. Also, land use and land use change effects should be more highlighted in "Executive Summary". [Mostafa Jafari, Iran]	Rejected: No justification is provided for highlighting this further than is already done.
33043	45	3	45	51	land use and land use change and its effects should be more highlighted .also land use change effects should be more highlighted in Executive summery [Sahar Tajbakhsh Mosalman, Iran]	Rejected: No justification is provided for highlighting this further than is already done.
132403	45	3	45	51	This section does not put the biophysical effects of land use in perspective with its CO2 effects. However, this is highly relevant when discussing the potential of afforestation in limiting global warming, in particular in low-emissions scenarios (BECCS). I strongly suggest that the authors make an effort to provide a more in-depth assessment on this topic, maybe on ca. 1 page. Some relevant publications on this topic include e.g.: Betts, R.A., et al. 2007: "Biogeophysical effects of land use on climate: Model simulations of radiative forcing and large-scale temperature change", Agr. For. Met., doi:10.1016/j.agrformet.2006.08.021; Lejeune et al. 2018, Nature Climate Change: "Historical deforestation locally increased the intensity of hot days in northern mid-latitudes". https://doi.org/10.1038/s41558-018-0131-z"; Windisch et al., in review (I can provide a copy of this article to the chapter 7 authors). [Sonia Seneviratne, Switzerland]	Rejected: These papers do not relate to the TOA radiative forcing.
32713	45	3	45	51	land use and land use change and its effects should be more highlighted .also land use change effects should be more highlighted in Executive summery [sadegh zeyaeyan, Iran]	Rejected: No justification is provided for highlighting this further than is already done.
14861	45	5	45	5	Which part of the land cover change is due to human activities and which part is due to climate? How did this values evolve with time? [Marie-France Loutre, Switzerland]	Taken into account: This has been rephrased. We now refer to chapter 2
33187	45	5	45	5	This is an overly broad opening sentence; it implies land use forcing is defined by every human activity! It needs to be more specific what "changes", i.e. changes in what? I suggest something like "Land use forcing is defined as those changes *in land surface properties* directly caused…." or similar. [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been rephrased. We now refer to chapter 2
132387	45	5	45	6	Start by mentioning the different effects of land use and land cover changes on effective radiative forcing, namely, through: 1) albedo, 2) evapotranspiration, 3) roughness length, and 4) CO2 effects. These effects need to be considered together. [Sonia Seneviratne, Switzerland]	Taken into account: This has been rephrased. We now refer to chapter 2
96713	45	5	45	6	"Land use forcing is defined as those changes directly caused by human activity rather than by climate response" is unclear: changes refer to TOA energy fluxes? Directly caused by human activity should please specify "on the continents' vegetation cover". [Nicole Wilke, Germany]	Taken into account: This has been rephrased. We now refer to chapter 2

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
132389	45	6	45	8	This is somewhat too simple. It should also be discussed that this effect is particularly high in regions with snow cover (e.g. Boisier et al. 2013: Biogeosciences, 10, 1501–1516, 2013. www.biogeosciences.net/10/1501/2013/doi:10.5194/bg-10-1501-2013.) and can compensate the cooling resulting from afforestation in such regions (Windisch et al., in review: I can provide the chapter 7 authors a copy of this article). [Sonia Seneviratne, Switzerland]	Rejected: This section is focussed on TOA fluxes, not warming more generally
96715	45	6	45	8	This statement is too general, there are forest-crop-species combinations for which this is not true, also it is unclear if canopy or canopy+background albedo is referred to -- in the latter case forests may mask dark soil (e.g. chernozem) more extensively and longer during the year than cropland, so deforestation leads to a decrease in albedo in such cases. Please qualify the statement accordingly. [Nicole Wilke, Germany]	Taken into account: This has been reworded
132391	45	9	45	9	See also Thiery et al. 2017, JGR and Thiery et al. 2020, Nature Communications. References: 1) Thiery, W., E. L. Davin, D. M. Lawrence, A. L. Hirsch, M. Hauser, and S. I. Seneviratne (2017), Present-day irrigation mitigates heat extremes, J. Geophys. Res. Atmos., 122, doi:10.1002/2016JD025740; 2) Thiery, W., et al. 2020: Warming of hot extremes alleviated by expanding irrigation. NATURE COMMUNICATIONS | (2020) 11:290 | https://doi.org/10.1038/s41467-019-14075-4 | www.nature.com/naturecommunications [Sonia Seneviratne, Switzerland]	Rejected: This section is focussed on TOA fluxes, not warming more generally
132393	45	10	45	10	Boucher et al., 2004: This seems a rather old publication, is it still up to date? [Sonia Seneviratne, Switzerland]	Accepted: Boucher et al. removed
96717	45	10	45	12	That changes in latent heat flux do not impact TOA fluxes is incorrect -- indeed, cooling from evaporation and warming from condensation compensate, but cloud changes due to the altered atmospheric water content and turbulence may lead to low cloud formation, which influences TOA radiation (Ban-Weiss et al, doi:10.1088/1748-9326/6/3/034032). Maybe this is the reference that is missing behind "... low cloud amounts" in l. 14? Please revise. [Nicole Wilke, Germany]	Rejected: The cloud formation is already discussed three lines later
132397	45	13	45	14	How about soil moisture effects which lead to a different partitioning between latent and sensible heat fluxes and thus to a different atmospheric warming? REFS: Seneviratne et al. 2013, GRL doi:10.1002/grl.50956; Wilhelm, M., E. L. Davin, and S. I. Seneviratne (2015), Climate engineering of vegetated land for hot extremes mitigation: An Earth system model sensitivity study, J. Geophys. Res. Atmos., 120, 2612–2623, doi:10.1002/2014JD022293. [Sonia Seneviratne, Switzerland]	Rejected: This section is focussed on TOA fluxes, not atmospheric warming
96719	45	14	45	14	The term "land use change" should please be clarified. CH4 and N2O emissions occur on managed areas, without the need for *change* in land use. Throughout AR6, a better terminology would be "land-use changes and land management" (see DOI: 10.1111/gcb.13988 Fig. 1 for extensive definition). [Nicole Wilke, Germany]	Accepted: This has been rephrased
9873	45	18	45	19	I suggest change to "which form aerosols that change cloud properties and affect the atmospheric concentrations of ...". [Jiwen Fan, United States of America]	Rejected: The reference to aerosols is sufficient here.
132401	45	22	45	23	Why is the adjustment of land surface temperature ignored? [Sonia Seneviratne, Switzerland]	Rejected: changes in land surface temperature are a feedback and are excluded from the forcing definition.
31765	45	24	45	24	To be clear, does the vegetation change affect the snow cover, or is it that the effect of snow cover on surface albedo depends on vegetation type, or both? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been reworded
2695	45	32	45	35	spell out IRF, SARF, BVOC [Bryan Weare, United States of America]	Rejected: These are spelled out already
3551	45	35	45	38	Note that Zhu and Penner (in press, JGR, http://dx.doi.org/10.1029/2019JD032233) find that including land use change and temperature effects on BVOC emissions leads to 0.02 W/m2 in cirrus clouds and Zhu et al. Nat Comm., 2019 find including changes in BVOC increases the total aerosol forcing by 0.026 W/m2 (direct) and 0.06 W/m2 (indirect, in warm clouds). Zhu, J., Penner, J. E., Yu, F., Sillman, S., Andreae, M., and Coe, H., 2019: Organic aerosol nucleation, climate and land use change: Decrease in radiative forcing, Nature Communications, 10, Article No. 423, https://www.nature.com/articles/s41467-019-08407-7 [Joyce Penner, United States of America]	Taken into account: Papers cited
79153	45	45	45	45	What is 24,000 year solar cycle? Never heard of that [Natalie Krivova, Germany]	Accepted. Deleted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96721	45	45	45	51	The assessment of -0.12W/m2 for ERF from land-use change since 1850 seems flawed or is not clearly described (and it is not consistent with Ch. 2.2.8): The value of 0.12W/m2 seems to stem from l. 45 (Ghimire et al), where it refers to SARF from surface albedo changes. Andrews et al (referenced two paragraphs up) argue that including all adjustments and surface property changes beyond albedo may be one of the key reasons why their ERF estimate is so much larger than previous IRF or SARF estimates. But this would mean that the Ghimire SARF value cannot at all be used without adjustments as an ERF value, so it also should not be shown as such in Fig. 7.9 (the same goes for Tab. 7.8 and Fig. 7.11). It is right that the Andrews estimate shows importance of effects that are or may be rather peculiar in their model, but this should not exclude the study from being used in Fig. 7.9, since its advantage of calculating directly ERF is a strong point. The most objective way may be to use Andrews in Fig. 7.9, since it is the only ERF estimate, and additionally show the albedo-induced SARF across studies, also drawing from the AR5 estimates to include more than the single Ghimire study. [Nicole Wilke, Germany]	Taken into account: Section revised and made consistent with chapter 2.
79155	45	50	45	50	Reference to Lean is wrong. Those papers claimed a significantly lower UV contribution instead. The first paper to show the higher variability at 200-400 nm is https://doi.org/10.1051/0004-6361:20064809 This range contributed over 50% to the TSI variability [Natalie Krivova, Germany]	Taken into account - updated.
132399	45	50	45	51	I don't understand this sentence. It does not mention one of the most relevant publications on this topic: Boisier et al. 2013: Biogeosciences, 10, 1501–1516, 2013. www.biogeosciences.net/10/1501/2013/doi:10.5194/bg-10-1501-2013. I think the AR6 should be able to provide such an estimate given the newly available literature and datasets. [Sonia Seneviratne, Switzerland]	Taken into account: Section is revised
87105	46	1	46	24	A background study of water physics and water released in atmosphere is given below which therefore increases the confidence level in the environmental impact of contrails and the characterstic of water vapor as green house gas is still applicable even when water vapor does not manifaest itself as contrail. Details with references below Qureshi.S. (2016). [Sarah Qureshi, Pakistan]	Noted. Water vapour forcing from contrails is included in the Lee et al assessment which provides our assessment, although it is small.
110959	46	1	46	24	In this section, a table giving an overview of all the components of aviation climate impact should be included (with values and uncertainties). This could also be done in relation with section 6.5.2.1. There really is a need for this IPCC report to give as clear as possible an overview of the full climate impact of aviation (even if complex and with some uncertainties), because that is the information relevant for policy makers. Partial information (some climate impacts only, like CO2) is commonly taken as if it was full information (complete climate impact), which is misleading decision-makers, so an effort of clarity and pedagogy is really needed here. Most decision-makers don't even understand there are non-CO2 impacts for aviation and that these are as important as CO2 (or even more impacting). [Noé Lecocq, Belgium]	Not Applicable. All ERFs related to aviation are now assessed in Ch. 6.
51379	46	1	46	24	Contrails are also covered in Chapter 6, including their forcing. Suggest a reference to that chapter is added. Also, please ensure that they are consistent and not duplicative. Which chapter should be the go-to chapter on this information? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not Applicable. All ERFs related to aviation are now assessed in Ch. 6.
19403	46	1	46	24	I have never understood why forcing by contrails is given as much space as it is in these assessments. Now that the 95% range is less that 0.1 W/m2, maybe we could agree to focus our attentions elsewhere. [Isaac Held, United States of America]	Taken into account. Discussion moved to chapter 6 so no longer taking up space in chapter 7.
51381	46	3	46	3	Define "short-lived" here as other parts of the chapter talk about short-lived for other substances that have a lifetime much longer than a few hours. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Discussion now in chapter 6.
128955	46	3	46	3	Suggest "in the exhaust plumes of aircraft engines". [Trigg Talley, United States of America]	Not applicable. Discussion now in chapter 6.
3553	46	6	46	6	Aerosol emissions may also affect large-scale cirrus clouds (Penner, J. E, C. Zhou, A. Garnier, and D. L. Mitchell, Anthropogenic aerosol indirect effects in cirrus clouds, 2018: Journal of Geophysical Research: Atmospheres, 123, 11,652-11,677. https://doi.org/10.1029/2018JD029204 [Joyce Penner, United States of America]	Not applicable. Discussion now in chapter 6.
89219	46	8	46	9	The AR5 estimate is ERF not RF (aviation-induced cirrus is an adjustment). A RF for contrails was given in RF with a very low forcing. Most of the assessment was done in Boucher et al. 2013 rather than Myhre et al. (2013) so either include both or only Boucher et al. 2013. On the AR5 reference the same applies on line 36. [Gunnar Myhre, Norway]	Not applicable. Discussion now in chapter 6.
32077	46	8			Any recent evidence on contrails from the COVID lock down? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account in the cross-chapter box on Covid-19.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83763	46	11		13	"the SARF efficacy was 60% or smaller" for TCR, inferred ECS, or equilibrium ECS? [Marvel Kate, United States of America]	Not applicable. Discussion now in chapter 6.
79217	46	14	46	14	Better "… new studies that all used a 2006 aircraft inventory as their reference …" [as otherwise these studies have no particular referring to 2006 (atmospheric) conditions] [Michael Ponater, Germany]	Not applicable. Discussion now in chapter 6.
31767	46	15	46	16	Text confusing, by mentioning smaller and best estimate. Best estimate is 65% smaller, rather than 65%, right? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Discussion now in chapter 6.
41511	46	16	46	16	'The first published estimate' is ambiguous. State the Author, year. I don't think you mean Chen and Gettelman 2013, I think you mean Bickel et al 2019, so lead with that. [Andrew Gettelman, United States of America]	Not applicable. Discussion now in chapter 6.
79219	46	16	46	16	" … accounts for the efficacy of the contrail forcing …", better " … accounts for the rapid adjustments to the contrail forcing …" [Michael Ponater, Germany]	Not applicable. Discussion now in chapter 6.
128957	46	18			Should be Bickel et al., 2020. [Trigg Talley, United States of America]	Not applicable. Discussion now in chapter 6.
31769	46	19	46	19	"carefully" I'm sure it was, but seems inappropriate word here, as that word is not used to describe any other papers [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Discussion now in chapter 6.
79221	46	20	46	20	"… used estimates of contrails to …" [I find this rather vague, what is meant? Specific contrail forcing per unit fuel consumption? Anyway, please do not repeat the error from the AR5 that mixed up contrail radiatiatve forcing with ist ERF, but rather separate IRF, SARF, and ERF if there is insufficient basis to convert between the various estimates] [Michael Ponater, Germany]	Not applicable. Discussion now in chapter 6.
128959	46	20	46	21	[CONFIDENCE] This cites Lee et al. as giving an estimate of aviation-induced cirrus in 2018 of 0.04 Wm2 with a 5-9% CI of 0.01-0.07 W/m2, and assigns "medium confidence" to the estimate. However, Lee et al. give 2018 an estimate of ERF by contrails and contrail-induced cirrus of 0.050 Wm/2 with a CI of 0.015-0.085W/m2, and a "low confidence" level. Why the discrepancy? [Trigg Talley, United States of America]	Taken into account. We use results from the accepted version of Lee et al (2020). Note contrail discussion moved to Chapter 6.
31771	46	21	46	21	Is it virtually certain to be positive? I am not sure of the answer, but Piers will know that many years ago we tried to understand whether this is the case by pushing various boundaries. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Noted. "virtually certain to be positive" was not included in the original text. Bickel et al. (2020) show cases in which contrail ERF can be negative. We therefore stay with our assessment of medium confidence. Note contrail discussion moved to chapter 6.
79157	46	27	46	28	This has nothing to do with the "later recovery in solar modulation potential from the Maunder Minimum". This statement is meaningless and misleading, just copied from the paper without any understanding of the context. The higher change comes from the use of poorly-constrained solat atmosphere models. When used for irradiance reconstructions, they return a high uncertainty range. [Natalie Krivova, Germany]	Accepted. Changed.
128961	46	27			For aerosol-climate models that consider this effect, the positive radiative forcing induced by the present-day change in light-absorbing particles on snow/ice should have been accounted in their aerosol ERFari estimate. If this BC/OC on snow/ice effect is quantified separately here (Figure 7.9 and Table 7.8), should it be deducted from ERFari? [Trigg Talley, United States of America]	Taken into account. The ERFari estimates in this chapter do not include change in light absorbing particles on snow and ice. The method used to derive model values in table 7.6 for ERFari and ERFaci is also able to account for the albedo change, but this is not reported as this effect is not accounted for in all models and also includes contributions from land-surface warming. Note this discussion is moved to Chapter 6.
79159	46	33	46	34	Please, remove the "slow recovery" and ref to Muscheler. This is not correct, and not the reason for the higher chage in Egorova's model. The model returns a higher TSI change compared to all other existing models independetly of the isotope record used as input. The reason is the used of the solar atmosphere models that are not constrained in the photosphere, where TSI actually originates. [Natalie Krivova, Germany]	Accepted. Changed
3555	46	38	46	39	The forcing by organic particles by Lin, G., J. E. Penner, M. G. Flanner, S. Sillman, L. Xu, and C. Zhou, 2014: Radiative forcing of organic aerosol in the atmosphere and on snow: Effects of SOA and brown carbon, J. Geophys. Res. Atmos., 119, 7453–7476, doi:10.1002/2013JD021186. found that OC forcing was 0.001 to 0.003 (W/m2) while BC contributed additional 0.013 to 0.014 W/m2 [Joyce Penner, United States of America]	Taken into account. For BC, the initial range from Bond et al is increased to take into account Lin et al, providing a RF range of 0.01 - 0.09 W/m2. For OC, Lin et al is the only global study, so this is why the estimate is based on this, but the forcing is small and with one study confidence is low, so it is not formally included in the remainder of the assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
39133	46	50	46	55	The text does not seem to consider the results from Yasunari et al. (2015), who highlight the role of dust alongside EC, who note that OC should not be completely neglected (significant for some regions), and who suggest a larger radiative effect of snow impurities (natural and anthropogenic) than the stated 0.04 W/m2. Please consider revising the text to at least note the potentially large effect of mineral dust in snow darkening. [Yasunari, T. J., Koster, R. D., Lau, W. K., & Kim, K. M. (2015). Impact of snow darkening via dust, black carbon, and organic carbon on boreal spring climate in the Earth system. Journal of Geophysical Research: Atmospheres, 120(11), 5485-5503.] [Aku Riihelä, Finland]	Accepted. No formal assessments are made available but this study is referenced to indicate snow darkening from dust is not considered but potentially large.
3557	46	52	46	52	Why hasn't this been revised downward? Wasn't it based on BC estimates by Bond et al. 2013, which has been revised down? [Joyce Penner, United States of America]	Taken into account. The BC estimate of ERF is revised down, the test has been clarified.
46243	46	52	47	1	This paragraph confuses me because it states that the efficacy of black carbon on snow is needed for estimating ERF. However, efficacy shouldn't be relevant for the forcing; it only appears when relating the forcing to the temperature response. [Twan van Noije, Netherlands]	Noted. The ERF is defined such that is has better correspondence to long-term equilibrium temperature change than RF, so by implication it is assumed to have efficacy of approximately one.
116609	46		46		There is duplication between section 7.3.4.2 and chapter 6, please check. [Valerie Masson-Delmotte, France]	Not Applicable. All ERFs related to aviation are now assessed in Ch. 6.
87107	46				BACKGROUND STUDIES WATER IN THE ATMOSPHERE Water vapour accounts for 0.25% of the mass of the atmosphere on average. It has a residence time that ranges from a few seconds to days and this makes water a highly variable constituent. Water is released from aircraft exhaust emissions into the upper troposphere. This water is released as vapour at a high temperature whereby it condenses in the cooler atmosphere to form contrails and eventually cirrus clouds. In one example reported by Knollenberg (1972), "the amount of moisture released by the burning of jet fuel from a research aircraft was 1.7 grams of water for every meter of flight path. However, the total water measured in a persistent contrail produced by the aircraft was conservatively measured to be between 20700 to 41200 grams of water for every meter of the contrail path!" Almost the entire contrail is created from the moisture in the atmosphere through the process of collision-coalescence. According to Schumann et al. (2015) contrail water maybe 103 to 106 times the amount of water emitted. The tropospheric region constitutes 80% of the mass of the atmosphere. Commercial aircraft cruise along the upper troposphere just below the tropopause. The lower troposphere hosts all kinds of weather patterns. Hence the flight altitude is an intermediary layer between the troposphere and the stratosphere and has been chosen as such as the optimum flight altitude in order to fly above the weather. This study aims to devise a method to reduce the greenhouse effect of water vapour that is released from the exhaust emissions of the aircraft into the upper troposphere. The main focus revolves around condensing the water within the engine before releasing a predetermined size of water droplets into the atmosphere so as to prevent the formation of contrails at the cruise altitude. WATER PHYSICS A basic study is undertaken unfolding the behaviour of water in the atmosphere. The parameters governing this behaviour are tabulated in Table 8 1. The most crucial parameters for this study are the saturated vapour pressure of water at a given temperature as well as the size of the condensate particle formed at any given conditions of temperature and pressure. The size of the condensate particle of water defines its state and is a function of the saturated vapour pressure, ambient temperature and ambient pressure. A particle in the order of 0.1 microns acts as a cloud condensation nucleus promoting cloud	Noted. See response to #87105.
89223	47	5	47	54	I think the section can be structured more logical. Start with change in solar irradiance change since 1750, much emphasis on the last solar cycle and the satellite measured trend, and then adjustments and indirect effects. [Gunnar Myhre, Norway]	Taken into account. Section has been reworked
34913	47	5	48	33	The SOD acknowledges the work of Svensmark on galactic cosmic rays but clearly does not understand the implications of his work and others. Please see general comment #13 above. [Jim O'Brien, Ireland]	Noted. Unfortunately, we could not find the general comment you are referring to here.
10785	47	13	47	15	A more appropriate reference would be something like Lean GRL (2001). This idea is much older than 2015! [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted but we prefer to cite the review paper
22157	47	14	47	14	For reader clarity I wonder whether noting why the /4 divisor is applied should be noted. I'm not sure that the reason can safely be assumed to be a priori knowledge. [Peter Thorne, Ireland]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
80041	47	23	47	23	the adjustment due to ozone is small in the global mean at the TOA, but actually, the surface effect is negative as shown in Chiodo and Polvani (2016), i.e. it reduces the sensitivity to solar forcing, and should therefore be noted here. Hence, the TOA or tropopause perspective for the ozone adjustment is misleading [Gabriel Chiodo, Switzerland]	Rejected. Surface effect is not relevant for TOA forcing described in this section
80039	47	47	47	49	To be fair, there is also no direct observational evidence for the absence of an (albeit small) long-term trend either. Solar observations are limited to the latest 30 years or less. The irradiance back to 1750 is reconstructed based on semi-empirical models (e.g. NRLSSI) which are also based on a set of (non-observed) assumptions. For a fair assessment, it should be clarified somewhere here that the Egorova 2018 reconstruction is excluded from the report because it's deemed as "unlikely", but not because there is no "observational" evidence for it. I think a better way to defend the "no trend" argument, and thus the exclusion of the Egorova 2018 forcing, would be that model simulations driven with large solar forcings such as Shapiro 2011 (to which the Egorova 2018 value is a small correction) are inconsistent with the proxy records. See Feuler and Rahmstorf (2010) "On the effect of a new grand minimum of solar activity on the future climate on Earth" [Gabriel Chiodo, Switzerland]	Taken into account. The argument is now supported by two additional papers, Lockwood and Ball and Yeo et al. and also additional text
116611	47		47		There is some overlap with the assessment of TSI variations during the last millennium in chapter 2, please check. [Valerie Masson-Delmotte, France]	Accepted. Now using consistent solar ERF.
10787	48	1	48	33	I am astonished that so much text is given over to this unproven hypothesis. More text than is given to the known and substantial effects of Volcanic influences or contrails. All that is needed is lines 3-7 (with just one recent reference) and lines 14-20. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text has been shortened for the final draft.
93073	48	1	48	33	Length of a section should be linked to relevance; already in the last report this chapter was over-emphasized. [Claudia Stubenrauch, France]	Taken into account. The text has been shortened for the final draft.
83605	48	1			7.3.4.5 Galactic Cosmic Rays .... 48 This Section summarizes work done seeking a causative link between cosmic ray activity and Earth climate and concludes absence of a material link. There are two shortcomings in this logic: 1) References are incomplete; the latest reference in the Svensmark et al series is missing H. Svensmark 1, M.B. Enghoff 1, N.J. Shaviv2 & J. Svensmark , 2017, Increased ionization supports growth of aerosols into cloud condensation nuclei, NATURE COMMUNICATIONS | 8: 2199 | DOI: 10.1038/s41467-017-02082-2 | 2) The Section does not discuss the body of literature finding an association between observed climate change and cosmic ray activity (especially periods ~1000 and ~200 years). The association does not necessarily imply causation (we all understand that elementary scientific logic) but existence of the association deserves more considered comment in order that future scientists may have opportunity to further consider or negate the association in the light of possible future new insights into underlying mechanisms . This Section is limited to consideration of a hypothetical mechanism and in finding a lack of support in published papers for that mechanism it is disregarding a considerable body of published observational evidence. See my comment on the Whole Report for a philosophical comparison with debate around Galileo and with Lord Kelvin. The closing conclusion of the Section says "Published literature since then [AR5] robustly support these conclusions with key laboratory, theoretical and observational evidence. An assessment can now be made with high confidence that GCRs contribute a negligible ERF." This statement ignores that body of observational evidence on cosmic ray flux density and observed global temperatures. See my comments on AR6 Section 7.5.4 for more discussion on this point. [michael asten, Australia]	Taken into account. The latest Svensmark et al. paper is now included in the text. Associations between GCR and climate will not be discussed. The mandate of this report is to assess literature on processes shown to affect climate. The literature concludes that there is only a very weak link, so no need for further discussion of speculative associations that would make it much stronger
1877	48	3	48	4	Fix the tenses to all be plural: is -> are, its-> their [Alan Robock, United States of America]	Accepted.
93075	48	6	48	7	The initial analysis of Svensmark used ISCCP data to get a positive relation, already a relation is not a proof that there is a link; in addition the data he used (IR data from ISCCP) are not feasible for reliable low cloud identification (as they can be mixed with cirrus). It is better to omitt tha last part of the sentence after the ','. [Claudia Stubenrauch, France]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4359	48	9	48	11	This list of references leaves out some work that points in the same direction, e.g. Tomicic et al (2018) which corroborates (and expands to higher ionization levels) the results of Dunne et al (2016) and Gordon et al (2016), and Enghoff et al (2017) that also shows how ions affect the nucleation process. Furthermore there have been laboratory studies showing that ions enhance the growth rate of aerosols increasing their chance of reaching CCN sizes (Svensmark et al 2013) as well as a detailed description of the physical mechanism, showing good agreement between theory and experiments (Svensmark et al 2017). This work is crucial since the growth rates of the newly nucleated aerosols is currently what causes the models to report a small effect of ions on CCN formation. The paragraph could be rewritten to something like "Since the AR5, considerable progress has been made connecting GCR to new particle formation, particularly by work performed at the CERN CLOUD chamber (Cosmics Leaving OUtdoor Droplets) (e.g. Dunne et al., 2016; Gordon et al., 2016, 2017; Kirkby et al., 2016), but also by others (e.g. Yu and Luo, 2014; Tomicic et al, 2018; Enghoff et al, 2017). Furthermore laboratory studies have shown that ions enhance the growth rate of aerosols increasing their probability of reaching CCN sizes (Svensmark et al 2013) and a physical mechanism taking into account the additional mass added to aerosols ions compared to neutrals has been described, showing good agreement between theory and experiments (Svensmark et al 2017)." Additional references. Tomicic, M., Enghoff, M. B., Svensmark, H., 2018, Experimental study of H2SO4 aerosol nucleation at high ionization levels, Atmospheric Chemistry and Physics 18, 5921-5930 M. B. Enghoff, J. Svensmark, 2017, Measurement of the charging state of 4-70 nm aerosols, Journal of Aerosol Science 114, 13-20 Svensmark, H., Enghoff, M. B., and Pedersen, J.O.P., 2013, Response of Cloud Condensation Nuclei (> 50	Noted. That cosmic rays affect CNN is evidenced. The size of the effect is the important consideration here.
93077	48	9	48	20	suggestion: Since the AR5, this link between GCR and new particle formation has been more thoroughly studied, particularly by experiments in the CERN CLOUD chamber (Cosmics Leaving OUtdoor Droplets). Kirkby et al. (2016) and Gordon et al. (2017) found indeed that a considerable fraction (up to 50 %) of atmospheric particle nucleation involves ions, yet the dependence on ion concentration is relatively weak (Dunne et al., 2016). By linking the GCR-induced new particle formation from CLOUD experiments to CCN, Gordon et al. (2017) found the CCN concentration for low clouds to differ by 0.2% to 0.3 % between solar maximum and solar minimum of the solar cycle. Combined with small variations in the atmospheric ion concentration over centennial time scales (Usoskin, 2017), it is therefore unlikely that cosmic ray intensity impact present day climate via nucleation (Yu and Luo, 2014; Dunne et al., 2016; Pierce, 2017; Lee et al., 2019). [Claudia Stubenrauch, France]	Taken into account. Thank you for proposing a more efficient way of formulating the paragraph. Parts of your suggestion are used in the final draft.
37733	48	9	48	27	Should be some acknowledgement of russian work (Stozhkov et al., 2017) [Howard Brady, Australia]	Rejected. This article lack information about which methods and time periods are used to obtain the results presented and the science can therefore not be assessed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4361	48	15	48	17	It should be noted that the models used to come to the conclusion that cosmic rays do not contribute to climate via nucleation do not include the enhanced growth rate caused by ions described by Svensmark et al (2017) - including this may change the interpretation of the results. This will possibly already be found out before AR6 is released so it could be an idea to pre-empt those results by mentioning the lack of this mechanism in the models. In any case it deserves to be mentioned as the growth from nucelation to CCN size in the models is what currently dampens the effect of GCR on cloud effects the most. After l20 it could be added that "The modelling of cosmic ray impacts on cloud formation does not yet include the mechanism from Svensmark et al (2017) where ions increase the growth rate of small aerosols, which could change the conclusions." Additional reference Svensmark, H., Enghoff, M. B., Shaviv, N. J., Svensmark, J., 2017, The role of ions in the growth of aerosols into cloud condensation nuclei, Nature Communications 8:2199, 2017 [Martin Bødker Enghoff, Denmark]	Noted. That cosmic rays affect CNN is evidenced. The size of the effect is the important consideration here.
1879	48	15			Change "impact" to "impacts" [Alan Robock, United States of America]	Accepted. Thank you. Fixed.
93079	48	22	48	27	suggestion: Nevertheless, studies continued to seek a relationship between GCR and properties of the climate system based on correlations and theory. The positive relationship between GCR and clouds, using satellite data, found by the analyses of Svensmark et al. (e.g. 1997, 2009, 2016) has not been corroborated by other studies (e. g. Kristjansson et al., 2008; Calogovic et al., 2010; Laken, 2016). [Claudia Stubenrauch, France]	Taken into account. Thank you for the suggested rewrite. Parts of it has been used in the final draft.
4365	48	27	48	27	While it is true that no study has corroborated Svensmark et al (2016) no study has contradicted the findings either, since the issues with the Svensmark et al (2009) paper pointed out in the other listed studies have been addressed in the 2016 paper. Writing only that it has not been corroborated seems like a biased statement. The line could simply be changed to "No study has corroborated nor contradicted the new findings of Svensmark et al. (2016) to date". [Martin Bødker Enghoff, Denmark]	Taken into account. We agree that this statement appears biased. Sentence was removed for the final draft.
4363	48	29	48	33	This conclusion does not take into account the experiments and theory by Svensmark et al (2013 and 2017) that show how ions enhance the growth rate from nucleation to CCN size which could easily change the picture since the conclusion is based on models which do not contain the additional growth effect. I suggest that the conclusion be changed to reflect that while the level of scientific understanding has increased it is still too early to say anything with high confidence. Additional references Svensmark, H., Enghoff, M. B., and Pedersen, J.O.P., 2013, Response of Cloud Condensation Nuclei (> 50 nm) to changes in ion-nucleation, Physics Letters A 337 (37), 2343-2347 Svensmark, H., Enghoff, M. B., Shaviv, N. J., Svensmark, J., 2017, The role of ions in the growth of aerosols into cloud condensation nuclei, Nature Communications 8:2199, 2017 [Martin Bødker Enghoff, Denmark]	Rejected. While these studies show a possible effect from GCR on the aerosol growth rate, the effect is small over the period over which ERF is assessed in this chapter (1750 to present) due to small variations in the atmospheric ion concentration over this time period (e.g. Usoskin (2017)). The papers do not study to what extent the proposed change in aerosol growth rate may induce an ERF of GCR. As such, our conclusions are not affected by the findings in these studies.
1881	48	38	48	40	Volcanoes do not inject important aerosols into the stratosphere. Rather, they inject sulfur dioxide gas, which transforms into sulfuric acid droplet aerosols. [Alan Robock, United States of America]	Taken into account - a more appropriate explanation presented.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
111981	48	38	49	8	paleoclimate aspect should be considered as well, in relation to the adequate solar activity effects (from Little Ice Age) to better quantify the notice from Chap. 1., p. 14, l. 23-24, see comment there: "These changes were primarily driven by a clustering of volcanic eruptions (PAGES 2k Consortium, 2013; Owens et al., 2017; Brönnimann et al., 2019b). Actually, I am aware of the shift in the LIA explanation, but still it is not exactly said there is no influence of solar activity I would say. At least, the recent papers referred are not so strong in the statement, Owens et al. (2017) is saying that "Overall, it is likely that the effect of volcanic eruptions was the largest influence, followed by the drop in solar activity and changes in land use." and Brönnimann et al. (2019b) titled the paper Last phase of the Little Ice Age forced by volcanic eruptions, thus, no mention of the solar effects seems to me not to be fully appropriate. Maybe should be elaborated in more details, with some calibration. One could expect it perhaps in the Section 2.2, where solar and volcanic forcing are summarized in the past, but this is not the case, there is nothing about these relations, as well as in the Ch7" [Tomas Halenka, Czech Republic]	Taken into account - not really applicable to our chapter, considered by chapter 2
54557	48	43	48	43	"Shortwave clouds" is not a typical term I believe. [Matthew Toohey, Canada]	Accepted - should be reduction in clouds (meant to imply that SW forcing increases)
1883	48	43			There is no such thing as "shortwave clouds." What are you trying to say? [Alan Robock, United States of America]	Accepted - should be reduction in clouds (meant to imply that SW forcing increases)
54559	48	44	48	44	per unit SAOD [Matthew Toohey, Canada]	Accepted
99447	48	45	48	49	No observational study mentioned here. Please also consider this observational study on volcanic sulfate's impact on cirrus clouds. (It illustrates that variations in cirrus optical properties coincided with volcanic sulfate subsiding from the stratosphere): Friberg, J. et al., (2015), Influence of volcanic eruptions on midlatitude upper tropospheric aerosol and consequences for cirrus clouds, Earth and Space Science, 2(7), doi:10.1002/2015ea000110. [Johan Friberg, Sweden]	Accepted
17343	48	46	48	49	Meyer et al. (2015) investigated the influence of the Nabro eruption in 2011 on ice clouds using CALIPSO sattelite data and found no significant impact. Meyer, A., J.-P. Vernier, B. Luo,U. Lohmann, and T. Peter (2015),Did the 2011 Nabro eruptionaffect the optical properties of iceclouds?,J. Geophys. Res. Atmos.,120,doi:10.1002/2015JD023326. [David Neubauer, Switzerland]	Accepted. This reference is now added
41513	49	1	49	2	There are regional ERF estimates from Holuhraun illustrating this in Gettelman et al 2015 if you want a reference. Gettelman, Andrew, Anja Schmidt, and Jón Egill Kristjánsson. "Icelandic Volcanic Emissions and Climate." Nature Geoscience 8, no. 4 (April 2015): 243–243. https://doi.org/10.1038/ngeo2376. [Andrew Gettelman, United States of America]	Accepted. This reference is now added
54561	49	5	49	5	this sentence refers to three climate model-based results, but only two are quoted earlier. [Matthew Toohey, Canada]	Noted. It is now made clearer that Marshall et al. is included here.
54563	49	6	49	6	per unit SAOD [Matthew Toohey, Canada]	Accepted
16179	49	11	49	33	Seems like there are other noteworthy changes since AR5? For example the new convergence of model and observational estimates of aerosol forcing, and the increase in the estimated indirect effect. [Steven Sherwood, Australia]	Taken into account: This has been reworded
69211	49	13	49	28	"Summary" sounds inappropriate for the section titles of 7.3.5.1 and 7.3.5.2. The titles make enough sense without "Summary". [Kaoru Magosaki, Japan]	Accepted: This has been reworded
27157	49	15	45	15	This formulation is confusing and should be modified. It might be wrongly understood as AR5 has introduced adjustments. [Eric Brun, France]	Taken into account. Sentence slightly reworded for clarity, but our initial point that the concept of ERF and radiative adjustments being introduced in AR5 stands.
46245	49	15	49	15	It should also be mentioned that a new definition of ERF is adopted in the AR6, which differs from the previous definition used in the AR5 in that surface air temperatures over land are not allowed to respond anymore. [Twan van Noije, Netherlands]	Accepted: sentence added at the end of this paragraph
32079	49	22	50	0	CH4 - ERF Does this update include Chapter 6 ref Thornhill et al? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Discussion of chemical adjustments has been added.
46247	49	33	49	33	I suggest to change "ERF" to "anthropogenic ERF". [Twan van Noije, Netherlands]	Rejected: This is understood from the context
37167	49	38	49	38	The caption should mention that the data is estimated for most, if not all, of the period. [John McLean, Australia]	Rejected: The methodology for the ERFs is described in the text
114591	49	43	50	2	This is a very useful overview. It contains a lot of info and I suggest you try to imporve the layout to make the AR6 values more visisble. [Jan Fuglestvedt, Norway]	Taken into account: The layout has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96723	49	43	50	2	Please enhance Table 7.8 with the information that the Aerosol ERF is -1.1 W/m² for aci+ari, with 75% and 25% shares, respectively. [Nicole Wilke, Germany]	Taken into account: Ari and Aci have been separated
89227	49	45	49	49	Include in the caption that SAR, TAR and AR4 values are RF. [Gunnar Myhre, Norway]	Taken into account: The caption has been revised
81399	49	45	49	50	This table (and also Figure 7.9 plus other parts of the chapter) lists “Halogens”, which is the elemental form of fluorine, chlorine, etc.. Presumable “halocarbons” are meant here? If yes, which ones? [Johannes Laube, Germany]	Taken into account: Halogenated Species used.
71721	49	45	50	1	(Table 7.8) As noted for Table 7.5 in section 7.3.2.7, I would recommend merging this with the earlier table and leaving it in section 7.3.2.7. A slightly different table in this summary section could show separate columns for “instantaneous/direct RF”, ERF, and “Total ERF” including the indirect effects. [Martin Manning, New Zealand]	Taken into account: The tables have been rationalised
18277	49	45	51	20	Table 7.8. Total anthropogenic would be 2.53 [1.58 to 3.34], not 2.53 [1.56 to 3.32]? Consistency is necessary with Executive summary and the table caption. [Yugo Kanaya, Japan]	Taken into account: These numbers have been revised
18279	49	45	51	20	Table 7.8. Were "halogens" defined? Were previous "halocarbons" renamed to "halogens", as they include SF6 etc? Note that Table 7.5 uses "halogens" and Figure 7.10 uses "halocarbons". [Yugo Kanaya, Japan]	Taken into account: Halogenated Species used.
46249	49	45			Table 7.8: Please mention the different definition of adjustments included in the AR5 and AR6 estimates. [Twan van Noije, Netherlands]	Taken into account: The caption has been revised
37173	49	50	49	50	The ERF in 1750 is unknown and unknowable because measurements were not made back then. All that you have are estimates, not only for 1750 but for almost every year since then. [John McLean, Australia]	Rejected: The methodology for the ERFs is described in the text
37175	49	50	49	50	Please explain how the confidence limits for the "total anthropogenic" was derived. Was it by simple addition or quadrature? [John McLean, Australia]	Taken into account: The uncertainties have been discussed.
84847	49	50	49	50	There is a need for footnote to clarify why radiative forcing decreased from SAR to TAR [Jayaraman Srinivasan, India]	Rejected: This report addresses the most recent evaluations, and is not a historical review of past studies.
89225	49	50	50	1	Correct the AR6 ari & aci values, only given for total. Actually, it is useful to include a row for total aerosol effect (at least availble for AR5) [Gunnar Myhre, Norway]	Taken into account: Ari and Aci have been separated
22159	49	50	50	1	Quasi-random use / non-use of + before the positive ERF values is offputting. Why does AR6 have just one value for aerosol effects when the prior text supports two values for comparability and traceability? [Peter Thorne, Ireland]	Taken into account: Ari and Aci have been separated
130525	49	50	50	1	In table 7.8 Aerosol-radiation interactions AR6 -is 1.1? [Panmao Zhai, China]	Taken into account: Ari and Aci have been separated
16181	49	50			Table 7.8 gives no value for ERFaci. [Steven Sherwood, Australia]	Taken into account: Ari and Aci have been separated
1885	49	55			Delete "also" [Alan Robock, United States of America]	Accepted: This change has been made.
128963	50	1	50	1	For Table 7-8, it's very confusing to only report the sum of ARI and ACI in the AR6 column but to split them out in the columns for the previous assessments. The text reports ACI and ARI separately, with confidence intervals, so not sure why that information can't be provided in the summary table. [Trigg Talley, United States of America]	Taken into account: Ari and Aci have been separated
103613	50	5	50	5	ERF units missing [Philippe Tulkens, Belgium]	accepted: Units have been added.
2697	50	5			the uncertainty range differs from the table above [Bryan Weare, United States of America]	Taken into account: These values have been revised
37169	50	6	50	6	The AR5 value was an estimate too, not an assessment. [John McLean, Australia]	Rejected: AR5 assessed these values
46251	50	8	50	8	Change to "concentration increases" or "concentration changes". [Twan van Noije, Netherlands]	Accepted: This change has been made
37171	50	10	50	10	The expression "22% more negative" is poor English. I don't even know what it means. Is it a negative number that has become even more negative? [John McLean, Australia]	Rejected: The meaning is clear.
46253	50	10	50	13	Please mention that inverse estimates require information on efficacies, and that this introduced additional uncertainties. Maybe briefly explain what assumptions are made in the mentioned study. [Twan van Noije, Netherlands]	Rejected: This study is mentioned as additional supporting evidence and is not used as the main assessment.
114593	50	14	51	2	I find this uncelar. It is used as an indicator of human-induced climet change. [Jan Fuglestvedt, Norway]	Taken into account: This has been rephrased
46255	50	14	51	2	Was this offset by aerosols explained already? [Twan van Noije, Netherlands]	Taken into account: This has been rephrased
51383	50	30	50	30	Explain "semi-direct effect" here. The same term is used elsewhere in this chapter but refers to something different. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been rephrased
26163	50		50		Table 7.8: The value -1.1 [-2.0, -0.4] in AR6 is a total aerosol forcing. This may be misunderstood as ERFari. [Toshihiko Takemura, Japan]	Taken into account: Ari and Aci have been separated

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68369	51	4	51	11	While well-mixed GHGs may produced the largest contribution to ERF, the more useful insight is that the non-CO2 SLCPs can avoid more warming going forward well past 2050. Include further breakdown of what contributes the most; WMGHGs contribute the most and CO2 the largest impact, explaining breakdown in percentages of CO2 and CH4 (and the others) to the total. [Durwood Zaelke, United States of America]	Taken into account: The percentages has been added.
66807	51	4	51	11	Percentage breakdown of the GHGs mentioned here would be helpful for seeing how much CO2 contributes compared to the others. [Kristin Campbell, United States of America]	Taken into account: The percentages has been added.
46257	51	6	51	6	I would suggest to change "well-mixed greenhouse gases" to "well-mixed greenhouse gases, methane, and halogenated gases". [Twan van Noije, Netherlands]	Not applicable: This section has been removed.
32081	51	7	51	8	Should give numbers and should also insert a brief adding up of methane and knock-on impacts: text could read: "Carbon dioxide (X ±x Wm-2) continues to contribute the largest part of this ERF (High confidence), followed by methane and methane-related species (Y±y Wm-2) (High confidence). [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This section has been removed.
31773	51	8	51	8	"significant increase" - it was previously ignored, rather than assessed to be zero. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This section has been removed.
68371	51	13	51	15	Possible to include a breakdown of aerosols that exert positive forcing (BC)? [Durwood Zaelke, United States of America]	Rejected: This is covered in chapter 6
99445	51	13	51	15	What does 'high confidence' refer to here? Is it 'high confidence' that only 25% of the ERF comes from aerosol's direct influence on the radiative balance? The estimated uncertainties are high for both the aerosol's direct effect (and especially) the aerosol-cloud interactions' impact on the radiative balance. [Johan Friberg, Sweden]	Taken into account: This has been rephrased
66809	51	13	51	15	Possible to include a breakdown of aerosols that exert positive forcing (BC)? Black carbon directly warms the atmosphere by absorbing solar radiation and indirectly by darkening snow and ice surfaces. The goal should be to ensure that reductions of black and brown carbon—in addition to mitigation of other SLCPs that may arise from similar sources—occur faster than reductions of the cooling sulfates. While organic carbon is reflective, the warming effect of black and brown carbon components overall amplify warming. Nearly 90% of black carbon emissions come from residential solid fuels, diesel engines, and residential coal; the rest of the emissions come from aviation, shipping, and flaring. Reducing black carbon is especially beneficial for the Arctic because black carbon not only warms the atmosphere but also facilitates additional warming. Once black carbon is deposited on the snow and ice, it reduces the reflectivity (albedo) and absorbs extra solar radiation, which leads to further melting than pristine snow and ice. Since 1890, black carbon has contributed about 0.5–1.4 ºC of warming to the Arctic. Bond T. C., et al. (2013) Bounding the role of black carbon in the climate system: A scientific assessment, J. GEOPHYSICAL RESEARCH–ATMOSPHERES 118(11):5380–5552; Qian Y., et al. (2014) Light-absorbing Particles in Snow and Ice: Measurement and Modeling of Climatic and Hydrological impact, ADVANCES IN ATMOSPHERIC SCIENCES 32:64–91; Arctic Monitoring and Assessment Programme (AMAP) (2017) ADAPTATION ACTIONS FOR A CHANGING ARCTIC: PERSPECTIVES FROM THE BARENTS AREA; International Energy Agency (IEA) (2016) WORLD ENERGY OUTLOOK SPECIAL REPORT: ENERGY AND AIR POLLUTION; World Bank & International Cryosphere Climate Initiative (2013) ON THIN ICE: HOW CUTTING POLLUTION CAN SLOW WARMING AND SAVE LIVES. Myhre G., et al. (2013) CHAPTER 8: ANTHROPOGENIC AND NATURAL RADIATIVE FORCING, 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, Table 8.A.6; Shindell D. & Faluvegi G. (2009) Climate response to regional radiative forcing during the twentieth century, Nature Geoscience 2:294–300; Feng Y., et al. (2013) Brown carbon: a significant atmospheric absorber of solar radiation?, ATMOS. CHEM. PHYSICS 13:8607–8621. [Kristin Campbell, United States of America]	Rejected: This is covered in chapter 6
23913	51	17	51	42	See the above comment. [Branko Grisogono, Croatia]	Noted: No suggestions made

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
32083	51	18	51	25	Fig 7.10 is very confusing because it took me a while to realise it referred to 1850, not 1750 as in the previous figure and also in AR5 etc. This needs to be made very explicit for sleepy readers like me (I'm recovering from Covid) as i missed the shift in historic perspective from 1750 to 1850 and got very confused. It is not exactly clear why this historic context is changed? - I can't think of any really major reason to select 1850, as the coal industry was very well under way by then. The only reason for choosing 1850 is perhaps the start of the oil industry in Poland and then later in Pennsylvania in the late 1850s (but the natural gas industry came much later). This 1750/1850 switch is incredibly confusing. Thus I think it would be wise NOT to suddenly introduce 1850 as the reference point in history as the industrial and agricultural revolutions were both very well under way by then. Also 7.10 needs to have CO2 in it. I know this causes an artistic problem as the bar is so much bigger but it is really important that for public use there should be a clear diagram with the various gases placed in mutual context. In AR5 the comparable figure to 7.9 and 7.10 is 8.15 (SPM.5), which is very heavily used, as these figures here will be. It is vital that there should be this clear, simple comparison for use in teaching classes and media and that if the shift from 1750 to 1850 is retained, it should be made very obvious and explicit. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This figure has been removed.
31775	51	25	51	25	Can CO2 be included in Figure 7.10 please? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This figure has been removed.
46259	51	25	51	26	As the emission-based ERFs also account for the forcing by CO2, I would suggest to reformulate this sentence. [Twan van Noije, Netherlands]	Taken into account. We are not completely sure what the reviewer requires here, but added "short lived" for reduction of ambiguity.
77423	51	25	51	45	Could this material not have been addressed in the earlier section on methane and other gases? [Emer Griffin, Ireland]	Taken into account. Good suggestion - we can see arguments both ways. However, earlier, the forcing from the methane present in the atmosphere is discussed - here, the total methane-attributed forcing (and forcing from other emitted species like halocarbons) including indirect effects is discussed
71723	51	25	52	12	As noted in my general comment on the three different types of RF being treated here, I suggest that for clarification this section uses the term “total ERF” when including indirect effects as that is the terminology used in Thornhill et al. [Martin Manning, New Zealand]	Not applicable: This section has been removed.
46261	51	30	51	31	Would it be possible to separate the contributions from the semi-direct effects and ERFaci? [Twan van Noije, Netherlands]	Taken into account. Unfortunately with the Ghan 2013 set of diagnostics provided by models, it is not.
32085	51	31			My instant reaction was methane AR5 ERF 0.97, AR6 ERF 0.99 - thus Etminan make no difference, cancelled by other factors. It was only later I realised that I was not comparing like with like as the 0.99 Wm-2 value removes a whole century of rapid coal and cow increase - just chopped off! Surely the snow firn/ice core data are adequate to detail a 1750-onward ERF number?: it is really really confusing to diminish all the ERF values by moving to 1850 and ignoring the huge changes in 1750-1850. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reviewer is correct. Both provided in table
31777	51	37	51	37	"halocarbons" or "halogens"? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Halogenated compounds is more accurate in general and is our preferred term. There is one specific context in the sentence where "halocarbons" is correct, because the experiments undertaken in AerChemMIP perturbed only CFCs and HCFCs and these results are reported here.
71067	51	37	51	37	Is the "very likely" the IPCC uncertainty language? [Yu Kosaka, Japan]	Not applicable: This section has been removed.
100459	51	37	51	39	This range from 0.0-0.16 W m-2 for net ERF due to halocarbons seems too small. Sorry if I misunderstand, but one of the cited papers (which I assume is Thornhill et al., ACPD, doi: 10.5194/acp-2019-1205) gives a much larger range with an ERF from halocarbons of 0.15+/-0.27 W m-2, compared to 0.18+/-0.15 W m-2 in IPCC AR5. [Øivind Hodnebrog, Norway]	Not applicable: This section has been removed.
69837	51	37	51	39	The sentence is inaccurate. HFCs do not deplete ozone and accounted for 0.03 Wm-2 in 2016. World Meteorological Organization, United Nations Environment Programme, National Oceanic and Atmospheric Administration, National Aeronautics and Space Administration, and European Commission (2018). Scientific Assessment of Ozone Depletion: 2018. Geneva. Global Ozone Research and Monitoring Project-Report No. 58. ES.38 (“Radiative forcing from measured HFCs continues to increase; it currently amounts to 1% of the total forcing from all long-lived greenhouse gases. The radiative forcing arising from measured atmospheric mole fractions of HFCs totaled 0.030 W m−2 in 2016, up by 36% from 0.022 W m−2 in 2012; HFC-134a accounted for 47% of this forcing in 2016, while the next largest contributors were HFC-23 (17%), HFC-125 (15%) and HFC-143a (10%). Total HFC radiative forcing in 2016 accounted for ~10% of the 0.33 W m−2 supplied by ODSs (see Chapter 1), and 1.0% of the 3 W m−2 supplied by all long-lived GHGs combined, including CO2, CH4, N2O, ODSs and HFCs.”) [Gabrielle Dreyfus, United States of America]	Not applicable: This section has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
81393	51	37	51	39	How can there be high confidence in the halocarbon ERF extending to zero if this is a) a new finding and b) only supported by two studies? AR5 did not include any negative forcing effect on halocarbon (or indeed methane, N2O or NOx) from cloud feedback, so it looks like the lower end of the range presented here and in Figure 7.10 is based on only one study – for which it is explicitly stated that "There is low confidence in this cloud attribution due to the limited number of models studied". I find it problematic to then include this in a figure (7-10) that is going to be one of the most widely used ones. In addition, I was surprised to find that Polvani et al., 2019 ("Substantial twentieth-century Arctic warming caused by ozone-depleting substances"), which appears to be at odds with the two new studies, was not even mentioned in this chapter. [Johannes Laube, Germany]	Not applicable: This section has been removed.
81395	51	37	51	39	Also, given the inconsistencies between this chapter and Chapter 2 in terms of the number and type of species used to calculate ERFs: Which compounds were used to calculate this ERF range? Any non-ODSs? These species should be explicitly listed somewhere and there should be some consistency with other Chapters or at least a more open approach on possible differences. Finally, are these two studies published yet and do they indeed invalidate all previous ones? If so, a little more explanation and context is needed, not just for halocarbons. [Johannes Laube, Germany]	Not applicable: This section has been removed.
31779	51	38	51	38	This is confusing/surprising. It is unclear if you mean in models or obs. I thought observations constrained the stratospheric ozone variation quite well; so is this text implying that the observed ozone depletion record is incorrect or somehow the forcing resulting from it had previously not been computed properly? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This section has been removed.
81397	51	40	51	40	The definition of WMGHGs as used here and elsewhere in this chapter is inconsistent with the one used in Chapter 2, where these are separated into WMGHGs (CO2, CH4, N2O) and Synthetic Greenhouse Gases. "Synthetic Greenhouse Gases" includes a number of shorter-lived gases that are no longer well-mixed in the atmosphere. A similar problem is posed by the term "halocarbons" which encompasses a range of synthetic but also many biogenic compounds. Some coordination with Chapter 2 and also Chapter 6 (which includes a section on Short-lived Halogenated Species ranging from HCFCs, HFCs, halons, methyl bromide, and VSLS) would help to ensure consistency throughout AR6. [Johannes Laube, Germany]	Not applicable: This section has been removed.
46263	51	40	51	42	To the extent that these changes are associated with changes in surface temperatures over land, they should not be included in the ERF. If they are, it is a consequence of using an inconsistent definition of ERF (at fixed SSTs). Please clarify this. [Twan van Noije, Netherlands]	Not applicable: This section has been removed.
71069	51	42	51	42	Is the "low confidence" the IPCC uncertainty language? [Yu Kosaka, Japan]	Not applicable: This section has been removed.
51385	51	47	51	48	At which regional scale do BC emissions offset negative IRF from scattering aerosols? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This section has been removed.
77425	51	47	51	49	The statement on black carbon foring could be better quantified. What does significant mean here? [Emer Griffin, Ireland]	Not applicable: This section has been removed.
68373	52	2	52	12	Include CO2 in Figure 7.10. [Durwood Zaelke, United States of America]	Not applicable: This section has been removed.
66811	52	2	52	12	CO2 should be included in Figure 7.10 for comparison with the SLCPs, and the relative contribution of each should be further expanded upon in the text. [Kristin Campbell, United States of America]	Not applicable: This section has been removed.
32087	52	3			Again, I query the start date of 1850 - there is no obvious historic justification for this and it removes a whole century of massive land use change, cattle breeding and coal-fired industrialisation. 1750 is more accurate and valuable. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This section has been removed.
77427	52	17	56	10	This is very important material that could be better reflected in the Exec Summary and SPM. [Emer Griffin, Ireland]	Taken into account. Material has been addedd to the Executive Summary.
114599	52	17	56	12	Could you also give the temp development for the period 1750 to 1850-1900? (Sorry if I overlook somethat that is there) [Jan Fuglestvedt, Norway]	Taken into account. This is added and compared to the Chapter 2 assessment
114603	52	17	56	12	An obvious and perhaps not needed comment, but be sure to coorinate closely with ch2, 3 and 4 here. [Jan Fuglestvedt, Norway]	Taken into account. This has been done
10849	52	17			I am surprised to say the least to see this section in the same chapter that assesses ERF and ECS also assesses the forcing contributions to the observed warming between 1750-2018. Given the dependencies of ERF and ECS on GCMs and observations, any claims of understanding contributions to observed temperatures are overstated. This approach can be used as a guide to what the contributions are to observed warming, but nothing more than that. Circular reasoning should be discouraged. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We disagree on the circular reasoning point. An improvement over AR5 is that ECS. TCR and ERF estimates are largely independent of climate models. Translating forcings into temperature contributions is policy relevant. This discussion has been heavily expanded on in Section 7.3.5.4 and clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
31781	52	19	52	19	I don't think it is sufficiently clear, given the plots in in AR5, that this uses historical time series of concentrations. It needs to be said where they come from, and perhaps note uncertainties [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Details have been added on the sources of data with reference to chapter 2
46265	52	19	52	29	Please clarify if and how the energy balance models account for differences in efficacies of the various forcing agents. [Twan van Noije, Netherlands]	Taken into account. They do not - section 7.3.1 now referred to
32089	52	19			Goes back to 1750! Hooray!! But this makes 7.10 even more misleading as the reader immediately compares Fig 7.11 in C to Fig 7.10 in Wm-2, without realising there is a whole century missing from 7.10. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Figure 7.10 moved to Chapter 6
10789	52	22	52	24	Much more is needed than the rather obtuse "chosen to approximately maintain..." Details are needed. Or, more preferably, a reference of a study that has done this. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Further details are provided in the cross Chapter Box and appendix
15413	52	24	52	24	TCR is determined by Equation 7.A.2.2 in the two-layer model, not an emergent property. [Junichi Tsutsui, Japan]	Accepted. Agree, text changed
20423	52	28	52	29	Would this sentence mean that for the majority of contributions the part of uncertainty due to forcing is the largest, but for WMGHGs it is the other way around? The word "overall" is not completely explicit. The conclusion one might draw, concerning the future science effort, seems to be that progresses are called for on both sides. [philippe waldteufel, France]	Accepted. Yes, this is now clarified
37177	52	31	52	31	No. The previous paragraph say that the data in Figure 7.11 are estimates (which is correct given that no data is available from 1750 or, in most instances, for at least 200 years after that) but this sentence tries to imply that they are definite facts. Also, if it is so clear then why do you say it? [John McLean, Australia]	Taken into account. Text reworded to make line of evidence stronger
19405	52	31	52	31	"clear" is not standard terminology [Isaac Held, United States of America]	Taken into account. "clear" removed
10791	52	31	52	32	No, this inferred attribution claim is unfounded. It can be said that as ECS and ERF are both positive, that the estimated effect of anthropogenic forcing has a warming influence in a MODEL simulation of historic temperature changes. Given the use of temperature observations and ERF in constraining ECS, in similarly simple models, and the statistical and logical framework used in detection analyses, an attribution statement like this cannot be made here. Attribution conclusions inferred from circular reasoning should be avoided (e.g., Chapter 3, Section 3.1 Page 8:54) [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We disagree and don't make a statistical attribution test here. As in reply to other comments, we refute the circularity of the argument and carefully explain why. Text has been substantially expanded on
31783	52	33	52	33	1.9C is an implausible upper limit unless you posit a significant missing natural negative RF, large unforced variability or a serious error in the observational temperature record. This sentence could be completely misrepresented, if it went forward like this. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text reworded and analysis has been redone
114595	52	33	52	33	You could briefly mention that this is as GSAT. Just in parentheses. [Jan Fuglestvedt, Norway]	Accepted. agree, edited
20425	52	35	52	37	Figure 7.11 does not supply information about the aerosol cooling staying constant over the last 20 years; the reference to Figure 2.10 or 7.12 might be added here. [philippe waldteufel, France]	Accepted. references updated as suggested
77429	52	35	52	38	Rework or perhaps break this into two sentences as the combination reduces clarity, i.e. warming is comprised of warming and cooling. [Emer Griffin, Ireland]	Accepted. Suggestion adopted
77431	52	35	52	38	Should the cooling be -0.6C rather than 0.6C? [Emer Griffin, Ireland]	Noted. Sign convention is correct as is
77433	52	35	52	38	Is the level or aerosol cooling similar to GHG warming until the mid 20th century? [Emer Griffin, Ireland]	Taken into account. Yes, as seen in Figure. Further text has been added and figure properly referenced
77437	52	35	52	38	It should be clear that global aerosol cooling has remained relatively constant for 20 years but that there are major regional variations. [Emer Griffin, Ireland]	Taken into account. Agree, this refers to global, Chapter 6 is referred to for regional changes
77435	52	36	52	38	Should the cooling be -0.6C rather than 0.6C? [Emer Griffin, Ireland]	Noted. Sign convention is correct as is
31785	52	37	52	37	Fig 7.12? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Yes, corrected
38351	52	37	52	37	This sentence is mis-referenced. It is suggested that last 20 years (Figure 7.11) should be changed to last 20 years (Figure 7.12). [Yaming LIU, China]	Accepted. Yes, corrected
130527	52	37	52	37	last 20 years (Figure 7.11) shud le last 20 years (Figure 7.12). [Panmao Zhai, China]	Accepted. Yes, corrected
10793	52	37	52	40	No, this is not a "bottom up" estimate independent of results in chapter 3. The estimated "GSAT" rise, uses ECS which uses the observed temperature record as an important constraint (7.5.5), as well as historical estimates of ERF which have been influenced by climate models. This text must be amended to remove any claim that the simple model trends are somehow independent of the observational temperature record and other sophisticated climate models. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We disagree and don't make a statistical attribution test here. As in reply to other comments, we refute the circularity of the argument and carefully explain why we trust our estimates in the text

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114597	52	39	52	40	The sentence may cause some confusion among some readers. I suggest adding a bit more explananation [Jan Fuglestvedt, Norway]	Taken into account. Only to a small degree, this is now clarified to be more explicit
3559	52	39	52	40	Doesn't the ECS used rely on the temperature record? Is this what is meant here by "more or less"? [Joyce Penner, United States of America]	Taken into account. Only to a small degree, this is now clarified to be more explicit
114601	52	43	52	51	Figure 7.11 is really useful and is a significant step forward comapred to the ERF verion used in previous ARs [Jan Fuglestvedt, Norway]	Noted. Thank you
71071	52	45	52	48	It would be better to clarify that "Natural" does not include internal variability (especially in the bottom row). [Yu Kosaka, Japan]	Taken into account. Clarified
675	52	45	52	48	Figure 7.11 needs to specify the confidence bounds of the uncetainty shown. Also confusing that the same color is used for Halogens (top of figure) and Natural Forcing (bottom of figure). Need to change color of one of these. [Bruce Wielicki, United States of America]	Taken into account. Bounds added and Figure updated
31787	53	5	53	6	Arent these timeseries required for Fig 7.11 too? Apologies if I am confused. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Sections now merged
10795	53	16	53	17	Figure 7.12 does not show that CO2 "closely follow"s the multi-decadal trends. Maybe generally, and certainly it is the most dominant contributor to simulated trends. There is no need to overstate CO2's influence on multidecadal trends, the contribution to the century timescale trends is the most important thing to highlight. An attribution study framework is needed to make the statement about contributions to observed trends. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, edited to say long time scale trends
77439	53	16	53	17	Is this the temperature trend? Over multiple decades? [Emer Griffin, Ireland]	Accepted. Agree, edited to say long time scale trends
10855	53	16	53	20	This paragraph needs to be rephrased in several places to make clear that it refers to simple model simulations. The way it reads at the moment sounds like attribution of contributions to observed temperatures. Such claims cannot be made here (Hegerl et al, Good Practice Guidance Paper on Detection and Attribution Related to Anthropogenic Climate Change, IPCC 2009) [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We are clear it is independent of the attribution effort and cite Chapter 3
2699	53	16	53	22	This paragraph does not properly sumarize Fig. 7.12, which says nothing directly about "multi-decadal trends" . Nothing is said about the dominance of volcanoes in the earlier part of the record. Neither anthropogenic CO2 or aerosols played an important role before about 1950. How does the "total" compared to observed temperature changes? This paragraph and accompanying figure should be redone from scratch. [Bryan Weare, United States of America]	Taken into account. We agree, paragraph has been reworked
77441	53	18	53	18	global aerosol cooling? [Emer Griffin, Ireland]	Accepted. Text edited as per suggestion
77443	53	18	53	19	Important message for SPM. [Emer Griffin, Ireland]	Noted
114605	53	19	53	19	I suggest adding more references here [Jan Fuglestvedt, Norway]	Taken into account. Section 2.2 cited instead
32091	53	19			The wording is a bit misleading here as it implies there is a one to one connexion between non-CO2 warmers and aerosol. I think what is intended is to say that these two bars cancel each other out, but the subtext implies that they are linked in some way. Actually if you burn gas instead of high SO2 coal the aerosol load drops. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, paragraph has been reworked
10797	53	20	53	22	These are not "bottom up" estimates, because they use ECS - which has used observed temperatures, and ERF - which has used climate models. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We are clear it is independent of the attribution effort and cite Chapter 3, Sections now merged and text greatly expanded
132383	53	34	56	12	It would be useful to mention here a new type of emulator which can emulate multiple realisations of geographically explicit single-model simulations based on a calibration with a single projection from a given climate model (MESMER emulator: Beusch et al. ESD 2020: https://www.earth-syst-dynam.net/11/139/2020/). This allows to sample a larger phase space than CMIP5 and CMIP6 at regional scale. [Sonia Seneviratne, Switzerland]	Accepted. Link made to Chapter 11.
114607	53	34	56	12	Cross chapte box 7.1 is important given the role of emulators across chapters as well as for the link to WGIII. Please clarify interface with Ch1, as well as with WGIII, ch3 and Annex C. [Jan Fuglestvedt, Norway]	Accepted. Agree, links made more explicit
114611	53	34	56	12	Need to be clear on whether the same emulator is sued across chapters, and if now how consistent these emulators are. [Jan Fuglestvedt, Norway]	Accepted.
54513	53	34	56	12	Please update Cross-Chapter Box 7.1 so that it provides an overview of all emulators used in the WGI report. Make clear where (and why) emulators are not used in the report; and add an assessment of the implications and validity of the use of emulators. [Veronika Eyring, Germany]	Accepted. Agree, Box edited

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54515	53	34	56	12	Please ensure consistency of the emulators and parameter sets across chapters for related indicators, in particular between the emulators used in Chapters 4 and 7. [Veronika Eyring, Germany]	Accepted. Agree, Box edited and emulators made consistent
54517	53	34	56	12	Can the key outputs and inputs and the parameters alongside the underlying dataset for each emulator be specified for example in a table? [Veronika Eyring, Germany]	Accepted. Extra details are added as suggested
110847	53	36	56	10	The use of emulators is very well explained in this box. Perhaps, including the confidence interval numbers, within this box, in the past use of emulators in IPCC reports and comparisons of those numbers within the AR6 report will bring clarity/readability on the use of emulators in the AR6 report. [Monika Sikand, United States of America]	Accepted. A small intro on past use has been added
79093	53	37	55	10	For someone not super familiar with emulators, my first questions would: what is an emulator and how does it work? This information can sort of be gathered from this box but I think it could be brought out more clearly/directly so you have everyone on board from the beginning, maybe even with an instructive figure? [Aimee Slangen, Netherlands]	Accepted. A small intro on past use has been added
107655	53	37	56	10	I found parts of the box a bit sprawling and unfocused (fine for main text, but less for a box) and the takeaway message in the final paragraph was not clearly led up to by the previous text. Suggest some reworking/refocusing of the material to make crystal clear the value of emulators across AR6. [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, Box edited and emulators made consistent
4637	54	17	54	17	(e.g. Skeie et al., 2018) -->(e.g. Skeie et al., 2018, Nadiga and Urban, 2018) Reference: Nadiga, B. T., & Urban, N. M. (2019). Improved representation of ocean heat content in energy balance models. Climatic change, 152(3-4), 503-516. [Balasubramanya Nadiga, United States of America]	Noted. We considered citation but did not use
79091	54	25	54	49	for consistency with ch9, I'd suggest to change 'sea level rise' to 'sea level change'? [Aimee Slangen, Netherlands]	Accepted. Agree, change made
44311	54	30	54	35	This is also shown in Leach et al. (submitted 2020; https://www.geosci-model-dev-discuss.net/gmd-2019-379/), where the FaIRv2.0 model is introduced. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted, now cited
44313	54	32	54	32	This doesn't seem to be consistent with table in Chapter 1 (page 78/79), where simple models are classified into emulators and 'SCMs'. Chapter 1 seems written as if there is less intrinsic value because of reduced sophistication in an emulator class model than in a physical processes based SCM. Chapter 7 then classifies both FaIRv1.3 and MAGICCv6 as being emulators in this line in chapter 7. In Chapter 1 table they are labelled as more physically based SCMs. This is inconsistent. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text made consistent with Chapter 1
107653	54	32	54	35	This finding should be put in the context of GCM/ESM studies that run both RCPs and SSps with the same model, e.g. https://iopscience.iop.org/article/10.1088/1748-9326/ab81c2/pdf [Maycock Amanda, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, reference added
114609	54	32	54	35	The preliminary results susggested here are improtant for Ch4 as well as for WGIII. Please update and coordinate with the Ch4 and WGIII [Jan Fuglestvedt, Norway]	Taken into account. Text has been coordinated with Chapter 4
4639	55	4	55	7	These days, there are three basic approaches to address this shortcoming of the "too-simple" ocean formulation: an approach that dates back to Schlesinger and Jiang (1990) is to use an upwelling-diffusion modelling approach. (Here the following sentence could be added either parenthetically or as a footnote) However, also see Nadiga and Urban, 2018 who improve the representation of the vertical distribution of ocean heat uptake in EBMs by introducing a parameterization of the effect of ocean ventilation on heat uptake. [Balasubramanya Nadiga, United States of America]	Noted, references added
44315	55	8	55	9	This should further have references to FaIRv1.0/FaIRv1.3/FaIRv2.0 texts? [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text changed
15415	55	8	55	15	The layered box model and impulse response model are mathematically equivalent; the latter is a solution form of the former. Also, a model using a step response kernel (e.g., Good et al., 2011, https://doi.org/10.1029/2010GL045208) is another solution form and essentially the same as the impulse response model. Tsutsui (2020, https://doi.org/10.1029/2019GL085844) has recently been published and can be added to another reference of the MCE. The MCE references describe the derivation of the impulse response model from the box model as well as their parameters relationship. Note that the MCE uses the sum of three exponentials, which is equivalent to three-layer model. Anyway, I do not think that the variation of layers is related to model categorization. However, including a time evolution mechanism in the forcing-response relationship is a distinct property affecting model categorization. Models using such a mechanism are no longer within a linear response theory. [Junichi Tsutsui, Japan]	Accepted. Agree, discussion has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20427	55	26	55	37	What does the SSP-3-4-over mean on figure CCB7.1a? [philippe waldteufel, France]	Taken into account. Figure has been changed
54531	55	40	56	10	There is a section "Comparison of emulators with CMIP6 scenario results" but can a section "Comparison of emulators with observations" be added? [Veronika Eyring, Germany]	Taken into account. Structure changed in line with comment to show historical changes
44319	55	44	55	45	Schwarber et al (2019) offers a comparison of the impulse responses between SCMs. But it fails to propoerly discuss how the input parameter shoice largely guides the output reponse shape, and doesn't adequately attempt to set each simple model up in an identical way. Further, Schwarber et al (2019) seems to assume MAGICC6 model is the best, and compares responses to MAGICC for a goodness of fit. This seems biased when the response of FaIRv2.0 is flexible and determine entirely by user input parameter choice. FaIR can emulate the response of any other simple model, or GCM by changing input parameters. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Discussion altered in line with comment
44321	55	52	56	2	RCMIP also suffers the problem of not adequately demonstrating each model is being set up in an identical way. The only requirement for the comparison is they set model ECS to 3.0K. This is insufficient to constrain the thermal response of the models, since even in a 2-layer energy balance model the TCR (i.e. shorter timescale response) should be specified to define full response characteristics. How would one expect models to have similar responses when they arent run in similar set ups? This lack of clarity on input parameter assumptions reduces the utility of the RCMIP exercise. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Input parameter choices are clarified
3561	55	55	56	2	It is not clear why the lack of inclusion of natural variability causes an underestimate of warming; needs some short explanation. [Joyce Penner, United States of America]	Taken into account. Details added
22161	56	2	56	2	The assertion around carbon budgets needs a link to chapter 5 and to be assessed for consistency with their assessment if it is to be retained. Equally, why only call out the carbon budget as presumably this would affect a range of metrics being estimated from emulators? [Peter Thorne, Ireland]	Taken into account. Carbon budget text is deleted
10799	56	4	56	6	I find the confidence given here to this statement unconvincing. The example shown in Cross-chapter 7.1 Figure 1 is not a very good example of a simple climate model emulating the forced GSAT trends simulated by ESM. Can a better example be shown that supports the statement made here? If not, then confidence has to be reduced. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The example is now changed
44317	56	4	56	10	Could add for clarity that an impulse response based model is adequate to reproduce the behaviour of all other simple models. FaIRv2.0 text (leach et al (2020 submitted)) shows how a multigas impulse response based framework can be tuned to emulate all CMIP6 GCMs, and can reproduce MAGICC6-like behaviour. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text added as suggested
71073	56	5	56	6	Does this "natural variability" mean internal variability or include response to natural forcing? [Yu Kosaka, Japan]	Taken into account. It means natural forcings, text clarified
20085	56	10	56	10	Ocean heat contest change? Perhaps content? [philippe waldteufel, France]	Editorial. Typo corrected
46267	56	10	56	10	"ocean heat contest" should be "ocean heat content". [Twan van Noije, Netherlands]	Editorial. Typo corrected
69609	56	10	56	10	contest' -> 'content' [Nicholas Golledge, New Zealand]	Editorial. Typo corrected
41515	56	18	56	18	Might want to note that a feedback is a change in the energy budget dR that results from a change in surface temperature (dTs). I think that was noted earlier, but not here. [Andrew Gettelman, United States of America]	Accepted. Some text has been added
2701	56	18			remove "loosely" [Bryan Weare, United States of America]	Accepted. Text has been modified as suggested
83765	56	19	56	20	"the physical, biophysical/biogeochemical, and long-term feedbacks associated with ice sheets." is a little confusing because it can be interpreted as all feedbacks associated with ice sheets. Suggest rewording: "... three groups: (1) physical feedbacks (2) biophysical or biogeochemical feedbacks (3) long-term feedbacks associated with ice sheets [Marvel Kate, United States of America]	Accepted. Text has been modified as suggested.
46269	56	20	56	20	Please add "sea-ice albedo" as an example of a physical feedback. [Twan van Noije, Netherlands]	Accepted. Text has been modified and surface albedo has been added.
46271	56	21	56	22	Please add "natural sources of aerosols and precursors of aerosols and tropospheric ozone". [Twan van Noije, Netherlands]	Accepted. The text has been modified but not all suggestions have been included as the list does not need to be exhaustive here, it is only illustrative.
41517	56	37	56	37	uncovered several shortcomings in global climate models, which are starting to be corrected. [Andrew Gettelman, United States of America]	Accepted. Text has been modified as suggested
46273	56	43	56	43	The formulation "time- and state-dependence" suggests the time dependence is not governed by the state dependence only. Please explain what other time dependencies may arise. [Twan van Noije, Netherlands]	Taken into account. Reworded

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
3641	56	50	57	19	The treatment you are showing does not take into consideration time scales of the feedbacks. A detailed discussion of this essential matter is given in Ghil and Lucarini, The Physics of Climate variability and Climate Change, https://arxiv.org/abs/1910.00583, Rev Modern Physics, in press (2020). Also, see Lucarini et al. (2017) https://link.springer.com/article/10.1007/s10955-016-1506-z [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Reworded
9993	56	50	71	5	There is inconsistent emphasis on conventional vs RH-based feedbacks. Fig. 7.13 shows only RH-based lapse-rate and water vapor (RH) feedbacks, whereas table 7.10 shows only the conventional feedbacks. No mention is made of this inconsistency. Section 7.4.2.1 does not mention the RH-based Planck feedback at all. Section 7.4.2.2 refers to the RH-based Planck feedback as the C-C feedback (which is not widely used nomenclature), but line 11 of page 7-57 refers to this as P*. [Nadir Jeevanjee, United States of America]	Taken into account. The text has been modified in order to clarify the relationship between the two feedbacks decompositions, and when possible, the numbers for these two approaches are given.
67959	56	52			The section might better be labeled something like "Decomposing earths radiative response" [Robert Pincus, United States of America]	Taken into account. This is no longer a separate subsection.
67965	56	52			An overall discussion of feedback decomposition ought to express clearly the point made by Held and Shell (2012) that the choice of analysis framework for feedbacks represents a null hypothesis. The choice of relative humidity as a basis for evaluating feedbacks has the practical impact of reducing the inter-model spread in WV and LR feedbacks, but it's also a consequence of a particular assumption about warming. [Robert Pincus, United States of America]	Taken into account. Text has been modified.
15983	56	54	57	19	The paragraph states that the aggregate feedback can be decomposed into a sum of the individual feedbacks, and section 7.4 goes on to discuss the dependence of feedbacks on climate mean state with the general conclusion that the feedback sensitivity increases with increasing temperature (see page 72, line 13). In reality, the aggregated feedback can never be decomposed into an approximate sum of individual feedbacks due to the interacting nature of the feedback, and this is especially the case as the climate system moves a way from its previous equilibrium state. For example, increasing ocean stratification causes increased ocean heat content in the upper layers of the ocean. This stratification causes faster melting of the Arctic sea ice over the summer months for a given unit of solar energy input by constraining heat at the ocean surface, with the result that the feedback sensitivity of sea ice to rising temperatures increases. As the feedback sensitivity of the sea ice increases, then stratification of the oceans will be increased due to increased energy input into the upper layers of the ocean. This cycle repeats with the sensitivity of each feedback loop being a function of the output state of all the other feedback loops, until the loops reach their end state. The sensitivity of the climate (α) to temperature change is then a function of the number of feedback loops, the correlations between them and the temperature of the climate. This leads to a system dynamic where the initial response to an increase in radiative forcing is so small as to be imperceptible, but once a threshold is past, such as certain level of ocean heat content at the surface, then a rapid change occurs in all feedback loops and the planet's temperature transitions quickly into a new hothouse state. In effect, the feedback mechanisms act in parallel, rather than in series or independently as assumed by the statement made here. The uncertainty that this induces into the rate of change of temperature is reflected in the comments on page 75, line 53, which states, "the uncertainty in the magnitude of the Arctic amplification ranges from a factor of two to four" and in page 86, line 2, which states "the uncertainty in α is approximately three times as large as contribution of uncertainty in ΔF." Ultimately, the probability of moving through a "phase" transition from Holocene to Hothouse due to	Noted. The feedback decomposition as presented is the text is supported by a large amount of literature. The sum of these terms gives a total climate feedback which value make sense. Indeed, the ECS that can be derived from this total climate feedback is fully consistent with other estimates of the ECS.
23915	56	55	56	55	As usual, the Eqn. cited is hard to find directly in the text. Poor citation and hundreds of pages remote Figs, boxes, eqns. Continues throughout the entire report. - This can be equally well related to the whole Report, not only this Chapter. [Branko Grisogono, Croatia]	Taken into account. Referencing improved
128965	57	1	57	4	"As surface temperature changes in response to the TOA energy imbalance, many other climate variables also change, thus affecting the radiative flux at the TOA." This sounds like a circular chicken-egg argument. Change in radiative flux changes temperature changes radiative flux. Is this really the intention of this sentence? Clarify. [Trigg Talley, United States of America]	Noted. Indeed, this is the intention of this sentence and this is in fact the definition of climate feedbacks.
128967	57	1	57	4	What exactly is the rationale of using near surface temperatures in the estimation of climate feedback? Work by A. Dessler suggests tropospheric temperatures provide more robust estimates. [Trigg Talley, United States of America]	Noted. Tropospheric temperatures provide more robust feedbacks estimates but do not allow to analyse the change of the surface temperature, which is the topic addressed here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41519	57	2	57	2	..flux at the TOA (del N). [Andrew Gettelman, United States of America]	Accepted. Text has been modified.
23917	57	4	57	4	The variable N is unexplained in the partial derivative; please also see the above two comments. [Branko Grisogono, Croatia]	Accepted. Text has been modified.
67961	57	4			The decomposition described would be better labgeled "historical" than conventional [Robert Pincus, United States of America]	Taken into account. No longer relevant as the sentence has been changed.
103615	57	5	57	6	Suggest to make remark why GCMs etc use the term Planck response (there are many Planck terms in Physics) - it done nicely on page 59, maybe move this up earlier [Philippe Tulkens, Belgium]	Noted. This section gives only a quick overview of the climate feedbacks. To avoid repetition, we explain the Planck response only later.
67963	57	7	57	13	The discussion of a constant relative humidity framework is repeated in section 7.4.2.2. It need be included only once. [Robert Pincus, United States of America]	Accepted. The discussion is now only in 7.4.2.2
20429	57	7	57	13	It looks like climate scientists discovered meteorology after all! Just joking. [philippe waldteufel, France]	Noted
64517	57	7	57	15	I got confused in the WV+LR section because you said in the section introduction that you were going to use RH-based feedback definitions in the assessment, then you mentioned constant RH and Held+Shell 2012 in the WV intro paragraph... but then you provide WV feedback values in a specific humidity framework. I'd suggest deleting mention of RH-based feedbacks until you actually define them on p60 L14. [Peter Caldwell, United States of America]	Taken into account. The text of the WV + LR feedback has been revised to avoid confusion.
18629	57	9	57	12	Humidity change under fixed RH at a perturbed temperature are included in the Planck AND LR feedbacks (need to add LR* here, I think) [Masakazu Yoshimori, Japan]	Taken into account. This short discussion was incomplete, and has now been removed such that only the more complete discussion in 7.4.2.2 remains.
46275	57	15	57	18	It would be fair to acknowledge the work of Raes, F., Liao, H., Chen, W.-T., and Seinfeld, J. H. (2010), Atmospheric chemistry-climate feedbacks, J. Geophys. Res., 115, D12121, doi:10.1029/2009JD013300. [Twan van Noije, Netherlands]	Accepted. Citation has been added
17957	57	24	57	24	OK, I think I understand why you use newly here. But it seems awkward. [Dennis Hartmann, United States of America]	Accepted. Text has been modified
22165	57	24	57	30	I'm not sure this text adds much to the assessment and it applies more broadly to many aspects of the chapter anyway so if retained would surely make more sense in the introduction rather than introducing a 4th level subheading? [Peter Thorne, Ireland]	Taken into account. Text has been reworded and this section has been moved
46277	57	26	57	30	Wouldn't it be better to apply a similar weighting as for the temperature projections (Chapter 4)? [Twan van Noije, Netherlands]	Noted. Chapter 4 does apply weightings
20431	57	28	57	29	What about Box 4.1 ("Ensemble Evaluation and Weighting") [philippe waldteufel, France]	Noted. Chapter 4 does apply weightings
67967	57	29	57	30	This statement is inaccurate. It is not the number of models contributing to CMIP5 or CMIP6 that prevents the characterization of model uncertainty -- it is the fact that there is no systematic attempt to explore that uncertainty. [Robert Pincus, United States of America]	Accepted. Text reworded
83769	57	29		30	"the ensemble sizes…" is this true? What would be a sufficiently large ensemble size? This seems to contradict the above (true) statement that the models themselves do not span the entire range of possible parameters and parametrizations [Marvel Kate, United States of America]	Taken into account. Text has been reworded
16183	57	29			This is a strange statement -- how many models would one need to "fully sample model uncertainty"? And what would that really mean, and how would one ever know if it were true? [Steven Sherwood, Australia]	Taken into account. Text has been reworded
83767	57	32	57	34	I assume "radiative flux" and "radiation flux" are the same thing- maybe pick one? And clarify that this is TOA flux? [Marvel Kate, United States of America]	Taken into account. Text has been reworded
46281	57	40	57	58	Please also explain how the results are affected by the first ~20 years of the simulations, as these may be outliers in a regression analysis. See also the paper by Rugenstein et al., where regression results for the first 150 years are compared with results using only years 20-150. [Twan van Noije, Netherlands]	Taken into account. Text has been reworded
5155	57	42	57	43	Consider an adjective to provide some guard against “there is an inconsistency” being quoted out of context. “There is a formal inconsistency” or something like that. [Daniel Murphy, United States of America]	Taken into account. Text has been reworded
33189	57	42	57	47	I didn't understand these sentences that describe an "inconsistency" between the regression over years 1-150 and the definition of ERF in Box 7.1 that gives rise to the regression method giving overly positive feedback. Is it trying to say that the regression method includes a component of land warming adjustments in its ERF, despite having a global-mean dT=0 ERF? If so, I think it needs to be more explicit. If it is, then Andrews et al. (2015; JCLIM; https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-14-00545.1) point to such temperature adjustments - with zero global mean - in the regression methods ERF definition, which might be useful. Or maybe something else was meant? [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been reworded

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19407	57	43	57	48	it would be better to more formally assess these two effects -- the disntinction between the true model ECS and the 4X_150yr_regression estimate, and the TOA response to fast land warming -- rather than saying so casually that these probably cancel, without any statement concerning sensitivity. Some models have values twice the 10% number for the former (ie. Winton et al 2020, https://doi.org/10.1029/2019MS001838 . [Isaac Held, United States of America]	Taken into account. Text has been reworded. The long term dependence feedback is considered in detail in Section 7.4.3
46279	57	45	57	47	According to study by Rugenstein et al. the equilibrium warming is underestimated by about 17% in the model median when regression is applied to the first 150 simulation years. This would imply that the impact on the feedback parameter is larger than the quoted 10% (consistent with the results presented in Figure 2b of the paper). Please give a more accurate estimate of the effect. [Twan van Noije, Netherlands]	Taken into account. Text has been reworded
24197	57	45	57	48	Rugenstein et al. 2019b show "a median 17% larger equilibrium warming than estimated from the first 150 years of the simulation" based on an analysis of 27 millennial-length simulations from 15 climate models. Given this, it seems that a more accurate figure to quote here is 15% (since an increased equilibrium warming of 17% corresponds to a reduction of roughly 15% in the feedback parameter; 1.17= 1/0.855). Reference: Rugenstein, M., Bloch-Johnson, J., Gregory, J., Andrews, T., Mauritsen, T., Li, C., et al. (2019b). Equilibrium climate sensitivity estimated by equilibrating climate models. Geophys. Res. Lett., 2019GL083898. doi:10.1029/2019GL083898. [Mitch Bushuk, United States of America]	Taken into account. Text has been reworded
46283	57	48	57	48	The argument of compensating errors (associated with ERF and ECS) may hold for the feedback estimate but not for ECS. I would prefer a formulation which acknowledges that the 150-year regression will underestimate the models' actual ECS. Where this is possible, the errors associated with the different methods should be made explicit. [Twan van Noije, Netherlands]	Taken into account. Text has been reworded
41521	57	50	57	51	better to say " a 'radiative kernel' method is often used (Soden et al 2008). [Andrew Gettelman, United States of America]	Taken into account. Text has been reworded
46285	57	50	57	52	x is not a scalar but a 4-D field. Please describe the space and time dependence of the perturbations in more detail. [Twan van Noije, Netherlands]	Taken into account. Text has been reworded
83771	58	1	58	1	"in GCMs" -> "in most GCMs" (some fail the clear-sky linearity test [Marvel Kate, United States of America]	Taken into account. "Most GCMs" used but sentence changed
93081	58	1	58	2	I would not count atmospheric reanalysis as 'observations' [Claudia Stubenrauch, France]	Taken into account. Text has been reworded to refer to these as observations
128969	58	5	58	5	The assessment of climate feedback seems flawed. Assume feedbacks are additive and that they are linear. Is the uncertainty in feedback estimates due to these assumptions? [Trigg Talley, United States of America]	Noted. This is a standard approach
2705	58	10	58	23	Table 7.10 is about 10 pages later [Bryan Weare, United States of America]	Taken into account. Text has been reordered to align table and text
67969	58	10			When explaining that many GCM-simulated feedbacks are similar in the CMIP5 and CMIP6 ensembles it would be better not to rely on the multi-model mean. This measure would be relevant if the two sets of simulations were normally distributed with each member fully independent within and between the sets of simulations. None of these assumptions is valid. This chapter can exemplify the community's understanding and still make the point that many feedbacks are well-understood and consistently simulated [Robert Pincus, United States of America]	Taken into account. Text has been reworded. We no longer just discuss the multi model mean values
16185	58	14			I assume this the multimodel mean feedback that is 20% larger? [Steven Sherwood, Australia]	Taken into account. Text has been reworded. Yes the mean feedback is larger, this is now discussed
67971	58	17			Inter-model spread in CMIP simulations does not characterize uncertainty. [Robert Pincus, United States of America]	Taken into account. Text has been reworded to talk about range of model results rather than uncertainty
46287	58	38	58	38	Please change "non-biogeochemical" to "biophysical and non-CO2 biogeochemical". [Twan van Noije, Netherlands]	Taken into account. Revised.
128971	58	42			Should be Zelinka et al., 2020. [Trigg Talley, United States of America]	Accepted.
51301	58	47	58	47	When you say that "models have improved", what specifically do you mean by this? According to some metrics (representation of past historical temperature trends, for example) they don't (all) have appear to have improved. It would be helpful for you to describe exactly what you mean by this remark, perhaps with reference to specific metrics. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been reworded to clarify we mean some improvements to the cloud schemes
17961	58	47	58	48	Exactly which improvements are you referring to in this context? What follows in this paragraph sounds like continuing confusion. Paragraph is not helpful as it stands. [Dennis Hartmann, United States of America]	Taken into account. Text has been reworded to clarify we mean some improvements to the cloud schemes

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22167	58	47	58	48	This should surely make cross-reference to chapter 3 where the substantive assessment of this was performed? Paragraph should be checked against chapter 3 for consistency and it may be possible to shorten the paragraph accordingly via cross-referencing? [Peter Thorne, Ireland]	Taken into account. Text has been reworded to be consistent with Chapter 3 and cross referenced
2703	58	47	58	52	The sweeping statement is uncited with neither published papers or earlier AR6 analyses.Furthermore, consistency or inconsistency between an arbitary selection of models is no proof of improvement. This paragraph needs t be completely redone. [Bryan Weare, United States of America]	Taken into account. Text has been reworded to describe model differences in a more complete way
67973	58	47			The assertion that models have "improved" would benefit from specificity and supporting evidence [Robert Pincus, United States of America]	Taken into account. Text has been reworded to be clearer on how models have changed
33191	58	48	58	49	This reads as if the spead in all non-cloud feedbacks has reduced in CMIP6 compared to CMIP5. Was that what was meant? If so, it seems inconsistent with Zelinka et al. (2020, GRL; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL085782) who found the variance in feedbacks - except surface albedo - essentially unchanged. How can this be reconciled? [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been reworded to clarify the range of non-cloud feedbacks in CMIP6 compared to CMIP5
1671	58	48	58	50	Or models now share more common components than in CMIP5 as hinted in p. 7-57, line 26. [Lazaros Oreopoulos, United States of America]	Taken into account. We agree, this overlap is now discussed
67975	58	48	58	50	This argument borders on the specious. It will be difficult to demonstrate that model changes uniformly improve simulations; even if this were the case the community has plenty of examples in which more detail and/or "realism" leads to less agreement, not more. [Robert Pincus, United States of America]	Taken into account. Text has been reworded to make clear that not all model changes are improvements
128973	58	48	58	50	Or models now share more common components than in CMIP5 as hinted in p. 7-57, line 26. [Trigg Talley, United States of America]	Taken into account. We agree, this overlap is now discussed
51303	58	50	58	55	This is an important statement as it touches upon one reason why the models may be seeing more realistic behaviour in particular processes but an emergent property like ECS is/may be incorrect. However, it is not entirely clear in the section that refers to tuning. Is the intention to say that cloud processes are improved but then have to be tuned (along with other parameters maybe) in order to reflect, for example radiation budgets as a whole? Or are cloud processes becoming more accurate because they are being tuned. We would welcome a clarification, including on the relationship between improved parameterisation and tuning. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been reworded to increase the clarity around model improvement
5157	58	52	58	54	“This happens because physical processes in models may have been tuned…”. Although correct, I don’t think this clearly capture the reasons inter-model spread can increase. I believe a better way of explaining the spread is to look at it from a different angle. Inter-model spread is increasing because we do not yet even know, let alone implement in models, a reasonably complete list of the important physical processes for cloud feedback in a GCM. Crucially, physical processes have both signs. As models add/improve individual processes the cloud feedback can either increase or decrease. Until we have a reasonably complete list individual model feedbacks will both increase and decrease. [Daniel Murphy, United States of America]	Taken into account. Text has been reworded to increase the clarity around model improvement
5159	58	52			At the same time, this “incomplete list” concept is a good reason why the multi-model consensus can be better than any individual model: different models are exploring different portions of the parameter space (sort of stated in the sentence starting line 56). For example, I’m slightly involved in an effort to massively increase vertical (but not horizontal) model resolution in selected GCM grid cells only when they have meteorological situations suitable for certain kinds of clouds. It is extremely promising for improving comparisons to satellite cloud data at reasonable computational cost. But I wouldn’t recommend that every model immediately adopt it – having one model adopt it might well increase confidence and model spread at the same time [Daniel Murphy, United States of America]	Taken into account. Text has been reworded to increase the clarity around model improvement and how it relates to model spread
67977	58	52			Inter-model spread in CMIP simulations does not characterize uncertainty. [Robert Pincus, United States of America]	Taken into account. Text has been reworded to increase the clarity around model improvement
128975	58	56	59	2	The intent and meaning of this sentence is unclear. [Trigg Talley, United States of America]	Taken into account. Text has been reworded to increase the clarity around model improvement

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64541	58	58	59	2	I think you're being too optimistic here. You say narrowing of spread in non-cloud feedbacks means models are improving, but then claim that *lack* of narrowing in cloud feedbacks must be due to exposure of compensating errors rather than simply a lack of progress. My feeling is that a). CMIP6 cloud feedback spread is driven by some models improving their cloud parameterizations (particularly of supercooled liquid) while others haven't and b). improvements in modeled clouds are modest because modeling centers lack clarity in *how* to improve cloud representation. [Peter Caldwell, United States of America]	Taken into account. Text has been reworded to increase the clarity around model improvement
79223	59	5	59	14	It should be clear that feedback assessments mainly refer to CO2 increase are here. While the assessed feedbacks are induced from surface temperature change, there is no guarantee that the feedbacks are independent from the forcing agent (e.g. for stratospheric water vapour). Moreover, there should be some referrence to ozone feedbacks in this chapter that are currently only mentioned in Chapter 6. While work on this subject is by far not as comprehensive as for the physical and biogeochemical feedbacks, there is still some evidence for chemical feedbacks to have sufficient impact on climate sensitivity (Dietmüller et al., JGR 2014; Muthers et al, GMD 2014), Nowack et al (Nature Geoscience 2015). [Michael Ponater, Germany]	Taken into account. The contribution of ozone is assessed in chapter 6 and is only mentioned in this chapter.
67979	59	5			The section's aspiration to assess feedbacks based on a range of evidence, including but not limited to CMIP simulations, is terrific. [Robert Pincus, United States of America]	Noted. Thank you.
83777	59	7	67	27	I found this section a little confusing in light of the previous and subsequent discussion of the pattern effect and the difference between the ECS that would be inferred from transients and the "true" equilibrium ECS. Presumably all observational constraints on the feedbacks are derived using recent historical observations, and thus reflect a climate in disequlibrium, I think that a sentence making this clear should be added to the beginning of the section. [Marvel Kate, United States of America]	Taken into account. Introduction improved
93683	59	11			“individual feedbacks” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you.
20433	59	19	59	21	An enlightening way to interpret this response might be to comment how the planet, submitted to additional heating, reacts in becoming warmer, so as to radiate back the extra energy received and achieve again a balanced energy budget. [philippe waldteufel, France]	Noted
39603	59	36	60	47	According to NOAA, the relative atmospheric humidity has declined since 1948 by 10 % at the altitude of 4 km and even by 20 % at the altitude of 10 km where the CO2 molecule mainly radiates towards space. These decreases do not fit the hypothesis of positive feedbacks of water vapor. [François Gervais, France]	Rejected. Not supported by the peer-reviewed literature. Contrary to what the reviewer claims, Blunden and Arndt (2017) show an almost constant RH in the upper troposphere since 1980.. Blunden, J., and Arndt, D. S. (2017). State of the Climate in 2016. Bull. Am. Meteorol. Soc. 98, doi:10.1175/2017bamsstateoftheclimate.1
71075	59	39	59	39	"global mean surface temperature" GMST or GSAT? [Yu Kosaka, Japan]	Accepted. It should be GSAT. Text has been modified.
20435	59	39	59	42	While the trend is qualitatively beyond discussion, other parts of the WG1 (see §2.3.1.3.3) are less strongly affirmative. [philippe waldteufel, France]	Taken into account. Report text harmonised
128977	59	40	59	40	This a bit of a nit-pick but hardly think 'Soden and Held' and 'Held and Shell' are the definitive references for fixed relative humidity as this has been understood for decades before these references appeared. [Trigg Talley, United States of America]	Accepted. References have been modified.
67981	59	40			Is there evidence beyond climate model simulations for the assumption that RH stays roughly constant with warming? If not the source of this assumption might be clarified. [Robert Pincus, United States of America]	Taken into account, text reworded and reference to AR5 added
15985	59	48	59	49	Clarify if the two alphas are means or if one is a mean and one is standard deviation. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Text has been modified.
67983	59	48			The "structural uncertainty arising from the radiative kernel" referred to here and throught the chapter is more accurately described as "methodological uncertainty" [Robert Pincus, United States of America]	Accepted. Text has been modified.
22169	59	53	59	55	While this is, I believe, true, the current assessments of chapters 3 and 4 are more equivocal on the matter. I made detailed comments on their drafts which would bring them closer in to line with this text but there is a clear need for at least chapters 2,3,4 and 7 to discuss this matter in further detail. [Peter Thorne, Ireland]	Taken into account. Report clarified
17963	59	54	59	55	I thought that in the relative humidity frame work you are using that the lapse rate feedback is small, because it is mostly cancelled by water vapor increases associated with the assumption of fixed RH. You are randomly going between fixed RH and fixed specific humidity feedback analysis references. Do we really need all this review? [Dennis Hartmann, United States of America]	Taken into account. The text has been modified in order to clarify the relationship between the two feedbacks decompositions. These two decompositions have been kept as both are used in the literature that analyse the water vapour feedback.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16187	59		61		These clear-sky feedback sections are very nicely done. One thing they fail to do, however, is to highlight what is new since AR5 (maybe not too much, but the stratosphere feedbacks are definitely new and probably some of the albedo?). There is a long section earlier describing how CMIP6 models differ from CMIP5, but to me it is more important to explain what we understand that is new, which is missing here. [Steven Sherwood, Australia]	Taken into account. Text has been modified.
37179	60	1	60	1	This line mentions "meridional heat transport" (which takes time) conflicts with Figure 7.3 which purports to be an instantaneous energy balance. Either it is wrong or this text on page 60 is. [John McLean, Australia]	Taken into account with some minor rewording
19329	60	2	60	4	Consider making explicit the connection to bottom-heavy warming, i.e., "Strong wintertime temperature inversions lead to warming that is larger in the lower troposphere, and a positive lapse rate feedback in polar regions." In addition to Manabe and Wetherald (1975), the following paper demonstrates the connection between wintertime inversions and boundary layer warming: Bintanja, R., van der Linden, E.C. & Hazeleger, W. Boundary layer stability and Arctic climate change: a feedback study using EC-Earth. Clim Dyn 39, 2659–2673 (2012). https://doi.org/10.1007/s00382-011-1272-1 [Nicole Feldl, United States of America]	Accepted, reference has been added
19331	60	2	60	4	The following submitted paper would be a useful addition as it draws a clear connection between changes in wintertime inversions and the positive polar lapse rate feedback: Feldl, N., S. Po-Chedley, H. A. K. Singh, S. Hay, and P. J. Kushner, Sea ice and atmospheric circulation shape the high-latitude lapse rate feedback lapse rate feedback, submitted. [Nicole Feldl, United States of America]	Accepted
67985	60	11			Readers would be grateful for a one-phrase explanation about the physical mechansims coupling RH and LR feedbacks [Robert Pincus, United States of America]	Taken into account. A explanation paragraph has been added to the beginning of the section, as suggested.
67987	60	14			The choice of relative humidity as a basis for evaluating feedbacks has the practical impact of reducing the inter-model spread in WV and LR feedbacks, but it's also a consequence of a particular assumption about warming. [Robert Pincus, United States of America]	Noted
19333	60	19	60	26	Consider "modified lapse rate feedback" as LR* doesn't convey much meaning. [Nicole Feldl, United States of America]	Accepted. Text has been modified.
16693	60	24	60	24	change "These three feedbacks are shown Figure 7.13a." to "These three feedbacks are shown in Figure 7.13a." [Chuanfeng Zhao, China]	Accepted. Text has been modified.
128979	60	24	60	26	The three components of the feedbacks referred to in the previous paragraph are hard to discern in Figure 7.13. [Trigg Talley, United States of America]	Taken into account. Figure improved
93083	60	29	60	30	change in RH': how large is the change; in the sentence above it is mentioned to be close to 0 [Claudia Stubenrauch, France]	Taken into account. Text reworded as too detailed
67989	60	29			It is unclear how the observational study of Bony et al. 2020 is to be connected to the distribution of feedbacks inferred only from climate models (Fig 7.13 a) [Robert Pincus, United States of America]	Taken into account. Text has been reworded to increase the clarity
128981	60	33	60	34	Where do authors discuss the RH biases that are claimed to be reduced? [Trigg Talley, United States of America]	Taken into account. Text reworded and shortened as too detailed
37181	60	44	60	44	This assertion that the water vapour and lapse rate feedback is positive appears to be based on three unvalidated climate models that are constructed at least in part from assumptions. It is very unscientific and unprofessional to claim that they are proof of anything. [John McLean, Australia]	Rejected. This is not true
19409	60	44	60	47	After highlighting the RH based decomposition, the assessed ranges obtained with this decomposiiton are not provided; instead the text just switches bacl to the traditional formulation [Isaac Held, United States of America]	Accepted. Text has been modified. Both decompositions are presented and assess separately, before a common assessment at the end of this section.
22171	60	47	60	47	Is this likely range correct? It seems implausible that the upper bound shift by only 0.02? [Peter Thorne, Ireland]	Taken into account. Likely range revised
103617	60	47	60	47	Comment on why are intervals for very likely and likely are so close? (it does not really make sense to have two intervals like that .. considering the errors of the error estimates) [Philippe Tulkens, Belgium]	Taken into account. Likely range revised
38053	60	50	60	50	I think that surface albedo feedback should be separated by snow albedo feedback and ice albedo feedback. [Junhee Lee, Republic of Korea]	Taken into account. This separation is not possible with the literature
82859	60	52	60	55	Another study discussing surface and cloud contributions to changes in planetary albedo in response to strong forcing, and pointing at spread in cloud contribution (and its relation to estimated climate sensitivity) is Bender (2011). Refs: Bender, F. A.-M. (2011) Planetary albedo in strongly forced climate, as simulated by the CMIP3 models, Theor. Appl. Climatol., DOI: 10.1007/s00704-011-0411-2 [Frida Bender, Sweden]	Accepted. Reference has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67991	60	54			Inter-model spread in CMIP simulations does not characterize uncertainty. [Robert Pincus, United States of America]	Taken into account. The statement does not imply that CMIP inter-model spread characterizes uncertainty.
20437	61	1	61	3	This albedo is of historical interest, as Budyko used it in 1969 to illustrate a 2-equilibria situation through what may have been the first climate model ever. Without any doubt WG1 authors know this much better than the present reader… [philippe waldteufel, France]	Noted
90553	61	3	61	10	In the assessment of surface albedo feedback you may consider the emerging observational analysis of the "Spatially varying Signatures of Surface Albedo Feedback on the Northern Hemisphere Land Warming" just performed in Alessandri et al 2020 (Submitted in ERL; I'm available to provide the submitted draft). In this paper we show that snow-cover reduction due to climate warming consistently provides a significant positive feedback. On the other hand, vegetation greening can provide both positive and negative feedbacks. During the historical period (1982-2012) under investigation the negative component of vegetation feedback is shown to prevail, therefore significantly reducing the regional temperature increase. Citation: A. Alessandri, F. Catalano, M. De Felice, B. van den Hurk and G. Balsamo, 2020: Spatially varying Signatures of Surface Albedo Feedback on the Northern Hemisphere Land Warming, Submitted to Environmental Research Letters. [Andrea Alessandri, Italy]	Noted. The mentioned article has been accepted very late and not able to be included. We feel the text already adequately covers the feedback discussion
24201	61	12	61	28	The text of this paragraph is generally confusing, since some of the values refer to NH averages (Flanner et al., 2011; Crook and Forster, 2014.), whereas others refer to global averages (Pistone et al., 2014; Cao et al., 2015). This distinction should be made more explicitly in the text. Additionally, some of the quoted values appear to be inconsistent with their corresponding references. Firstly, Flanner et al. (2011) report a NH cryosphere albedo feedback of 0.62 (0.33-1.07) W/m^2/K, which disagrees with the values quoted on page 61, line 16. Secondly, I was unable to find the quoted value of 0.8±0.3 W/m^2/K (on line 18) in Crook and Forster (2014). Thirdly, the Cao et al. (2015) reference has two estimates for the Arctic sea ice contribution to global albedo feedback: the 0.31 W/m^2/K value quoted on line 20, as well a value of 0.19 (0.11-0.30) W/m^2/K. The 0.19 value is their initial estimate, whereas the 0.31 value is obtained after an adjustment based on a calibration to CERES data. I suggest reporting both of these estimates here, in order to highlight the sensitivity of albedo feedback estimates to the chosen method. Also, I suggest adding a reference to the recent study of Donohoe et al. (2020), who reported an Arctic sea ice contribution of 0.16±0.04 W/m^2/K to the global surface albedo feedback, which agrees quite closely with the value reported in Flanner et al. (2011). They also argue that the estimate of Pistone et al. (2014) may be biased high due to covariance between atmospheric optical properties and sea ice. This offers a different interpretation to the text on lines 21-24. Donohoe et al. (2020) also report a global surface albedo feedback value of 0.37 W/m^2/K, which agrees quite well with the central estimate provided in Table 7.10. Reference: Donohoe, A., Blanchard-Wrigglesworth, E., Schweiger, A. and Rasch, P.J., 2020. The effect of atmospheric transmissivity on model and observational estimates of the sea ice albedo feedback. Journal of Climate, (2020). [Mitch Bushuk, United States of America]	Taken into account. Although the articles refer to either the NH average or the global average, in the text all values refer to the global average and have been converted when necessary. This is why the values in the text may be different from those in the articles, as mentioned in the comment. Text has been modified to clarify that the values given correspond to values brought to the global scale. The suggested references have been added.
90555	61	14	61	21	In the quantitative assessment of the surface albedo feedback from observations you may consider the observational analysis in Alessandri et al 2020 (Submitted in ERL as mentioned above submitted draft can be shared). Over Northern Hemisphere land, we obtain the following quantitative estimates: a large positive surface albedo feedback of -0.87 [Confidence Interval 95%: -0.68, -1.05] W/(m^2·K) reflected solar radiation per degree of increase in temperature is estimated in the domain where only snow dominates. On the other hand the surface albedo feedback becomes predominantly negative where vegetation dominates. It is largely negative (+0.91 [0.81, 1.03] W/(m^2·K)) in the domain with only vegetation dominating, while it is moderately negative (+0.57 [0.40, 0.72] W/(m^2·K)) where both vegetation and snow are significantly present. Citation: A. Alessandri, F. Catalano, M. De Felice, B. van den Hurk and G. Balsamo, 2020: Spatially varying Signatures of Surface Albedo Feedback on the Northern Hemisphere Land Warming, Submitted to Environmental Research Letters. [Andrea Alessandri, Italy]	Noted. The mentioned article has been accepted very late and not able to be included. We feel that some of the detail of the paper is too technical here and that the text already covers the feedback discussion in sufficient detail

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
67993	61	14	61	39	Simply enumerating the results of a range of studies is less helpful than a synthesis or assessment [Robert Pincus, United States of America]	Taken into account, section reworded
17965	61	43	61	43	Comiso 2017 is out of date. SH sea ice has undergone major changes in the past 4 years. [Dennis Hartmann, United States of America]	Accepted. Text reworded
67995	61	53			Is there any evidence for this assessment that does not derive from GCM simulations? [Robert Pincus, United States of America]	Taken into account. Text reworded
128985	62	1	62	1	Figure 7.24 is stale and poorly conveys how clouds regimes are importantly established by circulation patterns. While the figure supposedly captures these cloud regimes at least meridionally, it doesn't convey in any explicit way the essence of the meridional circulation that they are connected to. Map these cloud regimes onto a better representation of the meridional circulation that also includes stratospheric Brewer Dobson circulation (BDC) as well as the main tropospheric circulation features. Also there is indeed an observed coupling between high tropical cloud changes, for example, and the BDC that hasn't been noted (Li and Thompson, 2013; JGR,118, 3486-3494, doi:10.1002/jgrd.50339) that is relevant to multi-annual high cloud changes and any interpretation of such changes in the context of feedback. [Trigg Talley, United States of America]	Noted. Neither the figure number nor the page number correspond to the commentary. You are probably referring to Fig. 7.14, which is very close to Fig. 7.11 already published in the AR5. Adding the mentioned elements would complicate the figure too much, which we want to keep simple and schematic.
128983	62	1	62	3	[CONFIDENCE] Not convinced that the simple empirical method used by Cao et al., for example, adequately deals with the complicating effects of clouds on sea ice sensitivity estimates. Those authors argue their methods produce a "more realistic estimate" (p. 1257 of their paper) but provide no real demonstration this is so and that the correction is at all realistic or even physical other than it falls between two other estimates. Don't think you can state a high confidence based on such meager evidence. [Trigg Talley, United States of America]	Taken into account, section reworded
64501	62	6	67	16	The balance of discussion does not reflect the importance of each topic. For example, tropical low cloud feedback has long been considered the largest source of uncertainty in ECS, yet it receives only a third of a page of discussion while 6 pages are devoted to polar amplification. Discussion of cloud feedbacks would benefit from more detail. Several suggestions for points to add are made in my other comments. [Peter Caldwell, United States of America]	Taken into account. The polar amplification section has been shortened. The assessment of cloud feedbacks, including the marine low cloud feedback, occupies about 5 pages in the FGD and it is indeed the longest subsection in 7.4.2.
37183	62	8	62	32	IPCC reports often assert that correlation proves cause and there are plenty of papers my own (McLean, 2014) "Late Twentieth-Century Warming and Variations in Cloud Cover" among them, that show an inverse correlation between cloud cover and temperature. It is very hypercritical of the IPCC to ignore these papers. [John McLean, Australia]	Rejected. Statistical correlation alone is not a basis of the assessment in 7.4.2, but process understanding is the key. We sometimes cite papers that present correlation between temperature and cloud fraction for example, but only when these results are supported by other lines of evidence by using numerical models or theory that directly reveal the causal relationship.
20439	62	8	62	32	There is no reason to criticize this passage. However, it is not essential; in case one wishes the WG1 report to become more compact, it might be deleted. [philippe waldteufel, France]	Rejected. No, this introductory paragraphs are necessary for readers to understand how the cloud feedback was assessed, what the sources of information are, and why we did so.
110849	62	8	66	42	The assessment in section 7.4.2.4 cloud feedbacks begins with an introduction of cloud droplets, ice crystals, and their mixture and how the microphysical processes interacting with aerosols, radiation and atmospheric circulation, resulting in a highly complex set of processes governing cloud formation and lifecycles across a wide range of spatial and temporal scales. The introduction adds more information in the next paragraph, esp. lines 22-25 on page 62. The next few sections connect the evaluation of clouds in climate models and the different cloud feedback mechanisms simulated in GCM supported by theoretical, observational, and process modeling studies and are assigned high confidence. However, the assessment in Arctic cloud feedback due to cloud microphysical properties, such as size or shape of cloud particles, linked with processes comes across limited. There is some description of cloud optical depth feedback resulting in negative feedback (Boucher et al. 2013) and the observationally constrained SW feedback over the Southern Ocean (Terai et al., 2016). The section on extratropical cloud optical depth feedback includes a brief description about the weakening of the phase change feedback in GCMs at the same time resulted in positive optical depth feedback over other extratropical oceans where LWP decreased in response to surface warming (Zelinka et al., 2020). The extratropical cloud optical depth feedback and the Arctic cloud feedback at the TOA are assessed with low confidence. The mixed-phase clouds are predominately found in the Arctic (Shupe et al. 2006). The assessment may be enhanced by incorporating the mixed-phase clouds microphysical properties and how such properties can impact the feedback systems in GCM. The observational constraints on the feedback system within GCM may be assessed using the in-situ measurements collected during the field campaigns in the Arctic. [Monika Sikand, United States of America]	Taken into account. We have stated that our assessment of the cloud feedbacks does not rely only on GCMs, but is based on a combined lines of evidence using GCMs, observations, and process models. To avoid confusion, we have moved the model evaluation section 7.4.2.4.1 behind the synthesis. Regarding the Arctic cloud feedback, several studies showed that the value at TOA is small no matter how cloud physical processes matter. This is the reason why we did not deeply discuss the detail of the feedback in this cloud regime.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
109313	62	9	62	9	condensate - should this be "condense"? [Paul Edwards, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
83773	62	9	62	9	condensate -> condense [Marvel Kate, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
65725	62	9	62	9	Suggest remove 'or small water droplets': this would mean that you already have a cloud therefore it does not fit with this sentence describing cloud formation. [Kushla Munro, Australia]	accepted.
3563	62	9	62	9	condensate should be condense [Joyce Penner, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
41523	62	12	62	12	I think it should be noted that because of these different processes and different cloud regimes, there are different types of cloud feedbacks that arise from processes in different types of clouds. This is detailed in Gettelman and Sherwood 2016 Gettelman, A., and S. C. Sherwood. “Processes Responsible for Cloud Feedback.” Current Climate Change Reports, October 1, 2016, 1–11. https://doi.org/10.1007/s40641-016-0052-8. [Andrew Gettelman, United States of America]	Accepted. Thanks for the suggestion. Gettelman and Sherwood paper has now been cited here.
67997	62	14			Clouds are described as coming in various "types", foreshadowing how the assessment of cloud feedbacks later is based on decomposing the feedbacks by regime (altitude, latirtude) and the quantity that changes. Could these ideas be linked more explicitly? [Robert Pincus, United States of America]	Taken into account. We have explicitly stated that cloud feedbacks were decomposed into different regimes and assessed for each.
17967	62	22	62	22	Liquid drops can remain stable above -40C, but they do not have to. There are many things that could induce them to freeze above 40C. Do not is safer than cannot. [Dennis Hartmann, United States of America]	accepted. The sentence changed.
31473	62	22	62	25	The presence of supercooled liquid and ice cystals in extratropical clouds are confirmed by satellite-borne lidar and radar measurements (Hu et al. 2009; Yoshida et al. 2010; Cesana and Chepfer 2013; Kikuchi et al. 2017). The following references are recommended to be added to support the description. Hu, Y., D. Winker, M. Vaughan, B. Lin, A. Omar, C. Trepte, D. Flittner, P. Yang, S.L. Nasiri, B. Baum, R. Holz, W. Sun, Z. Liu, Z. Wang, S. Young, K. Stamnes, J. Huang, and R. Kuehn, 2009: CALIPSO/CALIOP Cloud Phase Discrimination Algorithm. J. Atmos. Oceanic Technol., 26, 2293–2309, https://doi.org/10.1175/2009JTECHA1280.1 Cesana, G., and Chepfer, H. (2013), Evaluation of the cloud thermodynamic phase in a climate model using CALIPSO-GOCCP, J. Geophys. Res. Atmos., 118, 7922– 7937, doi:10.1002/jgrd.50376. Yoshida, Y., Okamoto, H., & Hagihara, Y. (2010). Global analysis of cloud phase and ice crystal orientation from Cloud–Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIPSO) data using attenuated backscattering and depolarization. Journal of Geophysical Research, 115, D00H32. https://doi.org/10.1029/2009JD012334 Kikuchi, M., Okamoto, H., Sato, K., Suzuki, K., Cesana, G., Hagihara, Y., Takahashi, N., Hayasaka, T., and Oki, R., Development of algorithm for discriminating hydrometeor particle types with a synergistic Use of CloudSat and CALIPSO, J. Geophys. Res. Atmos., 122, doi: 10.1002/2017JD027113, 2017. [Maki Kikuchi, Japan]	Rejected. We do not aim at evaluating clouds in current climate in this section but at assessing their feedback under warmed climate, so did not describe observational advances in measuring cloud properties.
64511	62	27	62	27	I disagree with the framing that breaking cloud feedback into thermodynamic and dynamic parts is a "challenge". I'd say instead that "Cloud feedbacks occur both as a direct response to local warming and in response to changes in environmental conditions due to warming-induced changes in large-scale circulation". [Peter Caldwell, United States of America]	accepted
128987	62	27	62	29	It is quite unrealistic and too simplistic to suppose the dynamics and thermodynamics of the atmosphere are uncoupled or their influences can simply be isolated from one another. The challenge is to understand how these components in fact interact -- one affecting the other, shaping clouds as a result. This in particular comes to the fore with high clouds and convection. [Trigg Talley, United States of America]	Taken into account. The sentence has been modified.
22173	62	27	62	32	Arguably the larger challenge is that aerosol forcing affects clouds and the available observational records are coincident with large scale changes in aerosol emissions making it hard to deconvolve the purely physical response from the forced response. Should this not be noted here? [Peter Thorne, Ireland]	Rejected. The issue related with the aerosol-cloud interaction was included in 7.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65727	62	27	62	32	Suggest clarification by including a similar description of the thermodynamic component in this paragraph. Currently there are 2 cloud feedback concepts discussed in the introductory sentence of this paragraph: thermodynamics and dynamics. But only the dynamic component is described. [Kushla Munro, Australia]	Taken into account. The sentence has been modified.
16189	62	27			I would see this as an opportunity rather than a challenge. There is no law that this particular decomposition must be done, it has simply been seen by many to be useful. [Steven Sherwood, Australia]	accepted.
68001	62	27			The decompostion of cloud feedbacks into thermodynamic and dynamic components is due to Bony et al. 2004 (doi:10.1007/s00382-003-0369-6). But the paragraph does raise the question - in what way does this present a particular challenge for clouds, and how is this reflected in the assesment of cloud feedbacks? [Robert Pincus, United States of America]	Taken into account. The sentence has been modified.
1673	62	35	63	5	A general comment for subsection 7.4.2.4.1 is that it does not cite the most recent relative work. Here are some specific suggestions: (1) p. 7-62, lines 43-44, "Although current GCMs lack the ability to reproduce some cloud regimes correctly…", two very appropriate works about this are Jin et al. (2017a,b), https://doi.org/10.1007/s00382-016-3064-0, and https://doi.org/10.1007/s00382-016-3107-6 which evaluate CMIP5 models that have provided daily ISCCP simulator output in AMIP-style experiments, using either the original global ISCCP Cloud Regimes (Weather States) or a simplified definition of cloud regimes. (2) p. 7-62, line 52, "Recent satellite measurements resolve the vertical distribution of clouds…", not only that, but they provide a decomposition of the CRE by cloud vertical distribution, see Matus and L'Ecuyer (2017), https://doi.org/10.1002/2016JD025951 and Oreopoulos et al. (2017), https://doi.org/10.1002/2017JD026629. (3) p. 7-62, line 54, "a thorough evaluation of the vertical profile of simulated clouds…", models are now even being evaluated by how well they simulate cloud vertical structure, but also about how well they decompose CRE by cloud vertical structure, see Lee et al. (2020), https://doi.org/10.5194/gmd-13-673-2020. [Lazaros Oreopoulos, United States of America]	Taken into account. The subsection has been moved behind the synthesis. Because of limitation of space, we did not much expand the cloud evaluation in models by referring to recent satellite measurements.
128989	62	35	63	5	A general comment for subsection 7.4.2.4.1 is that it does not cite the most recent relative work. Here are some specific suggestions: (1) page 7-62, lines 43-44, ""Although current GCMs lack the ability to reproduce some cloud regimes correctly, ..."", two very appropriate works about this are Jin et al. (2017a,b), https://doi.org/10.1007/s00382-016-3064-0, and https://doi.org/10.1007/s00382-016-3107-6 which evaluate CMIP5 models that have provided daily ISCCP simulator output in AMIP-style experiments, using either the original global ISCCP Cloud Regimes (Weather States) or a simplified definition of cloud regimes. (2) page 7-62, line 52, ""Recent satellite measurements resolve the vertical distribution of clouds, ..."", not only that, but they provide a decomposition of the CRE by cloud vertical distribution, see Matus and L'Ecuyer (2017), https://doi.org/10.1002/2016JD025951 and Oreopoulos et al. (2017), https://doi.org/10.1002/2017JD026629. (3) p. 7-62, line 54, ""... thorough evaluation of the vertical profile of simulated clouds ..."", models are now even being evaluated by how well they simulate cloud vertical structure, but also about how well they decompose CRE by cloud vertical structure, see Lee et al. (2020), https://doi.org/10.5194/gmd-13-673-2020. [Trigg Talley, United States of America]	Taken into account. The subsection has been moved behind the synthesis. Because of limitation of space, we did not much expand the cloud evaluation in models by referring to recent satellite measurements.
67999	62	35			How is this section seen as fitting into a broad assessment of cloud feedbacks that relies on many lines of evidence? Climate models do provide one line of evidence but the strength of that evidence could be assessed as needed. Perhaps this section could be eliminated. [Robert Pincus, United States of America]	Taken into account. In order to avoid a wrong impression as if our assessment was based only on GCMs, we have moved this subsection behind the synthesis section 7.4.2.7.
46289	62	35			Section 7.4.2.4.1. I don't think this is the most logical place for this section. [Twan van Noije, Netherlands]	Accepted. This subsection has been moved behind the synthesis evaluation 7.4.2.7.
17969	62	36	62	36	solar insolation is a redundant expression. It should never be used. Clouds both reflect and absorb solar radiation. The latter is not trivial. [Dennis Hartmann, United States of America]	Taken into account. Text has been revised.
64515	62	36	62	43	I think it's important to point out that LW cloud absorption strengthens with height and SW reflection doesn't. This allows the warm pool/subsiding region results you show to be framed as something that makes physical sense. [Peter Caldwell, United States of America]	Taken into account. In other words, we've stated that the greenhouse effect of clouds (via LW) strengthens with height whereas the SW reflection depends on the cloud optical properties (but not height).
83775	62	37	62	37	L W -> LW [Marvel Kate, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17971	62	40	62	40	CRE is the difference between clear and average conditions, not between clear and cloudy. [Dennis Hartmann, United States of America]	Accepted. Corrected.
28871	62	40			CRE can also be defined at the surface and in the atmosphere where heating acts to stabilise and cooling acts to destabilise the temperature profile [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The point is true, but here we discussed the TOA energy budgets to which clouds influence.
128991	62	42	62	47	This statement is not authoritative and too simplistic to have any value. [Trigg Talley, United States of America]	Noted. We described here that GCMs in general reproduce CREs in the present climate (due partly tuning).
17347	62	43	62	46	That the global mean net CRE of the CMIP5 multi-model mean agrees with satellite observations is not surprising as global mean TOA fluxes are main tuning targets (Hourdin et al., 2017). Since it's unclear which of the CMIP5 models had which tuning targets and it's likely that the agreement of the net CRE of the CMIP5 multi-model mean with satellite simulations is the result of tuning, this agreement can't be used as an assessment of model performance. That the overall distribution agrees with observations is likely not a result of tuning and indeed shows the performance of the models. [David Neubauer, Switzerland]	Noted. We stated here that the overall distribution of CRE in GCMs is similar to satellite observations, and as you suggest this agreement would have been affected less by tuning than the global-mean CRE.
64513	62	43	62	47	I'm uncomfortable with the assertion that GCM CRE matches satellites. This only happens because GCMs are explicitly tuned to do so. You seem to be implying here that GCMs represent an independent estimate of CRE. [Peter Caldwell, United States of America]	Taken into account. While the global-mean CRE might be the result of tuning, the spatial pattern were probably not (it's so difficult to tune the CRE distribution in GCMs). Anyway, we have modified the sentences.
65729	62	44	62	44	Suggest clarification of which cloud regimes are not reproduced correctly in GCMs. [Kushla Munro, Australia]	Taken into account. This subsection has been moved to 7.4.2.8, where we have mentioned that GCMs still do not represent well the marine low cloud and tropical anvil cloud.
95883	62	44	62	46	It should be clear here that this similarity is the consequence of model tuning, not model skill per se. [Philip Philip Stier, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
17973	62	46	62	46	How has the cancellation hampered efforts to quantify cloud feedback? Estimating the change in unbalanced CRE is equally significant. [Dennis Hartmann, United States of America]	Not applicable. This sentence has been deleted.
31479	62	49	62	52	The following papers on satellite simulators are recommended to be added. Masunaga, H., T. Matsui, W. Tao, A.Y. Hou, C.D. Kummerow, T. Nakajima, P. Bauer, W.S. Olson, M. Sekiguchi, and T.Y. Nakajima, 2010: Satellite Data Simulator Unit. Bull. Amer. Meteor. Soc., 91, 1625–1632, https://doi.org/10.1175/2010BAMS2809.1 Matsui, T., et al. (2014), Introducing multisensor satellite radiance‐based evaluation for regional Earth System modeling, J. Geophys. Res. Atmos., 119, 8450– 8475, doi:10.1002/2013JD021424. Hashino, T., Satoh, M., Hagihara, Y., Kubota, T., Matsui, T., Nasuno, T., and Okamoto, H. (2013), Evaluating cloud microphysics from NICAM against CloudSat and CALIPSO, J. Geophys. Res. Atmos., 118, 7273– 7292, doi:10.1002/jgrd.50564. [Maki Kikuchi, Japan]	Rejected. References were limited to papers providing information on satellite simulators for clouds and CRE (e.g. COSP) in GCMs. Suggested papers do not fit the purpose.
93085	62	52	62	52	it is worthwhile to add ';Stubenrauch et al., 2019', another satellite simulator study, as it uses a new cloud system approach: Stubenrauch, C. J., Bonazzola, M., Protopapadaki, S. E., and Musat, I. (2019). New cloud system metrics to assess bulk ice cloud schemes in a GCM. Journal of Advances in Modeling Earth Systems, 11, doi : 10.1029/2019MS001642. [Claudia Stubenrauch, France]	Rejected. References were limited to papers providing information on satellite simulators for clouds and CRE (e.g. COSP) in CMIP GCMs. Suggested paper sounds nice but shows an application to a single model.
31477	62	52	62	54	Please clarify the name of the satellite. [Maki Kikuchi, Japan]	Rejected. Due to limitation of space we could not give too much detailed information such as the names of satellites.
51305	62	60	62	62	Given that many of the important CMIP6 changes have related to cloud processes, is it appropriate to make reference to the Norris paper in support of this statement? Norris et al is based on CMIP5, does this hold true for CMIP6? Perhaps it would be worth clarifying this? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Since full analyses to CMIP6 models is not yet available, we referred to studies based on both CMIP5 and 6. This does not make problem because the feedback was assessed by combining lines of evidence, in which CMIP6 was regarded as one of them.
128993	63	1	63	1	[CONFIDENCE] Welcome the attempt of authors to do a more bottoms-up estimate of cloud feedback separated by cloud regime, but too many issues glossed over or overlooked in what seems a concerted effort to state a high degree of confidence and advance since AR5 that cannot be justified. [Trigg Talley, United States of America]	Taken into account. We acknowledge that there is a possibility that unknown cloud feedbacks that we did not discuss affect our assessment of the net cloud feedback. However, the aggregated feedback was also compared to an estimate of the net cloud feedback directly obtained from GCMs and observed interannual variability; it supports that we considered all major cloud regimes. We have stated that the high confidence of positive net feedback is justified by progresses in understanding the marine low-cloud feedback that had been a heart of the cloud feedback uncertainty.
41525	63	4	63	4	add Gettelman et al 2019 reference with Bodas-Salcedo et al 2019 [Andrew Gettelman, United States of America]	accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2707	63	4			the citation refers to a single model, not "some CMIP6 models" [Bryan Weare, United States of America]	Taken into account. We have added a reference to support the argument.
65731	63	5	63	5	Suggest clarification of the error associated with the subtropical low clouds, e.g. fraction, water contents, optical properties. [Kushla Munro, Australia]	Taken into account. We have added sentences to specify model errors related with the subtropical marine low clouds.
41527	63	13	63	13	I don't understand the Mid-latitude 'Reduced LWP (+)' in Figure 7.14 [Andrew Gettelman, United States of America]	Noted. The reduced LWP is an emerging property in CMIP6 models but not CMIP5, due partly to a weak sensitivity of LWP change to SST increase (Fig. 3 of Zelinka et al. 2020).
128995	63	19	63	19	Cloud optical depth feedback other than in the extra-tropics is neglected. The CERES flux and cloud data suggest very robust co-variability of TOA SW flux and cloud optical depth in the deep tropics (unpublished) largely driven by SST changes. In the short-term (observational record), the SW changes appear to cancel the LW effects of deep high clouds, suggesting an overall negative short-term feedback. Unfortunately, this study is not submitted yet and only concerns tropical short-term variability in response to SST interannual variability, but expect there must be similar findings in the existing literature. [Trigg Talley, United States of America]	Noted. We did not find references that examined a role of optical depth feedback in the tropical high clouds, so could not assess this effect although it might be implicitly included in the estimate of the tropical high cloud amount/altitude feedback based on observational records and/or CRMs.
34413	63	19			The assessed values of cloud feedbacks is very important as a basis for this assessment report's ECS estimate and particularly its smaller likely range. In the subsections of this section, it is not clear what is the basis of the assessed range of components (e.g. page 64 line 33) as well as the choice of disaggregation into cloud types and the associated assumption that of uncertainty in the feedback for each type is independent. Suggest adding some explanation about the basis for each. [Haroon Kheshgi, United States of America]	Taken into account. We have revised the introductory paragraphs of 7.4.2.4 to explain clearly the basis of the assessment (method) and assumption behind.
1679	63	26	63	26	"…the tropical high cloud regime occupies about 7% of the globe...", a citation is needed here. [Lazaros Oreopoulos, United States of America]	Rejected. This is based on our own calculation using ISCCP.
128997	63	26	63	26	"… the tropical high cloud regime occupies about 7% of the globe …" A citation is needed here. [Trigg Talley, United States of America]	Rejected. This is based on our own calculation using ISCCP.
102079	63	42	66	42	Chapter 7.4.2.4.2. Some physical processes are described in clouds in a specfic geographical distributions, but actually they are also common to clouds in other regions. I would like to suggest that explanations of main feedback mechanisms (marine low cloud feedback with the two main dominant factors, phase feedback, high cloud altitude feedbacks) come first, then describe cloud feedbacks in individual geographical regions. This restructuring would require paragraphs in 'tropical marine low cloud feedback' and in 'extra-tropical cloud optical depth phase feedback' to be moved earlier. Specific modifications are listed below. [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We decomposed the cloud feedback into cloud 'regimes' but not 'processes' because when aggregating these feedbacks we converted individual local feedbacks to global contribution by multiplying the area fraction. This approach does not fit the decomposition by processes, so we have retained the current structure.
1675	63	43	63	51	The longwave emission to space is presented here in a simplistic way. It does not depend only on the physical temperature of the cloud top. Aside from changes in the atmosphere above (e.g. water vapor – I understand that the feedback calculation assumes that everything other than the parameter of interest remains constant), what matters for emission to space is the radiative cloud top height which is different than the cloud top height because clouds are generally not black emitters. This is not an issue for thick convective towers (which are the subject of the next subsection), but is a factor to be considered if this subsection is about "high" clouds in general. [Lazaros Oreopoulos, United States of America]	Rejected. We agree that the longwave emission from high clouds depends not only on the cloud top height but partly on the optical property. However, a simplified explanation in the current text is sufficient to conclude that the upward shifted high clouds without changing the cloud top temperature causes a positive feedback. Because of limitations of space, we did not discuss further details of emissivity of high clouds.
129001	63	43	63	51	The longwave emission to space is presented here in a simplistic way. It does not depend only on the physical temperature of the cloud top. Aside from changes in the atmosphere above (e.g. water vapor), what matters for emission to space is the radiative cloud top height which is different than the cloud top height because clouds are generally not black emitters. This is not an issue for thick convective towers (which are the subject of the next subsection), but is a factor to be considered if this subsection is about "high" clouds in general. [Trigg Talley, United States of America]	Rejected. We agree that the longwave emission from high clouds depends not only on the cloud top height but partly on the optical property. However, a simplified explanation in the current text is sufficient to conclude that the upward shifted high clouds without changing the cloud top temperature causes a positive feedback. Because of limitations of space, we did not discuss further details of emissivity of high clouds.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
128999	63	43	64	33	[CONFIDENCE] Is the altitude feedback more predominantly for tropical high clouds as it seems the discussion is mostly with respect to tropical clouds? While it is true the tops of high clouds (and in a related way deep convection) appear to be lifted in response to a warming, this cloud top response jointly occurs with other changes to high clouds that imply a much more complicated set of interconnected feedbacks. Welcome discussion on the high cloud amount feedback which is a good addition since AR5. The co-variability of height and amount feedbacks is well illustrated in the study of Vaillant de Gulis et al., 2018; DOI:10.1038/s41598-018-34943-1 who show how cloud top changes come with an associated and high cloud amount change with observations showing the latter change is more profound in the thicker anvil regimes. Associated with these regimes are changes to cloud optical depths with then significant impacts on solar reflection (not shown in the Vaillant de Gulis et al. but part of ongoing study). Furthermore studies such as Protopapadaki et al. (2017, Atmos. Chem. Phys., 17, 3845-3859, www.atmos-chem-phys.net/17/3845/2017/ doi:10.5194/acp-17-3845-2017) indicate how proportions of thin to thick high clouds distinctly change with changes in convective intensity that one can interpret to be connected to changes in depth (and thus heights) of deeper convection. The point is the high cloud feedbacks are more complicated than is conveyed as simple positive altitude feedback. As these altitude changes also come with other cloud changes and at least for the case of tropical clouds are connected to deep convective processes, high confidence in their representation is not appropriate. [Trigg Talley, United States of America]	Taken into account. As we stated at L.43-44 on p.63, the cloud height increase will occur at all latitudes, and the assessed feedback consequently includes radiative effect not only over the tropics but over the globe (even though the feedback may be dominated by tropical change). We acknowledge that the tropical high cloud altitude feedback is not fully independent on others such as the high cloud amount feedback, but we could not assess the degree of covariation so assumed that they are independent. This assumption does not alter the mean value of the net feedback, but will widen the range, which will be appreciated given uncertainty in the high cloud feedback. We agree to your point that the high cloud amount feedback is complex and not well understood compared to the altitude feedback, and we have downgraded the level of confidence of this cloud feedback.
18631	63	51	63	51	Yoshimori et al. (2019, in press) is Yoshimori et al. (2020) now. [Masakazu Yoshimori, Japan]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
1677	63	51	63	54	There is also a study that didn't find a trend in cloud top height for 15 years of the Terra satellite record, Davies et al. (2017), https://doi.org/10.1002/2017JD026456. [Lazaros Oreopoulos, United States of America]	Taken into account. The Davies et al. paper has been cited where we assess the cloud altitude feedback. Thanks.
129003	63	51	63	54	There is also a study that didn't find a trend in cloud top height for 16 years of the Terra satellite record: Davies et al. (2017), https://doi.org/10.1002/2017JD026456. [Trigg Talley, United States of America]	Taken into account. The Davies et al. paper has been cited where we assess the cloud altitude feedback. Thanks.
116613	63		66		The issue of liquid water in low Arctic clouds is not discussed. [Valerie Masson-Delmotte, France]	Noted. The cloud phase change dominates over the Southern high latitudes where background temperature allows co-existence of ice and liquid in clouds. Since the summer Arctic surface temperature, slightly below zero degree C, prefers liquid phase and therefore the phase change process is not a primary factor of the Arctic cloud feedback.
17975	64	5	64	33	I don't think this perspective is correct. The convective plumes occupy a vanishingly small fraction of the tropics compared to the high clouds in total (Simpson in the 1950's). The high clouds have a neutral effect on the current climate, so reduced the cloud area does not have a strong effect, and might actually be positive if the net effect is slightly negative, which it seems to be Hartmann, D.L., 2016: Tropical Anvil Clouds and Climate Sensitivity Proc. Nat. Acad. Sci. , , doi:10.1073/pnas.1610455113. The dominant mechanism leading to fractional reductions in high cloud area most likely the mass flux effect discussed by Held and Soden (2006) and Knutson and Manabe(1995). If the tropical high cloud feedback is negative, it will most likely be because the ice content increases, but that is also highly uncertain, since climate models do not resolve the physics key to anvil cloud evolution. I would give the assessment here low confidence, not medium confidence. [Dennis Hartmann, United States of America]	Taken into account. We understand your point. The high-cloud amount feedback, if the area shrinks in response to warming, could be both positive and negative depending on what type of clouds (thin cirrus or thick anvil) actually reduces their amount. We have cited more observational estimates for those different cloud feedbacks, and compared their sum with an independent estimate of the net high-cloud amount feedback by Williams and Pierrehumbert (2017). Those estimates agree to each other and support the net negative high-cloud amount feedback. However, model results (GCMs, RCMs, GSRMs) do not show an agreement about the feedback even thou they tend to commonly show an enhanced convective aggregation (and resultant reduction of high cloud area) under warming. This is now better understood based on the stability iris mechanism (Bony et al. 2016) that was built on the Held-Soden arguments. Given the lack of modelling evidence, we have downgraded the confidence level but kept the central estimate being negative.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16191	64	5	64	33	The evidence here for a negative feedback from anvil clouds is overblown in my opinion and conclusions much too confident. Since high clouds in the tropics are associated with little net radiative effect it is not clear what the impact should be even if we know what happens to their area. The Mauritsen and Stevens 2015 study is heavily relied upon here, but its key relevant findings were based on a single GCM (and in particular I don't think the statement at line 28 is supported by this study). While their GCM performs poorly in interannual variability until the authors dial in a strong "iris" effect, other GCMs can do this without such an effect. I.e. this was a really nice paper but needs to be repeated in more models and other hypotheses considered. Some observational studies such as Zhou et al. (10.1002/2014GL062095) find an *increase* in cirrus during warm periods and a positive feedback. This study should be cited at least. The warming-induced convective aggregation noted in the cited small-domain CRM studies does not necessarily imply any feedback, and was found by Tobin et al. 2012 in observations not to correspond to any TOA net radiation change; moreover the warmed-CRM aggregation increase has not proven robust in the latest RCEMIP study (Wing et al., submitted). As far as observations go, interannual variability is anyway a questionable proxy for long-term warming when it comes to convective organisation since this is so sensitive to SST patterns, which vary greatly during small changes in the global mean (thus producing a "pattern effect" like the one discussed with respect to the historical warming record, only bigger!). Because of this ambiguity of evidence, and paucity of mechanistic analysis, I think the confidence in the assertions made here should be low at best, surely not 'medium" as claimed, and I would question whether we even know the sign of the total feedback from upper-level clouds. [Steven Sherwood, Australia]	Taken into account. We understand your point. The high-cloud amount feedback, if the area shrinks in response to warming, could be both positive and negative depending on what type of clouds (thin cirrus or thick anvil) actually reduces their amount. We have cited more observational estimates for those different cloud feedbacks, and compared their sum with an independent estimate of the net high-cloud amount feedback by Williams and Pierrehumbert (2017). Those estimates agree to each other and support the net negative high-cloud amount feedback. However, model results (GCMs, RCMs, GSRMs) do not show an agreement about the feedback even thou they tend to commonly show an enhanced convective aggregation (and resultant reduction of high-cloud area) under warming. Given the lack of modelling evidence, we have downgraded the confidence level but kept the central estimate being negative.
51307	64	5	64	33	While the plausibility of reduction in anvil clouds exterting a negative feedback is noted, is this operating to the extent advocated by the Lindzen study? If not, consider adding some clarifying language to state how the magnitude of the negative feedback proposed in that study can largely be ruled out. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. As stated at l.13-14, the Lindzen's hypothesis (including the magnitude of the feedback) was not supported by a number of studies thereafter. The negative anvil cloud feedback assessed here is based on more recent studies that explain a different mechanism from Lindzen.
129005	64	6	64	33	[CONFIDENCE] Tropical high cloud amount feedback is assessed as medium confidence but, given the lines of evidence offered, downgrade it. Model sensitivity studies, like Mauritsen and Stevens and others quoted, do not provide any convincing evidence of the reality of such feedbacks. [Trigg Talley, United States of America]	accepted. We have now assessed it with low confidence.
65733	64	18	64	22	Suggest clarification of how enhanced convective aggregation supports an iris-effect. [Kushla Munro, Australia]	Taken into account. In the FGD, we have substantially revised this part. The enhanced convective aggregation with surface warming has been assessed as a robust response supported by different lines of evidence, but the radiative feedback is still highly uncertain so the feedback has been assessed with low confidence.
68003	64	18	64	23	One of the largest impacts of convective aggregation is a drying of the clear troposphere and increased radiative cooling. Is this part of the negative feedback? [Robert Pincus, United States of America]	Taken into account. This is an impotent point which is now stated in the text. We think that the feedbacks estimated from GCMs or observations implicitly include this effect given that the drying occurs surrounding the anvil clouds but not over the globe.
93089	64	19	64	23	Should be mentioned: One study seems to indicate that self-aggregation is weaker at higher SSTs than at lower SSTs, in contrast to the findings of many simulations: Xu, K.-M., Yongxiang, H., and Wong, T. (2019). Convective Aggregation and Indices Examined from CERES Cloud Object Data, J. Geophys. Res. Atmospheres, doi: 10.1029/2019JD030816. [Claudia Stubenrauch, France]	Taken into account. We have substantially revised this section. However, we have not cited Xu et al. because their estimate of the aggregation measure dependent on SST has a very large error range and not quite reliable.
93087	64	21	64	21	Stein et al., 2017 instead of 2016 [Claudia Stubenrauch, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
93091	64	23	64	23	Observational studies which may indicate an anvil shrinking using interannual variabilities over 13 yrs find a small decrease of cirrostratus of -0.64+-0.23%/K (Liu et al., 2017; using MODIS data) and -0.76+-0.21%/K (Stubenrauch et al., 2017; using AIRS data); but the latter also find an increase of cirrostratus and thin ci at high altitude (< 330 hPa), relative to all clouds (+1.87+-0.52%/K and 1.70+-0.54%/K, respectively). Liu, R., K.-N. Liou, H. Su, Y. Gu, B. Zhao, J. H. Jiang, and S. C. Liu (2017). High cloud variations with surface temperature from 2002 to 2015: Contributions to atmospheric radiative cooling rate and precipitation changes, J. Geophys. Res. Atmos., 122, 5457–5471,doi:10.1002/2016JD026303. Stubenrauch, C. J., A. G. Feofilov, E.-S.Protopapadaki, and R. Armante (2017). Cloud climatologies from the InfraRed Sounders AIRS and IASI: Strengths and Applications, Atmosph. Chem. Phys., 17, 13625-13644, doi :10.5194/acp-17-13625-2017. [Claudia Stubenrauch, France]	Rejected. We need observational estimates of the total high-cloud amount but not a particular type of cloud besides their response to warming has to be converted to radiation change in order to assess the feedback. Therefore, we could not cite these papers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93093	64	23	64	33	It is difficult to follow how to get a high-cloud amount feedback of -0.15+-0.2 Wm-2K-1, see below [Claudia Stubenrauch, France]	Taken into account. We have substantially revised this section, and clearly explained how we reached the assessed value of -0.15+/-0.2 Wm-2K-1.
93097	64	25	64	25	The LW feedback study uses only 7 years of observations and the LW cloud feedback between observations and models doesn't seem to agree well. [Claudia Stubenrauch, France]	Rejected. It's true that Vaillant de Guélis et al, (2018) used a relatively short observational record. But, their estimate combined with another study by Zhou et al. leads to an estimate of the net high-cloud amount feedback quite consistent with Williams and Pierrehumbert (2017). Modelling evidence is too weak to date because of disagreement among models and experiments, so that this particular feedback has been assessed mainly using observational evidence.
93095	64	26	64	26	'tropical high-cloud regime occupies 7% of globe': this seems to me underestimated: Williams and Pierrehumbert seem to use 30N to 30S for their investigation (see map of supplement); with about 30% high cloud amount within 30N-30S, or 40% within 15N-15S (from satellite observations, e.g. Stubenrauch et al. 2017); tropics (23N-23S) cover 40% of the Earth's surface; just by multiplying 35% x 40% I come very roughly to 14%, which is twice the value given. [Claudia Stubenrauch, France]	Rejected. Williams and Pierrehumbert did not show the climatological area fraction of high clouds (area of deep convection in their Fig. S1 is not necessarily fully overcasted). Using the latest ISCCP climatology we obtain the area fraction of 13.3% by high clouds within the tropics (30S-30N), which leads to about 7% over the globe.
28875	64	27			There is no mechanistic discussion or assessment of the quoted Li et al. modelling study which seems to show a positive IRIS feedback is possible if increased precipitation efficiency with temperature is prescribed with cirrus cloud thinning overwhelming reduced coverage but this is not discussed here https://doi.org/10.1175/JCLI-D-18-0845.1 (Li et al. 2019 J. Clim) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Li et al suggested there is potentially a positive feedback by reducing cirrus ice contents, but they also pointed out the process is highly uncertain. Because of limitation of space we did not discussed the mechanism.
129007	64	29			Add (Su et al., 2017) after (Mauritsen and Stevens, 2015). Su et al. (2017) showed that the GCMs underestimate cloud LW feedback due to the underestimate of anvil cloud reduction with surface warming. [Trigg Talley, United States of America]	accepted
102087	64	30	64	31	According to my understanding, Ohno et al (2019) studied the impact of the vertical resolution on high cloud feedback (with the turbulence scheme). The impact of the turbulent scheme on high cloud feedback was shown in Tsushima et al (2014) (doi: 10.1002/2013MS000301). [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We are asked to cite recent papers but not those published before AR5.
34415	64	32	64	33	It is not clear how the value and uncertainty range was arrived at from the preceeding discussion? Suggest adding information on how this conclusion was reached so the assessment is transparent and reproducable. [Haroon Kheshgi, United States of America]	Taken into account. We have revised the text to increase transparency of the assessment. Thanks.
17349	64	33	64	33	This uncertainty range seems small given the limited amount of studies on the tropical high-cloud amount feedback and many studies didn't apply the high-resolution necessary to resolve the relevant processes for tropical high-cloud evolution (Gasparini et al., 2019). Gasparini B., Blossey, P. N., Hartmann, D. L., Lin, G., & Fan, J. (2019). What drives the life cycle of tropical anvil clouds? Journal of Advances in Modeling Earth Systems, 11, 2586–2605. https://doi.org/10.1029/2019MS001736 [David Neubauer, Switzerland]	Rejected. We are aware of Gasparini et al. study that proposed an interesting process associated with the anvil cloud feedback. But their experiment is highly idealized and premature to incorporate to the feedback assessment here. We have counted methodological uncertainty for assessing the range, but not included uncertainty due to un-assessed processes.
129009	64	36	65	8	[CONFIDENCE] Is this actually meant to refer to subtropical low clouds (as referred to on page 65, line 4)? If not, where are the subtropical low clouds discussed since these have a large effect on the Earth system? There is no real discussion on transition from stratiform low cloud to convective, closed to open cellular, etc., an issue important to ACI. The assessment argues for high confidence on low cloud feedback (Table 7.9) as in previous assessments. Perhaps this is true, but major biases in model representation of low clouds goes completely unnoted like the too few too bright bias of subtropical low clouds. This bias is quite extreme, and has persisted throughout the CMIP series of experiments. Added to this is the fact the models have serious low cloud precipitation biases (all low cloud types) that also is a problem given precipitation affects cloud lifecycle, its water balance and is a fundamental issue in dealing with aerosol effects. Until these major flaws in the representation of low clouds are addressed, high confidence cannot be placed on future projections of their change and feedbacks they induce (e.g., Table 7.9). [Trigg Talley, United States of America]	Taken into account. Yes, this part refers to the subtropical low clouds (we have made it clearer). The level of confidence to the positive low-cloud feedback has been increased since AR5 because we do not rely much on GCMs that are still insufficient to resolve the processes. As we explained in this paragraph, the assessment was based on multiple lines of evidence from cloud controlling factors and process modelling.
51309	64	44	64	49	Slightly unclear as to whether this approach described in this section applies specifically to tropical marine feedbacks or clouds in general? You describe its clear benefits, can it be applied to other cloud feedbacks? If yes, has it? It would be useful to clarify because, as written, it sounds like it is valuable but has been narrowly applied to this specific feedback. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The cloud controlling factors are now discussed for Subtropical marine low-cloud feedback, Mid-latitude cloud amount feedback and the Extratropical cloud optical depth feedback.
102081	64	44	64	52	This description of controlling factors for low clouds and two main dominant factors should be moved to 'marine low cloud feedbacks'. Then you can point 'Tropical marine low-cloud feedback' and 'Middle latitude cloud amount feedback'. [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The title of this paragraph has been changed to 'Subtropical marine low-cloud feedback'

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68005	64	45			This paragraph emphasizes studies that exploit the idea of "cloud controlling factors" to understand feedbacks. (The term is attributable to chapters in the Strüngmann Forum volume "Clouds in the Perturbed Climate System" ISBN 9780262012874). Acknowleging that the term is used in the literature, one wonders whether its use here adds value. The idea is simple enough - cloud properties depend on a few key aspects of their local environement, and because the response is rapid the relationships inferred from inter-seasonal or inter-annual time scales are expected to hold under climate change. [Robert Pincus, United States of America]	Noted. The book is not freely available, so we cited Qu et al. (2014) that first referred to as the term 'cloud controlling factor'. We could have explained the method without this term, but it'd be easier for readers to find other related studies by explicitly referring to this term.
23777	64	46	64	51	The following paper clearly showed that the sensitivity of low cloud cover to SST can be connected to cloud top entrainment. The sensitivity of low cloud cover to SST found in Qu et al. (2014, 2015) is quantitatively attributed to cloud top entrainment index in the paper. Therefore, the paper could be added as a reference after a sentence "a thermodynamic effect due to rising SST that acts to reduce low cloud by enhancing cloud-top entrainment of dry air (P64 L51)" or as one of the references in P64L47. Kawai, H., T. Koshiro, and M. J. Webb, 2017: Interpretation of Factors Controlling Low Cloud Cover and Low Cloud Feedback Using a Unified Predictive Index. J. Climate, 30, 9119-9131. [Hideaki Kawai, Japan]	accepted
64503	64	50	64	50	While I agree with the conclusions of your low cloud feedback assessment, your support seems flimsy to me: basically that regression analysis shows SST and EIS predict current-day low cloud variations. I think it is critical to explain that we understand the physical reasons for these relationships. To my mind, Bretherton and Blossey 2014 (https://doi.org/10.1002/2013MS000250) does that for low cloud sensitivity to SST. Weirdly, even though the idea that EIS controls low cloud fraction is ubiquitous, a physical explanation is not. The only citation for this I can find is in the first paragraph of the conclusions in Caldwell et al 2013 (https://doi.org/10.1175/JCLI-D-12-00188.1). [Peter Caldwell, United States of America]	Taken into account. We stated that the inversion strength increases with rising SST in a warming climate and therefore works to increase the low cloud (l.51-52) But, recent studies cited in the paragraph commonly show that the EIS effect is overcompensated by other factors that act to reduce the low cloud amount.
19411	64	55	65	8	No sure what these "local" feedbacks are -- what is being held fixed? [Isaac Held, United States of America]	Taken into account. The 'local' feedback means the value estimated only over the particular region or regime but not to the global mean surface temperature. To avoid misleading, the sentence has been reworded to 'in-situ'. We converted the in-situ feedback to global feedback by multiplying the area fraction.
17977	65	4	65	5	I would tend to agree that the confidence here is high. The thermodynamic effect seems to win in observational, GCM and CRM simulations. [Dennis Hartmann, United States of America]	Noted. Thanks.
28877	65	7			Has the confidence and magnitude changed since AR5? There seems to be some evidence of more strongly amplifying feedbacks in CMIP6 simulations e.g. Zelinka et al. (2020) https://doi.org/10.1029/2019GL085782 but possibly still more moderate than observations imply for ENSO e.g. Loeb et al. (2020) https://doi.org/10.1029/2019GL086705 that was also the case for CMIP5 e.g. Yuan et al. (2018) https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2018GL077904 though this may be a poor proxy for climate feedback responses e.g. Marvel et al. (2017) http://dx.doi.org/10.1002/2017GL076468 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Regarding the subtropical low clouds, the level of confidence to the feedback was increased since AR5 (cf. Table 9.7). However, this was not directly due to CMIP6 models showing a more positive cloud feedback than CMIP5 models.
68009	65	10			Is there any evidence on land cloud feedbacks that does not rely on climate model simulations? [Robert Pincus, United States of America]	Taken into account. We have downgraded the level of confidence as no other lines of evidence are available.
28879	65	13			I think it should be Section 8.2 (phyiscs) or 8.4 (projections) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	accepted
68007	65	15			Will readers know what a "super-parameterized" model is? [Robert Pincus, United States of America]	Taken into account. The word rephrased.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5647	65	17	65	17	I would like to draw authors' attention that the reduction of low clouds with surface warming is also confirmed by recent high-resolution global simulations (Noda et al. 2014, 2019). I consider that those high-resolution simulation results without using cumulus parameterization results also strengthen the authors' conclusion mentioned in this subsection. References Noda, A.T., C. Kodama, Y. Yamada, M. Satoh, T. Ogura, and T. Ohno, 2019: Responses of clouds and large-scale circulation to global warming evaluated from multi-decadal simulations of a global nonhydrostatic model. J. Adv. Medelling Earth Systems., 11, doi:10.1029/2019MS001658. Noda, A. T., M. Satoh, Y. Yamada, C. Kodama, and T. Seiki, 2014: Responses of tropical and subtropical high-cloud statistics to global warming. J. Climate. 27, 7753-7768. [Akira Noda, Japan]	Rejected. Thanks for the suggestion, but the 14-km simulation does not actually resolve stratocumulus/tradecumulus clouds that are responsible for the feedback.
17979	65	17	65	17	solar insolation again. Should be insolation OR solar radiation. Only one kind of insolation exists - solar. [Dennis Hartmann, United States of America]	Accepted.
5645	65	19	65	23	I would like to draw authors' attention that, different from idealized planet or low-resolution models, stronger convective aggregation is not necessarily assured in high resolution and more realistic simulations, which considers topography and SST distributions. For example, A very recent study using cloud-system-resolving GCM, NICAM, data (Noda et al. 2019) reveals that the tropical convection becomes disorganized with surface warming. Higher resolution models with more realistic planet simulations would be needed to obtain improved conclustion. References Noda, A.T., C. Kodama, Y. Yamada, M. Satoh, T. Ogura, and T. Ohno, 2019: Responses of clouds and large-scale circulation to global warming evaluated from multi-decadal simulations of a global nonhydrostatic model. J. Adv. Medelling Earth Systems., 11, doi:10.1029/2019MS001658. [Akira Noda, Japan]	Not applicable. This paper is out of scope of the feedbacks being discussed here
129011	65	22	65	22	[CONFIDENCE] Why is land cloud rated medium confidence when it seems it is based soley on model results? [Trigg Talley, United States of America]	Taken into account. We have downgraded the level of confidence as no other lines of evidence are available.
51311	65	22	65	23	It would be useful to clarify here the medium confidence statement as this is preceeded by biases in GCMs, which might imply low confidence? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have downgraded the level of confidence as no other lines of evidence are available.
17351	65	22	65	23	Also this uncertainty range seems small given the limited evidence [David Neubauer, Switzerland]	Rejected. We have retained the range as no other sources of information were available.
129013	65	25	65	40	The critical first papers that support what is written in this paragraph were published in 2014. These papers should be cited as they pre-date all of the references that are cited in draft that is currently available: Kay JE, Medeiros B, Hwang Y-T, et al. Processes controlling Southern Ocean shortwave climate feedbacks in CESM. Geophys Res Lett. 2014;41:616-22. doi:10.1002/2013GL058315. Abstract from paper first published in December 2013 says: ""More broadly, these results suggest that thermodynamics (warming and near-surface stability), not poleward jet shifts, control 21st century Southern Ocean shortwave climate feedbacks."" Ceppi P, Zelinka MD, Hartmann DL. The response of the Southern Hemispheric eddy-driven jet to future changes in shortwave radiation in CMIP5. Geophys Res Lett. 2014;41:3244-50. doi:10.1002/2014GL060043. Abstract from paper published in April 2014 says: ""We demonstrate that the ASR changes are the cause, and not the result, of the intermodel differences in jet response by comparing coupled simulations with experiments in which sea surface temperature increases are prescribed. "" [Trigg Talley, United States of America]	Rejected. we are asked to cite recent papers but not those published before AR5, so cited Ceppi and Hartmann (2015) instead of Ceppi et al. (2014) for example. Also, in this paragraph midlatitude (equatorward of 60 degrees) cloud amount feedback was assessed, so Kay et al. (2014) was not quite relevant.
112679	65	33	65	53	In line 39 it is noted that the "The reduction of sulphur emission from shipping lead to a slight warming.." Here the past tense 'lead' is used and slight warming are mentioned with medium evidence, medium agreementent, while the true effect is not yet measured. The referenced paper of Sofiev et al. (2018) uses a very low sulphate load and lifetime compared to other studies. More research is needed to asess the impact of reduced sulphur emission from shipping and currently there is low confidence on the net total ERF. [Leon Simons, Netherlands]	Not applicable. Cannot find the text in question.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65735	65	39	65	40	Suggest shifting the last sentence of this paragraph to become the introductory sentence of the next paragraph. [Kushla Munro, Australia]	accepted
27159	65	40	65	40	The meaning of this sentence is not clear [Eric Brun, France]	Taken into account. Sentences have been rephrased.
41529	65	42	65	42	There is lots of use of observational variability of clouds with SST with the assumption that this carries over to future changes. But we know with models that this is not really the case (present variability is not necessarily correlated with the future, e.g. Dessler 2010, Science). There is no discussion of that. If that is what the bottom up methods in Sherwood et al are based on, I think it is flawed. [Andrew Gettelman, United States of America]	Noted. Even though the observed interannual fluctuations in temperature and cloud (or associated radiation budget) may not be very highly correlated, Studies such as Colman and Hanson (2017) and Zhou et al.(2015) demonstrate that the climate feedbacks in ESMs correspond well between different time scales (interannual and long-term). Sherwood et al. (2020) therefore used the observed interannual variability as one of the lines of evidence, and here we adopted similarly. It will be true that the pattern effect due to different SST pattern changes matters , it will not be large enough to collapse the correspondence.
23779	65	42	65	52	Marine fog is one of the typical low "clouds" over the mid-latitudes. It was found that most CMIP5 models consistently show similar response of marine fog to changes in surface warm air advection accompanied by changes in subtropical highs. There are no descriptions about marine fog in the draft. Therefore, a sentence like the following could be worth adding. "Most CMIP5 model simulations show consistent changes in mid-latitude marine fog that corresponds to changes in surface warm air advection accompanied by changes in subtropical highs (Kawai et al. 2018), although the cloud feedback by marine fog could be small." Kawai, H., T. Koshiro, H. Endo, and O. Arakawa, 2018: Changes in Marine Fog over the North Pacific under Different Climates in CMIP5 Multi-Model Simulations. J. Geophys. Res., 123, 10,911-10,924. (Actually, the radiative feedback of marine fog is discussed in Kawai et al. (2016, ASL, 10.1002/asl.691) and it is found that the cloud feedback by marine fog is not significant. But it is based on only one model.) [Hideaki Kawai, Japan]	Rejected. The purpose of this section is to assess the cloud radiative feedback, but not the cloud response per se. As there is no evidence that the marine fog has a significant radiative impact (as was suggested by Kawai et al. 2016), it was not included in the assessment,
129015	65	49	65	52	[CONFIDENCE] It is not clear if this 50% inflation of the standard deviation the result of some kind of expert judgment process among the author team (or some other group)? [Trigg Talley, United States of America]	Noted. Yes this inflation reflects an expert judgement, and the reason was given in the text.
102089	65	52	65	52	The meaning of 'thermodynamic condition' is not clear. Stability? Entrainment? Phase? [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Noted. It means the stability change controlled mainly by increasing SST, as was explained earlier in the paragraph.
109395	65	54	66	22	The discussion of extratropical cloud optical depth feedback (pp. 65-66) has a few problems in my opinion. Firstly, and most significantly, it ignores thermodynamic and other effects for increase (McCoy et al. 2015, Ceppi et al 2016, and especially McCoy et al 2019 — all of which discuss the role of increased water vapor path in a warmer atmosphere according to the Clausius-Clapeyron relation to increase liquid water path) beyond a brief mention of their possible existence (“other processes” [pg. 66, line 6] while ascribing too much power to the phase-change effect (both in the discussion and Figure 7.14—see my separate comment on Figure 7.14). Second, the discussion oversimplifies and overemphasizes the phase change effect. The phase change effect has been shown to not be a simple conversion of ice to liquid, but rather to involve process efficiencies: “most of the cloud liquid water increase in the middle to high latitudes in global warming experiments results from a decrease in the efficiency of the processes depleting cloud water” (Ceppi et al., 2016). The phase change effect has been shown to only explain part of the extratropical liquid water path increase in GCMs (see Figure 2 in McCoy et al., 2015). Thirdly, and admittedly least significantly,, the discussion seems to misrepresent Tan et al., 2016 as being all about insufficient liquid in models (“Due to insufficient amounts of super-cooled liquid water in the atmosphere mean state…” [pp. 66, lines 7-8]). Rather, that work (as well as others) noted insufficient supercooled liquid fraction (SLF) in CMIP5 models and demonstrated a connection between SLF and climate sensitivity. This connection was assumed to be due to lower SLF meaning there was more ice present in the model to be transformed into liquid in a warmer world. In other words, Tan et al.’s argument seemed to rely on the amount of ice rather than the amount of liquid because it was all about the phase change effect. So, I am confused why it is presented as otherwise in this text. [Michelle Frazer, United States of America]	Taken into account. We have revised the text to increase transparency of the assessment. Actually, we assessed that the phase change, which may dominate though (Tan et al. 2019), is not a single cause of the optical depth feedback as we have stated 'other processes that increase or decrease liquid water path (LWP) may also affect the optical depth feedback (McCoy et al., 2019)'. We cited Tan et al. (2016) to explain a common error in CMIP5 models in terms of the mixed phase cloud, but the paper was not included in the assessment of the feedback per se. The quantitative assessment of the optical depth feedback has been made by combining observational estimates and ESM results, including papers that you suggested. Further discussion of the detail was not possible due to limitation of space.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
1681	65	54	66	22	One thing subsection “Extratropical Cloud Optical Depth Feedback” neglects to address is that changes in temperature can change cloud optical depth even within the same phase, i.e., without a phase change (see Tselioudis et al. 1992 for warm clouds). Tan and Oreopoulos (2019), https://doi.org/10.1029/2018GL081590, looked at the relative strength of cloud optical depth variability with temperature when there is no phase change (i.e., within clouds of ice or liquid phase) and the cloud optical depth change when a phase change is involved, and found that the phase change component is stronger. The bottom line is that cloud optical depth can be affected by temperature changes without a phase change. [Lazaros Oreopoulos, United States of America]	Rejected. We have cited Tan et al. (2019) supporting that the phase change from ice to liquid is the major contributor to the optical depth feedback.
129017	65	54	66	22	One thing the "Extratropical Cloud Optical Depth Feedback" subsection neglects to address is that changes in temperature can change cloud optical depth even within the same phase, i.e., without a phase change (see Tselioudis et al., 1992, for warm clouds). Tan and Oreopoulos (2019, https://doi.org/10.1029/2018GL081590) looked at the relative strength of cloud optical depth variability with temperature when there is no phase change (i.e., within clouds of ice or liquid phase) and the cloud optical depth change when a phase change is involved, and found that the phase change component is stronger. The bottom line is that cloud optical depth can be affected by temperature changes without a phase change. [Trigg Talley, United States of America]	Rejected. We have cited Tan et al. (2019) supporting that the phase change from ice to liquid is the major contributor to the optical depth feedback.
64505	65	54	66	22	You should also mention adiabatic LWP increase with warming as noted in Somerville and Remer 1984 https://doi.org/10.1029/JD089iD06p09668 [Peter Caldwell, United States of America]	Rejected. A more recent paper by McCoy et al 2019 has been cited
129019	65	54	66	22	One important advance that is totally missing from the discussion here is the fact that the Southern Ocean is a region with delayed warming. So, while the Southern Ocean cloud feedbacks are important for equilibrium climate change, they are much less important for transient climate change. Please see the following paper, that is cited in Zelinka et al. (2020) but should be discussed in this section: Frey, W. R., Maroon, E. A., Pendergrass, A. G., and J. E. Kay (2017), Do Southern Ocean cloud feedbacks matter for 21st century warming?, Geophysical Research Letters, DOI:10.1002/2017GL076339. [Trigg Talley, United States of America]	Rejected. In this section we assessed climate feedbacks relevant to the equilibrium temperature response (i.e., ECS), so that discussion about transient climate response does not fit the purpose (it is actually thoroughly discussed in 7.5).
19335	65	54	66	42	The Extratropical cloud optical depth feedback section focuses exclusively on the Southern Ocean, and the Arctic cloud feedback section focuses on longwave feedbacks. As a result of this structure, it reads as though the cloud optical depth feedback is unimportant in the Arctic. Clarification may be needed. [Nicole Feldl, United States of America]	Noted. Over the Arctic where summer temperature is higher than over the Southern Hemisphere polar region, the cloud phase change is not a primary contributor to the cloud feedback, which is driven more by changing amount. We did not clearly separate them in the assessment as the net of the Arctic cloud feedback is assessed very small anyway.
68011	65	55	66	22	The explanation here is less easy to follow than most others in the chapter [Robert Pincus, United States of America]	Taken into account. Text has been revised.
65737	66	1	66	42	Suggest discussing Antarctic cloud feedbacks. These do not appear to be part of the "Land cloud feedback" Section, nor the "Southern Ocean cloud feedback" discussions. [Kushla Munro, Australia]	Rejected. There is little reference that explored possible cloud feedbacks over Antarctica, so we did not assess them.
28881	66	1			Suggest combining sentences e.g. "It has been argued that the cloud optical depth (opacity) will increase over the Southern Ocean (50°–80°S) as warming drives the replacement of ice-dominated clouds with 'brighter' liquid-dominated clouds, thereby resulting in a negative feedback." [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	accepted
102083	66	2	66	5	The introduction of 'phase feedback', the mechanisms (i.e. radius effect because of the difference in ice particles and liquid particles described in P66 L2-5, as well as the life-time effect according to the difference in precipitation efficiency with referencing Tsushima et al (2006) (10.1007/s00382-006-0127-7) and some results (P66.L1-L11) should be moved to 'phase feedback' . [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Not clear what this comment is referring to.
102085	66	2	66	5	The desription of the phase feedback is currently under 'Extra-tropical cloud optical depth feedback', but it is is not quite right, because it is not necessarily only in optical depth but could be found in the amount/frequency. [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The section focuses on Extratropical cloud optical depth feedback and a discussion on amount and frequency changes is not deemed relevant here.
93717	66	3			Suggest citing Ceppi et al. 2016 (doi: 10.1175/JCLI-D-15-0327.1) which demonstrated the importance of the phase change mechanism in GCMs. [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. That paper was cited in the paragraph (a few lines later).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15857	66	6	66	7	Tan et al. (2019) (doi: 10.1029/2018GL081590) also showed that phase changes and liquid processes contribute to extratratropical cloud optical depth. Tan et al. (2019) further decomposed changes in cloud optical depth with temperature and showed that not only do phase shifts and liquid processes matter, but also ice cloud processes, which is not considered in the statement here as it only discusses phase changes and liquid cloud processes. The contributions of phase changes, liquid cloud processes and ice cloud processes are dissected in Tan et al. (2019). Please consider generalizing this statement as reflected by the results in Tan et al. (2019). [Ivy Tan, United States of America]	Taken into account. We have cited Tan et al. (2019) supporting that the phase change from ice to liquid is the major contributor to the optical depth feedback.
102091	66	7	66	7	Compared with the northern Hemisphere extra-tropics, a wider variety of parameters, e.g. those relating to clouds, cloud microphysics and aerosols, contribute to the variance of net CRE feedback in the southern hemisphere extra-tropics, which indicates the additional complexity of the drivers of feedbacks in the Southern Ocean (Tsushima et al 2020: doi: 10.1007/s00382-020-05318-y) [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The suggested paper does not discuss much cloud processes over the Southern Ocean and therefore has not been cited.
31481	66	7	66	11	Please clarfy the name of the satellite used for the phase evaluation. [Maki Kikuchi, Japan]	Rejected. It is a too much detail so readers can refer to the cited reference
129021	66	9	66	9	This text "many CMIP5 models overestimated the negative phase change feedback (Tan et al., 2016)" is incorrect as written. Tan et al. (2016) is based on one model, not "many CMIP5 models". [Trigg Talley, United States of America]	Taken into account. We have added references that support the argument.
41531	66	9	66	10	Another example of where interannual is presumed to be the same for long term feedback, and I do not think this is well founded. [Andrew Gettelman, United States of America]	Noted. Even though the observed interannual fluctuations in temperature and cloud (or associated radiation budget) may not be very highly correlated, Studies such as Colman and Hanson (2017) and Zhou et al.(2015) demonstrate that the climate feedbacks in ESMs correspond well between different time scales (interannual and long-term). Sherwood et al. (2020) therefore used the observed interannual variability as one of the lines of evidence, and here we adopted similarly. It will be true that the pattern effect due to different SST pattern changes matters , it will not be large enough to collapse the correspondence.
19413	66	9	66	16	What do these global cloud feedback numbers mean in the context of a possibly large pattern effect? Is there an implicit assution concerning the patern of warming? The observational studies must effectively be assuming a warming pattern similsr to that obeved over the last few decades. [Isaac Held, United States of America]	Taken into account. As we aggregated estimates of cloud feedbacks for individual regimes, based on different lines of evidence (observation, GCMs, LES etc), it is hard to quantify how much the past pattern effect has influenced the assessed value of the net cloud feedback. However, given that the pattern effect on cloud feedbacks is operated mainly over the tropical oceans via changing low clouds (Zhou et al. 2016), the estimate of marine low cloud feedback would have been less affected by the pattern effect because the main evidence was the LES experiments with increasing local SST for this cloud regime.
129023	66	10	66	11	The papers cited here (Gordon and Klein, Ceppi) are based on passive remote sensing. Yet one of the key advances made since AR5 is the use of active remote sensing observational constraints on cloud phase from space borne lidar (CALIPSO). The CALIPSO observational constraints are critical to show the excessive cloud ice at southern mid-latitudes. The observational constraints from active remote sensing should be discussed here: Kay, J. E., Bourdages, L., Chepfer, H., Miller, N., Morrison, A., Yettella, V.,and B. Eaton (2016), Evaluating and improving cloud phase in the Community Atmosphere Model version 5 using spaceborne lidar observations,Journal of Geophysical Research - Atmospheres, 121:8, 4162-4176, DOI: 10.1002/2015JD024699 Cesana, G., and T. Storelvmo, 2017: Improving climate projections by understanding how cloud phase affects radiation. J. Geophys. Res. Atmos., 122, no. 8, 4594-4599, doi:10.1002/2017JD026927. [Trigg Talley, United States of America]	Taken into account. As Kay et al. (2016) focused primarily on the cloud phase over the Arctic, so the paper has been cited where we assess the Arctic cloud feedback.
129025	66	14	66	14	The draft is missing key papers here. Bodas-Salcedo (2019) is 3 years after the first papers to ""improve"" the cloud phase in climate models. Recommend citing these key first papers as well: I Tan, T Storelvmo, MD Zelinka, Observational constraints on mixed-phase clouds imply higher climate sensitivity Science 352 (6282), 224-227. Kay, J. E., Wall, C., Yettella, V., Medeiros, B. Hannay, C., P. Caldwell, and C. Bitz (2016), Global climate impacts of fixing the Southern Ocean shortwave radiation bias in the Community Earth System Model, J. Climate, 29:12, 4617-4636, doi:10.1175/JCLI-D-15-0358.1 [Trigg Talley, United States of America]	Rejected. Suggested papers are not for CMIP6 models.
17353	66	16	66	17	Also shown by Lohmann and Neubauer (2018) [David Neubauer, Switzerland]	Taken into account. Lohmann and Neubauer (2018) has been cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16193	66	18			Although you claim "low confidence," it sounds like there is actually pretty reasonable understanding here (better than other places where you are claiming higher confidence), it's just that the net result is small. Rather than emphasising your inability to discern the sign of a small number, you could say that you have medium confidence that the net feedback is not very large. If its small we don't care about the sign anyway. [Steven Sherwood, Australia]	Taken into account. We have upgraded the level of confidence.
93685	66	21			"cloud controlling *factors*" [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	accepted
129027	66	30	66	31	When feedback thus far has always been always defined in terms of TOA flux changes, one cannot switch arbitrarily to the surface as a reference point. This is always done for the Arctic but, without a surface feedback parameter being formally defined, it remains a qualitative description. [Trigg Talley, United States of America]	Noted. The feedback is commonly assessed at TOA for all the cloud regimes, but for the Arctic clouds we had to first explain a coupling with surface conditions such as sea ice and the TOA feedback was assessed by transferring the feedback estimated at the surface.
1683	66	30	66	31	When feedback thus far has always been defined in terms of TOA flux changes, one cannot switch arbitrary to the surface as a reference point. This is always done for the Arctic, but without a surface feedback parameter being formally defined, it remains as a qualitative description. [Lazaros Oreopoulos, United States of America]	Noted. The feedback is commonly assessed at TOA for all the cloud regimes, but for the Arctic clouds we had to first explain a coupling with surface conditions such as sea ice and the TOA feedback was assessed by transferring the feedback estimated at the surface.
15859	66	30	66	31	Tan & Storelvmo (2019) (doi: 10.1029/2018GL081871) supports this statement, and further elucidates that the increased cloud fraction may be the result of a feedback associated with phase changes in the Arctic. This result is an extension to the results shown in Tan et al. (2016) (doi:10.1126/science.aad5300) cited in the previous section on the extratropical cloud optical depth feedback, but shows that the impact potentially results in more local Arctic warming due to downwelling longwave radiation. Please consider including the role of thermodynamic phase shifts as described in Tan & Storelvmo (2019) in the Arctic in this section. [Ivy Tan, United States of America]	Taken into account. We could not explain details of the process due to limited space, but has cited the suggested paper here.
129029	66	34	66	34	""and cannot overcome the cloud effect in autumn (Taylor et al., 2015, Morrison et al., 2018)."" What is meant by ""cannot overcome the cloud effect in autumn""? What precisely did Taylor et al. (2015) contributed to the finding anyway? [Trigg Talley, United States of America]	Taken into account. Rephrased. Taylor et al made analyses to A-Train data and contributed to this argument from observational point of view.
129031	66	36	66	36	Change ""Such a seasonality of the cloud response to sea-ice variability is captured by GCMs (La et al., 2016; Yoshimori et al., 2017)"" to ""Such a seasonality of the cloud response to sea-ice variability is captured by some GCMs (La et al., 2016; Yoshimori et al., 2017, Morrison et al. 2019)."" The word ""some"" is important because not all GCMs get this cloud response to sea ice variability correct. For example, Kay et al. (2011) discuss a model with a pathologically incorrect cloud response to sea ice loss because the cloud parameterization was based on assumptions that are correct for low-latitude low clouds but not high latitude Arctic clouds. This work was already mentioned in AR5 so it doesn't need to be included again here. Additional citation: Morrison, A. L., Kay, J. E., Frey, W. R., Chepfer, H. and R. Guzman (2019), Cloud Response to Arctic Sea Ice Loss and Implications for Future Feedbacks in the CESM1 Climate Model, 124 (2), 1003-1020, JGR-Atmospheres, https://doi.org/10.1029/2018JD029142. [Trigg Talley, United States of America]	accepted. Morrison et al. paper had been cited in this paragraph.
129033	66	36	66	42	Based on feedback estimates from short-term climate variations occurring in reanalysis, satellite, and global climate model data sets, Zhang et al. (2018) found that the Arctic cloud feedback strongly depends on the data used for all-sky and clear-sky radiative fluxes at the top of the atmosphere, the historical time periods considered, and the methods used to estimate the cloud feedback. The finding is worth noting here. Reference: Zhang, R., Wang, H., Fu, Q., Pendergrass, A. G., Wang, M., Yang, Y., et al. (2018). Local radiative feedbacks over the Arctic based on observed short,Äêterm climate variations. Geophysical Research Letters, 45, 5761-5770. https://doi.org/10.1029/2018GL077852. [Trigg Talley, United States of America]	Accepted. Thanks for the reference, which has been cited.
129035	66	37	66	38	Does this positive feedback come mostly from LW? What's the competition between SW and LW contributions? [Trigg Talley, United States of America]	Noted. There may be a partial compensation by SW contribution, but the estimate contains uncertainty (e.g. Zhang et al. 2018 GRL https://doi.org/10.1029/2018GL077852),
1685	66	37	66	38	Does this positive feedback come mostly from LW? What's the competition between SW and LW contributions? [Lazaros Oreopoulos, United States of America]	Noted. There may be a partial compensation by SW contribution, but the estimate contains uncertainty (e.g. Zhang et al. 2018 GRL https://doi.org/10.1029/2018GL077852),

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129037	66	45	67	16	The role of clouds has not been done very thoroughly. Is it justifiable to conclude that their effects are more likely to amplify changes? For instance, Trenberth et al. (2015) do relate clouds to radiation and concludes that CESM1 is quite wrong. Reference: Trenberth, K. E., Y. Zhang, and J. T. Fasullo, 2015: Relationships among top-of-atmosphere radiation and atmospheric state variables in observations and CESM. J. Geophys. Res., 120, 10,074-10,090. Doi: 10.1002/2015JD023381. [Trigg Talley, United States of America]	Rejected. Our assessment of the net cloud feedback was based on aggregation of the feedback assessed for individual cloud regimes and was not dependent only on GCMs.
68013	66	49			The assessment of feedbacks made no use of emergent constraints so their mention here is confusing. [Robert Pincus, United States of America]	accepted. The sentence deleted.
129039	66	50	66	50	It should be clarified whether the "high confidence" for positive low-cloud feedback refers to a specific value/range or simply to the sign of the feedback. [Trigg Talley, United States of America]	Accepted. The 'high confidence' is about the sign (cf. p.67 L.9).
22175	66	50	66	53	Surely also, the uncertainties may be correlated with one another? In which case its unclear how to sum the uncertainty components together? [Peter Thorne, Ireland]	Rejected. We indeed assumed that the cloud feedbacks at individual regimes are uncorrelated (p.67 L.11) as there is no strong suggestion that they covary to date.
51313	66	55	67	7	Given the substantive changes in some cloud processes in some CMIP6 models, would the conclusions in this paragraph change if the analysis were performed with CMIP6? While this analysis might not have yet been undertaken with CMIP6, it would be helpful to clarify here if this is a possible area of uncertainty based on these latest models. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have inserted sentences that the updated analyses using CMIP6 models are not yet available, but cannot discuss further.
41533	67	2	67	4	It is not my impression that interannual time scale variability is a good surrogate of the CO2 feedback, but I am not familiar with these studies, only earlier work (Dessler 2010) that contradicts this. Intuitivity the pattern effect of SSTs would argue this is NOT the case, and present day variability for many cloud types in the tropics and subtropics is driven by ENSO, not CO2, and the pattern WILL matter because circulation is affected. I find that logic hard to dismiss. [Andrew Gettelman, United States of America]	Noted. Even though the observed interannual fluctuations in temperature and cloud (or associated radiation budget) may not be very highly correlated, Studies such as Colman and Hanson (2017) and Zhou et al.(2015) demonstrate that the climate feedbacks in ESMs correspond well between different time scales (interannual and long-term). Sherwood et al. (2020) therefore used the observed interannual variability as one of the lines of evidence, and here we adopted similarly. It will be true that the pattern effect due to different SST pattern changes matters , it will not be large enough to collapse the correspondence.
46291	67	3	67	3	Change "a surrogate of" to "an emergent constraint for"? [Twan van Noije, Netherlands]	Rejected. No, The interannual variability of the observed net climate feedback was not used here as an emergent constraint.
65739	67	5	67	7	Suggest including a statement here that as well as this estimate being sensitive to time period, it will also be sensitive to model errors that are present in reanalyses. [Kushla Munro, Australia]	accepted.
129041	67	6	67	7	In the expert opinion of the authors, how many years of observations are needed to derive an observation-based estimate of cloud feedback? [Trigg Talley, United States of America]	Noted. Although we did not state explicitly in the text, we think observational records of at least ~10 years are needed. Otherwise, a particular ENSO event may affect much the estimate of the feedback.
1687	67	6	67	7	In the expert opinion of the authors, how many years of observations are needed to derive an observation-based estimate of cloud feedback? [Lazaros Oreopoulos, United States of America]	Noted. Although we did not state explicitly in the text, we think observational records of at least ~10 years are needed. Otherwise, a particular ENSO event may affect much the estimate of the feedback.
93719	67	6			I'm surprised about the narrow 5-95% uncertainty range for the net cloud feedback estimate from Dessler 2013. Table 1 of the paper gives 2-sigma ranges of 0.69-0.70 depending on the reanalysis dataset. Figure 1 of that paper (bottom left panel) also suggests a large uncertainty in the slope of the relationship. [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thanks for point this out. We have corrected the range from Dessler (0.35->0.7).
689	67	7	67	7	Table 7.9 needs to include the feedback magnitudes and uncertainty ranges discussed in this section [Bruce Wielicki, United States of America]	Rejected. The purpose of the table is to compare the level of confidence between AR5 and AR6, so we did not quote values of the individual feedbacks.
83779	67	9	67	9	feedback sign -> cloud feedback sign [Marvel Kate, United States of America]	accepted.
51315	67	9	67	9	A high confidence statement is used here but this is preceded by statements which highlight that understanding net feedbacks is challenging because many processes are poorly understood (page 66, line 52). It would be helpful to clarify why high confidence is used here (and an increase in confidence since AR5), despite there still being a poor process understanding. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the text to make clear why we can now assess the net cloud feedback with high confidence.
34409	67	9	67	13	The assumption of independence of uncertainty, and the associated disaggregation of feedback by cloud type needs further justification to support the assessment conclusion on ECS. A consequence of this assumption is that tropical high and marine clouds dominate the assessed uncertainty in cloud feedback. The lack of a basis for assuming independence is a gap in the assessment of ECS uncertainty that should be filled. [Haroon Kheshgi, United States of America]	Rejected. The assumption of independence matters for the range but not the central value. There is no literature supporting that there is a co-dependence among individual cloud feedbacks to date, we assumingly ignored it. The text has been revised to clarify our approach.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129043	67	9	67	16	Here the reader should be reminded how the overall estimate in Table 7.10 is derived. Earlier (page 7-63, lines 34-37) a reference of Sherwood et al. has been given about the methodology ("weighting by the ratio of fractional coverage"), but not all cloud feedback subsections provide info on fractional coverages (and those that do do not provide relevant citations ... is all the info in Sherwood et al.?). [Trigg Talley, United States of America]	Taken into account. The global contributions of individual cloud feedbacks (after fractional coverage was multiplied to the local feedback value) were noted earlier in each cloud regime, and the synthesis explains simply that they were summed (in quadrature for the range). The text has been revised.
1689	67	9	67	16	Here the reader should be reminded how the overall estimate in Table 7.10 is derived. Earlier (p. 7-63, lines 34-37) a reference of Sherwood et al. has been given about the methodology ("weighting by the ratio of fractional coverage"), but not all cloud feedback subsections provide info on fractional coverages (and those that do do not provide relevant citations ¬ is all the info in Sherwood et al.?) [Lazaros Oreopoulos, United States of America]	Taken into account. The global contributions of individual cloud feedbacks (after fractional coverage was multiplied to the local feedback value) were noted earlier in each cloud regime, and the synthesis explains simply that they were summed (in quadrature for the range). The text has been revised.
22177	67	10	67	10	But you have just said 0.54 in the last paragraph? [Peter Thorne, Ireland]	Taken into account. We did not use this value directly but compared it with our aggregated assessment. We have changed order of the text to avoid confusion.
15987	67	10	67	11	The paragraph states: " By assuming that uncertainty of individual cloud feedbacks is independent of each other" It is highly unlikely that the individual cloud feedbacks will be independent of each other, and in reality, there will be a matrix of correlation co-efficients, for which any discussion is lacking. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The assumption of independence matters for the range but not the central value. There is no literature supporting that there is a co-dependence among individual cloud feedbacks to date, we assumingly ignored it.
22179	67	10	67	12	I'm not convinced that independence is a valid assumption here? Many of these cloud components are either overlapping or linked by dynamics which may affect via colinearities the best guesses and / or ranges. Is there literature that can support this assumption? If so it should be cited here I think. [Peter Thorne, Ireland]	Rejected. The assumption of independence matters for the range but not the central value. There is no literature supporting that there is a co-dependence among individual cloud feedbacks to date, we assumingly ignored it.
17981	67	15	67	15	Tropical high cloud area feedback would depend upon what type of high cloud decreases with warming. To be a negative feedback one would have to decrease the thin cloud that has a positive CRE preferentially to the thicker cloud that has a negative CRE. A general reduction in average convective cloud area would give little or no feedback, or even a positive feedback if the average effect is slightly negative. [Dennis Hartmann, United States of America]	Taken into account. We have assessed both thin (cirrus) and thick (anvil) cloud area feedbacks based on separate estimates, which have also been compared with an independent estimate of the total high-cloud area feedback.
17983	67	15	67	15	Why only over the Southern Ocean? Would not thickening of midlatitude clouds in the summer over the Northern Oceans also produce a negative feedback? [Dennis Hartmann, United States of America]	Taken into account. Rephrased.
27161	67	21	67	21	Please chnage "difference" with "different" [Eric Brun, France]	Accepted. Text reworded
84849	67	21	67	21	Table 7.9 should have numerical values [Jayaraman Srinivasan, India]	Rejected. The purpose of this Table is to compare the sign and assessed confidence level of individual cloud feedbacks between AR5 and AR6. Because AR5 did not provide values for most of them, we could not make quantitative comparisons. Although values are not added, we have e slightly increased the discussion
129045	67	23	67	24	In Table 7.9, it would be helpful to add the values of each cloud feedback. [Trigg Talley, United States of America]	Rejected. The purpose of this Table is to compare the sign and assessed confidence level of individual cloud feedbacks between AR5 and AR6. Because AR5 did not provide values for most of them, we could not make quantitative comparisons. Although values are not added, we have slightly increased the discussion
18635	67	28	67	40	The next two paragraphs discuss the biochemical and biophyiscal feedbacks respectively. It may be good to define in this paragraph both biochemical and biophysical feedbacks first by stating that apart from physical climate feedbacks there are other feedbacks related to changes in biogeochemical cycles (C, N, S, P, etc..). The feedbacks associated with biogeochemical cycle changes can be divided into biochemcial (changes to chemical composition) and biophysical feedbacks. [Govindasamy Bala, India]	Taken into account, some rewriting has occurred
78579	67	28			nice section and good that you have coordinated with section 6.3.6. A missing link is that vegetation changes will affect dust production. 6.3.6 assess climate-dust, and here you assess climate-vegetation. What about climate-vegetation-dust? Andrews et al (2012, GRL, 10.1029/2012GL051942) show it can affect climate sensitivity in HadGEM2-ES. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This is a single model study so the evidence is not sufficient to include this feedback

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
78581	67	28			In general many studies looking at the impact of land-cover changes on climate are motivated by human land-use rather than internal feedbacks of the Earth system, but their results can still be instructive as to the sensitivity of climate to changes in vegetation. Lena Boysen is leading a study comparing CMIP6 ESMs doing the idealised deforestation simulations from LUMIP. This will help identify more robust climate signals due to changes in tree cover. Winckler et al is a nice study too in this realm (https://www.earth-syst-dynam.net/10/473/2019/). [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted
105617	67	30	67	35	Tsushima et al 2020 (doi: 10.1007/s00382-020-05318-y) conducted an analysis of PPE (from a single model) to identify leading processes driving the spread of the present-day cloud radiative effects and the feedbacks across the ensemble. They found that the variance of the amount of low clouds amounts over deep convective land regions in Congo and Brazil are led by two vegetation processes controlling stomata on evapotranspiration and second by surface photosynthesis. Since this result links biophysical feedback and physical feedbacks, I wonder this can be mentioned somewhere in this section. [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Noted, this is too technical here
90557	67	30	68	17	Again, concerning the surface albedo feedback, you may consider the emerging observational analysis of the "Spatially varying Signatures of Surface Albedo Feedback on the Northern Hemisphere Land Warming" we just obtained in Alessandri et al (2020; submitted in ERL; submitted draft available for sharing). By employing an original methodology and quantitative approach, this work provides understanding of the highly variable – and sometimes competing – governing mechanisms related to the dominance of snow and/or vegetation coverage. Citation: A. Alessandri, F. Catalano, M. De Felice, B. van den Hurk and G. Balsamo, 2020: Spatially varying Signatures of Surface Albedo Feedback on the Northern Hemisphere Land Warming, Submitted to Environmental Research Letters. [Andrea Alessandri, Italy]	Noted, this is too technical here
28883	67	39			Although carbon cycle feedbacks are assessed in Chapter 5 it would be useful to provide an estimate in terms of effects on ECS for comparison with the concentration driven feedbacks. It could also be mentioned that greenhouse gas feedbacks are instrumental in explaining the magnitude of climate change over glacial cycles. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted, taken into account with some rewording
18641	67	41	67	42	It may be a good to discuss the permafrost melt and the associated increase in CO2 here in a sentence or so. The reduced uptake of carbon by land and ocean due to climate change could be also discussed. This will help the readers to apprciate the biochemcial effect of climate change. [Govindasamy Bala, India]	Noted, this is covered in Chapter 5
83133	67	44	68	3	Note that methane and N2O feedbacks are not treated in section 6.3.6, but in 5.4.7. Section 5.4.7 also includes a quantiative estimate of these feedbacks. This should be included in the text with a cross-reference. [Terje Berntsen, Norway]	Noted, Chapter 6 now includes details
116615	67		68		Please also refer to the assessment of SRCCL biophysical feedbacks (as a starting point?). For past climate, what about vegetation feedbacks during past warm phases, especially at high latitude (interglacial periods etc)? Would there be a possibility to provide a perspective of the coupling between energy, carbon and water fluxes involved in this context (with Ch5 and ch 8)? [Valerie Masson-Delmotte, France]	Accepted. We now reference the SRCCL in the initial paragraph, and we later link to paleo vegetation feedback, in the context of the Pliocene, which is CO2 forced, and therefore more appropriate than the interglacial
32093	68	1			Section 6.3.6. doesn't really say much and refers back to 5.4. Maybe here better to cite Table 6.5? [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Noted, Chapter 6 now includes details
64349	68	6	68	7	There is also biophysical feedback/effect associated with physiological effect of CO2. Under higher CO2 concentration, stomatal conductance decreases, plants transpire less water per unit of leaf area. The effect on climate is less latent heat flux, higher surface temperature, and increased runoff. It is supported by observations in FACE experiments. This feedback is already included into ESMs, so to quantify its forcing one need to do an extra run without feedback, eg 4xCO2. The forcing is model-dependent and small, but it is positive as it amplifies the surface warming. We discussed the feedback with Dan Lunt, Ch7 LA, and he thinks as it is CO2-induced it fits rather into Ch 5. However, Ch 5 takes care only about the carbon effect of CO2 fertilization, not the biophysical effect whch falls into a crack between chapters. I see it naturally metioned in the biophysical section in the Ch 7 (this para). Please consider. [Victor Brovkin, Germany]	Taken into account - changes in physiology (e.g. stomatal conductance) in response to CO2 change are included in the radiative forcing, see Section 7.3.2.1. This is now made more explicit in the text.
46293	68	6	68	7	On page 29, line 14 changes in dust were also mentioned as a biophysical feedback. To the extent these are related to vegetation changes, these can indeed be called biophysical. It would be instructive to mentioned them here as well. [Twan van Noije, Netherlands]	Taken into account. Changed Section 7.3.2.1 so that we use the language "biogeochemical/physiological" rather than "biophysical" for dust ERF.
38485	68	6	68	17	Vegetation change could also alter momentum flux by chaning roughness lenghth. This point is missing. [LONG CAO, China]	Accepted - text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23313	68	6	68	39	There are many important new findings for biophycial feedbacks after the AR5, but have not been reported in this new Assessment. For example, the biophysical feedbacks of vegegation change on terrestrial water cycle and climate change: 1. Zeng, Z., et al. (2017). "Climate mitigation from vegetation biophysical feedbacks during the past three decades." Nature Climate Change 7: 432–436; 2. Zhenzhong, Z., et al. (2018). "Global terrestrial stilling: does Earth's greening play a role?" Environmental Research Letter; 3. Zeng, Z., et al. (2018). "Impact of Earth greening on the terrestrial water cycle." Journal of Climate; 4. Zeng, Z., et al. (2018). "Response of terrestrial evapotranspiration to Earth's greening." Current Opinion in Environmental Sustainability 33: 9-25; 5. Zeng, Z., et al. (2016). "Responses of land evapotranspiration to Earth's greening in CMIP5 Earth System Models." Environmental Research Letters 11(10): 104006. [Zhenzhong Zeng, China]	Rejected - We looked through all these papers. They primarily consider the response of climate to changes in LAI ("global greening") over the observations record, from satellites and models. This work is important, but we don't think it's relevant to this section, which concerns the response of vegetation to temperature change, and the resulting radiative response. We are primarily concerned with assessing an alpha, in units of Wm-2K-1. in our concentration-driven framework, and given our definition of ERF, the processes discussed in these papers are largely forcings, not feedbacks, as they are a direct response to the CO2 forcing itself (via LAI change) rather than a response to temperature change.
46295	68	7	68	7	Please change "induced by climate" to "induced by surface air temperature changes". [Twan van Noije, Netherlands]	Accepted - text revised.
64351	68	7	68	9	Actually, biophysical effects of changing albedo and latent heat due to vegetation change are always faster than biogeochemical effects. This is clearly seen in afforestation/reforestation model experiments: it takes decades to centuries to gain carbon in biomass and soils, while physics is controlled by GPP and leaf area which are changing quickly. It just takes long time for plant succession to induce changes in vegetation cover, but if one ignores timescale of vegetation dynamics - biophysical effects are faster than biogoechemical ones. whether CO2 or non-CO2 ones. The LUMIP community would find this statement as it is confusing, this is against the findings in CMIP6 experiments where most ESMs ignore vegetation dynamics anyway but there are biophysical/biogeochemical consequences of land use changes. [Victor Brovkin, Germany]	Accepted - text revised to remove comparison with biogeochemical feedback timescales.
22181	68	9	68	10	It is a combination of temperature and precipitation changes and not temperature changes alone. [Peter Thorne, Ireland]	Accepted - text revised.
46297	68	12	68	14	The time scale of decades to centuries is already mentioned in lines 7 to 9. These two sentences should be merged, or put together. [Twan van Noije, Netherlands]	Accepted - text revised.
64353	68	12	68	15	Here, time scale of veg dynamics (eg norward movement of boreal forest) is mixed up with time scale of carbon dynamics. Veg dynamics depends much on the climate change rate, if it is fast, plants could occupy new area or get extint by distirbance like fire quickly. Paleo-examples are useful here, but a rate of orbital forcing change in the past is much slower that the current rate of woody enchroachment in high latitudes and subtropical drylands. [Victor Brovkin, Germany]	Noted - no text changes required.
18637	68	12	68	24	The dynamic changes in vegetation could be discussed here by citing this paper: Bala, G., K. Caldeira, A. Mirin, M. Wickett, C. Delire, 2005: Biophysical effects of CO2-fertilization on global climate, Tellus B, doi:10.1111/j.1600-0889.2006.00210.x [Govindasamy Bala, India]	Rejected - the physiological response to CO2 is considered part of the forcing. Cao et al and others are cited for this in Section 7.3.2.1.
13523	68	17	68	18	Add line spacing between paragraphs. [Maria Amparo Martinez Arroyo, Mexico]	Taken into account - combined the paragraphs.
64355	68	24	68	24	warming should be specified as surface warming. Less latent heat may rather cool the atmosperic column. [Victor Brovkin, Germany]	Accepted - text revised.
78583	68	26			Falloon et al (2012, Biogeosciences, www.biogeosciences.net/9/4739/2012/bg-9-4739-2012.html) also showed a strong feedback in high latitudes as vegetation changes [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - This sentence is about post-AR5 studies. More importantly, as far as we can tell that study does not isolate purely the biophysical response because it includes a dynamic carbon cycle and so diagnoses rather than prescribes CO2 changes.
78585	68	26			many climate-vegetation feedbacks are via the water cycle – e.g Betts (2004, TAC) show strong changes in rainfall over the Amazon which reinforce vegetation dieback. These are hard to capture in global temperature metrics, but are important for local climate and ecosystems. [Chris Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - see earlier in this section regarding altering radiative fluxes directly via albedo or water vapour changes, or indirectly via surface momentum flux changes"
38487	68	28	68	28	This section discusses biophysical feedback over land. Over ocean, phytoplankton could also have biophysical feedback through the effect on sunlight absorption. This point should also be discussed. [LONG CAO, China]	Accepted - text revised.
71077	68	33	68	35	The likely positive (medium confidence) assessment despite insufficient evidence to assess the likely range sounds strange to me. Just assessing "positive with medium confidence" would be more straightforward. [Yu Kosaka, Japan]	Accepted - text revised.
105771	68	37	68	39	This statement about the "green Sahara" should cross reference to Chapter 8, p105, where it is also mentioned [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account, text reworded
22183	68	41	68	41	Which processes described above? Also, it may not be above in the final lay out so previously would be a safer term here surely? [Peter Thorne, Ireland]	Accepted - text revised.
18639	68	44	68	44	Should "model" be changed "simulations" for accuracy [Govindasamy Bala, India]	Accepted - text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64357	68	44	68	46	I am not sure whether comparison of models of different complexity and resolution could inform on the strength of biospheric feedbacks; it could be a comparison of apples and oranges. Most of literature says that the biophysical effect of boreal forest dynamics (taiga-tundra feedback) is positive due to snow-masking effect. This palys a role in paleo, eg, in glacial inception. Of course, this might be reversed in case of strong warming as there is less snow cover, and in summer trees transpire more than grasses so they can rather cool the surface air. In any case, I agree that biophysical feedbacks are small, and its sign could be season-dependent. [Victor Brovkin, Germany]	Noted - no text changes required.
46299	68	46	68	46	Please change "all" to "the same set of". [Twan van Noije, Netherlands]	Taken into account - sentence re-written.
46301	68	51	68	52	Isn't this a very bad assumption? Please clarify. [Twan van Noije, Netherlands]	Rejected - given that the atmospheric component and radiation code are identical, this is probably a reasonable assumption.
46303	69	3	69	5	How can this range be so small, given the limited representation of biophysical and biogeochemical feedbacks in models (e.g. related to natural sources of methane, sea spray, and mineral dust, and aerosol/precursor emissions from fires and vegetation)? [Twan van Noije, Netherlands]	Taken into account. We have revised our assessment the non-CO2 biogeochemical feedbacks, which includes the feedbacks mentioned by the reviewers, in light of revised assessments in Chapters 5 and 6.
46551	69	11	69	12	This sentence would be more clear if the end was changed from "..., which are particularly important for the West Antarctic ice sheet." to "... , with the latter being particularly important for the West Antarctic ice sheet." As currently written, I don't think there's enough of a distinction to make it clear that w.r.t. dynamics, you are talking mainly about West Antarctica (assuming that was the intention here). [Stephen Price, United States of America]	Accepted - text revised.
77755	69	12	69	12	I would delete "which are particularly ...". I don't think it's right. Dynamics and SMB together determine the evolution and steady state of all land ice masses. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - conflict with Review ID 46551.
69611	69	12	69	12	west Antarctic ice sheet' should all be capitalised; it is a proper noun [Nicholas Golledge, New Zealand]	Accepted - text revised.
77757	69	12	69	14	These two sentences seem unnecessary to me, and the second in particular is not quite relevant. I would say that you could just put refs to chap 9 for both SMB and dynamics in the previous sentence. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised. Combined the two sentences and removed some less-relevant material.
46553	69	14	69	15	"The dynamic ice flows of the Antarctic ice shelves are observed to be accelerating and there are known mechanisms of ice sheet instability that depend on ocean temperatures and basal melt rates." Suggest changing to: "Grounded ice flow in Antarctica is accelerating as a result of reduced ''buttressing'' from floating ice shelves, which are thinning due to increased submarine melting in response to changing ice-ocean interactions." (or something like this). [Stephen Price, United States of America]	Rejected - the point we are making here is that there is a temperature dependence of the feedbacks, and as such the feedbacks are relevant for discussion in this section.
46555	69	17	69	18	Suggest: " … and they influence global ocean circulation through freshwater inputs from iceberg calving and surface and submarine melting." Since this chapter is about feedbacks, you could also mention that clearly there's the opportunity for feedbacks here -- e.g., increased freshwater inputs from ice shelf melting can stratify the S. Ocean, further trapping heat at depth, with the possibility of even further increases in submarine melting as a result, e.g. Jeong et al., 2020 (H. Jeong et al., Impacts of ice-shelf melting on water mass transformation in the Southern Ocean from E3SM simulations, J. Climate, doi: 10.1175/JCLI-D-19-0683.1). [Stephen Price, United States of America]	Rejected - this chapter/section is focussed on temperature feedbacks, not ice volume feedbacks which are assessed in Chapter 9.
77767	69	18	69	18	There are also regional and perhaps global climate effects from the change in land area due to sea level change [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised - reference to Abe-Ouchi et al added.
51317	69	18	69	20	When mentioning the ice sheet - volcano link, it would be helpful to contextualise this by noting details from the paper that "We calculate an apparent time lag of ~600 yr between the climate event and change in eruption frequency" [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - text on volcanoes removed because the released CO2 is not a feedback in our concentration-driven framework.
103619	69	18	69	20	This is highly speculative, no consensus [Philippe Tulkens, Belgium]	Accepted - text revised - This mechanism was added in response to a review comment in the FOD, but have now removed this.
71965	69	19			Here and elsewhere, it is more appropriate to refer to loss of mass of ice sheets. In Antarctica especially, melting is not the main mechanism of mass loss. [John Church, Australia]	Accepted - text revised.
69613	69	20	69	21	timescale of response...thousands of years' - this is meaningless, because 'response' is not defined. Ice sheet volume responds instantaneously to melt, it is only the dynamic response that is lagged. This can initiate instanteously with respect to an applied forcing, but can then persist for many millennia. If it is this aspect that is meant here, then it should be referred to as the 'equilibrium response'. For Antarctica, this is unlikely to ever be reached, so we tend to refer to 'long-term commitment' of mass loss (especially in a future projections context - see eg Golledge et al 2015, 2019, Nature or Golledge 2020 WIREs). [Nicholas Golledge, New Zealand]	Accepted - text revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77759	69	22	69	22	“fully coupled climate-ice sheet simulations with full complexity models” could be simplified to e.g. “coupled climate-ice-sheet models” - it’s not clear what information “full” and “fully” add [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
77761	69	23	69	23	“and/or are asynchronously coupled” is rather technical. Since it’s a kind of simplification, and thus covered by the previous phrase, you could omit it. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - We think it's important to highlight the origin of some of the uncertainties.
77763	69	25	69	25	I would say “lose mass” rather than “melt”, since melting goes on even in a steady state. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
77765	69	25	69	31	I suppose that chapter 9 has a more up-to-date assessment and thus supersedes AR5 and SROCC. If so this para could be replaced by a shorter summary of and refs to ch 9. If you quote AR5, there’s more in the sea level chapter (13), and if you quote SROCC, I suppose the citation should be to the sea level chapter. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - the purpose of this sentence is to summarise state-of-knowledge prior to AR6.
51319	69	37	69	40	In the Golledge et al paper these freshwater fluxes lead to warming at high arctic latitudes. It would be useful here to clarify the implications of these findings on ice sheets and temperature feedback effects in this region. If there is significant regional heterogeneity it would be helpful to explain this here too. Additionally, the Golledge paper is based on RCP8.5 - is the magnitude or the sign of the feedback potentially contingent on the forcing scenario used? Could this also please be clarified. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - The Golledge paper shows that at the surface, the freshwater fluxes lead in most regions to a cooling, not a warming, by 2100 (their Figure 4a,b), and that as a global mean: "These anomalies, as well as our predicted reduction of approximately 0.3 °C in the increase in global-mean air temperature by 2100". Added "surface" to the text to clarify that we are discussing the surface cooling, not the subsurface cooling. Because we are concerned primarily with the global mean response here, a detailed discussion of the regional affects is not warranted, but see Section 7.4.4 where we state: "GCM simulations indicate that large freshwater input to the Southern Ocean from melting ice shelves could substantially delay the emergence of polar amplified warming by stratifying and cooling the surface ocean around Antarctica (Bronselaer et al., 2018; Golledge et al., 2019) (low confidence due to medium agreement but limited evidence). ". Whether or not the signals are dependent on scenario is challenging to assess in the absence of additional studies.
22185	69	44	69	44	Reference should be to cross-chapter box 2.4 here [Peter Thorne, Ireland]	Rejected - cross-chapter box 2.1 actually defines the term MPWP used here.
77769	69	46	69	47	For “feedback parameter, αx, associated with ice sheets” I suggest “feedback on global temperature change due to ice sheets” because I don’t think you need to give it a symbol since it’s never quantified, and because not mentioning a “parameter” avoids problems with the sign convention of the climate feedback parameter [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this language and symbol is used throughout this section and chapter, so we think it is appropriate to use it here.
77771	69	48	69	49	I suggest deleting “(or completely melt) and freshwater fluxes reduce (or stop)” because (a) complete mass loss (not “melt”, as in line 25) is a special case of equilibrium, and (b) the freshwater flux into the ocean (liquid or solid) is not the whole mass budget, which also involves accumulation (in a new eqm, if accumulation is larger, freshwater flux into the ocean will be larger too, and the freshwater flux into the ocean will never stop unless the ice sheet has vanished completely). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
77773	69	49	69	49	Delete “parameter” (see comment on lines 46-47) [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this is the language used through this chapter and section.
77775	69	54	69	55	Please could you state the definition of the “long-term Earth sensitivity” which is “up to two times greater than ECS”. Does this mean the warming for 2xCO2 concentration, where ice sheets and vegetation are allowed to reach a new equilibrium, perhaps? If so it’s only a partial sensitivity because maintaining a CO2 concentration implies that the C feedbacks are excluded. On the other hand, if the C cycle is included, it doesn’t make sense to consider a fixed-eqm CO2 concentration, and it can’t be compared with ECS. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - Reworded much of this section to make it clearer and avoid the use of ESS.
112409	69	55	69	55	To my knowledge, the long-term Earth sensitivity is up to two times of ECS not two times greater than ECS. [Feng Ran, United States of America]	Rejected - we think they mean the same thing! In AR5, there are two quotes with slightly different wording that mean the same thing: "Global mean temperature estimates for these three past climates also imply an Earth system climate sensitivity to radiative perturbations up to two times higher than the equilibrium climate sensitivity (Lunt et al., 2010; Haywood et al., 2013)" and also "The limited number of models for MPWP, which take into account slow feedbacks such as ice sheets and the carbon cycle, imply with medium confidence that Earth-system sensitivity may be up to two times the model equilibrium climate sensitivity (ECS) (Lunt et al., 2010; Pagani et al., 2010; Haywood et al., 2013).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77777	69	55	70	1	I think it would be clearer to say e.g. "a positive net feedback on temperature from biophysical and ice sheet changes"; there's no need for a symbol or mention of a parameter (see comment on p69 lines 46-47). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - this language and symbol is used throughout this section and chapter, so we think it is appropriate to use it here.
116617	69		69		Please check consistency of the ice sheet statements with chapter 9 (maybe use chapter 9 as a starting point). [Valerie Masson-Delmotte, France]	Accepted - this section has been reviewed by an LA of Chapter 9 for consistency (Nick Golledge).
77779	70	3	70	5	I'm confused by this. You have just said that including biophysical and ice-sheet feedbacks (I think) doubles the sensiitvity (wrt ECS). Now you say that if non-CO2 biogeochemical feedbacks are included as well the sensitivity is halved (presumably wrt ECS). Does that mean a large negative feedback from non-CO2 biogeochemical feedbacks? Why is that? Or are these two statements intended to be demonstrably inconsistent, representing a divergence of opinion in the literature? Or is the "net feedback parameter" the reciprocal of sensitivity, in which case 2x sensitivity and ½ x feedback are consistent? I would say once again that mentioning a feedback parameter is likely to be confusing (and it's not necessary to give it a symbol). [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This section has been re-worded to avoid the term ESS, and sticking with feedback parameters. Although we appreciate that feedback parameters are not always as intuitive as climate sensitivities when it comes to "increases" and "decreases", it is a chapter-wide decision that this section uses feedback parameters throughout.
112411	70	5	70	5	Need to specify whatkind of geological time frame this assessment applies to. [Feng Ran, United States of America]	Not applicable - this paragraph removed due to space limitations.
17355	70	10	70	16	This paragraph is unclear. Please rewrite it. The medium confidence refers to AR5, right? [David Neubauer, Switzerland]	Taken into account, text reworded
46305	70	10	70	16	Please also mention our limited understanding of biophysical and non-CO2 biogeochemical feedbacks. [Twan van Noije, Netherlands]	Taken into account. A new section (7.4.2.5.3) Synthesis of biogeophysical and non-CO 2 biogeochemical feedbacks has been added
83781	70	12	70	16	I'm confused by the difference between the net cloud feedback (high confidence) and the cloud feedback (medium confidence, but not included in Table 7.10). Clarification? [Marvel Kate, United States of America]	Taken into account, text harmonised
2709	70	12			the confidence assigned to cloud feebacks in Table 7.10 is high, not medium as stated in the text. [Bryan Weare, United States of America]	Taken into account, text harmonised
23919	70	14	70	16	The sentence '...as the net cloud feedback is assessed positive with high confidence, the total..' is unclear. On the one side, it was said that total cloud feedback amplifies global climate warming. But now it is said that the total climate feedback is negative. An easier ending formulation is needed. [Branko Grisogono, Croatia]	Taken into account, text harmonised
18633	70	25	70	25	Yoshimori et al. (2020, listed as Yoshimori et al. 2019 in the reference) pointed out that the LW high-cloud altitude feedback (under fixed anvil temperature mechanism, in particular) discussed on page 63 has negative covariance with temperature feedback, and thus its contribution to net climate feedback parameter is small if any. This understanding can be included here (p.70) which may lead to a more balanced view than only stating "a positive (cloud) feedback" on page 63 in terms of the net/total feedback. [Masakazu Yoshimori, Japan]	Taken into account, text reworded and reference corrected
129047	70	29	70	30	Upon what evidence can it be claimed feedback co-variability can be neglected? The few references in this paragraph are hardly comprehensive. It would be better to acknowledge the possible (even likely) covariance of feedbacks; but, as there hasn't been enough study of them at this time, that these are neither considered nor quantified in this assessment. [Trigg Talley, United States of America]	Taken into account. The co-dependency have implications on the ECS, whose assessment is made Sect 7.5.5. Text has been added.
114613	70	31	70	31	This is an importnat reminder , but I wonder if you could say something more about the implciations of this. [Jan Fuglestvedt, Norway]	Taken into account. The co-dependency have implications on the ECS, whose assessment is made Sect 7.5.5. Text has been added.
46307	70	36			Table 7.10: It is unclear what the ranges given for non-CO2 biogeochemical feedbacks are based on. In the text, the assessed very likely range of -0.1 to 0.1 W/m2/K also includes biophysical feedbacks, which have their own uncertainties. [Twan van Noije, Netherlands]	Taken into account. A new section (7.4.2.5.3) Synthesis of biogeophysical and non-CO 2 biogeochemical feedbacks has been added
19415	70	41	70	41	You might as well drop the Rh based feedback analysis if you are not including it in Table 7.10 [Isaac Held, United States of America]	Taken into account. The added value of the RH based feedback analysis is certain, and various studies use it, therefore it has been presented. The differences and the link between the two WV feedback analysis have been clarified.
22187	70	41	71	1	I'm not sure what high confidence in a value greater than 0 in the biophysial and long-term ice sheet feedbacks on the millenial scale really means to a policy maker. There may well not be a better way to assess this but the final assessment feels odd for these. [Peter Thorne, Ireland]	Taken into account. The values in the table have been retained, but the text has been reworded.
13525	70		71		Check table format (Table 7.20). Add lines that divide the Table into columns and cells for easy Reading. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - Table format revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64519	71	5	71	5	"Dependence of feedbacks on climate mean state" could refer just as well to feedback sensitivity to SST pattern as to sensitivity to the magnitude of global-average warming. Since SST pattern sensitivity is left for a different section, I suggest changing the wording for this title. [Peter Caldwell, United States of America]	Accepted - tried to make this clearer
102183	71	5			General for this section: I find some logic using "temperature dependence of feedbacks" (as in Rohrschneider 2019, which is still missing from the references) instead of state-dependence. The climate state is more than global mean temperature, e.g. ice sheets or continental configurations. [Maria Rugenstein, Germany]	Accepted - tried to be clear when discussing general state dependence of feedbacks and when discussing temperature-dependence of feedbacks. Also added reference.
28885	71	5			It may be useful to policymakers to state what approximate time-scale is relevant here: is it multi-century? This section is presumably not covering the effect of climate model present day biases on feedbacks which I guess are small in comparison to what is being discussed here. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - we now clarify that this section concerns equilibrium climates, not time-varying response.
17985	71	7	71	7	Well they have always been expressed as a linearization about the current state of the climate. That was the right place to start. [Dennis Hartmann, United States of America]	Taken into account - unfortunately there is not sufficient room for this level of detail. However, removed "assumed" from the text and changed to "approximated".
18909	71	7	71	34	State dependence: The state could be also different after fast adjustments to an abrupt forcing. Can this be also responsible for differing sensitivity? The following paper talks about this possibility: Modak, A., G. Bala, K. Caldeira, and L. Cao, 2018: Does shortwave absorption by Methane influence its effectiveness? Climate Dynamics, https://doi.org/10.1007/s00382-018-4102-x [Govindasamy Bala, India]	Rejected - this section considers state-dependence in feedbacks. Non-linearities that arise due to forcings (or associated fast adjustments) are not considered here, and instead influence the forcing estimates.
5161	71	7			Section 7.4.3 on climate state is too long and I think because of the length the basic messages get lost. To me the basic messages are (1) the dependence of feedback on climate state is important for comparing to paleo climates with large deviations and (2) alpha may become less negative for large increases in CO2. One bit of shortening that could help the message would be to delete the sentences page 72 lines 7-17 on CO2 increases beyond 4000 ppmv. Such increases are hopefully not relevant to IPCC and just confuse the message. [Daniel Murphy, United States of America]	Taken into account. Made some attempt to highlight the importance of this Section by adding "Such considerations are important for the assessment of ECS (Section 7.5)". Removed sentence about extremely high CO2 forcings. However, it is hard to see what else could be cut from this section. The issues of state dependence was only very briefly touched upon in AR5, so there is a lot of new and relevant literature to assess here.
102185	71	9			"individual feedback parameters" to feedback parameter components, alpha_x [Maria Rugenstein, Germany]	Rejected - this is the language used through this chapter and section.
102187	71	14			Mention in first or second paragraph that the "standard framework" is derived from a Taylor expansion of a *small* perturbation (e.g. Roe 2009 or Knutti and Rugenstein 2015) but that there was never a good understanding what is small. [Maria Rugenstein, Germany]	Taken into account - unfortunately there is not sufficient room for this level of detail. However, removed "assumed" from the text and changed to "approximated".
72171	71	16	71	17	Add reference to: von der Heydt, A. S., P. Köhler, R. S. W. van de Wal, and H. A. Dijkstra (2014), On the state dependency of fast feedback processes in (paleo) climate sensitivity, Geophys. Res. Lett., 41, 6484–6492, doi:10.1002/2014GL061121. [Anna von der Heydt, Netherlands]	Rejected - this section is more about the theoretical framework than the application to paleoclimates.
102189	71	20			Unclear what "non-linear behaviour" refers to (non-linear in what?) change to temperture dependence (?) [Maria Rugenstein, Germany]	Accepted - text revised to indicate the key aspect is constant feedback parameter, rather than "non-linear behaviour"
37185	71	37	71	46	Models NEVER provide evidence unless it can be shown that the models are accurate in every regard. Climate models are not accurate, so this section is dishonest. [John McLean, Australia]	Accept - modified subsection title to simply "State-dependence of feedbacks in models". Similarly for the next subsection.
112413	71	37	72	11	I wonder a diagram can go along way with educating the public about the state-dependency of ECS. It is a fairly new idea. [Feng Ran, United States of America]	Noted - agreed, see Figure 7.15.
102191	71	43			"may be" --> is [Maria Rugenstein, Germany]	Rejected - we think "may be" is better here, because the forcing may be constant, with the state-dependence solely in the feedbacks, or vice-versa, or a combination of both.
51321	71	46	72	2	This section is confusing. If feedback is becoming less negative with warming (e.g. due to cloud positive feedback increasing), how is this offset by albedo decreasing? This is also a positive feedback and so surely is not an offset? Is this a mistake in the text? Could this please be clarified. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - they key thing is that the albedo feedback parameter decreases. The albedo feedback parameter is still positive, but has decreased in magnitude. Added "and therefore associated feedbacks" to make clear that a reduction in snow-sea ice means that the associated feedbacks will therefore be weaker (and indeed will become zero in the limiting case in which snow and sea ice has all melted)
102193	71	46			there could be CO2 dependence of feedbacks as well [Maria Rugenstein, Germany]	Rejected - not completely sure what is meant here, but we think this is covered in the non-logarithmic dependence of forcing on CO2.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15989	71	47	71	48	It is likely that alpha also dependent on the rate of change as well as the magnitude of the change. So intuition would suggest that alpha can be defined as alpha =f(T, dT/dt). Note also, that alpha will tend towards zero as the temperature rises towards its equilibrium. Thus, alpha on its own gives a poor representation of the strength of the feedbacks. So as well as discussing alpha, this section should also discuss the function that gives d(alpha)/dT. This is eventually referred to in 7.4.4.3, but it would add clarity to discuss it here. It would be relevant to include discussion at this point on the hystereses within the climate system, or it how feedback loops affect irrervisbiltiy. One would assume that the alpha function would have a different characteristic in the case of temperature reductions, with the prospect that if the planet had stabilized at a high temperature, then alpha would behave in a way that would causes stablisation of the temperature at the higher level. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - here we are considering solely the equilibrium response to forcing. The time-varying transition to that equilibrium is discussed in more detail in the later sections on the pattern effect. Also, as the system approaches equilibrium, alpha does not go to zero - indeed, to first approximation it is constant. However, we agree that mention of hystereses and irreversibility is important, and so we now highlight this more clearly later in this section.
102195	71	51			add Rugenstein et al. 2020 (GRL, Equilibrium ...) This paper conovirms many of these findings with more recent models. The information is somewhat burries, Fig.2b, but more so SM Fig.4 (compare feedback evolution for different forcing levels). [Maria Rugenstein, Germany]	Accepted - added this reference here, but most of the analysis in Rugenstein et al (2020) is concerned with the time evolution of feedbacks as a model approaches equilibrium, which, as the authors state, includes both state-dependence and the pattern-effect. Isolating the pure state-dependence is tricky from this paper. Supp info Figure 4b contains the relevant information but this is not discussed in detail in the paper.
102197	72	1			again Rugenstein 2020 shows this for many more models [Maria Rugenstein, Germany]	Accepted - text modified.
68015	72	4			Is this explanation settled? Another is provided by doi:10.1073/pnas.1809868115. [Robert Pincus, United States of America]	Taken into account - we highlight that it was one model that showed this.
102199	72	10			haha, sorry, again Rugenstein 2020 next to Mauritzen 2019 [Maria Rugenstein, Germany]	Accepted - text modified.
691	72	13	72	14	it is easy for readers to be confused by feedbacks changing in one direction while ECS changes in another. Look at this closely to see how it could be more clearly and consistently handled. Figure 7-16 is much easier to understand for a general reader. This has always been a challenge with component feedbacks and ECS [Bruce Wielicki, United States of America]	Accepted - modified text in several places to highlight the change in alpha's magnitude, its sign, and the change in ECS, so there is no room for confusion.
79275	72	15	72	15	Also cite and add Duan et al. (2019) to the figure (Estimating Contributions of Sea Ice and Land Snow to Climate Feedback; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JD029093) "In contrast to “None” simulations, λBoth increases under higher CO2 levels, suggesting a less sensitive climate response (Figure 6). As shown in Figure 6, the decrease in λBoth is primarily a result of diminishing sea ice feedback at higher CO2 levels." Values are reported in the supporting material of the paper. [Martin Stolpe, Switzerland]	Accepted - Added Duan et al to the text and figure.
5163	72	17	72	20	This sentence says modeling studies support both decreased and increased temperature response during cold climates, but Figure 7-15 shows only increased response (of varying magnitude) compared almost all of the model responses at near zero global anomaly. [Daniel Murphy, United States of America]	Accepted - added paleo cold periods to this figure.
83783	72	22	72	22	Would be useful to redefine EMIC here- I had to look it back up. And I'm not completely convinced this paragraph needs to be here- isn't it obvious that a simple model that doesn't simulate WV/Cld feedbacks wouldn't have state dependence? [Marvel Kate, United States of America]	Taken into account - Redefined EMIC. However, have kept this paragraph as its point about millennial simulations is important. Have cut it down though.
2711	72	22			define ESM, EMIC [Bryan Weare, United States of America]	Taken into account - ESM is now used as standard, and defined EMIC.
102201	72	24			"perhaps unsurprisingly" -- decide whether or not ? [Maria Rugenstein, Germany]	Accepted - text revised.
102203	72	25	72	27	So Jonathan Gregory does not like if FAMOUS is called an EMIC (I tried once), it's fully dynamic and has cloud etc. Also I don't think this is an important enough point to discuss here. [Maria Rugenstein, Germany]	Accepted - text removed.
102205	72	29	72	31	I don't think this is necessarily true. Time here is just a means to get the tempertaure high. If you've several forcing levels which are high enough to produce warm temperature, then that is sufficient to study temperature dependence of feedbacks. See Jonah Bloch-Johnson's papers. [Maria Rugenstein, Germany]	Taken into account - we do think that the EMIC work shows that long simulations can improve confidence. However, we take the point in part so modified "required to" to "could"
9863	72	33	72	37	I suggest using assessment language ('low confidence') here [Robert Kopp, United States of America]	Accepted - text revised.
17357	72	33	72	37	Can a lower CO2 threshold be assessed? [David Neubauer, Switzerland]	Noted. No, we don't think so, the associated processes are not necessarily included in GCMs, and our own climate is not fully equilibrated to the forcing.
17359	72	33	72	37	I assume the reason these studies are not considered is that they don't agree with the paleoclimate proxy record. It would be useful to mention this to the reader. [David Neubauer, Switzerland]	Rejected - it is not clear to us that the paleo proxy record can rule out the changes in state found in these studies.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72173	72	33	72	37	Include some more discussion in this paragraph on abrupt vs non-abrupt changes. From theoretical and enegery-balance model considerations it has been shown recently that abrupt changes are not necessary to see state-dependence. State-dependence can express itself in several ways: (i) If there is an abrupt transition possible between different climate states, then each of these states may have a different feedback parameter because the sum of all individual feedbacks is different (some may be absent, others stronger, etc.); (ii) Within each of these states, feedbacks may vary in strength, such that even if no abrupt transition occurs, the ECS is state-dependent. See for example Ashwin, P., & Heydt, von der, A. S. (2019). Extreme Sensitivity and Climate Tipping Points. Journal of Statistical Physics, 370(1962), 1166–24. http://doi.org/10.1007/s10955-019-02425-x. [Anna von der Heydt, Netherlands]	Taken into account. This paragraph has been edited somewhat in response to other comments, and now contains some more details. Not enough space to include more detail. However, have now cited this new paper.
102207	72	34			"changes in state" --> unclear which states you're refering to here. Before you talk only about feedbacks. Do you mean sudden changes in radiative feedbacks here as well? [Maria Rugenstein, Germany]	Accepted - text revised.
102209	72	39	72	46	Wasn't this kind of the state of the knowedge in AR5? What is new? It's such a bummer than Jonah's recent paper is not out yet. Maybe you could read it and discuss with him which statements one could make here without citing him? i.e. which statements are relatively well backed up by other papers. He really has developed a much clearer understanding and nicer formalism and is solidly quantitative ~ [Maria Rugenstein, Germany]	Accepted - Jonah's paper did come out just in time to be included, and a very brief summary has been added. We think that work since AR5 has further supported, with more models, what was known at the time of AR5. State-dependence only received a sentence or so in AR5.
79277	72	53	72	54	also cite Snyder (2019), https://link.springer.com/article/10.1007/s10584-019-02536-0 [Martin Stolpe, Switzerland]	Accepted - text revised.
116619	72		72		Reference to recent insights from Eocene simulations with high sensitivity models needs to be added (maybe through a x chapter box on Eocene with ch 5 which already covers the PETM in two sections so that related elements are brought together). [Valerie Masson-Delmotte, France]	Taken into account - Zhu et al (Eocene multiple CO2 simulations with CESM1.2) is already included in the discussion and in Figure 7.15. However, added some text to clarify which model studies are modern and which are paleo. We think that a new cross-chapter box at this stage would not be plausible, in my opinion.
5165	73	5	73	7	Like my comment for page 72 line 17, this paleo climate statement doesn't appear to match the models in Figure 7-15, which shows only higher climate sensitivity in the cold periods than today. [Daniel Murphy, United States of America]	Accepted - added cold climates to the figure.
16695	73	7	73	37	in addition to the poleward transport of heat, amplification of CO2, the polarward transport of anthropogenic pollution through aerosol-cloud interaction (Zhao and Garrett, 2015), which warm the Arctic by enhancing cloud thermal emissivity, belongs to the contributing factors. It is recommended to add. Zhao, C., and T. Garrett, 2015: Effects of Arctic haze on surface cloud radiative forcing, Geophys. Res. Lett., 42, 557-564, doi:10.1002/2014GL062015. [Chuanfeng Zhao, China]	Taken into account. The assessment of polar amplification in Chapter 7 focuses on the response to CO2 forcing. Text has been added to point readers to Chapter 4, Section 4.5.1.1 and Chapter 6, Section 6.4.3 for the assessment of the role of other forcing agents in causing polar amplification.
79279	73	9	73	14	Further literature should be added, e.g., Martinez-Boti et al., 2015 https://www.nature.com/articles/nature14145 [Martin Stolpe, Switzerland]	Accepted - text revised.
2713	73	10			define PETM [Bryan Weare, United States of America]	Taken into account - PETM is defined in Cross-Chapter Box 2.1; made this clearer.
129051	73	19	73	21	This conclusion (climate sensitivity increases as temperature increases) needs to be made more clear and reworded. On line 23, "this behavior" references the conclusion but might be muddled to the reader. [Trigg Talley, United States of America]	Accepted - text revised
129049	73	19	73	28	[CONFIDENCE] The description here doesn't suggest a high level of confidence and it seems very much weighted to modelling lines of evidence. [Trigg Talley, United States of America]	Rejected - Since the SOD, Anagnostou et al 2020 (Nature Communications) has given additional weight to the paleoclimate lines of evidence for past warm climates, raising that from "low" to "medium" confidence. As such, we have two independent lines of "medium" evidence (proxies and models) that have combined to give "high" confidence.
17987	73	19	73	28	What is the evidence that paleoevidence from the LGM to present is of relevance for assessing the sensitivity of the current climate to further warming by CO2? [Dennis Hartmann, United States of America]	Rejected - Work on emergent constraints show that LGM tropical temperature change in models correlates with ECS.
102213	73	26			Like above, I don't think the length of the simulation is the essential thing, but that enough temperature and CO2 space is covered (which of course can be done easily by longer simulations). In principle, 100 or so years of 2x,4x, 8x would be enough to start estimating feedback temperature dependence (of course 1000 years each would be better). You could cite bloch-Johnson 2015 and Rohrschneider 2019 for that. [Maria Rugenstein, Germany]	Taken into account - changed "would" to "could", and added that multiple CO2 simulations would also increase confidence.
102211	73	28			Can you quantify how the CO2 concentraion estimates recently reduced? If they did so? I'm just craving for some new information :-) [Maria Rugenstein, Germany]	Rejected - this is a statement that reduced _uncertainties_ in past CO2 estimates would be helpful to assess state-dependence in feedbacks. There won't really be much (any) new information here; IPCC is an assessment of published information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72175	73	31	73	42	This is an update of Figure 1 in 'Heydt, von der, A. S., Dijkstra, H. A., Van De Wal, R. S. W., Caballero, R., Crucifix, M., Foster, G. L., et al. (2016). Lessons on Climate Sensitivity From Past Climate Changes. Current Climate Change Reports, 2(4), 148–158. http://doi.org/10.1007/s40641-016-0049-3. Why are the glacial-interglacial cycle data not included here (they are present in Fig.1 of vdHeydt et al)? [Anna von der Heydt, Netherlands]	Accepted - added cold climates to the figure.
76843	73	33	73	41	Chapter 7, Table 7.15 and Table 7.A.3 leave out metrics with timescales shorter than 50 years as does all the accompanying text. The CCAC SAP recommends that such metrics should be included (e.g. GWP20, GTP10/20) as metrics are used not only for analysis of consistency with long-term temperature targets, which is the usage the SOD implicitly seems to be referring to in its discussion of metrics for SLCFs and long-lived gases, but also for life-cycle analyses, for carbon-equivalent footprints of nations/companies/etc., for analysis of the rate of change in the near-term (which is also part of agreements under the UNFCCC), and by policy-makers who have developed near-term climate mitigation plans such as Norway and the US State of California. Including climate metrics with timescales shorter than 50 years would be consistent with climate metrics reported in the AR5 and AR4 Working Group I reports. AR5 Table 8.A.1 includes GWP values at 20, 50, and 100-year time horizons for GWP and GTP. AR4 Table 2.14 reports GWP of greenhouse gases at 20, 100 and 500 year time horizons. (https://ccacoalition.org/en/resources/recommendations-comments-ipcc-ar6-second-order-draft) [Nathan Borgford-Parnell, Switzerland]	Taken into account: GWP20 added to the supplement
22191	73	49	73	50	or even southern hemisphere high latitudes' is a very odd phraseology here. Consider redrafting for clarity [Peter Thorne, Ireland]	Taken into account. This sentence has been revised.
71079	73	51	73	51	"gradient" should be "zonal gradient" [Yu Kosaka, Japan]	Accepted.
17989	73	52	73	53	What level of confidence is assigned to the eventual emergence of the enhanced warming of the eastern Pacific huge warming. This relies on an assumption that the models are correct and most of the slowdown in tropica ciiculation will express itselfe in the Walker Circulation. I would assess the confidence in this feature as low, since model ensembles are unable to show that observed trend is likely to occur as a natural variation. Here it is state as a sure thing that just has not happened yet. [Dennis Hartmann, United States of America]	Taken into account. The level of confidence for this is assessed at the end of Section 7.4.4.2. The assessment has been modified to reflect low confidence that enhanced eastern Pacific warming this century, but medium confidence on millennial timescales based on paleoclimate evidence.
11549	74	3	74	3	Section 7.4.5.2 does not exist. Should probably be 7.4.4.3 [Gerhard Krinner, France]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
40069	74	10	74	10	Currently only 'Polar amplification' is defined in the glossary. Suggest to also define polar amplification. [TSU WGI, France]	Taken into account. Polar amplification defined in glossary
65077	74	10	76	31	The conclusion that of polar amplification in the southern hemisphere is not well supported in the preceeding text and it's causes could be more clearly explained [Magnus Joelsson, Sweden]	Taken into account. This section has been revised.
19417	74	10	78	6	The section on polar ampliifcstion can be deleted without affecting the rest of the chapter. I presume the motivation for inclusion is the pattern effect, but thst connection is never made clearly. Most of the nalaysis is focused on the Arctic rather than the Antarctic which the gather is presumably more relevant for the pattern effect. In contrast, the following tropical SST gradient section is fine. [Isaac Held, United States of America]	Taken into account. The discussion of polar amplification serves multiple purposes. First, the delayed but eventual warming of the southern hemisphere high latitudes has important implications for radiative feedbacks (the pattern effect), which affects estimates of ECS. Second, this is the only location in AR6 that goes into detail about the mechanisms. We have tried to streamline the text.
9999	74	10	78	39	The discussion of polar amplification (PA) and paleoclimate PA feels perhaps too long and detailed, especially since a detailed understanding of PA is not required to understand the state-of-the-science of radiative forcing or climate sensitivity, which seem to be the foci of this chapter. [Nadir Jeevanjee, United States of America]	Taken into account. The discussion of polar amplification serves multiple purposes. First, the delayed but eventual warming of the southern hemisphere high latitudes has important implications for radiative feedbacks (the pattern effect), which affects estimates of ECS. Second, this is the only location in AR6 that goes into detail about the mechanisms. We have tried to streamline the text.
28887	74	25			Fig. 7.15: presumably ocean heat uptake also implicitly includes export/import by ocean currents. The atmospheric heat transport could be further subdivided into sensible and latent components which would illustrate the contrasting effects of Arctic amplification on these fluxes [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The atmospheric heat transport has been decomposed into sensible and latent heat components and these components are shown separately in Figure 7.12. A discussion of the role of ocean heat transport has been added to the text.
17991	74	44	75	5	This section does not give any credence to what diffusion of moist static energy does to the temperature gradient. Much of the analysis cited here assumes that feedbacks are latitude dependent, but feedback strength itself is dependent on the temperture change. Graverson and others have shown that polar amplication occurs in the absense of surface albedo feedbacks. This controlling factor then induces feedbacks that balance the energy flow with a weaker temperature gradient. Warmer air has more moisture, clouds and melts ice. The thought process fails to distinguish cause and effect adequately. [Dennis Hartmann, United States of America]	Taken into account. This section has been revised to better explain the role of moist static energy transport and the causality of polar amplification.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16267	74	44	76	31	While this section makes thiings look complicated, it seems like a simple framing is that, in the first instance, polar amplification is driven by enhanced poleward latent heat transport in a warmer atmosphere. This has been shown nicely in idealised modelling by Russotto and BIasutti 2020 (DOI: 10.1029/2019GL086771). Local radiative feedbacks can make it stronger or weaker; in models, these are less robust and typically have opposing signs yielding a relatively small net effect. [Steven Sherwood, Australia]	Taken into account. This section has been revised to better explain the role of moist static energy transport and the causality of polar amplification.
19337	74	45	74	45	This is the central argument of Feldl, N., S. Po-Chedley, H. A. K. Singh, S. Hay, and P. J. Kushner, Sea ice and atmospheric circulation shape the high-latitude lapse rate feedback lapse rate feedback, submitted. [Nicole Feldl, United States of America]	Accepted. Cited.
9995	74	53	74	53	This line refers to latitudinal structure in the lapse rate feedback, but the magnitude of this latitudinal structure depends heavily on whether one uses RH-based or conventional feedbacks (Po-chedley et al. 2018, Fig. 3) [Nadir Jeevanjee, United States of America]	Taken into account. A caveat to this effect has been added.
116621	74		74		The issue of liquid water in low Arctic clouds is not discussed. Insights from chapter 6 on Arctic warming links to changes in aerosol emissions need to be integrated. [Valerie Masson-Delmotte, France]	Accepted. Revised accordingly.
129053	75	1	75	55	It is surprising here that there is not more on a simple explanation related to the fact that in the tropics heat goes into precipitation while in high latitudes (cold regions) it goes mainly into temperature. The role of the hydrological cycle is not appreciated. [Trigg Talley, United States of America]	Taken into account. This section has been revised to better explain the role of moist static energy transport and the causality of polar amplification.
23921	75	2	75	2	The formulation '...temperatures are colder,...' is wrong, i.e., temperature cannot be cold or colder, or warmer. Temperature can be lower, or higher, etc. [Branko Grisogono, Croatia]	Accepted.
9997	75	4	75	4	Evidence against the influence of planck function non-linearities on polar amplification was given in Henry and Merlis 2018, "The Role of the Nonlinearity of the Stefan–Boltzmann Law on the Structure of Radiatively Forced Temperature Change" [Nadir Jeevanjee, United States of America]	Taken into account. This references has been added and discussed.
693	75	8	75	9	Text states that ocean heat uptake is the major difference between arctic and antarctic warming, but Figure 7.16 shows that it is actually the radiative feedback differences that dominate. Inconsistency needs resolution [Bruce Wielicki, United States of America]	Taken into account. The text has been revised to resolve this inconsistency.
46309	75	17	75	17	Start a new paragraph here. [Twan van Noije, Netherlands]	Accepted
19339	75	21	75	24	This is a key point of Feldl et al. (2017b): Feldl, N., Anderson, B. T., and Bordoni, S. (2017b). Atmospheric eddies mediate lapse rate feedback and Arctic amplification. J. Clim. doi:10.1175/JCLI-D-16-0706.1. [Nicole Feldl, United States of America]	Accepted. Cited
99591	75	26	75	26	Add a reference before "Woods and Caballero, 2016": Zhang et al., 2013. This paper was the first one showing enhanced poleward moisture transport into the Arctic and resulting amplified warming. Reference: 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]	Accepted. Cited
99593	75	32	75	32	Add a reference before "Screen et al., 2012": Zhang et al., 2008. This is an early paper showing how enhanced poleward heat transport into the Arctic causes an amplified warming. Reference: Zhang, X., A. Sorteberg, J. Zhang, R. Gerdes, and J. C. Comiso, 2008: Recent radical shifts in atmospheric circulations and rapid changes in Arctic climate system. Geophys. Res. Lett., 35, L22701, doi:10.1029/2008GL035607. [Xiangdong Zhang, United States of America]	Rejected. This sentence discusses how radiative feedbacks depend on heat transport. The paper referenced appears to pertain to something else.
19341	75	34	75	37	This is a key point of Feldl et al. (2017a): Feldl, N., S. Bordoni, and T. M. Merlis (2017a), Coupled high-latitude climate feedbacks and their impact on atmospheric heat transport, Journal of Climate, 30, 189–201, doi:10.1175/JCLI-D-16-0324.1. Note this paper should not be confused with Feldl et al. (2017b), which is correctly cited in L31-34 (and elsewhere). [Nicole Feldl, United States of America]	Accepted. Cited.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19343	75	39	75	42	This section emphasizes the ubiquity of polar amplification, however, so long as insolation is seasonally varying, models are capable of simulating climate change that is not polar amplified. Feldl et al. (2017a) showed this for a simulation in which surface albedo is suppressed by reducing ice albedo, which also results in a polar lapse rate feedback that is negative rather than positive (similar to, though more dramatic than, the reduction of the polar lapse rate feedback evident in the albedo-locking experiments of Graversen et al. 2014). Kim et al. (2018) further examined this result and demonstrated that models that produce polar amplification in the absence of sea ice do so under unrealistic insolation conditions. Perpetual equinox simulations, in particular, exhibit large polar static stability, producing a positive lapse rate feedback and polar amplification. (Continued in next comments.) Feldl, N., S. Bordoni, and T. M. Merlis (2017a), Coupled high-latitude climate feedbacks and their impact on atmospheric heat transport, Journal of Climate, 30, 189–201, doi:10.1175/JCLI-D-16-0324.1. [Nicole Feldl, United States of America]	Taken into account. The text has been revised accordingly.
19345	75	40	75	41	(Continued from above comment). Consider the following addition: "Polar amplification still occurs …, though it is substantially reduced when both are suppressed (Graversen et al. 2014, Feldl et al. 2017a)." [Nicole Feldl, United States of America]	The text has been revised accordingly.
19347	75	41	75	42	(Continued from above comment.) I recommend the following edit: "It also occurs in equinoctial simulations without any sea ice (Rose et al., 2014; Kim et al., 2018)." In addition to specifying insolation, I omit Feldl and Roe (2013) as their simulations include sea ice. [Nicole Feldl, United States of America]	Taken into account. The text has been revised accordingly.
38057	75	44	76	4	I think line 5 at page 75 should be removed. In addition, cirtical processes driving polar amplification should be explained in terms of stefan-boltzman equation. [Junhee Lee, Republic of Korea]	Taken into account. This text has been revised.
15991	75	51	75	52	"Because many factors contribute to polar amplification, projections of polar warming are inherently more uncertain that global mean warming." This significance of this point needs further emphasis and its criticality is lost by it being buried in the text. Probably the most critical measurements from the climate models is the extent of polar warming given the consequent impact that this has on sea level rises through destablisation of the Greenland ice sheet. This sentence is basically acknowledging that with this most important output, the models cannot be relied upon and that the temperature profile in the Arctic is most likely un-computable to any degree of accuracy. It is of further note that the references cited are from 2003, 2015, and 2018. Since these dates extraordinarily and unprecedented heating has been observed in the Arctic which is commensurate with argument that the feedbacks interact and accelerate change rapidly once it starts. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The fact that polar warming is relatively more uncertain than global warming is a common feature of climate model projections, as can be seen in Chapter 4. This does not mean that Arctic warming cannot be projected with any degree of accuracy, however. Note that Chapter 9 discusses sea level, and Working Group II considers sea level rise impacts.
19349	76	5	76	8	Feldl et al. (submitted) is also appropriate here as it concerns the effect of changing seasonal dynamics of sea ice on the lapse rate feedback. Feldl, N., S. Po-Chedley, H. A. K. Singh, S. Hay, and P. J. Kushner, Sea ice and atmospheric circulation shape the high-latitude lapse rate feedback lapse rate feedback, submitted. [Nicole Feldl, United States of America]	Accepted. Cited.
17993	76	5	76	14	The previous paragraph had a more balance, if confused, presentation of the relative roles of transport and feedbacks in explaining polear amplification, but now we go back to a surface process oriented explnation of the seasonal cycle. You are just citing lots of papers with different view of the elephant. How does all this discussion support the policy relevant conclusion on line 16? You should be able to say it more succinctly and more convincingly that this list of citations. [Dennis Hartmann, United States of America]	Taken into account. This paragraph has been revised.
20441	76	16	2	85	Considering such a massive feature, it is frustrating that an explanation even widely qualitative cannot be offered by a single mechanism. Encouragingly, the report mentions (Page 75 lines 40-46) that several contributions might be stripped off without major consequences. But it continues on line 51, saying that many factors contribute to polar amplification. And yet: while page 75 lines 49-53 the report privileges the latitudinal structure of radiative feedbacks, this contribution does no longer seem critical when reading again lines 42-46. In contrast, ironically, the present absence of polar amplification on the Antarctic seems firmly assigned to a single major cause (page 75, lines 8-10). [philippe waldteufel, France]	Taken into account. This section has been revised to better explain the relative roles of the different mechanisms driving Arctic amplification and delaying Antarctic amplification.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16195	76	16	76	31	It is strange to draw this conclusion before even considering the paleoclimate information which is arguably the strongest evidence, given that there is a factor of two spread in the model-predicted amplification over the 21st century and it results from the residual of a host of competing factors. I see that later after the paleo section you basically repeat these claims a second time. I suggest that claims here should only be about mechanisms and models; that you flag that paleo evidence will be examined next; and defer all assessment of expected future change to after all the evidence is covered. [Steven Sherwood, Australia]	Taken into account. This overall assessment has been moved to after the assessment of paleoclimate evidence.
28889	76	16			The mature process understanding for Arctic amplification could be briefly stated (e.g. dominated by local feedbacks) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This text has been revised.
31551	76	20	76	21	I find it is difficult to understand where the high confidence for future SH polar amplification comes from. I am not at all in disagreement with the statement, but I make the point it is difficult to trace the reasoning of the authors in ascrbing the confidence level here. Does it come from process understanding or also from climate models (e.g. climate models suggest heat uptake in the SO will reduce (do they show that?) therefore, based on the arguments presented in this section it means SH polar amplification will peak up?) (surely it is not coming from observational evidence, except paelo, but this is next section). It'd be great to make that point clearer [Jean-Baptiste SALLEE, France]	Taken into account. This overall assessment has been moved to after the assessment of paleoclimate evidence, and has been revised to clarify what lines of evidence are supporting the confidence statements.
31553	76	24	76	26	Change in sea-ice regime (which might be caused by increased stratification due to ice-shelf melt or increased precip) is also one aspect that has been describe. Maybe adjust the sentence, and more generally you could consider these two recent papers : https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019AV000132 https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/2019GL086892). [Jean-Baptiste SALLEE, France]	Taken into account. This sentence has been modified to discuss the potential role of freshwater input in reducing Southern Ocean warming over the 21st century, and cites several more relevant studies that discuss those projections. Understanding the role of freshwater forcing in historical Southern Ocean trends is left to other chapters.
17995	76	34	77	17	If the major conclusion that polar amplification will continue into the future has been made based on models and observations of the historical period and supporting theory, why is this section on paleoclimates needed? [Dennis Hartmann, United States of America]	Noted - the paleoclimate evidence provides additional support, and allows us to have higher confidence in our overall assessment.
73835	76	34	77	56	Section 7.4.4.1.2 outlines polar amplification but it doesn't make clear if this is for both poles or with a focus on the northern hemisphere. For the MPWP we don't have temperature data south of 45*S so the polar amplification discussed in the papers cited here has been focussed on the North but also on the North Atlantic region. The models obviously give a global picture but we often can't test with the data. [McClymont Erin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
68887	76	34	78	16	I believe that this is the only paleo polar amplification text currently in the WG1 report. This metric is important to support the paleo key message about prominent recurring patterns, and is a prime target for data-model comparison. If this is going to be the primary account of this topic, then it needs to be expended to include the missing paleo reference periods (LIG, MH and possibly LIA), and both land and sea, not only SST. In addition, the treatment should be quantitative. State the values for polar amplification from the proxies and the models and compare the values to those in AR5 Box 5.1. Also, include the apparently contrary statement by Fischer et al. (2018, 10.1038/s41561-018-0146-0) who state, "Climate models underestimate Arctic amplification…" [Darrell Kaufman, United States of America]	Taken into account. Only CO2-forced time periods are relevant to this section. However, other time periods are assessed in Chapter 3. We now make a more quantitative assessment about the amount of polar amplification by showing the proxy polar amplification in the Figure. However, there is not a single metric that works for all time periods and values, so we let the Figure speak for itself in terms of quantification. The Nature geoscience paper is not including more recent work by e.g. Zhu et al, and so is out of date in this regard.
93033	76	34			Suggest renaming this section as it addresses both high CO2 climates (mPWP and Eocene) and low CO2 climates (LGM) [Bette Otto-Bliesner, United States of America]	Accepted - text revised
100669	76	37	76	37	Add references: Herold et al. (2008), Goldner et al. (2014), Burls et al. (in review) [Matthew Kohn, United States of America]	Taken into account - see response to Comment ID 104725.
100671	76	44	76	44	Verify that these pCO2 values are consistent with other chapters and sections [Matthew Kohn, United States of America]	Taken into account - we now just reference Cross-Chapter Box 1.2 for consistency.
104725	76	46	76	46	Add: "...2016b), the Miocene Climatic Optimum (MCO, Chapter 2, Table 2.1about 16 million years ago, pCO2 concentrations of 400-600 ppm, global mean surface temperatures 8-9 °C above preindustrial, possible 80-100% decrease in ice volume; Herold et al., 2008; Goldner et al., 2014; Frigola et al., 2018; Sosdian et al., 2018; Miller et al., 2020)," [Matthew Kohn, United States of America]	Taken into account - added reference to the Miocene (Steinthorsdottir). However, here in this section we are focussing on time periods for which there is a coordinated modelling study and community-developed datasets.
16197	77	1	77	8	Why isn't this discussion in the polar amplification section above? It would make much more sense to put all information we have on that together into one place and then draw a conclusion. [Steven Sherwood, Australia]	Rejected - we do effectively do this - we assess lines of evidence from models and observations (7.4.4.1.1), and then paleo (7.4.4.1.2), and then draw an overall conclusion (7.4.4.1.3).
100675	77	2	77	2	"…from the MPWP, MCO, and Eocene…" [Matthew Kohn, United States of America]	Taken into account - see response to Comment ID 104725.
100677	77	3	77	3	"…(all three periods; Dowsett…" [Matthew Kohn, United States of America]	Taken into account - see response to Comment ID 104725.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
4205	77	10	77	12	Also may be worth including first accurate terrestrial temperature for mPWP in Northwestern Europe using branched GDGTs (Dearing Crampton-Flood et al., 2020; Climate of the Past). [Emily Dearing Crampton Flood, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - here we are focussing on community synthesis of multiple reconstructions that result in global datasets, rather than single sites.
73837	77	10	77	17	In addition to the advances noted here for MPWP there has also been an expansion of proxy data beyond a mostly Atlantic and equatorial Pacific focus (especially that we now have multiple temperature data from the mid-latitudes of the South Pacific). I'm not familiar enough with Eocene data to know if an expanded geographical coverage of proxy data has also occurred here - but it may be worth pointing out that we continue (and are continuing) to expand our proxy data coverage with new sites and/or new proxies. [McClymont Erin, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - AR5 used the PRISM database, which does have good global coverage, in fact there are more sites in PRISM (N=86) than in PlioVAR (N=31), so coverage has decreased, but precision (because of the reduced time-window) has improved.
100679	77	11	77	11	Add: "…MPWP, MCO, and…" [Matthew Kohn, United States of America]	Taken into account - see response to Comment ID 104725.
100681	77	11	77	11	Add references: Goldner et al. (2014), Burls et al. (in review) [Matthew Kohn, United States of America]	Taken into account - see response to Comment ID 104725.
2715	77	11			define MPWP [Bryan Weare, United States of America]	Rejected - it is defined at the top of this section
100683	77	14	77	14	Add: "…for all three of these time periods…" [Matthew Kohn, United States of America]	Taken into account - see response to Comment ID 104725.
83513	77	15	77	15	Include here also the high-resolution data of de la Vega et al. (submitted) cited in Chapter 2: de la Vega, E., Chalk, T. B., Hain, M. P., Wilson, P. A., and Foster, G. L. (submitted). Multi-site Late Pleistocene high resolution CO2 record using boron isotopes and constraints on CO2 climate forcing. (submitted). [Antje H. L. Voelker, Portugal]	Accepted - text revised
695	77	22	77	22	would help the readers if Figure 7.17 included in the upper and lower section titles "surface air temperature" or SST to clarify what temperature is being used. Its in the text but not as obvious as it should be [Bruce Wielicki, United States of America]	Accepted - Figure revised
73831	77	22	77	33	Figure 7.17 compares data for MPWP with model output. The data used are here Foley and Dowsett (2019). There needs to be clarity here on whether the simulations and data are for the wider 3.3-3.1 "MPWP" interval or if this is zoomed in on the KM5c interval cited in line 13 (same page). The Foley & Dowsett (2019) study collated published data without any revisions to age models, whereas the McClymont et al. (2020, submitted - cited in line 11/12) study carefully checked the stratigraphy of every site and made revisions accordingly. Those revisions mean that Foley and Dowsett likely includes some errors or added uncertainty (a data-model comparison manuscript in preparation by Christian Stepanek makes a direct comparison between the 2 paleo data sets and shows some differences). The McClymont 2020 data set is available at pangaea: https://doi.pangaea.de/10.1594/PANGAEA.911847 [McClymont Erin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - the Pliocene SST proxy data has been changed to the PlioVAR (McClymont et al, 2020) dataset, with the Bayspar calibration.
100685	77	25	77	25	Note: It's a little worrisome at this moment that these references are all submitted, not in press. Doublecheck as we go along [Matthew Kohn, United States of America]	Noted - only published papers referenced.
27163	77	31	77	31	Please reformulate as "Pannels (g,h,i) are like pannels (a, b, c) but for SST….." [Eric Brun, France]	Noted - Figure caption changed.
112415	77	43	77	46	Please also check out Feng et al., (2019) simulations of mid-Pliocene with the post CMIP5 model CESM1.2. This study demostrated the importance of aerosol-cloud interactions as the newer model development to improve simulatons of polar amplification of the mid-Pliocene:Feng, R., Otto-Bliesner, B.L., Xu, Y., Brady, E., Fletcher, T. and Ballantyne, A., 2019. Contributions of aerosol-cloud interactions to mid-Piacenzian seasonally sea ice-free Arctic Ocean. Geophysical Research Letters, 46(16), pp.9920-9929. [Feng Ran, United States of America]	Accepted - text revised.
73833	77	47	77	48	McClymont et al. (2020, submitted - cited on line 11/12 of this page) also shows the better alignment of data and models for high latitudes and considers both the time window narrowing as a part of this but also notes the seasonality in the signal. [McClymont Erin, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised.
68889	77	51	77	53	The assertion that Arctic gateways are “better” represented in recent Pliocene model simulations is contrary to abundant and robust geological evidence (shorelines, biotic exchange between the Pacific and the Arctic Oceans) that shows Bearing Strait was open during the Pliocene. I understand that, in the models, the strait needs to be closed to make the North Atlantic as warm as indicated by the proxy data, but that doesn’t make it a “better” representation of the gateways. Change “a better representation” to “a different representation” and add the fair/balanced criticism that this new representation improves the match with proxy evidence in the North Atlantic, but is contrary to geological evidence around Bering Strait. [Darrell Kaufman, United States of America]	Taken into account. Dowsett et al, on which the closed Bering Strait in the model paleogeography is based, state that "Our decision to follow the paleogeographic model is based upon the shallow depth of the seaway and evidence for repeated episodes of subaerial exposure in both the early Pliocene and during the Pleistocene (Hopkins, 1959, 1967; Nelson et al., 1974)." However, they also imply that one of the benefits of this may be an improve model-data agreement. As such changed "improved" to "modified".

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93031	77	52	77	53	A sensitivity study with one CMIP5 GCM illustrates the importance of the Arctic gateway closures, individually and together, for producing the warmer SSTs in the North Atlantic (Otto-Bliesner et al., GRL, 2017). [Bette Otto-Bliesner, United States of America]	Accepted - text revised.
112417	77	54	77	55	Given that the warming is likely state dependent, it may be worth mentioning that the warming is overestimated in a CMIP6 model for a less warm mid-Pliocene world (Feng et al., 2020): Feng, R., Bette L, O.B., Brady, E.C. and Rosenbloom, N.A., 2020. Increasing Earth System responses and sensitivity in mid-Pliocene simulations from CCSM4 to CESM2, in review at Journal of Advances in Modeling Earth Systems [Feng Ran, United States of America]	Accepted - text revised
112419	78	14	78	16	Please see Feng et al., (2020, in review) for the new assessment of polar amplification of mid-Pliocene as simulated by CMIP5 and CMIP6 models from NCAR. Here is a link to the manuscript: https://www.essoar.org/doi/abs/10.1002/essoar.10501546.1 [Feng Ran, United States of America]	Taken into account - see response to Comment ID 112417.
103621	78	16	78	16	missing line break [Philippe Tulkens, Belgium]	Accepted - text revised
13527	78	16	78	17	Add line spacing between paragraphs. [Maria Amparo Martinez Arroyo, Mexico]	Accepted - text revised
46311	78	17	78	18	Start a new paragraph here. [Twan van Noije, Netherlands]	Accepted - text revised
78727	78	20	78	21	In the here cited Chapter 2, there is no explicit mentioning of Arctic or polar amplification. To be on the safe side and in tune with wording used in Chapter 2, this sentence could be "Stronger warming in the Arctic than in other locations, i.e., Arctic amplification, has already been observed (Chapter 2, Section 2.3) ...". [Heike Wex, Germany]	Taken into account. The text has been modified along the lines of this suggestion.
2717	78	25	78	26	Fig 7.19a does not show that the E. Hemisphere Southern Ocean is slow to warm, and maybe not the Ocean as a whole. There is nothing in this figure pertaining to 1980. [Bryan Weare, United States of America]	Taken into account. This sentence has been revised.
31555	78	25	78	27	Sea-ice regime change is missing in this list I think. [Jean-Baptiste SALLEE, France]	Rejected. The focus of this paragraph was on sea-surface temperature changes, which where sea ice is present is tightly coupled to the concentration of sea ice. Thus, rather than discussing sea ice and sea-surface temperatures separately, we have focused only on sea-surface temperatures here.
15993	78	30	78	31	The paragraph makes the statement of polar amplification in the Southern Hemisphere that, "However, there is only low confidence that this feature will emerge this century." This statement should be qualified with a further statement about its irreversiblity should it happen. Thus, if polar amplification starts can it be reversed at the current levels of atmospheric CO2? The answer would presumably be no. This then begs the following questions of what is the implication of Southern Hemisphere polar amplification, for example in term of additional sea level rise, and what level must atmospheric CO2 be reduced to prevent it, or to what extent must solar radiation management be deployed. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Tipping points and reversibility of warming are discussed in Chapter 4.
71081	78	39	81	2	The section title is "Tropical SST gradients" but the section assesses the zonal gradient in the tropical Pacific only. How about "Tropical Pacific sea-surface temperature gradient" as the section title? [Yu Kosaka, Japan]	Accepted. Section title revised accordingly.
28891	78	44			I assume that robust tropical circulation weakening also contributes which is quite well understood in terms of atmospheric radiative cooling being unable to keep pace with water vapour increases as well as direct greenhouse gas forcing of stability (e.g. Chapter 8, Section 8.2). [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This mechanism is discussed in the following paragraph, and Chapter 8, Section 8.2 is referenced.
23923	78	47	78	47	As the abvove and many other places. That is inadequate English language. [Branko Grisogono, Croatia]	Accepted. Corrected.
116623	78		78		Coordination is needed on these aspects especially with ch 3 and ch 6 (SST patterns, links with circulation) to ensure coherency and avoid duplication. [Valerie Masson-Delmotte, France]	Taken into account - these sections are coordinated with Chapters 2 (cross chapter box) and 3 (PMIP results).
129055	79	1	79	53	Shouldn't the models be evaluated first for how well they do ENSO? None do it really well. Some are downright poor. Moreover all models have major errors in precipitation (distribution, amount, intensity, and frequency). How can there be confidence in these results? [Trigg Talley, United States of America]	Taken into account. The inadequacy of models to simulate tropical Pacific variability is a key reason we are now assigning low confidence in equatorial Pacific zonal sea-surface temperature trends this century. This section has been revised to reflect this.
16221	79	5	79	53	Here I have the same comment as my earlier one on polar amplification. This text jumps to conclusions about the future based only on models, even though the authors are just about to look at observational evidence that bears directly on the question. It is much easier to conclude that the recent E-W trend is transient, when we know that reconstructions of past warm periods show the opposite change. A bunch of GCMs that fail to reproduce the observed trend don't make a very compelling case on their own. [Steven Sherwood, Australia]	Taken into account. This overall assessment has been moved to after the assessment of paleoclimate evidence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18003	79	23	79	23	The difference between ESM2M and G does not go away during the next century. Paynter et al (2018) do not reference Kohyama et al(2017), who show in Fig 12 a the difference in SST pattern between ESM2M and ESM2G for the RCP8.5 trend over 2006-2100. This still shows a strong gradient with cooling in the east relative to warming in the west with a difference of about 1K. The color scale in Paynter et al is too coarse to see this. The IPCC authors are making a judgment here, not quoting a result. Equilibrium takes 500 years or so, we are more concerned with the next century, I think. The modeling community is a bit too eager to accept the consensus of models on an important problem for which the models are inadequate. The fact that one model can simulate something closer to what is being observed is significant. If the observed trend is a transient response to forcing, that is equally significant. How will this section look in 10 years if the east Pacific warming has not yet emerged then? [Dennis Hartmann, United States of America]	Noted. The reference to Paynter et al is indeed accurate (checked with the authors). Note change to low confidence that E Pacific will warm this century.
71083	79	32	79	37	The internal variability within the Pacific and the inter-basin coupling with the Indian Ocean (e.g. Luo et al. 2012 PNAS doi: 10.1073/pnas.1210239109) are missing. The "coupled ocean-atmosphere dynamics" may include them, but the references are all on response pattern to radiative forcing. [Yu Kosaka, Japan]	Accepted. Cited.
18001	79	32	79	43	Medium confidence that one of the many proposed explanations is part of the answer to the observed trend. Then later medium confidence that the models are correct about this trend eventually reversing. [Dennis Hartmann, United States of America]	Taken into account. These confidence levels have been revised to 'low'.
28893	79	35			Chung et al. 2019 Nature Clim https://doi.org/10.1038/s41558-019-0446-4 also find a dominant role of internal variability on the recent strengthening of the Pacific Walker circulation [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cited.
2719	79	37	79	38	this sentence should make it clear that this refers to earlier CMIP5 models, unless there is further evidence [Bryan Weare, United States of America]	Taken into account. Clarified.
98325	79	37	79	43	Recent research has shown that coupled GCMs are generally able to replicate observed trends in Pacific Ocean SSTs over the historical record. We suggest the following changes (in bold): “Coupled GCMs have difficulties to replicate observed trends in the Walker Circulation and Pacific Ocean SSTs over the historical record (Zhou et al., 2016; Coats and Karnauskas, 2017), possibly due to model deficiencies including insufficient multi-decadal Pacific Ocean SST variability (Laepple and Huybers, 2014; Bilbao et al., 2015), mean state biases affecting the forced response or the connection between Atlantic and Pacific basins (Kucharski et al., 2014; Kajtar et al.,2018; Luo et al., 2018; McGregor et al., 2018; Seager et al., 2019), and/or a misrepresentation of radiative forcing (Chapter 9, Section 9.2 and Chapter 3, Section 3.7.6). However, the observed trends in the Pacific Ocean SSTs are still within the range of internal variability as simulated by large single-model initial condition ensembles (Olonscheck et al., 2020).” Reference: Olonscheck, D., M. Rugenstein, and J. Marotzke (2020), "Broad consistency between observed and simulated trends in sea surface temperature patterns", Geophysical Research Letters 47, 1-10, doi:10.1029/2019GL086773 [Dirk Olonscheck, Germany]	Accepted. Cited.
38059	79	37	79	43	The authors may want to refer the following literature "Observational evidences of Walker circulation change over the last 30 years contrasting with GCM results BJ Sohn, SW Yeh, J Schmetz, HJ Song Climate Dynamics 40 (7-8), 1721-1732 [Junhee Lee, Republic of Korea]	Accepted. Cited.
71085	79	37	79	43	Masahiro Watanabe et al. (submitted) find that large ensemble simulations capture the 1951-2010 trend of zonal SST gradient. [Yu Kosaka, Japan]	Accepted. Cited.
83785	79	45	79	45	I don't understand what "medium evidence" means [Marvel Kate, United States of America]	Taken into account. Sentence revised.
71087	79	45	79	48	"transient in nature" sounds to me that it is a transient response to forcing, without a possibility that internal variability has dominated. The same is the case for P81L1. (I am not native in English and this could be my language problem, though.) [Yu Kosaka, Japan]	Taken into account. This was intended to mean that it could either be a transient forced response or a temporary phase of internal variability. The text has been revised to clarify.
17997	79	46	79	46	I would probably go with low confidence here until we actually see the east Pacific warm more than the west Paciffic. Models mostly warm a lot in the east, but models mostly have a double ITCZ and a poor simulation of ENSO. Neither of these deficiencies has been reduced much in the past 30 years, and they seem key to a confident prediction. This is a very important problem both for estimates of climate sensitivity and impacts of a given amount of global warming on regional climate. [Dennis Hartmann, United States of America]	Taken into account. This has been revised to give low confidence of enhanced eastern Pacific warming this century.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98327	79	50	79	53	Our recent publication highlights the role of internal variability for SST trends and may serve as a reference as suggested: “There is emerging evidence that the Walker circulation has weakened again since around 2011, suggesting that a transition to an El Niño–like warming pattern may currently be underway (Cha et al., 2018) with low confidence due to the possibility that this could be a reflection of natural variability (Olonscheck et al., 2020).” Reference: Olonscheck, D., M. Rugenstein, and J. Marotzke (2020), "Broad consistency between observed and simulated trends in sea surface temperature patterns", Geophysical Research Letters 47, 1-10, doi:10.1029/2019GL086773 [Dirk Olonscheck, Germany]	Rejected. In our judgement, no reference for internal variability is needed here.
71967	79	50		53	An emerging trend since 2011? Pacific decadal variability has a time scale of order 50 years so I have no (not low) confidence in this "emerging trend". [John Church, Australia]	Not applicable. This sentence has been removed.
116625	79		79		For past high CO2 climates, there is a need to link to chapter 2 (Pliocene box) and chapter 5 (PETM sections). Check also with chapter 4. [Valerie Masson-Delmotte, France]	Noted - linked to cross-chapter boxes.
38061	80	1	80	1	The section of 7.4.4.2.2 does not seem to fit for this section. [Junhee Lee, Republic of Korea]	Rejected - This section assesses important lines of paleo evidence for changes in longitudinal gradient.
68891	80	1	80	43	My comment regarding polar amplification mainly applies to this section on tropical gradients as well. Proxy evidence is less robust than for polar amplification, but this key large-scale metric is needs to be treated comprehensively by including more information from more than just the Pliocene. Can values be calculated for the EECO and LGM based on proxy data and models? If these are controversial, then it would be useful to point to the low confidence in this key climate indicator. Also, please be sure that the information on the Pliocene is consistent with CCB2.4, which is devoted to the Pliocene, but isn’t mentioned in this section. [Darrell Kaufman, United States of America]	Rejected - The focus here is on warmer climates and the records required to estimate the longitudinal SST gradient don't extend beyond the Pliocene. Our opinion is that any work on the LGM would go beyond an assessment and stray into new work.
68893	80	1	80	43	The outcome of the assessment should be included in the ES so that it can be used in the TS to address one of the paleo key messages: “What are the prominent large-scale, recurrent spatial patterns associated with past global changes, including land-sea contrast, polar amplification, tropical Pacific gradients?” [Darrell Kaufman, United States of America]	Rejected - in the end we unfortunately had to cut the ES statement on paleo zonal gradients due to a lack of space.
23925	80	5	80	11	There and elsewhere: what is ODP? [Branko Grisogono, Croatia]	Not Applicable - text revised and ODP no longer used.
2721	80	11	80	12	define OPD 806, TEXH86, Ukt37 [Bryan Weare, United States of America]	Accepted - removed these as not needed any longer
105773	80	11	80	15	erroneous "a" prior to "new SST records". Additionally structuring this sentence a statement with a list after a colon would increase it's readability. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
112421	80	14	80	15	Temperature gradient is usually measured as °C/distance. Not sure how to interpret these gradient values without introducing how the values are calculated. [Feng Ran, United States of America]	Accepted - changed to "difference" where appropriate.
45453	80	15	80	23	Part of the interpretation variation can be attributed to time frame of comparison. The Late Quaternary isn't a particularly useful point of reference when models projections and hindcasts are compared to pre-industrial/modern. This could be addressed in the paragraph as including the community abiguity for the time frame of comparison as a potential issue or re-calculated using Ravelo et al., 2014. [Heather Ford, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - This is an important point for evaluating the absolute warming and comparing to models but should not strongly affect the estimate of the east-west difference. However, have added the Ravelo et al reference at an appropriate point.
105775	80	19	80	23	I concur with this sentence's conclusion. However, I wonder if it could be expressed in a more succinct fashion. [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
105777	80	25	80	28	This sentence is correct, but could be rephrased to stress that models are equivocal about the change in SST gradient (contrary to the obs discussed above) [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
2723	80	27			define PlioMIP1 and explain basics [Bryan Weare, United States of America]	Accepted - text revised
112423	80	28	80	28	See Feng et al., (2020, in review), from CMIP5 to CMIP6 model, there is an increase in gradient reduction across the tropical Pacific. The CMIP6 model (CESM2) now shows 1°C gradient reduction within the range of both Zhang et al., (2014) and Tierney et al., (2019) stimates. Here is a link to the manuscript: https://www.essoar.org/doi/abs/10.1002/essoar.10501546.1 [Feng Ran, United States of America]	Accepted - text revised
105779	80	29	80	31	This sentence uses "simulate/ions" 3 times. I suggest: "To simulate reconstructed Early Pliocene gradient reductions, models require with hypothetical modifications to their physical parameterisations such as cloud albedo or ocean mixing (Fedorov et al., 2013; Burls and Fedorov, 2014b)." [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
16223	80	29	80	31	Confusing sentence -- seems to say X is required to simulate X [Steven Sherwood, Australia]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83789	80	33	80	43	I agree with this assessment that the cooling trend in the eastern tropical Pacific is likely transient, but I think the text should contain at least a sentence engaging with the main argument of Seager et al 2019: that models simulate too-cold equatorial cold tongues, and the resulting biases in relative humidity and wind speed make the model tropical SSTs too sensitive to forcing. This isn't just an argument about whether the recent trends are forced or due to internal variability; it's an argument that the weakening of the tropical east-west SST gradient projected under 4xCO2 conditions is an artifact of model bias, and therefore the pattern effect (discussed in the next section) is overestimated by models. It's probably justifed to assign medium confidence to the projected El Nino-like warming pattern given the Pliocene evidence, but I think the possibility that the model tropical response to forcing is biased high needs to be explicitly taken into consideration. [Marvel Kate, United States of America]	Taken into account. We agree that the question of the role of model cold tongue biases on the ability of the models to accurately capture the magnitude of the transient gradient strengthening (aka Thermostat) response is important (Seager et al). That said, we don't think this is the place to discuss this and it should rather be addressed in the pattern effect section.
45455	80	36	80	39	thermocline was either deeper AND/OR less stratified… [Heather Ford, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
71089	80	39	80	40	The trend for "the last 60 years" is not described in Section 7.4.4.2.1 but should be for consistency. [Yu Kosaka, Japan]	Accepted - referenced Seager et al., 2019; Chapter 9, Section 9.2.1.1; Figure 9.3
83787	80	40	80	40	internal-variability -> internal variability [Marvel Kate, United States of America]	Accepted - text revised
105781	80	42	80	42	the "may have" near the end of the sentence seems unnecessary. I believe that the uncertainty in the conclusion is already expressed via the earlier word "likely". [Chris Brierley, United Kingdom (of Great Britain and Northern Ireland)]	Accepted - text revised
18005	81	5	84	46	IPCC chapters have a tendency to read like review papers in which every publication and numerical result is mentioned. They are meant to be assessments leading to significant, policy relevant statements. I keep looking for bullet points that are supported by an efficient scientific argument. Medium confidence that something will happen at some unspecified time in the future are OK, but are only going to be read by scientists, and only with moderate interest. Can the arguments be focused on a statement that pattern effects reconcile model projections with observations of the past century, or is that already known by the policy community? If so, how have we refined that conclusion? Do you plan to bring some of these probability statements out of the text as bullet points that can be elevated to the TS or SPM? [Dennis Hartmann, United States of America]	Taken into account. This section has been streamlined.
27165	81	56	81	56	Please add "which" before " has been" [Eric Brun, France]	Accepted. Revised.
28895	81				Fig. 7.18 - it is a bit difficult to distinguish cloud from land. Figure 7.19 seems a bit redundant given most of this is shown in Fig. 7.18 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Fig. 7.18 has been revised to make the clouds easier to see.
17999	82	14	82	14	I am perplexed by why authors take pattern trends from 1870, when the data at the beginning are insufficient to detail the patterns, except by interpolating with patterns drawnn from moderrn data. Most of the pattern trend of relevance to this issue occurred in the past 40 years. [Dennis Hartmann, United States of America]	Noted. Trend patterns are taken from 1870 to reflect the fact that energy budget constraints on ECS are produced using the late 1800s as the reference period. Indeed, this introduces uncertainty in the warming pattern and thus in the pattern effect, as discussed in the text.
83791	82	33	82	46	I understand the attempt here to treat the Lewis and Curry 2018 estimates as a good-faith effort to constrain ECS, but I feel they're better understood as an edge case assuming no pattern effect and adjusting assumptions to yield the smallest possible ECS. Instead of comparing and contrasting the LC18 and Armour studies, this paragraph would be more useful if it focused on the difference between using the early portion of abrupt4xCO2 experiments to estimate ECS vs later years. A useful additional reference might be Dunne et al (submitted), which reviews ECS calculations from the US climate modeling groups (NCAR, DOE, GFDL, GISS) and clearly shows that the ECS depends on the calculation method, and that regressing over years 51-300 gives results that agree with the longer-term coupled simulations. (reference:@article{Dunne.et.al20, year={2020}, title={Comparison of equilibrium climate sensitivity estimates from slab ocean, 150-year and longer simulations}, author = {John P. Dunne and Michael Winton and Julio Bacmeister and Gokhan Danabasoglu and Andrew Gettelman and Jean-Christophe Golaz and Cecile Hannay and Gavin A. Schmidt and John P. Krasting and L. Ruby Leung and Larissa Nazarenko and Lori T. Sentman and Ronald J. Stouffer and Jonathan D. Wolfe}, journal = {Geophys. Res. Lett.}, note={submitted}, } [Marvel Kate, United States of America]	Taken into account. The discussion of Lewis and Curry has been improved and placed into the wider context and the emphasis adjusted in line with the comment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
18009	82	33	82	46	Do I need to rememer all these numbers? Why? All the info is here, now boil it down to an efficient question-data-answer format that a scientist who does not specialize in this niche can follow with interest. [Dennis Hartmann, United States of America]	Taken into account. This section and paragraph have been streamlined.
87951	82	33	83	8	Pages 82-83 is the part of the chapter where you discuss estimates of α' which are critical to your case for revising ECS upwards. You cite Lewis and Curry 2018 and Dong et al (submitted) for values around α' =0.05, which are very small, and Andrews et al (2019) for α' =0.23, and Dessler et al (2018) model simulations that say α' can vary naturally by 0.5. Then on page 83 you discuss climate model simulations showing α' =0.6 and then on page 84 lines 43 to 46 you conclude "Thus, α' is estimated to be in the range 0.0–1.0 but with a low confidence in the upper end of this range." You are over-privileging model simulations here. i think an assessment that really conveys the situation for a reader would show more clearly that if α' =0.05 or 0.06 then the ECS estimates based on historical energy balance estimates will look like those in Lewis and Curry and similar papers, but if α' is closer to 0.5 or 1.0 then ECS will go up in the future, and this issue can't yet be decided. You can't ask readers to take a position based on an assumption that climate models provide accurate forecasts of climate features they have inaccurately represented in the past. Commenting on a different but related issue (aerosol forcing) and whether to privilege model projections over observations, Stevens and Fielder (https://journals.ametsoc.org/doi/10.1175/JCLI-D-17-0034.1) said "Surely after decades of satellite measurements, countless field experiments, and numerous finescale modeling studies that have repeatedly highlighted basic deficiencies in the ability of comprehensive climate models to represent processes contributing to atmospheric aerosol forcing, it is time to give up on the fantasy that somehow their output can be accepted at face value." This warning applies here too. [Ross McKitrick, Canada]	Noted. The quantification of the pattern effect (α') is indeed quite uncertain because, as we point out, its estimate relies heavily on climate models. We address this uncertainty in several ways. First, we rely not only on coupled GCMs, but also on atmospheric GCMs driven by observed warming patterns to correct for errors in coupled models' patterns of warming. Second, assessed range of α' is quite wide, allowing a value of α' = 0 and α' = 1 with low probability, with high confidence in the low end of α' but low confidence in the high end based on process understanding and model agreement. Third, in Section 7.5.2 we do what is suggested here -- showing how ECS estimated from historical energy budget constraints varies depending on whether our assessed range of α' is used or whether α' = 0 (no pattern effect) is used. The lower end of ECS is not sensitive to this choice.
23927	82	35	82	38	What is 1pct? - The abbrev. has not been defined previously in Chapter 7. [Branko Grisogono, Croatia]	Taken into account. The abbreviation is defined in the paragraph referenced.
19419	82	52	82	52	The definition of \alpha' needs to be more prominent rather that relegated to a footnote [Isaac Held, United States of America]	Accepted: Revised to move the definition to the text.
78067	83	4	83	4	I think it's more that the simulated *response* to historical forcing should be realistic, especially in SST patterns; the forcing itself is probably not badly simulated. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised to note this interpretation.
98329	83	10	83	15	In contrast to the framing here, our recent findings showed that GCMs are able to reproduce the observed cooling of the eastern tropical Pacific or Southern Ocean over recent decades. We suggest the following changes: “In general, coupled GCMs hardly reproduce the observed cooling of the eastern tropical Pacific or Southern Ocean over recent decades, even within historical simulations where non-CO2 forcing agents are included and even when allowing for different phasing of internal variability (Zhou et al., 2016; Coats and Karnauskas, 2017; Kostov et al., 2018). However, large initial condition ensembles which only differ in the internal variability show a few members which resemble the observations. This suggests that internal climate variability has played an important role in these observed SST trends that GCMs replicate only in a few realizations (Olonscheck et al., 2020); or that GCMs may have errors in either their applied forcing or forced response (Chapter 3, Section 3.7.6; Chapter 9, Section 9.2).” Reference: Olonscheck, D., M. Rugenstein, and J. Marotzke (2020), "Broad consistency between observed and simulated trends in sea surface temperature patterns", Geophysical Research Letters 47, 1-10, doi:10.1029/2019GL086773 [Dirk Olonscheck, Germany]	Taken into account. Discussion revised and cited.
2725	83	10	83	15	this is all pretty sloppy. Does this refer to CMIP6? The 4xCO2 map in F.7.19b is poor guidance [Bryan Weare, United States of America]	Taken into account. Paragraph revised to clarify which model ensembles are used.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
19421	83	24	83	46	The upper bound on the pattern effect is based on AMIP simulations entirely, as I undertand it, but the chapter is elsewhere very critial of GCM cloud feedbacks; This requires soem discussion. Also, it is uncelar how the assessment of cloud feedback (at least to the extent that it is based on observations) does or does not take into account the cloud respone to the observed SST warming pattern; [Isaac Held, United States of America]	Taken into account. Text has been added to discuss our reliance on ESMs as a reason that we have only low confidence in the magnitude of the pattern effect. Given the low confidence in the magnitude of the pattern effect, the instrumental record provides only a constraint on the lower bound of ECS (Section 7.5.2). It is difficult to say how the pattern effect may influence estimates of cloud feedback based on observations, but one key line of evidence using cloud controlling sidesteps the issue by placing observational constraints on how clouds respond to changes in their environment (e.g., SSTs, inversion strength, surface wind speed, subsidence rate, etc), and then uses models to project how these environmental variables will change with warming, which implicitly depends on the patterns of warming projected by models. To our knowledge, there has not yet been work done to evaluate how estimates of cloud feedbacks by this method may be influenced by the pattern effect, and the WCRP ECS assessment (Sherwood et al. 2020) notes that an important goal is to "develop a more complete understanding of how the climate feedbacks from short-term variability we observe relate to the feedbacks from long-term forced climate change we seek."
68017	83	30	40		This seems to repeat some of the material on page 81, lines 27-46 [Robert Pincus, United States of America]	Taken into account. Repetitive material has been removed.
46313	83	30	83	31	Change "transient adjustment" to "transient response". [Twan van Noije, Netherlands]	Accepted. Revised.
78069	83	30	83	31	Probably the climate sensitivity in the AMIP period is particularly low because of the SST pattern in the Pacific. Your statement here correctly describes the possible reasons for this (unforced variability or transient response to forcing) but as far as I can see sect 7.4.4.2 (referenced here) doesn't specifically say more about this period. I'm not sure that the text states clearly enough that in amip-piForcing (Andrews et al. 2018) the climate sensitivity from the AMIP period is lower than at any previous time in the historical record. AMIP simulations with historical forcing agree about low sensitivity in the AMIP period (Gregory and Andrews, 2016). Gregory et al. (accepted in 2019, published in 2020) show that CMIP5 historical simulations do not reproduce this effect, perhaps because they don't respond adequately to volcanic forcing (that's a conjecture which they make on the basis of the analysis). The SST pattern observed in those decades, with an east-west Pacific dipole anomaly of such a magnitude and duration, is not produced by CMIP5 AOGCMs either in piControl or in response to historical forcing, but it can be reproduced by applying observed windstress anomalies (e.g. England et al. 2014, 10.1038/nclimate2106). Maybe this comment applies better to 7.5.2.1 - I'm not sure. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This section has been revised.
71093	83	42	83	53	To substantiate the observational uncertainty in the SST trend pattern, addiing e.g. ERSST5-based trend to Fig. 7.19 may be useful. [Yu Kosaka, Japan]	Rejected. Assessment of observational uncertainty in SST trends is beyond the scope of this chapter. Thus, we show only one observational dataset for illustrative purposes here.
22193	83	46	83	47	Section reference should be to 2.3.1 not 2.2 [Peter Thorne, Ireland]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
71091	83	46	83	47	Chapter 2 Section 2.2 -> Section 2.3? [Yu Kosaka, Japan]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
116627	83		83		What are the potential implications of these model biases for their feedback characteristics and their projections? [Valerie Masson-Delmotte, France]	Noted. To our knowledge there has not yet been work done to examine the implications of model biases in their historical sea-surface temperature patterns for their projections. Watanabe et al. 2020a may be most relevant to this question, which suggests that observed trends in the Pacific Ocean SSTs are within the range of internal variability as simulated by large initial condition ensembles of CMIP5 and CMIP6 models (cited in Section 7.4.4.2.1).
46315	84	5	84	6	Please clarify what is meant with "if the ECS values are higher than those spanned by climate models". If not derived from models, which "ECS values" are referred to here. [Twan van Noije, Netherlands]	Taken into account. Revised to clarify that this means at high values of ECS.
13529	84	13	84	13	Add space between “to” and “Figure”. [Maria Amparo Martinez Arroyo, Mexico]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16225	84	24	84	46	This nice analysis seems to conclude that we are more confident about the long-term SST warming pattern than we are about transients. This raises the question of whether the near-term warming (over the next few decades or even the whole century) might be less than projected on average by CMIP6 models, even if their mean ECS and longer-term warmings are correct. I haven't reviewed the projection chapters but has this possibility been raised there? This also relates to a comment later on Box 7.2. [Steven Sherwood, Australia]	Noted. To our knowledge there has not yet been research done to assess this possibility. However, the assessed temperature projections in Chapter 4 rely on more than just the CMIP6 models, and in fact show less warming than the raw model output. This is in part due to the assessed range of ECS being lower than the raw ECS output of CMIP6 models. As shown in Tables 7.12 and 7.13, the assessed ECS range is in good agreement with evidence from emergent constraints based on the rate of recent warming (e.g., papers such as Jiménez-de-la-Cuesta and Mauritsen, 2019 and Nijsse et al., 2020) which implicitly assume that the pattern of SSTs will evolve from its present state in a way that is similar to the anomalies projected from present by CMIP5/6 GCMs (i.e., any future pattern effect will be small). The assessed range of future warming is thus broadly consistent with the possibility that the warming pattern will not change substantially this century.
33193	84	29	84	37	A piece of evidence not mentioned when discussing the unlikely potential for a "negliable pattern effect" is that it would imply zero role for any other forcing agent than CO2 in impacting historical SST patterns (or that all forcing agents produce the same pattern of temperature change). This is unlikely given all the evidence/literature pointing for example to the role of aerosols impacting on the Pacific PDO and Altantic SST trends (I assume this is assessed elsewhere in the report and can be linked to?). These non-CO2 forcing specific historical SST trends would not occur under 2xCO2 ECS patterns by definition. [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text in this section has been modified to discuss the role of non-CO2 forcing agents as well.
71969	84	37		40	The minimum in warming in the Southern ocean is unlikely to disappear this century and I am unconvinced that we have medium confidence of the greater eastern equatorial Pacific warming. I do not see how these assessments lead to the conclusion of High confidence in the radiative feedback changes for this century. [John Church, Australia]	Taken into account. We now assess low confidence that the eastern tropical Pacific will warm by more than the western Pacific this century, and low confidence that the Southern Ocean will show enhanced warming this century. However, we assess medium confidence that the eastern Pacific will warm on timescales longer than several centuries, and high confidence that the Southern Ocean will warm on timescales longer than several centuries. We thus have low confidence in feedback changes this century, but high confidence that there will be feedback changes (alpha' > 0) as equilibrium is approached (which is relevant for ECS). This has been clarified.
28897	84	37			It would be useful to be more precise about "eventually" (e.g. multi-century or millenial time-scales?) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Clarified that this response is expected to occur within multiple centuries.
83793	84	38	84	39	How much of this "high confidence" that STs in the Southern Ocean will eventually warm by more than tropical SSTs is due to the explicit exclusion of land ice feedbacks and cold fresh water injection in the SO from this definition of ECS? [Marvel Kate, United States of America]	Taken into account. Clarified that these are transient effects.
93687	84	39			“less negative” (no hyphen) [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
15995	84	44	84	46	The statement "Thus, α’ is estimated to be in the range 0.0–1.0 W m–2 °C–1 but with a low confidence in the upper end of this range. Section 7.5.2 assesses the implications of changing radiative feedbacks for estimates of ECS based on the historical temperature record," is confusing given the previous discussions that have stated that α is state dependent. Are the values of α for today's conditions of the temperature being 1 degC above the pre-industrial baseline? If so, this should be clarified. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised to clarify that this alpha' refers to the pattern effect only. Changes in alpha from state dependence become relevant for estimating ECS from past climates that are much warmer or colder than today, as discussed in Section 7.5.3.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96729	84	49	104	13	_Assessment of the ECS in Section 7.5: We are strongly concerned that the assessment of the ECS, a highly policy relevant parameter, remains unclear and nontransparent. Section 7.5 describes the different constraints that have been considered for this assessment but we cannot comprehend the expert judgement that shaped the final estimates of the ECS presented in this Section: • The summary presented in Section 7.5.5 introduces the best estimate ECS of only 3.0°C without a comprehensive explanation of this specific value. All lines of evidence, with small exceptions, would suggest a higher value (say, 3.5°C, or even up to 4°C). • Why is the best estimate from paleoclimates 3.1°C (according to Section 7.5.3), although Table 7.11 indicates a most likely best estimate between 3.5°C and 4°C? And why is it omitted in Table 7.13? • Section 7.5.4 states that emergent constraints do not allow for a best estimate, just for a very likely range of 1.5°C to 5°C. However, Table 7.12 shows a lot of best estimates, with an average value of exactly 3.3°C (computed from the third column). Also the 'very likely range' of 2°C to 5°C indicates rather a best estimate of 3.5°C. • The 'very likely range' column in Table 7.13 includes two upper limits (6.4°C from process understanding and 5°C from emergent constraints). Why are the ranges from CMIP6 indicating higher upper limits not included in this table? And why do the emergent constraints rule out the 6.4°C-value from process understanding? • Please explain why emergent constraints are considered to be the most reliable line of evidence, although their predictive power depends on many assumptions? Why can they be used to exclude higher ECS estimates? • Please explain as well the criteria that informed the expert judgement to choose a very likely range of 2-5 °C instead 2°C-5.6°C which seems justified based on CMIP6 results and process understanding. • Please explain also in more detail why CMIP6 results are not included in the summary assessment, including table 7.13 We kindly ask the authors to reconsider the assessment of the ECS or at least to improve their line of argument supporting their choices for the best estimate and very likely range that are lower than most lines of evidence suggest. [Nicole Wilke, Germany]	Taken into account. Section 7.5 has been revised to increase the transparency with respect to how different lines of evidence are combined to arrive at the TCR and ECS assessments. ECS estimates are made to the closest 0.5C, so it was an expert judgement to choose 3.0C against 3.5C for the best estimate. This is now clarified. All lines of evidence do not suggest a higher value. Not all paleo studies have been given the same weight in our evaluation, this is now clarified. This is why the paleo studies and emergent constraint studies are not directly transferred. Paleo studies are best at ruling out high estimates, not emergent constraints. This is now clarified. We now clarify why the CMIP6 model results are not used as a line of evidence
18351	84	49	108	20	Please note that the new findings of Dai et al. (2020) have major implications for this section on ECS and TCR: 1). The ECS estimates from the various methods (e.g., those based on climate variability and paleoclimate changes) may not be the same as the ECS resulting from the response to CO2 forcing, as internal variations often result in different slopes between dN and dT in eq. 7.1 (and past climate changes often include both internal variations and forced changes); 2) The ECS values reported in IPCC AR5 and estimated for CMIP6 models by Zelinka et al. (2020) are underetimated by 10-25% due to the problems in the Gregory et al. (2004)'s regression method used in these studies. Dai et al. (2020) provided improved estimates of the ECS for both the CMIP5 and CMIP6 models. Refs cited: Dai, A., D. Huang, B.E.J. Rose, J. Zhu and X. Tian, 2020: Improved methods for estimating equilibrium climate sensitivity from transient warming simulations. Climate Dynamics, DOI :10.1007/s00382-020-05242-1. https://link.springer.com/article/10.1007/s00382-020-05242-1 Gregory JM, Ingram WJ, Palmer MA, Jones GS, Stott PA, Thorpe RB, Lowe JA, Johns TC, Williams KD (2004) A new method for diagnosing radiative forcing and climate sensitivity. Geophys Res Lett 31:L03205. https ://doi.org/10.1029/2003G L018747. Zelinka MD, Myers TA, McCoy DT, Po-Chedley S, Caldwell PM, Ceppi P, Klein SA, Taylor KE (2020) Causes of higher climate sensitivity in CMIP6 models. Geophys. Res Lett 47:e2019GL085782. https ://doi.org/10.1029/2019G L0857 82 [Aiguo Dai, United States of America]	Noted. Estimates model ECS from CMIP6 is not used as an independent line of evidence in AR6, and furthermore some of the low-bias pointed to in this and other studies is compensated by a high bias due to using 4xCO2 instead of 2xCO2. This issue is dealt with in detail in Chapter 7.
37745	84	51	106	17	Limitations of the ECS in modelling should be noted as the present warming path ocsillates upwards [Howard Brady, Australia]	Noted. The comment provides no concrete suggestions.
10801	84	55	84	57	No, ECS and TCR cannot be "inferred from observational records". They can be estimated by using simple expressions or models, combined with estimated ERF, constrained to observational records (as described later in this section). It is rather important to not give the false impression that ECS and TCR are observed quantities or can be deduced from observations without the need for models of one kind or another. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The reviewer is correct that the concept of statistical inference includes formulating a model and making assumptions, but this is explained within the section text and further elaboration is not deemed appropriate for this introductory paragraph.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
20443	85	2	85	8	Inasmuch as TCR lies somewhere on the road leading to the equilibrium and ECS, it makes sense that TCR<ECS, provided of course that the relation between both quantities, although nonlinear as indicated, remains monotonous, which is obviously the case and it might be relevant to mention it. See also Page 88 Lines 53-54. [philippe waldteufel, France]	Accepted.
51323	85	3	85	3	Are we confident that TCR remains correlated with ECS in the CMIP6 models? Presumably it will be but as the analysis has not yet been undertaken yet, should this be caveated? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The statement of correlation and the respective correlation coefficient was deleted, since this relationship is not linear.
15417	85	5	85	8	Tsutsui (2020, https://doi.org/10.1029/2019GL085844) has shown that the ratio of TCR to ECS tends to decrease as ECS increases from the CMIP5 and CMIP6 models, and that this tendency is consistent with a theoretical relationship between feedback strength and response timescales. These findings have something to do with the fact that historical simulations with high-sensitivity models do not necessarily exceed observed warming considerably, and have implications for uncertainty ranges of climate projections during this century. [Junichi Tsutsui, Japan]	Taken into account. This reference as well as Hansen et al. 1985 and Flynn and Mauritsen 2020 were added.
10803	85	8			Where has it been outlined that ECS is related to 1/alpha? It is later in this chapter (7.5.1.1), but I could not find it earlier. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text in the parenthesis was deleted.
46317	85	13	85	13	Change "1%CO2" to "1pctCO2"? [Twan van Noije, Netherlands]	Accepted.
93721	85	22			Cite Zelinka et al. 2020 here (doi: 10.1029/2019GL085782). [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rather than citing multiple studies here, the reference was deleted.
99549	85	26			This subsection is exceptionally lucid and easy to understand [Robert Pincus, United States of America]	Noted. Thanks!
99547	85	34			This section (process-based estimates) and the next (Estimates based on the historical temperature record) have much in common but especially the use of low-dimensional energy balance models to interpret information. There might be some benefit to making explicit links, even if the models are elabroated differently in the two sections (two-layer model in 7.5.1, variable alpha prime in 7.5.2) [Robert Pincus, United States of America]	Taken into account. Thank you for the suggestion. In 7.5.1.2, we have referred to sections explaining the pattern effect (7.4.4.3 and 7.5.2) and added a brief explanation of how the two-layer model in this section is connected to that in the later section.
18011	85	41	85	41	I forgot what SARF was. It was defined within text back on page 23, and I skimmed that section a month ago. [Dennis Hartmann, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
46319	85	46	85	46	ΔF appears twice in this sentence. Please reformulate. [Twan van Noije, Netherlands]	accepted.
16227	85	46	85	47	The way this is worded makes it sound like alpha and dF were both first estimated in Section 7.3.2.1 so can you reword please [Steven Sherwood, Australia]	Taken into account. Reworded.
10807	85	46	85	49	These two sentences give the impression that the feedback assessment (Section 7.4.2) was independent of GCMs. That is incorrect as CMIP6 was used in the assessment (e.g., page 65). So "different approaches" were not used. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We explained our approach as "base not only on GCMs but also on theory, observations and high-res process modelling" at L.43-44.
10809	85	46	85	49	These two sentences give the impression that the Effective radiative forcing for a doubling of CO2 is independent of GCMs. That is not strictly correct. Adjustments applied to line-by-line models are deduced from GCMs such as CMIP (7.3.2 page 28:9-10). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We explained our approach as "base not only on GCMs but also on theory, observations and high-res process modelling" at L.43-44.
10805	85	47			Should this be "Table 7.10" not "Table 7.9"? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
46321	86	1	86	1	In the graph the upper limit is 6.3 degC. [Twan van Noije, Netherlands]	Taken into account. The number and figure have been revised slightly and they match each other.
16263	86	6	86	28	Since this is an assessment it might be worth noting here what the literature has assumed about uncertainty in dF_2xCO2 or its independence from alpha. For example the Sherwood et al review made the same assumption made here, and there may be a few previous ECS studies that have allowed for the uncertainty in dF_2xCO2 (though I think most treated it as known) and if so it would be worth knowing what they assumed especially if their estimates are being quoted in the chapter. [Steven Sherwood, Australia]	Taken into account. We explicitly stated that dF_2xCO2 and alpha are assumed independent (their uncertainty has been discussed in earlier sections). At the same time, we made a test estimate assuming that they have a covariance as seen in CMIP5/6 models. However, the physical processes responsible for the weak covariance is not well understood to date, our final assessment based on the process evidence have not included the covariance between dF and alpha.
2727	86	6	86	28	the final sentence seems inconsistent with earlier statements concerning statistical significance and correlations not being an artefact. [Bryan Weare, United States of America]	Taken into account. Even though the correlation in GCMs can be statistically significant at the 90 or 95% level, we assessed the co-dependence to have low confidence because of reasons explained at L.23-26.
33195	86	7	86	10	I might be mistaken but I had in mind that this anti-correlation between F and alpha is even weaker with the more complete set of CMIP6 models than was included in Zelinka et al (2020). It would be worth checking this against the most up to date data. [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	We have checked that the correlation coefficient in CMIP6, after adding more models than in Zelinka et al. (2020), was very close to CMIP5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
15997	86	8	86	9	It is not clear with r2 = 0.34 if there is correlation as indicated in the text, especially when this represents very weak correlation. To determine if correlation exists, then the number of data points need to be known and the question needs to be subjected to a t-test. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Andrews et al. used 15 CMIP5 models, Webb et al. used 11 CMIP3 models and PPEs, and Zelinka et al. used 27 CMIP6 models. Despite different number of samples and models (from CMIP3 to CMIP6), the correlation was very close and the value of r=0.58 is statistically significant at the 95% level when we treat each model as independent.
24199	86	11	86	14	I find the text on page 86 lines 11-14 difficult to parse. I believe that line 12 should read "assuming that they are correlated" and that line 14 should refer to the "red curve in Figure 7.21." [Mitch Bushuk, United States of America]	Taken into account. The text has been revised to address these issues.
71095	86	12	86	12	"assuming that they are not correlated" Is this correct? Or "they are correlated"? [Yu Kosaka, Japan]	Taken into account. The latter is correct, and the text has been revised accordingly.
46323	86	12	86	12	Change "assuming that they are not correlated" to "assuming that they are correlated". [Twan van Noije, Netherlands]	Taken into account. The text has been revised accordingly.
99545	86	12	86	13	Both alternatives in these lines are descibred used the "not correlated" assumption but one of them must assume the opposite [Robert Pincus, United States of America]	Taken into account. One of them at L.11 was a type of "correlated". This has been corrected.
64509	86	12	86	13	The wording here is very confusing. It sounds like a 14% reduction is found by assuming F and alpha are not correlated instead of independent... but doesn't independence imply lack of correlation? How are these cases different? Also, "not correlated" and "not adopted" are just really awkward wording. [Peter Caldwell, United States of America]	Taken into account. The sentence "not correlated" was a type of "correlated". This has been corrected.
13531	86	13	86	13	Add space between the matematical formula and the word “between”. [Maria Amparo Martinez Arroyo, Mexico]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
102099	86	17	86	17	Cite Ringer et al.(2014) (doi:10.1002/2014GL060347) for the results from prescribed SST and CO2 experiments. [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Ringer et al. paper was cited in the paragraph at three lines below.
46325	86	17	86	17	Change to "with prescribed SST" to "with prescribed SST and sea-ice concentration". [Twan van Noije, Netherlands]	Accepted.
51325	86	26	86	28	If these two paramaters are co-dependent what are the implications for ECS estimates? Could this please be elaborated upon here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. If we accept the co-dependence of r^2=0.34, the ECS range will be narrower by 14% but the central estimate does not change. This was explained at L.10-14.
46327	86	26	86	28	However, the process-based approach provides estimates of feedbacks for the present climate. However, evidence suggests that the net feedback become less negative as the climate warms, in which case ECS would be underestimated in this approach. Please clarify this. [Twan van Noije, Netherlands]	Taken into account. The feedback calculated using a regression to GCM data might be slightly underestimated by ignoring the state-dependence as you point out, but the effect tends to be compensated by another error in calculating ERF (p.57 L.32-48). We assessed the net effect is small and furthermore the assessment of the net climate feedback was base not only on GCMs but also other lines of evidence, so this problem will not matter.
64507	86	27	86	28	I disagree with the conclusion that forcing and feedback are most likely not correlated. This has been an extremely robust finding in climate models since CMIP3. Even if we still don't understand the result fully, the likelihood that all GCMs are wrong for reasons we don't understand is lower than the probability that this relationship is correct even though we still only partially understand the reasons. Note also that assuming F and alpha aren't correlated is inconsistent with p87 L21-23 and p90 L1-2, where the opposite assumption seems to be made. [Peter Caldwell, United States of America]	Rejected. We did not state that the co-dependence between F_2xCO2 and alpha identified in GCMs is unlikely, but rather assessed that the co-dependence to have low confidence (because of the physical reason not well understood and further not verified using other lines of evidence). Assumption of co-dependence on p.87 L.21-23 was between the fast and slow responses, and on p.90 L.1-2 was between F_2xCO2 and F(t). They are different from the assumption between F_2xCO2 and alpha.
3565	86	33	86	40	what are the numbers in white and black on the graph? [Joyce Penner, United States of America]	Accepted. Explained
27167	86	37	86	37	"we suggest to add "red" before "ellipse" [Eric Brun, France]	Accepted
18013	86	45	87	47	The document is really long, and it seems like we are in the weeds here. [Dennis Hartmann, United States of America]	Taken into account. The text has been a bit shortened.
83795	86	47	87	4	I'm a little confused by the boundary between "simple" climate models, models of intermediate complexity, and GCMs. Clearly the linear EBM that defines ECS is a simple model and a fully-coupled ESM is complex, but does the two layer EBM count as an EMIC? I'm just a little lost in the terminology here. [Marvel Kate, United States of America]	Noted. We did not call the two-layer EBM an EMIC but an emulator (cf. Chapter 4 Box 4.1) although the EMIC community may include the two-layer EBM.
3643	86	51	86	51	You might want considering adding something like. Recently, the exact mathematucal relationship between ECS and TCR has been elucidated in Ragone et al. (2016). [See https://link.springer.com/article/10.1007/s00382-015-2657-3] [Valerio Lucarini, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The formulation by Ragone et al. is too complex, and conventional two-layer energy balance model could connect ECS with TCR with parameters estimated from CMIP6 models in a simpler way. We therefore adopted this model.
46329	86	54	86	55	Please include reference to the paper by Winton, 2010: https://doi.org/10.1175/2009JCLI3139.1. [Twan van Noije, Netherlands]	Accepted
46331	87	3	87	4	Change "very low degrees of freedom" to "very low number of degrees of freedom". [Twan van Noije, Netherlands]	accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71097	87	32	87	32	"1.5 and 2.2 ºC" Is this a likely range? [Yu Kosaka, Japan]	Taken into account. The word "likely" has been inserted.
2729	87	35			define K and E [Bryan Weare, United States of America]	Taken into account. They were defined at L.7.
46333	87	36	87	38	How does this compare with the approximate relationship mentioned in line 23? [Twan van Noije, Netherlands]	Not applicable. The relationship between TCR and ECS at L.23 has been deleted as it is misleading and we actually did not use it.
68647	87	37	87	37	According to equation (4) in Jiménez-de-la-Cuesta and Mauritsen (2019), the expression of TCR should be $TCR \cong -\Delta F_{2\times CO2}/(\alpha \minus \kappa\varepsilon)$ （i.e. it is a '-' instead of '+' in denomination） [Jiacan Yuan, China]	Accepted. Thanks. The sign was corrected.
98331	87	38	87	41	I am missing the logic flow here. It is not plausible to me why the crude representation of multiple processes in EBMs causes challenges to constrain kappa and epsilon from observations. [Dirk Olonscheck, Germany]	Taken into account. The sentences have been rephrased.
10001	87	39	87	39	Another relevant reference for ocean heat uptake seems to be "A Conceptual Model of Ocean Heat Uptake under Climate Change", MARSHALL AND ZANNA 2014, J. Clim. [Nadir Jeevanjee, United States of America]	Rejected. This is a nice conceptual model study but does not fit the purpose of this section.
35129	87	45	87	47	This statement about the TCR being dominated by alpha the net climate feedback parameter rather than ocean heat uptake is consistent with diagnostics of 9 CMIP6 models by Williams et al. (2020) ERL. In fact, this statement can go further and actually the uncertainty in the physical climate feedbacks dominates the intermodel uncertainty in the TCRE. See Tables 2 and 3 in Williams, R.G., P. Ceppi and A. Katavouta (2020) Controls of the Transient Climate Response to Emissions by physical feedbacks, heat uptake and carbon cycling. Environmental Research Letters, doi:10.1088/1748-9326/ab97c9 [Richard Williams, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have cited Williams et al that supports the statement. Thanks.
71099	87	50	87	50	Why is this "1.5-2.4ºC" range different from the range given in the preceding paragraph (1.5-2.2ºC)? [Yu Kosaka, Japan]	Taken into account. The range of 1.5-2.2degC does not take into account uncertainty due to heat uptake, and our final assessment leads to a wider range of 1.5-2.4degC.
677	88	3	88	10	Figure 7.22 needs to specify the confidence bounds of the uncertainty shown [Bruce Wielicki, United States of America]	Accepted. Confidence bounds added.
10811	88	15	88	18	The title of this section and the introduction is inaccurate. As is subsequently explained in this section, ECS and TCR are estimated by using simple expressions or models, combined with estimated ERF, constrained to observational records. It is rather important to not give the false impression that ECS and TCR are observed quantities, unrelated to models of one kind or another. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Section title revised.
22203	88	15			Many if not almost all of the studies in 7.5.2 will have used old versions of surface temperature products which are now assessed (chapter 2) to have been low biased by a considerable margin. Has the impact of this estimation issue been fully accounted for in 7.5.2? [Peter Thorne, Ireland]	Noted. We are using updated temperature records in all of our calculations here, with references to Chapter 2.
99551	88	15			The title of this section could be revisited. The estimates of ECS and TCR developed here rely on a much wider range of observations and estimates that simply the historical temperature record [Robert Pincus, United States of America]	Taken into account. Section title revised.
99553	88	15			This section would benefit from the same high degree of polish as was applied to section 7.5.1. The same messages expressed in fewer words would be easier to follow [Robert Pincus, United States of America]	Taken into account. Section revised to be more concise.
65413	88	16	88	16	It would be better to discuss how the pattern effect can be broken down into two components: the forced and unforced parts. The unforced part reflects the impact of internal variability over the historical period: our historical climate record is just one of an infinity of possible trajectories, and these different trajectories can generate estimates of ECS. The second is the forced pattern effect, which reflects the difference between the average transient pattern over the 20th century and the equilibrium pattern. There are many papers that talk about the "pattern effect", but they often are only evaluating one part. By considering them seprately, you can make a better estimate of what the pattern effect is. I have a publication that quantifies these in a model ensemble: Dessler, A. E. (2020). "Potential problems measuring climate sensitivity from the historical record." Journal of Climate 33(6): 2237-2248. [Andrew Dessler, United States of America]	Taken into account. This difference between forced and unforced pattern effects is discussed in Section 7.4.4.3. This paper is now cited there. This section simply uses the overall assessment of the magnitude of the pattern effect, which includes both forced and unforced components.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65415	88	20	88	20	When the text says "There is high confidence that radiative feedbacks will become less-negative in the future …" this is really just referring to the FORCED pattern effect. In other words, if you run an ensemble of models over the 20th century, the average pattern of warming will give you an ECS about 10% less than the equilibrium 2xCO2 pattern. This calculation does not include the ACTUAL pattern that we have experienced. I think that there's good agreement in papers that this is about 10%. This is close the number that Lewis and Curry 2018 came up with, as well as Dessler, A. E. (2020). "Potential problems measuring climate sensitivity from the historical record." Journal of Climate 33(6): 2237-2248. I think it is correct that we have high confidence that this is a positive number. [Andrew Dessler, United States of America]	Noted. This refers to the full pattern effect (forced plus unforced), as estimated by AMIP simulations. We also have high confidence that the forced pattern effect alone is positive, and the full appears to be larger than this (though we can't separate forced from unforced in nature vary well.
697	88	25	88	25	section 7.5.2.1 gives a very clear and useful discussion of energy balance, TCR, and ECS [Bruce Wielicki, United States of America]	Noted. Thank you.
9683	88	25	91	28	The role of internal climate variability in such estimates is mentioned on lines 36-37 of page 84. But there is no discussion in the rest of the section as to whether internal climate variability is well accounted for in published estimates or if the current treatment could result in an underestimate or even a bias in the ECS estimates. [Olivier Boucher, France]	Taken into account. Revised to assess role of internal variability.
51387	88	27	88	27	Ensure that this sentence is exactly the same as in Chapter 2. This statement will a key one from the WGI report. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Checked and revised for consistency.
71755	88	27	88	28	The figure of warming since pre-industrial may now need to be updated to 1.1K? Check for consistency with Chapter 2. [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Checked and revised for consistency.
22195	88	27	88	28	It would be preferable here to provide the actual assessment result arising from chapter 2 and avoid the use of qualifiers such as small which could be accused of being subjective. [Peter Thorne, Ireland]	Taken into account. Checked and revised for consistency.
83797	88	27	88	42	"that are biased low" I'd replace this with "are biased low when they fail to take into account improvements…" and then "may be biased low due to a pattern effect". I think it's important to differentiate between biases due to known coverage issues and biases due to (model-dependent) evolution of SST patterns. [Marvel Kate, United States of America]	Taken into account. Revised to distinguish these different factors.
10813	88	28	88	31	Assumptions used in the forcing/temperature/feedback framework are also needed. This should be noted. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised.
51327	88	30	88	33	This is a very important point. Suggest it would be very helpful here to explain that a conceptual model is involved in sensitivity estimates based on the historic temperature record to avoid potential misconceptions that these estimates are based only on observations. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you. Yes, this is what these sentences are intended to convey.
103623	88	33	87	33	GCS inform? (meaning not clear (contain better) [Philippe Tulkens, Belgium]	Not applicable. Not clear what this comment is referring to.
83799	88	47	88	47	Please add the qualifier "if alpha remains constant with time"? [Marvel Kate, United States of America]	Accepted. Revised to note this.
10003	88	51	88	51	This expression for TCR neglects differences in forcing efficacy between CO2 and other agents contributing to the historical forcing, particularly aerosols. This expression also neglects the effects of differing deep ocean warming between the present day and year 70 of a 1%/yr simulation, which manifests as a reduced "efficiency" of ocean heat uptake. These errors and their significance are discussed in Winton et al. 2019, "Climate Sensitivity of GFDL's CM4.0". [Nadir Jeevanjee, United States of America]	Taken into account. Revised to note these caveats.
51329	89	4	89	27	This section states that a pattern effect biasing historical ECS estimates is low, however Lewis and Thoritsen find a minimal pattern effect over the historic period. Suggest that the findings of this study is also referenced here and some explanation provided of the relevance of these in context of a possible emergence of a pattern effect in the future. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. This study is referenced and assessed in Section 7.4.4.3, and along with other studies informs the overall assessment of the magnitude of the pattern effect that is used here.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87947	89	4	89	27	I find it difficult to reconcile the uncertainty and conflicting nature of the information here with your claim of High Confidence. Specifically, you are trying to make a case that the findings of Lewis and Curry 2018, Otto et al 2013, Skeie et al (2014) and others (most of which you don't discuss) who find a low ECS can be set aside because the feedback parameter -α will change over time to become -α+α' and α' >0. Specifically your argument is: "There is high confidence that radiative feedbacks will become less-negative in the future (α' > 0) owing to the fact that historical warming has shown relatively more warming in key negative feedback regions (e.g., western tropical Pacific Ocean) and less warming in key positive feedback regions (eastern tropical Pacific Ocean and Southern Ocean) than is projected in the near-equilibrium response to abrupt4xCO2....implying that the true ECS will be larger than the effective ECS inferred from historical warming." Paraphrasing, you are confident the models are right, namely that ECS is high, because the historically-observed warming gradient between the western and eastern tropical Pacific runs opposite to what models predict should have happened, therefore it will happen in the future, therefore the gradient will change, therefore ECS will go up. But isn't it also possible that the models simply get the gradient wrong? That is the argument in Seager et al. (2019), nature.com/articles/s41558-019-0505-x, who say "State-of-the-art climate models predict that rising GHGs reduce the west-to-east warm-to-cool sea surface temperature gradient across the equatorial Pacific. In nature, however, the gradient has strengthened in recent decades as GHG concentrations have risen sharply. This stark discrepancy between models and observations has troubled the climate research community for two decades... erroneous warming in state-of-the-art models is a consequence of the cold bias of their equatorial cold tongues. The failure of state-of-the-art models to capture the correct response introduces critical error into their projections of climate change in the many regions sensitive to tropical Pacific sea surface temperatures." They find that the historically-observed warming gradient is actually consistent with rising GHG levels, which implies it is not going to reverse. Consequently your argument in this section, upon which your main chapter conclusion rests, namely that α will start rising any day now so we'll assume it has already happened, is not only at odds with historical evidence but is based on projections of the Pacific temperature gradient from models now believed to be erroneous. [Ross McKitrick, Canada]	Noted. Please see Section 7.4.4, which assesses a wide range of studies on this topic from climate modelling, satellite observations, and paleoclimate proxy records. Taken altogether, the evidence provides medium confidence that the eastern tropical Pacific will warm as equilibrium is approached and high confidence that the Southern Ocean will warm as equilibrium is approached. If either of these happen, radiative feedbacks will become less negative compared to those over the historical period (alpha'>0), and thus we assign high confidence to this scenario. The fact that coupled models generally do not produce SST patterns that resemble recent observations is the reason that we rely on AMIP simulations with prescribed observed SSTs in our estimates of the pattern effect. Note also that we provide an estimate of ECS and TCR in the absence of any pattern effects; using up-to-date estimates of radiative forcing, ocean heat uptake, and global temperature, the values of ECS and TCR are higher than those in the studies mentioned. Consideration of the pattern effect increases confidence in the lower bound of ECS and TCR based on energy budget constraints, while decreasing confidence in the upper bound of ECS and TCR. Owing to the large uncertainty in the pattern effect, we can only use the lower bound estimates of ECS and TCR (assuming no pattern effect) from global energy budget constraints to inform our overall estimate of these quantities in Section 7.5.5.
87949	89	4	89	27	Further on the discussion of the tropics, you are basing your conjectures about future increases in ECS on the ability of models to represent the tropical climate accurately. But Chapter 2 acknowledges that models don't get the tropical troposphere correct, they systematically over-estimate warming trends there. Many papers have pointed this out. In McKitrick and Christy (2019) which AR6 Ch2 cites, we show that every run from every model in CMIP5 over-predicts warming in the 200-300 hPa layer where the feedback effect is supposed to be strongest, and in most cases the discrepancies are large and statistically significant. Yet here you are making statements with High Confidence that rely on models' ability to characterize accurately the feedback effect over the tropics. See McKitrick, 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. [Ross McKitrick, Canada]	Noted. Please see above comment.
24195	89	4	89	37	I suggest adding reference to Winton et al. (2020) in the discussion on page 7-89 lines 4-37. This study employs a perfect model approach to show that the effective ECS computed using energy budget methods does not provide a reliable constraint on the true ECS of the GFDL CM4.0 model. In the case of GFDL CM4.0, the energy budget method underestimates the true ECS by 3.2°C. Reference: Winton, M., Adcroft, A., Dunne, J.P., Held, I.M., Shevliakova, E., Zhao, M., Guo, H., Hurlin, W., Krasting, J., Knutson, T. and Paynter, D., 2020. Climate Sensitivity of GFDL's CM4. 0. Journal of Advances in Modeling Earth Systems, 12(1), e2019MS001838. [Mitch Bushuk, United States of America]	Accepted. Reference added.
46335	89	20	89	20	Remove hyphen in "less-negative". [Twan van Noije, Netherlands]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
78071	89	22	89	22	Please see comment on p83 line 30-31, which might apply better at this point - I'm not sure. [Jonathan Gregory, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Comment applies better to text above.
9685	89	24	89	24	Sure but your definition of ECS is for 2xCO2 and not for 4xCO2 equilibrium so why is this relevant? [Olivier Boucher, France]	Taken into account. Revised to say at equilibrium under CO2 forcing.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
28901	89	49			Paleodata also indicates a positive imbalance of around 0.2 Wm-2 sustained for many thousands of years since the last glacial: Baggenstos et al. 2019 PNAS https://doi.org/10.1073/pnas.1905447116 [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cited.
65417	89	52	89	52	I'm not 100% sure what "anomalous energy imbalance" means, but I'm guessing it's the change in TOA flux due to the pattern effect. If so, then this number includes both the TOA energy response to unforced climate variability. It would be useful to categorize the various numbers to keep clear whether they include forced, unforced, or both pattern effects. [Andrew Dessler, United States of America]	Taken into account. Revised to clarify that this is the anomalous global energy imbalance relative to the period 1850-1900. This comes from the observed energy imbalance estimated from section 7.2. Due to observational limitations, we are not able to separate this estimate into the different contributions mentioned.
69615	90	3	90	3	values a TCR' - word order? [Nicholas Golledge, New Zealand]	Accepted. Revised
116629	90	9	90	23	The argument developed here needs to be reflected in the corresponding chapter 2 box on the reasons and benefits for choice of temperature metrics. [Valerie Masson-Delmotte, France]	Taken into account. Text has now been substantially changed in light of revisions of cross Chapter Box in Chapter 2. Quantification removed
106331	90	9	90	43	This explanation of the differences between effective climate sensitivity measures and formal assessments of these is very valuable to support the public discussion around these concepts. Please do keep it for the FGD. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Explanation has been retained for the FGD.
10857	90	10	90	13	I think it should be clarified that there is incomplete coverage because there are places and times with no actual observations. The way it is written suggests some oversight by the creaters of the dataset! [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Clarified as suggested
10859	90	10	90	13	I think some historical context about the use of "temperatures measured below the surface" is needed (Jones AAS, 2016). It isn't done on a whim! The use of sea surface temperatures was considered a reasonable proxy for marine air temperatures (e.g., Jones et al, Evidence for global warming in the past decade. 1988), and the difference between century trends of SSTs and night-time marine air temperatures is not detectable (e.g. Cowtan et al (2015)). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has now been substantially changed in light of revisions of cross Chapter Box in Chapter 2
10815	90	13	90	15	No. The "16%" refers to an estimate of the reduction of the difference between simulated global surface air temperature and simulated blended land and sea surface temperature. The text needs to be amended to make it clear this is a model estimate, with appropriate caveats and assumptions (e.g., 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. Text has now been substantially changed in light of revisions of cross Chapter Box in Chapter 2
10817	90	13	90	15	The word 'around' is doing a lot of work in this sentence. Richardson et al (2018a) give an estimate of the reduction of CMIP5 historical+RCP26 trends of 16.2 (5.2-28.7)% between 1861-1880 to 2007-2016 periods. For the 2007-2016 to 2090-2099 period the estimate is 10.6 (1.2-29.7)%. Don't give an over confident representation of a result from a study. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has now been substantially changed in light of revisions of cross Chapter Box in Chapter 2. Quantification removed
10831	90	13	90	15	There is at least one global surface air temperature dataset that might provide a more appropriate estimate of the percentage difference between HadCRUT4 and a global air temperature dataset, that does not use climate models. (Rayner et al.. The EUSTACE project: delivering global, daily information on surface air temperature. Bulletin of the American Meteorological Society, 2020. In press.) Suggest finding out what they conclude? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has now been substantially changed in light of revisions of cross Chapter Box in Chapter 2. Quantification removed
18015	90	46	91	28	This is an important section. [Dennis Hartmann, United States of America]	Noted. Thank you.
65419	90	48	90	49	It's useful to point out that the Andrews et al. 2018 and Lewis and Mauritsen papers are only evaluating the UNFORCED pattern effect. So this should ADD to the forced pattern effect [Andrew Dessler, United States of America]	Rejected. This is incorrect. Andrews et al. 2018 and Lewis and Mauritsen 2020 evaluate the pattern effect in the context of AGCM simulations using observed SSTs and sea-ice concentrations, which includes contributions from both the forced response and unforced variability. Thus, they evaluate the total (forced plus unforced) pattern effect.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65423	90	50	90	50	I find the estimate of alpha-prime being 0.5 +/- 0.5 to be too low and too uncertain. We have high confidence that the forced patter effect is positive, and most analyses come up with about a 10% magnitude (perturbation on alpha). The unforced pattern effect can be much larger than this (Dessler et al., 2018), and the evidence we do have suggests strongly that it is also positive (Andrews, Gregory, Zhou, others), with a magnitude of 10-20%, giving us a total pattern effect of 20-30%. You'll have to convert this to W/m2/K units, but doing so quickly gives me a "likely" range of something like 0.30-0.45 W/m2/K. The Lewis and Maurtisen paper does argue that it's smaller, but I think that leaving it as "likely" gives the wiggle room if that paper turn out to be correct. [Andrew Dessler, United States of America]	Taken into account. This is our current best estimate of the forced + unforced pattern effect given the observed temperature trend pattern, and thus we have kept this assessment. Note that this value and range takes into account Lewis and Mauritsen 2020, which is why it is revised slightly lower than Andrews et al. 2018. Text revised to clarify that this is capturing both the forced and unforced components simultaneously.
46337	90	52	90	52	It seems the corrected range is based on the implicit assumption that alpha' is not correlation with any of the other relevant parameters. Please clarify to what extent relaxing this assumption would change the results. [Twan van Noije, Netherlands]	Noted. alpha' is not significantly correlated with alpha or F2x within CMIP5 or CMIP6 models, so we make the assumption of no correlation here.
83801	90	52	90	53	What accounts for the difference between these values and Sherwood et al (submitted), which finds a maximum likelihood ECS of 3.8K with uncertainty of 2.8 to 18.6 K? The Sherwood et al paper also assumes alpha' = 0.5 W/m2K +/- 0.5. [Marvel Kate, United States of America]	Noted. The updated Sherwood et al. 2020 values appear to be 4.3K with a 5-95% range of 2.0-16.1K, which is in good agreement with the range assessed here. The differences arise from slightly different values for radiative forcing and global temperature change, combined with different methodological choices for how the energy budget equation is applied (Bayesian in Sherwood et al. versus non-Bayesian here; see Sherwood et al. 2020 for details).
103625	90	53	90	53	It could be really useful if the extreme upper limit of ECS of 19.7 degrees was commented more, e.g. where on the planet would such an excessive ECS be predicted?. It would highlight the uncertainty related to the alpha'-terms. [Philippe Tulkens, Belgium]	Noted. The extremely high value simply means that high ECS cannot be ruled out based on historical global energy budget constraints alone. See Section 7.5.5 for the overall assessment of ECS based on multiple lines of evidence, producing a very likely range of 2-5C. Note also that ECS refers to the global average temperature change under a CO2 doubling, so there will be some places on the planet that warm by more and some that warm by less than this value.
65421	90	54	90	55	This estimate that alpha-prime is 0.1 from Andrews et al., 2019; Armour, 2017; Dong et al., submitted; Lewis and Curry, 2018. Are all estimates of the FORCED pattern effect. This leaves out any contribution of the unforced pattern effect due to internal variability of the climate system, which papers like Gregory and Andrews 2016 and Zhou et al. 2016 and Andrews et al. 2018 show is an additional low bias. Dessler 2020 (Dessler, A. E. (2020). "Potential problems measuring climate sensitivity from the historical record." Journal of Climate 33(6): 2237-2248.) evaluated the two terms separately and found that (in one model) they add to produce a larger pattern effect. The total pattern effect should be the sun of the forced and unforced part. [Andrew Dessler, United States of America]	Taken into account. Text revised to discuss the magnitude of the forced pattern effect, the role for internal variability, and the best estimate of the forced + unforced pattern effect.
83803	90	55	91	4	Optional suggestion: Assuming α' = 0.1 ± 0.3 W m–2 °C–1, this implies a substantial probability that the pattern effect acts to *stabilize* feedbacks (ie make them more negative). I think it's worth emphasizing this even more- the manuscript notes weak dependence on the value of alpha' when ECS is small, but it's a bit counterintuitive (but correct) that the lower bounds on ECS can't be revised down even if the pattern effect works the other way. [Marvel Kate, United States of America]	Taken into account. Revised to emphasize this important point.
13533	91	1	91	1	Homogenize numeration. Change “iv” for “4”. [Maria Amparo Martinez Arroyo, Mexico]	Accepted.
46339	91	11	91	11	Remove hyphen in "more-negative". [Twan van Noije, Netherlands]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
71101	91	11	91	11	"iv)" -> "(4)" [Yu Kosaka, Japan]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
16233	91	13			The Sherwood et al. WCRP study did include all these factors (albeit not using exactly the same numbers for all of them). True it isn't yet published… [Steven Sherwood, Australia]	Not applicable. Text removed.
9687	91	15	91	17	Then ECS inferred from the historical global energy budget is not ECS. Maybe rephrase as "… that the true (or canonical) ECS is larger than estimated inferred from …" [Olivier Boucher, France]	Taken into account. Revised to clarify this.
16229	91	15			This needs rewording to specify that that what we are comparing to here is a traditional/naïve EB approach [Steven Sherwood, Australia]	Taken into account. Revised to clarify this.
65425	91	17	91	19	"The accuracy ... hinges on ..." No, I don't think this sentence is correct. I think the main problem is with the unforced pattern effect, not the forced pattern effect. In other words, how representative is the observed pattern of warming compared to an average of a theoretical ensemble of 20th century warming patterns? This is the huge uncertainty identified by Dessler et al. 2018. The forced pattern effect is much smaller than this. [Andrew Dessler, United States of America]	Rejected. The text is correct as written: alpha' depends on (i) the difference between historical and future warming and (ii) the radiative response to that. We need GCMs to estimate both of these things, so alpha' does indeed hinge on these things being accurate.
46341	91	23	91	23	The formulation is confusing, as "it" seems to refer to "the lower bound of ECS". Please change "it" to "ECS". [Twan van Noije, Netherlands]	Taken into account. Revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83601	91	25			7.5.2 Estimates based on the historical temperature record Page 7-91 line 25 author reviews multiple sources and concludes "Estimates of α' that are informed by 24 idealized CO2 forcing simulations of coupled GCMs (Andrews et al., 2019; Armour, 2017; Dong et al., 25 submitted; Lewis and Curry, 2018) indicate a median value of ECS of around 3°C while.." This is quite misleading. Lewis and Curry 2018 conclude "a median of 1.66K for ECS (5%–95% range: 1.15–2.7 K) …. These ECS estimates reflect climate feedbacks over the historical period, assumed to be time invariant. Allowing for possible time-varying climate feedbacks increases the median ECS estimate to 1.76K (5%–95% range: 1.2–3.1 K),…" The author may well disagree with Lewis and Curry, but the report should quote Lewis and Curry correctly. I would argue that the ECS from Lewis and Curry deserves some mention for its lower value. Is it just coincidence that their lower value is consistent with some results from studies which include natural cycles in climate models ? eg Asten 2012 ECS 1.1; Abbot and Marohsy ECS=0.6; Scafetta ECS=1.5; (see Reviewer Comment on Table 7.11; Table 7.12; Table 7.13; for details). The section omits entirely a body of literature which seeks to incorporate observed natural cycles of the past 2000 years into climate models and consequent predictions of climate to 2100CE. The common thread is that when the natural cycles are admitted, then part of the temperature increase since 1850 is attributable to those natural cycles, and the resultant estimate of ECS is reduced relative to the AR5 range. The following is a very detailed study using a wide range of CMIP5 models; the same author has a significant number of related papers, none of which appear to be referenced in AR6: 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 A few other useful references are	Noted. This section follows the methodology of Otto et al. 2013 and Lewis and Curry 2018, but uses updated estimates of surface temperature change, global energy imbalance, and ERF, thus arriving at an updated estimate of the effective ECS and TCR.
37187	91	31	92	25	Models NEVER provide evidence unless it can be shown that the models are accurate in every regard. Climate models are not accurate, so this section is dishonest. [John McLean, Australia]	Rejected. No models are accurate in every regard, but they are useful for their intended purpose here.
83603	91	31			7.5.2.2 Estimates based on simple climate models p7-91 line 31 The section omits entirely a body of literature which seeks to incorporate observed natural cycles of the past 2000 years into climate models and consequent predictions of climate to 2100CE. The common thread is that when the natural cycles are admitted, then part of the temperature increase since 1850 is attributable to those natural cycles, and the resultant estimate of ECS is reduced relative to the AR5 range. The following is a very detailed study using a wide range of CMIP5 models; the same author has a significant number of related papers, none of which appear to be referenced in AR6: 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 A few other useful references are 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 Scafetta, N., , Aberto Mirandola2*, Antonio Bianchini, 2017, Natural climate variability, part 1: Observations versus the modeled predictions , INTERNATIONAL JOURNAL OF HEAT AND TECHNOLOGY Vol. 35, Special Issue 1, September 2017, pp. S9-S17 . DOI: 10.18280/ijht.35Sp0102 Abbot, J. and Jennifer Marohasy, 2017, The application of machine learning for evaluating anthropogenic versus natural climate change , GeoResJ 14 (2017) 36–46 V. V. Babich, 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 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. [michael asten, Australia]	Rejected. This suggested literature is off topic for the evaluation here on anthropogenic forced response

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38589	91	31			There is a large range of evidence cited in this report and it is important to maintain coherency across sections. Section 7.5.2.1 was very thorough and well explained, especially the pattern effect. It is then followed by this section, 7.5.2.2. which catalogues estimates based on SCM fits to the observational record. These all (I think) assume constancy of feedback parameter and ECS, yet there is no mention of the 'pattern effect' covered by section 7.5.2.1. Please can a clarifying sentence be added to this section to caveat against the lack of accounting for a pattern effect, alpha'. [David Sexton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised to clarify.
35855	92	3	92	5	"suggesting that the results depend on the details of vertical heat transport in the ocean" is not correct. It is not due to details of the vertical heat transport. Observational data on OHC below 700 meter was not added in the analysis for this estimate. As a large fraction of heat has recently been stored in the ocean below 700 meter, this heat has to be included in estimates of climate sensitivity. I will suggest to delete the whole sentence. [Ragnhild Skeie, Norway]	Taken into account. Text revised.
71103	92	5	92	8	Though Johansson et al. (2015) refers to "El Niño/Southern Oscillation-related variability", "El Nino-Southern Oscillation and Pacific Decadal Variability" would be more relevant here instead of ENSO solely, since in this report ENSO is defined as interannual variability (frequency < ~10 yrs). Also please cite the Technical Annex. [Yu Kosaka, Japan]	Taken into account. Revised, citation to Technical Annex added.
10819	92	12	92	15	Is this relevant when referring to simple climate models, which can't simulate such nuances as the difference between surface v air temperatures? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised to remove this statement.
35857	92	14	92	15	"may be biased low". In Skeie et al 2018, an estimate of "true ECS" based on the infered effective climate sensitivity was provided. [Ragnhild Skeie, Norway]	Taken into account. Text revised.
51331	92	22	92	25	The Schwartz (2018) paper mentioned earlier is outside this range. In addition to describing it, please explain why it is such an outlier. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Explanation provided in preceding paragraphs.
10821	92	28			Title of section is inaccurate, "and simple models of global energy balance" should be added. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Section title revised.
28899	92	43		55	The papers by Brown et al (2014 GRL, 2016 J. Clim) seem relevant http://onlinelibrary.wiley.com/doi/10.1002/2014GL060625/abstract, http://dx.doi.org/10.1175/JCLI-D-15-0384.1 particularly in relation to albedo feedbacks [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cited.
16231	93	20			Shouldn't this be more specific e.g. "likely to be *significantly* different"? They are certainly different at some level. [Steven Sherwood, Australia]	Taken into account. Text revised.
5167	93	21	93	22	This sentence “It is also a challenge..” could be moved to before the previous sentence. [Daniel Murphy, United States of America]	Accepted. Revised as suggested.
93723	93	21			Cite Ceppi and Gregory 2019 here (doi: 10.1007/s00382-019-04825-x) as they also found a difference in feedback parameter for volcanic forcing compared with CO2. [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Reference added.
71105	93	23	93	24	The assessment text "Estimates based on the response to volcanic eruptions … do not constitute a direct constraint on ECS" is qualitative, so confidence assessment would be more appropriate. [Yu Kosaka, Japan]	Taken into account. Text revised.
129057	93	27	93	42	This subsection 7.5.2.4 appears quite thin compared to other subsections. It does not cite any publications to support the conclusions. And it seems to be a summary of 7.5.2, which has a similar title. One solution is to remove the subtitle of 7.5.2.4 and mark these paragraphs as a summary. In fact, line 9-23 on page 90 is quite relevant to the subject of using historical temperature record to estimate ECS and TCR. It explains the low bias of GSAT trend in HadCRUT4 contributed to the lower ECS in AR5. That paragraph may be moved here in 7.5.2. [Trigg Talley, United States of America]	Taken into account. This subsection is meant as a summary of previous subsections and to provide an overall assessment based on these lines of evidence. The text has been revised to clarify this purpose.
18017	93	27	93	42	It seems like we already had this discussion, It seems redundant. [Dennis Hartmann, United States of America]	Taken into account. This section summarizes the previous subsections and provides an overall assessment based on these lines of evidence. The text has been revised to clarify this purpose and to avoid redundancy where possible.
10823	93	27			Title of section is inaccurate. It is rather important to not give the false impression that ECS and TCR are observed quantities, unrelated to models of one kind or another. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Revised.
22197	93	27			Section 7.5.2.4 if, as I assume it is a summary of the preceding subsections should make this clearer both in the title and the opening sentences of the piece because I started out wondering where the supporting references were and only latterly twigged it was meant to be summarising what had come before. [Peter Thorne, Ireland]	Taken into account. Revised to clarify.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99555	93	27			As with the overall section, the title here could be revisited [Robert Pincus, United States of America]	Taken into account. Revised.
51333	93	29	93	42	This section states with very high confidence, based on evidence from the historical record, that ECS is extremely likely higher than 1.9C. However, one of the key papers for estimations based on the historical record is Lewis and Curry 2018, which has a median ECS lower than 1.9. This section makes the point that such estimates are likely biased low because of the pattern effect (the magnitude of which is also uncertain). A number of the papers cited here do not agree with this ECS value and partly require an argument to be made about biases to justify the confidence statement assigned. Please clarify if this agreement would still hold with a small pattern effect, and if yes, please clarify the reasons for this. In addition, please clearly outline the reason for very high confidence, extremely likely and high agreement statements overall here given that several references in this section seem to disagree with these conclusions (the same general points apply to TCR here). [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text in this section has been revised to better explain why the effective ECS, TCR, and ECS ranges are different from those of previous studies, including Lewis and Curry 2018. The pattern effect plays a role, but even in the absence of a pattern effect (alpha'=0), updated records of temperature, ERF, and global energy imbalance lead to higher assessed values.
34411	93	32	93	34	To help with traceability and transparency, it would help to provide some connection to the prior subsections for the assessment conclusion on the one-sided (e.g. > 2.6C) assessment of probability and its distribution. [Haroon Kheshgi, United States of America]	Taken into account. Revised to reference subsections above and to clarify the purpose of this subsection as providing an overall assessment based on those subsections.
65427	93	34	93	34	the statement that "ECS is likely greater than 2.6 C" actually implies a value of alpha-prime that's greater than zero. So you should be able to back out an alpha-prime value that it is likely greater than. I strongly suggest doing that and replacing the 0.5 +/- 0.5 value. [Andrew Dessler, United States of America]	Taken into account. The values and confidence ranges given here correspond to zero pattern effect. This has been clarified.
46343	93	36	93	37	Maybe change "owing to limited evidence" to "owing to limited evidence and methodological limitations"? [Twan van Noije, Netherlands]	Taken into account. Text revised.
10825	93	45			Title of section is inaccurate. It is rather important to not give the false impression that ECS and TCR can be deduced from proxies of paleoclimate in isolation, unrelated to models of one kind or another. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - the use of "based on" makes it clear that it is not only raw paleo data that is used. The use of "largely independent from" makes it clear that it is not entirely independent of the same tools that are used to inform other estimates of ECS.
116631	93		93		I am striving to find an assessment of the response of CMIP6 models to volcanic eruptions and insights on feedbacks in the AR6 WGI drafts (even if they are different from those acting on longer term responses). It is relevant for the discussion in ch 4 on possible effects of future eruptions. The statement here needs to be nuanced with respect to the use of Pinatubo for an emergent constraint in section 7.5.4.1. [Valerie Masson-Delmotte, France]	Taken into account. We did not have the room in our chapter to assess the CMIP6 response to volcanic eruptions. However, this paragraph was revised to differentiate what can be learned about ECS from observing the direct response to volcanic eruptions discussed here from what can be learned from emergent constraints (pointing readers to Section 7.5.4.1).
27169	94	1	94	3	ESM feedbacks could also be included in paleoclimate estimates and not in model estimates. This should be discussed here as it can potentially explain quite a bit of the difference beyween paleoclimate estimates and other estimates. [Eric Brun, France]	Accepted - text revised.
9689	94	1	94	3	ESM feedbacks could also be included in paleoclimate estimates and not in model estimates. This should be discussed here as it can potentially explain quite a bit of the difference beyween paleoclimate estimates and other estimates. [Olivier Boucher, France]	Accepted - text revised.
83805	94	6	94	6	lays -> lies [Marvel Kate, United States of America]	Taken into account - lays-> lay.
707	94	13	94	13	Add a new reference in this list: Snyder 2019. Full reference: Snyder, C. W. Revised estimates of paleoclimate sensitivity over the past 800,000 years Climatic Change, 2019, 156, 121-138, doi: 10.1007/s10584-019-02536-0. [Peter Köhler, Germany]	Accepted - text revised
83515	94	14	94	15	de la Vega et al. (submitted) -see line above- could also be added here. [Antje H. L. Voelker, Portugal]	Accepted - text revised
83807	94	31	94	54	Is it worth at least mentioning here the challenges in calculating ERF from paleoclimate data as another source of uncertainty? We have no proxies for adjustments to orbital and ice sheet forcing that don't depend on surface temperature, and thus estimating the ERF requires GCMs. [Marvel Kate, United States of America]	Accepted - text revised
83517	95	2	95	3	You could check if this recent paper on a long-term modeling study would also fit the criteria: Willeit, M., Ganopolski, A., Calov, R., Brovkin, V., 2019. Mid-Pleistocene transition in glacial cycles explained by declining CO2 and regolith removal. Science Advances 5, eaav7337, doi: 10.1126/sciadv.aav7337. [Antje H. L. Voelker, Portugal]	Rejected - this paper does not provide estimates of ECS using paleo data. It is a very interesting study, but not relevant here.
709	95	3	95	3	Add 2 new references to this list: Snyder 2019. and Friedrich & Timermann 2020. Full references: Friedrich, T. & Timmermann, A. Using Late Pleistocene sea surface temperature reconstructions to constrain future greenhouse warming Earth and Planetary Science Letters, 2020, 530, 115911, doi: 10.1016/j.epsl.2019.115911. Snyder, C. W. Revised estimates of paleoclimate sensitivity over the past 800,000 years Climatic Change, 2019, 156, 121-138, doi: 10.1007/s10584-019-02536-0. [Peter Köhler, Germany]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68895	95	8	95	9	If this Liu et al., 2014 citation refers to the Holocene temperature conundrum paper (doi: 10.1073/pnas.1407229111), then I don't think it says what this phrase states. I think it says the opposite, at least from the model simulations. [Darrell Kaufman, United States of America]	Accepted - text revised. This was based on Figure 2a in their Supp info - cyan line shows an orbit-only signal of about 1 degree C in the global mean. However, we agree that this is not huge, so removed "relatively large".
100687	95	25	95	25	Note: Check values for pCO2 against other sections and chapters [Matthew Kohn, United States of America]	Accepted - taken these out, and time-period definitions, so people can refer instead to Cross-Chapter Box 2.1.
100689	95	25	95	25	Note: I couldn't find where section 5.1.3.1 contains this information [Matthew Kohn, United States of America]	Not applicable - sentence removed
100691	95	25	95	25	Add: "..of modern climate sensitivity" [Note: ECS can be defined for any time, but is it relevant to future warming if it's for, say, the Eocene?] [Matthew Kohn, United States of America]	Rejected - We think this is clear from the context. The usage of "ECS" means this is modern by definition.
100693	95	34	95	34	Add: "...time, in the middle Miocene (about 16 million years ago) and Eocene (about 50 million years ago)..." [Matthew Kohn, United States of America]	Rejected - insufficient studies at this time to include the Miocene in this section.
100695	95	36	95	36	Add references: Steinthorsdottir et al. (in press, Paleoceanography and Paleoclimatology), Goldner et al. (2014, Clim Past), Royer (2016; AREPs) [Matthew Kohn, United States of America]	Rejected - insufficient studies at this time to include the Miocene in this section.
711	95	50	95	50	Add 2 new references to this list for "glacial/interglaicl" changes: Snyder 2019. and Friedrich & Timmermann 2020. Full references: Friedrich, T. & Timmermann, A. Using Late Pleistocene sea surface temperature reconstructions to constrain future greenhouse warming Earth and Planetary Science Letters, 2020, 530, 115911, doi: 10.1016/j.epsl.2019.115911. Snyder, C. W. Revised estimates of paleoclimate sensitivity over the past 800,000 years Climatic Change, 2019, 156, 121-138, doi: 10.1007/s10584-019-02536-0. [Peter Köhler, Germany]	Accepted - text revised
116633	95		95		How is LGM dust feedback addressed here? [Valerie Masson-Delmotte, France]	Taken into account. The definition of ECS has been extended to include ESM feedbacks, including dust, but we now add a note that dust is included as a forcing in some studies.
116635	95		95		Could it be possible to develop one table related to past periods used in this chapter, and related insights, to have an easy to find overview (Table 7.11 describes individual studies, but a complementary approach could be looking at specific past periods, and related aspects). Also, building on outcomes from ch 2 and 5 first, and then adding the perspective of RF and temperature response, would help save space and avoid duplication. In fact, I would suggest to place insights for sensitivity in x chapter boxes on these past warm phases, and discuss how to combine insights from various past periods in this section, having in mind the question of the state dependence. [Valerie Masson-Delmotte, France]	Rejected - there is a cross-chapter box on the Pliocene, but not other time periods, and we think it is too late to add them at this stage given that there will not be any further public review.
20087	96	6	97	6	The star shown in the top left case may not be useful as it appears nowhere else. The note about S might indicate that the classification due to Rohling et al is built upon the considered climate components for their radiative roles. [philippe waldteufel, France]	Accepted - table revised
27171	96	7	96	14	It would be useful to indicate which climates are warmer and colder than pre-industrial as the estimates of the ECS may depend on the sign of the perturbation. [Eric Brun, France]	Taken into account - added a reference to Cross-Chapter Box 2.1 which gives a definition of time periods.
9691	96	8	96	15	It would be useful to indicate which climates are warmer and colder than pre-industrial as the estimates of the ECS may depend on the sign of the perturbation. [Olivier Boucher, France]	Taken into account - added a reference to Cross-Chapter Box 2.1 which gives a definition of time periods.
96725	96	8	96	16	Column (5) of Table 7.11 shows a lot of upper ECS ranges well beyond 5.4°C. Please explain how this relates to the reasoning on page 7-98 that 5°C must be the upper limit for ECS. [Nicole Wilke, Germany]	Accepted - text revised. Highlighted more clearly that we are relying more on the warm climate estimates for the upper-end, due to higher confidence in these. In addition, there is now one more study (Inglis et al) that further supports this range.
18019	96	8	97	4	The paleoclimate estimates span a very large range and do not seem to constrain climate sensitivity, since the range of estimates is wider than than 1975 estimate of the range. [Dennis Hartmann, United States of America]	Rejected - many independent estimates with relatively wide uncertainty ranges can combine to produce an overall estimate with relatively narrower uncertainty ranges.
68897	96	8			Table 7.11. "S" is used to represent a different variable in columns 3 versus column 4. [Darrell Kaufman, United States of America]	Accepted - text revised
68899	96	8			Table 7.11 column 1: what is the asterisk designation is not used. [Darrell Kaufman, United States of America]	Accepted - text revised
68901	96	8			Table 7.11 column 6: Change column heading from "range accounts for uncertainty in:" to "range accounts for:". This is because the values for the ranges in some studies (e.g., Royer 2016 for the Pliocene) includes multiple intervals, which is not "uncertainty" but actual/certain variation among subintervals. [Darrell Kaufman, United States of America]	Accepted - text revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83607	96	8			Table 7.11 p 7-96 line 8 Missing estimate from the Eocene-Oligocene boundary , which yields ECS= 1.1 ± 0.4 ◦C (66 % confidence) Asten, M.W., 2012, Estimate of climate sensitivity from carbonate microfossils dated near the Eocene-Oligocene global cooling. Climate of the Past Discussions, 8, 4923-4939, doi:10.5194/cpd-8-4923-2012, online at http://www.clim-past-discuss.net/8/4923/2012/cpd-8-4923-2012.pdf Missing item from Abbot and Marohasy 2017 ECS = 0.6K Abbot, J. and Jennifer Marohasy, 2017, The application of machine learning for evaluating anthropogenic versus natural climate change , GeoResJ 14 (2017) 36–46 [michael asten, Australia]	Rejected. The first paper cited is a submitted paper that was not published in the peer-reviewed literature as far as we can tell. The second paper uses an approach of estimating natural variability over the initial period of the record and then extrapolating this forward based on machine learning. As far as we can tell this does not account for the variable forcing during the pre-industrial period, instead assuming that all climate variations are stochastic. Furthermore, a limited range of sites, coupled with a lack of clarity over the selection of these sites, coupled with a lack of out-of-sample testing of the methods, results in an estimate of the residual warming that is highly uncertain. In addition, the relationship between the local temperatures and the global mean temperature, and then to ECS, is also not made clear in the paper. Overall, we feel that this second study has a level of uncertainty that is likely very high, and not quantified in the paper, meaning that it is not possible to use in this assessment.
713	96	16	97	1	Changes to Table 7.11: In Köhler et al (2018) column 6 change in "Range of 3 different temperature reconstructions" (instead of 2) [Peter Köhler, Germany]	Accepted - text revised
715	96	16	97	1	Changes to Table 7.11: Add new line: Friedrich & Timmermann (2020) | Last glacial cycle | CO2: ice cores; T: SST stack from 64 cores and climate model | S[GHG,LI,AE] | 4.2°C (3.4-6.2°C) | range from 25 transient simulations . Full ref: Friedrich, T. & Timmermann, A. Using Late Pleistocene sea surface temperature reconstructions to constrain future greenhouse warming Earth and Planetary Science Letters, 2020, 530, 115911, doi: 10.1016/j.epsl.2019.115911. [Peter Köhler, Germany]	Accepted - text revised
717	96	16	97	1	Changes to Table 7.11: Add new line: Snyder (2019) | Warm states of glacial-interglacial cycles of last 800 kyrs | CO2: ice cores; T: SST stack from 61 cores | S[GHG,LI,AE,VG] | 3.1°C (0.7-7.0°C) | 95% CI. Full ref: Snyder, C. W. Revised estimates of paleoclimate sensitivity over the past 800,000 years Climatic Change, 2019, 156, 121-138, doi: 10.1007/s10584-019-02536-0. [Peter Köhler, Germany]	Accepted - text revised
68903	97	3	97	8	Mixing two probability levels (likely and very likely) and two directions (less than and greater than) seems unnecessarily complex and potentially confusing. Why not phrase as, "very likely higher than 2C and very unlikely higher than 7C"? Or if < and > is preferred then why not stick with "very likely" for both (very likely <7C)? This also avoids underplaying the upper end of the estimates from the assessed literature, nearly all of which extend beyond 5C. This also avoids issues with column headings in Table 7.13. The fact that the value for the upper bound is uncertain and more variable among studies is expressed by the confidence level. [Darrell Kaufman, United States of America]	Rejected - The upper and lower bounds were each assessed separately, based on a careful consideration of the uncertainties in (and the independence of) the various studies, and we don't think there is a problem with having different likelihood and confidence ranges for each.
51335	97	6	97	6	Nic Lewis had a paper trying to incorporate palaeo data into his estimates of sensitivity. For completeness sake it may be worth considering whether to evaluate this as well https://link.springer.com/article/10.1007/s00382-017-3744-4 [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Lewis and Grunwald (2018) is a paper primarily about methods for combining lines of evidence. The paleo data itself used in the Lewis paper is simply the AR5 paleo estimate itself, so nothing new to assess.
17361	97	9	97	9	Should it be Table 7.11? Otherwise Table 7.13 would be referred to before Table 7.12 [David Neubauer, Switzerland]	Accepted - text revised
719	97	9	98	16	The concluding estaimte that ECS was never below 2°c and less likely above 5°C needs to be adjusted for the range found in Snyder 2019, that reaches from 0.7°C to 7°C, violating both boundaries. The Snyder approach take core on uncertainties in a more rigorous way than before. Full ref: Snyder, C. W. Revised estimates of paleoclimate sensitivity over the past 800,000 years Climatic Change, 2019, 156, 121-138, doi: 10.1007/s10584-019-02536-0. [Peter Köhler, Germany]	Rejected - Snyder et al give a likely range for ECS of 2.6 to 3.7, which is consistent with many of the other studies: " The median paleoclimate sensitivity parameter estimate (S[GHG,LI,AE,VG]) of 0.84 °C/W/m2 and the 67% likelihood range of 0.69 to 1.0 °C/W/m2 for interglacial periods and intermediate glacial climates "
83609	97	9			P7-97 line 9 says "9 None of the post-AR5 studies in Table 7.13 [**NB should that table number be 7.11??] have an estimated lower range for ECS below 2.0°C per CO2 doubling. Although.." The statement is very misleading since post AR5 studies quoted in these review comments do in fact yield ECS<2K. Thus the table must be corrected with missing published values, and the summary statement above should be corrected. [michael asten, Australia]	Taken into account - see response to Comment ID 83607.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
22199	97	13	97	20	Equally, these results would support the contention that the feedback weakens at higher mean states that was concluded in an earlier section of the chapter. I suspect it is worth considering teasing this out a little further? [Peter Thorne, Ireland]	Rejected - Section 7.4.3 assesses that ECS increases with temperature, not decreases. The ECS studies based on glacial-interglacial cycles in general provide evidence for greater climate sensitivity in warmer interglacials than in cooler glacials, but their interglacial values are relatively high compared with estimates from high-CO2 states. Because the methods are very different in the glacial-interglacial studies compared with high-CO2 studies, we don't think they can be used to assess state-dependence - that requires similar methods across multiple time periods.
100697	97	16	97	16	Note: the ECS for the Eocene spans 16.3 million years, from the EECO to the late Eocene. While the value is accurate, is it comparable to other values in the table, which span much less time? Royer (2016) documents numerous other calculations of ECS (in addition to the Pliocene example farther down the table) that span less time. [Matthew Kohn, United States of America]	Rejected - while the EECO example does span a relatively large time, it is also a large signal in CO2 and temperature, so temporal variability is relatively less crucial than for other time periods. We highlight the Pliocene in Royer et al because it is given special treatment in that paper (see their Section 4), more than other time periods.
100699	97	16	97	16	Note: For the MCO, the models of Frigola et al. (2018) imply ECS of 2.5 °C, and Burls et al. (in review) imply an ECS of ~3.6 °C [Matthew Kohn, United States of America]	Rejected - Neither Frigola or Burls give an ECS estimate based on proxies. These are both model-based sensitivities. Burls et al was not published in time.
100701	97	16	97	16	Note: A value of ECS across the Miocene Climatic Transition (14.7 to 13.7 Ma) could be calculated (it's implicitly in Royer, 2016), but that's a long span of time, and I think that's not really what this table is supposed to show. I can be convinced otherwise. [Matthew Kohn, United States of America]	Rejected - insufficient studies at this time to include the Miocene in this section.
84851	98	7	98	7	If ECS is very likely greater than 2, what upper limit on CO2 it implies to restrict warming to within 1.5 C [Jayaraman Srinivasan, India]	Rejected - this is not the subject of this section. It is an interesting question but one that is not addressed here.
51337	98	9	98	10	Suggest that in emphasing the findings by Sherwood et al here, it would also be relevant to reference Rohling et al (https://www.nature.com/articles/nature11574) who also found similar values. Please also elaborate on the significance of this approach to clarify the level of prominence given to it here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected - Here we are assessing post-AR5 science so not sure it is relevant to compare here with Rohling et al which is included in the AR5 assessment, and the findings of which are summarised at the beginning of this section. There is already a long discussion of the strengths and weaknesses of the paleoclimate approach at the beginning of this section.
16235	98	10			Likelihoods are not distribuitions so it is not really appropriate to quote percentile values. I realise you don't want to go down the Bayesian rabbit-hole here, but I think trying to do detailed comparisons of those likelihoods with the conditional PDFs being asserted here is probably unwise. In particular I believe you are effectively applying a prior to our result (uniform in ECS) that is different from the one you've implicitly used to obtain your conditional PDFs (where you combine dF and alpha posteriors in a way that is effectively like a uniform prior on those variables, hence highly nonuniform in ECS). You could instead simply do as you have done in the previous section and point out that your result is roughly consistent with the WCRP one rather than attempting a precise comparison. [Steven Sherwood, Australia]	Accepted - text revised. Made it clear that a direct comparison is not possible, but that the results of the assessment are "broadly comparable".
5173	98	13	98	24	This paragraph seems out of place, as if it was written separately and not yet merged into the chapter. There was an entire section 7.5.2 on the instrumental record constraining ECS. [Daniel Murphy, United States of America]	Taken into account. The paragraph has been revised and expanded considerably and should now be better integrated in the emergent constraint section.
16237	98	15			Not true that Sherwood et al. did not account for ice sheet efficacy, it was included in the ice-sheet radiative forcing uncertainty, based on expert judgment and consideration of many published studies (which led to a smaller correction than Stap et al.). The most likely reason you and we have come up with different values is that yours was a qualitative judgment based on looking at the studies, while we did a quantitative calculation analogous to what you did for the historical record, and which treats the LGM and MPWP periods as independent (it is unclear from your text to what extent you do this in reaching your judgment, i.e. trust one period to rule out parts of the ECS range even if another period cannot back that up). [Steven Sherwood, Australia]	Accepted - removed the text about efficacy, and revised this whole paragraph.
64521	98	19	102	1	I'm surprised you don't mention that emergent constraints generally suggest higher ECS than other methodologies (as noted, for example, by Tian 2015, Klein and Hall, 2015, Brient 2020). Also, your conclusions that most likely ECS values are < 3.3 K and all constraints agree that ECS is virtually certain to be <5 K seem to be due to the strangely limited selection of constraints in table 7.12. You should either include a broader selection of constraints (e.g. from Hall et al 2019 https://www.nature.com/articles/s41558-019-0436-6) or defend your subsetting of Hall's list. As documented in Bretherton and Caldwell (submitted to JCLI, but not before the IPCC cutoff), Tian and Sherwood D constraints have substantial probability of having ECS > 5 K and many constraints from Caldwell et al 2018 have peak probability > 3.3 K. Based on these results, I strongly disagree that emergent constraints give us high confidence that ECS is lower than 5 K. [Peter Caldwell, United States of America]	Taken into account. Section 7.5.4 has been revised in order to more clearly justify why only a subset of published emergent constraints have been considered in the section and included in Table 7.12

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
54507	98	19	102	2	An emergent constraint derived from one ensemble (e.g. CMIP5) can have a different skill in predicting ECS when derived from another ensemble (e.g. CMIP6). It would be good to update this assessment with CMIP6 results if they become available in time or if not to at least mention this caveat somewhere in this section. [Veronika Eyring, Germany]	Taken into account. The updated text now includes comparison of several emergent constraints between CMIP3/5 and CMIP6.
5151	98	19			The one general problem I see with the chapter is section 7.5.4 through 7.5.4.3 on emergent constraints on ECS. Much of the detailed assessment in that section is also presented in section 7.5.2 through 7.5.2.4. I would recommend, and this may be a little hard to reorganize after the SOD, folding the emergent constraints section 7.5.4 into the process-based section 7.5.2. I think I am very familiar with the logic and still find it hard to see the distinction between, for example, constraints on ECS from the "historical temperature record" in section 7.5.2 and constraints on ECS from "near-global temperature change" in section 7.5.4. If I can't see the distinction very few readers will be able to see it. [Daniel Murphy, United States of America]	Rejected. Emergent constraints is a set of methods that take advantage of the diversity, non-linearity and complexity of CMIP models. It is true that there is some overlap with the other lines of evidence, which is generally unavoidable as models are used as tools in various ways in all of them, but this is taken into account in the conservative approach of the overall assessment. That is, we only go as far as any single line of evidence, whereas the approach of Sherwood et al. (Reviews of Geophysics, 2020) apply a Bayesian approach which yields a tighter constraint than in any of their lines of evidence.
5153	98	19			Another way to look at what I am trying to say here is the very true statement on page 98 line 30 that for emergent constraints "it is important to have physical and theoretical basis for the connection between the observable and the target quantity". Following this line of logic, it doesn't make sense to separate constraints on ECS due to processes from constraints on ECS due to emergent constraints. Emergent constraints only make sense when viewed in tandem with processes. [Daniel Murphy, United States of America]	Rejected. As explained in Section 7.5.4 the process-based emergent constraints are best thought of as constraints on the respective feedback processes that they address. Their connection to ECS is weak because they rely on all other feedbacks and forcings being unbiased in models. In fact when tested on CMIP6 models, most of them fail (Schlund et al. 2020)
22201	98	19			There is a possible issue if a number of these studies used GMST rather than GSAT based observations. Particularly studies that used HadCRUT4 or earlier versions of GISS and NOAA products in that in these where the constraint is will be systematically incorrect relative to the new AR6 assessment performed in chapter 2. Has this issue and the potential implications for the resulting estimates of ECS and TCR been fully taken into account in your assessment here? [Peter Thorne, Ireland]	Noted. The updated assessment of the GSAT to GMST difference is that it is close to zero. Nevertheless, since models exhibit a substantial difference, it is possible that the outcome of individual emergent constraints are high biased if they used surface air temperature to compare to observed surface temperature.
99557	98	19			The authors are to be commended on an even-handed, clear, thoughtful, and precise section. Wide classes of emergent constraints have attracted substantial publicity despite being based on shaky reasoning. The assessment here, and especially the recognition that many emergent constraints have their roots in the same tropical low cloud feedback, allows the literature to inform the estimates in a thoughtful way [Robert Pincus, United States of America]	Noted. We thank the reviewer for the positive comment.
99559	98	21	98	27	It should be made explicit naïve application of emergent constraints relies on the idea that the collection of available models samples along the axes of uncertainty, but that there is no guarantee at all that the models participating in CMIP do so. It would be even better if the assessment was explicit that the collection of CMIP models is not a designed ensemble. [Robert Pincus, United States of America]	Noted. Though we believe this is taken into account in the overall assessment.
39907	98	23	98	25	"observable and either ECS or TCR"? Needs rewritten. [TSU WGI, France]	Rejected. The statement is believed to be clear.
64523	98	29	98	44	The emergent constraint section contains a horrifying lack of skepticism about the validity of each constraint. Tthe fact that Caldwell et al (2018) found 3 out of 4 emergent constraints trained on CMIP3 data broke down when faced with CMIP5 data is sobering. Most (or all?) of the constraints you mention lack satisfying explanations for why they should hold. Based on the CMIP3 to CMIP5 experience, I suspect many of the relationships you quote are spurious. More discussion of this important caveat in this introductory paragraph is needed. Additionally, I think it is outrageous to say at this point that we have 'high confidence' in ECS and TCR bounds based on emergent constraints. [Peter Caldwell, United States of America]	Taken into account. The text has been revised and better justification for the choice of emergent constraints now included in Table 7.12 has been given. Note that of the 19 studies investigated in Caldwell et al. 2018, only Cox et al. is of the type that leverage global surface temperature change and used in the assessment presented. Furthermore little weight is given to Cox et al. as discussed in the updated text. The remaining are discussed and classified as process-based and therefore not considered useful constraints on ECS.
51339	98	31	98	31	Could you explain what is meant by "target quantity" please [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We now specify that ECS or TCR are target quantities.
5169	98	31	98	32	The point about chance relationships is a good one to make. But I think this is another sentence that could be quoted out of context. The out-of-context quotation would be for a nay-sayer to say the report says that statistically significant relationships are really all chance. You might say "thousands of relationships can be found that pass tests of statistical significance by chance because there are an enormous number of variables in a climate model and neither the variables nor the models themselves all represent independent information." I like "pass statistical significance" better than "are statistically significant" because one can argue both ways about whether a relationship that looks statistically significant but might be subject to the "many hypotheses" problem is significant or not. [Daniel Murphy, United States of America]	Taken into account. The sentence was revised.
99561	98	32	98	34	This sentence is hard to follow [Robert Pincus, United States of America]	Taken into account. The sentence was revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
5175	98	32	99	5	These two paragraphs overlap arguments on page 92 about lag correlations and considering ocean heat uptake. Also, the sentence on unforced variations influencing regression estimates duplicate a sentence on page 92 line 54. Indeed, the entire section 7.5.4.1 could be deleted. The logical arguments are all already presented in the section on energy budget and temperature history. The energy budget section is generally better written. [Daniel Murphy, United States of America]	Rejected. See also reply to comment 5151.
46345	98	49	98	50	I don't see why this would be called an emergent constraint. I think this should be moved to Section 7.5.3 discussing estimates based paleoclimates. [Twan van Noije, Netherlands]	Rejected. An emergent constraint is defined as a statistical relationship between an observable and a desired quantity found among an ensemble of models. As such these studies are emergent constraints.
5171	98	49	99	11	This is one of the few paragraphs in the chapter I simply could not understand. I think the key is that I don't know if the "past equilibrium paleoclimate temperature change" means global mean temperature or a temperature pattern. If global mean temperature, I don't understand how this is any different than section 7.5.3, If temperature pattern, I don't know how that was defined. [Daniel Murphy, United States of America]	Taken into account. An emergent constraint is defined as a statistical relationship between an observable and a desired quantity found among an ensemble of models. The specific studies discussed here use tropical temperature change as an observable, and this is now specified in Table 7.12.
16239	98	52	99	11	What is the difference between this approach and what you did in the previous section, other than that these studies use GCMs whereas the studies in the previous section look at the same evidence using EBMs? It seems that the term "emergent" should apply to a new or unexpected constraint, rather than using GCMs to test a constraint that was already assumed/expected based on simpler models. [Steven Sherwood, Australia]	Rejected. An emergent constraint is defined as a statistical relationship between an observable and a desired quantity found among an ensemble of models. As such these studies are emergent constraints. The studies discussed here take advantage of the diversity, non-linearity and complexity of CMIP models, in ways that is not done when using a simple EBM.
83809	99	7	99	11	I don't understand what Renoult et al found. Was there an emergent constraint? What model ensemble? How was the 95th percentile determined? These sentences are unclear. [Marvel Kate, United States of America]	Taken into account. The sentence was revised overall, nevertheless it is clear that the study used an emergent constraint. The table now states which models were used in all emergent constraints.
93099	99	10	99	11	Does this mean that the state dependecy is small ? [Claudia Stubenrauch, France]	Taken into account. No not necessarily since these emergent constraints, unlike simple energy balance model estimates, take into account state-dependency as it is represented in CMIP models.
93689	99	15			"accurately depict" [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
93691	99	16			"exhibits" [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
51341	99	20	99	24	This suggests that the Jiminez and Mauritsen paper alleviated the problems of Bengttson and Schwartz by including a pattern effect. And yet without the pattern effect they have a significantly higher ECS. Please explain why this is. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The study by Jimenez-de-la-Cuesta and Mauritsen (2019) accounts for a pattern effect as it is represented in CMIP5 models.
99563	99	20	99	24	Are not these inferences, based on energy balance models, already covered in previous sections? [Robert Pincus, United States of America]	Noted. No the previous sections dealt with long-term historical warming.
46347	99	26	99	30	Evidence from volcanic eruptions was also discussed earlier. Please improve the structure. [Twan van Noije, Netherlands]	Taken into account. The volcanic sections are now crossed referenced
2731	99	26	99	30	this is quite unsatisfying. Is there only one study of the constraint suggested by volcanoes, when there are many such events in the last ~200 years? What is meant by the bias of strong pattern effects? [Bryan Weare, United States of America]	Noted. There are unfortunately no other emergent constraints based on volcanic eruptions in the literature. Pattern effects have been discussed in great detail in Section 7.4 and are therefore not explained again here.
9693	99	26	99	30	Given the text on page 93, lines 20-24, this paragraph does not strike me as particularly useful. The two paragraphs weaken each other. [Olivier Boucher, France]	Noted. The strong pattern effects discussed here pertains to volcanic eruptions which are of short term and not deemed to affect much longer term pattern effects as also shown in the cited Gregory et al. 2019.
71757	99	32	99	48	Glad to see discussion here of Emergent Constraints on ECS based-on interannual variability. However (and I obviously have a vested interest here) the discussion of Cox et al., 2018a seems rather unbalanced. This paragraph reads like a critique of that study, and is based-on Brief Communications submitted after the paper was published. These BCAs were answered in detail in Cox et al. (2018b), but you wouldn't know it from this text. Can I ask for a bit more even-handedness here? In addition, it would be worth noting here that subsequent emergent constraints based-on global warming over the last 50 years (Jimenez & Mauritzen, 2019; Nijsse et al., submitted) agree on a very likely (5-95%) range of 1.5-4.1K, which is broadly consistent with the equivalent estimate from Cox et al. (2018) - 1.8-3.8K (see my comments on page 7). [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reply by Cox et al. (2018b) is indeed cited and used to defer arguments in one of the BCAs (Po-Chedley et al. 2018). As for the other two BCAs they only obtain relatively small changes to the ECS estimates. That other studies obtain similar ranges based on a completely different rationale does not make the approach valid, though.
99565	99	33			"which is derived from a mixed-layer model" is potentially confusing. [Robert Pincus, United States of America]	Taken into account. Changed to a single heat capacity model.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
72177	99	34	99	35	Please add an additional sentence: "A relationship between ECS and decadal variability has also been seen in CMIP5 pre-industrial control simulations (Nijsse et al., 2018)". Nijsse, F.J.M.M., Cox, P.M., Huntingford, C., Williamson, M.S., 2019. Decadal global temperature variability increases strongly with climate sensitivity. Nature Climate Change, 9, 598-601. [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The paper is not deemed relevant here and the result was already shown in Cox et al. (2018a) and Po-Chedley et al. (2018).
46349	99	39	99	40	Use "likely range" instead of "17th to 83rd percentiles". [Twan van Noije, Netherlands]	Rejected. The likely statement is p >= 0.66.
2733	99	39	99	40	why have you shifted to the non-standard 17%-83%? [Bryan Weare, United States of America]	Noted. The paper provided this range.
87953	99	51	100	5	I haven't looked at the papers cited herein but having published several papers comparing models to observations in the tropical troposphere using satellite and balloon data I cannot see how inferring ECS based on which models match observations better would give you anything other than a preference for the lowest-possible ECS value. The observed warming from 1958 onwards (balloons) or from 1979 onwards (satellites) is at the bottom of the range for models following observed forcings. See most recently McKitrick, 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. We have a new paper under review (this link might work for the preprint 10.1002/essoar.10503288.1) showing that of 38 CMIP6 models we tested, all 38 over-predict warming not only in the tropical troposphere but globally as well, and using an "emergent constraint" type of analysis comparing model ECS to global tropospheric warming, only the models with the very lowest ECS are in the range of possibility. [Ross McKitrick, Canada]	Noted. The papers referred to here do not deal with the trend in mid-tropospheric warming, rather anomalies and how they relate to TOA imbalance as is clearly stated in the text.
93693	99	52			What is the subject of “has” in this sentence? [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Deemed clear that 'has' refers to short term variations.
51343	100	4	100	5	Why is this problem largely overcome when using tropospheric temperatures? Could you please elaborate. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The papers that are referenced provide explanations, and it is not deemed necessary to elaborate these here.
93725	100	4			I would say “mitigated” rather than “largely overcome”. An issue with using mid-tropospheric temperature as the independent variable is that there is no direct connection to ECS, which is based on surface temperature. [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The statement is found to be clear.
96727	100	8	100	55	Please consider also literature that is more critical towards emergent constraints in this context. [Nicole Wilke, Germany]	Taken into account. The discussion of the robustness of various emergent constraints has been expanded, and also includes literature that is critical to the various emergent constraints.
38593	100	8			Many of these cloud-based constraints are tight. Rostron et al (accepted and was submitted in time; The impact of performance filtering on climate feedbacks in a perturbed parameter ensemble DOI: 10.1007/s00382-020-05281-8) describes the effect of filtering based on performance of LW cloud forcing on cloud feedbacks estimated from AMIPfuture runs. It shows that the filtering and therefore the effect of the constraint is susceptible to the treatment of structural errors. A lot of the studies here will be based on observables that have large structural uncertainties and this paper could usefully flag the issue here. [David Sexton, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The paper was not deemed useful at this point in the text, but is now included elsewhere.
104699	100	10	100	24	Studies which applied emergent constraints (e.g. Sherwood et al 2014) to parameter-perturbed ensemble (PPE) of single models found that they are not linked to the type of cloud response hypothesized by their authors and concluded that more research would be needed before these constraints could be applied (Wagman et al 2018: doi:10.1175/JCLI-D-17-0682.1, Kamae et al 2016: doi:10.1175/JCLI-D-16-0042.1). [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The Sherwood et al. (2014) study is not used to inform the assessment of ECS.
83811	100	10	100	34	A key criterion for an emergent constraint to be credible is that it must be robust across model ensembles. All of these emergent constraints are reported for CMIP3 or CMIP5 ensembles- I don't see any studies evaluating them in CMIP6. It's probably worth at least mentioning that these constraints have yet to be evaluated in the most recent model ensemble. [Marvel Kate, United States of America]	Taken into account. A discussion of which emergent constraints are robust across ensembles is now included.
99567	100	13			The assessment should acknowledge, either here or at the beginning of the section, that emergent constraints are best at finding relationships based on aspects of the model collection with the largest spread, but that there is no guarantee that these are the largest sources of true uncertainty. That does not make the method inherently "attractive" [Robert Pincus, United States of America]	Taken into account. We meant to say that the approach attracted some attention.
38599	100	16	100	16	Wagman and Jackson (2018) would be a good reference to include here. They have used a perturbed parameter ensemble to show that emergent constraints such as Sherwoord et al LTMI and Fasullo and Trenberth, and show that neither emergent constraint is reproduced for their PPE. This is a demonstration of the usefulness of experiments like PPEs to test the robustness of the emergent constraints. [David Sexton, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The Sherwood et al. (2014) study is not used to inform the assessment of ECS, so the findings of Wagman & Jackson (2018) are thus not relevant.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129059	100	21	100	24	The statement is misleading. The physical mechanisms for the circulation and ECS connection are not well understood, and the three papers described different aspects of tropical circulation. It is not known if these processes are dominant or not. Qu et al. (2018) did show that the Tian metric and Su metric are correlated to ECS because they are strongly correlated with shortwave cloud feedback. Suggest changing the sentence to "Related emergent constraints that focus on aspects of the tropical circulation and ECS have led to conflicting results (Su et al., 2014; Tian, 2015; Lipat et al., 2017), probably because the physical processes that link tropical circulation and cloud feedbacks are rather complicated and not yet well understood." [Trigg Talley, United States of America]	Rejected. First off, the studies dealt with here are not used to inform the overall assessment. Nevertheless, even if an emergent is not well understood, it may still provide information. It is irrelevant here whether or not tropical processes, circulation and cloud feedbacks are well understood as to why the referenced studies lead to conflicting results.
67561	100	21	100	24	“Related emergent constraints that focus on aspects of the tropical circulation and ECS have also mostly led to rather consistent results (Su et al., 2014; Tian, 2015) that ECS is in the high end of its range because ECS is closely tied to the tropical circulation but the exact physical mechanisms between their connection are still unclear because of their complexicity (Caldwell et al., 2018).” [Baijun Tian, United States of America]	Rejected. There is no reason per se to leave out a study that obtains conflicting results.
102101	100	26	100	34	Chapter 7.5.4.2. Tsushima et al. (2020) (doi: 10.1007/s00382-020-05318-y) used a large parameter-perturbed ensemble (PPE) of a single model to understand the link between the relationship between radiative feedbacks and the present-day simulation (emergent constraint) and the associated physical processes. Three tropical regimes (deep convection over ocean and land, and marine stratocumulus) are highlighted, in which the amount of the dominant cloud types show correlations between the present-day cloud amount and the response in warmer climate. Each of the relationships is attributable to a set of common leading parameters contributing to the variance for the present-day and that of the response. Even if multiple parameters, or their interactions, are dominant, the spread of response to warmer climate in a variable can be related to the spread of that variable in the present-day if the leading parameters are consistent between the present-day and the response. These correlations indicate that there is a fine balance between process parameters, i.e. in how much each process parameter matters for the representation of the present day and also for the response for these variables. Since it is difficult for the observations to identify such a balance between processes, the relationship could be regarded to be an emergent relationship. In deep convective regions, convective process parameters lead the spread among multiple contributing processes, with vegetation processes contributing as well for the land regions. Multiple parameters, such as boundary layer processes, drive stratocumulus regions. However, the low-thick clouds are systematically over estimated, suggesting a structural error in their process representations which would limit the efficacy of the constraint. [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The type of emergent constraints studied here are not used to inform the assessment, and hence there is not a need to understand the potential underlying processes.
99569	100	40			The distinction between global constraints and othersis perfect, but "processes" is not the right word, since the contstraints are applied to low-order measures of simulations. "Phenomena?" [Robert Pincus, United States of America]	Accepted. 'processes' was replaced by 'phenomena'.
93695	100	45			“Dessler and Forster (2018)” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
66997	100	49	100	50	"borad agreement" probably does not mean "unanimity", but Ribes et al (submitted) clearly falls into this group and finds a best estimate of 3.7°C for ECS considering the CMIP6 ensemble (i.e. the ensemble closest to reality in terms of historical forcings over 2006-2020) to construct the constraint. [Aurélien Ribes, France]	Not applicable. Comment was retracted by reviewer after revisions of paper.
51345	100	49	100	52	Is the point here that the Cox paper doesn't have a robust upper bound estimate, on the basis of the Annan et al paper? At the moment this isn't entirely clear as '4C with the exception of Cox et al' implies Cox et al could think ECS is higher than 4C (which the study does not). Suggest this is revised for clarity on these points. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was revised.
71759	100	49	100	55	This is a very poorly justified attempt to stave-off constraints on the upper likely, and very likely, ranges of ECS. Why would you side with an unpublished critique from Annan et al. of Cox et al. (2018a), ahead of studies that agree on a very likely upper range of around 4 K (Jimeniz & Mauritzen, 2019; Nijsse et al., submitted)? Please re-think and reassess your motivations here. [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was revised. That said, defending one method with results from another method is not justified.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
71761	100	49	100	55	This section also separates out Cox et al. (2018a) for some reason but does it on a false basis. The Cox et al. (2018a) very likely range (5-95%) was 1.8-3.8K, but you misleading quote here the likely range from that study (2.2-3.4K), and compare that to 5-95% confidence limits. In fact, if you use consistent metrics you will see a growing agreement on the very likely range from Jimenez&Mauritzen (2019) - 1.6 to 4K; Nijsse (submitted) - 1.5 to 4K; Cox et al. (2018a) - 1.8 to 3.8K; Hargreaves & Annan (2016) - 1.9 to 3.7K; Knutti et al., 2006 - 2.2 to 4.4K etc (see your table 7.12). None of these studies give an upper very likely range above 4.5K (and most are closer to 4K). So why chose an upper very likely range of 5K? [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was revised.
38591	100	53	100	55	There is no discussion of the effect structural uncertainty can have on emergent constraints. It comes to a head at this sentence about the independence of emergent constraints. It might be the case that the constraints are independent, but it is extremely likely that across the constraints, it's different models that match the observed value with a tolerance. This implies there are structural uncertainties in the problem. A clear example is the 'pattern effect' and the fact that climate models to not demonstrate the observed behaviour of periods of large cooling in Pacific SSTs described in section 7.4.4.3. Sexton et al 2012 (DOI: 10.1007/s00382-011-1208-9) shows that constraints are weakened when structural uncertainty is accounted for. Williamson and Sansom (https://doi.org/10.1175/BAMS-D-19-0131.1) describes this specifically in the context of emergent constraints. A caveat needs to be added here by citing these papers. [David Sexton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The discussion of uncertainties related to various emergent constraints has now been expanded considerably.
10827	100	53	101	3	These two sentences say emergent constraints are independent and also dependent. Which is it? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The sentences are found to be clear.
28261	100	55	100	55	Why "sufficient"? Should probably say "substantial" [Sebastian Bathiany, Germany]	Rejected. No we meant 'sufficient' in as to enough to justify being cautious.
10829	101	6	101	9	A similar study, (Jones, Mitchell and Stott, Uncertainties in the attribution of greenhouse gas warming and implications for climate prediction, JGR, 2016), found that TCR estimates were also dependent on methodological choices used in the regression analysis. e,g, TCR was estimated to be in ranges of 1.07-2.06K, 1.84-2.40K, or 1.54-2.17K depending on what historical CMIP5 experiments are used. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Noted. A discussion of uncertainty related to the regression was already included in the text, so the specific point mentioned here and the suggested reference were not added.
98879	101	6	101	19	A relevant publication can be found at https://www.gfdl.noaa.gov/wp-content/uploads/2020/06/SMH_rev.pdf. It provides an estimate of TCR. The methodology is very different from the traditional AOGCM-based detection and attribution. An ealier version has been communicated to a number of lead authors. The paper is going through the last round of minor revisions, and will be accepted by Science Advances very soon. [Yi Ming, United States of America]	Noted. The primary purpose of the paper referred to is to estimate aerosol forcing, not TCR.
98009	101	6	101	19	Based on Winton et al. (2020), who take a modeling perspective, it is difficult to reproduce the shape of the 20th century global mean temperature record with high aerosol forcing/high TCR combination. This evidence steers toward the lower end of the likely range, and casts doubt on the upper end. So the range seems skewed compared to this line of evidence. Ref: Winton, Michael, Alistair Adcroft, John P Dunne, Isaac M Held, Elena Shevliakova, Ming Zhao, Huan Guo, William J Hurlin, John P Krasting, Thomas R Knutson, David J Paynter, Levi G Silvers, and Rong Zhang, 2020: Climate Sensitivity of GFDL's CM4.0. Journal of Advances in Modeling Earth Systems, 12(1), DOI:10.1029/2019MS001838. [Thomas Knutson, United States of America]	Accepted. The paper is now used to discuss the evolution of temperature in high ECS models in section 7.5.6.
88959	101	9	101	9	Although both papers are related and are both relavent here I don't think that Schurer et al 2018 should be seen as an update to Gillett et al 2013 since they use slightly different methods. [Schurer Andrew, United Arab Emirates]	Noted. If two studies use "slightly different methods", one may still be considered an update over the other.
66999	101	9	101	11	This description is correct. However, the CMIP5 and CMIP6 ensembles use different forcings over the period 2006-2020, which can explain differences. Also, the latest version of our paper provides some additional discussion on the lower bound of TCR, suggesting that 1.2°C is a really unlikely value: the estimated GSAT warming in 2020 is (already) 1.2°C, while in all CMIP6 models, TCR is substantially (at least 25%) higher than the total warming in 2020. [Aurélien Ribes, France]	Taken into account. The paper is now referenced, however it is noted that the lower bound is sensitive to the underlying model ensemble.
93697	101	9			“Schurer et al. (2018)” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
98881	101	13	101	15	Any effort to constrain TCR and/or ECS with the post-1970 warming has to been caveated by the fact that the SST pattern during this period favored low sensitivity through cloud feedback. This pattern is not representative of GCM warming experiments. This has been documented in many studies (e.g. Andrews et al., GRL, 2018). [Yi Ming, United States of America]	Taken into account. This is now discussed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129061	101	22	102		Table 7.12 does not list all relevant publications on emergent constraints of ECS, such as Fasullo and Trenberth (2012), Su et al. (2014), Tian (2015), Lipat et al. (2017), Tan et al. (2016), etc. One solution is to state in the caption that the listed emergent constraints are based on low-cloud feedback or global or near-global temperature change. A more fundamental question regarding the emergent constraint section is that most of the cited studies analyzed CMIP5 simulations, not CMIP6. If these emergent constraints were applied to CMIP6 simulations, the likely range of ECS will be very different from CMIP5, because of the upward shift in ECS in many CMIP6 models. The emergent constraint based on observations is valid to determine the relative fidelity of the models, but it cannot constrain the shift in absolute value of ECS. The intrinsic limitation of this method should be noted. [Trigg Talley, United States of America]	Taken into account. Rather than including more studies in the table, we reduced the studies to only include those that are directly informing the assessment since the presence of cloud/process/phenomenological constraints was confusing a number of reviewers.
54509	101	24	102	2	Table misses some emergent constraint studies on ECS, for example Lipat et al., 2017 (Southern hemisphere Hadley cell extent), Su et al., 2014 (error in vertical profile of relative humidity). Please expand. [Veronika Eyring, Germany]	Taken into account. Rather than including more studies in the table, we reduced the studies to only include those that are directly informing the assessment since the presence of cloud/process/phenomenological constraints was confusing a number of reviewers.
54511	101	24	102	2	Please clarify in this table that these values are derived from the CMIP5 ensemble. If available in time, the estimates could be complemented with CMIP6 results. [Veronika Eyring, Germany]	Taken into account. Respective MIPs are now stated in the table.
83611	101	24			Table 7.12 p7-101 line 24 We have a problem of categorization: the following two references should be referenced in Section 7.5.2.2 (but are not) They should also be included in one of the summary Tables; perhaps include in table 7.12 as "Emergent and Values based on Climate Models". Or create an additional Table 7.12a for the purpose. Missing item Lewis and Curry 2018, median of 1.66K for ECS (5%–95% range: 1.15–2.7 K) Missing item Scafetta 2013 ECS = 1.5K 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 [michael asten, Australia]	Rejected. Page 101 line 24 is the caption of a table and does not deal with the cited studies or anything related to this. Beyond that the types of studies do not belong in section 7.5.4.
71763	101	26	102	1	Table 7.12: This is potentially a very useful table, but is diminished by not using comparable metrics for all studies. In some cases this is unavoidable (e.g. because 5-95% confidence limits were not calculated in the study), but in others it can be corrected. Studies that calculate likely (17-83%) ranges almost certainly also calculated very likely (5-95%) ranges too. Quote the latter where you can. [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. In the updated table with fewer studies only one of them is not 5-95 percentiles.
71765	101	26	102	1	Table 7.12: Entry on Cox et al. (2018a) - "Upper bound not deemed reliable". This seem to be the only entry where some value judgement is passed. You should either pass judgement on all the other studies, or remove this. [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. The statement was removed.
71767	101	26	102	1	Table 7.12: Entry on Jimenez & Mauritzen (2019). You should note errors found in this study by Nijsse et al. (submitted), which suggests that this upper bound derived from CMIP5 is in fact very unreliable (see correction in Nijsse, submitted). [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. As per comment 71765 we choose to not place judgments in the table.
71769	101	26	102	1	Table 7.12; include entry on Nijsse et al. (submitted), ECS = 1.5-4K (5-95%). [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
71771	101	26	102	1	Table 7.12: include entries on Goodwin (2016) & Goodwin (2018), and any other relevant studies discussed in the text. [Peter Cox, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Rather than including more studies in the table, we reduced the studies to only include those that are directly informing the assessment since the presence of cloud/process/phenomenological constraints was confusing a number of reviewers.
67563	101	26	102	1	Add Tian (2015), the southern-ITCZ index, 4.0 (3.5-4.5), 20%-80% [Baijun Tian, United States of America]	Taken into account. Rather than including more studies in the table, we reduced the studies to only include those that are directly informing the assessment since the presence of cloud/process/phenomenological constraints was confusing a number of reviewers.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51347	102	7	102	12	Please clarify here that the overall ECS assessment here does not take into account the ECS derived from the latest climate model estimates, and the reasons for this. As this is a change in approach from AR5, sugggest this rational is also clearly communicated in the exec summary, page 7 lines 3-12. Are you saying that ECS as derived from climate models isn't taken into account at all in your overall assessment? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The section describing why climate models are not treated as a separate line of evidence has been rewritten.
106333	102	9	102	11	This sentence has the potential to cause great confusion and consternation. Instead of a formulation that actively dismisses the direct ECS information coming from climate models, it would be helpful to highlight how it is being used instead, for example, to inform the possible range once combined with emergent constraints. The statement is in that sense not a very accurate. The direct range and distribution coming from climate models is not used but the ECS information of models themselves actually is (again, e.g. for emergent constraints). [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paragraph was rewritten and now mention how model ECS spread is useful for emergent constraints.
46351	102	14	102	14	Yes, but the estimate of anthropogenic forcing depends on the aerosol contribution. This is given high confidence but is that really justified? Also, it is not clear how differences in forcing efficacies have been accounted for. [Twan van Noije, Netherlands]	Rejected. Page 102 line 14 contains no discussion of anthropogenic forcing.
51349	102	14	102	15	There is broad agreement for around 3C across different lines of evidence partly because of the pattern effect. Is there less agreement on this value if the pattern effect is weaker? Please clarify. "However, there is substantial uncertainty in the magnitude of the net radiative feedback change between the present warming pattern and the projected equilibrium warming pattern in response to CO2 forcing owing to the fact that its quantification currently relies solely on GCM results and is subject to uncertainties in historical SST patterns." - If the pattern effect is weaker, could the historical estimates not be biased quite as low and therefore partly contradict an ECS of 3C? Or is the updated ERF values sufficient for this? Given the high policy-relevance of these values, suggest that it would be extremely helpful in this summary to include a point by point rationale of the multiple lines of evidence used here and how these are combined to arrive at this conclusion. Perhaps a table could be used to do this, highlighting each bit of evidence and how it has been combined. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. If the pattern effect would have been weaker than assumed here, then estimates of ECS based on Warming over the historical record would have been slightly lower. It would nevertheless not be sufficient to warrant a best overall estimate of ECS below 3C. The reason for this is indeed increased warming, increased ocean heat uptake and increased aerosol cooling in the latest assessment. The paragraph on page 102 lines 14-22 sufficiently captures this.
87955	102	14	102	22	Your reliance on pattern effects regarding aerosols doesn't seem consistent with the recent developments in the Detection&Attribution literature, summarized in Figure 3.6 but also shown in, for instance Jones et al JGR 2016, that when the anthropogenic forcing signal is divided into GHG and Other (chiefly aerosols) detection results more or less fall to pieces. In Jones et al they could only detect the GHG signal in 8 out of 15 cases (each case representing signal vectors from a unique climate model) and the influence of other forcings including aerosols could only be detected in 5 out of 15 cases; they also mentioned a few other papers indicating "little consistency in the magnitude of the scaled greenhouse gas warming across a sample of CMIP5 models". Yet here you are placing a great deal of weight on the ability of climate models to explain and simulate spatial patterns of responses to aerosol forcings. [Ross McKitrick, Canada]	Rejected. The pattern effect discussed here does not relate to aerosols, but rather refers to the improved understanding of how SST patterns influence feedbacks.
83813	102	17	102	17	pattern-effects -> pattern effects [Marvel Kate, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
83815	102	28	102	28	"though such a process is fairly complex and involves formulating subjective priors": frequentist statistics also rely on implicit priors. It's not clear to me that making the subjective priors explicit makes things "more complex". Additionally Sherwood et al show that the estimates of ECS do not strongly depend on the prior. [Marvel Kate, United States of America]	Taken into account. The discussion as to whether or not a requirement to formulate a prior is irrelevant here due to the conservative approach taken here. We deleted 'fairly' and 'subjective' to avoid any associations that this is a negative thing. See also 16241.
16241	102	28			This statement implies that priors are a unique requirement or weakness of a Bayesian approach, but any way of specifying a PDF has the same requirement (though it may not be explicit, which is arguably worse than making it explicit). [Steven Sherwood, Australia]	Taken into account. The discussion as to whether or not a requirement to formulate a prior is irrelevant here due to the conservative approach taken here. We deleted 'fairly' and 'subjective' to avoid any associations that this is a negative thing. See also 83815.
16243	102	29	102	33	This text repeats the main point of Stevens et al. 2016 which should probably be cited for further explanation. [Steven Sherwood, Australia]	Accepted.
16245	102	33			This statement seems to need support. We did calculations to support it in Sherwood et al. submitted. [Steven Sherwood, Australia]	Accepted.
27173	102	38	102	39	Can you explain what the precision is about and what role it plays when combining the lines of evidence? [Eric Brun, France]	Rejected. Precision on numbers is a well-defined concept. It means here that we round off to the nearest 0.5C, e.g. 3.2C is rounded to 3.0C.
106335	102	38	103	29	This combined assessment of ECS and TCR is a great advance compared to AR6 and does an excellent job at clearly and convincingly describing the chosen approach. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you for the positive comment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24193	102	38	103	37	The likely range for ECS narrowed from 1.5-4.5°C in AR5 to 2.5-4.0°C in AR6. While tightening the lower bound seems well-justified based upon multiple lines of evidence, it is less clear why the upper bound was also tightened. The effective ECS determined from energy budget methods in Fig. 23b has a likely range of roughly 2.3-4.0°C (based on my reading of the figure). This effective ECS estimate is known to be an underestimate of the true ECS, due to the high confidence that radiative feedbacks will become less negative (alpha' > 0) in the future (as discussed on Page 7-89 lines 4-27). Therefore, the upper bound of 4.0°C for the ECS likely range seems too low, given the new findings since AR5 related to the pattern effect. Related to this point, none the four lines of evidence presented in Table 7.13 have a likely range upper bound as low as 4.0°C. Given this, how was the upper bound of the ECS likely range chosen as 4.0°C? [Mitch Bushuk, United States of America]	Taken into account. The assessment of the likely range was done after the very likely range, since not all lines of evidence provided the likely range estimates. Since very likely covers more probability than the likely range, the latter can be tighter. For most reasonable distributions the 83rd percentile is roughly half-way between the median and the 95th percentile. The SOD explained this at page 102 lines 43-45, but the statement has been expanded.
9695	102	39	102	39	Can you explain what the precision is about and what role it plays when combining the lines of evidence? [Olivier Boucher, France]	Rejected. Precision on numbers is a well-defined concept. It means here that we round off to the nearest 0.5C, e.g. 3.2C is rounded to 3.0C.
46353	102	41	102	41	I didn't quite get if and to what extent the upper bound from emerging constraints has been corrected to account for pattern effects. If these are not accounted for, it would be misleading to use this value as an upper bound for ECS. Please clarify. [Twan van Noije, Netherlands]	Noted. The pattern effect influence on emergent constraints is discussed in section 7.5.4.
22205	102	41	102	41	Table 7.13? [Peter Thorne, Ireland]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
83817	102	43	102	43	process-understanding -> process understanding [Marvel Kate, United States of America]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
93727	102	45			I found the paragraph slightly confusing, and didn't fully follow how the ECS ranges were derived. Note that I don't have any issues with the numbers themselves, but I didn't find it clear how they were arrived at. It sounds like there was some amount of subjective expert judgement, in which case it would be good to note this. [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The explanation of how different lines of evidence have been combined to arrive at the assessed ECS ranges has been improved.
3361	102		24	36	This section is fundamental, I consider, respectfully, that they carry out analysis and contributions, highlighting examples, with other disciplines, sciences, in order to give more progress to their studies, which in themselves are already very valuable and of great contribution [Eduardo Erazo Acosta, Colombia]	Noted. The comment provides no concrete suggestions.
12121	102		108		Sections 7.5.5, 7.5.6 and 7.5.7 are well written and the rationale and arguments clear. The fit with other chapters (especially Chapter 4) is much improved from FOD. The approach to (not) using GCM estimates directly in ECS estimates is pragmatic and reasonable and the slight narrowing of the range from AR5 results is a positive. I guess that the lowering of the "status" of GCMs will invite climate change deniers to comment along the lines that "even the IPCC doesn't believe its GCMs now" but at least that might be moderated by an appreciation that the decision has resulted in the higher ECS values being downplayed. I know this comment doesn't help the review but maybe some thought about Comms when the report is released is useful. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Noted. We appreciate the comment, even though it provides no concrete suggestions.
64525	103	4	103	17	I think this paragraph needs a concluding sentence like "While it is theoretically possible for ECS to fall outside the very likely bounds provided here, the fact that >100 yrs of enthusiastic attempts to prove that ECS is in fact between 2 and 5 C" [Peter Caldwell, United States of America]	Noted. This conclusion is being drawn in the following paragraph.
16247	103	4	103	17	Bravo, nice paragraph! A perspective much needed, and one that might be passed on to Chapter 1? [Steven Sherwood, Australia]	Noted. Thank you. We have communicated with Chapter 1.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
87957	103	4	103	17	I think you should mention that this is the 2nd time the IPCC has raised the likely lower bound of ECS from 1.5 to 2C, and the last time it was subsequently lowered again to 1.5 as new evidence emerged. Groupthink is only one of the cognitive biases you need to consider. In IPCC circles a big problem is conflict of interest: asking people to assess their own work versus that of those who disagree with them, and not being troubled by the fact that the LA's always conclude in their own favour. You have cited a number of papers that place the center of the distribution of ECS values in the 1.5-2.0K range (and you could have cited many more), which implies a substantial part of the distribution lies below 1.5, yet you say it is "virtually certain" it can't be below 1.5. The papers that place the center of the distribution in the 1.5-2.0K range are based on historical observations. Your "virtual certainty" is based on climate model projections that a longstanding temperature gradient over the Pacific will eventually reverse. That is not grounds for virtual certainty, especially when there is evidence (Seager et al. (2019), nature.com/articles/s41558-019-0505-x) that the historically-observed gradient is consistent with rising GHG forcing and the models that have said otherwise are erroneous. It would be more accurate to say something like "If the feedback parameter rises in the future then the feedback parameter will be higher than it is presently." But it is not permissible to add "therefore it is virtually certain no part of the ECS distribution is below 1.5K." Alternatively, if you had a lot of empirical papers showing the entire ECS distribution was > 1.5K and none that showed otherwise, you could claim very high confidence (I'd still be reluctant to claim virtual certainty on anything where you are measuring a weakly-defined physical variable on noisy data) that ECS>1.5K, but you are not in that situation. If your "virtual certainty" is based on the fact that you took a poll around the office and the opinion was unanimous, then re-read your own cautions about the dangers of groupthink. [Ross McKitrick, Canada]	Noted. The comment contains no concrete suggestions. The studies based on historical warming do not account for pattern effects, as they are assessed here and do not use the latest information on forcing, warming and radiation balance.
34915	103	4	103	29	It is welcome that the SOD admits that ECS and TCR may be based only on group-think. See general comment #3 above. [Jim O'Brien, Ireland]	Noted. The comment provides no concrete suggestions.
28903	103	4		17	This is an excellent and important discussion and I wonder if there should be a suitable link to Chapter 1 historical context? [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you. We have communicated with Chapter 1.
93699	103	7			"Ångström (1900)" [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
93701	103	8			"Arrhenius (1896)" [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
83819	103	14	103	14	Scneider et al 2019 suggest the disapperance of stratocumulus decks at >1200 ppm CO2, which is not the "near future" under any remotely plausible emissions scenario [Marvel Kate, United States of America]	Taken into account. The accuracy of determining a potential threshold with such a model should be considered low. The paper states "Such transitions to a much warmer climate may also occur in the future if CO2 levels continue to rise". The word 'near' was deleted.
13535	103	18	103	18	Eliminate period (.). [Maria Amparo Martinez Arroyo, Mexico]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
96731	103	32	103	37	Table 17.3: As commented earlier on the whole subsection 7.5.5, this table does not fully reflect the statements in earlier subsections of 7.5 on ECS. The central value from 7.5.1 is 3.2°C, but 3.4°C would not be less likely. The warming over the instrumental record (7.5.2) actually has a central value of 3.5°C (cf. Figure 7.23). The best estimates from the paleoclimates (7.5.3) also range between 3.5°C and 4°C (cf. Table 7.11). And then again, emergent constraints (7.5.3), by nature conservative and not at the upper end of possible ECS, give a central value of 3.3°C (average of 3rd column in Table 7.12), and not between 2.4 and 3.3°C. Hence, it remains unclear why the conclusion from these values is a "combined assessment" value of 3°C instead of 3.3°C or rather 3.5°C. With regard to the CMIP6 results, the value of 3°C looks even more debatable. But incomprehensibly, the value of 3°C and the very likely range 2-5°C are subsequently used to disqualify CMIP6-models with high ECS-values, at several crucially important passages in the entire report. We kindly ask the authors to reconsider if 3°C is the right value for a best estimate and if a very likely range of 2°C-5°C is the optimal way to deal with CMIP6 results. [Nicole Wilke, Germany]	Noted. We have reconsidered this and found no need to change the best estimate. Note that several of the numbers referred to here have changed for the FGD in favour of a best estimate of 3 C.
721	103	32	103	39	Table 7.13 needs to get adjusted for the Paleoclimate estaimte of ECS, since Snyder 2019 finds values of 0.7-7.0°C. This needs to be done together with refinement of Table 7.11. Full ef: Snyder, C. W. Revised estimates of paleoclimate sensitivity over the past 800,000 years Climatic Change, 2019, 156, 121-138, doi: 10.1007/s10584-019-02536-0. [Peter Köhler, Germany]	Rejected. The uncertain outcome of a single study does not per se invalidate other more accurate studies insofar as these are deemed reliable, and the assessment is based on the combination of all evidence, see SOD page 102, lines 27-36 for an explanation.
32095	103	32			in this table, which is likely to get press attention, spell out the acronym ECS [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83613	103	34			Table 7.13 p7-103 line 34 Missing values for paleoclimate 1ka Abbot and Marohasy 2017, and Asten 2012 ECS = 0.6K Missing values for "Values based on Climate Models" from Lewis and Curry 2018, and also from Scafetta, N., 2013, as per previous review comment; these perhaps should become an additional line item within Table 7.13. [michael asten, Australia]	Rejected. The cited study uses machine learning methods to estimate ECS from the period from year 1000 and up until early industrialisation. This is a period dominated by natural variability and so it is not deemed useful for estimating ECS. The other study uses a specific transition 33 million years ago, not necessarily driven by CO2.
68905	103	35			Column headings say "range", but two rows do not list "ranges"; they list > or < values. I suggest using "very likely" for both upper and lower bounds for paleoclimates and adding upper bound for the "ranges" for the instrumental record. [Darrell Kaufman, United States of America]	Taken into account. The table echoes what is provided from earlier sections and so is not in a position to add bounds. To accommodate the reviewers comment two instances of the word 'range' have been deleted.
64527	103	40	103	43	This is a run-on sentence. I can't figure out what it is saying. [Peter Caldwell, United States of America]	Taken into account. The sentence was split in two.
116637	103		103		I suggest to ask Paul Edwards (chapter 1) help bring his expertise in history of science to the discussion of consensus. The last paragraph, lines 19-29, could be sharpened. [Valerie Masson-Delmotte, France]	Taken into account. The section on consensus has been revised.
31547	104	3	104	3	It is unclear how the authors reach a combined assessment of 2.2 for upper likely range, when the two line of evidence used suggest 2.4°C. Could that be clarified? Also the very likely range upper range of 2.4 is significantly lower than two of 3 line of evidence. Again some clarification would be helpful. [Jean-Baptiste SALLEE, France]	Taken into account. The text has been revised.
51351	104	8	104	12	This is an example of a number of comments in this section which, while maybe true in a narrow sense, lack context and if not communicated clearly could risk undermining perception of modelling robustness (obviously if models aren't robust in particular ways, this is important to recognise). You say that it is difficult to interpret collection of models. Is this restricted to ECS? Or could it be applied to a wide range of problems where ensembles are used. If just the former, you need to explain why. If the latter, then this opens up a vast series of questions about many aspects of AR6. As it stands this paragraphy is too general to be useful and potentially open to significant misinterpretation so could you please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This subsection has been revised considerably to reflect the fact that the primary consideration that led to excluding ECS and TCR directly derived from ESMs is that information from these models is incorporated in the lines of evidence used in the assessment.
32097	104	10			as above, spell out the acronym TCR for the press readers. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
51353	104	16	104	16	This section outlines a major departure from the approach take to estimating ECS in previous assessment reports in that ECS estimates from climate models are not used as an independent line of evidence in this assessment. This has implications for interpretation of projections from the latest models and poses communication challenges associated with perceived trust of latest model results. While the reasons for the ECS assessment approach are detailed here, suggest that the addition of a clear and succinct summary of the rationale for this decision is included here too, and in the executive summary. Additionally, suggest that unpacking the following points would be very helpful in communicating the rationale that supports this ECS assessment approach. (1) The model range remains large – please explain how understanding around this has changed in relation to previous assessment reports. (2) Model results can be difficult to interpret – please specify if this was also the case for previous assessment reports and whether this is relevant to other aspects of model interpretation (as well as ECS). Please also clarify that model results for other variables are still useful to avoid confusion that the issues discussed here present issues with the use/interpretation of all model variables rather than ECS only. (3) Models are used to inform the ECS estimates of the other lines of evidence considered – please specify that this was also the case for previous assessment reports, and how specifically this assessment incorporates model estimates more comprehensively. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paragraph discussing climate models as a separate line of evidence has been updated and further explanations have been added in several places such as section 7.5.6, 7.5.7 and FAQ7.3.
96733	104	16	106	17	The decision to reduce the importance the CMIP6-results on the ECS assessment in the report is highly problematic. The reason for this is that the models deliver results based on the simulated ECS which are now disconnected from the amended AR6-value. The larger CMIP6-ECS implies that the climate signals in CMIP6-SSP5-8.5-runs are often stronger than those of CMIP5-RCP8.5. This leads to inconsistent statements within the AR6 and compared to the AR5. We strongly urge the authors to reconsider their new approach to the ECS and explain the consequences for other climate relevant quantities in a consistent way across chapters and avoiding duplications. If the results remained as presented, the consequences must please be explained much more clearly. [Nicole Wilke, Germany]	Rejected. As explained in section 7.5.6, results from climate models are included in the separate lines of evidence going into the combined assessment of ECS. Keeping ECS from climate models as a separate line of evidence would be flawed due to dependency. Following the reviewer's suggestion would be scientific step backwards.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38595	104	16			The build up to section 7.5.6 has seemed a thorough assessment of a wide range of evidence. The assessment itself (7.5.5) will depend on the degree of interdependence between the four lines of evidence. This has probably been accounted for in some mathematical way in the overall assessment but there are no real details in this crucial part. It clearly needs to be accounted for properly - the four lines of evidence are to some extent based on climate models (it says so p.105, l.50-52). Climate models all have structural uncertainties, some common to all state-of-the-art models, some peculiar to individual models. To some extent these structural uncertainties impart interdependence on lines of evidence based on a climate model. An example would be the known unknown of why models don't seem to capture the observed trend of greater warming in recent decades of the warm pool compared to the east Pacific (Seagar et al 2019; https://ocp.ldeo.columbia.edu/res/div/ocp/people/seager/SeagerEtAl2019NCC) which is relevant to the pattern effect. Please can more detail be added about the assumptions made to capture the interdependence in the "somewhat independent" (p.105 l.41) lines of evidence. [David Sexton, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The assessment rationale was described on page 102 lines 27-36, and there is no underlying assumptions happening elsewhere. This text also discusses the effects that interdependence may have on the combined assessment, and how the choice of combining conservatively accounts for this risk.
38597	104	16			This section 7.5.6 and 7.5.5 are about the very important assessment of ECS lower and upper bounds. A lot of the detail seems to be in an submitted paper. The key details need to be presented here because with new evidence arriving all the time, it is impossible to know what has been factored in and what has not. The effect of this for the upper bound of the ECS concerns me most - the upper bound from the assessment is 5K, yet the CMIP6 model range goes beyond that and that raises questions like why has the new modelling evidence not raised the upper bound - or maybe it has been factored in indirectly, it's hard to tell. The assessment has been done over a long period that started well before CMIP6 data was available, lots of pre-2019 studies are cited in chapter 7, so what is the relative influence of CMIP5 and CMIP6. Some re-ordering of the text and some more detail on the extent to which CMIP6 information has been included in the assessment would help section 7.5.6. Point 1) Section 7.5.6 goes to great lengths to explain why the methodology to make the combined assessment does not treat the multimodel ensembles as an independent line of evidence. This argument would be helped by clearer presentation - maybe move (p.105 l.50-51) before p.105 (l.8-48) and bullet point those four points. Without this I wasn't clear where those points were heading. Point 2) Section 7.5.6 is also very unclear on how much CMIP6 information has found its way in to the assessment. Page 105 l.1-2 imply some process information has been included but it needs to be a lot more transparent than this. The effect of the lack of detail about this on the reader is to leave them wondering what has happened and how to interpret this assessment. Is it A) CMIP6 information has been accounted for fully by informing the 4 lines of evidence and this is definitively the most up-to-date assessment; or B) the 2-5oC range is largely based on older work? The section needs to be explicit on what information was not included in the assessment. If it is B) then how does the assessment stack up in light of CMIP6 - do the priors on the process-based assessment capture the stronger SW cloud feedbacks in the models that have more supercooled cloud liquid? Did Andrews et al (2019) make it in to the assessment as its upper bound is 8.2K and raises the possibility that the high-end ECS cannot be ruled out because of a potentially large structural error (see my point above) not in ECS but in the pattern effect, a'. Of course, it could be a structural error in both as Seagar et al 2019 suggests. What happens to the ECS assessment come CMIP7 when all the other models have this better treatment of supercooled liquid, and the associated negative extratropical optical depth feedback is reduced in all runs - does this ECS assessment still stand? Is the assessment robust to this kind of possible shifting of	Noted. CMIP models ECS are not directly taken into account in the overall assessment. It is furthermore described in the beginning of section 7.5.5 how the different lines of evidence are combined to yield lower uncertainty. The questions raised should now be answered in the revised Sections 7.4 and 7.5.
99571	104	16			This section explains why AR6 assessments of TCR and ECS do not rely on CMIP simulations directly. I fully support this decision and the section is terrific at explaining the reasonsing. The section on how to interpret the increased sensitivity of CMIP6 models is excellent. [Robert Pincus, United States of America]	Noted. Thank you for the positive comment.
16249	104	18	104	26	Although it is true that past IPCC assessments didn't do this, it might be fair to note here that the most recent, WCRP-associated assessment (submitted) did do exactly what you are doing here, and that this was advocated previously by Stevens et al. 2016. [Steven Sherwood, Australia]	Taken into account. The sentence was revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
7481	104	18	106	17	I think the arguments laid out to exclude the direct ECS from CMIP6 in the ECS estimate are not strong enough to support their exclusion. The discussion here is informative and good, but one could easily write similar texts about the many limitations of emergent constraints, paleoclimate proxies, simple energy models or any of the other evidence that made it into the assessment. In fact, some of the problems with the climate models like their interrelatedness is a problem also when they are used indirectly. In essence I miss a smoking gun and the text did not convince me that this line of evidence is weaker than any of the other that ended up being used. I do not wish to insist on including the direct estimates, I think that should be 100% the authors decision, but the arguments for exclusion need to be strengthened. I my opinion this is rather an important choice, which if not explained well enough could fuel all sorts of conspiracy theories. [Magnus Hieronymus, Sweden]	Taken into account. This text has been revised considerably to clarify exactly how the CMIP6 models inform the ECS assessment through the various lines of evidence.
20445	104	23	104	26	Maybe this report has a problem with what is called climate sensitivity. Earlier on the reader struggled to discover that it was the inverse of the climate feedback. Later on confirmation was given by the Rohling et al reference, who went as far as designating it by S. Now it sounds rather as a generic name for several concepts (which unfortunately are not expressed in the same unit). [philippe waldteufel, France]	Noted. The report clearly defines ECS and other expressions pointed to are different ways to infer or estimate ECS, not new definitions.
64529	104	30	104	30	I suggest "...is HISTORICALLY dominated..." because elsewhere in this document you correctly point out that CMIP6 spread is greatly affected by extratropical clouds as well. [Peter Caldwell, United States of America]	Taken into account. The sentence was revised to talk about all low-level clouds.
3953	104	35	104	35	authors' names should not be in capital letters [Sabine Baumann, Germany]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
46355	104	36	104	38	Please mention that these ranges are based on linear regression over the first 150 years of the abrupt-4xCO2 simulations, and that the actual model ECS values would be higher. In the study by Rugenstein et al. (2019), the model-median increase is about 17%. [Twan van Noije, Netherlands]	Noted. The paper by Rugenstein et al. (2020) also looks at the bias introduced by using 4xCO2 instead of 2xCO2 and it is found to be of similar magnitude. We see no reason to elaborate on this here.
99573	104	37			Perhaps "CMIP6 models to date"? These numbers will no doubt continue to change for some time [Robert Pincus, United States of America]	Taken into account. The specific sentence refers to a paper, so this is implicit, but 'to date' has been added to line 39 when referring to the table.
93729	104	39			The statistical significance is in disagreement with Zelinka et al. 2020. Can the two studies be reconciled? I also wonder whether it’s a good idea to cite the exact numbers from a single study, as the numbers will vary depending on the set of models. [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was revised and the less relevant discussion of statistical significance omitted.
33197	104	40	104	40	Do you mean "significantly" in a statistical sense, but I thought Zelinka et al. (2020) said the mean change in ECS between CMIP5 and CMIP6 wasn't actually statistically significant. [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was revised and the less relevant discussion of statistical significance omitted.
10833	104	40			I do not think "significantly" is the right word to use here. The two ensembles of opportunity overlap quite substantially! [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was revised and the less relevant discussion of statistical significance omitted.
10835	104	41			"The upward shift does not apply to all models" - this implies the same models are in both CMIP5 and CMIP6. This is not the case. There are differences between models from the same institutions, but that was the case for models from the same institution in an individual CMIP. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. the addition "....traceable to specific modelling centres..." has been added to the text.
71107	104	43	104	43	Is this "likely" the IPCC language? [Yu Kosaka, Japan]	Taken into account. The sentence was revised.
116639	104		104		The part on model developments needs to be considered carefully together with ch 1 and ch 3. It can be counterintuitive that progress in knowledge and representation of processes leads to a larger spread. It is also a sign of vitality and choices of not tuning model versions to the earlier range. This for the first time helps test systematically models with large sensitivitiy on multiple aspects, which is key to make progress. I would suggest to consider how to best express this development, and the outcome of the assessment. [Valerie Masson-Delmotte, France]	Taken into account. We agree that it is great that there is more spread among models, and this is now noted in several places.
99575	105	8	105	12	The point that the models participating in CMIP are not only dependent on one another but also are not systematically assembled to sample e.g. structural uncertainty could be made again here [Robert Pincus, United States of America]	Noted. The paragraph does mention common limitations and systematic biases, which is deemed sufficient.
96735	105	10	105	12	Please explain that this holds especially for low- and medium-range ECS models. [Nicole Wilke, Germany]	Rejected. There is no mention of low- and medium range ECS models here. The sentence regards the intrinsic difficulties associated with interpreting a model ensemble.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
51355	105	14	5	25	Again, another paragraph that risks undermining the credibility of models. Clearly there are important debates to be had about tuning. But as written, this section basically reads as though the models are enormous black boxes that no one understands and when something looks weird they are just run and run until the user is given the answer they want. This isn't the case in reality, but as written this implication as a wide range of consequences for interpretation of model output, not just for ECS. While you are writing your rationale for why model ECS estimates are being excluded from the overall assessment, it would be helpful to phrase your language in such a way as to not leave the reader questioning whether the models are any good for anything! This isn't to say do not be critical of them, clearly you must, but your text here should be specific and targetted to the issue of ECS and leaving as little room for miscommunication as possible. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The text regarding tuning has been revised considerably, and it has also been clarified that the main reason for excluding ECS and TCR directly derived from ESMs is that information from these models is incorporated in the lines of evidence used in the assessment.
102093	105	14	105	25	Chapter 7.5.6. Tsushima et al 2020 (doi: 10.1007/s00382-020-05318-y) conducted an analysis of PPE (from a single model).They identified leading processes driving the spread of the radiative feedbacks across the ensemble. They wrote that 'Such understanding will be useful in determining a strategy for our model development to reduce uncertainty in feedbacks. Understanding the processes controlling feedbacks in an individual model is ultimately the only way to improve that model. This argues that SMEs are more informative than MMEs for a strategy of individual model development to choose target processes in the aim of reducing uncertainty in its feedback. There are differences in how the process is represented in other models, and it is possible that different process-based constraints might be inferred in other models. The collective understandings could be valuable for improving model process and feedbacks more generally.' Could these messages fit well in this paragraph? [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The reference was added to section 7.5.7.
96737	105	16	105	25	Please reconsider if "tuning" is the correct and optimal scientific word in this context. Please be aware of recipients with no background in numerical modelling. [Nicole Wilke, Germany]	Noted. The word is widely used in the community to describe the process discussed here.
20447	105	34	105	37	However, when considering GCM used for weather forecasting, improved parameterizations are steadily implemented as time goes (along with improved resolutions, same as for climate models), and the forecasting performances become better and closer. Should then not one expect the same to occur in the climate domain? [philippe waldteufel, France]	Taken into account. FAQ3.3 now clearly demonstrates that with respect to many key climate variables, ESMs are indeed improving over time.
96739	105	34	105	37	Please explain that the higher ECS in some models is the result of improved (more realistic) cloud parameterizations. [Nicole Wilke, Germany]	Noted. The specific point in the text does not discuss the relationship between process-level model improvement and ECS, and this is discussed elsewhere.
96741	105	38	105	41	Please explain in more detail how dependent on each other the other lines of evidence really are. "somewhat independent" does not sound very convincing. Also, line 39 states that this information was not routinely used to inform GCM. But line 50 states the opposite. Please revise and explain this crucial issue in more detail. [Nicole Wilke, Germany]	Taken into account. The words 'somewhat independent' were deleted. The statement on line 39 was modified.
51357	105	43	105	48	The mischievous reader might question why, if models seem to be somewhat unreliable in key aspects, why should we trust them when being used to inform these other ECS methodologies. So this would be a good opportunity to briefly restate why they are indeed fit for purpose in making these complementary assessments. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. The paragraph contains references to the sections where the reliability, or not, of models for these specific purposes is discussed at length. There is no simple statement that can be made in short here.
10837	105	50	105	52	Are any of the lines of evidence "independent" from another line of evidence? [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The sentence was revised.
96743	105	50	106	17	Please explain in more detail why the "raw model output" is not considered as an own line of evidence, and why the other lines should be better predictors for future developments. When climate models are informed by the other lines of evidence, why do they have to be replaced by "emulators" which are (tightly) constrained by the other lines? Isn't it the case that the other lines do not include more information about possible futures than well-informed climate models? Please explain with physical arguments why high-warming models would only show 'tail risks' and why their high ECS values would not be "robust samples" (p.7-106, line 14). [Nicole Wilke, Germany]	Accepted. The text has been revised to clarify how CMIP6 models contribute to the different lines of evidence for ECS and TCR. It is not correct that climate models incorporate all information available. Therefore, inevitably, considering all available information leads to less uncertainty than what models alone can achieve.
10839	105	52	105	53	It appears that using emulators instead of GCMs is effectively given zero weight to climate models for projections. Given the use of observed temperatures in the assessment of ECS, the subsequent use of simple models using ECS is highly contridictory. i.e. simple models have effectively been weighted by the observations, but not complex physics based models. This is a strange state of affairs. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Emulators are in the report simply used as a means to convey the assessed ECS and TCR to projections. These models are thus not an independent line of evidence, and CMIP6 models still play an important role in projections in AR6 for a wide range of climate variables, but these are generally scaled to various global warming levels.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96745	106	1	106	17	The decision to define models with an ECS > 5° as outside the likely range disqualifies well-known and frequently used models worldwide, e.g. CanESM, CESM2, UKESM, HadGEM fall into this category. Since this is very difficult to communicate, we strongly urge the authors to please extend the explanations here in providing the reasons in a few but clear words. In addition, we are concerned that these models are not consistently treated across the report, or at least not in a transparent manner, when reading Ch4. [Nicole Wilke, Germany]	Noted. The prominence of a certain modelling institute with regards to the ECS of their models has had no influence on the assessment. There are several places in which the relation between the assessment of ECS and CMIP6 models as well as the value of the wide range is discussed. We have communicated with Chapter 4 and believe the treatment is now more consistent.
129063	106	7	106	9	The conflicting treatment of CMIP5 and CMIP6 ECS values is puzzling. The ECS estimates based on the emergent constraints applied on CMIP5 simulations are OK, but recommend against using CMIP6 ECS values. Are there more convincing reasons that direct outputs of ECS from CMIP6 are not realistic? Are the CMIP6 historical temperature simulations too off from observations? The argument in this section reads quite weak. Don't quite understand the rationale that AR6 relies heavily on CMIP5 simulations, but not on CMIP6 simulations. It appears CMIP6 studies are quite limited. Why rush to compile an assessment report without thoroughly analyzing the state-of-the-art climate model simulations? [Trigg Talley, United States of America]	Rejected. Neither CMIP5 nor CMIP6 ECS values are recommended here. The updated section 7.5.4 compares emergent constraint studies based on both ensembles.
83821	106	11	106	17	I really like this summary. At some point here or in the preceding paragraph, is it worthwhile to mention the non-Gaussian structure of the CMIP6 ECS distribution? There is a group of models with quite high climate sensitivities centered around ~5K, a group with sensitivities more in line with CMIP5 values centered around ~3K, and INM-CM4/5 with a sensitivity <2K. This means that the model average warming, or "generally higher projected warming", doesn't necessarily reflect a new model consensus, but the influence of the higher-sensitivity group of models. I feel it may be important to clarify that only *some* CMIP6 models have higher sensitivites. [Marvel Kate, United States of America]	Noted. It is likely that this non-gaussian distribution is simply an effect of having a low number of models. Hence it is not deemed warranted to speculate.
116641	106	11	106	17	What is missing is an understanding of the reasons for larger responses of extra tropical clouds; and what is the implication of this response for projected patterns (also including links between energy carbon and water fluxes over extra tropical regions). Are their specific behaviors linked with this larger feedback in projections (effects on emergence of other aspects)? [Valerie Masson-Delmotte, France]	Noted. These are questions that the current literature does not address, as research on this topic has just started.
93703	106	11			“the distribution … has” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
28905	106	11			The CMIP6 vs CMIP5 quoted ranges of ECS could be repeated here even if it is not explicitly used as an independent line of evidence [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is not deemed relevant here by how much.
93705	106	15			“on *its* ECS or TCR values” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
46357	106	16	106	17	Please clarify to what extent differences in the forcing between the SSPs and RCPs may also play a role. [Twan van Noije, Netherlands]	Noted. This was discussed in Chapter 4.
38271	106	20	106	20	I am wondering whether the tilte of 7.5.7 is proper. The main topic in section 7.5.7 is to estimate the uncertainty in several feedback processes to explain the global warming. [Junhee Lee, Republic of Korea]	Rejected. This section discusses the role of both radiative feedbacks and ocean heat uptake, so the title has been left the same.
20449	106	20	108	20	This subsection begins by identifying 3 main masses in the energy budget and discussing the role of each of them, with a growing focus on their contributions to uncertainties. This nice plan is followed throughout, except in the middle (e.g. page 107 lines 33-51) where it seems to be temporarily lost... [philippe waldteufel, France]	Taken into account. This section has been revised.
99577	106	20			This section is less well-connected to the previous disucssion than it might be. [Robert Pincus, United States of America]	Taken into account. Introduction to this sections revised to better connect to previous sections.
83823	106	22	108	20	I found this section to initially be confusing. I think it needs a sentence or two to segue between the preceeding discussion of ECS/TCR and the subsequent discussion. Perhaps something that re-iterates that ECS/TCR are realizable only in models and depend on highly idealized forcing scenarios, but future warming out to 2100 will be determined by transient emissions scenarios, and it's important to understand whether radiative feedbacks, forcing, or OHU contribute the most uncertainty. [Marvel Kate, United States of America]	Taken into account. Introduction to this sections revised to better connect to previous sections.
96747	107	2	108	20	This sub-section would benefit from an analysis and a discussion of the different model approaches with regard to the cloud feedbacks. Which parameterizations are more successful than others and why? [Nicole Wilke, Germany]	Noted. This level of detail is far beyond the scope of this chapter, however section 7.4.2 provides an assessment of our current understanding of cloud feedbacks and compares to models.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17363	107	6	107	9	This is misleading. It suggests that the largest contribution to the uncertainty of feedbacks is from low-clouds. However this is in contradiction to section 7.4.2.4, section 7.4.4.3 and FAQ 7.1. The largest contribution to the uncertainty of feedbacks in AR6 comes from the feedback of tropical high-cloud amount and the pattern effect. [David Neubauer, Switzerland]	Taken into account. Text revised for consistency with 7.4.2.
93101	107	8	107	9	Zelinka et al. (2016) showed that high-cloud feedbacks are also still very uncertain, when separated into LW and SW (Fig 1). Zelinka, M. D., C. Zhou, and S. A. Klein (2016), Insights from a refined decomposition of cloud feedbacks, Geophys. Res. Lett., 43, 9259–9269, doi:10.1002/2016GL069917. [Claudia Stubenrauch, France]	Taken into account. Text revised for consistency with 7.4.2.
116643	107	11	107	17	What about highresMIP? [Valerie Masson-Delmotte, France]	Rejected. This paragraph discusses interactions between feedbacks in the climate system and alternative feedback decompositions. It was not clear what a discussion of highresMIP would add here, so it was not added.
89355	107	22	107	31	Using an AOGCM, Watanabe et al. (2020, ERL) showed that changing the distribution of mixing intensity (vertical diffusivity) in the ocean changes TCR by 0.16 degreeC by changing the ocean heat uptake. The mixing intensity distribution in the real ocean is still unknown, and studies that have used AOGCM to investigate the relationship between the mixing intensity distribution, ocean heat upake and transient climate response are still less. Therefore, while cloud feedbacks are the most important source of uncertainty, it would be premature to note that global ocean heat uptake plays a minor source of uncertainty. https://doi.org/10.1088/1748-9326/ab8ca7 [Michio Watanabe, Japan]	Taken into account. Cited, and text revised.
103627	107	27	107	28	The opening sentence convoluted (make to some aspect of ..?) [Philippe Tulkens, Belgium]	Not applicable. It is unclear what this comment refers to.
102095	107	33	107	51	Chapter 7.5.7. Although PPE is a single model ensemble, it is robust among PPE among different GCMs that cloud feedbacks are the largest source of the spread in the atmospheric radiative feedback (e.g. Rostron et al. 2020, Tsushima et al. 2020, Tomassini et al. 2015, Kamae et al., 2016, Gettelman et al. 2012). Rostron et al 2020: doi:10.1007/s00382-020-05281-8 Tsushima et al 2020: doi: 10.1007/s00382-020-05318-y Kamae et al 2016: doi:10.1175/JCLI-D-16-0042.1 Tomassini et al 2015: doi:10.1002/qj.2450 Gettelman et al 2012: doi:10.1175/JCLI-D-11-00197.1 [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cited.
102097	107	33	107	51	Chapter 7.5.7. Tsushima et al. (2020) investigated leading processes driving the spread of the radiative feedbacks across found that the influence of convection is dominant in the tropical deep convective regimes which is largely confined there in the present day but extends to other regimes up to mid-latitudes under warming. Because of this, influence of convection contributing processes to the spread in the present-day and the response are different in the extra-tropics, making it much more difficult to establish links between the present-day and the feedback within the region. This suggests that identifying a constraint on convective processes in the tropics for the present-day simulations could constrain both the tropical feedbacks and feedbacks in the extra-tropics. [Tsushima Yoko, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Cited.
99579	107	34			The term of art is "perturbed parameter ensembles" because it is numerical values, not formulations, which are sampled [Robert Pincus, United States of America]	Taken into account. Revised.
15999	107	53	108	1	The statement that cloud feedbacks are the dominant source of uncertainty is unsupported in light of the exclusion of Earth System feedbacks (see page 56, line 27). So, do cloud feedback dominate over methane feedbacks, and would these two feedback loops be independent? [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Methane feedbacks are assessed in Chapter 6 to be relatively small with small uncertainty. They are neglected here because this section draws largely on models projections that do not include methane feedbacks, but their inclusion would not change these findings. This has been clarified in the text.
20451	107	53	108	4	These firm statements need to be supported by arguments preceding them, or at least references providing such arguments. The reference to subsection 7.4.4.3 is not concerned by uncertainties. [philippe waldteufel, France]	Not applicable. Comment unclear.
22207	108	10	108	12	Should this not cross-reference the more substantive assessment in chapter 6? [Peter Thorne, Ireland]	Accepted. Revised to reference Chapter 6.
66411	108	12	108	14	The post-2100 dynamics in chapter five are discussed in section 5.4.9. [Charles Koven, United States of America]	Accepted. Revised to references Chapter 5 Section 5.4.9
71173	108	13			It is not clear to me what the authors mean by "the uncertainty permafrost thawing". It may simply be an editorial problem, or maybe I’m missing something scientifically. [Lukas Arenson, Canada]	Taken into account. Revised.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89357	108	16	108	20	Using an AOGCM, Watanabe et al. (2020, ERL) showed that changing the distribution of mixing intensity (vertical diffusivity) in the ocean changes TCR by 0.16 degreeC by changing the ocean heat uptake. The mixing intensity distribution in the real ocean is still unknown, and studies that have used AOGCM to investigate the relationship between the mixing intensity distribution, ocean heat upake and transient climate response are still less. Therefore, while cloud feedbacks are the most important source of uncertainty, it would be premature to note that global ocean heat uptake plays a minor source of uncertainty. https://doi.org/10.1088/1748-9326/ab8ca7 [Michio Watanabe, Japan]	Accepted. Cited.
77445	108	16	108	20	Can the timescales and quantification of other factors mentioned be included in this summary? [Emer Griffin, Ireland]	Taken into account. Timescales clarified in the text.
701	108	23	108	23	Section 7.6 on Metrics to evaluate emissions seems out of place in this chapter. Shouldn't it be in the radiative forcing chapter? This chapters title implies that page 7-107 should complete the chapter. The material is valuable but seems very out of place here. [Bruce Wielicki, United States of America]	Rejected. The structure was agreed prior to the Zero Order Draft.
64771	108	23	116	20	There are inconsistencies across IPCC AR6 drafts in terms of the roles of near- and long-term warming, SLCFs vs LLCFs, and near- and long-term metrics; WGI Chapter 6 does acknowledge short timescales and the role of near-term warming, whereas these are deemphasized in Chapter 7, and WGIII includes more discussion of GWP20 and other short-term time horizon metrics. There needs to be consistency across IPCC AR6 regarding the importance of climate change over all timescales, the roles of different pollutants in contributing to warming over different timescales, and metrics and values that convey climate impacts across all timescales. [Steven Hamburg, United States of America]	Taken into account. We have tried to be clearer and more consistent across the report. We have focused more on the physical relationships between pollutants and climate than on timescales per se, and we don't think this is inconsistent with the rest of the report. However, we have harmonised more closely with chapter 6.
68109	108	23	116	20	As written, Section 7.6 entirely prioritizes addressing long-term climate impacts, without acknowledgement of the importance of addressing near-term warming. This is especially evidenced by the absence of any metric time horizon less than 50 years; and even for 50 years, only GTP is reported, which does not account for the path up until the 50 years (and the path plays a major role in determining the climate impact of several non-CO2 gases). Near-term warming is an essential component of climate change and plays a major role in determining impacts to society and ecosystems. Providing options for near-term metrics, shorter time horizons, and acknowledgement of the role of near-term warming is a missing yet critical component of this section. [Ilissa Ocko, United States of America]	Rejected. In this assessment we have endeavoured to reflect new, policy-relevant literature. This is why step-pulse metrics have received prominence, and why we have discussed multi-metric approaches. In keeping with the rest of the chapter, we have attempted to be clear about physical relationships (such as the difference between) rather than focus on specific timescales. Warming over short timescales depends on the mix of gases at that point in time - currently, warming rates are strongly dominated by fossil CO2 emissions.
86801	108	23	116	20	Ch. 7 does not seem to take into account the 10-100 years time frames laid out in chapter 6 (e.g on chp. 6, p. 6, l. 16-17). Timescales shorter than 50 years are not included in table 7.15 and table 7.A.3 and are not mentioned in the accompanying text either. We suggest that you include at least the 20 years time horizon in the table and text (consistent with AR 5 table 8.A.1) to pay justice to the SLCFs and the climate effect in the short term analysis. Even a 10-years time horizon should be considered. This is useful for analysis of the rate of change in the near-term both for policy makers and for countries which are developing near-term mitigation plans. [Oyvind Christophersen, Norway]	Taken into account. We have tried to be clearer and more consistent across the report. We have focused more on the physical relationships between pollutants and climate than on timescales per se, and we would like to retain that focus. We have harmonised more closely with chapter 6.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68121	108	23	116	20	It is clear to me when reading this section that the authors are promoting newer, innovative metrics and moving away from traditional ones like GWP (although the chapter outline adopted by the panel in the 46th session of the IPCC in 2017 clearly states that the chapter would address "GWP, GTP, and other metrics"). While from a scientific perspective I understand this decision, and I agree that these metrics are more scientifically appropriate for several applications and in many cases do a better job connecting emissions to impacts, there is a severe disconnect between the technical science and the users of metrics. The decision for which metric to use is rarely one based on pure science, and decades of alternatives to GWP that have failed to replace it show how difficult it is to convince the community to adopt a non-GWP metric. Other reasons include political ones, such as international consensus (was incredibly difficult for parties of UNFCCC to agree to GWP100, and now that they have, are unlikely to revisit that decision), and already-existing tools that employ GWP (such as Climate Action Tracker, EN-ROADS, G-Res, EPA Equivalency Calculator, ClimateWatch, IEA Methane Tracker, etc.) may require major modifications to adjust to a new metric. Further, GWP is already a legally required component of policies and trading schemes (such as the Paris Agreement which requires countries to use GWP in the Enhanced Transparency Framework Article 13 and the EU Emissions Trading System). Given that there are major barriers preventing users from changing and adopting these new metrics, it is critical that the metrics section provides more information on GWP and its deriative, CO2e. As of now, there are only three mentions of CO2e in the entire Section 7.6, and without any explanations or definitions of what it is. This is a major disconnect with the user community when the majority of climate metric users are using CO2e. I recommend more discussion about GWP and CO2e, their history and use, shortcomings, and suggestions/cautions for most appropriate uses (see Comment 8 for more). [Ilissa Ocko, United States of America]	Taken into account. We do address "GWP, GTP, and other metrics." We think it is vitally important that physical scientists explain as clearly and simply what the relevant issues are, and what the various metrics are more or less well-aligned to. The idea that GWP should be used for everything has never been endorsed by IPCC, and this assessment continues in the same non-prescriptive vein. As the commenter acknowledges, there are strong scientific reasons for us to do this. If there are reasons from scientific or environmental integrity to alter a customary approach to carbon accounting, then WGI ought to articulate these reasons. Where there are adjustment costs for changing metrics or for separating out forcing species, then the people in the relevant organisations should take these into account. But factoring these costs in is not part of the WGI assessment.
68123	108	23	116	20	The best approach is to keep greenhouse gas emissions separate and not lumped together in a metric. If this is not possible, then selecting the most scientifically appropriate metric is the next best option. Unfortunately, this is not as simple as it appears for the myriad non-climate scientist users that are employing climate metrics in their assessments and decision-making. I have vast experience with metric users who refuse to stray from GWP despite better alternatives. The result is that people use GWP100/CO2e in isolation. Therefore, as a bare-minimum approach to improving the widespread use of climate metrics, but with a realistic chance for success, my colleagues and I have found that using a two-valued GWP (GWP20/100 and CO2e20/100) when possible at least improves the GWP shortcoming associated with timescale (see Ocko et al., Unmask temporal tradeoffs in climate policy debates, Science, 2017). We have used this strategy successfully on many occasions to articulate the temporal tradeoffs associated with particular climate actions, make people aware that a time horizon assumption is even included in CO2e, and to ensure that decisions do not lead to near- or long-term disbenefits when only one time horizon is considered. For example, if Brazil only uses GWP100 in their accounting for their Paris Agreement NDC target (which is what they use now), then they can technically still achieve their target while making the climate worse in the near-term (by following a pathway that mitigates CO2 but increases methane). Use of GWP100 would not make this near-term warming evident at all. But if Brazil has to use GWP20 as well when conducting their target accounting, it will be clear that their emissions pathway is worse for the climate in the near-term, and therefore will serve as a "check" to make sure that methane emissions do not grow beyond a certain point. By the IPCC AR6 including this 2-valued approach for use of GWP (which is similar in construct to other familiar 2-valued metrics such as blood pressure (systolic/diastolic) and gas mileage (city/highway)), a small statement by the IPCC can have a huge return on improving the use of climate metrics for policy applications and decision making. [Ilissa Ocko, United States of America]	Taken into account. We are incorporating more material on multi-metric approaches as several independent reviewers have asked for this. We agree that these have value and that the most clear thing to do is to treat gases separately.
68131	108	23	116	20	There are inconsistencies across IPCC AR6 drafts in terms of the roles of near- and long-term warming, SLCFs vs LLCFs, and near- and long-term metrics; WGI Chapter 6 does acknowledge short timescales and the role of near-term warming, whereas these are deemphasized in Chapter 7, and WGIII includes more discussion of GWP20 and other short-term time horizon metrics. There needs to be consistency across IPCC AR6 regarding the importance of climate change over all timescales, the roles of different pollutants in contributing to warming over different timescales, and metrics that convey climate impacts across all timescales. [Ilissa Ocko, United States of America]	Taken into account. Thank you for this comment. We have endeavoured to seek a more consistent treatment between chapters 6 and 7.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112433	108	23	116	20	The section on climate metrics is wholly focused on long-term temperature stabilization and essentially neglects the complementary and critical goal of reducing the rate of climate change over the coming decades. Given the the substantial impact of climate change in the coming decades, this is inappropriate. [David McCabe, United States of America]	Rejected. Current warming rates are dominated by recent CO2 emissions, so a refocus of the material to emphasise reducing the rate of warming would also lead to an emphasis on CO2. In any case, step-pulse or multi-metric approaches can do a better job (than simple pulse metrics) of simulating the temperature effects on any timescale.
89927	108	23	116	20	IPCC AR5 WGI did an excellent job with its metric section and accompanying supplemental material. I strongly encourage the authors of AR6 to revisit Section 8.7 in Chapter 8 of IPCC AR5 WGI to review the content and organization. Even though a lot of work on innovative metrics has been done since AR5, the user community has not changed its approach, and so the content in AR5 is still entirely relevant to metric users. [Ilissa Ocko, United States of America]	Rejected. We point to the AR5 material in a couple of places. We do not think it is valuable to reproduce it here. In this section we are explaining what is new in the physical science literature regarding emissions metrics. Being WGI, we think it is important that users understand the physical effects of different gases on the climate, especially temperature. Distinctions that are commonly drawn in other parts of environmental science and policy are relevant here, especially that between stock and flow pollution. Many users will be aware of these points, but the habit of thinking in terms of CO2e or GWP may have misled some users - they may not have realised these distinctions are relevant to greenhouse gases. We aim to make the relevant science clear, so that they can adapt (or not) their practices accordingly.
89945	108	23	116	20	AR5 had an entire section about metrics for near-term climate forcers, with subsections for each near-term forcer (such as black carbon). This was an incredibly valuable section clarifying how SLCFs relate to metrics. On the other hand, the metrics discussion in AR6 doesn't even mention black carbon, leaving users in the dark. I hope this is just an oversight and not intentional. Omitting discussion of very short-lived climate forcers will do a major disservice to the policy community looking for the best available information on metrics relating to all types of climate forcers. [Ilissa Ocko, United States of America]	Taken into account. Chapter 6 has primary responsibility for the treatment of short-lived climate forcings, though we can supplement this a little in our discussion of metrics. We have focused on the physical relationship between gases and the climate response to try to stimulate a better understanding of the issues arising from gas comparison exercises. We think this is the best way to get people to realise the point you made at the start of comment 68123. We have joint material with chapter 6 in the Technical Summary. We have now included GWP20 in the assessment.
77713	108	23	116	20	The material in this section is very policy relevant and highlighs the need to communiate clearly the range of factors that are included in the calculation of emissions metrics and their updates to policy. Further steps may be needed for this due to the complexity of these issues. [Emer Griffin, Ireland]	Taken into account. We have a tight word limit in the chapter, but have endeavoured to provide a clear, usable treatment.
89775	108	23	116	20	The entire section 7.6 has no discussion on near-term climate impacts. The metric listed that has a time horizon less than 100-year is GTP50, which does not consider climate impact up untill year 50. This would give the audience the impression that near-term climate change is not important, which is misleading and concerning. Even though mean temperature increase in the near-term may not emerge from internal variability of the climate system, it can still amplify many extreme events such as heat waves that the society and ecosystems have to respond to. As someone who researches climate impacts of SLCFs, I know the rate of warming that SLCFs are responsible for is just as important as the peak warming that LLCFs are responsible for, and we deperately need climate actions to address both. Only emphasizing long-term metrics would devalue the efforts to address SLCFs and near-term warming. Therefore, I strongly recommend to balance the emphasis of near- and long-term climate impacts in this section, by adding discussion on metrics with time horizon of 20-30 years that align with major mitigation efforts. [Tianyi Sun, United States of America]	Taken into account. In keeping with the thinking behind the rest of the chapter, we have focused primarily on physical relationships between emissions and the climate response. We have added some more material on the issue of timescale in our expanded section on multi-metric approaches.
114615	108	23	116	20	Check consistency if metric sections in Ch1 and in TS are consistent with this underlying assessment done here [Jan Fuglestvedt, Norway]	Taken into account. Thank you. Yes, we have worked on liaising more closely.
114629	108	23	116	20	This paper contains updates that may be relevant to this section: "Updated global warming potentials and radiative efficiencies of halocarbons and other weak atmospheric absorbers". Hodnebrog et al. (revision submitted to Reviews of Geophysics) [Jan Fuglestvedt, Norway]	Taken into account. Thank you for the reference. It has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64753	108	23	116	20	The current metrics section (Section 7.6) does a disservice to climate actions with near-term benefits. The only time horizons provided for metrics are 50, 100, and 500 years, which will misrepresent near-term climate impacts; be inconsistent with policy-relevant timescales; and undervalue the role that SLCFs can play in limiting near-term warming. This is a major deficiency and will either make the chapter irrelevant or more likely counterproductive to the implementation of actions that are consistent with the policy goals agreed to by the global community through the Paris Agreement. This also reinforces strategies by governments to delay action as many do not want to take actions in the near-term, because they can focus on long-term impacts which undervalue near-term actions. Therefore, the whole policy process is not well served by the approach this chapter takes. I strongly recommend reporting metrics with a 20-year time horizon in order to provide decision makers the option to assess near-term impacts of actions, be consistent with policy-relevant timescales of 10-20 years as well as midcentury targets, be consistent with previous IPCC assessment reports, and encourage implementation of mitigation actions of short-lived climate forcers. [Steven Hamburg, United States of America]	Part rejected and part taken into account. We disagree. We are focusing on the physical climate effects of forcing species, and in this section we are discussing recent literature which bears on the long-standing criticisms that traditional metric cannot simulate temperature response. New metrics can. This material is relevant, and squarely within the WGI domain. We do not discuss co-benefits or the costs of changing metrics because these are not physical science issues. We are now assessing GWP20 as asked.
64757	108	23	116	20	The section (Section 7.6) is written in a way that only scientists can really follow (very technical). However, the users of these types of metrics are rarely scientists, unless the scientists are trying to advance the field of climate metrics. Therefore, more attention needs to be given to improve the accessibility of the content; provide important background information; and reference specific metrics in a more consistent and comparable way. [Steven Hamburg, United States of America]	Taken into account. Comment noted and section carefully improved
64759	108	23	116	20	The discussion in Section 7.6 is focused on which metric is most scientifically defensible for certain applications, without attention to the accessibility of the metrics themselves nor a survey of preferences of the user community. There is therefore a disconnect between what the scientists propose as technically appropriate metrics and what metrics the user community will likely use. For example, GWP and CO2e are used almost exclusively in the climate policy community and already baked into policies and trading schemes (such as the Paris Agreement and EU Emissions Trading Scheme), and yet there are only a few mentions of CO2e in the entire metrics section and no definition of it at all. This is a sign of a clear disconnect between the scientific and user communities. Further, the past few decades have revealed that users resist alternative metrics and continue to rely on traditional ones; many favor the simplicity and familiarity of GWP, and there are often non-science reasons to stick with GWP (such as consensus among parties of the UNFCCC and existing tools such as Climate Action Tracker). The net result will be that the innovative metric-intensive section will not be relevant to policy makers and even more problematic will confuse the policy conversations and not contribute to moving forward based on the best available science. Therefore, more information regarding GWP and CO2e is warranted given that they are used almost exclusively by metric users. [Steven Hamburg, United States of America]	Noted. We discuss the physical science issues arising from attempts to compare forcings via emissions metrics. The issues of costs of changing metrics, co-benefits, and so on are discussed in WGIII. We think it is extremely important that we make clear the long-standing critiques of customary practice, and that there are newer ways of comparing gases that avoid these issues, and to highlight multi-metric approaches, which have also become more prominent since AR5.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64761	108	23	116	20	Widespread adoption of innovative metrics will require far more effort than the text in this section (Section 7.6) alone. But given the likely inability to pursue a resource- and time-intensive effort to train users to use the most scientifically appropriate metric for their specific application, along with the urgency of climate action, there are simple ways to improve the science of metrics but retain familiarity with the user community (and thus a much better chance for adoption) that are not discussed. For example, reporting GWP20/GWP100 or CO2e20/CO2e100 values simultaneously can convey impacts across near and long-term timescales and serve as a check to make sure that trades or targets do not provide near-term disbenefits to the climate (Ocko et al., Unmask temporal tradeoffs in climate policy debates, Science, 2017). Including this strategy as a 'bare minimum' approach to using GWP for several applications can improve the science with a good chance of success compared to uptake of innovative metrics. The IPCC can make it clear that a non-GWP metric is often more scientifically appropriate for comparing SL and LLCFs, but if its use is prohibitive --- which the record of the past 20+ years suggests it will be --- then this 2-valued GWP should be implemented rather than a single time horizon GWP, which would prioritize one timescale over another. Further, a two valued GWP is not nearly as complicated for a non-technical user to adopt as a new metric entirely (because they are just combining two already familiar metrics – and there are also several everyday examples of 2-valued metrics that have set the precedent for such an approach, including blood pressure (systolic/diastolic), gas mileage (city/highway), SAT (verbal/math), etc.) and this approach would also make clear that there is a time horizon embedded in the use of CO2e – a detail that many users I interact with are currently unaware of, for example when reporting the share of total greenhouse gas emissions from a particular country or sector. [Steven Hamburg, United States of America]	Noted. We have added more material on multi-metric approaches.
64763	108	23	116	20	Several climate metric users that my colleagues and I have spoken with from industry and government have made it clear that they will only change their GWP100 use if the IPCC tells them to do so. They take what is in the IPCC report as guidelines. The IPCC has made it clear that there are many metrics and that they don't recommend one in particular, and the AR6 reiterates that. However, this approach to intentionally be vague and open-ended actually does a disservice to the community, because it allows users to continue with the GWP100/CO2e status quo. It would considerably improve climate policy if the IPCC was more firm in articulating how certain approaches are misleading – such as the status quo. If the IPCC was more firm that no metric (keeping forcers separate) is best; some of these newer metrics are second best; but as a last resort/bare minimum a two-valued GWP should be adopted that includes two time horizons (see Comment 5), this small statement will have a huge return on clarifying the role of climate forcers in climate change in the near- and long-term. [Steven Hamburg, United States of America]	Rejected. The purpose of the section is to explain recent advances in research relevant to the emissions metrics, and by extension to the way gases are compared. It is neither arguing for or against changes to how different groups make these comparisons, though it is attempting to point out that emissions metrics can be more or less well-aligned with different targets and aims. Because of the explicit mandate of the IPCC to be policy-relevant but not prescriptive, we cannot make the recommendations that you suggest.
90229	108	23	116	21	We find that section 7.6, in particular section 7.6.3, is too policy prescriptive since it promotes GWP* as a new metric that is better suitable to quantify the surface warming. While we acknowledge that GWP* gives better representation of the short-term effect of emissions on radiative forcing and temperature, it seems to us not suitable for the long-term effect of GHG emissions. As the Paris Agreement however aims at limiting climate change in the long-term GWP* does not seem suitable for this objective. In addition, Ch07 should provide data for the full suite of metrics that had been provided in previous reports. [Georges Gehl, Luxembourg]	Rejected. The main claim in the comment is incorrect. Since GWP* and CGTP are both able to simulate the temperature effects of a time-series of greenhouse gases, they are better-matched for use in the context of long-term temperature goals than is GWP. See Cain et al., 2019, and Collins et al 2019, both of which use step-pulse metrics to simulate temperatures over time. However, step-pulse metrics, like cumulative CO2 emissions, only works from when the emissions timeseries is started, so if emissions data are presented from 1900, then warming will be simulated from 1900; if data are presented from 1990, then warming will be simulated from 1990. (Some of the critics of these approaches have not understood this point.)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96749	108	23	116	21	_Presentation of GHG metrics: We are very concerned about the presentation of GHG metrics in this report and request the authors to comprehensively revise the text. Section 7.6 , in particular section 7.6.3, is to a great extent policy-prescriptive since it promotes GWP* as a new metric that is better suitable to quantify the surface warming. This is not appropriate for the IPCC and not scientifically justified since it ignores the time dimension of the warming. GWP* only refers to the short term situation while the long term warming is dominated by LLCF. In GWP* a steady emission of SLCF and a pulse of CO2-emissions are treated as equivalent. This enables a better representation of the short-term effect of emissions on radiative forcing and temperature, but is not suitable for the long-term effect of GHG emissions. The Paris Agreement however aims at limiting climate change in the long-term and hence, GWP* does not seem suitable for this objective. In addition, the inherent temporal dimension of the GWP* might not be transparent for policy makers and it is not well explained in this report. The decision on the temporal dimension of the GHG metrics is however a political one. Please refrain from recommendations and explain the qualities of GWP* in a clear manner instead, also taking into account Schleussner et al. 2019 ('Inconsistencies when applying novel metrics for emissions accounting to the Paris agreement', https://iopscience.iop.org/article/10.1088/1748-9326/ab56e7). [Nicole Wilke, Germany]	Rejected. Step-pulse metrics such as GWP* and CGTP really are able to simulate the temperature effects of a time-series of greenhouse gases, and the customary GWP metric is not. Multi-metric approaches also represent an improvement over GWP in this regard. It is WGI"s responsibility to make this new science clear to policymakers. The commenter is incorrect about the science here. Given a time-series of greenhouse gases, GWP* simulates the temperatures over any time horizon. GWP does not. This is why GWP* provides a more accurate metric, if the purpose of the comparison is to consider the temperature effects of a time-series of gases. If one is concerned with long-term temperature stabilisation, then the ability to simulate temperatures would seem to be a valuable property in a metric.
23547	108	23			This section is a significant improvement compared to the FOD in how it presents and frames the issues of different metrics and link to policy objectives. Some more care is required though in wording, to further increase clarity and avoid misleading conclusions, which I flag in detailed comments on this section. [Andy Reisinger, New Zealand]	Noted. Thank you.
68133	108	25	108	44	The intro of Section 7.6 should be considerably strengthened. Here is how I recommend redoing the intro: Suggest moving definition of metrics in 7-108-53 ("Emission metrics are a simple way of representing the magnitude of the effect a unit mass emission of a species has on a key measure of climate change" to first sentence in intro section, then followed by first paragraph in intro. Suggest next paragraph discuss why metrics were designed and are needed. Then would pull text from Box (lines 7-114-34 to 7-114-38) and include it here in: "No single emission metric captures the relative roles of different emissions across all potential climate change variables of interest. No matter how it is done, the way emissions of different gases are compared is value-laden. Value judgements are implied or embedded in several choices which underpin emission metrics, such as the variable against which the comparison between forcing agents is made, as well as the associated functional form, and timescales across which comparisons are made." Then go into overview of uncertainties and challenges, specifically with comparing climate forcers with very different lifetimes. Finally followed by the fact that the most scientifically accurate quantification of how human activities impact climate involves not using metrics and keeping short- and long-lived greenhouse gas emissions separate in scenarios and reporting procedures. But that if metrics are still needed, this section provides information on various options and their strengths and weaknesses. [Ilissa Ocko, United States of America]	Rejected. We are satisfied with the current structure.
68135	108	25	108	44	Overall there are several key insights that are buried and scattered throughout the section, but that I think should be brought up right away in the intro: i.The purpose of metrics and why they were designed at all (right now in applications subsection at the end of the section) ii.The fact that the most scientifically accurate quantification of how human activities impact climate involves keeping short- and long-lived greenhouse gas emissions separate in scenarios and reporting procedures. iii.The fact that NO single metric can capture relative roles of different emissions across all potential climate change variables of interest. iv.Then specifying that if a metric is still necessary, this section provides information on the types of metrics, their scientific integrity, challenges, and applications. [Ilissa Ocko, United States of America]	Taken into account. Thank you for these comments. We have discussed these points with WGIII, and we have used them as a kind of narrative checklist in this section - we chose to not rewrite in the exact order suggested, but all these points are now covered and clearly articulated.
73937	108	25	116	20	The presentation of the metrics concepts of GWP and GTP has been much clearer in previous assessment reports. The chapter now needs to be read together with previous reports which presents a huge additional challenge for the readers. It would strongly benefit from adding clearer explanations of basic concepts from previous assessemnent reports. [Anke Herold, Germany]	Taken into account. The section has been rewritten. In this section we are primarily writing about the scientific accuracy of different metrics in terms of their ability to simulate the effects of different gases on climate. This involves a broader comparison than that between pulse metrics such as GWP and GTP.
114617	108	27	108	27	add "of" before "emissions" [Jan Fuglestvedt, Norway]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
83135	108	27	108	28	I find "some aspect of climate change" quite vague. In my opinion it has in general been related to the term "Dangerous Anthropogenic Influnece" in the UNFCCC [Terje Berntsen, Norway]	Taken into account. Phrasing changed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89777	108	27	108	44	The introduction to section 7.6 does not provide sufficient backgroud as it is written. The readers should be able to understand what are metrics, how and why they are designed, and the value judgements that are embedded in metrics before going into the new progress made since AR5. There are a few sentences in the section that would be very helpful to include in the first paragraphs of intro: line 7-108-53 “Emission metrics are a simple way of representing the magnitude of the effect a unit mass emission of a species has on a key measure of climate change”; lines 7-114-34 to 7-114-38 “No single emission metric captures the relative roles of different emissions across all potential climate change variables of interest. No matter how it is done, the way emissions of different gases are compared is value-laden. Value judgements are implied or embedded in several choices which underpin emission metrics, such as the variable against which the comparison between forcing agents is made, as well as the associated functional form, and timescales across which comparisons are made.” Then it can go into the two major challenges of metrics - align species with very different lifetimes and uncertainties in the cause-effect chain. At last, it should reinstate that no single metric can capture accruately the roles of different emissions across all potential climate change variables of interest. When a choice of metric is necessary, this section provides information on available types, their advantages and issues, and applications. [Tianyi Sun, United States of America]	Taken into account. Thank you for the comment, the introduction to 7.6 has been rewritten.
114619	108	28	108	28	Not necessarily "relative". (It is always difficult to know which way to introduce this; starting by absolute and then introducing relative, or starting with rerative…) [Jan Fuglestvedt, Norway]	Taken into account. It is usually relative, but not always. The statement has been rephrased.
129065	108	30	108	30	"some formula" reads too casual in this sentence. Recommend merging this sentence with the next sentence to read: "... according to formulae that are assessed by the climate modelling community and updated in Section 7.6.2." [Trigg Talley, United States of America]	Taken into account. Phrasing changed.
99373	108	34	108	44	Either this paragrap or the figure caption of Fig. 7.2 can include a statement indicating that the cause-effect chain is a simplification or a first-order representation (or perception) of how processes and feedbbacks operate in the earth system (implying that things like climate-carbon cycle feedbacks are omitted from the figure). [Katsumasa Tanaka, France]	Taken into account. Thank you for your comment. We chose to stay with our simplified explanation here for clarity. The paragraph has been rewritten.
69891	108	36	108	42	Policymakers should have access to multiple metrics, including metrics that allow for a two-basket approach and recognize the near-term impacts of SLCP (such as GWP20 and GTP20).. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII’s FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: “Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic].”). [Gabrielle Dreyfus, United States of America]	Taken into account. We are now assessing GWP20. In this assessment we have endeavoured to reflect new, policy-relevant literature, which is why step-pulse metrics have received prominence, and why we have discussed multi-metric approaches. In keeping with the rest of the chapter, we have attempted to be clear about physical relationships (such as the difference between) rather than focus on specific timescales.
68375	108	36	108	42	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC’s scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account. We are now assessing GWP20. If action on SLCFs comes at the expense of action on LLCFs, a warmer world results (Pierrehumbert, 2014). Action on LLCFs - specifically the need to get to net zero emissions of LLCFs - is a necessary condition of halting warming (at any level). This is not true of SLCFs, and it is why WGI are emphasising the centrality of LLCF reductions for climate policy. In this assessment we have endeavoured to reflect new, policy-relevant literature, which is why step-pulse metrics have received prominence, and why we have discussed multi-metric approaches. In keeping with the rest of the chapter, we have attempted to be clear about physical relationships (such as the difference between) rather than focus on specific timescales.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68377	108	36	108	42	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account. We are now assessing GWP20.
66813	108	36	108	42	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. We are now assessing GWP20. If action on SLCFs comes at the expense of action on LLCFs, a warmer world results (Pierrehumbert, 2014). Action on LLCFs - specifically the need to get to net zero emissions of LLCFs - is a necessary condition of halting warming (at any level). This is not true of SLCFs, and it is why WGI are emphasising the centrality of LLCF reductions for climate policy. In this assessment we have endeavoured to reflect new, policy-relevant literature, which is why step-pulse metrics have received prominence, and why we have discussed multi-metric approaches. In keeping with the rest of the chapter, we have attempted to be clear about physical relationships (such as the difference between) rather than focus on specific timescales.
66815	108	36	108	42	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. We are now assessing GWP20.
27175	108	38	108	38	The definition of GTP should be provided in the Glossary [Eric Brun, France]	Accepted. Comment noted and glossary clarified
114621	108	39	108	39	I suggest changing "lower" to "later" [Jan Fuglestvedt, Norway]	Taken into account. Phrasing changed.
51389	108	39	108	39	Not sure "policy relevance" is the correct phrase here. GWP100 is very policy relevant because that's what has been globally adopted as the common emissions metric. Similarly, temperature is important because that's been chosen as a global goal. Perhaps it's more accurate to say that those variables further down the cause-effect chain are closer to those things that societies value and experience? Suggest "policy relevance" is replaced with an alternative term. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. It is still "policy-relevant", even if it is not the default.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17829	108	39	108	40	It would also be worthwhile to point out that while impacts & damages on society & ecosystems are what we want to mitigate, the action lever we can use is to change emissions. [Marcus Sarofim, United States of America]	Taken into account. Sorry if this was not clear, section has been revised to be clearer
83137	108	40	108	40	Going down the cause effect chain does indeed add unceraint, but it also implies different levels of value judgements. E.g. choise of time-horizon. This should be noted. [Terje Berntsen, Norway]	Taken into account. Yes, we have tried to be clearer about value judgements and scientific aspects of emissions metrics.
106339	108	41	108	41	It would be useful to clarify "multi-metric approaches" when used here in the introduction. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you. This has been clarified.
104531	108	47	116	20	As written, Section 7.6 almost entirely prioritizes addressing long-term climate impacts, without acknowledgement of the importance of addressing near-term warming. It must be revised to include near-term warming metrics. Currently there is no metric time horizon less than 50 years. Even for 50 years, only GTP is reported which only includes temperature outcomes at the end of the 50 years and thus eliminates consideration of SLCF like CH4 and BC whose forcing is highly path and time dependent. The time horizons provided (50, 100 and 500 years) are also inconsistent with the Paris agreement timeline which set goals for the next 20-30 years. The use of long time horizons disconnects this chapter from the important policy context it seeks to inform. The description and use of GTP as the dominant new metric needs to be explained and justified in a fashion that is accessible to the policy maker and relates to GWP over 20, 100 and 500 years. . Without that, GTP will cause confusion in the policy community and be highly counter-productive. At this stage in climate negotiations it is important to have an easy way for diplomats to relate all GHG and SLCP to each other so the focus does not rest nearly entirely on mitigating CO2 and mitigation of the other constituents can be encouraged and credited.50 and 100 year time horizons are useful and important, but one needs to be able to establish and use metrics that separate out the near-term impacts, and this is what a 20 year time horizon is valuable for. These long-term time horizons undervalue the role that SLCFs can play in limiting warming in the near-term. Further, the IPCC risks users continuing to use IPCC AR5 for GWP20 (which will now have outdated radiative properties) and GWP100 from IPCC AR6. These values will now be inconsistent as the underlying physics is inconsistent. To be consistent, users may just ignore IPCC AR6 updated GWP100 values, which is a shame because of the advancement of science since the last report. d.Therefore, I strongly recommend reporting metrics for greenhouse gases with a 20-year time horizon, at least for GWP, in order to: (1) provide an option for decision makers who need to assess near-term impacts of emissions, (2) be consistent with policy-relevant timescales of 10-20 years as well as midcentury targets, and (3) encourage implementation of mitigation actions of short-lived climate forcers by conveying their major role in limiting near-term warming. [Denise Mauzerall, United States of America]	Taken into account: GWP20 has been added
104533	108	47	116	20	There are several inconsistencies between Section 7.6 and the IPCC at large that need to be addressed. a.First, there are inconsistencies across IPCC AR6 drafts in terms of the roles of near- and long-term warming, SLCFs vs LLCFs, and near- and long-term metrics; WGI Chapter 6 does acknowledge short timescales and the role of near-term warming, whereas these are deemphasized in Chapter 7, and WGIII includes more discussion of GWP20 and other short-term time horizon metrics. There needs to be consistency across IPCC AR6 regarding the importance of climate change over all timescales, the roles of different pollutants in contributing to warming over different timescales, and metrics that convey climate impacts across all timescales. b.Second, the metrics section is inconsistent with previous assessment reports in that certain metrics/time horizons (such as GWP20) are absent for the first time since its inception, and without any explanation. This creates a discontinuity between reports and does not allow for active users to receive updated values that reflect the latest science. This also could lead to users continuing to use IPCC AR5 for GWP20 along with GWP100 from IPCC AR6 or ignoring IPCC AR6 updated GWP100 values altogether.c.Third, the section states that "Limiting on-going temperature increase at any level requires net zero CO2 emissions, and while stabilising, reducing or eliminating short-lived forcing agents can play a secondary role, the main requirement for stabilisation of temperature is to limit cumulative emissions of CO2," yet the IPCC Special Report on 1.5 Degrees (2018) was clear that considerable emissions reductions of methane and black carbon are required to achieve temperature targets of 1.5 or 2C targets. And fourth, the section itself has an inconsistency in that it states that decision makers should decide which timescale is relevant for their specific application, yet no near-term metrics are provided. [Denise Mauzerall, United States of America]	Taken into account: GWP20 has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
104535	108	47	116	20	There is a major disconnect between Section 7.6 and the relevant audience. The majority of the discussion focuses on innovative metrics designed to improve the underlying science connecting emissions to impacts such as radiative forcing and temperature change. As such, the IPCC seems to be distancing itself from the traditional and widespread GWP metric in favor of more technical and scientifically defensible metrics (GWP is barely discussed and GWP20 is dropped from the tables). However, major barriers exist that will prevent users from changing and adopting these new metrics. First, the past few decades have revealed that users resist alternative metrics and continue to rely on traditional ones; many favor the simplicity and familiarity of GWP, and there are often non-science reasons to stick with GWP. For example, governments were provided a framework (GWPs) by the IPCC decades ago and there are political reasons why they will continue with the status quo. Second, GWP is already baked into policies and trading schemes, several climate policy tools rely on GWP, and recent reports from major institutions are written in terms of GWP . Third, many metric users are non-scientists, and the highly technical scientific papers and discussion in this section alone will be prohibitive. If the IPCC really wants users to move away from GWP, a concerted effort to train users – far beyond the contents of this report and the associated scientific articles – would be necessary, and with the urgency of actions to address climate change, we want to avoid confusion and delay due to lack of clarity on metrics. Acknowledgement of the history and use of GWP, and its counterpart CO2e, would also be very valuable to the section's intended audience and justify its continued placement in the IPCC despite improved metrics. There are also simple ways to improve the science but retain familiarity with the user community (and thus a much better chance for adoption), for example by reporting GWP20/GWP100 or CO2e20/CO2e100 values simultaneously to convey impacts across near and long-term timescales using a 2-valued metric. [Denise Mauzerall, United States of America]	Taken into account: GWP20 has been added
104537	108	47	116	20	Not including near-term time horizons will undervalue the role of SLCFs in near-term warming and could lead to reduced ambition to mitigate their emissions, of which there are major benefits of early action including improved air quality and food security in addition to reducing radiative forcing. Including a 20-year time horizon is essential in order to (i) provide an option for decision makers who need to assess near-term impacts of emissions, (ii) be consistent with policy-relevant timescales of 10-20 years as well as midcentury targets, and (iii) encourage implementation of mitigation actions of short-lived climate forcers by conveying their major role in limiting near-term warming. [Denise Mauzerall, United States of America]	Taken into account: GWP20 has been added
51391	108	49	108	49	Will WG3 cover other metrics not related to physical changes? If so, suggest that this is explicitly mentioned here. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: A mention of WG III has been added
83695	108	50	108	50	It would be useful to indicate what policy purposes the GWP100 metric has been used for here i.e. explicitly state/quote "UNFCCC 2018. Decision 18/CMA.1, paragraph 37" - the UN climate change reporting rules require it to be used, and an alternative IPCC approved metric can also be used; the NDC target accounting rules are required to mimic the reporting rules. Other current uses we are aware of which could be mentioned here include footprinting, and life cycle analysis. [Dan Zwartz, New Zealand]	Rejected: This section is explicitly about the physical description of metrics
51393	108	51	108	51	Delete "yet" as it implies a judgement. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Yet has been deleted.
51395	108	54	108	55	Recalling the sentence in l.49, the focus of this section is on physical changes, while there are other metrics that focus on non-physical changes. Therefore, the list of examples given here on key measures might be somewhat confusing to the reader who is seeking to understand the full suite of options. Suggest that it is clarified that the examples provided here are only for physical changes, and that there may be other non-physical measures of climate change. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: The range of metrics has been clarified
31791	109	2	109	3	This bit on regional response is confusing, as the sentence is as true of CO2 as SLCFs. CO2 has a quite strong eq-pole variation in forcing pattern and strong regional variations in temperature response (I know you know this!). I guess the point is that the regional variation from CO2 can be well characterised using the global-mean response plus pattern scaling, but this is not appropriate for all forcings. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been rephrased
114623	109	4	109	5	Regarding mentioning CH4 as reference gas: I suggest you try to make this point a bit more clear or leave out. (Using CO2 as reference gas also gives a dimensionless metric) [Jan Fuglestvedt, Norway]	Taken into account: CH4 has been left out

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66817	109	13	109	14	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account: GWP20 has been added
66819	109	13	109	14	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account: GWP20 has been added
68379	109	13	109	14	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account: GWP20 has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68381	109	13	109	14	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account: GWP20 has been added
114625	109	13	109	14	This last sentence gives the impression that there is one single correct way to do this comparison between SLCF and CO2. I suggest you reformulate to something like "developments in approaches for comparing SLCF to CO2 in the context of mitigation" [Jan Fuglestvedt, Norway]	Accepted: This change has been made.
99375	109	14	109	14	A statement on Section 7.6.2.5 is missing, for the sake of completeness. [Katsumasa Tanaka, France]	Accepted: a reference to 7.6.2.5 has been added.
106341	109	14	109	14	SLCF = short-lived "climate" forcers [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This change has been made.
68137	109	17	109	34	This section (7.6.2.1) goes straight into parameters. Strongly suggest some contextual information about how radiative properties are a fundamental and foundational component of most (all?) metrics, and that a lot of work to improve understanding has been done since the last report. [Ilissa Ocko, United States of America]	Accepted: The radiative properties have been introduced
23549	109	17	109	34	It seems odd to have Etminan et al as the only reference in this section. It leaves unclear what specific reasoning was applied in the assessment to arrive at the specific numberts presented in lines 27-29. This may not require references to the primary literature, but at least a careful cross-referencing to other sections of the WGI report so that it becomes transparent where exactly those specific numbers are coming from, and what explains the changes compared to AR5. Right now the numbers are given "ex cathedra", with qualitative explanations for the directions of change, but no traceable account for their quantification (unless Table 7.15 is meant to do this fully?) [Andy Reisinger, New Zealand]	Accepted: Section 7.3.2 has been referenced here
89779	109	19			The readers need more context on how radiative properties are so fundamental in developing any metrics before going into the progress made since AR5. [Tianyi Sun, United States of America]	Accepted: The radiative properties have been introduced
23551	109	32	109	32	My understanding is that accounting for rapid adjustment results in a lower RF, not an increase as currently stated? [Andy Reisinger, New Zealand]	Accepted: This has been rephrased
68139	109	37	110	27	Would appreciate an initial sentence to this section (7.6.2.2) providing some context on how this builds from previous section, such as once we have radiative properties, we can use analytical expressions and models to calculate how emissions impact various climate parameters, such as forcings, temperature change, precipitation, sea level rise, damages etc. [Ilissa Ocko, United States of America]	Accepted: The analytical derivation has been explained
68141	109	37	110	27	As currently written, this section (7.6.2.2) dives straight into various metrics, acronyms, and technical features, without a sense of any logical arrangement, and thus will be overwhelming and inaccessible to the user community that is the audience for this topic. [Ilissa Ocko, United States of America]	Accepted: The analytical derivation has been explained
68143	109	37	110	27	The discussion of the instantaneous/endpoint aspect of all metrics is critical, and I think should be moved to before discussion of specific metrics. There can also be some mention here about the importance of time horizon in affecting the value of a metric, and this is discussed in more detail in section [insert]. [Ilissa Ocko, United States of America]	Taken into account: The time horizon has been discussed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68145	109	37	110	27	The term "quantities" in the title of Section 7.6.2.2 suggests that this is the section discussing values of the metrics. Rather, it just discusses physical *indicators* - so I recommend changing title of this section to "physical indicators." [Ilissa Ocko, United States of America]	Accepted: This has been changed.
96751	109	37	115	21	We are surprised that the discussion of different metrics does not focus more on the fundamental difference between endpoint / instantaneous metrics and integrated metrics. This difference should dominate the value for short vs. long lived forcers and be more important than the quantity chosen, e.g. forcing or temperature in GWP and GTP. This is only mentioned in 110-25, but not discussed earlier, e.g. 109-51. Please provide comparisons that disentangle the effects of the physical quantity considered, endpoint/integrated and possibly normalisation to improve transparency and understanding of these different approaches, in particular in Box 7.3. It would also be useful to mention how these metrics are used in practice (GWP as integral and GTP as end-point metric). [Nicole Wilke, Germany]	Taken into account: The difference between endpoint and integrated has been highlighted.
23553	109	39	109	40	I think this first sentence is the wrong way round - it makes it sound as if IRFs (which I presume is meant by 'analytical expressions') are somehow more complex/fundamental than the results of (simple) climate models. I would argue the opposite: IRFs are the most simple way of representing the response of some part of the climate system to an emission; simple climate models are more complex and fundamental because they actual model some specific processes and hence their responses can depend on specific input assumptions that can then also change over time (e.g. changes in feedback strengths and radiative efficacy); complex climate models give the most complex but also most cumbersome representation of real-world outcomes. (This is also the logic presented in chapter 1 of the WGI SOD.) In addition, some of the models referred to in the references given are also more complex than one would normally consider as a 'simple' climate model; and even MAGICC and FAIR have non-trivial representations of some important aspects of the climate system, such as climate-carbon cycle feedbacks. So I would change the sentence to read (keep the first part) "... Gasser et al., 2017), but can be more simply represented by analytical expressions derived from climate models of varying complexity." [Andy Reisinger, New Zealand]	Taken into account: This has been reworded to explain that analytical expressions represent the models
114665	109	39	109	40	It is a question what is most fundamental here. The IRF_dT and lifetimes as well as radiative effeciencies are derived from more detailed models. So you may consider the explanation of how this is built up from detailed models to analtical expressions [Jan Fuglestvedt, Norway]	Taken into account: This has been reworded to explain that analytical expressions represent the models
51397	109	39	109	49	It is difficult to follow this paragraph or to understand what the key message or purpose of it is. For example, it's not clear to the reader what "can be derived by convolving the radiative forcing with a temperature response function" means in layperson's terms. Similarly, how is the precipitation metric derived from a combination of AGTP and AGFP? Could each metric be described in a simpler manner, including a sentence on why it's important to know this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This explanation has been simplified
89781	109	39	110	27	This section is hard to follow. It goes right into acronyms and technical features that would be inaccessible to the user community. It would help to have a transition from the previous section - radiative properties. In addition, the implications of time embedded in metrics are discussed before introducing the concept of time horizon. I suggest moving the last paragraph before the current second paragraph that dicusses timescales and SLCFs. [Tianyi Sun, United States of America]	Taken into account: This explanation has been simplified
99377	109	40	109	40	It is unclear what "more fundamentally" indicates. I think this can be just removed. [Katsumasa Tanaka, France]	Taken into account: This has been reworded to explain that analytical expressions represent the models
83139	109	40	109	40	I disagree that a metric based on an analytical formula is more fundamental than those based on simple climate models. However, they are more transparent. [Terje Berntsen, Norway]	Taken into account: This has been reworded to explain that analytical expressions represent the models
2735	109	46			define AGFP [Bryan Weare, United States of America]	Noted. Metric no longer used in the chapter
31789	109	51	109	51	I find this Para a bit confusing, as the perspective seems to assume an endpoint rather than an integrated metric and could confuse as to why SLCFs are smaller for gtp than gwp at long time horizons. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The endpoint/integrated discussion has been moved up
32955	109	51	109	52	Longer timescales of response actually lead to lower values for SLCF metrics going from RF to temperature, not higher, e.g. GTP100 vs GWP100 for methane. This is because the temperature is weighted toward recent forcing whereas GWP is equally weighted over time, so for long time horizons GTP decreases the effect of an SLCF more than GWP. For short time horizons there is much less difference across metrics (e.g. GWP20 and GTP20 are similar). [Drew Shindell, United States of America]	Taken into account: The endpoint/integrated discussion on the role of time periods has been clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103629	109	51	109	55	The assertion that SLCFs become relatively more important for SLR than for temperature or radiative forcing needs to be explained better. Is there any inherent reason why SLCFs are more important, or is it just because the SLR metrics assume a flow of emissions, while the RF & temperature metrics assume a pulse? [Philippe Tulkens, Belgium]	Taken into account: The effect of integrated metrics has been discussed
66171	109	51			The statement here is correct ("Each step from radiative forcing to temperature to SLR includes longer timescales and therefore prolongs further the contribution of short-lived species."), but the chapter is missing an equivalent statement about timescales of the SLCF+N2O perturbations due to chemical feedbacks. These are an essential part of the metrics and need to be reviewed and included explicitly as part of the time delay. For example, the SPM has mistaken statements that the mitigation of short-lived SLCFs like NOx would 'emerge' within a year. NOx is a short-lived species but the impact on CH4 makes the perturbation last for decades. [Michael PRATHER, United States of America]	Taken into account: This is now discussed in section 7.6.2.5
114627	109	52	109	53	This could need some more explanation [Jan Fuglestvedt, Norway]	Taken into account: This explanation has been revisited
77715	109	52	109	55	Include examples of SLCFs here e.g. does this refer to balcak carbon, ozone? [Emer Griffin, Ireland]	Accepted: Examples have been given
2737	109	52			define SLCF [Bryan Weare, United States of America]	Rejected: This is defined previously
83141	109	53	109	55	This is quite technical and could either be simplified, e.g. SLR dependens on how much heat is added to the ocean … If kept as now, it should be spesified that it is the radiative imbalance at TOA. [Terje Berntsen, Norway]	Taken into account: This explanation has been revisited
77717	110	2	110	6	Can situations where such regional variations are used or would be imporntant be included? [Emer Griffin, Ireland]	Rejected: There is no intention to further explore regional variations beyond that discussed here.
103631	110	2	110	6	Is it possible to comment on the comparability of global and regional temperature/forcing? In other words, to what extent is it reasonable to target regional forcing as a contribution to limiting global temperature increase? [Philippe Tulkens, Belgium]	Rejected: There is no intention to further explore regional variations beyond that discussed here.
83143	110	8	110	8	Maybe discussed later, but the relation between emissions and ERF are not necessarlity linear. [Terje Berntsen, Norway]	Taken into account: The non-linearity of emission sizes has been added.
106343	110	8	110	9	This statement would benefit from references supporting it. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The non-linearity of emission sizes has been added.
23555	110	8	110	14	This para mixes up linearity of metrics to RF (line 9), and linearity to the size of emission (which is what actually matters for an emissions metric, e.g. line 13). Neither GWP nor GTP nor most other metrics are strictly linear to the size of emission if the emission is large enough (because a large enough pulse will change radiative efficacy by changing global concentration). Conversely, global damage potential metrics ARE linear to the size of emission as long as emissions are small enough. I.e. the marginal damage caused by emitting 2 tons of CH4 is, to a very high degree of accuracy, twice the damage of emitting 1 ton of CH4 - so linearity does hold for practical purposes as long as the emission is small enough (which is the case for almost any individual emission source, be it a sector or a country). I suggest this para is revised to be clearer about how linear metrics are to the size of emissions, and for what size of emission the linearity starts to become problematic. [Andy Reisinger, New Zealand]	Taken into account: The non-linearity of emission sizes has been added.
77719	110	10	110	12	Do the social costs include impacts on human health and ecosystems for SLCFs? [Emer Griffin, Ireland]	Rejected: This section focusses on the physical impacts so there is no need to add further description of health
17831	110	10	110	12	Note that Mallapragada and Mignone (2020) also addresses the same subject as Sarofim & Giordano from a more theoretical viewpoint: https://link.springer.com/article/10.1007/s10584-019-02486-7 [Marcus Sarofim, United States of America]	No longer applicable: This sentence has been removed.
99379	110	11	110	14	The metric CCIP (Kirschbaum, 2014, ERL, doi:10.1088/1748-9326/9/3/034014) is also a type of impact-based metrics that can be included here. [Katsumasa Tanaka, France]	Rejected: This section does not intent to include an exhaustive list of metrics
66173	110	14			CH4 data unchanged? Please clearly state that the feedback factor increasing the effective lifetime of a perturbation, as well as the budget lifetime, is reviewed in 6.2.2.4 (and has not changed since AR5). [Michael PRATHER, United States of America]	Taken into account: The methane contributions has been revised following chapter 6

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
103633	110	16	110	27	It would be useful to mention here to provide a more complete explanation of how integral and end-point metrics are used in practice: i.e. that GWP is typically expressed as an integral metric, while GTP is typically expressed as an end-point metric, but that the alternative is possible and equally valid in both cases (end-point GWP and integral GTP) - as explained in Collins et al 2019. See also page 111 lines 53-55 which mention the possibility of integrating AGTP to obtain iAGTP. Why not also mention the possibility of normalising this to CO2 (creating iGTP). This would improve transparency by providing a 'pure' comparison between the forcing and temperature metric, whereas the 'standard' metrics of GWP (integrated) and GTP (end-point) conflate two different effects (temp vs forcing & integrated vs end point). [Philippe Tulkens, Belgium]	Taken into account: The endpoint/integrated discussion has been expanded
51359	110	17	110	18	The sentence "These are appropriate when the goal is to not exceed a fixed target such as a temperature limit or sea-level rise limit at a specific time." should be altered to avoid the perception of being policy specific, perhaps by changing the beginning of the sentence to 'These may be more relevent to policy when the goal...' or 'These may seem more intuitive when the goal...' or something similar. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been rephrased
51399	110	22	110	23	Radiative forcing can be quite an abstract concept for many non-experts so it might be useful to also put it and GWP's integrated nature in terms they might be able to visualise. e.g. "GWP could be described as a measure of the total amount of energy added to the climate system over a particular time horizon" or similar. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: Radiative forcing is explained in detail right at the beginning of the chapter
66821	110	23	110	27	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account: GWP20 has been added
66823	110	23	110	27	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account: GWP20 has been added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68383	110	23	110	27	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account: GWP20 has been added
68385	110	23	110	27	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account: GWP20 has been added
44323	110	25	110	27	Should it be noted that this is a problem, because GWP100 therefore overweights the relative cliamte impacts of SLCPs? Surely it is not enough to just state they are relatively higher integrated metrics compared to endpoint metrics. They are physically not representative (Allen et al. 2018, Cain et al. 2019, Lynch et al 2020 on demonstrating GWP* metric , Jenkins et al. 2018 'Framing climate goals in terms of cumulative CO2-foricng-equivalent emissions'). [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: A statement on the relevance of endpoint and integrated metrics has been added.
73913	110	25	110	27	The effect of the integration on the different metrics should be displayed for the key gases (at least CO2., N2O, CH4 and some relevant SLCFs) in a table added after this paragraph in this section: In chapter 7 the data shown is only comparing GWP (integrated) and GTP (non-integrated), but not the integrated version of 'GTP' (was is called iGTP? in earloer assessment reports) whch has been presented in earlier assessment reports. For the reader, it is important to understand the differences which are clearer when the values are presented. [Anke Herold, Germany]	Rejected: the difference between GTP and iGTP was discussed and values presented in AR5. The algorithms for all metrics are provided in the supplementary material so these values may be calculated by the interested reader.
51401	110	25	110	27	This sentence seems a little out of place here and a partial discussion of the implications of these different types of metrics, focusing on SLCFs only. Could this be expanded to a fuller discussion on the implications of the differences between integrated and endpoint metrics? For example, it would be useful to refer to the paper by Peters et al. (2011 doi:10.1088/1748-9326/6/4/044021) which is still relevant to this discussion. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: A reference to Peters et al. has been added.
99381	110	26	111	30	Regardless the origin, oxidation of methane leads to a production of CO2. This paragraph needs a clarification for under which circumstances the correction factor of 2.75 is needed, especially regarding the accounting system. [Katsumasa Tanaka, France]	Taken into account: The fate of CO2 from biogenic methane has been added.
114631	110	27	110	27	I think you could add more references here [Jan Fuglestvedt, Norway]	Taken into account: More references have been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23557	110	30	111	34	The greater clarity around carbon cycle responses is very useful. I have only one concern, which is that I'm still not entirely clear/convinced why using Gasser et al is justified as proxy for the median/best estimate for the strength of climate-carbon cycle coupling based on the best currently available knowledge (which is what the assessment should deliver). Additional explanation/justification for why you use Gasser et al as the single study to represent the sum total of best available knowledge would be useful. There's a fairly high bar to justify this, and the text is too vague in my view: line 53 says "Gasser is based on parameters derived from CMIP5 models" - but does that mean it was calibrated to the median or mean response across the full suite of CMIP5 model runs (I didn't have that impression, but I may be wrong)? Or did it simply try to emulate some aspects? It's not clear to me what it means that the "climate-carbon feedback magnitude is SIMILAR to the CMIP5 multi-model mean" - either Gasser is emulating the mean, or it is not. I also need to point out that Reisinger et al (2011), doi:10.1029/2010GL043803, provides probabilistic estimates of GWP and GTP (using C4MIP results and calibrated to emulate a CH4 GWP of 25 consistent with the AR4). That study found a mean GTP100 for CH4, including climate-carbon cycle feedbacks, of of 7.6 using the CMIP3/C4MIP model range (3.9-13.5 90%ile), and 8.7 (4.2-14.7 90%ile) using a probabilistic approach, both using MAGICC as emulator for CMIP3 and/or C4MIP model results (i.e. not simply using static IRFs). The results from Gasser et al are certainly within that range, but the probabilistic evaluation in Reisinger et al gives higher mean values - so, has the strength of climate-carbon cycle feedbacks been reduced between C4MIP and CMIP5? I wonder whether Reisinger et al (sorry about the self-citation) should not be included in this assessment even though it is not a new study, but highly relevant for this issue. [Andy Reisinger, New Zealand]	Taken into account: More work on carbon-response has been added. This is now a whole paragraph of the FGD. Additional clarity around the method has been added and in response to your suggestion we now also average the results from Gasser et al and Sterner and Johansson (2017). your earlier paper is not based on the latest climate model data so is not used here
68147	110	30	111	34	I would appreciate the first sentence of Section 7.6.2.3 as context of how there is an additional component of radiative effects beyond specific radiative efficiencies of each gas – which include carbon cycle feedbacks and chemistry interactions, rather than starting off with what AR5 did with carbon cycle. [Ilissa Ocko, United States of America]	Accepted: An introductory sentence has been added.
68149	110	30	111	34	I actually think that this section (7.6.2.3) should come before "Physical quantities" (7.6.2.2) because physical quantities rely on this information. So the logical order seems to be radiative properties of climate forcers, radiative impacts of carbon cycle responses and other indirect contributions, and then moving beyond these radiative properties to physical indicators of climate change – such as forcings, temp, precip, SLR, damages, etc. [Ilissa Ocko, United States of America]	Rejected: The current order is preferred
103635	110	32	110	33	Please be more explicit about the statement that including carbon cycle responses constitutes more of the causal chain displayed in Figure 7.2. Which is the 'extra' link in the chain compared to a metric that does not include these responses? [Philippe Tulkens, Belgium]	Accepted: This phrase has been deleted.
96753	110	32	110	33	Please explain why "including carbon cycle responses" allows considering a "more of the causal chain displayed in Figure 7.2". [Nicole Wilke, Germany]	Accepted: This phrase has been deleted.
20453	110	34	110	35	This may of course be true, although taking the example of a pulse on the solar constant, it is not clear why and how the resulting increase in GSAT would influence carbon fluxes. Would other parts of the report be of any help? The focus seems very generally on surface warming resulting from CO2 emissions rather than the other way around. Please explain or give references. Collins et al do mention the relationship but do not explain it. [philippe waldteufel, France]	Taken into account: The link between temperature and carbon fluxes has been linked to Ch 5
93637	110	34	110	37	The carbon cycle response from non-CO2 agents (where additional CO2 is released from temporary reservoirs such as ocean/biosphere) appears to be a feedback that must be attributed partly to the particular agent in question, and partly to the higher level of CO2 in these reservoirs. Please clarify if this attribution is adequately addressed when accounting for carbon cycle responses. [Jon Magnar Haugen, Norway]	Taken into account: The link between temperature and carbon fluxes has been linked to Ch 5
114633	110	36	110	36	You may add a ref to Joos et al after "pulse of CO2" [Jan Fuglestvedt, Norway]	Accepted: This change has been made.
51403	110	38	110	38	Could more detail be added on how it affects calculations of allowable carbon budget? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The effect on the carbon budgets has been explained
16001	110	43	110	45	It should be explained that this response will most likely be asymmetrical as the temperature and carbon will take longer to return to the previous equilibrium, as demonstrated by the saw tooth profile of the Vostok Ice core data. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: The paleo timescales are not discussed here.
83145	110	50	110	50	I agree that including the carbon cycle response (which is just one feedback mechanism) for the non-CO2 agents makes it more consistent. But there is a danger of double counting if AGTPs are used as simple climate models and the response functio rnelating ERF to temperature is already including teh carbon cycle feedback. [Terje Berntsen, Norway]	Taken into account: It has been explained that the response functions could include the carbon cycle feedback.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114635	110	50	110	51	Re "high confidence": you mean for both non-CO2 and CO2? For non-CO2 there are not many papers out there on how this can be done in metrics. But on the other hand the dT-carbon cycle feedback in general is well understood. So some more nuances may be needed here. [Jan Fuglestvedt, Norway]	Taken into account: This has been clarified that there is high confidence in the method of using carbon cycle models.
106345	110	50	111	4	These confidence statements need to be adequately strengthened by making explicit the lines of evidence that lead to this high and medium confidence. Some of the evidence might be provided in earlier paragraphs, but making the authors' assessment of their value explicit and transparent would be essential. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This has been made more explicit
17833	110	50	111	4	It is clear that if climate-carbon feedback responses are included in the AGWP of CO2, they should also be included in the AGWP of non-CO2 forcers before calculating the GWP (or other metrics). However, it appears that there are differences that arise based on the choice of climate-carbon feedback response for non-CO2 GHGs (see Gasser et al. vs. Sterner & Johansson). It appears to me that this may arise because the climate-carbon feedback for the non-CO2 gases is being modeled differently than the climate-carbon feedback for CO2. This could be resolved if climate-carbon feedbacks were removed from the calculation entirely for both CO2 and non-CO2 gases. It would be of great value if the IPCC would present such a calculation, at least for the GWP100. A GWP100 without climate-carbon feedbacks would be simpler, easy to calculate for a novel gas, and more transparent. [Marcus Sarofim, United States of America]	Taken into account: The possibility of removing c-cycle has been mentioned, but not quantified.
21139	110	54	110	54	The reference given (Lade et al. 2018) summarises the results from different feedback assessments of CMIP5 models. However the authoritative reference is Arora et al. (2013). I recommend adding this as a reference for this sentence. Reference: https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-12-00494.1 [Steven Lade, Sweden]	Accepted: This reference has been added.
3363	110		8	14	It is essential to have recourse to examples of a broadening of ideas [Eduardo Erazo Acosta, Colombia]	Noted: No suggestions given
51361	111	1	111	3	While the explanation of the two models that have attempted to calculate the climate-carbon response is clear, it is unclear why this has lead to an error or +/- 100%. This seems like an arbitrary figure as opposed to one based on any analysis. Is it simply because the value is assumed to be between zero and that of the Sterner study, which happens to be roughly equal to the Gasser study +/- 100%? If this is the reason, this would imply that the possibility of both studies underestimating the true figure is not included in the error. This could be clarified. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	This has been taken into account: More description of the carbon-response added
114637	111	1	111	4	I think the discussion on climate - carboncycle feedback can be made more clear. The medium confidence on cc fb for non-CO2 in metrics seems reasonable, but it is a bit confuing that way high confidce a few lines up is introudced. [Jan Fuglestvedt, Norway]	This has been taken into account: More description of the carbon-response added
23559	111	2	111	3	As noted in my main comment on section 7.6.2.3, Reisinger et al (2011), doi:10.1029/2010GL043803, provide a probabilistic assessment of GTP including climate-carbon cycle feedbacks (from C4MIP), which is a relevant study for this issue that can also help provide an estimate of the uncertainty (although the contribution to uncertainty specifically from the climate-carbon cycle feedback has not been separated out in that study). [Andy Reisinger, New Zealand]	Taken into account: More work on carbon-response has been added. This is now a whole paragraph of the FGD. Additional clarity around the method has been added and in response to your suggestion we now also average the results from Gasser et al and Sterner and Johansson (2017). your earlier paper is not based on the latest climate model data so is not used here
106347	111	6	111	9	An additional study highlighting such a link is: Mahowald, N.M. et al., 2017: Aerosol Deposition Impacts on Land and Ocean Carbon Cycles. Current Climate Change Reports, 3(1), 16–31, doi:10.1007/s40641-017-0056-z. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This reference has been added.
22209	111	6	111	24	It is probably worth checking whether this passage raises any fundamental inconsistencies with the assessments performed in chapters 5 and, in particular, 6. Chapter 6 I think came to some high confidence findings on some of these issues so their non-inclusion may be problematic from a whole of report perspective? [Peter Thorne, Ireland]	Taken into account: Chapter 6 has been referred to
106349	111	10	111	10	References to the studies that have been assessed by the authors to reach their assessment here should be included here. If this assessment is based on the four references in the previous sentence this should be made explicit, together with how this limited number of studies translates into robust evidence. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: References to chapter 6 discussions have been included.
77721	111	11	111	12	Can this be explained? Some impacts such as strtaospheric ozone loss are problematic but may appear to reduce the climate impacts. The treatement of these issues may need to be more nuanced rather than considered idea. [Emer Griffin, Ireland]	Taken into account: References to chapter 6 discussions have been included.
23561	111	21	111	21	delete "from" [Andy Reisinger, New Zealand]	Accepted: This change has been made.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96755	111	22	111	24	Is it really the case that there are no new findings about the contribution of ODS? We kindly ask the authors to assess the findings of Polvani, L.M., Previdi, M., England, M.R. et al. Substantial twentieth-century Arctic warming caused by ozone-depleting substances. Nat. Clim. Chang. 10, 130–133 (2020). https://doi.org/10.1038/s41558-019-0677-4 [Nicole Wilke, Germany]	Taken into account. This is out of scope here However, we now refer to the ODS discussion in Chapter 6.
66825	111	26	111	30	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account: GWP20 added
66827	111	26	111	30	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account: GWP20 added
68387	111	26	111	30	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account: GWP20 added

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68389	111	26	111	30	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account: GWP20 added
106351	111	26	111	30	It would be useful to highlight explicitly that the contribution of CO2 produced from the oxidation of CH4 is not included in the metric values (as was also done in AR5), and that this was also already the case in all earlier assessment reports. This is a recurring misunderstanding with stakeholders interested in biogenic methane. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: The CO2 from biogenic methane has been added
22211	111	26	111	30	This is assuming that all methane gets removed via oxidation. While most does it is worth being consistent with chapters 5 and 6 who I think alluded to additional removal pathways which may not lead, for a small %age to CO2 production. As such, the addition here may need to be proportionately scaled down. However, this point is really important to make so I support its retention overall! [Peter Thorne, Ireland]	Taken into account: More work on fate of oxidised CO2 has been added
77723	111	27	111	28	How is this apparent in the tables that are provided? [Emer Griffin, Ireland]	Taken into account: The contribution of CO2 in the tables has been clarified
77725	111	27	111	28	This should be "additional fossil CO2" rather than "new CO2" which is added to the atmosphere. [Emer Griffin, Ireland]	Accepted:: This change has been made
32953	111	27	111	28	Indeed 1 kg CH4 leads to 2.75 kg CO2 if every atom of C from methane is oxidized to CO2, but some will be removed along the way. We discussed this in Shindell, Collins & Fuglestvedt (2017) in which we reported that "In the GISS ModelE2, 88% of the C emitted as fossil CH4 is eventually oxidized to CO2, with the remainder lost via the oxidation products CH3OOH and HCHO (61% was reported in another study (Boucher et al, 2009))." I would thus suggest that the likely yield is ~1.5-2.5 kg CO2. References are: Shindell, D., J. S. Fuglestvedt, W. J. Collins, The Social Cost of Methane: Theory and Applications, Faraday Disc., 200, 429-451, doi: 10.1039/C7FD00009J, 2017 and O. Boucher, et al., Environ. Res. Lett., 2009, 4, 044007. [Drew Shindell, United States of America]	Accepted: The removal of methane has been discussed
23563	111	28	111	28	add "pulse emission" after "methane" - the number would be (very) different for CGTP metrics (might be useful to say what this would be - simply 2.75 times the number of years?) [Andy Reisinger, New Zealand]	Not applicable: This sentence has been removed.
51363	111	28	111	30	The sentence "The CO2 can already be included in carbon emission totals (Muñoz and Schmidt, 2016) so care needs to be taken when applying the fossil correction." is not sufficiently detailed to be helpful. Given only a single value for the methane metrics are given, this would imply no difference is being made between biogenic and fossil methane. Therefore, this section need to explain that different accounting practices can lead to different outcomes, what 'care' needs to be taken to avoid inconsistency, and what the implications (ie. double-counting) of doing doing so would be. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: Discussion of fossil correction has been expanded
116645	111	32	111	34	However chapter 5 provides new assessments of responses to inverse pulses (decreases in emissions). [Valerie Masson-Delmotte, France]	Taken into account: Reference has been made to chapter 5 pulses
114639	111	32	111	34	This part is a bit disconnected as placed now. Should be integrated to discussion further up. [Jan Fuglestvedt, Norway]	Accepted: This has been moved to section 7.6.1.1

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68151	111	37	112	14	This section (7.6.2.4) needs a lot of work as this is the main challenge with metrics and also the main purpose of many metrics (to cross compare impacts of different forcers). I've wondered if it belongs in a different/new section altogether, but I think it can work here after the physical quantities section (based on my proposed new order of radiative, cc + indirect, then physical indicators) because it is a crucial component of the construction of metrics that follows naturally after physical indicators. [Ilissa Ocko, United States of America]	Rejected: It is important to mention specific metrics earlier on, rather than reordering so they appear later.
68153	111	37	112	14	I suggest a different title for Section 7.6.2.4, such as: "Comparing forcing agents with different lifetimes." The title right now seems too specific, and implies comparing short-lived species to CO2 only when it aplues to all LL species (even though many metrics are ratios between non-CO2 agent and CO2). [Ilissa Ocko, United States of America]	Rejected: This is a specific example of how to compare short-lived with CO2.
68155	111	37	112	14	I strongly recommend starting this subsection (7.6.2.4) with an overview of the challenge of comparing climate impacts of species with different lifetimes. The first sentence right now is very confusing, as it jumps right into GTP and CO2 equivalence for long lived forcers. Also, CO2 equivalence is traditionally derived from GWP, not GTP – I'm not sure if the connection of GTP to CO2e is a mistake or intentional, but this can be confusing to a nonscientist metric user. [Ilissa Ocko, United States of America]	Taken into account: Discussion of different lifetimes has been added. GWP has been referenced
68159	111	37	112	14	I suggest more discussion in this section (7.6.2.4) of why it is so difficult to compare SLCFs and LLCFs (not just CO2), and then discussion about step vs pulses in general. I would reference the time horizon issue and refer readers to more details in subsection [insert]. [Ilissa Ocko, United States of America]	Taken into account: Discussion of different lifetimes has been added
89783	111	37	112	14	The beginning of this section goes straight into GTP for CO2 equivalence, it can be confusing to the audience as CO2e is commonly calculated with GWP. It is odd to not discuss the traditional use of GWP in CO2e which most of the user community is only familiar with. I recommend starting this section with an overview of the challenges of evaluting climate impacts of SLCFs and CO2 on the same scale, the definition of CO2 equivalence and it can be calculated with any metric, traditionally done with the 100-year GWP. Then point out how it is problematic because SLCFs are senstive to choice of time horizon, and 100-year GWP has embedded value judgement that discounts near-term impact. After that it can go into the other issues such as pulse/step emissions. [Tianyi Sun, United States of America]	Taken into account: GWP has been referenced.
51407	111	37	112	14	This section is currently only explores a comparison in terms of temperature. Could you also include a comparison for other key measures of climate change, such as ocean heat content or sea level rise. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: This section compares forcing and temperature. Other metrics are mentioned in 7.6.1.2
18289	111	37	112	26	This subsection is titled with "SLCFs" but discussion is only on short-lived "gases" - does this mean if short-lived "particles" (e.g., black carbon) may not be well compared with CO2? If so it would be worth mentioned as a limitation. How about ozone? It should also be noted that "short-lived gases" is the term for stratospheric water vapor and stratospheric/tropospheric ozone in section 2.2.5. [Yugo Kanaya, Japan]	Taken into account: Implications for aerosols has been mentioned.
103637	111	37	112	26	The text in this section needs to state more clearly that a number of metrics are available such as GWP and GTP. The current formulation (standard emission metrics such as GTP) implies that GTP is the standard metric (since it does not mention the others). Yet this is not the case. For example, the Collins et al (2019) paper cited proposes both CGTP and CGWP metrics - not just CGTP. Consider also including CGWP in Figure 7.25 and Table 7.15 [Philippe Tulkens, Belgium]	Taken into account: GWP has been referenced.
96757	111	37	112	26	The formulation "standard emission metrics such as GTP" does not reflect the fact that the Paris Agreement refers to the GWP and not the GTP. Please provide more balanced information without highlighting individual metrics. Please also check Figure 7.25 and Table 7.15 for the balance of information provided. [Nicole Wilke, Germany]	Taken into account: GWP has been referenced.
104525	111	37	112	28	This section does not adequately compare short-lived climate forcers (SLFCs) with CO2. More details about the rapid effect of SLCF on climate should be included. The discussion comparing AGTP and GTP for HFC-32 and CH4 is brief and does not include black carbon (BC) which also has a high radiative forcing. Text goes straight into GTP without adequate coverage of how SLCF affect the rate of change of climate which is critical for adaptation. Including metrics to quantifyf short-term effects is critical. [Denise Mauzerall, United States of America]	Taken into account: Implications for aerosols has been mentioned. Rate of change has been covered under time horizons

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112435	111	37	113	5	It is problematic that the SOD does not include any metrics with time horizons shorter than twenty years. This is inconsistent with the SOD language, which correctly states that "it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice. Physical science can only form a subset of the inputs to those choices." [page 116, lines 9-12]. Similar concepts are stated in Box 7.3. Dropping GWP20 and/or not including any time horizon shorter than 50 years takes that choice away from policymakers regarding timescale, and thus inherently makes the policy decision for them. Specifically, simply dropping near-term time horizon metrics such as GWP20 or GTP20 implicitly, but clearly, devalues mitigation measures (SLF abatement) that will have benefits on those timescales, despite the findings of (for example) IPCC SR1.5 that near-term SLF mitigation is essential to limiting temperature rise to 1.5 degrees. [David McCabe, United States of America]	Taken into account: GWP20 added
111357	111	37	113	5	Discussion of metrics - particularly for SLCF. There are a couple of discontinuities from AR5. (1) The 20 year GWP is not presented anywhere. I understand there should be caution about using it, but that could be stated. Since some people do use the 20-year GWP they will be looking for updates that might come from changes in forcing estimates (ch6). (2) Metrics for many common SLCFs except for methane are not discussed at all. Again I realize these are not without controversy, but the purpose of presenting them would be to update the inputs that are reviewed earlier in this report. [Tami Bond, United States of America]	Taken into account: GWP20 has been added. Implications for aerosols has been mentioned.
51405	111	39	111	39	What does "standard" mean here? Suggest GWP is included an example as it is the internationally-adopted metric that policy makers and practitioners will be familiar with. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: GWP has been referenced.
98451	111	39	111	50	The Chapter states that for climate forcers with lifetimes of over a century, the standard emission metrics such as GTP vary only slowly with time horizon, so an approximate CO2 equivalence can readily be determined. In contrast, emission metrics for SLCFs with lifetimes less than twenty years are very sensitive to the choice of time horizon. GTPs compare the response to a pulse emission of a species with a pulse emission for CO2. GTPs for 50-year and 100-year time horizons for methane are estimated as 14.6 to 6.7, respectively (Table 7.15 and Table 7.A.1). The 100-year time horizon GTP differs greatly, over 60% lower than GWP. This is because the GTP figure measures at the end-point and does not account for the strong forcing prior to this time. At 100 years the proportion of the pulse emission remaining in the atmosphere is relatively small. Overall, the estimation of GTP incorporates additional assumptions about physical processes, such as climate sensitivity and the exchange of heat between the atmosphere and the ocean. This consequently brings more uncertainty compared to GWP. The IPCC AR5 estimate an uncertainty of GTP100 of ±75% (with a 90% confidence), compared to ±30% and ±40% for GWP20 and GWP100, respectively. The selection of metric and time horizon for technology and policy evaluations is likely to change the rank order of preference. Hence, it is not advisable or conservative to use only a long-time horizon, e.g. 100 years. Decision makers need to use metrics in different categories of applications. Short-term emissions estimates of facilities or regions should be transparent and use a single metric and include the separated contribution from each GHG. Multi-year technology assessments should use both short- and long-term static metrics (e.g. GWP) to test robustness of results. Longer term energy assessments or decarbonization pathways must use both short and long-term metrics and where this has a large impact on results. Overall, dynamic metrics offer insight into the timing of emissions, but may be of only marginal benefit given uncertainties in methodological assumptions. [nehzat Motallebi, United States of America]	Taken into account: GWP20 added
66829	111	39	111	51	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account: GWP20 added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66831	111	39	111	51	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account: GWP20 added
68391	111	39	111	51	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account: GWP20 added
68393	111	39	111	51	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account: GWP20 added
83699	111	39	111	51	It could be helpful to bring the narrative on stock and flow pollutants from pg 114 line 29 - pg 115 line 11 up to here (it gets very technical here using terms such as step and pulse - but these are only explained in plain English/with a helpful example and linked to flow and stock pollutants on pg 114). [Dan Zwartz, New Zealand]	Taken into account: This section has been reworded to improve the clarity.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
77727	111	39	112	14	This information on approximation of CO2e is importnant for policy and may warrant inclusion in the Exec summary and SPM [Emer Griffin, Ireland]	Taken into account: This has been added to ES and SPM
68157	111	40	111	40	This is the first mention of CO2 equivalence in this section (7.6), and it isn't defined anywhere. Given that it is the most popular metric used, it deserves more attention than just being cited three times without any explanations. Please see Comment 7. [Ilissa Ocko, United States of America]	Taken into account: CO2 equivalence has been mentioned
96759	111	47			We do not understand the concept of "step change". Please explain in a more comprehensible manner. [Nicole Wilke, Germany]	Taken into account: Step change has been explained.
83147	111	48	111	48	There will a slow but over time rather substantial enhanced warming due to the thermal inertia of the deep ocean. A sentence to explain this should be added. [Terje Berntsen, Norway]	Taken into account: Warming of the ocean has been mentioned
23565	111	50	111	50	would be useful to be more specific than "a few" - I think "about four" would be appropriate since the concentration will be within 98% after 4 times the e-folding time. [Andy Reisinger, New Zealand]	Taken into account: This has been edited to be more specific about the timescale
99383	111	50	111	55	I found it confusing to see that the methodology for calculating the carbon cycle response is "high confidence." The magnitude of the carbon cycle response calculated by the two models are different by a factor of two as stated there. What leads to this high confidence in the methodology? [Katsumasa Tanaka, France]	Taken into account: This has been clarified that the methodology of using a carbon cycle model is appropriate.
68161	111	53	112	14	The step metric discussion is incredibly technical as written and will be inaccessible to the desired audience (metric users). [Ilissa Ocko, United States of America]	Taken into account: this has been revised.
32957	111		116		This section leaves out metrics with timescales shorter than 50 years (tables & text). I recommend that such metrics should be included (e.g. GWP20, GTP10/20) as metrics are used not only for analysis of consistency with long-term temperature targets, which is the usage the SOD implicitly seems to be referring to in its discussion of metrics for SLCFs and long-lived gases, but also for life-cycle analyses, for carbon-equivalent footprints of nations/companies/etc., for analysis of the rate of change in the near-term (which is also part of agreements under the UNFCCC), and by policy-makers who have developed near-term climate mitigation plans such as Norway's and the US State of California's. The authors do not provide a rationale for removing the short-term metrics, only indirectly hint at this when discussing the benefits of comparing a step-change in short-lived forcing with a pulse change of long-lived gases. There would be enormous implications, policy and financial, of switching to a metric such as CGTP that would have the potential to enormously increase the value of SLCF removals in the short-term but eliminate their value in the long term, thereby radically changing financial incentives. These could be discussed in the WGIII report, but WGI should not simply eliminate the prior short-term metrics without consideration of the implications including the impact on policy makers already using 20-yr metrics. [Drew Shindell, United States of America]	Taken into account: GWP20 added
103639	112	2	112	7	If CGWP and CGTP are measured as the ratio of kg/yr vs kg pulse of CO2, would it be valid for policymakers wishing to regulate pollutants per tonne to divide by the number of years? E.g. divide the CGWP100 figure for CH4 by 100, in order to divide the step change into increments. [Philippe Tulkens, Belgium]	Rejected: This section focusses on the physics. Policy considerations are covered in section 7.6.3. Other reviews suggest removing all policy statements from this section.
83703	112	2	112	11	It would be useful to have more explanation for what the GWP* equation/method is here [Dan Zwartz, New Zealand]	Accepted: It has been clarified that Cain et al. 2019 definition is used.
99387	112	2	112	14	Like any metrics, there are pros and cons for GWP* and C-GTP. Smaller variations in GWP* and C-GTP values may be a good feature of these metrics, but it is difficult to grasp the sense of comparing a pulse emission of CO2 with a sustained emission of CH4, especially in the policy context. At the least, the policy application of these metrics has been untested. I think this paragraph is a place where limitations like this for GWP* and C-GTP can also be stated, not just those for time-varying metrics. [Katsumasa Tanaka, France]	Rejected: This section focusses on the physics. Policy considerations are covered in section 7.6.3. Other reviews suggest removing all policy statements from this section.
83149	112	2	112	14	The basic idea behind emissions metrics is (at least in my mind) to provide policymakers with simple tools to compare different scenarios or mitigation options. The wide use of GWP_100 shows this. The combined CGTP and the GWP* are based on comparing long-lasting changes in the rate of emissions of SLCFs to pulse emissions of CO2. In my mind this makes them much less usefull for policy making, since policy makers must committ future generations to follow up on the sustaind changes in the emissions of the SLCFs.A caveat on the usefullness for policymaking should be added. [Terje Berntsen, Norway]	Rejected: This section focusses on the physics. Policy considerations are covered in section 7.6.3. Other reviews suggest removing all policy statements from this section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69893	112	3	112	14	Policymakers should have access to multiple metrics, including metrics that allow for a two-basket approach and recognize the near-term impacts of SLCP (such as GWP20 and GTP20).. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Gabrielle Dreyfus, United States of America]	Taken into account: GWP20 added
66833	112	3	112	14	Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account: GWP20 added
66835	112	3	112	14	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account: GWP20 added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68395	112	3	112	14	Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). Aggressive mitigation of SLCPs can cut the rate of warming in half, Arctic warming by two-thirds, and avoid up to 0.6C of warming by 2050. UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335(6065):183–189; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323. [Durwood Zaelke, United States of America]	Taken into account: GWP20 added
68397	112	3	112	14	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account: GWP20 added
68399	112	3	112	14	For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. [Durwood Zaelke, United States of America]	Taken into account: GWP20 added
23567	112	6	112	8	I think this sentence will be far too cryptic for most stakeholders. Given the prominence given to GWP* by some stakeholders, I think it is useful to add an explanatory sentence that says somethnig like "This means that using GWP*, a permanent change in the rate of emission of a short-lived gas such as CH4 by 1 kg is calculated as equivalent to a one-off emission of GWP100 × 100 × 1kg CO2, or 3.2 tons of CO2-warming equivalent when using the GWP100 value derived in this assessment." It may also be useful to point out that even though the broader discussion in this para is about combined GTP metrics, GWP* is using GWP (not GTP) to approximate CO2-warming equivalent temperature outcomes (this might be confusing otherwise). [Andy Reisinger, New Zealand]	Taken into account: This description has been revised.

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103	112	6	112	8	Allen et al 2018 consider the effect 20 years after a pulse of SLCF, but also 20 years prior. So the time horizon is not H, but they set t = 20. Please assess a) whether there is a scientific basis for t =20; b) whether any sensitivity analysis to varyiing this parameter has been undertaken; and c) the equity and distributional implications of limiting consideration of historical emissions. [Harald Winkler, South Africa]	Rejected: Equity and distributional implications are not considered in this chapter.
106353	112	6	112	8	This sentence is inaccurate. If the GWP* approximation indeed consists in "simply scaling" GWP by the time horizon, GWP* values for methane would be constant at GWP-100*100, which is not the case. "Simple scaling" is thus not an accurate description of the method and the sentence needs a more precise rewording. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable: This sentence has been removed.
73917	112	6	112	8	GWP* was not mentioned in AR5, but only in the IPCC 1.5degree report. Here it is introduced without a proper explanation of what GWP* is and it is not very clear for the reader how the equation looks like without reading through all the references. The IPCC 1.5 degree report included a much better explanation of the concept of GWP* and how it is constructed. It would be more transparent to add better explanations as in the 1.5 degree report. [Anke Herold, Germany]	Taken into account: The Cain reference has been added, but not the formula
114667	112	6	112	8	I suggest you add a bit more explanation here [Jan Fuglestvedt, Norway]	Taken into account: This description has been revised.
98453	112	7	112	14	The Chapter introduces a new metric GWP* which compares pulse emissions of long-lived climate forcers like CO2 and N2O against changes in emissions of SLCFs, such as CH4, stating that metrics like CGTP and GWP* provide a more accurate way than either GWP or GTP of assessing the temperature implications of a time-series of emissions. Studies indicate that the GWP* metric could predict the total warming effect from constant methane emissions which is greater than that from fossil carbon dioxide emissions, provided those emissions reach zero by 2050, and about the same if fossil carbon dioxide emissions reach zero by 2100. However, the results also show the modelled actual temperature change is slightly greater than that indicated by the GWP* metric because the metric ignores the warming due to climate-carbon cycle feedbacks from methane emissions. In addition, it appears almost impossible to reduce fossil carbon dioxide emissions to zero by 2050 as some unavoidable emissions of fossil carbon dioxide will need to be compensated for by carbon removals e.g. through forestry, to achieve net zero carbon dioxide emissions. [nehzat Motallebi, United States of America]	Noted: No suggestions made
106355	112	8	112	10	This sentence has some editorial issues. I suggest not introducing a term that is not readiily understood to then clarify it immediately thereafter if it is not used further throughout the text. Instead, I would simplify the statement to read something like: "The combined-GTP can be calculated for any species, but it is least dependent on the chosen time horizon for species with lifetimes equal or less than half the time horizon of the metric." [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This description has been revised.
44325	112	10	112	11	Jenkins et al (2018) 'Framing climate goals in terms of cumulative CO2-forcing-equivalent emissions' should be referenced here as well. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been referenced
106357	112	10	112	11	Other metrics have been described in great detail inthe chapter, in contrast to this CO2 forcing equivalent metric. An explanation of how this metric is calculated is essential for readers to understand this and the following sentence. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: CO2-feq has been described.
112595	112	11	112	11	Please insert: the equivalence between CO2 emissions and CO2-warming-equivalent emissions of methane can be further improved by including a small contribution that scales with cumulative methane emissions with a dominant contribution that scales with the methane emission rate (Cain et al, 2019). The GWP* metric allows the rate of CO2-warming-equivalent emissions in year t, E*(t), to be calculated from GWP100 CO2-equivalent emissions in year t, E(t), and in the year 20 years prior to t, E(t-20): E*(t) = 4 x E(t) - 3.75 x E(t-20). Please provide this formula, because it is frequently stated in AR6 that these "new" metrics are more complicated than the old ones, so it's important to make clear to people that this additional complexity is nothing more intimidating than a subtraction. [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The Cain reference has been added, but not the formula
112597	112	11	112	11	Please include a reference to Jenkins et al (2018), which I believe was the first paper to actually use the phrase "forcing-equivalent emissions" [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This has been added.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
99385	112	11	112	13	I looked into the Wigley forcing metric FEI closely before (Tanaka et al., 2013, Climatic Change, doi:10.1007/s10584-013-0693-8). FEI is not at all comparable with GWP* and fundamentally different from GWP* in its construction. The metric that ensures equivalent temperature changes is TEMP (Tanaka et al., 2009, Climatic Change, doi:10.1007/s10584-009-9566-6). [Katsumasa Tanaka, France]	Rejected: We do not say here that FEI is similar to GWP*
23569	112	11	112	14	More care is needed in wording here, to make clear that CGTP metrics use the CHANGE in emission rate of he SLCF as input, not necessarily the absolute emission rate; and likewise, the measure of comparison is the CHANGE in global averrage temperature (relative to the temperature resulting from whatever reference level is used to calculated the CHANGE in SLCF emissions). These points are not clear in the current formulation. Suggested re-phrase: "Such metrics effectively compare a sustained change in the rate of emissions of short-lived greenhouse gases, relative to a given reference emission rate, with a one-off pulse-emission of long-lived greenhouse gases with regard to their effect on changing globally averaged surface temperature, relative to a reference temperature." [Andy Reisinger, New Zealand]	Taken into account: This has been reworded.
83701	112	11	112	14	This sentence isn't matched by a corresponding discussion in 7.6.3. It would be helpful to explain in this chapter why and in what situations it will be challenging to use these metrics for policy . It may also be useful to move the single-basket narrative to box 7.3 - to keep the science and policy implications separate/so the reader knows to go to one section (rather than several bits) of the chapter to access the policy implications assessment. [Dan Zwartz, New Zealand]	Taken into account: This sentence has been moved.
96761	112	11			The statement "such metrics provide a way to effectively compare emissions of short and long-lived greenhouse gases at global average surface temperature" is scientifically correct and politically relevant for the near future and possibly for the medium term, where the effect of SLCFs is significant to avoid exceeding ambitious temperature targets. However, it is not valid in the long term, as up to 40% of CO2 emissions remain in the atmosphere for up to 1000 years. Do the authors assume that SLCF fluxes remain stable over hundreds of years? Their "better choice" (L114 38-41) could better represent the temperature from a purely scientific perspective, where one can play with stable global SLCF emissions and study the effects on required LLCF emissions and thus reduction targets in a hypothetical model world. In the real world, however, some policy choices have consequences hidden in this seemingly scientific advice: 1) the allowed level of SSCF vs. LLCF emission reduction; 2) the choice of a global reference level for constant SLCF emissions; 3) the distribution to corresponding national reference levels. This could lead to wrong incentives, political uncertainty and thus create instability of the emission reduction regime. [Nicole Wilke, Germany]	Taken into account: The "better choice" discussion has been replaced by a more nuanced discussion of metric types.
106359	112	12	112	12	Please state explicitly how effectiveness of the metric is being thought of in the following sentence: "Such metrics provide a way of effectively comparing emissions of short- and long-lived greenhouse gases on globally averaged surface temperature." Currently, readers have no way to judge how this was interpreted. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: This sentence has been rephrased
16003	112	13	112	14	It would make sense to integrate the radiative forcing function up to some point in time, ideally defined as a critical point of irreversibility, such as a blue ocean event in the Arctic, or a predicted collapse of the GIS, or a significant release of subsea methane. (see also section 7.6.3.1) [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Rejected: This section does not deal with specific events such as these
71725	112	19			To avoid confusion with other values given for the CH4 lifetime, the figure caption here should refer to “perturbation time” or “perturbation lifetime” [Martin Manning, New Zealand]	Accepted: This change has been made.
77731	112	24	112	32	This is very important for policy and particularly for consideration of carbon budgets and should be reflected in the SPM [Emer Griffin, Ireland]	Noted : The importance for policy has been noted.
68173	112	26	112	26	I cannot figure out what this figure means… [Ilissa Ocko, United States of America]	Taken into account: This figure has been improved

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68163	112	29	112	29	I recommend a new section before Section 7.6.2.5 (Emission metrics by species - which goes straight into values) that introduces selected metrics: e.g. “Select climate metrics." As of right now, different metrics are introduced sporadically (some without definitions such as CO2e!), and thus cannot be cross compared easily. This section (7.6) could really use a table that outlines and describes the various metrics discussed here, such as GWP/CO2e, GTP, AGTP, CGTP, etc. The table would also indicate if the metric is integrative or instantaneous at an endpoint, what the indicator is (such as forcing or temp), when it was developed (such as 1990 or 2018), and whether or not it is a comparison to CO2 via a ratio (such as GTP) or an absolute value (such as AGTP). The text from 7-108-49 to 7-108-51 would fit in the first paragraph of this section: that the most common metric is GWP, but other metrics exist, etc. Also recommend a nod to there having been dozens of metrics introduced since the first IPCC report, as well as an explanation of why only select metrics are discussed here (popularity? Scientific integrity?). Also important to address why certain metrics are left out of this section (7.6) entirely, but others emphasized repeatedly. More discussion of GWP is also warranted and appropriate here. My assessment of the discussion of metrics in Chapter 7 is that the authors are pushing GTP and step changes, and that GWP is only included for consistency with other reports. While these metrics may be more technically appropriate for several applications, unless there is a major campaign to train users to employ them, the majority of nontechnical and nonscientist users will most likely continue to use GWP, as they have for decades despite developments of new metrics; especially given that there is no perfect metric. Acknowledgement of the history and use of GWP, and its counterpart CO2e, deserves more discussion considering it is by far the most widely used metric across the climate community. For example, I often work with the user community (governments/industries/advocacy orgs/education orgs/consulting firms etc.) and I have never seen a metric other than GWP used in reports. I understand the scientific desire to change that, but without a dedicated effort to educate these users, GWP will be used, and therefore it merits attention in this section. Please see Comment 7 for more examples why it is so hard to replace GWP. [Ilissa Ocko, United States of America]	Taken into account: WE have taken your detailed comments on board and completely rewritten the sections to be much clearer on the implications and the associated caveats
68165	112	29	112	29	I also think that after the previous new section ("Select climate metrics"), there could be another new section specifically about the choice of a time horizon for all of the metrics: e.g. “Importance of time horizon.” Again, these would be before Section 7.6.2.5 (Emissions metrics by species). The justification is that aside from sporadic references to time horizons, the bulk of information about time horizons is found in the Application of emission metrics subsection. While it is appropriate to discuss time horizon choice in the application section, because the Emission metrics by species subsection (7.6.2.5) chooses select time horizons to provide values for, I strongly suggest moving up the bulk of the timescale discussion to a new subsection following "Select climate metrics" and before 7.6.2.5. This would provide explanations for why particular time horizons are selected here. Also, some discussion of the role of timescale in the metric value, and why timescale is even needed at all, would be useful here. Then I would discuss popular time horizons and what they represent (20-year near-term, 50-year mid-term, 100-year long-term, 500-year stabilization). Emphasis of the arbitrary nature of a time horizon is also important, as is discussion of the shortcomings of selecting one time horizon – prioritizing one timeframe over another. This is also where the challenges associated with the near-exclusive use of GWP-100 by the user community can be brought up. For example, because everyone uses GWP100, they drop the 100 and then a climate impact becomes disassociated with a time horizon. This is something I see on countless occasions in reports, presentations, and more, and over and over people do not realize that a time horizon is even part of the metric! Further appropriate in this subsection is the proposal to always report GWPs for two time horizons simultaneously – 20 and 100 years. This vastly improves the use of GWP by covering impacts in the near- *and* long-term, for communities who will not adopt new metrics such as CGTP. This will have the added benefit of ensuring that people don’t drop the time horizon when reporting climate impacts (Ocko et al. Unmask temporal trade-offs in climate policy debates, Science, 356, 6337, p.492-493 (2017)). See more about a two-valued GWP in Comment 8. [Ilissa Ocko, United States of America]	Taken into account: WE have taken your detailed comments on board and completely rewritten the sections to be much clearer on the implications and the associated caveats. The timescale discussion has been moved up. GWP20s are now given as requested

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89785	112	29	113	4	This section is incomplete and inconsistent with previous IPCC reports. GWP20 is omitted from the table without an explanation. This is the table the users will reference to update their numbers with the new knowledge of radiative properties, and GWP20 is the second most used metric in the user community. Without including updated GWP20 the users will likely stick to AR5 values that have outdated radiative properties. Additionally, the section seems biased toward long-term impacts that are not consistent with policy goals that are often set for the next 20-30 years. As someone who works with industries and organizations that are trying to address their emissions, this long-term bias would hurt their efforts to address SLCFs emissions and discourage them to address emissions NOW. Although GWP20 is not a perfect metric, it is technically accessible to most users. Therefore, I strongly recommend including GWP20 in Table 7.15 and section 7.6.2.5. [Tianyi Sun, United States of America]	Taken into account: GWP-20 now included as asked for
68111	112	29	113	5	The time horizons provided (50y, 100y, 500y) are also inconsistent with the Paris Agreement timeline and other policy goals set for the next 20-30 years, making the values provided in this chapter disconnected from the policy context in which they are going to be used. This is a major deficiency and will either make the chapter irrelevant or more likely counterproductive to the implementation of actions that are consistent with the policy goals agreed to by the global community through the Paris Agreement. The 50 and 100 year time horizons will misrepresent climate impacts for these goals, and therefore the whole policy process is not well served by the approach this chapter takes. 50 and 100 year time horizons are useful and important, but one needs to be able to establish and use metrics that separate out the near-term impacts. I understand that GWP100 is approximately equal to GTP30 for short-lived climate forcers, and 30 years is consistent with midcentury targets; however GTP, as an endpoint metric, does not consider the path, and therefore the strong warming from SLCFs like methane will be masked for the majority of time that these forcers are in the atmosphere - 30 years after emission the majority of methane has been oxidized. [Ilissa Ocko, United States of America]	Taken into account: GWP20 added
68113	112	29	113	5	As someone who is deeply involved with working with governments and industries to reduce emissions of methane, I am shocked and saddened by the omission of GWP20 in AR6. I cannot underscore enough how much this will hurt efforts to reduce emissions of methane - which are essential to curbing climate change over all timescales. There is no explanation of why it isn't included, other than a reference that it had been included in past reports. My colleagues at other climate policy organizations agree that it is a big mistake to not include GWP20, including Climate and Clean Air Coalition, Clean Air Task Force, Institute for Governance and Sustainable Development, Rocky Mountain Instutite, Climate CoLab, and The Nature Conservancy. It is not whatsoever that we endorse GWP and GWP20 in particular as perfect scientific metrics, however, the reality is that GWP is what the user community uses almost exclusively, and so given that context, it is much better to include GWP20 as an additional option rather than omit it entirely. More regarding this in Comment 7. [Ilissa Ocko, United States of America]	Taken into account: GWP20 added
68115	112	29	113	5	Another issue with the omission of GWP20 is that GWP20 users (including McKinsey, California Air Resources Board, and International Energy Agency) may end up using IPCC AR5 for GWP20 (which will have outdated radiative properties) and GWP100 from IPCC AR6. These values will now be inconsistent with one another as the underlying physics is inconsistent. Or, users may just ignore IPCC AR6 values in order to use GWP20 and GWP100 values that are consistent with one another, which is a shame because of the advancement of science since the last report. [Ilissa Ocko, United States of America]	Taken into account: GWP20 added
112437	112	29	113	5	AR6 should update climate metrics included in previous ARs, especially those that have been adopted by the policy community. As noted in the above comment, the SOD is clear that the choice of metrics cannot be made simply using physical & earth science, but is a "matter for policy-makers." Indeed, policy-makers in California, Norway, and perhaps other jurisdictions use GWP(20), as have numerous life-cycle analyses designed to inform policy choices. If the GWP(20) is not updated in AR6, this will confuse and disrupt these policy processes. [David McCabe, United States of America]	Taken into account: GWP20 added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64755	112	29	113	5	Omission of GWP20 is incredibly worrisome as someone who not only interacts regularly with the user community, but who works on near-term climate issues and policies. I cannot underscore this enough. GWP20 has been in every IPCC report, and has become the second most popular time horizon used by the user community; I have never come across anyone who uses GWP500. Further, there is no explanation of why it isn't included, other than a reference that it had been included in past reports. GWP20 users (including McKinsey, California Air Resources Board, and International Energy Agency) may end up using IPCC AR5 for GWP20 (which will have outdated radiative properties) and GWP100 from IPCC AR6. These values will now be inconsistent as the underlying physics is inconsistent. Or, users may just ignore IPCC AR6 values in order to use GWP20 and GWP100 values that are consistent with one another, which is a shame because of the advancement of science since the last report. I can foresee many problems arising from this strategy. For example, based on the metrics and time horizons provided in Table 7.15, if one were to evaluate current greenhouse gas emissions in Russia, it would appear that the country's carbon dioxide emissions play a larger role in contributing to climate change than methane. However, using a 20-year time horizon for GWP would reveal that current methane emissions in Russia will have a climate impact 5 times larger than that of CO2 over the following two decades. Without near-term metrics, the powerful near-term warming mitigation opportunity to reduce methane emissions from oil and gas operations in Russia could be overlooked or downplayed. Another example is that using the current metrics in Table 7.15 makes it possible for countries like Brazil to technically achieve their Paris Agreement CO2e goal, but increase warming in the near-term via emitting more methane, because it is undervalued using the existing metrics. This is a loophole that arises from using long-term metrics in isolation, and clearly shows why both near- and long-term time horizons are needed in unison (see Comment 5; Ocko et al., Unmask temporal tradeoffs in climate policy debates, Science, 2017). [Steven Hamburg, United States of America]	Taken into account: GWP20 added
100461	112	31	112	32	There are several issues with Table 7.A.3 (mistakenly referred to here as Table 7.A.2): 1) Lifetimes and radiative efficiencies are supposed to be from WMO (2018), but many of the values seems to be outdated (e.g., the CFC-11 lifetime should be 52 and not 45 years), and also different from those given in Table 7.15. 2) Why are there two columns for each of AGWP100 and GWP100? 3) The GWP and GTP values are very strange (e.g., a GWP100 value of 4954268 is given for CFC-11 in one column and 1855 in the other, while it should be around 5500). [Øivind Hodnebrog, Norway]	Accepted: The table has been revised.
69895	112	31	112	47	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. As noted on 7-116 L9-12: "it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice. Physical science can only form a subset of the inputs to those choices." [Gabrielle Dreyfus, United States of America]	Taken into account: GWP20 added
66837	112	31	112	47	GWP500 is included, but GWP20 is not; GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account: GWP20 added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66839	112	31	112	47	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account: GWP20 added
68401	112	31	112	47	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. This is important because many feedbacks and tipping points are anticipated within the next 10 to 20 years, as the 1.5C guardrail is approached and likely breached. Masson-Delmotte V., et al. (eds.) (2018) SUMMARY FOR POLICYMAKERS, in IPCC (2018) GLOBAL WARMING OF 1.5 ºC; Lenton T. M., et al. (2019) Climate tipping points—too risky to bet against, NATURE, Comment, 575:592–595; Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259, 8254; and Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5784. GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-	Taken into account: GWP20 added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68403	112	31	112	47	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account: GWP20 added
14943	112	31	113	1	I very strongly urge that GWP20 values be added, as they were in AR5. Several papers have recommended the use of both GWP20 and GWP100 for methane in policy debates: Howarth et al. 2011 Climatic Change Letters 106: 679–690; Ocko et al. 2017 Science 356: 492-493, and Fesenfeld et al. 2018 Nature Climate Change 8: 933-936. And the State of New York in 2019 adopted the GWP20 by law. It would be extremely unfortunate for the AR6 report to seem to impoly there is something wrong with using GWP20. [Robert Howarth, United States of America]	Taken into account: GWP20 added
112447	112	32	112	32	Please be clear if CH4 metric now includes shortwave absorption bands. Also - is this propagated to WG3? If shortwave absorption is included, and rapid adjustments are considered, how certain are the contributions of these rapid adjustments to the reduction in the GWP metric? [Cynthia Randles, United States of America]	Taken into account: This has been made clearer.
74005	112	35	112	36	Please delete the sentence "GWP100 values are included for consistency with previous reports, but his does not imply a recommendation of their use." Or add all other GWPs and GTPs included in this section to this sentence. It is biased to add such statement only for GWP100 and not to any of the other metrics presented. Given the fact that GWP100 is the generally used metric under the Paris Agreement and by all countries in their implementation, it would be absurd if it would be no longer presented in this chapter. There are clearly other reasons for presenting GWP100 and not only the consistency with previous reports. The fact that GWP is directly related to radiative forcing and radiative forcing is closely linked with all climate impacts is certainly a good reason to use GWP and it is not totally arbitrary that this choice has been made. [Anke Herold, Germany]	Taken into account: This has been rephrased.
106361	112	35	112	36	As the IPCC does not make policy recommendations, this statement is a good clarification, but at the same time unnecessarily singles out an individual metric. It would be more balanced and accurate to state that "Inclusion or discussion of any metric in this section does not imply a recommendation of their use." Alternatively, this stattement can also be deleted here as Box 7.3 already speaks to this issue explicitly. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: The statements on policy relevance have been moved to box 7.3
65741	112	35	112	36	Suggest the IPCC maintain the same structure of GWP details, and include GWP(20) values as in previous assessments to maintain continuity and policy relevance. For example, the text notes that "GWP(100) values are included for consistency with previous reports", however, GWP (20) values have not been included despite also being available in previous reports. [Kushla Munro, Australia]	Taken into account: GWP20 added
83151	112	35	112	37	All previous IPCC WGI reports have given also the GWP(20) numbers. There might be reasons for omitting this now, but that should be given as part of the assessment. Since this is the SOD and it will not go out for another review, I recommend to include also GWP(20) for consistency. [Terje Berntsen, Norway]	Taken into account: GWP20 added

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
114641	112	37	112	37	I wonder if it is meaningful to provide GWPs for 500 years. What do these values tell us - given that GWP is based on an integral of RF and not the response of the climate system? What is GWP500 for a gas with lifetimes in the order of one or two decades telling poliycmakers? (It gives a value determined by the denominator (CO2) and the integral of RF-CH4 in the very beginning of the 500 yrs period, while the dT response has dissapeared after 500 yrs). See discussion in AR5 WGI Ch8. [Jan Fuglestvedt, Norway]	Rejected. These tables with GWP500 are provided for completeness and backwards comparability with past reports.
52003	112	37	112	39	Unless I missed earlier reference to it, CGTP comes out of the blue here, and it's not clear why it suddenly appers, or what it is. Why is is added in to the table? GWPs and GTPs are included in past IPCC reports, so there is precedence for those. Given other availble metrics like GWP*, why is CGTP chosen? In the table, the CGTP is shown with different units to GWP and GTP. THis is going to be confusing for an end-user without more explanation of why this metric isn't dimenionless like the usual ones are. [Michelle Cain, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account: Metrics are now more carefully introduced. GWP* is not a single number, but depends on the prior emission history and can therefore not be included in the table.
77729	112	38	40	14	Could examples of SLCFs be included e.g. does this include black carbon? [Emer Griffin, Ireland]	Rejected: There is no information on these metrics for black carbon since AR5.
83153	112	39	112	40	I thought the linear relation between CO2 emissions and ERF, even with the reduced radiative efficiebcy was due to an increase ine the airborne fraction (or longer adjustment time if you like) for CO2 at higher concentrations. Cf. Caldeira K, Kasting JF. 1993. Insensitivity of global warming potentials to carbon-dioxide emission scenarios. Nature 366:251–53 [Terje Berntsen, Norway]	Taken into account: This has been explained better - i.e. this refers to the change in radiative efficiency between 2011 and 2018
77733	112	43	112	47	Given that SLCFs also impact on health and ecosystems as considered under the UNECE Convention on Long Range Transport of Air Pollution (CLRTAP) has there been any consideration of inclusions of these impacts? [Emer Griffin, Ireland]	Rejected: Health and ecosystem impacts are not considered in this chapter.
66175	112	43			N2O metric is adjusted because of radiation (2%? Etminan) but not adjusted because of N2O-CH4 chemistry coupling that reduces CH4 (Prather & Hsu 2010, Coupling of nitrous oxide and methane by global atmospheric chemistry, Science, 330: 952-954). This latter is a -4.5% effect and has not been incorporated in IPCC to date (even AR5). It should be noted and included in N2O metrics here. [Michael PRATHER, United States of America]	Accepted: This has been included in N2O metrics
23571	112	44	112	47	I'm not clear about this sentence: I presume you need to add at the end "... than in the AR5 when climate-carbon cycle feedbacks are included." The values are higher, not lower, than in the AR5 if people compare the AR5 non-ccfb values with the now recommended AR6 values that include ccfb. Be clear what is being compared with what. [Andy Reisinger, New Zealand]	Accepted: This change has been made.
64775	112	50	113	5	Please include GWP20 in Table 7.15 to provide metric users with a near-term metric option. GWP20 has been in every IPCC report, and has become the second most popular time horizon used by the user community. There is no explanation of why it is not included in AR6 WGI, and the metrics/timescales currently included in Table 7.15 misrepresent climate impacts in the near-term and devalue the role of SLCFs in addressing near-term warming. This will hurt efforts to curb emissions of SLCFs such as methane, which are essential in addressing climate change, and of which studies have shown the climate and other benefits of their early mitigation. [Steven Hamburg, United States of America]	Taken into account. We are now assessing GWP20.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68117	112	50	113	5	There are many examples of how the long-term metrics and time horizons provided in Table 7.15 and Table 7.A.3 are misleading regarding near-term climate impacts and how this could hurt climate change mitigation efforts. For example, when comparing the relative importance of emissions by sector, using the current GWP and GTP metrics and values provided in Table 7.15 makes it seem that present-day carbon dioxide emissions from the power sector have a climate impact 3 to 13 times higher than present-day methane emissions from agriculture and fossil fuel production and distribution, each (emissions data from JRC EDGAR 2010). This completely masks the fact that today’s methane emissions from agriculture and fossil fuel production and distribution each will have similar climate impacts as CO2 from the power sector over the following two decades – making the combined climate impact of methane from both of these sectors twice as high in the near-term as CO2 from power generation. The same is true when comparing the relative roles of different pollutant emissions within a country: using the current GWP and GTP metrics and values provided in Table 7.15 in AR6 makes it seem that present-day carbon dioxide emissions in Brazil have a climate impact 3 to 16 times higher than its present-day methane emissions (emissions data from JRC GECO 2019). However, in Brazil, today’s methane emissions will have nearly the same climate impact as today’s CO2 emissions over the following two decades. And using the current GWP and GTP metrics and values provided in Table 7.15 in AR6 makes it seem that present-day carbon dioxide emissions in India have a climate impact 3 to 13 times higher than its present-day methane emissions (emissions data from JRC GECO 2019). However, in India, today’s methane emissions will have a slightly larger climate impact than today’s CO2 emissions over the following two decades. On the other hand, if GWP and GTP with 20 year time horizons are also included, a decision maker would have the necessary information to understand the varying roles of sectoral and country-level greenhouse gas emissions over time, and the temporal tradeoffs in climate impacts brought about by various decisions. [Ilissa Ocko, United States of America]	Noted. At the same time, steeply mitigating SLCFs at the expense of LLCFs either at GWP100 or GWP20 values leaves behind a warmer world for future generations. The tendency of traditional emissions metrics to either under- or over-value the contributions of gases is why we think it is important for WGI to be very clear that there serious issues arise if traditional emissions metrics are used to make these trade-offs - it is important that users and people from outside the physical sciences can get a clear sense of the issues that arise and that that there is no universally accepted methodology for combining all the relevant factors into a single metric.
68119	112	50	113	5	I strongly recommend including GWP20 in Table 7.15 and Table 7.A.3 for at least four reasons: (1) to provide an option for decision makers who need to assess near-term impacts of emissions, (2) to be consistent with policy-relevant timescales of 10-20 years as well as midcentury targets, (3) to encourage implementation of mitigation actions of short-lived climate forcers by conveying their major role in limiting near-term warming, and (4) to be consistent with all past IPCC assessment reports. [Ilissa Ocko, United States of America]	Taken into account. We are now assessing GWP20.
104527	112	50	113	5	Table 7.15 should include metrics for 20 years. It is important to include GWP20 to quantify the high impacts of SLCF with high radiative forcing. Inclusion of black carbon (BC) is also important so it is clear that mitigating BC, which will bring health benefits through reduced air pollution, will also bring immediate climate forcing benefits. [Denise Mauzerall, United States of America]	Taken into account. We are now assessing GWP20.
100463	112	50	113	5	It would be useful to know how much of the GWP / GTP values that come from the carbon cycle response (e.g. in parenthesis). This can also be considered for Table 7.A.3. [Øivind Hodnebrog, Norway]	Taken into account. We have added the point that it is becoming more routine to consider the carbon cycle in emissions metrics.
100465	112	50	113	5	The GWP(100) value of 670 for CF4 is probably wrong given that the GWP(500) value is 9600. [Øivind Hodnebrog, Norway]	Taken into account. Values are corrected
99389	112	52	112	52	I suggest that AR6 WG1 should include values of GWP20 and GTP20 in the metric table because there are users who need updated values of these in AR6. It is useful to keep the 20-year time horizon to ensure continuity from AR5. I do not see any scientific reason to drop the 20-year time horizon. Furthermore, the choice of metrics and their time horizons in this table need to be consulted with WG3. WG3 FOD contains a table for GWP100, GTP20, GTP40, GTP100, MGTP50, and MGTP100 (but only for CH4 and N2O). [Katsumasa Tanaka, France]	Taken into account. We are now assessing GWP20.
83705	112	52	113	3	The table 7.15 gives Nitrous Oxide a lifetime of 109 years and GWP100 of 271. However, the 7A3 table on page 162 gives nitrous oxide a lifetime of 121 years and GWP100 of 260. If these are both correct, please explain why there is a difference between these figures, and ensure consistency of these important figures throughout all chapters. [Dan Zwartz, New Zealand]	Taken into account. Numbers revised and now consistent

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66841	112	52	113	4	GWP500 is included, but GWP20 is not; GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. We are now assessing GWP20.
66843	112	52	113	4	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. We are now assessing GWP20. The comment on 20 year timescales is noted, but it should also be remembered that steeply mitigating SLCFs at the expense of LLCFs either at GWP100 or GWP20 values leaves behind a warmer world for future generations.
71727	112	52	113	4	(Table 7.15) The lifetime given for methane is quite different to that used in other chapters looking at the methane budget because the value used here is a perturbation lifetime. For clarity the column heading should be changed or a footnote added to the lifetime value given for methane. [Martin Manning, New Zealand]	Taken into account. Lifetime checked for consistency with other chapters.
71729	112	52	113	4	(Table 7.15) There are several reasons why the 20-year time horizon that has been used consistently in previous IPCC reports should be retained. Two obvious ones are to have consistency between the AR5 and AR6, and to flag the importance of prompt reduction in CH4 emissions in order to allow more time for reducing CO2. Dropping the 20-year GWPs will probably be seen by some as an indication that the IPCC no longer sees SSP119 and SSP126 as feasible. [Martin Manning, New Zealand]	Taken into account. We are now assessing GWP20.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68405	112	52	113	4	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. This is important because many feedbacks and tipping points are anticipated within the next 10 to 20 years, as the 1.5C guardrail is approached and likely breached. Masson-Delmotte V., et al. (eds.) (2018) SUMMARY FOR POLICYMAKERS, in IPCC (2018) GLOBAL WARMING OF 1.5 ºC; Lenton T. M., et al. (2019) Climate tipping points—too risky to bet against, NATURE, Comment, 575:592–595; Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259, 8254; and Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5784. GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-	Taken into account. We are now assessing GWP20. The comment on 20 year timescales is noted, but it should also be remembered that steeply mitigating SLCFs at the expense of LLCFs either at GWP100 or GWP20 values leaves behind a warmer world for future generations.
68407	112	52	113	4	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account. We are now assessing GWP20. The comment on 20 year timescales is noted, but it should also be remembered that steeply mitigating SLCFs at the expense of LLCFs either at GWP100 or GWP20 values leaves behind a warmer world for future generations.
79939	112	52	113	4	Table 7.15. Metrics with shorter timescales than 50 years need to be included. [Valentin Foltescu, India]	Taken into account. We are now assessing GWP20.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69897	112	52	113	5	Table 7.15: GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. As noted on 7-116 L9-12: "it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice. Physical science can only form a subset of the inputs to those choices." Policymakers should have access to multiple metrics, including metrics that allow for a two-basket approach and recognize the near-term impacts of SLCP (such as GWP20 and GTP20).. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Gabrielle Dreyfus, United States of America]	Taken into account. We are now assessing GWP20.
9865	112	52	113	5	Despite its flaws, 20-year GWP is used in some policy context, and if the IPCC is dropping its assessment of this metric, it should be clealry stated and justified [Robert Kopp, United States of America]	Taken into account. We are now assessing GWP20.
103641	112	52	113	5	Why does the text mention only CGTP (not CGWP)? Please explain also the choice to give CGTP values only for gases with a lifetime of under 20 years (relates to Figure 7.15). [Philippe Tulkens, Belgium]	Taken into account. We have tried to make the chapter consistent in its treatment of metrics. There are many to cover and not much space.
96763	112	52	113	5	Again, the focus of this text is on GTP, not GWP, which seems an unbalanced choice. Please explain or revise. Why are CGTP values only given for gases with a lifetime of less than 20 years?. [Nicole Wilke, Germany]	Taken into account. GWP values are given in the table.
116647	112		113		In Table 7.15, showing clearly changes since AR5 (and reasons for changes) would be helpful. [Valerie Masson-Delmotte, France]	Taken into account: Discussions of the changes since AR5 have been added to the text, but not the tables.
44925	113	1	113	5	In Table 7.15, metrics should be included for time horizons less than 50 years. For instance, IPCC AR5 CH8 Table 8.7 includes GWP20 and GTP20. The dominance of using 100-year time horizons as the primary basis for evaluating climate impacts, and the failure to include here any metrics for time horizons less than 50 years, is disadvantageous in two major ways: it obscures potential trade-offs in short- vs. long-term effects when making policy decisions, and it undervalues the positive near-term effects that can be achieved via SLCF mitigation – and the associated political benefits of motivating action based on near-term self-interest. The IPCC should provide a selection of metrics for reference that also includes one(s) with 20-year (and possibly also 10-year) time horizons. The IPCC serves as a reference for a scientific and policy community that goes far beyond what is required for reporting under the Paris Agreement, and should reflect this. Relevant peer-reviewed journal publications related to this topic include Ocko et al., Science, 2017; Shindell et al., Science, 2017; Victor et al, Nature Climate Change, 2015. [Kathleen Mar, Germany]	Taken into account. We are now assessing GWP20.
65743	113	1	113	5	Suggest Table 7.15 also provide GWP(20) values for consistency with previous reports, as per the text statement on page 112 line 35. [Kushla Munro, Australia]	Taken into account. We are now assessing GWP20.
65745	113	1	113	5	Suggest including values for CO2 and N2O (CGTP 50 and 100) in Table 7.15, similar to how CH4 and other gases have values included? These values appear to be available for CO2 and N2O in Table 7A3 on page 162. [Kushla Munro, Australia]	Taken into account. Numbers have been checked and edited for consistency.
65747	113	1	113	5	Suggest including a short discussion on the CGTP values, to put them in context and explain why they are so high compared to GWP and GTP values, as well as to explain what the values represent in terms of policy relevance. [Kushla Munro, Australia]	Taken into account. See Section 7.6.2.1, where this is covered

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65749	113	1	113	5	Suggest reviewing Tables 7.17 and Appendix Table 7.A.2 for consistency. The text on page 112 line 31 is that emission metrics for selected species are presented in Table 7.15, with further species presented in the Appendix Table 7.A.2. There appears to be some value rounding and also some differences in values. For instance, Lifetime(years) for CH4 of 12.4 years is the same in both tables, which is consistent, however for N20 it is 109 years (Table 7.15) and 121 years (Table 7.A.2). Likewise, for N2O the GWP and GTP values are not consistent across the two tables. Please also note that CH4 (CGTP 50) appears to have been rounded up to 3100 from 3048, where as it should be rounded down to 3000. [Kushla Munro, Australia]	Taken into account. Numbers have been checked and edited for consistency.
17857	113	3	113	4	Given that 20, 100, and 500 year GWPs were presented in most of the previous IPCC assessments (with the exception that 500 year GWPs were dropped from AR5, a decision I'm glad to see reversed), it would be great to have those 3 presented somewhere. In particular, a number of groups use the 20 year GWP frequently. [Marcus Sarofim, United States of America]	Taken into account. We are now assessing GWP20.
103643	113	3	113	4	Why not include CGWP in table 7.15? Also please explain the rationale for only reporting combined metric values for species with a lifetime under 20 years. [Philippe Tulkens, Belgium]	Rejected. Space constraints limit the amount of information we can present.
83697	113	3	113	4	The table at the beginning of pg 113 needs an explanation for why the CGTP figures are so different/missing. [Dan Zwartz, New Zealand]	Taken into account. We have revised for consistency.
17835	113	3	113	25	Table 7.15: the 100 year GWP of CF4 appears to be a typo (maybe it should be 6700?). The 100 year GWP of N2O is inconsistent with table 7.A.3 which lists 271. [Marcus Sarofim, United States of America]	Taken into account: The tables have been revised.
32099	113	3			Two comments on this table. 1. Lifetime for methane is defined in various ways, but normally the default is the 9-ish year removal lifetime. So switching here to the adjustment value of 12.1 years needs to be explained. Maybe cite Prather, Michael J. "Lifetimes and eigenstates in atmospheric chemistry." Geophysical Research Letters 21.9 (1994): 801-804. Incidentally Ch 6 page 9 gives lifetime as 9-12 years suggesting this is an error margin, not a difference of definition. Second comment is that GWP (20) has been omitted. I'm African and I see my home country farmers in a terrible state from the current drought. We support very smart kids in rural high schools. For these folk it is GWP(20) that matters psychologically - maybe not at the elderly communal farmer level, but quite widely people are surprisingly well informed from their children's school internet classes. We'll be running a climate workshop for univ. students in Matabeleland next year. They're smart and they'll be asking us: what can we do? How can we value our little effort? They'll say to us "Climate Change is here and now! Please, can we do something." Twenty years is a scale that really matters if you are on the front end of global change. So please restore GWP(20). [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thank you for this comment. We attempt to provide a clear and traceable rationale for our treatment of the lifetime of CH4. We are now assessing GWP20. However, it should also be noted that if trade-offs between SLCFs and LLCFs are made at GWP20 values then it will leave behind a much warmer world for future generations.
77735	113	8	114	49	Very useful and largely accessible material. [Emer Griffin, Ireland]	Thank you.
69899	113	8	114	50	"Box 7.3: Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. As noted on 7-116 L9-12: “it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice. Physical science can only form a subset of the inputs to those choices.” Policymakers should have access to multiple metrics, including metrics that allow for a two-basket approach and recognize the near-term impacts of SLCP (such as GWP20 and GTP20).. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII’s FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: “Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic].”)." [Gabrielle Dreyfus, United States of America]	Taken into account. We have now included GWP20 in our assessment.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
64773	113	10	114	48	Box 7.3 needs work. The purpose of this box seems to be to clarify how to select a metric with the audience of metric users (rather than traditional scientists), yet the content is quite disorganized and hard to follow. [Steven Hamburg, United States of America]	Taken into account. Box heavily revised
66845	113	10	114	48	In the FOD for Chapter 7, Box 7.3 stated that it would be expanded for the SOD, and that expansion was slated to include GWP20. [Kristin Campbell, United States of America]	Taken into account. We have now included GWP20 in our assessment.
66847	113	10	114	48	GWP500 is included, but GWP20 is not; GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. We have now included GWP20 in our assessment. The point about the near-term is noted. We also note, however, that if trade-offs between SLCFs and LLCFs are made at GWP20 values then it will leave behind a much warmer world for future generations. (Pierrehumbert, 2014.)
66849	113	10	114	48	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We have worked with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.
44331	113	10	114	48	Is there not a place here for adding a formula showing the temperature response due to a non-CO2 forcing change Delta_F and a cumulative CO2 budget Delta_G is: Delta_T = TCRE * (Delta_G + Delta_F/AGWP_{CO2,100}). Reference for this is Jenkins et al (submitted 2020). [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We felt this was not needed here as other equations used in supplement

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68409	113	10	114	48	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. This is important because many feedbacks and tipping points are anticipated within the next 10 to 20 years, as the 1.5C guardrail is approached and likely breached. Masson-Delmotte V., et al. (eds.) (2018) SUMMARY FOR POLICYMAKERS, in IPCC (2018) GLOBAL WARMING OF 1.5 ºC; Lenton T. M., et al. (2019) Climate tipping points—too risky to bet against, NATURE, Comment, 575:592–595; Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259, 8254; and Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5784. GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-	Taken into account. We have now included GWP20 in our assessment. The point about the near-term is noted. We also note, however, that if trade-offs between SLCFs and LLCFs are made at GWP20 values then it will leave behind a much warmer world for future generations. (Pierrehumbert, 2014.) Furthermore, the main driver behind the current rate of temperature increase is CO_2. There is some scientific controversy over the status of tipping points, but if they exist, then avoiding them will require halting warming; which means net zero CO_2 emissions. SLCFs are second-order considerations in this regard.
68411	113	10	114	48	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO_2 forcers in comparison with CO_2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO_2 and non-CO_2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO_2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO_2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account GWP-20 added to tables and Figure and discussed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68171	113	10	114	48	The text in Box 7.3 is quite disorganized and hard to follow, detracting from its potential value. I suggest reorganizing with the following sequence: 1. It is best to keep forcing agents or groups of forcing agents (such as short- and long-lived) separate if possible, because no single metric can capture the relative role of different emissions across all potential climate change variables of interest and over all timescales. 2. However, if one needs to compare the effects of different gases simply, emission metrics can be employed. 3. Consideration of what is appropriate to use depends on scientific aspects and value related choices. Therefore, the IPCC does not recommend a particular metric. 4. Scientific aspects to consider include: what are the forcers that are being considered (e.g. short and long-lived, stock and flow), are the emissions increasing or declining (although I have heard many compelling scientific arguments that argue against the claim that GWP gives the wrong "sign" for declining emissions and how GWP* resolves these issues. My take is that the GWP* argument of wrong GWP sign is an incorrect interpretation and use of GWP. Even if methane emissions from human activities are declining, you still are warming the climate relative to no human influence.) Then go into best choices for each decision here. 5. Value related choices include climate indicator of interest and time horizon of interest (e.g. near or long-term or both). Then go into best choices for each decision here. [Ilissa Ocko, United States of America]	Taken into account. Thank you for this comment. We have used it as an aide memoire for the narrative in our chapter. We now make use of these points.
104529	113	10	114	48	Long and wandering discussion. Should be tightened up and a discussion of SLCP with the implications of short vs long time-horizon metrics should be included. Inclusion of short time horizons is critical to maintain. This is important for policy makers to appreciate the large benefits of rapidly decreasing SLCF to slow the rate of climate change. Also, it is not correct to say that the climate effects of methane decline to zero as emissions decline to zero as the methane is oxidized to CO2 which continues to contribute to RF. [Denise Mauzerall, United States of America]	Taken into account. The Box has been heavily revised to take this into account.
106375	113	10	114	48	The role of this box compared to section 7.5.3 is unclear, as it repeats some of the messages, and its style is at times very a-typical of an IPCC assessment. It can be shortened to avoid the conceptual overlaps with section 7.5.3, or deleted altogether. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The Box has been heavily revised to take this into account.
73919	113	10	114	48	The introduction of box 7.3 is very useful. However an additional similar box would be required that discusses the use of metrics and the purposes related to the 6th assessment report as such. The key scenarios in Chapter 4 are based on radiative forcing (e.g. pages 4-13 to 4-14), thus on the first step in the cause -effect chain presented in this chapter and all gases are integrated in these scenarios which underly the other considerations in the WG1 report and reports of other WGs. There are no scenarios that start from temperature or SLR. The choice of GWP based on radiative forcing seems to be the only metric consistent with the approach chosen in chapter 4 for the scenarios. The 6th assessment report itself includes choices of methods, timescales and purposes that would change if different metrics would be chosen. It would be important to outline which metric concepts are in line with the value choices made within the AR6, e.g. related to the scenarios and budgets discussed, related to the chapter on SLCFs but also related to the policy scenarios discussed in WG3. It does not seem to be possible to derive messages on mitigation strategies in WG3 without using a metric to aggregate different gases and it seems that IPCC itself uses GWP100 for this purpose. If the concept of GWP100 is no longer supported in this chapter, a more appropriate metric choice would need to be made for the purposes of the AR6. At least a box describing the consequences of changes in metrics on the messages of the entire AR6 report would be necessary. This is on particular the case for the key message of specifying short and long-lived GHG separately in emission scenarios. Is this implemented across all WGs and chapters in AR6 in a consistent way? If not, it would create inconsistencies with key messages of AR6 , if policy makers would start implementing this recommendation at policy level, when it was not implemented in AR6. Thus, it is required to add a box describing the impacts of the key recommendation on p. 116, line 14ff on AR6 itself. [Anke Herold, Germany]	Noted. IPCC does not endorse metrics, though there has been perhaps been "inadvertent consensus" around the use of GWP100 (see Shine, 2009). Our aim is to contextualise recent physical science research in emissions metrics, and show how recent developments lead to a better match between time-series of emissions and resulting temperature change. WGIII will have their own perspectives. We are working alongside them. We anticipate some areas of agreement, and some areas of enduring disagreement.
111873	113	11	113	11	I'm not sure if the "I" works so well here. Who are the perceived groups of users? Probably better to name them explicitly, or simply say "Which metric should be used?" [Oliver Geden, Germany]	Taken into account. Box title has been revised

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93635	113	11	114	47	The reluctance from giving recommentations in the first sentence of the textbox seem unfounded, as newly developed metrics seem a big step forward compared to traditional metrics. Most fundamentally, to reach the objective of the Paris Agreement, it is clear that emissions of long lived gases must be kept within an absolute limit (the "carbon budget"), while for SLCF, they must simply be limited. Traditional metrics fail to capture this fundamental difference as they give the impression that SLCF "consumes" the emission budget equivalent to CO2. For assessing progress towards the long term goal of the Paris Agreement (i.e. the global stocktake), traditional metrics clearly fall short when trying to establish an equivalence between emissions that decay, and those that accumulate. There simply is no single methane/SLCF emission that equals a CO2 emission when the goal is to stabilize temperature. Rather, a pulse emission of CO2 is comparable to a time-series of SLCF, which is reflected in newly developed metrics i.e. GWP*. The advantages of this is clearly demontrated as results from GWP*-calculations are similar to those in advanced climate models. Further, they overcome the bias from the choice of time horizon inherent in GWP(100), and also enables us to take black carbon and other SLCF into the account (which have hiherto been omitted exactly due to this lack of equivalence with CO2). The only disadvantage with new metrics is that traditional metrics are so firmly established in reporting and common use, so that a change of thought is required. However, it seems like a fundamental mistake of the WG1 assessment to avoid a clear message on the choice of metrics. Probably, much remains to be done in establishing new metrics and investigating their use for reporting and policy development. Such research needs should be an important part of the message. [Jon Magnar Haugen, Norway]	Noted. Thank you for the comment. IPCC cannot make policy recommendations. You summary of the situation is scientifically accurate. Thanks for the positive comment.
103645	113	11	114	48	It would be useful for the box to include discussion of the relative merits of integrated vs endpoint based pulse emission metrics. As per Collins et al. 2019, there are two differences between GWP & GTP. i) the choice of forcing vs temperature; and ii) the fact that GWP is typically integrated while GTP is endpoint based. Therefore, when the box discusses the relative meritrs of GWP & GTP, it is not clear what is the contribuion of i) & ii) to these strengths & weaknesses. [Philippe Tulkens, Belgium]	Taken into account. Thank you for this. We have attempted to bring out some of these aspects in 7.6.2.1 where we introduce the C-metrics.
111355	113	11	114	50	"Which metric should I use?" This box is a special call-out, presumably because the authors recognize that many non-experts will have this question. Unfortunately, the box doesn't help those non-experts, but instead introduces more details in a way that is likely impenetrable to decision-makers. I suggest that the technical information in the box could be moved out to the text, and replaced with a simpler presentation, possibly a table. For example, "If you use X, it represents Y, and its disadvantages are A, B and C." I recognize that metrics are complex issues, but I hope authors can recognize that there's an existing level of confusion in the decision-making community, and assist in clarifying that. None of the needed principles are absent from the text, yet they are never distilled into an accessible presentation. [Tami Bond, United States of America]	Taken into account. The box has been heavily revised. Given the sensitivities of some commenters to even minor revisions regarding emissions, it is probably impractical to develop a simple presentation such as a table or flowchart for emissions metric choice. Developing such flowcharts and tables could be a valuable addition to the literature, but IPCC is probably not the place to introduce it.
23583	113	11	115	20	I have an overall concern about consistency and alignment across IPCC AR6 reports, which arises from a simple technicality. As the WGI report is published before the WGIII report, WGIII can and does cite WGI, but WGI doesn't cite WGIII. This seems odd especially for Box 7.3 and Section 7.6.3, which discuss applications of metrics - which clearly is something where WGIII has insights to offer (specifically through Box 2.2 in the WGIII report). To the uninformed reader, this sections gives the impression that this discussion in WGI is the only place where the AR6 is discussing metrics as the AR6 WGIII report is not mentioned at all. To addres this misleading asymmetry, we could either break IPCC citation rules (as an exception to facilitate cross-WG coordination across the AR6), or these sections could make a more generic reference to a discussion of metrics from a mitigation and policy implementation perspective in AR6 WGIII (without giving an actual citation). Perhaps this is something that the respective WG TSUs need to give guidance on. [Andy Reisinger, New Zealand]	Taken into account. We have worked alongside WGIII on the issue. Generally, there are many areas of agreement, but some areas of enduring disagreement.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
89423	113	11			The box reflects upon different metrics and their potential applications and advises on their specific applicability. There are some quite fundamental issues with their characterization that need to be addressed: Firstly, the full comparability of emissions is key. For pulse-based metrics like GWP100 this is indeed the case. One emissions unit independent of time and location is evaluated equally. This is not the case for other metrics such as GWP* if applied to any other but the global level (see e.g. Rogelj & Schleussner 2019), as those metrics are dependent on historical emissions of SLCPs. A policy regime (or market) cannot operate without a common base. Secondly, the authors convey the view that pulse emissions might only be advisable to compare 'single year' emissions and not for emissions pathways, for which they advise other metrics such as GWP*. However, the purpose of analyzing emission pathways in the context of climate policy is not to solely to track progress against an instantaneous temperature response. But rather to assess progress towards net-zero GHGs as per Article 4 of the Paris Agreement. Such an assessment requires metrics that provide a long-term perspective of the radiative forcing and warming response. A focus on the representation of the instantaneous warming response does not provide good guidance in that regard, as changes in SLCPs would have a much stronger imprint and divert from long-term relevant changes in CO2. Lastly, the box completely misses to mention that the UNFCCC is using GWP100 as the cardinal metric for comparison including in the PA Rulebook. [Carl-Friedrich Schleussner, Germany]	Rejected. This is incorrect. Like cumulative CO2 (and other LLCF) GWP* gives the warming from when the emissions time-series are first provided. Neither give warming from before that time. Second, the level of warming associated with constant emissions of SLCF is fairly easily calculated. There is no scientific reason why we cannot ascertain the warming associated with flows of gases. Third, there are multiple possible interpretations of the Paris Agreement in terms of emissions (fuglestvedt et al. 2018). Finally, what the UN currently does or does not do is not a reason to favour or disfavour an approach. If IPCC WGI has relevant points to make regarding environmental integrity that bear on metric choice, then we should make them.
99391	113	13	113	14	This is in principle so, but seeing the two drastic changes in the kind of metrics in the table (Tables 7.15 and 7.A.3) compared to those in AR5, I would be tempted to interpret that the current draft intends to make a recommendation for metric choices. First, compared to AR5, the current draft does not show any values for GWP20 and GTP20. I think these metrics, especially their updated values in AR6, are in need for many users. I do not see any justification nor discussion for why the time horizon of 20 years has been abruptly dropped. Second, the current draft replaced the 20-year time horizon metrics with the combined-GTP. While the combined-GTP is interesting in itself and expected to be useful for some theoretical applications (Allen et al., 2018, npj Climate and Atmospheric Science, doi:10.1038/s41612-018-0026-8), the combined-GTP is based just on one paper recently published. I don't think that the science is matured enough to include this new metric in the table like this, which will serve as a source for many real-world applications in policies and assessments. There are some papers that express concern over this type of metrics from the practical point of view (e.g. Schleussner et al. 2019, ERL, doi:10.1088/1748-9326/ab56e7). The usefulness of the combined-GTP for policies and assessments has not been proven yet, and I think this is still a subject of future research. [Katsumasa Tanaka, France]	Taken into account. We have now included an assessment of GWP20. The issues you raise will no doubt be traversed in WGIII. In WGI, we are aiming for a clear physical presentation of the new metrics and how they represent a scientific advance (progress compared to AR5) over pulse metrics. We have also added material on multi-metric approaches.
5051	113	20			The replacement of the existing GWP100 weighting metrics easily causes confusion among the users. The use of GWP weights is simple and the people are accustomed to it. Thousands of individuals around the world use them for various tasks, such as emission reporting and life cycle analysis. - The renewal of the metrics must bring clear benefits e.g. in the accuracy of the results in order to be seen as profitable, especially if the new metrics is more complicated to use than the old one. [Ilkka Savolainen, Finland]	Taken into account. In WGI, we are aiming for a clear physical presentation of the new metrics and how they represent a scientific advance (progress compared to AR5) over pulse metrics. We point to the relevant material in WGIII on the use of metrics.
106377	113	22	113	27	This paragraph implicitly and inaccurately assumes that all metrics aim to map to global warming. The statement is thus too vague to be useful. Metrics exist for sea-level rise, forcing or precipitation (both highlighted earlier in the chapter) and it would be an error to compare these directly to warming. Instead of focussing on the metric failing to capture a certain behaviour, it is rather the metric's use where issues can occur. This can be dealt with through careful rewording. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. We think the text in the section is clear regarding how different metrics map to different variables. In this Box we are pointing out that a new strand of research has led to a large improvement in terms of the ability to map to temperature. Many people are unaware of the long-standing scientific critiques of GWP100, and the development of new and improved metrics for temperature gives us the chance to make this point more clearly than has been done before in IPCC Assessments.
114657	113	22	113	27	I think this para needs some further work and explanations. The GWP100 and CO2eq approach does take the lifetimes into account, but there are other ways of doing this in the formulation and application of metrics [Jan Fuglestvedt, Norway]	Taken into account. We have rephrased this point

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112441	113	23	113	24	Statements that the GWP of methane and other SLCFs "have the wrong sign when emissions are declining" are a gross over-generalization of observations from several papers studying the GWP* formulation, which is designed to be a useful means of estimating SLCF 'budgets' under global schemes designed to limit warming to a given level. At best, these sections will be extremely misleading for the vast majority of policymakers and other members of the AR6 audience. The plain language here is stating that in a context of declining emissions of an SLCF, that SLCF should have a negative climate metric - that is, while emissions of CO2 cause warming, emissions of the SLCF (in this context) would be COOLING. This is false. Under any climate scenario, the temperature of the earth will be warmer in the years after an SLCF is emitted, relative to the counterfactual where the SLCF is not emitted. Therefore, the SLCF certainly would not have a negative metric. The statement needs to be removed. [David McCabe, United States of America]	Taken into account. We have rephrased the text. Scientifically, it is clear that declining methane emissions reduce warming (and consistent with the OED definition which defines the word "cool" as a negative temperature trend, this can legitimately be called a cooling). Nevertheless, the plain language we used was probably too plain for some tastes, so we have refined it.
23573	113	23	113	25	More care is needed in wording here around when and why metrics 'get the sign wrong'. Suggest re-phrasing (insertions in capitals): "However, some emission metrics can fail to give the correct sign of contributions to warming FROM SUSTAINED EMISSIONS OF SLCFS under scenarios in which those emissions decline SUBSTANTIALLY, due to limitations in their ability to represent the combined effects of CUMULATIVE EMISSIONS OF pollutants with different lifetimes over extended time periods." Pulse emission metrics like GWP and GTP get the sign wrong ONLY if they are applied to sustained emissions within a scenario of rapidly/substantially declining emissions. They don't get the sign wrong for individual emissions, as every individual emission causes positive warming relative to this emission not occurring. This is a crucial difference in meaning and the difference is highly relevant for policy applications: do we care about the contribution to temperature change of an emission, relative to the emission not occurring, or do we can about the contribution of an emission relative to the warming that has occurred already at a fixed reference time. [Andy Reisinger, New Zealand]	Taken into account. We have revised the phrasing of this point.
17837	113	23	113	25	Negative metrics for declining emissions of short-lived forcers only make sense when considering metrics within the framing of the GWP* (e.g., Cain et al. 2019). This concept has applicability issues: e.g., many (most?) users just care about how much the emissions of their facility/nation/natural-gas-vehicle today are contributing to future warming of the planet. The GWP* is great in cases where metrics are being used to approximate a simple climate model, or as a way to calculate stabilization pathways, but using this framing to claim that other metrics are failing "to give the correct sign of contributions" is misleading. For 99 perecent of metrics users, it makes sense that today's emissions of methane contribute to future warming, regardless of what yesterday's emissions were, even if a declining emissions trend means that the net total contribution of methane to warming is declining. [Marcus Sarofim, United States of America]	Taken into account. We have rephrased the text. Scientifically, it is clear that declining methane emissions reduce warming. This pointhas been made more literally. WGIII also assesses emissions metrics, and to a greater extent can reflect different perspectives.
129067	113	23			While the GWP* is a novel metric with interesting applications, it will likely lead to confusion for many users (as well as being arguably incorrect) if the IPCC uses the GWP* framing in order to state that the traditional GWP metric provides the “wrong sign” for short-lived gases when emissions are declining (see also page 115, line 52). Outside of the GWP* framing, all GHG emissions contribute to future warming compared to a counterfactual of no emissions, and therefore a positive sign is appropriate. Also, the IPCC should recognize the UNFCCC has continued to commit to emissions reporting using the traditional GWP100. [Trigg Talley, United States of America]	Rejected. The WGI aim is to assess the most significant emissions metrics literature from a physical science perspective. GWP* accurately simulates the warming from a time-series of gases, while GWP100 does not. WGIII will assess metrics from other perspectives, including habit (UNFCCC) and convenience (customary use).
23575	113	29	113	35	Whether something is a stock or flow pollutant depends entirely on the time frame that is being considered. From a geological perspective (thousands of years), N2O is a flow pollutant. From the perspective of decision-making in 4-year election cycles, methane is a stock pollutant. it would be useful to insert somewhere in this text a clarification that calling methane, HFC-22 and other SLCFs a flow pollutant is only true if one truly adopts a centennial to millennial perspective on decision-making. [Andy Reisinger, New Zealand]	Noted. This is beyond the scope of WGI.
27177	113		113		Raw 2 (CO2) / Column 3 : the value is slightly different of the value given page 109 line 28 (1.35 10-5) [Eric Brun, France]	Taken into account. Numbers have been checked and edited for consistency.
116649	113		114		I think that inputs from chapters 5 and 6 are needed for this box, which should be a cross chapter box. If I understand correctly there can be a cumulative effect of SLCF through consequences for ocean heat and sea level (check the description of "climate effects") (See Zickfeld et al, PNAS, 2017). [Valerie Masson-Delmotte, France]	Taken into account. Taken into account. Box reworded
44327	114	1	114	2	Jenkins et al (2018) is a reference here too. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
2739	114	1	114	47	much of this is very repetitive, especially lines 13-22. What does one take away from the final paragraph? [Bryan Weare, United States of America]	Taken into account. We have revised the text and aim to reduce repetition.
699	114	4	114	7	Since methane is converted into CO2 within a few decades, and once it becomes CO2 it is then a long lived greenhouse gas: why does the text state that as methane emissions tend to zero so does its forcing? Couldn't understand this. [Bruce Wielicki, United States of America]	Taken into account. We have revised the text to make our point clearer - if the CH4 is from rice paddies, for instance, then there is no additional C added to the atmosphere in the oxidation of CH4 to CO2.
23577	114	4	114	11	This para needs a bit more work to be clearer about where metrics such as GWP and GTP are falling short compared to CGTP metrics. GWP and GTP fully capture the fact that zero emissions result in zero warming. What they don't capture is that past emissions of CO2 entail an ongoing commitment to future warming, whereas past CH4 emissions don't (because they are defined as the warming relative to a fixed background concentration, i.e. they capture only the warming from a given emission relative to that emission not occurring - not relative to the combined effect of both past and future emissions on warming). I suggest the text is revised/clarified to make clear that the difference between CO2 and SLCFs is that cumulative CO2 emissions result in cumulative warming over many centuries, whereas cumulative SLCF emissions do not result in cumulative warming (over many centuries - they do over one to two centuries for CH4). Again though, care is needed when saying this: using GWP100 to understand the contribution of future emissions to warming (relative to the absence of those emissions) would in fact UNDERESTIMATE the cumulative warming from constant CH4 emissions over the first century and would only OVERESTIMATE warming beyond the first century. A lot of people seem to think that using GWP100 is wrong (in the sense of 'overestimates the actual contribution to warming' as soon as any time period of emissions is concerned. Clearly stating this would be important to avoid and correct existing misinterpretations. [Andy Reisinger, New Zealand]	Taken into account. Comment noted. We have revised the text for clarity.
31793	114	6	114	6	"climate effects" is a bit vague. What you say is clearly correct for temperature, but as noted earlier, is not clearly so for SLR. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This point has been unpacked
106379	114	7	114	9	The statement is too generalising to be useful and doesn't appreciate that these limitations are not in the metric but in their use and application. This can be dealt with through careful rewording. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the text to make the point more clearly.
114659	114	8	114	8	Re "fail to capture this difference": Yes, when CH4 is transferred to CO2-eq and then seen to behave like CO2. SO I suggest adding a bit more explanation here [Jan Fuglestvedt, Norway]	Taken into account. We have attempted to make this point more explicitly.
73923	114	8	114	11	The chapter fails to discuss why GTP(100) is an appropriate choice of time horizon given the strong dependence of GTP values on the time horizon. It would be important to add which GTP time horizon would be an appropriate choice for the purposes under the Paris Agreement. In servak Workshops under the UNFCCC IPCC authors answered this questions with 40 to 60 years. It would be important to add such discussion in this chapter, because this is key for policy makers to know which time horizons would correcpond to the objectives they established in the Paris Agreement. Please add a short discussion on the choice of time horizons for GTP as this seems very important for non-integrated metrics. [Anke Herold, Germany]	Rejected. WGIII is also assessing metrics, and will refer to the aspects you describe. The WGI aim is to assess the most significant emissions metrics literature from a physical science perspective.
114661	114	11	114	11	AGTP(t) can do this. See Shine et al., 2007 [Jan Fuglestvedt, Norway]	Taken into account. Thank you for this comment. We have rephrased to avoid error.
23579	114	13	114	15	Include "including the time horizon for decision-making" - as this is a crucial value judgement that should be brought out clearly. This would match well with the clarification I'm suggesting for lines 4-11 in the preceding para. [Andy Reisinger, New Zealand]	Taken into account. We have worked this in to help clarify the issue for readers.
99393	114	13	114	22	I am not fully sure if my comment fits into this paragraph, but let me share a paper that my colleagues and I recently wrote, currently under review (https://nature-research-under-consideration.nature.com/channels/1337-under-consideration/posts/cost-effective-implementation-of-the-paris-agreement-using-flexible-greenhouse-gas-metrics). The paper shows how cost-effective metrics, so-called price ratios, vary under a range of pathways including those with large overshoot. Then we approximated the time-dependent cost-effective metrics with more common metrics like GWP100 and GTP100 along each pathway and analyzed the cost-effectiveness of updating metrics at some points in future time (e.g. from GWP100 to GWP50). We show, in a sense, a combined use of GWP100, GWP50, and GWP20 in time in a cost-effective way. Please consider the paper above if something like these can be added to/addressed in this paragraph or maybe better elsewhere. [Katsumasa Tanaka, France]	Taken into account. Thank you - we have included this material in our discussion. But not in this paragraph, which is being revised, but alongside material on the performance of metrics over time. In general we are trying to point to WGIII for the non-physical aspects of emissions metrics, but we have added a short summary section.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83707	114	13	116	28	This section discusses the suitability of particular GHG metrics for certain policy purposes, but the selection of metrics/purposes is unclear. Is it illustrative, or a comprehensive assessment of the literature? If intended to be comprehensive, it would help to include further explanation of what is meant by CBA and cost-effectiveness in this context, and also comment on the full range of possible policy purposes for using GHG metrics (i.e. around UN reporting and accounting for NDCs, setting domestic mitigation targets, the global stocktake, footprinting, and LCA). In addition, this section could reference LCA GHG metrics analysis from the UNEP report Global Guidance for Life Cycle Impact Assessment Indicators Volume 1 page c.70. [Dan Zwartz, New Zealand]	Taken into account. In response to commenters we have decided to reduce this section. We have bundled several of the points on use together into a short paragraph pointing to the WGIII report, where perspectives other than those grounded in physical science can be explored.
17839	114	15	114	18	I think both Sarofim and Giordano (2018) and Mallapragada and Mignone (2020) are good citations to support the alignment of the GWP and the global damage potential. [Marcus Sarofim, United States of America]	Not applicable as sentence removed after pushback from other commenters. Thank you for the reference, though.
103647	114	15	114	20	The idea that GWP is more useful for 'cost-benefit' but GTP is better for 'cost effectiveness' is not explained and not obvious. Please either elaborate on the statement or remove it. [Philippe Tulkens, Belgium]	Taken into account. It draws on material in AR5, and on more recent work. We have amended the reference.
96765	114	15	114	20	The idea that GWP is more useful for a 'cost-benefit' framework and GTP is better for 'cost effectiveness' is not explained and not obvious. Please either elaborate on the statement or remove it. [Nicole Wilke, Germany]	Taken into account. It draws on material in AR5, and on more recent work. We have amended the reference.
111875	114	16	114	19	I don't think that policymakers are usually concerned about cost-benefit or cost-effectiveness frameworks (we might wish they were, and surely some of them do) but this formula seems to capture more how researchers imagine policymakers (and how policymakers sometimes present themselves). There's not much (literature-based) evidence how policymakers actually "use" metrics. I guess these sentences work well even without mentioning policymakers [Oliver Geden, Germany]	Taken into account. Thank you for this comment. We were trying to err on the side of generosity. We have amended the reference.
23581	114	18	114	20	The sentence needs to be modified to make clear that GTP works as proxy for the global cost potential only if it is used as a dynamic GTP (i.e. with a time horizon aligned to the year when temperatures are expected/intended to peak). By contrast, GTP100 has very little to do with a global cost potential and using it would not support cost-effective mitigation choices. This is important and needs to be made very explicit here (including recognising that using a dynamic GTP would imply a commitment to using a metric whose values would need to be regularly updated, and for SLCFs would result in inexorably increasing metric values over time). [Andy Reisinger, New Zealand]	Taken into account. We have rephrased this for clarity.
112599	114	19	114	20	should this not read "the effect of emissions on a single target year, then GTP might be a more appropriate choice (this is just true -- not clear what relevance alignment with GCP has here). Metrics of "CO2-warming-equivalent" emissions such as GWP* (Allen et al, 2016; Cain et al, 2019) or CGWP (Collins et al, 2019) provide a more accurate indication of impact of future warming over a range of timescales, which is useful for policies aiming to limit future warming without specifying a target year." [Myles Allen, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have rephrased this for clarity.
23585	114	24	114	25	replace "portfolio" with "trajectory involving multiple gases". Portfolio (at least in the way it is understood mostly on the policy side) refers to a mix of emissions or policies at a given point in time, whereas the preceding paras make it clear that the main issue arises if pulse emission metrics are applied to a sustained time-series (i.e. a "trajectory") of emissions of gases with widely differing lifetimes. The word 'trajectory' is already used in the following sentence, consistent with this view. [Andy Reisinger, New Zealand]	Taken into account. We have rephrased this for clarity.
44329	114	33	114	41	There needs to be a better discussion of CO2-fe here. CO2-fe is the basis for the design of any physically credible emissions metric. Work of Allen et al 2018, Cain et al 2019, Lynch et al 2020 are all based on the response characteristics of CO2-fe metric. CO2-fe is not simply another metric, but the most physically representative way of comparing GHGs in complex multi-gas emissions scenarios. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Space has limited the discussion of this point
52001	114	34	114	41	It would be worth discussing CO2-forcing-equivalent (eg Jenkins et al 2018) as a way where you can compare different GHGs. [Michelle Cain, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Thank you. This is now done
5053	114	38	114	39	Emissions data for a single year could be made comparable with the weights based on pulses. I think this would also apply to the time series of yearly emissions if the intention is to describe the development of the emissions themselves (for example, Figure 2.2 of AR6-WG3-FOD Chapter 2). Emissions would then be presented by gas or by agents. - If the purpose is to describe the climate impact of a scenario, then a method that distinguishes LLCFs and SLCFs could be used. [Ilkka Savolainen, Finland]	Taken into account. Thank you for this comment. We have tried to reflect some of these different purposes.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106381	114	38	114	41	This formulation can easily be misinterpreted as being policy prescriptive. Rewording would be useful here. It can also be valuable for context that country targets as included in NDCs are currently expressed as single-year targets and do not define a continuous scenario over time. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have tried to make it clear in the first sentence of the Box that IPCC cannot be policy prescriptive. (This also applies regarding customary practice.)
96767	114	38	114	41	"may be advisable", "may be a better choice" - please refrain from providing policy prescriptive recommendations and keep to the facts. [Nicole Wilke, Germany]	Taken into account. We have rephrased this for clarity.
73931	114	39	114	41	The sentence "If the purpose of the comparison are to consider the effects of scenario emissions over time..." does not fit to the previos sentence. The previous sentence refers to effects of a species emitted in a single year", then it would be logic to explain the purpose of an integrated metric and not a singel year metric. But to add the purpose of emission sccenarios over time, is a different logic and purpose. The sentence as it is 'a metric which captures the fundamental differences between LLCfs and SLCFs' is very unclear as this chapter shows that there is no single metric available that captures the fundamental differences bteween LLCF and SLCFs and the recommendation is to use separate metrics. Please add a statement linking this purpose consideration to integrated metrics and revise the sentence of a metric capturing the fundamental differences. [Anke Herold, Germany]	Rejected. Metrics are used for both purposes. Our main point is that the latter implies a different metric choice than the former. Also, the fact that there is no single, perfect metric, should not be misread as an endorsement of the status quo. That is not a logical inference.
17841	114	43	114	44	Emissions metrics are useful to compare impacts of different gases (e.g., to give a rough estimate the net contribution of a given entity's emissions to climate change), but they are also useful for trading regimes to provide "what" flexibility, which might be worth noting here. [Marcus Sarofim, United States of America]	Noted. WGIII is also assessing metrics, and will refer to the aspects you describe. The WGI aim is to assess the most significant emissions metrics literature from a physical science perspective.
5055	114	43	114	44	On the other hand, wouldn't it be better if, in the case of a scenario, each gas or agent will be treated with its own concentration and radiative forcing model and the warming or other end-point could be calculated for each agent using these? IPCC could provide an approved simple programme package that could be downloaded to the computer of the user. [Ilkka Savolainen, Finland]	Noted. We are suggesting that the only way to capture all the climate effects of each species is to treat the gases separately. If metrics are required, then users should choose one that aligns with the variable of interest.
23587	114	43	114	47	This para is factually correct but paints an overly simplistic solution. If one adopts multiple baskets, one then has to decide how stringent the targets for each individual basket should be - and that decision inevitably will have to consider all the same issues like time horizon and discounting, and whether we care about warming in addition to previous levels of warming, or warming that occurs compared to the absence of a given emissions source, that one thinks one has successfully avoided by avoiding the use of emission metrics within a single basket. I don't expect this text to go into details here (as that is outside of WGI scope), but some recognition is important here in my view that using a multi-basket approach simply shifts the value judgements elsewhere in the policy process, it cannot avoid them. It also doesn't necessarily make such judgements easier or more transparent since targets for individual baskets are then prone to pressure from special interest groups with their own means of influencing decisions and framing debates. [Andy Reisinger, New Zealand]	Noted. Policymakers make trade-offs like this all the time. Cumulative pollutants such as lead are regulated differently from flow pollutants such as alcohol or air pollution. This is not usually controversial.
99395	114	43	114	47	Here it is perhaps possible to touch upon the recent implementation of the two-basket approach in New Zealand as a new development. [Katsumasa Tanaka, France]	Rejected. Thank you for the comment. We have consulted with colleagues from WGIII. That the choice was made in view of the new physical science research on emissions metrics is relevant to this assessment. However, the options that were discussed and are under development are much more clearly in WGIII's realm.
114663	114	43	114	47	The paper by Daniel et al., 2012 in Climate Change is relevant here: Limitations of single-basket trading: lessons from the Montreal Protocol for climate policy [Jan Fuglestvedt, Norway]	Accepted
114669	114	43	114	47	Related to this: I think you need to make it clear that there will always be - at some level - considerations of SLCF vs LLGHGs. By having a two basket approach, this weighting will have to be done at the level where the targets are decided - and not left to the emitters as for a single basked approach. A two basket apporach will give more control of the outcome. [Jan Fuglestvedt, Norway]	Noted. One, two, and multi-basket approaches are all possible.
96769	114	44	114	47	"Although there is a history of using single-basket approaches, supported by emission metrics, in climate policy via the Kyoto Protocol, multi-basket approaches also have many precedents in environmental management, including the Montreal Protocol." Please provide context for this statement, i.e. the MP deals with the ozone layer in the stratosphere which is a much simpler problem than climate change, and in this case a multi-basket approach might be useful. The current statement is one-sided and hence policy prescriptive. [Nicole Wilke, Germany]	Rejected. We do not think that pointing out that policymakers have options, and that other global atmospheric commons issues have used multi-basket approaches, is at all policy prescriptive. (It is relevant, but essentially descriptive.)

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
73921	114	44	114	47	The comparison with the approach in the Montreal Protocol is misleading and not scientifically based. The Montreal Protocol targets artificial substances with very specific applications for which replacement substances are available and the targeted substances have very different impacts. The single-substance targets are derived from the availability of replacement options and substances are phased out while replacements are phased-in. Only fluerinated gases are comparable to the Montreal Protocol, not CO2, N2O or CH4 having natural sources and key functions in the gloabl carbon cycle. The history of policy approaches are linked to scentific differences of the underlying chemicals and it is diappointing if a WG1 chapter does not recognize these differences. Thus, it is suggested that this comparison with the Montreal Protocol is deleted. [Anke Herold, Germany]	Rejected. Emissions metrics in the climate change sense grew out of research into ozone-depleting substances. The researchers conducting that research were aware of the difficulties in porting the ozone depletion potential approach across, which is why the FAR's discussion was so heavily caveated. We think the current text is relevant, and reasonable.
112439	115	1	116	20	This section only nominally reviews applications of climate metrics in many policy uses, and focuses heavily on "comparison against climate change targets." In so doing, the section fails to address the qualities of metrics that make them more useful for basic planning (such as comparing emissions from various sectors or industries when those sectors / industries emit multiple GHG), life-cycle work, etc. For example, it is critical that metrics be simple and straightforward for these purposes. Metrics which, in contrast, change over time, dependent on broader mitigation, are simply far more complex, which is detrimental for users. In addition, more complex metrics are more dependent on complex analysis or modeling, which will inevitably and reasonably reduce policy-makers' confidence in them. These advantages of metrics like GWP are not addressed in this section and should be added. In short, simplicity is a valid advantage of GWP and other simple metrics, and this should be mentioned. [David McCabe, United States of America]	Noted. WGIII is also assessing metrics, and will refer to the aspects you describe. The WGI aim is to assess the most significant emissions metrics literature from a physical science perspective.
106363	115	1	116	20	The section on interpretation of emission metrics beyond their physical climate properties is outside the scope of WG1. Given the mandate of WG1 and expertise present in the chapter, this discussion outside the physical science realm would be better left to either WG3 or the SYR, where all evidence can be adequately reflected. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have been in contact with WGIII and revised the section in light of those exchanges. Many of the points in here about uses of metrics were included in response to review comments on the FOD. We are comfortable with the expertise on the chapters.
106371	115	1	116	20	Some of the paragraphs in this section do not meet the standards of an IPCC assessment. Several critical statements are unreferenced, and use of language is imprecise. For example, p115 lines 50ff. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. This comment is vague, though we have rephrased this section.
51415	115	1	116	20	This section starts an important discussion on the use and implications of different metric. It is also important to be clear on the scope of this WGI contribution to the issue and suggest that this is clearly outlined at the beginning. At present, the section is too brief and presents an incomplete picture and limited assessment, which is understandable to an extent given the remit of WGI. A full assessment of the implications requires information that falls under the scope of WGIII and we wonder if this entire discussion would be best placed there. This section does attempt to briefly touch on those issues, but not in sufficient depth to build a full understanding among policy makers so as to inform their decision making. For example, the text mentions that there is new information on the implications for net zero emissions without further discussion. It also suggests that different metrics might affect achievement of the Paris Agreement temperature goal without further details and potentially contradicts the findings of the SR1.5. Another use of metrics is to understand different mitigation options, and relating to this, a discussion on what different metrics might mean for different sectors, e.g. agriculture, would be useful. These issues all lie within WGIII territory so we wouldn't suggest they are included in detail here but they are vital for understanding the metrics issue and we hope that the WGI and WGIII authors can work together to address these in a complete way. We suggest that this section is moved to WGIII where it can be covered in its entirety. If authors, however, feel WGI should include part of this discussion, then we suggest that (1) it is limited to areas that directly relate to the physical science and it is clearly highlighted that that this is the case; (2) areas that would be better covered by WGIII are removed and placed in WGIII where a fuller discussion is possible in the appropriate context; (3) it is highlighted that a full assessment of the implications of different metrics, this section must be considered alongside information in WGIII. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have been in contact with WGIII and revised the section in light of those exchanges. We anticipate some areas of agreement and some areas of disagreement. Many of the points in here about uses of metrics were included in response to review comments on the FOD. We have been clearer regarding the bounds of the assessment.
14867	115	1			Additional FAQ should be considered. What are the cause(s) of atmospheric heat transport changes in the remote past, recent past, future? What are the cause(s) of ocean heat transport changes in the remote past, recent past, future? Aerosols - What have we learned since IPCC AR5? GHG- What have we learned since IPCC AR5? etc ... [Marie-France Loutre, Switzerland]	Taken into account. We have considered the addition of new FAQs and concluded that adding one on the energy budget would be the most appropriate thing to do, in terms of public interest, reflection of the chapter content and feasibility.
68167	115	3	116	20	I am not sure why there is one sub-subsection (7.6.3.1) for this subsection (7.6.3)? Suggest deleting “Interpretations of emission metrics” and just having this text under “Application of emissions metrics.” [Ilissa Ocko, United States of America]	Taken into account. We have revised the structure along with the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68169	115	3	116	20	There are additional applications that should be included, such as: education/communication resources that are attempting to inform public/policymakers about the relative contribution of sectoral and country emissions to a total, which requires a metric to add up emissions of different species. These are in online graphics, in museum exhibits, in prominent reports, in journalism reporting, etc. Another application is the reporting of emissions from activities that emit multiple pollutants, but aren't necessarily lifecycle assessments, such as "carbon" footprints of an individual, household, business, food item, energy source (such as hydro and natural gas), etc. For example, every carbon footprint calculator tool I have ever encountered sums up emissions using GWP100. [Ilissa Ocko, United States of America]	Noted. Scientists may have a range of perspectives on attempts to use CO2e in educational contexts - some physical scientists argue these may mislead more than they illuminate.
32101	115	5	115	8	See comment on page 113 above. Please reinstate the 20yr GWP, as it will help in talking to farmers (crop waste biomass burners and methane emitters) in rural Africa. Yes, there are sophisticated casuistries going on, and there are many reasons why the GWP concept is shaky, but when a poor farmer wants to understand and perhaps to do something that might help soon, GWP 20 is a valuable metric: it is real, here and now, it is easily understood, and it shows that even rural folk can help a lot. My age-10 rural primary school had no electricity. I did my homework by candle light. We feared fires. Just putting out those little fires before they get vastly out of control really does contribute to climate! [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We are now assessing GWP20. It's up to policymakers whether they think giving people an exaggerated view of the consequences of SLCF mitigation will aid or hinder trust in climate policy in the long run.
98455	115	5	115	12	The chapter states that the timescale associated with the comparison is an important choice. Partly to show the effects of timescale on emission metrics, previous IPCC reports reported 20-year, 100-year, and 500-year values for GWP, and 20-year and 100-year values for GWP and GTP. Time-varying emission metrics also involve the choice of a time-horizon, though in these cases the time horizon is usually derived from a climate target (most commonly a temperature target). Time horizon is a choice that, ideally, ought to reflect decision-makers' needs, depending on the specific application and the appropriate weighting of different aspects of climate change for a given situation. IPCC sets the stage for the development of policy worldwide and 20-year GWP values are important for nations/subnationals developing climate mitigation plans which factor in the importance of policies directed at reducing SLCFs in the near term. CARB has developed an SLCF Strategy for California (which primarily uses 20-year GWP values) and tracks emissions of SLCFs in California through our GHG Inventory (which reports emission in 100-year values). CARB utilizes both 20-year and 100-year GWP values to characterize impacts and benefits of policies and regulations. Updating the 20-year and 100-year GWP values in parallel is important to CARB to ensure there are no inconsistencies when comparing metrics for our emissions. SLCF mitigation is not always a secondary co-benefit of long-term reductions in CO2 emissions. The importance of specifically focusing on policies which mitigate SLCPs is more clear with the use of 20-year values. For example, air conditioners use hydrofluorocarbons, which are potent SLCPs, as refrigerants. Reducing SCLP emissions from refrigerant is not a co-benefit of policies which focus on CO2 emissions from air conditioners as the latter focuses solely on energy efficiency. A good example of this is the U.S. Department of Energy setting minimum energy efficiency standards for air conditioners. There is no reduction in SLCF used as refrigerants as a co-benefit of energy efficiency standards for these appliances. To address SLCFs from refrigerants, CARB has proposed setting GWP limits on the refrigerants used in air conditioners as a part of the SLCF Strategy for California. CARB's GWP limit on air conditioners and the timeline for the proposal to take effect in the next few years is informed by an understanding of how impactful these SLCFs are in the 10 to 20-year period. CARB recommends that such metrics should be included (e.g. GWP20, GTP10/20) as metrics are used not only for analysis of consistency with long-term temperature targets, but also for life-cycle analyses, for	Taken into account. We have now included an assessment of GWP20. We are liaising with WGIII, who will have their own section on metrics.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
69901	115	5	115	15	Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. As noted on 7-116 L9-12: "it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice. Physical science can only form a subset of the inputs to those choices." Policymakers should have access to multiple metrics, including metrics that allow for a two-basket approach and recognize the near-term impacts of SLCP (such as GWP20 and GTP20).. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]")." [Gabrielle Dreyfus, United States of America]	Taken into account. We have now included an assessment of GWP20. We are liaising with WGIII, who will have their own section on metrics.
66851	115	5	115	15	GWP500 is included, but GWP20 is not; GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We are liaising with WGIII, who will have their own section on metrics.
66853	115	5	115	15	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We are liaising with WGIII, who will have their own section on metrics.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68413	115	5	115	15	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. This is important because many feedbacks and tipping points are anticipated within the next 10 to 20 years, as the 1.5C guardrail is approached and likely breached. Masson-Delmotte V., et al. (eds.) (2018) SUMMARY FOR POLICYMAKERS, in IPCC (2018) GLOBAL WARMING OF 1.5 ºC; Lenton T. M., et al. (2019) Climate tipping points—too risky to bet against, NATURE, Comment, 575:592–595; Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259, 8254; and Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5784. GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-	Taken into account. We have now included an assessment of GWP20. We are liaising with WGIII, who will have their own section on metrics.
68415	115	5	115	15	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account. We have now included an assessment of GWP20. We are liaising with WGIII, who will have their own section on metrics.
17843	115	5	115	15	Both Sarofim and Giordano (2018) and Mallapragada and Mignone (2020) discuss the translation of GWP time horizon to discount rates, which can be useful for policy makers that use discount rates in other settings in order to value impacts across time (and both find that the 100-year GWP is roughly consistent with a 3% discount rate, which is a common discount rate used in policy settings). This concept would be worth raising in a paragraph about time horizons. [Marcus Sarofim, United States of America]	Rejected. This is more in the domain of WGIII, who will have their own section on metrics. Our aim is to assess the recent literature from a physical science perspective.

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89787	115	5	115	15	This paragraph is the only concentrated discussion on timescales and time horizons, which is important enough to warrant its own subsection and thorough discussion. I recommend adding a new section "Importance of time horizon" or "Choice of time horizon" under section 7.6.3 OR before section 7.6.2.5 where metrics of various time horizons are given. This new section can start with some context on role of timescale in the metric value, then move on to commonly used time 20-, 100-, and 500-year time horizons and their interpretations. It would be useful to emphasize here that choosing one time horizon is prioritizing that specific timescale over the others, which makes the near-exclusive use of GWP100 problematic. Many users apply CO2e/GWP without knowing it has a 100-yr time horizon embedded, it would be very valuable to point that out and recommend to always specify time horizon when using any metric. IPCC is in the position to inform users that the time horizon is a choice to make before applying any metrics to calculate emissions. The bare minimum recommendation would be adopting a reporting routine to include both 20- and 100-year time horizons that represent near- and long-term climate impacts. It is not perfect but simple enough illustration for the temporal tradeoffs associated with short-lived and long-lived climate forcers' impacts (Ocko et al. Unmask temporal trade-offs in climate policy debates, Science, 356, 6337, p.492-493 (2017)). More importantly, this approach is technically accessible to most users since they are already using GWP100. For the users that have the technical expertise, on the other hand, a more scientifically appropriate metric should be adopted. [Tianyi Sun, United States of America]	Rejected. Thank you for the suggestion but we are satisfied with the current structure.
116651	115	5	116	6	This section could be shortened and sharpened and could benefit from also including the perspective of authors of WGIII (including on ethics of metrics). [Valerie Masson-Delmotte, France]	Taken into account. We are liaising with WGIII who will have their own section on metrics. We anticipate some areas of commonality and some differences.
100467	115	7	115	8	AR5 also included 50-year values for GTP [Øivind Hodnebrog, Norway]	Taken into account. We are including GWP20, and have tried to make the tables clearer.
105	115	8	115	10	Questionable that for emission metrics "… the time horizon is usually derived from a climate target (most commonly a temperature target)." Time horizons may be chosen by policymakers to relate to mitigation targets, which themselves have a variety of time-horizons - NDCs to 2025 or 2030; net zero in second half of century globally, some countries choosing to aim at net zero in 2050, others longer. Emission metrics may be applied at global scale (and temp limits necessarily are global) or national and local scales.. Before Paris in 2015, there were no agreed global temperature goals. Now there are, but there are also other time-horizons. The next sentence goes on to decision-makers, but this one is too absolute, please rephrase. "Climate target" is a vague term - it might relate to impacts, or mitigation targets etc. Unless there is a definition or glossary entry, consider more precise language (see also comment on p. 115, line 31) [Harald Winkler, South Africa]	Taken into account. Good point. We have rewritten this sentence.
114643	115	9	115	10	I think you can delete the parentheses with "most … target", and change "climate target" to "temperature goal" [Jan Fuglestvedt, Norway]	Taken into account. We have rephrased.
68127	115	10	115	12	The text states that time horizon is a choice that ought to reflect decision-makers' needs, but then provides no options for metric users interested in near-term metrics and short-term time horizons. By not providing users with near-term metrics, the IPCC is therefore inherently making a policy decision because the community is left with no option to make this value judgement for themselves – despite the IPCC saying that it is up to them to do so. [Ilissa Ocko, United States of America]	Taken into account. We have now included an assessment of GWP20.
64767	115	10	115	12	The text states that "Time horizon is a choice that, ideally, ought to reflect decision-makers' needs, depending on the specific application and the appropriate weighting of different aspects of climate change for a given situation," yet there are no options provided for metric users interested in near-term metrics and short-term time horizons. By not providing users with near-term metrics, the IPCC is therefore inherently making a policy decision because the community is left with no option to make this value judgement for themselves – despite the IPCC saying that it is up to them to do so. [Steven Hamburg, United States of America]	Taken into account. We have now included an assessment of GWP20.
73933	115	12	115	13	Please add which timescales for which metrics are appropriate for the purposes of the Paris Agreement . For policy makers it may not be relevant what is universally applicable, but what is applicable to the objectives and tome horizons they have chosen under the Paris Agreement and for this purpose there are more apprpremiate and less appropriate time horizons. Such message is very important also in related to the discussion on GWP20 for methane that is dominating in many countries at the moment and which does not seem to be an appropriate choice for CO2 or N2O in relation to the Paris Agreement goals. [Anke Herold, Germany]	Rejected. The points we are making about the physical implications of flows of gases are not only tied to the PA targets, so we are trying to keep the discussion more general than that. It would be misleading to suggest that the only timescales that matter to anyone are those associated with PA targets.

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51409	115	12	115	13	could you expand on what is meant by "universially appropriate"? [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Noted. GWP100 is not a good metric for all purposes. It is a good metric for purposes that align with centennial time-integrated radiative forcing. For other purposes - such as the estimation of surface temperatures - GWP100 performs badly, as a long line of literature has repeatedly and uncontroversially shown.
107	115	12	115	15	The first clause of the first sentence, starting line 12. is correct, stating the common approach is a 100-year time-scale; the second clause "but this is not universally appropriate" sounds policy-prescriptive, suggesting that choices made by policymakers to use GWP100 from IPCC ARs for reporting (decision 18/CMA.1) are not appropriate in a universal agreement. In negotiations, common metrics have been debated - notably with GTP being advanced. The conclusion in the Paris rule-book is to require GWP100 as in AR5, and to allow - in addition - countries to report other metrics as well. It is based on a reference to AR5, which was published prior to the adoption of the Paris rule-book in 2018. Referring to the First Assessment Report is redundant, both analytically and in policy terms. Analytically, there have been four assessments since Houghton 1990, each of them is aware of what has gone before, so does not need to be restaed in a new context. Policy-makers are well aware of the IPCC guidance, explicitly referring to specific assessments - and requiring in a mandatory language ("shall") reporting using GWP 100 from AR5, and foreseeing that updated GWP100-values in future IPCC assessments can be adopted by the CMA; the reference to other metrics is voluntary and additional to GWP-100 ("may in addition also use"), not replacing (para 37, 18/CMA.1). The sentence starting "In fact..." should be deleted. [Harald Winkler, South Africa]	Rejected. We disagree. The decision 18/CMA.1 is a reporting convention, and does not exhaust the possible uses of metrics.
109	115	13	115	13	replace ; with : (or make clear that it is ch 8 in FAR, not AR6) [Harald Winkler, South Africa]	Rejected. It's AR5.
51423	115	14	115	15	Please provide further information on how big this improvement is and how significant it is. This section is missing information on how much the quantification of surface warming is improved which makes it difficult to assess the benefits of separating short-lived and long-lived gases in the context of other considerations. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have added a figure adapted from fig 2 of Cain et al, 2019.
114645	115	17	115	19	This short para does not sit well here, in my view. Needs more integration in the text. [Jan Fuglestvedt, Norway]	Taken into account. We have revised the para
2741	115	21	115	29	Section 7.6.3.1 is overly wordy and repetive. Lines 21-29 could easily be integrated elsewhere [Bryan Weare, United States of America]	Taken into account. We have revised the text to reduce repetition.
73935	115	21	115	29	It remains unclear why the choice of metrics for LCA is different from the general considerations in this chapter. Given the fact that different metrics have very different values, it is clear that the choice impacts LCA. But LCA are a tool for implementing Paris agreement goals and do not have a purpose on their own. Therefore it is unclear why specifically for LCA additional or different policy goals should be discussed. This paragraph needs either clarification , but on the other side does not seem very essential in this chapter and may be deleted. [Anke Herold, Germany]	Rejected. Not everyone undertaking LCA sees them as wholly dependent on the PA. LCA existed before the PA, and would exist as long as people are working on mitigation, even if the PA targets are missed or become obsolete.
102103	115	21	115	29	A core recommendation of Cherubini et al. (2016, Environmental Science & Policy, doi:10.1016/j.envsci.2016.06.019) and Levasseur et al. (2016, Ecological Indicators, doi:10.1016/j.ecolind.2016.06.049) is the use of more than one metric (i.e. GWP100 and GTP100), with even additional ones for sensitivity analysis (i.e. GWP20 and GTP20). There was a debate between Tanaka et al. (2019, Nature Climate Change, doi:10.1038/s41558-019-0457-1) and Ocko et al. (2017, Science, doi:10.1126/science.aaj2350) regarding which "multiple" metrics should be used for impact assessments. The different views may arise from a difference in the way how to interpret the time scale in the policy context. This line of debate is an outcome of a task force consisting of more than 10 experts, supported by the United Nations Environment Programme (UNEP) and the Society of Environmental Toxicology and Chemistry (SETAC). I hope that this will be properly reflected to AR6. [Katsumasa Tanaka, France]	Taken into account. We have developed the section on multi-metric approaches, with a focus on the physical science dimensions.
23589	115	31	115	31	Care is needed when referring to the Paris Agreement: my understanding is that the PA doesn't have temperature 'targets', it has a temperature 'goal' - and it has only one single 'goal' that is expressed as limiting temperature to well below 2 degrees and pursing efforts for 1.5 etc - i.e. one, complex goal - not two separate goals. Please ensure that the PA is referred to consistently in this para, and not re-interpreted in mutiple different ways (within the same para!) as it is currently. [Andy Reisinger, New Zealand]	Taken into account. Thank you for this comment. We have tried to be clearer on this point.
111	115	31	115	31	How is a "climate change target" defined? It seems from the next sentences that you mean global temperature goals. In what sense is the global goal for adaptation in Article 7.1 not a "climate change" target? If you mean numerical targets to limit temperature increase, then call them that. Finally, these are limits rather than targets, in the sense that we do not aim at 1.5 degC, but to stay below that limit. [Harald Winkler, South Africa]	Taken into account. Thank you for this comment. We have tried to be clearer on this point regarding temperature targets.

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99399	115	31	115	48	Both Fuglestvedt et al. (2018, doi:10.1098/rsta.2016.0445) and Tanaka and O'Neill (2018, doi:10.1038/s41558-018-0097-x) showed that GWP100 leads to decreasing temperature and GTP100 to stable temperatures when these metrics are used to defined the net zero GHG emission target. I think this is an important element for consideration for the Paris Agreement implementation in the long run and I suggest this as an addition to this paragraph. [Katsumasa Tanaka, France]	Taken into account. We have added the point that net zero all gases using CO2e leads to a peak and decline temperature curve.
81531	115	31	115	48	This para is rather long and doesn't cite much research. The sentence on Paris Agreement Article 4 in the middle (l. 36-37) seems unnecessary for the discussion on metrics. There is also perhaps too much emphasis on 'net zero emissions', as the temperature limits are perhaps more characteristic and unambiguous 'headline targets' of the Paris Agreement. [Tommi Ekholm, Finland]	Taken into account. The paragraph has been rephrased. Net zero emissions targets for long-lived gases are implied by temperature limits.
106367	115	31	115	48	Parts of this section presuppose an interpretation of the Paris Agreement temperature goal and what the Paris Agreement's Article 4 intends to achieve. This is outside the scope of WG1 and not in the mandate of IPCC. Staying within the mandate and expertise of WG1, woudl probably imply that this section should be removed. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have tried to be clearer about temperature targets.
106373	115	31	115	48	Some of the claims in this paragraph go against the assessment presented in Chapter 1. To avoid duplication and inconsistencies, I suggest removing them here. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. The usual IPCC process is for the Introduction to take its lead from the substantive chapters.
73925	115	31	115	48	It is a very useful addition that metrics are assessed in relation to the objectives of the Paris Agreement. The message that for the purposes of the PA, short-lived forcning agents only play a secondary role does not seem to be in line with the other messages such as in line 51 to 54 that it is necessary to draw the distinction between short- and long-lived forcing agents. The Paris Agreement is not just one use of metrics, but the most important use of the AR6 as such. If for the purposes of the Paris Agreement SLCFs only play a secondary role, why is it the necessary for the same policy purposes to distinguish short-term and long-lived forcing agents by using different metrics? [Anke Herold, Germany]	Noted. SR1.5 also made the point that it was necessary to get LLCFs to zero and only to reduce SLCFs. The reason we need to distinguish the two is because they have quite different effects on the climate - ambiguities arise if we neglect those differences.
51411	115	32	115	32	Suggest changed to "is the global temperature goal in Article 2 of the Paris Agreement". Despite mentioning two levels of warming, it is one goal as referenced later in the Paris Agreement [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Scientifically, it is usual to distinguish between 1.5 and 2, though we accept this may be less customary in diplomatic circles.
113	115	33	115	33	The statement that the "Paris Agreement has no other numerical targets" is narrow in several senses, and not helpful. It accurate only when limited narrowly to the agreement; the decision adopting the agreement (1/CP.21) refers twice a numerical target ("goal") for finance, $ 100bn per year - in para 53 and para 114. More generally, why are numerical targets important? If the argument is about quantification, then why refer to "implicit science targets" under Article 4.1, and construct an argument of how qualitative language of a balance can be quantified; but not do the same for the global goal on adaptation in Art 7.1? [Harald Winkler, South Africa]	Taken into account. We have tried to be clearer on this point.
51413	115	34	115	34	What is meant by a "science target"? Please clarify. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have worked on the phrasing here. Some people argue, somewhat fancifully, that Article 4's sources and sinks reference represents a numerical target. We have been more explicit.
69903	115	36	115	48	This paragraph appears to focus exclusively on longer-term temperature, and ignores the short-term impact of each tenth of a degree on increasing climate extremes, which are the source of most health impacts and destruction. This is a subjective value judgement (that future peak temperature is more important than near-term impacts). Significant cuts to SLCP can significantly slow the rate of warming in the near term (e.g. Xu and Ramanthan, 2017; Harmsen et al., 2019), which will reduce risks and damages associated with climate extremes and reduce risks associated with triggering tipping points and feedbacks. These near-term mitigation strategies are essential complements to CO2 mitigation. [Gabrielle Dreyfus, United States of America]	Noted. Warming rates are also currently dominated by increases in CO2. Cuts to SLCFs may reduce the warming rate, but if those trade-offs are made at GWP100 values they imply a warmer world in the longer term. If those trade-offs are made at GWP20 values, they imply a warmer world, fairly quickly.

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66855	115	36	115	48	GWP500 is included, but GWP20 is not; GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We are working with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.
66857	115	36	115	48	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We are working with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.

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68417	115	36	115	48	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. This is important because many feedbacks and tipping points are anticipated within the next 10 to 20 years, as the 1.5C guardrail is approached and likely breached. Masson-Delmotte V., et al. (eds.) (2018) SUMMARY FOR POLICYMAKERS, in IPCC (2018) GLOBAL WARMING OF 1.5 ºC; Lenton T. M., et al. (2019) Climate tipping points—too risky to bet against, NATURE, Comment, 575:592–595; Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259, 8254; and Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5784. GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-	Taken into account. We have now included an assessment of GWP20. We are working with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.
68419	115	36	115	48	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account. We have now included an assessment of GWP20. We are working with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.
115	115	37	115	37	"constraints" sounds like a modeling term. Equity is a 'basis', and SD and poverty is 'context' [Harald Winkler, South Africa]	Taken into account. We have revised the text, but it's fairly standard to acknowledge that the presence of multiple goals brings constraints as well as opportunities.
81533	115	38	115	38	There's one paper that could be cited here, looking into the interpretation of the 2C target and the implication of this interpretation on 'optimal' emission metric: T Ekholm, TJ Lindroos, I Savolainen (2013) Robustness of climate metrics under climate policy ambiguity, Environmental science & policy 31, 44-52. https://doi.org/10.1016/j.envsci.2013.03.006 [Tommi Ekholm, Finland]	Not applicable - text rephrased. Thank you for the suggestion.
114647	115	38	115	38	Since you refer to Art 4 of the PA, you may use the same wording; i.e. "balance…" and not "net zero" [Jan Fuglestvedt, Norway]	Accepted-

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117	115	38	115	46	This part assesses the meaing of "net zero" - a phrase that does not occur in Art 4.1, you cite the 'balance' above. So "net zero" is itself an interpretation. Are both the emissions from sources and sinks anthropogenic ? See: Fuglestvedt, J, Rogelj, J, Millar, R J, Allen, M, Boucher, O, Cain, M, Forster, P M, Kriegler, E & Shindell, D 2018. Implications of possible interpretations of 'greenhouse gas balance' in the Paris Agreement. Philosophical transactions. Series A, Mathematical, physical, and engineering sciences 376 (2119): 20160445. 10.1098/rsta.2016.0445 Next, the phrase "Paris compatible" is extremely vague: unclear which part of the Agreement and / or decision is compatible; NDCs, the long-term goal on mitigation (or adaptation), or the global temperature limits (GTLs). Even if specified to the GTLs, does Paris compatiable mean "well below 2 degC", or pursuing 1.5?; how far below 2 is "well below"?, presumably above 1.5: The phrase occurs in the literature, certainly -but I doubt there is an agreed defintion (if so, cite it). The task of IPCC is to assess - in this case a critical assesement of the utility of the terms seems appropriate. [Harald Winkler, South Africa]	Taken into account. We reference Fuglestvedt et al 2018, and have made it clear that there are a range of possible interpretations. It doesn't really matter to the climate system whether a sink is anthropogenic or natural - what matters is how the gas interacts with the climate system. Insisting that there is an important scientific distinction between anthropogenic and natural sinks is a bit like arguing that the Blue Nile does not flow into the Mediterranean - in terms of customary categories, it flows into the White Nile; but the water ends up in the same place...
131	115	38	115	46	This part assesses the meaning of "net zero" - a phrase that does not occur in Art 4.1, you cite the 'balance' above. So "net zero" is itself an interpretation. Are both the emissions from sources and sinks anthropogenic ? See: Fuglestvedt, J, Rogelj, J, Millar, R J, Allen, M, Boucher, O, Cain, M, Forster, P M, Kriegler, E & Shindell, D 2018. Implications of possible interpretations of 'greenhouse gas balance' in the Paris Agreement. Philosophical transactions. Series A, Mathematical, physical, and engineering sciences 376 (2119): 20160445. 10.1098/rsta.2016.0445 Next, the phrase "Paris compatible" is extremely vague: unclear which part of the Agreement and / or decision is compatible; NDCs, the long-term goal on mitigation (or adaptation), or the global temperature limits (GTLs). Even if specified to the GTLs, does Paris compatiable mean "well below 2 degC", or pursuing 1.5?; how far below 2 is "well below"?, presumably above 1.5: The phrase occurs in the literature, certainly -but I doubt there is an agreed defintion (if so, cite it). The task of IPCC is to assess - in this case a critical assesement of the utility of the terms seems appropriate. [Harald Winkler, South Africa]	Taken into account. We reference Fuglestvedt et al 2018, and have made it clear that there are a range of possible interpretations. It doesn't really matter to the climate system whether a sink is anthropogenic or natural - what matters is how the gas interacts with the climate system. Insisting that there is an important scientific distinction between anthropogenic and natural sinks is a bit like arguing that the Blue Nile does not flow into the Mediterranean - in terms of customary categories, it flows into the White Nile; but the water ends up in the same place...
23591	115	39	115	39	insert "scientific" before "interpretations" since this is what Fuglestvedt et al were doing - the interpretation and intention of policymakers when the PA was agreed may or may not be a scientific one. Don't assume that science can be used to 'interpret' a geopolitcal document (or at least make clear that the only interpretation that WGI can offer is purely from a science perspective, not with regard to the actual intentions of the people and countries that agreed the document). [Andy Reisinger, New Zealand]	Taken into account. We have rephrased.
65751	115	39	115	48	Suggest deletion. The discussion of the interpretation of the Paris Agreement’s goal and the relevance of metrics to them is not relevant to this section. Suggest deleting the text from “New research” onwards. [Kushla Munro, Australia]	Rejected. We think the work is clearly relevant to policy. Other commenters certainly seem to think so.
103649	115	39	115	48	Is it possible to describe the non-CO2 conditions for 2°C/1.5°C in more tangible/quantitative terms? The existing phrasing (while stabilising, reducing or eliminating short-lived forcing agents) is challenging to interpret. Furthermore, the statement that they play a 'secondary role' is very open-ended. The phrase 'secondary role' seems at odds with the evidence presented in this chapter regarding temperature response to step-changes in SLCFs. [Philippe Tulkens, Belgium]	Taken into account. We have worked to make this material clearer. It is apparent that the main goal of climate policy is getting to net zero on CO2. CO2 determines whether we get 2, 3, 4 or more degrees of warming. The mitigation of other species make a few tenths of a degree's difference.
89425	115	39			This statement is oblivious to the fact that the PA was informed by the science of the time, which is the IPCC AR5 and the accounting in there which was based on GWP100. Indeed, using other metrics in the context of the Paris Agreement mitigation architecture has been shown to introduce major inconsistencies between Art 2 and 4 (see Schleussner et al. 2019). [Carl-Friedrich Schleussner, Germany]	Noted. Multiple papers make the point that GWP is badly-aligned with temperature targets - e.g. but not only Wigley 1998, Shine 2005, Collins et al., 2019, Cain et al., 2019, Denison et al., 2020. The Schleussner paper relies on a narrow interpretation of Art 4.
114649	115	40	115	40	Since you refer to Art 4 of the PA, you may use the same wording; i.e. "balance…" and not "net zero" [Jan Fuglestvedt, Norway]	Accepted
96771	115	41	115	42	The sentence "net zero greenhouse gas emissions are not necessarily required to remain below 1.5°C or 2°C" is at odds with the budget approach. Since LLCF remain in the atmosphere for a very long time, the cooling effect of SLCF becomes soon irrelevant. The statement "while stabilising, reducing or eliminating short-lived forcing agents can play a secondary" is difficult to interpret, and it seems inconsistent the temperature response to step-changes in SLCFs. Please revise. [Nicole Wilke, Germany]	Taken into account. It is not at odds with the budget approach, if the budget treats SLCFs differently from LLCFs, as is implied by the science in this chapter. Nevertheless, we have revised the text.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
23593	115	41	115	44	The sentence as written is misleading since it confuses scientific principle (if we were reducing CO2 emissions rapidly, net-zero GHG is not necessary) and practicality (but we are demonstrably not reducing CO2 emissions rapidly, hence net-zero GHG may well be necessary to recover from the inevitable overshoot). Also, the sentence is wrong without qualifications, because as long as there are substantial non-zero emissions of other long-lived non-CO2 GHGs, then negative CO2 emissions will indeed be needed for stabilising temperature. Suggest rephrasing: "Significantly, net zero greenhouse gas emissions are not necessarily required if the goal is to remain below 1.5°C or well below 2°C without any overshoot, and that a target of net zero CO2 emissions could be sufficient, as long as the net-zero CO2 emissions target is achieved sufficiently rapidly and emissions of long-lived non-CO2 greenhouse gases are also reduced close to net zero (Tanaka and O'Neill, 2018)." [Andy Reisinger, New Zealand]	Taken into account. We have rephrased.
51417	115	41	115	44	This is an important point but needs rephrasing to avoid confusing non-experts on the necessity of net-zero GHG vs net-zero CO2, the latter being necessary (but not sufficient) for limiting warming to 1.5C/2C. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have rephrased.
81527	115	44	115	45	Phrase "Limiting on-going temperature increase at any level requires net zero CO2 emissions" requires either clarification or some proof/citations. Does the text refer to anthropogenic or anthropogenic + natural net emissions? If anthropogenic, then I would argue that the natural sinks can balance some positive anthropogenic emissions for quite some time, and the statement about net zero emissions is not entirely valid. [Tommi Ekholm, Finland]	Taken into account. Rephrased.
76825	115	44	115	46	"Limiting on-going temperature increase at any level requires net zero CO2 emissions, and while stabilising, reducing or eliminating short-lived forcing agents can play a secondary role, the main requirement for stabilisation of temperature is to limit cumulative emissions of CO2." This is rather subjective in the sense that net zero CO2 is critical to temperature stabilization, but if SLCFs were increasing you still wouldn't have stabilization even with net zero CO2. [Nathan Borgford-Parnell, Switzerland]	Taken into account. We have worked to make this material clearer. It is apparent that the main goal of climate policy is getting to net zero on CO2. CO2 determines whether we get 1.5, 2, 3, 4 or more degrees of warming. The mitigation of other species make a few to several tenths of a degree's difference.
68125	115	44	115	46	The IPCC Special Report on 1.5 Degrees (2018) made it clear that action on methane and black carbon is essential to achieving temperature targets, but this sentence in Section 7.6 makes it sound like it is an option. I strongly suggest that the text "reducing or eliminating short-lived forcing agents can play a secondary role" be revised as you cannot solve climate change without addressing SLCFs. This is not to undermine the role of CO2 in stabilizing temperatures, but both set of pollutants are important! We no longer have the luxury to pick and choose what we reduce. [Ilissa Ocko, United States of America]	Taken into account. We think it is important to give a sense of the scale of the effects of different gases on the climate, and their role in warming with and without mitigation. Nevertheless, we have revised the text.
23605	115	44	115	46	The claim that action on SLCFs can only play a secondary role is only true for the global perspective; for any given country or sector, this may not be true. This sentence should make this clear, as otherwise it would be wrong or misleading in its generality. [Andy Reisinger, New Zealand]	Taken into account. Rephrased.
32949	115	44	115	46	The text here states "Limiting on-going temperature increase at any level requires net zero CO2 emissions, and while stabilising, reducing or eliminating short-lived forcing agents can play a secondary role, the main requirement for stabilisation of temperature is to limit cumulative emissions of CO2." This is rather subjective in the sense that net zero CO2 is critical to temperature stabilization, but if warming SLCFs were increasing you still wouldn't have stabilization even with net zero CO2. SR1.5 and chapter 6 make clear that reduction of SLCFs is indeed required to meet low warming targets as well, so the phrasing here of 'secondary role' seems too strong to me. Both are required, with CO2 clearly being the largest forcing agent to date. so I believe it would be most defensible simply to state that these are the two requirements for stabilization: net zero CO2 and stable or decreasing SLCFs rather than trying to rank these in importance by some difficult to quantify metric that is unstated. [Drew Shindell, United States of America]	Taken into account. Section was rephrased. However, we already make the point that stabilising, reducing, or eliminating SLCFs plays a role - so we don't give the impression that increases in SLCFs are compatible with temperature stabilisation. (Which is true as long as we don't consider negative CO2 emissions - if we considered those, then some step increase in SLCFs could be balanced by cumulative negative emissions. But that seemed a bit abstract and not very relevant for decision makers.) It is important that policymakers understand that choices around CO2 make up most the difference between the RCPs - whether we get 1.5, 2, 3, 4 or more degrees of warming is largely because of what we do with CO2. SLCFs make between a few and several tenths of a degree's difference. Most people will see that as a secondary role.
64765	115	44	115	46	There is a disconnect between IPCC AR6 and the IPCC Special Report on 1.5 Degrees (2018) regarding SLCFs. Specifically, the IPCC AR6 states that "Limiting on-going temperature increase at any level requires net zero CO2 emissions, and while stabilising, reducing or eliminating short-lived forcing agents can play a secondary role, the main requirement for stabilisation of temperature is to limit cumulative emissions of CO2," yet the IPCC Special Report on 1.5 Degrees (2018) was clear that considerable emissions reductions of methane and black carbon are required to achieve temperature targets of 1.5 or 2C targets, and that action on one or the other will not get you to your target. You must do both. Therefore, there is no primary and secondary, action on both sets of pollutants is needed. [Steven Hamburg, United States of America]	Taken into account. We have made the compatibility between these more clear. Our point is more general than the material in SR1.5 - basically, the nearer the temperature target, the harder it is to get to net zero CO2, so the more strongly you have to mitigate SLCFs. That's why the SR1.5 required steep-ish cuts to SLCFs. But our point is more general - temperature stabilisation *at any level* requires net zero CO2, and that SLCFs do not grow (and possibly reduce, depending on the level). We have tried to make this distinction clearer.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98457	115	44	115	48	The chapter states that "Limiting on-going temperature increase at any level requires net zero CO2 emissions, and while stabilizing, reducing or eliminating short-lived forcing agents can play a secondary role, the main requirement for stabilization of temperature is to limit cumulative emissions of CO2", this needs to be justified. As a growing number of countries around the world are setting and updating long-term (2050 and beyond) emission reduction targets, these differences become highly relevant. Every tonnee emitted of any greenhouse gas makes the world warmer than it would have been otherwise, and thus avoiding the emission of any greenhouse gas brings a benefit to the climate – but because of the differing contributions to cumulative warming from the different gases, the long term goals for emissions reductions of each gas may differ. The emission reduction target for long-lived greenhouse gases globally is unambiguously dictated by basic physics: net emissions of long-lived greenhouse gases must go to zero if the global average temperature is to be stabilized. By contrast, physical climate considerations alone are insufficient to set a target for short-lived gases: emissions of short-lived gases do not have to go to zero, but the lower they go, the less they will contribute to the overall warming the world will experience. [nehzat Motallebi, United States of America]	Taken into account. The text was revised. We think this point is already captured in the text, and was made in SR1.5. It important to make the point that the first-order determinant of warming is CO2 - this is the gas responsible for most of the difference between any/all of the RCPs. There seems to be some confusion about this, even amongst some of the commenters on this chapter, so we have endeavoured to make it clear.
89427	115	44			But not with pathways with overshoot (Schleussner et al. 2019) and the PA Article 4 clearly refers to GHGs and needs to cater all possible interpretations of Article 2. In addition, Article 2 does not refer to temperature stabilisation, but only sets upper limits. Thus, net-zero GHGs cannot be inconsistent. The statement as it stands is thus incorrect, [Carl-Friedrich Schleussner, Germany]	Taken into account. We have rephrased.
83155	115	45	115	45	I think basically everyone would agree that getting to net zero CO2 is the most important for climate policy, so in that sence everything else, including SLCFs, is secondary. However, I am afraid that "secondary" can be read as "not important". Mitigation of methane and HFCs in particular can be very important in the timing of when we have to get to net zero for a given temperature constraints. Please rephrase. [Terje Berntsen, Norway]	Taken into account. We have rephrased, but it's also important to give a sense of priority - we agree that it is obvious that getting to net zero CO2 is the most important thing for climate policy, but some of our commenters seem markedly hesitant that we say this.
51365	115	45	115	46	It would be helpful to expand upon the 'secondary role' implied in "stabilising, reducing or eliminating short-lived forcing agents can play a secondary role" i.e. it can temporarily slow or reverse warming in the short-term, or reduce the level of peak warming, but be clear that such measures cannot stop long-term warming. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text has been revised. Yes, that's the sense we are trying to convey by making a more general statement about the general (rather than 1.5C- or 2C-specific) requirements for temperature stabilisation. SLCFs can help, but they are no substitute.
89429	115	45			Indeed. But reducing SLCPs is key in order to achieve the PA limits. In fact, a reduction in methane emission by up to 50% is a prerequisite of emission pathways achieving the PA targets (Compare SR.15). [Carl-Friedrich Schleussner, Germany]	Taken into account. The text has been revised. Yes - our point is more general than the point in SR1.5. There's no scientific disagreement there - it's just that our statement is more general. (Plus, the SLCF reductions were imposed by IAM-centred constraints - it was because it is socially implausible to achieve the PA targets through CO2 alone that those SLCF cuts were required: it's not a biogeophysical requirement. We have chosen not to make this point
86803	115	46	115	46	"The main requirement for stabilisation of temperature is to limit cumulative emissions of CO2". While this is true, stabilisation will not be achived even though we reach net-zero CO2 emissions if other forcers are increasing. Please elaborate this section to reflect that. [Oyvind Christophersen, Norway]	Taken into account. Text revised.
119	115	46	115	46	add "global" to limiting cumulative CO2 emissions, to add spatial scale [Harald Winkler, South Africa]	Accepted.
121	115	47	115	47	Allen et al in 2009 could not have known the aims of the Paris Agreement in 2015 [Harald Winkler, South Africa]	Taken into account. We have moved the references to the end of the previous sentence.
96773	115	50	115	55	This paragraph is policy prescriptive and also one-sided because it implicitly promotes GWP* and explicitly disqualifies GWP. Please add the important information that GWP* is only superior to GWP for the short term situation. In the long-term however, the assumed equivalence of a steady rate of the SLCF with a pulse of CO2-emissions is not applicable any more, because the warming is dominated by LLCF. The Paris Agreement aims at limiting climate change in the long-term and hence, GWP* does not seem suitable for this objective. It might even lead to additional warming as found in Schleussner et al. (https://iopscience.iop.org/article/10.1088/1748-9326/ab56e7). Please revise this paragraph. [Nicole Wilke, Germany]	Rejected. The paragraph refers to a long series of scientifically well-established critiques of the customary metric, GWP. This science is well-established and uncontroversial. Newer step-pulse metrics do a better job of matching the temperature effects of a time-series of gases. This is emerging science since AR5, and draws on several studies. The science in these papers has not been challenged. We do not believe that the language we use is prescriptive. We believe the current text adequately reflects the available science.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98459	115	50	115	55	The chapter states that "It is clear that the traditional emission metric, GWP (100), gives the wrong sign of the contribution of SLCFs, including methane, to warming when emissions are declining, and this is a general property of pulse metrics", this require further justification. An emission pulse of methane persists in the atmosphere on average for 12 years, but a small fraction persists for longer. Methane is a powerful greenhouse gas, so an emission pulse results in significant warming over the first few decades, but this warming gradually decays again, with most of the warming gone within a century. As a result, emissions of short-lived greenhouse gases do not have to be reduced to zero to avoid additional warming, since the warming caused by ongoing emissions to a large extent simply maintains, rather than adds to, the warming caused by previous emissions. It takes about 50 years after the beginning of a constant rate of methane emissions for methane concentrations to stabilize. It then takes several hundreds of years for temperatures to stabilize in response to the increased methane concentrations, owing to both the inertia of the climate system and various feedbacks that further enhance the warming that comes from methane alone. Some of these feedbacks are unique to the chemical characteristics of methane, while others are common to all greenhouse gases. [nehzat Motallebi, United States of America]	Taken into account. We have rephrased this sentence to make the point more clear.
66859	115	50	116	1	GWP500 is included, but GWP20 is not; GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20
66861	115	50	116	1	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We are working with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68421	115	50	116	1	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. This is important because many feedbacks and tipping points are anticipated within the next 10 to 20 years, as the 1.5C guardrail is approached and likely breached. Masson-Delmotte V., et al. (eds.) (2018) SUMMARY FOR POLICYMAKERS, in IPCC (2018) GLOBAL WARMING OF 1.5 ºC; Lenton T. M., et al. (2019) Climate tipping points—too risky to bet against, NATURE, Comment, 575:592–595; Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259, 8254; and Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5784 GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate	Taken into account. We have now included an assessment of GWP20. We are working with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.
68423	115	50	116	1	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account. We have now included an assessment of GWP20. We are working with WGIII on emissions metrics. We anticipate some areas of commonality, and some enduring differences.
23595	115	50	116	4	More care is needed in this para to differentiate the effect of a pulse emission and that of a sustained time-series of emissions, and whether the metric measures climate change with or without an emission, or additional climate change compared to some reference level of climate change caused by previous emissions. Detailed comments follow. [Andy Reisinger, New Zealand]	Taken into account. We have included text to the effect that any cumulative emissions approach, including cumulative CO2 and GWP*, only reflects warming from when emissions are provided. Warming before that time is not included.
73927	115	50	116	4	Please add how and to what extent this message of distinction between short- and long-.lived forcing agents is relavant for the purposes and objectives of the Paris Agreement. The link to the Paris Agreement in the paragraph above implies that its is not very relevant. [Anke Herold, Germany]	Taken into account. It is very relevant. We have rephrased the sentence to make the point more clear.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
96775	115	50	116	5	The sentence "It is clear that the traditional emission metric, GWP(100), gives the wrong sign of the contribution of SLCFs, including methane, to warming when emissions are declining, and this is a general property of pulse metrics." is not justified since it ignores the fact that GWP is an integrated metric. Please remove this flawed and policy prescriptive comment. In addition, please explain why using multi-metric techniques in a regulatory setting that aims at limiting long term warming would be beneficial. The findings of Myhre et al are only relevant for the present and short term situation but do not apply for the long term. Please strongly revise this paragraph so that it provides a more objective assessment. [Nicole Wilke, Germany]	Taken into account. We have rephrased this sentence. We are pointing out that GWP implies warming from a declining flow of SLCFs, whereas the climate system (and GWP*) suggest cooling (i.e. a negative temperature trend). This is not prescriptive. It is descriptive.
103651	115	50	116	5	Please comment on the plausibility of using multi-metric techniques in a regulatory setting. Waiting until the end of a step-change (e.g. 50 years) seems impractical. And dividing a step change into annual emissions seems no different from a pulse metric. [Philippe Tulkens, Belgium]	Noted. We have raised this with WGIII - that is more in their domain.
123	115	50	116	12	These two paragraph contains only a single reference, Myhre, which is the corresponding chapter in AR5. Yet, the first para refers upfront to new critiques in the first sentence. Also, is the para based on a view, or assessment of post-AR5 literature? Please also add a confidence statement - is this based on many lines of argument, or just one? Some papers I have seen are Fuglestvedt et al 2018 (cited in my comment on l38-46); Carl-Friedrich, S, Alexander, N, Michiel, S, William, H & Joeri, R 2019. Inconsistencies when applying novel metrics for emissions accounting to the Paris Agreement. Environmental Research Letters 14: 124055; Rogelj, J & Schleussner, C-F 2019 Unintentional unfairness when applying new greenhouse gas emissions metrics at country level. Environmental Research Letters 14: 114039. https://doi.org/10.1088/1748-9326/ab4928 From reading the aforegoing, I already have found multiple arguments and not much agreement. To assess the full range of post-AR5 literature, it would seem appropriate to undertake a systematic literature search. Please read this comment together with my comment re p. 116, lines 16-18. [Harald Winkler, South Africa]	Taken into account. We have provided a stronger assessment of the literature. We are making two main physical points, neither of which is controversial. The first is that GWP100 does a poor job of simulating temperatures. We are providing several more references to show the long heritage of this point. The second is that step-pulse metrics do a much better job of surface temperatures (e.g. Allen et al., 2018, Cain et al., 2019, Collins et al., 2019, Lynch et al., 2020) and again the main point here has not been challenged, though the papers you cite do challenge some peripheral science aspects, and raise some issues regarding use of metrics. The papers you cite are themselves the subjects of rebuttals and responses. We aim to be clear about what is accepted (the two main points above) and what is contested (the appropriateness of GWP* under a restricted set of scenarios; distributional issues arising from the start date of emissions time series (which also arise with cumulative CO2 emissions)).
23597	115	51	115	52	The claim that GWP or GTP do not distinguish short- and long-lived forcing agents is misleading - all metrics are designed to recognise that gases do have different lifetimes (otherwise the design of metrics would be exceedingly simple). Also the issue discussed here only arises if applied to a time series of emissions. Suggest you rephease "... apply whenver a single pulse emission metric, which by design does not draw a distinction between the effect of short- and long-lived forcing agents on temperature outcomes over time, is applied to an extended time series of emissions." [Andy Reisinger, New Zealand]	Taken into account. We have rephrased - several readers have pointed out that lifetimes are folded into the integral.
76823	115	52	115	54	The following statement requires more explanation: “It is clear that the traditional emission metric, GWP(100), gives the wrong sign of the contribution of SLCFs, including methane, to warming when emissions are declining, and this is a general property of pulse metrics.” This indicates that it gives the wrong sign in the sense of the impact of changes from one year to the next but not of the absolute impact of a given years’ emissions. [Nathan Borgford-Parnell, Switzerland]	Taken into account. We have rephrased the sentence to make it clear that a declining flow of short-lived emissions represent a cooling, or a reduction on previous warming.
23599	115	52	115	54	More care is needed to explain the circumstances under which the 'wrong sign' occurs - it applies only for temperature relative to a (to be specified) reference level, and only when GWP100 is applied to an extended time series of non-CO2 emissions and emissions in that time series are declining (rapidly), and it is assumed that temperature outcomes from those emissions will be cumulative.The sign of GWP is never wrong when applied to a single emission - any emission of CH4 makes the climate warmer than it would be otherwise, hence the sign of GWP is, correctly, always positive. And the sign of a CGTP metric is always positive if it is used to understand the contribution to warming from a given time series of emissions relative to the warming in the absence of those emissions. CGTP can only be negative, and hence different in sign from GWP, if it uses a recent temperature reference level. These clarifications really matter to ensure the text is correct and unambiguous. Rephrase: "It is clear that the traditional emission metric, GWP(100), gives the wrong sign of the contribution of SLCFs, including methane, to warming relative to a given reference level when it is applied to an extended time series of emissions and those emissions are declining rapidly; this is a general property of pulse metrics since pulse metrics do not consider changes in future temperature due to past emissions." [Andy Reisinger, New Zealand]	Taken into account. We have rephrased the sentence to make it clear that a declining flow of short-lived emissions represent a cooling, or a reduction on previous warming.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
112443	115	52	115	54	Statements that the GWP of methane and other SLCFs "have the wrong sign when emissions are declining" are a gross over-generalization of observations from several papers studying the GWP* formulation, which is designed to be a useful means of estimating SLCF 'budgets' under global schemes designed to limit warming to a given level. At best, these sections will be extremely misleading for the vast majority of policymakers and other members of the AR6 audience. The plain language here is stating that in a context of declining emissions of an SLCF, that SLCF should have a negative climate metric - that is, while emissions of CO2 cause warming, emissions of the SLCF (in this context) would be COOLING. This is false. Under any climate scenario, the temperature of the earth will be warmer in the years after an SLCF is emitted, relative to the counterfactual where the SLCF is not emitted. Therefore, the SLCF certainly would not have a negative metric. The statement needs to be removed. [David McCabe, United States of America]	Taken into account. We have rephrased this sentence. We are pointing out that GWP implies warming from a declining flow of SLCFs, whereas the climate system (and GWP*) suggest cooling (i.e. a negative temperature trend). This is not an over-generalisation. The standard definition of "cooling" (e.g. the Oxford English Dictionary) refers to a negative temperature trend, which accurately describes the effect we were summarising. Nevertheless, we can be clearer that it is essentially undoing previous warming.
32103	115	52	115	54	"GWP(100) gives the wrong sign of the contribution of SLCFs, including methane, to warming when emissions are declining" This sentence might be rewritten a little. At first impression it gives the sense that it is bad to cut methane emissions. Likewise also see 113 line 25 "some emission metrics can fail to give the correct sign of contributions to warming under scenarios in which emissions decline". AR6 will be very widely read. The problem is that these lines might easily give one or two policymakers the impression that mitigating a short lived climate forcer might actually damage long term reduction efforts. I was adviser to the senior MDC leadership in Zimbabwe around the time of IPCC AR4, and was impressed by their commitment to environmental issues. Need to be careful to avoid phraseology that can be misconstrued by thoughtful but non-technical readers to suggest cutting emissions is a bad thing. In politics, when a metric becomes a target, it ceases to be a metric. [Euan G. Nisbet, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the text. "Wrong sign" meant that GWP100 gives a positive sign but the climate system gives a negative sign. The phrase was clear to many, but confused some, so we have rewritten it. We agree that it is important to be clear about how cutting emissions relates to climate consequences.
106369	115	52	115	54	This statement is imprecise and to some degree a strawman. It is not the metric which gives the wrong sign, but the use of the metric in a specific way. GWP(100) is designed to reflect the relative integrated radiative forcing of a pulse emission. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have revised the text. "Wrong sign" meant that GWP100 gives a positive sign but the climate system gives a negative sign. The phrase was clear to many, but confused some, so we have rewritten it. The sentence was accurate in terms of contributions to surface temperature over time, but you are also correct that if we view this year's emissions in isolation - i.e. ignore the time dimension and intergenerational aspects - then all GHG warm.
17845	115	52	115	54	Negative metrics for declining emissions of short-lived forcers only make sense when considering metrics within the framing of the GWP* (e.g., Cain et al. 2019). This concept has limited applicability: e.g., many (most?) users just care about how much the emissions of their facility/nation/natural-gas-vehicle today are contributing to future warming of the planet. The GWP* is great in cases where metrics are being used to approximate a simple climate model, but using that framing to claim that the GWP100 "gives the wrong sign of the contribution of SLCFs, including methane, to warming when emissions are declining" is going to mislead and confuse. Emitting a pulse of a greenhouse gas, compared to not emitting it, leads to warming. [Marcus Sarofim, United States of America]	Taken into account. We have revised the text. "Wrong sign" meant that GWP100 gives a positive sign but the climate system gives a negative sign. The phrase was clear to many, but confused some, so we have rewritten it. The sentence was accurate in terms of contributions to surface temperature over time, but you are also correct that if we view this year's emissions in isolation - i.e. ignore the time dimension and intergenerational aspects - then all GHG warm.
32951	115	52	115	54	Here the text says "It is clear that the traditional emission metric, GWP(100), gives the wrong sign of the contribution of SLCFs, including methane, to warming when emissions are declining, and this is a general property of pulse metrics." I think this needs more explanation as to when this is the case, which depends upon the endpoint one is looking at. This metric will indeed give the 'wrong sign' in the sense of the impact of changes from one year to the next but not of the absolute impact of a given years' emissions, so if one is looking at say the carbon footprint of a person/nation/action, the GWP doesn't give the wrong sign in any way I can see. [Drew Shindell, United States of America]	Taken into account. We have revised the text. "Wrong sign" meant that GWP100 gives a positive sign but the climate system gives a negative sign. The phrase was clear to many, but confused some, so we have rewriting it. The sentence was accurate in terms of contributions to surface temperature over time, but you are also correct that if we view this year's emissions in isolation - i.e. ignore the time dimension and intergenerational aspects - then all GHG warm.
51419	115	52	115	54	This chapter mentions the new metric GWP* briefly. It would be useful to provide more information on what the implications of use of this metric are for reductions in methane emissions. On one hand, some have used this metric to argue that methane emissions don't need to be decreased, which appears inconsistent with the findings of the SR1.5. On the other hand, it could emphasise the need for increased methane emissions reductions. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Yes, in revising we have attempted to make the consistency with SR1.5 clearer.
114653	115	52	115	54	I suggest some more explanation is given for this statement about wrong sign. If a pulse is simply multiplied by 28 it has the same sign, but I think you mean when CH4 is "transferred" to CO2-equivalents which are then given as cumulative emissions. Then the very different behaviou of CH4 and CO2 os not taken into accoint. I suggest you add more explanations here. [Jan Fuglestvedt, Norway]	Taken into account. We have revised the text. "Wrong sign" meant that GWP100 gives a positive sign but the climate system gives a negative sign. The phrase was clear to many, but confused some, so we have rewritten it.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
83709	115	52	115	54	For the sentence: "It is clear that the traditional emission metric, GWP(100), gives the wrong sign of the contribution of SLCFs, including methane, to warming when emissions are declining, and this is a general property of pulse metrics" please clarify what is meant by the "wrong sign", and refer to specific papers in making this assessment. [Dan Zwartz, New Zealand]	Taken into account. We have revised the text. "Wrong sign" meant that GWP100 gives a positive sign but the climate system gives a negative sign. The phrase was clear to many, but confused some, so we have rewritten it.
31795	115	52	116	52	Is it more precise to say that use of GWP to calculate CO2 equivalence gives the wrong sign? [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have rephrased the sentence to make it clear that declining emissions represent a cooling, or a reduction on previous warming.
16005	115	53	115	54	More generally, GWP(100) is the wrong metric when concentrations of short lived greenhouse gases are either rising of falling, and when there are critical points of irreversibility within the 100 year time frame. [Kevin Lister, United Kingdom (of Great Britain and Northern Ireland)]	Noted. For surface temperature, yes.
51421	115	53	115	54	There is a fundamental question around the choice of baseline against which a change of emissions is being considered. If, for example, pre-industrial is taken to be the start date, then can SLCF emissions still be considered to be declining, and hence, is it a cooling or is it actually less warming? If a baseline of 2010 were taken, then a downward change in decrease in methane emissions could suggest a cooling but it would neglect the impact up to that point. Suggest that this is expanded to include a discussion of this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. This is not really accurate. Like cumulative CO2 (and other LLCF) GWP* gives the warming from when the emissions time-series are first provided. Neither give warming from before that time.
73939	115	54	116	1	There does not seem to be one metric that combines step-changes in short-lived forcing with pulse of long-lived gases, but only the option to use different metrics for different types of gases/ substances, hence it is unclear to what 'new emission metrics' refer here. It would be very important that the application of multi-metric metrics for policy purposes would be explained in a better wa<. E.g. How would impacts on mitigation strategies on emissions sources that emit long and short-lived substances be calculated, e.g. fossil combustion plants emitting CO2, CH4 and N2O? Practical examples are necessary to understand how such approaches could be implemented in practice. At the scenario level, gases can be separated rather easily, but policies target emission sources that emit short and long-lived gases and the benefit of using different metrics concepts in such cases are unclear. It would be also important to outline how mutli-metric concepts should be implemented in WG3. In addition a discussion on the change of metric values over time, in particular in coming closer to peak emission levels is missing for the metric concepts presented, because this is also a very relevant aspect for policy makers. If metric values would need to be updated rather drastically in time horizons in which NDCs or policies are implemented, this would be a considerable difficulty for any mitigation strategies and would also need to be reflected in WG3 discussions. [Anke Herold, Germany]	Noted. Step-pulse metrics such as GWP* allow the combination of step-changes in SLCFs with pulses of LLCFs. The WGI assessment focuses on the physical science aspects of recent publications on emissions metrics. WGIII will cover other aspects and other perspectives.
73929	115	54	116	4	Please delete the sentence "In response to the fact that GWP does not under most scenarios do a good job..." This statement is contradicting the other statements that the choice of each metric is related to value choices and policy goals. This sentence implies that there is generally a scientifically better metric than GWP which is misleading. The additional considerations of metrics to other impacts such as sea-level rise in this chapter, seem to imply that a metric representing the tenparature effects may not 'do a good job' in representing impacts on sea level rise or other impacts. Previous statements of IPCC authors have been that GWP does a reasonable good job of representing temperature effects. 'Doing a good job' is certainly not a scenitifc way of characterizing the issue. In this chapter it is explained that effective radiative forcing is the key driver for surface tempertaure change and key scenarios in chapter 4 are based on radiative forcing and derive impacts on temperature from this basis. The implication that GWP which is strictly related to radiative focing does"not do a good job" of representing temperature impacts, seems to imply that the general choice of radiative forcing chosen in AR6i for the key scenarios does not do a good job in representing temperature effects either. How can a concept of radiative focring be correct as a general driver of temperature change, but inappropriate for the choice of metrics? It may be an appropriate metric because it seems to be more consistent with IPCC scenarios than any other approach while metric approaches targeting only one single cliamte impact such as tempertaure and not other impacts such as SLR may lead to other problems where these metrics do not 'do a good job'. It seems biased in the way how arguments are put together against GWPs which are not in the same way assessed in relation to other metrics. [Anke Herold, Germany]	Taken into account. We have rephrased, but readers should know that GWP does not adequately simulate contributions to warming, and newer step-pulse metrics do.
23601	116	1	116	4	insert "sustained but rapidly declining" before "emissions" in line 2, since otherwise the statement is wrong because too broad - it only applies GWP100 is applied to an extended time series of declining emissions. [Andy Reisinger, New Zealand]	Taken into account. We have edited the text for clarity and precision.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
17847	116	1	116	4	While it is true that Myhre et al. critiqued the name "global warming potential", I think that this criticism is overblown. After all, in theory a GWP is equivalent to an iGTP - so, a GWP is like an integrated temperature metric, and integrated global temperature change and "warming" have a pretty close relationship. Just like I don't think the IPCC needs to point out that GHGs don't act exactly like a greenhouse despite their name, I don't think the IPCC needs to be getting into whether the GWP is a perfect name. [Marcus Sarofim, United States of America]	Rejected. We think the issue of "warming" here is relevant, so we have retained it.
100469	116	2	116	2	(2013) -> (2013b) [Øivind Hodnebrog, Norway]	Accepted
52005	116	6	116	8	Alternative metrics are referred to here, but none are cited. Please cite which metrics have been evaluated by the authors as 'giving a more faithful simulation of the temperature effectas of a portfolio of gases'. It may be that if temperature is the [Michelle Cain, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have edited the text for clarity and precision.
23603	116	7	116	9	Again here, more care and precision is needed to ensure the sentence is correct and not too broad. Specificaly, make clearer that the porfolio of emissions has to occur over an extended period of time, and it is representing the CHANGE in temperature, not necessarily contribution to climate change compared to the absence of those emissions (depending on how the reference level for the change in the rate of SLCF emissions is set when using a CGTP metric). Suggested re-phrasing: "Some of these give a more faithful simulation of the effects on changes in temperature resulting from emitting a portfolio of gases over an extended time period, especially under mitigation scenarios, such as those implied by successful attainment of the temperature goals set out in Article 2 of the Paris Agreement. " [Andy Reisinger, New Zealand]	Taken into account. We have rephrased. We want readers to be clear that step-pulse metrics represent a better way to make this comparison where the variable of interest is temperature.
64769	116	9	116	12	The text states that "it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice," yet there are no options provided for metric users interested in near-term metrics and short-term time horizons. By not providing users with near-term metrics, the IPCC is therefore inherently making a policy decision because the community is left with no option to make this value judgement for themselves – despite the IPCC saying that it is up to them to do so. [Steven Hamburg, United States of America]	Taken into account. We have now included an assessment of GWP20. We have liaised with WGIII, who will have their own section on metrics.
68129	116	9	116	12	The text states that it is a matter for policy-makers to decide which emission metric to use because they have the social license to make the normative judgements regarding timescale, but there are no options provided for metric users interested in near-term metrics and short-term time horizons. By not providing users with near-term metrics, the IPCC is therefore inherently making a policy decision because the community is left with no option to make this value judgement for themselves – despite the IPCC saying that it is up to them to do so. [Ilissa Ocko, United States of America]	Taken into account. We have now included an assessment of GWP20. We have liaised with WGIII, who will have their own section on metrics.
69883	116	9	116	12	"it is a matter for policy-makers to decide which emission metric to use, because they have the social license to make the normative judgements regarding timescale, variable choice and functional form that underpin emission metric choice. Physical science can only form a subset of the inputs to those choices." I would argue that the authors of chapter 7 by pushing GWP* and eliminating established metrics are doing the exact opposite. This isn't to say that scientists can't introduce GWP* as another approach, but it is inconsistent to "force" this metric on policymakers. [Gabrielle Dreyfus, United States of America]	Noted. We use no prescriptive language around GWP*. Step-pulse metrics represent a better way to make this comparison where the variable of interest is temperature. This is policy-relevant physical science, which is why it receives prominence here. We have now included an assessment of GWP20.
114655	116	14	116	14	I suggest deleting "in emission scenarios" since for this models are often used, and the contributions are caluated. [Jan Fuglestvedt, Norway]	Taken into account. Thanks for this point. We agree, and have rewritten the sentence.
17851	116	14	116	16	I'm a bit confused by the first sentence of this summary paragraph. I'm not sure "quantification" is what is being improved by specifying short-lived and long-lived gases separately. It is true that providing a the GWP-equivalent time course of future global emissions does not uniquely identify a future forcing scenario because warming could be shifted earlier or later by changing the balance of shorter-lived and longer-lived gases - is that what the sentence is trying to say? [Marcus Sarofim, United States of America]	Taken into account. Thanks for this point. We agree, and have rewritten the sentence.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
125	116	14	116	18	What is the basis of the two statements of "high confidence", and are these confidence statement derived in a manner consistent with WGI guidance on uncertainty? The statements seem more like WGIII confidence statements; if so, that should be made clear; but still would have to show how they are supported by evidence and agreement. The first statement lacks any reference to spatial or temporal scale. Do new metrics really provide "more equivalence" in surface temperature response in the near-term and at sectoral or national level? In the present chapter, p12 l 52-3 says explicitly that "TCRE can also be related to the global warming potential (GWP) emission metric covered in Section 7.6", and relationship between the GWP common metric and transient climate response should be consistently shown, also here. The second statement, that new metrics "can lead" to better equivalence, seems not suited to assessing confidence - as it is only a possible outcome, so this is confidence is a possible outcome? For both, it seems to me that the summary omits key points - a clear statement on which policy questions GWP* is useful, and which not (e.g. is GWP* relevant to national mitigation targets, such as those in NDCs). It is also silent on the equity implications, on which there is an emerging literature (e.g. Rogelj and Schleussner 2019). The review editors for this chapter should ensure that a full range of literature is assessed, and that confidence statements are formulated consistent with the appropriate guidance. [Harald Winkler, South Africa]	Noted. We are providing a document that makes clear the basis for our use of the confidence language. Multiple papers and lines of evidence underpin each statement, and in many cases these have been scientifically well-established for decades.
83157	116	14	116	18	This is very close to a recommendation for using the new metrics. I agree that used as simple climate models to compare temperature impacts for KNOWN emissions scenarios (emissions for all future time periods) they are much better than the GWP,GTP. However, for reporting and committing to mitigations for the 5-year NDC cycles of the Paris agreement, I believe they are less well suited (cf comments above). I recommend to add a sentence with a warning that there are challanges as to how these metrics can be used to develop dynamic and uncertain climate policies. [Terje Berntsen, Norway]	Taken into account. Thank you for this point. We have not really focused on the 5-year reporting cycles - that's probably more a WGIII thing. The main points we are making concern how different forcings affect temperatures, and how different emissions metrics capture this (or fail to capture it). The text has been revised.
44333	116	14	116	20	Could CO2-forcing-equivalence be mentioned as a useful marker for the physical representativeness of all GHG metrics? It seems rather like the concept of CO2-fe has been overlooked in this entire section. [Stuart Jenkins, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text revised
66863	116	14	116	20	GWP500 is included, but GWP20 is not; GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and drastic mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-lived climate pollutants (black carbon, methane, tropospheric ozone, and HFCs). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We have liaised with WGIII, who will have their own section on metrics.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66865	116	14	116	20	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescales like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO2 forcers in comparison with CO2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO2 and non-CO2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO2 becomes in comparison with a SCLF [sic]."). [Kristin Campbell, United States of America]	Taken into account. We have now included an assessment of GWP20. We have liaised with WGIII, who will have their own section on metrics.
68425	116	14	116	20	GWP500 is included, but GWP20 is not. Given the short lifetimes of SLCFs, a shorter timescale than 50 or 100 years—specifically using a metric of GWP20—would provide a better understanding of the near-term warming from SLCPs. For policymakers, changes in the near-term and creating policies that are in line with the lower emissions scenarios would benefit from the ability to emphasize the amount of avoided warming from the SLCPs and the near-immediate impact that they can have, which is aided by having the appropriate metric in GWP20. See Climate and Clean Air Coalition (CCAC) , Mexico , Molina Center for Energy and the Environment (MCE2), & United Nations Environment Programme (UNEP) (2018) Progress and Opportunities for Reducing SLCPs across Latin America and the Caribbean; UNEP & Climate and Clean Air Coalition (2018) Integrated Assessment of Short-lived Climate Pollutants in Latin America and the Caribbean: Improving air quality while contributing to climate change mitigation; Climate and Clean Air Coalition & UNEP (2019) Air Pollution in Asia and the Pacific: Science-based solutions; European Environment Agency (2018) Air quality in Europe — 2018 report, EEA Report No 12/2018. This is important because many feedbacks and tipping points are anticipated within the next 10 to 20 years, as the 1.5C guardrail is approached and likely breached. Masson-Delmotte V., et al. (eds.) (2018) SUMMARY FOR POLICYMAKERS, in IPCC (2018) GLOBAL WARMING OF 1.5 ºC; Lenton T. M., et al. (2019) Climate tipping points—too risky to bet against, NATURE, Comment, 575:592–595; Steffen W., et al. (2018) Trajectories of the Earth System in the Anthropocene, PROC. NAT'L. ACAD. SCI. 115(33):8252–8259, 8254; and Drijfhout S., et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models, PROC. NAT'L. ACAD. SCI. 112(43):E5777–E5786, E5784. GWP20 is a far more useful metric for policymaking because people have a greater connection to the near-term and what they could face in their own lifetime; GWP500 is perhaps most helpful for something like SLR, but the timeframe of 500 years is far outside the scope of policy developments that may be happening in response to the current climate crisis. Speed is the metric of concern because of our proximity to 1.5C and aggressive mitigation efforts needed to meet that goal. As a result, policymakers that will rely on the IPCC's scientific expertise would greatly benefit from the access and analysis of climate metrics that consider the shorter timescales like GWP20, which was used in past assessments and throughout policy work. SLCFs are featured in Chapter 6 of this report, but their impact on the climate—especially in the crucial near-term—should not be relegated to only that chapter but instead considered as part of the whole, most importantly short-	Taken into account. We have now included an assessment of GWP20. We have liaised with WGIII, who will have their own section on metrics.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68427	116	14	116	20	GWP* being used throughout the AR6 Report can be a useful metric, but does not completely negate the need and utility of a metric for a shorter timescale like GWP20. In the IPCC 1.5C Report, GWP* is noted for its ability to describe the impacts from SLCFs, even providing a Figure in Cross-Chapter Box 2 that shows the differences between GWP100, GTP100, and GWP*. This does not help for shorter timescale concerns. In the First Order Draft for WGIII for AR6, GWP* is explained in Chapter 2 as allowing the comparison of a sustained change in emissions for non-CO_2 forcers in comparison with CO_2, but the chapter also notes that there are limitations to using GWP* for policy applications, including those relevant for the Paris Agreement (see WGIII FOD 2-23–2-24). Further, Chapter 2 does suggest that GWP20 may be useful alongside metrics like GWP100 and GTP100 to compare changes in emissions (WGIII FOD 2-22). In Chapter 6 of WGIII FOD, the authors note that a chosen climate metric and the time horizon for which it covers affect assessing the timing of achieving climate targets like net-zero emissions (WGIII FOD 6-100). In discussing the balance of CO_2 and non-CO_2 emissions from aviation, Chapter 10 of WGIII's FOD suggests that time horizon is a subjective choice of the whomever is using the information, and that if longer time horizons are chosen, CO_2 becomes more important (WGIII FOD 10-51: "Any GWP/GTP type emissions equivalency calculation always involves the user selection of a time horizon, over which the calculation is made, which is a subjective choice (Fuglestvedt et al., 2010). In general, the longer the time horizon, the more important CO_2 becomes in comparison with a SCLF [sic]."). [Durwood Zaelke, United States of America]	Taken into account. We have now included an assessment of GWP20. We have liaised with WGIII, who will have their own section on metrics.
68429	116	14	116	20	Both warming and cooling SLCFs are emitted alongside CO_2, and as CO_2 is reduced through efficiency and clean energy, there will be warming in the near-term from reduction in sulfates ("global brightening"). Xu Y. & Ramanathan V. (2017) Well below 2 ºC: Mitigation strategies for avoiding dangerous to catastrophic climate changes, PROC. NAT'L. ACAD. SCI. 114(39):10315–10323 ("Another complexity of the coemission issue is that a major part of the cooling aerosols (mostly sulfates and nitrates) is also coemitted by CO_2-dedicated measures. Hence, the CO_2 measures implemented in 2020 will unmask some of the aerosol cooling (red lines in SI Ap- pendix, Fig. S5) and offset the warming reduction by CO_2 and SLCP mitigation. In the baseline scenarios of this study, the cooling aerosols are regulated gradually between 2020 and 2100 (SI Appendix, Fig. S6), whereas in the mitigation scenario examined here, CO_2 mitigation is implemented starting from 2020 and CO_2 emission is brought to net zero in about three decades (SI Appendix, Fig. S2B). As a result, the unmasking of coemitted aerosol cooling (a net warming effect) is more rapid in the decreasing CO_2 emissions beginning in 2020 (CN2020) mitigation scenario (SI Appendix, Fig. S5B vs. S7)."); Ramanathan V. & Feng Y. (2008) On avoiding dangerous anthropogenic interference with the climate system: Formidable challenges ahead, PROC. NAT'L. ACAD. SCI. 105(38):14245–14250, 14245 ("The observed increase in the concentration of greenhouse gases (GHGs) since the preindustrial era has most likely committed the world to a warming of 2.4ºC (1.4ºC to 4.3ºC) above the preindustrial surface temperatures. …The estimated warming of 2.4ºC is the equilibrium warming above preindustrial temperatures that the world will observe even if GHG concentrations are held fixed at their 2005 concentration levels but without any other anthropogenic forcing such as the cooling effect of aerosols. …IPCC models suggest that ≈25% (0.6ºC) of the committed warming has been realized as of now. About 90% or more of the rest of the committed warming of 1.6ºC will unfold during the 21st century, determined by the rate of the unmasking of the aerosol cooling effect by air pollution abatement laws and by the rate of release of the GHGs-forcing stored in the oceans. The accompanying sea-level rise can continue for more than several centuries."); see also Ramanathan V. & Xu Y. (2010) The Copenhagen Accord for limiting global warming: criteria, constraints, and available avenues, PROC. NAT'L. ACAD. SCI. 107(18):8055–8062, 8056, Box 2 Figure ("CO_2 (1.65 Wm^{-2}) and the non-CO_2 GHGs (1.35 Wm^{-2}) have added 3 (range: 2.6–3.5) Wm^{-2} of radiant energy since preindustrial times. The non-CO_2 GHGs are methane (CH_4); nitrous oxide (N_2O); and halocarbons (HCs), which include CFCs, HCFCs, HFCs; and ozone	Noted. Aerosols are dealt with in Chapter 6, and their role in climate sensitivity earlier in this chapter.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68431	116	14	116	20	However, targeting SLCPs and reducing them quickly can result in near-term avoided warming, which is critical to slowing feedbacks and avoiding tipping points. There are strategies to specifically target SLCPs that will provide further benefits than what comes from SLCPs that are co-emitted with CO2. See Shindell D., et al. (2012) Simultaneously Mitigating Near-Term Climate Change and Improving Human Health and Food Security, Science 335:183–189, 183–184 ("Tropospheric ozone and black carbon (BC) contribute to both degraded air quality and global warming. We considered ~400 emission control measures to reduce these pollutants by using current technology and experience. We identified 14 measures targeting methane and BC emissions that reduce projected global mean warming ~0.5°C by 2050. This strategy avoids 0.7 to 4.7 million annual premature deaths from outdoor air pollution and increases annual crop yields by 30 to 135 million metric tons due to ozone reductions in 2030 and beyond. Benefits of methane emissions reductions are valued at $700 to $5000 per metric ton, which is well above typical marginal abatement costs (less than $250). The selected controls target different sources and influence climate on shorter time scales than those of carbon dioxide–reduction measures. Implementing both substantially reduces the risks of crossing the 2°C threshold. …The short atmospheric lifetime of these species allows a rapid climate response to emissions reductions. In contrast, CO2 has a very long atmospheric lifetime (hence, growing CO2 emissions will affect climate for centuries), so that the CO2 emissions reductions analyzed here hardly affect temperatures before 2040. The combination of CH4 and BC measures along with substantial CO2 emissions reductions [a 450 parts per million (ppm) scenario] has a high probability of limiting global mean warming to <2°C during the next 60 years, something that neither set of emissions reductions achieves on its own [which is consistent with (19)]."); UNEP & WMO (2011) Integrated Assessment of Black Carbon and Tropospheric Ozone; Xu and Ramanathan (2017) Well below 2 °C: Mitigation strategies for avoiding dangerous to catastrophic climate changes, Proc. Natl. Acad. Sci. 114(39):10315–10323 ("The mitigation of the coemitted SLCPs and cooling aerosols by CO2-dedicated measures requires special consideration (33). SLCP emissions are not entirely independent of CO2 emissions, and emission rates of SLCPs can decrease due to CO2 mitigation, and likewise CO2 emissions can decrease due to mitigation of SLCPs. The role of coemitted SLCPs that are dependent on CO2 is estimated in SI Appendix, Fig. S5. A fraction of CH4 (about 70%) and BC (about 30%) emissions can be mitigated through CO2-dedicated measures. While HFCs are not dependent on CO2 mitigation, CO2-	Noted. The issue of co-benefits will be dealt with in WGIII. Our aim is to assess recent research in emissions metrics from a physical science perspective.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
68433	116	14	116	20	Even though SLCPs avoid warming quickly (days to about a decade and a half), SLCP mitigation can have lasting benefits in 2100 and even 2200, plus avoids irreversible harm from sea-level rise. Shoemaker J. K., et al. (2013) What Role for Short-Lived Climate Pollutants in Mitigation Policy?, SCIENCE 342:1323–1324, 1323–1324 (“Direct comparisons of the climate influence of SLCPs and CO2 require making a judgment about the relative importance of short and long time scales. SLCPs have a powerful impact on climate, but they persist in the atmosphere for only a short time—days to weeks for BC, a decade for CH4, and about 15 years for some HFCs. Thus, immediate reductions in SLCPs will result in relatively immediate climate benefits, as the effects on climate depend largely on the emission rate, or flow, of SLCPs to the atmosphere. …It is also important to recognize that CO2 and SLCP emissions are not independent. Some of the steps to reduce CO2 emissions will drive down emissions of SLCPs, as some of the largest sources of BC and methane are associated with fossil fuel production and combustion.”); see also Shoemaker J. K., et al. (2013) What Role for Short-Lived Climate Pollutants in Mitigation Policy?, SCIENCE 342:1323–1324, Figure (“Climate temperature response to reductions in emissions of CO2, SLCPs, or both. Based on scenarios detailed in the supplemental material. Temperature change is shown relative to a pre-industrial baseline. In the Reference scenario, annual CO2 emissions peak in 2080, after which they decline rapidly, while SLCP (CH4, BC) emissions remain at or above current levels. In the “SLCP mitigation” scenario, deep cuts in BC (80%) and CH4 (40%) emissions, relative to 2010 levels, are implemented linearly from 2010 to 2050. In the “CO2 mitigation” scenario, CO2 emissions are reduced by 20% relative to the reference scenario by 2050, followed by slowly decreasing emissions that intercept the reference scenario emissions at 2150. In this scenario, emissions of both BC and CH4 are partially decreased relative to the reference scenario owing to those sources associated with fossil fuel consumption. The “HCM” scenario includes simultaneous mitigation of CO2, CH4, and BC, as described above. For simplicity we ignore HFCs as well as different sulfate aerosol trajectories. Including these would slightly change the shape of the curves, but not the relative time scales between them.”); Hu A., et al. (2013) Mitigation of short-lived climate pollutants slows sea-level rise, NATURE CLIMATE CHANGE 3:730–734, 730 (“Our results show that SLCP mitigation can have significant effects on SLR. It can decrease the SLR rate by 24–50% and reduce the cumulative SLR by 22–42% by 2100. If the SLCP mitigation is delayed by 25 years, the warming from pre-industrial temperature exceeds 2 ºC by 2050 and the impact of mitigation actions on SLR is reduced by	Noted. See also Pierrehumbert, 2014, https://www.annualreviews.org/doi/abs/10.1146/annurev-earth-060313-054843. “Eventual mitigation of SLCP can make a useful contribution to climate protection, but there is little to be gained by implementing SLCP mitigation before stringent carbon dioxide controls are in place and have caused annual emissions to approach zero. Any earlier implementation of SLCP mitigation that substitutes to any significant extent for carbon dioxide mitigation will lead to a climate irreversibly warmer than will a strategy with delayed SLCP mitigation. SLCP mitigation does not buy time for implementation of stringent controls on CO2 emissions.”
106365	116	14	116	20	The confidence statements in this paragraph provide no line of sight of how the authors reached their confidence assessment. This is inadequate and should be revised. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We are providing a document that makes clear the basis for our use of the confidence language. Multiple papers and lines of evidence underpin each statement, and in many cases these have been scientifically well-established for decades.
17849	116	14	116	20	The first sentence of this final summary paragraph is about specifying long-lived and short-lived forcers in emission scenarios. The second sentence of this final summary paragraph is basically noting how the GWP* (Cain et al. 2019) provides a metric that enables projection of future temperature response over time in a way that previous metrics (GWP, GTP, etc.) did not. The third sentence addresses the most-used metric of all (the GWP), but only to talk about how it has been updated since AR5. While it is interesting to think about alternative specifications of metrics (whether separate baskets, or pairing pulses of long-lived gases with sustained changes of short-lived gases), it would be worthwhile for the IPCC to engage more with the most-used metric (the GWP, and specifically, the GWP100). Here, two papers (Sarofim and Giordano 2018, Mallapragada and Mignone 2020) have found that the GWP100 is roughly equivalent, using damage metrics, with a 3% discount rate. That is a policy-relevant comparison that would be worth highlighting. [Marcus Sarofim, United States of America]	Noted. WGIII is also assessing metrics, and will refer to the damage side of things. We have aimed to present a physically-based assessment, and discuss the gases' roles in temperature change, since this is a new development in emissions metrics work.
89431	116	14			Metrics are not only about the immediate surface warming response, but also about appropriate accounting towards achieving long-term objectives, such as net-zero GHGs. Furthermore, applicable metrics require full comparibility of emissions where one tonne of a unit emissions, regardless of which gas or emitter is assessed equally. [Carl-Friedrich Schleussner, Germany]	Taken into account. We agree metrics are not only about surface warming. We have made that point in two places in the text. We disagree with the insistence that metrics must be pulse metrics. That may be a requirement for some users, but it is not a requirement for all.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
86805	116	16	116	18	"New metrics comparing pulse emissions of long lived greenhouse gases with sustained emission changes in short-lived gases can lead to more equivalence in surface temperature response." We anticipate that GWP* and combined-GTP are one of these new metrics. Please make clear in the chapter why this new and it seems, better metric in terms of determining the surface temperature respons, is not included in table 7.15. We would also appreciate to read about how the new metric influence the evaluation of measures countries needs to undertake in order to meet mitigation goals. In brief, both merrits and especially drawbacks with GWP* and combined-GTP should be described. We find the description regarding metrics in general quite unbalanced with a too large and favourable description of new approaches compared to the metrics that are currently in use under the convention GWP(100). Please include GWP 20 in the metrics table as has been provided in earlier IPCC reports. [Oyvind Christophersen, Norway]	Taken into account. We have now included GWP20 in the assessment. We have put the material on what the new metrics do better, and what they do not capture, earlier in the section, and summarise here.
40983	117	0			The logical flow of the FAQ is hard to follow at times. [TSU WGI, France]	Taken into account. The FAQ7.1 text has been revised accordingly.
41013	117	0			The summary of FAQ7.1 is very clear! [TSU WGI, France]	Noted. Thanks!
40807	117	0			sometimes the text appears technical/jargony to a lay audience, could you simplify the language? (e.g. L44-45 "marine boundary layer") [TSU WGI, France]	Taken into account. The FAQ7.1 text has been revised accordingly.
41109	117	0			To understand better the point of the FAQ, I can think of 2 options. 1) you would need to explain sooner in the main text why we should care about clouds in the context of climate change (i.e. what was the problem in AR5 / that it's a major source of uncertainty in climate models) and insist more on what was known what wasn't at the time of AR5 and what has been improved OR 2) You could change the title of the FAQ to focus more on the link between clouds and climate (change). At the moment it's a bit of both and it's a little bit confusing. [TSU WGI, France]	Taken into account. We have substantially rewritten the text and now explained in the beginning paragraph why we need to care about clouds under global warming.
40897	117	0			The 3 aspects presented in the figure are not as clear in the text I find [TSU WGI, France]	Taken into account. The FAQ7.1 text has been revised accordingly.
40675	117	0			references to sections should be removed from the FAQs [TSU WGI, France]	Accepted.
40935	117	0			the FAQ is a bit too long it should be 650-750 words long [TSU WGI, France]	Taken into account. The FAQ7.1 text has been revised accordingly.
37189	117	1	117	24	This FAQ should point out that total cloud cover decreased from 1985 to 1997 and thereafter there was a decrease in low level cloud that was taken up by mid and upper level cloud. See McLean (2014) "Late Twentieth-Century Warming and Variations in Cloud Cover". Warming can therefore be atributed to these changes in cloud cover. [John McLean, Australia]	Rejected. The purpose of FAQ is to explain the radiative feedback of cloud responses to warming based on processes, so discussing the bulk change in total cloud cover is not relevant.
38761	117	3	117	3	The audience of IPCC FAQs might not be able to recall what AR5 is and when it hs been published (other time stamps used in the text is "over the last four decades" in line 20, "have known for decades" in line 25 and "over the last century" in line 30 - so there is room for confusion because AR5 has not been published that long ago). They might not be able either to immediately understand the importance of clouds in the climate system (and why the recent progress is crucial). So my suggestion to rephrase the question of this important FAQ would be: "Why will changes in clouds amplify global warming?" or "...in the future charactertistics of clouds..." or "...in the altitude, amount and composition of clouds..." [Maike Nicolai, Germany]	Taken into account. The FAQ title has been revised.
16253	117	3	118	13	Maybe I'm being too critical but this FAQ title says it is about what is new, but that's only really covered in the penultimate paragraph. At least 80% of the FAQ is background/review material that would have been found in (for example) the AR5 FAQ on this same topic (which did actually try to explain the main cloud feedbacks). I was expecting a more informative summary of what new we've learned, but found even the penultimate paragraph to be a bit bland, not very informative, and giving the impression that most of the advances are from more careful analysis of models. At least superficially, this seems to contrast with the message from the ECS section that non-GCM information has enabled us to narrow the ECS range compared to the CMIP6 spread. [Steven Sherwood, Australia]	Taken into account. We have revised the text so that readers understand non-GCM information contributed to improve our understanding of the cloud feedback. However, the primary purpose of FAQ is to explain how changes in cloud are important for understanding climate change to lay audience but not to experts. Therefore, it is inevitable that some text may read trivial for experts (but will be still useful for non-experts).
96777	117	3	118	24	FAQ7.1 concludes that the understanding of clouds has much improved since AR5. However, Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity states that the reason that some of the CMIP6 models are too sensitive to GHG and hence too warm "can be traced to changes in extra-tropical cloud feedbacks that have emerged from efforts to reduce biases in these clouds compared to satellite observations" (TS-78-44). This statement is not fully consistent with the conclusion of FAQ7.1 on the improved understanding of clouds. Please add the issue of cloud representation in models to this FAQ and revise the conclusion accordingly. It can be expected that the high estimates of the ECS and global warming in CMIP6 will cause significant discussion and answers will be needed. This FAQ should provide some of these. [Nicole Wilke, Germany]	Taken into account. The reason some CMIP6 models have higher climate sensitivity can be traced to improved representation of extratropical clouds, but at the same time some of these high-sensitivity models struggle with reproducing past climate states (FAQ7.3). There is no contradiction between these two findings. Furthermore, a reduced bias in present-day cloud properties compared with observations does not automatically translate into a more correct cloud response to warming (and thus more correct cloud feedbacks).

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
93103	117	7	117	7	'significant', is this true? The spread on cloud feedbacks is still large. [Claudia Stubenrauch, France]	Noted. It is. The likely range of the net cloud feedback is much narrower than AR5.
96779	117	8	117	8	FAQ 7.1: In the summary, it is stated that the clouds will very likely change such that they amplify global warming. This is not sufficiently clearly represented in the following text. The different sections describe different effects of clouds (e.g., page 118 line 4-5 states the positive cloud feedback). But we are missing a summary statement in the full text that supports the statement in the summary at the top. [Nicole Wilke, Germany]	Taken into account. The FAQ7.1 text has been revised accordingly.
40995	117	10		13	The overall effect of clouds should be re-stated in the conclusion [TSU WGI, France]	Accepted. Done.
41089	117	11		15	this introduction paragraph could be shortened as a lot of information is not really relevant for the rest of the FAQ [TSU WGI, France]	Taken into account. The first two paragraphs have been merged with reducing information.
41073	117	17		34	These two paragraphs could be merged and shortened to save some space. [TSU WGI, France]	Accepted.
28907	117	18			Great FAQ! Possible modification here to "Clouds shade the surface by reflecting some of the incoming sunlight, which has a cooling effect. But clouds also trap and reduce the outgoing radiation emitted from the surface, resulting in a warming greenhouse effect." (clouds are not water vapour which is a gas) [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Thanks! The text has been revised accordingly.
703	117	19	117	19	Cloud greenhouse effect is stated to come from water vapor but this is not the case: it comes from the infrared absorption crossection of water and ice particles. Needs corrected text. [Bruce Wielicki, United States of America]	Taken into account. corrected.
38763	117	19	117	20	Into which direction is the outgoing radiation re-emitted? Into space or towards the Earth? Please clarify to avoid misinterpretation. [Maike Nicolai, Germany]	Taken into account. It is clear that the warming effect by clouds come from the radiation emitted to surface.
16251	117	19			PLEASE do not say "re-emit." There is no such thing as thermal re-emission, there is only emission. The misconception that the photons emitted by a substance are somehow contingent on those arriving is the single greatest source of confusion for students trying to understand atmospheric radiation and the greenhouse effect. [Steven Sherwood, Australia]	Taken into account. corrected.
28909	117	21			suggest "the highest clouds" since it is only the highest clouds that trap more that they reflect, at least averaged over a day [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	rejected. We explain here very briefly the role of low and high clouds to the energy budgets so preferred a simple wording.
40461	117	25		34	It is not clear why we now have more aerosols? Climate change? Human activities? [TSU WGI, France]	Taken into account. The point has been made clear.
28911	117	25		34	From the paragraph on aerosols the text seems more complex than earlier paragraphs and you could consider simplifying it further for the intended audience [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paragraph has been substantially rewritten.
38765	117	31	117	33	Would it be sufficient to address the challenge of quantification only once (has been/still is)? [Maike Nicolai, Germany]	Taken into account. First sentence has been dropped.
41075	117	35		51	These two paragraphs could be merged and shortened to save some space. [TSU WGI, France]	Accepted.
28913	117	36			Suggest splitting the sentences to simplify: ", and these" --> ". These…" [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Accepted
27179	117	37	117	38	It would be useful to add 1 or 2 sentence to clealry explain the 'cloud feedback' to understand the amplifying effect. It is nicely shown in the figure, but not catched up in the text [Eric Brun, France]	Rejected. Because of limited space and the figure is self-descriptive, we did not repeat the explanation in the text.
81807	117	37	117	38	This shows again the sensisitvity of the use of the wording (see my previous comments): This sentence cannot be applied to the surface part of the budget. Rewording is needed accordingly. [Karina von Schuckmann, France]	Taken into account. By definition, the radiative feedback is measured at TOA (Box 7.1). This has been pointed out in FAQ too.
17365	117	43	117	45	It would be useful to mention here that this was the case until, including AR5 [David Neubauer, Switzerland]	Accepted.
28915	117	44	118	1	Could simplify the language e.g. "subtropical marine boundary layer clouds" --> "low altitude clouds over the subtropical oceans"; climate models --> complex computer simulations/climate simulations; modelling --> capturing?; emergent constraints -->constraints [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. We have followed some of your suggestions.
28917	118	1		8	I don't think the audience wants to know what is no longer the biggest issue, more what is known e.g. that low level ocean clouds thin and disperse in response to warming, letting more suinlight heat the surface and so amplifying climate change. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. The paragraph has been revised and we have described more about what is known
38767	118	4	118	4	I simply have to comment on the "positive feedback" because it is a classic source of misunderstanding between the scientific communiy and the broader public. For people who are not familiar with the technical term, a "positive feedback" is something good, something they can benefit from. But this is not the case in the scientific context described here. To avoid confusion, the nature of the feedback could be mentioned once more (it amplifies warming). [Maike Nicolai, Germany]	Taken into account. We stated on p.117 L.42 that effect amplifying the greenhouse warming is called a positive feedback and vice versa.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
705	118	5	118	8	This statement does not agree with the rest of Chapter 7, which concludes that low clouds remain the largest feedback uncertainty and uncertainty in ECS. This is a critical statement to get consistent and correct. Needs to be rewritten. [Bruce Wielicki, United States of America]	Rejected. In the Chapter we clearly stated that the marine low-cloud feedback is not the largest source of uncertainty in AR6 (cf. 7.4.2.4).
103653	118	7	118	7	Suggest to explain where extratropical clouds are located (the FAQ gets the attention of non-specialist readers) [Philippe Tulkens, Belgium]	Taken into account. The word has been changed to "high-latitude".
96781	118	10	118	13	FAQ 7.1: the last paragraph of this FAQ basically states that the cloud processes are better understood now. However it does not clearly state that it is very likely that cloud changes will amplify global warming instead of a cooling effect. This is stated in the summary of the FAQ at the top, but it is not sufficiently explained in the text. Please amend. [Nicole Wilke, Germany]	Taken into account. The FAQ7.1 text has been revised accordingly.
27181	118	10	118	13	The intro text states 'Scientists have made significant progress over the past few years and can now conclude that it is very likely that clouds will change in ways that will amplify, rather than offset, global warming in the future.' We would have expected that the summary is somehow interlinked with what the intro part promises. [Eric Brun, France]	Taken into account. The FAQ7.1 text has been revised accordingly.
81809	118	10	118	13	the intro text states 'Scientists have made significant progress over the past few years and can now conclude that it is very likely that clouds will change in ways that will amplify, rather than offset, global warming in the future.' Could the interlink be improved? [Karina von Schuckmann, France]	Taken into account. The FAQ7.1 text has been revised accordingly.
65753	118	10	118	13	We appreciate this FAQ. It is well written and clear, however, the final paragraph partly contradicts the preceding paragraph. Suggest clarification of the final paragraph that "some" cloud processes are now better understood. [Kushla Munro, Australia]	Taken into account. The FAQ7.1 text has been revised accordingly.
16255	118	18	118	28	There are issues of proper English here -- for example evidence is not sentient and does not infer anything, scientists infer things from evidence. [Steven Sherwood, Australia]	Taken into account. English writing has been carefully checked and improved.
5049	118	23	116		Please, cooperate and coordinate the content of the text concerning metrics with AR6-WG3-FOD Chapter 2 Box 2.2 “Cross chapter box - GHG emission metrics”, also mathematical notations and abbreviations. [Ilkka Savolainen, Finland]	Rejected. There is no direct connection between the cloud feedback and emission metrics.
41021	119	0			the title and summary assume that everybody knows what equilibrium climate sensitivity so it would be worth briefly explaining what it is in the summary and maybe rephrasing the title to something such as "What is equilibrium climate sensitivity and how does it relate to climate projection" [TSU WGI, France]	Taken into account. Concepts are now explained better at start
40677	119	0			references to sections should be removed from the FAQs [TSU WGI, France]	Taken into account. Now deleted
40943	119	0			the flow is not always easy to follow. What do you think of adopting the following structure? 1) summary 2) general definition of ECS (L11-16) 3) how can you guess the ECS (shorter and more to the point version of L18-28) 4)More details on how ECS has changed over the years 5) implications of higher ECS, what does it mean for policy goals? Would it help? [TSU WGI, France]	Taken into account. FAQ flow is revised
40447	119	0			interesting FAQ7.2 with a nice summary. [TSU WGI, France]	noted. Thank you
38769	119	1	119	1	It might be difficult for a broader audience to understand the importance of this FAQ because they might neither be familiar with the concept of climate sensitivity nor aware of the latest updates of climate models and their implications. [Maike Nicolai, Germany]	Taken into account. FAQ has been simplified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
38793	119	1	119	40	I understand that information given here is very important for your chapter and the report, and that this FAQ tries to explain your approach and conclusions in very limited space. However, I would like to suggest to rephrase and restructure the text so that it can also be understood by people with little prior knowledge. Key concepts and technical terms that are absolutely necessary would have to be introduced and the issues between the various models, values and lines of evidence be described in very general terms. A possible structure and key statements I see could be: - What are climate sensitivity and equilibrium climate sensitivity, what is their role in model projections, what does the exact value tell you - AR5 is based on a previous generation of models. For AR6 new models are developed and run; major differences - What are the additional lines of evidence used in the report, how do they relate to model projections, how have differences between the additional lines of evidence and models evolved and what does this tell us (us being "society in general") - What is the overall conclusion and latest knowledge in this respect, what are the implications for people outside the scientific community (and international policies in case it makes sense to go that far). Apologies for the simplifications, but I think this FAQ offers a great opportunity to explain the key points to people with little prior knowledge and I would not overwhelm them. [Maike Nicolai, Germany]	Taken into account. FAQ has been simplified and restructured in line with comment
16261	119	1	119	55	This FAQ also doesn't quite address the question in its title, in the sense that it only discusses the dominant uncertainties toward 2100 and then focuses on ECS. I'd say you should either reword the question, or else discuss what the uncertainties are in the nearer term (and maybe the very long term e.g. true equilibrium) and factors other than ECS. TCR isn't even mentioned--this seems like a very good place to indicate whether (or when) TCR or ECS is a more useful measure, how important is the scenario, aerosols, decadal variability, etc. [Steven Sherwood, Australia]	Taken into account. FAQ has been simplified and restructured in line with comment
2743	119	3	119	9	where is the evidence for the sweeping statement "new models have higher average climate sensitivity than the best estimate of climate sensitivity from other lines of evident"? Cetaintly it is NOT if the following FAQ7.2 Fig. 1, which has NOTHING about other lines of evidence. Fig. 7.23 has some evidence of energy budget, but not in a form consistent with with FAQ7.2 Fig. 1. [Bryan Weare, United States of America]	Accepted. More explanation added
38771	119	4	119	9	If this FAQs aims to catch interest and to be understood by a broad non-specialist audience, the terms "climate sensitivity", "equilibrium climate sensitivity", "high climate sensitivity models" and "higher average climate sensitivity" would have to be described differently or replaced. This is a very technical introduction for a very technical text that might easily overwhelm your readers - which would be a pity because the issues described here are so important to understand. [Maike Nicolai, Germany]	Taken into account. FAQ is now simplified
38773	119	5	119	6	What exactly is meant by "have" here? Is the higher average climate sensitivity built into these models or is it a outcome of model calculations? The way this is phrased now, your readers might wonder why the average climate sensitivity is higher than suggested by "other lines or evidence" (and it might be unclear what these are): Where does the difference come from? What does it tell us? Is this an error in the models? Are models based on false assumptions? Can we trust those "other lines of evidence"? Perhaps this can be rephrased to reduce the number of questions triggered by this sentence. [Maike Nicolai, Germany]	Taken into account. Text now clarified
38777	119	5	119	7	"The new models" and "the latest generation of models" might need more context in this FAQ (updated, more detailed, used for the IPCC Sixth Assessment Report…). Please also tell your readers on how many newly available models your conclusions are based upon. [Maike Nicolai, Germany]	Taken into account. Text updated and models used clarified
38775	119	6	119	6	Please help your readers understand what "other lines of evidence" are. Perhaps a less technical term could be used or some examples be added? [Maike Nicolai, Germany]	Taken into account. These are now explained

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
66551	119	6	119	8	It is not just differences in ECS in models that can lead to changes between CMIP5 and CMIP6 projections and this needs to be clarified somewhere. For the EC-Earth model the ECS has increased from 3.3 to 4.3 from the version used in CMIP5 to the one used in CMIP6. However, this change only explains about half (or a little bit less) of the difference in global mean temperature. The remaining part is due to differences between forcing in the SSPs and RCPs despite the fact that the nominal radiative forcing is the same. For details, see Wyser Wyser, K., Kjellström, E., Königk, T., Martins, H. and Doescher, R., 2020. Warmer climate projections in CMIP6: the role of changes in the greenhouse gas concentrations from CMIP5 to CMIP6. Environ. Res. Lett., 15, 054020, DOI: 10.1088/1748-9326/ab81c2. Similar investigations for other GCMs would be valuable to include. [Kjellström Erik, Sweden]	Taken into account. Forster et al. 2019 found these differences to be small in other models, but text is clarified
31549	119	8	119	9	Chapter 4 assess those are very unlikely. Maybe that should be re-stated here, in addition to saying they are useful low probability [Jean-Baptiste SALLEE, France]	Accepted. Agree, text added
38779	119	9	119	9	The target audience of IPCC FAQs might not know what "high risk, low-probability futures" are. Please explain/expand. [Maike Nicolai, Germany]	Taken into account. Text simplified
38781	119	11	119	15	Can be said more clearly and in simplified terms what the purpose of this "idealised measure/quantity" is or why it is important to know the exact value? [Maike Nicolai, Germany]	Taken into account. Definitions explained more carefully explained
38783	119	20	119	20	Please replace "this report" by "the IPCC Sixth Assessment Report" or even refer to the Working Group I contrbution so that the reference is understood in case the FAQ is used separately from the report. [Maike Nicolai, Germany]	Taken into account. Reference clarified
38785	119	31	119	31	If "the climate models are not considered as a line of evidence in their own right", I think it is incorrect to speak of "other lines of evidence" in the introduction (line 6)? [Maike Nicolai, Germany]	Accepted. Lines of evidence are clarified
38787	119	37	119	37	The reference to Chapter 7 might not be fully clear in case the FAQ is read separately from the report. [Maike Nicolai, Germany]	Taken into account. Reference removed
38789	119	39	119	40	Can the difference between the last and the current report be spelled out in more detail please? I would not expect the readers of this FAQ to be able to recall the values from AR5 (they might not even know what "AR5" is). Does this sentence refer to values from the "four lines of evidence" in AR5, or are the models from the previous generation also considered? [Maike Nicolai, Germany]	Accepted. Text now clarified
38791	119	44	119	44	Do the 20% refer to the previous, the latest or boh generations of models? [Maike Nicolai, Germany]	Taken into account. Text updated and models used clarified
46359	119	44	119	45	Please mention that these model estimates are based on a regression over the first 150 years, and therefore underestimating the actual models' ECS. [Twan van Noije, Netherlands]	Rejected. Too technical for a FAQ
16257	119	44			Since more AR6 models are likely to roll in, should this say "20% of CMIP6 models available at this time"? [Steven Sherwood, Australia]	Taken into account. Text updated and models used clarified
16259	119	48			This might be clearer if you say "medium apparent senstiivity" (the problem is not that ECS changes over time, it's that nonequilibrium effects can superimpose on the response) [Steven Sherwood, Australia]	Rejected. Too technical for a FAQ
93105	119	52	119	55	‘individual high sensitivity models provide important insights’: only if they have been constrained by observations so that they represent the actual climate well, and even then, will they provide reliable projections of regional climate change? [Claudia Stubenrauch, France]	Rejected. Disagree - even a bad model can be useful
28919	119				FAQ7.2 is good though seems quite complicated in places for the audience - I expect the TSU can judge this. I also wasn't sure what the different colours represents in the figure [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. FAQ has been simplified
71109	120	4	120	8	Please describe what the color of dots indicates. [Yu Kosaka, Japan]	Taken into account. Colours added
106441	140	12	140	14	Replace citation by "Loeb, N. G., Wang, H., Allan, R., Andrews, T., Armour, K., Cole, J. N. S., et al. (2020). New generation of climate models track recent unprecedented changes in earth's radiation budget observed by CERES. Geophysical Research Letters, 47, e2019GL086705. https://doi.org/10.1029/2019GL086705" [Michael Schmitt, Germany]	Accepted. Reference was updated for the final draft.
103655	159	1	160	22	Do such formulas really belong in an IPCC assessment report? (formulas are fine for clarification of concepts, but references to papers could work just as well). [Philippe Tulkens, Belgium]	Rejected: These formula provide useful information.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
41273	159	10	159	10	I think I pointed out in FOD reviews that the units in the Etminan paper were not correct for the coefficients, as Steve Schwatrz pointed out to us. So for a_1, since it is multiplied by the square of CO2 mixing ratio, it ought to be W m^-2 ppm^-2. For methane and N2O it is even more complicated as they both involve square roots. a_2 has units W m-2 ppm^-1 ppb^-(1/2) and b_2 and c_2 has units W m-2 ppb^(-3/2). Personally, I feel it is much easier and neater to remove the units of the coefficients completely (they were never stated in the original Myhre et al (1988) paper), and state in the caption that if C is in ppm, M is in ppb and N is in ppb, the expressions yield the forcing in W m^-2. I would also suggest changing the second column heading in Table 7.A.1 to "Simplified expression (W m^-2)", as that is also not clearly stated in the original. With apologies, and please ask if this is at all unclear. [Keith Shine, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: These changes have been made.
33037	159	23	159	24	Equation 7.A.2.1 is wrong. d/dt must be added [Sahar Tajbakhsh Mosalman, Iran]	Rejected. We disagree. Equation in from reference
9265	159	23	159	24	Equation 7.A.2.1 is wrong. d/dt must be added [Morteza Pakdaman, Iran]	Rejected. We disagree. Equation in from reference
20089	159	23	159	24	Chances are that the potential reader will not know what "d/d" means on the lhs of equations 7.A.2.1 [philippe waldteufel, France]	Taken into account. We assume they would understand here for a technical appendix
32707	159	23	159	24	Equation 7.A.2.1 is wrong. d/dt must be added [sadegh zeyaeyan, Iran]	Rejected. We disagree. Equation in from reference
10931	159	25	159	36	The description of the two-box model needs to define and describe the ocean heat uptake efficiency (kappa) and efficacy (epsilon). Appendix 7.A.2 incorrectly suggests that the solution of the two-box model depends only on the product of the OHU efficiency and efficacy. The last sentence of the appendix becomes true if C_d is replaced with epsilon*C_d (see Geoffroy et al 2013). [Michael Winton, United States of America]	Taken into account. Details added
20091	159	29	159	29	Would ", TCR" be missing after "ECS"? [philippe waldteufel, France]	Taken into account. TCR added
20093	159	35	159	36	Replacing "the value of ek is given in Section 7.5.1.2" by "the value of ek is 0.86 ± 0.29 W m–2°C–1 (see Section 7.5.1.2)" might make things easier. [philippe waldteufel, France]	Accepted.
33039	160	1	160	24	Give some evidence or references for section 7.A.3 [Sahar Tajbakhsh Mosalman, Iran]	Accepted: References have been added.
9267	160	1	160	24	references for section 7.A.3 [Morteza Pakdaman, Iran]	Accepted: References have been added.
32709	160	1	160	24	Give some evidence or references for section 7.A.3 [sadegh zeyaeyan, Iran]	Accepted: References have been added.
10933	161	1	161	1	The model ECS estimates in Table 7.A.2 correspond to the inaccurate gray line Box 7.1 figure panel b which underestimates the true ECS. These estimates should be replaced by more accurate estimates made from individual models using longer runs when they are available. For GFDL models see https://www.gfdl.noaa.gov/transient-and-equilibrium-climate-sensitivity/ for these more accurate estimates and their published references. Better methods are also available for models that do not have abrupt CO2 increase runs longer than 150 years (Dunne et al, in revision for GRL). [Michael Winton, United States of America]	Taken into account. Agree, data added where available. Figure is only a schematic and not used
39593	161	1	161	7	ECS in Table 7.A.2 range from 1.81°C (INM) to 5.58°C (Can). There are 25 values which are nearly all different (including twice 2.6°C but also twice 4.5°C). The discrepancies indicate ECS are all wrong but may be one or a couple. What is the physical significance of taking the average of wrong results? 120 peer-reviewed papers reports climate sensitivity equal or lower than 1°C, notrickszone.com/50-papers-low-sensitivity/ published by prominent climatologists like S. Schneider who published in 1971 in Science a climate sensitivity of 0.8°C, or R. Lindzen who published a similar value. The discrepancies, therefore, are even wider showing in particular that there is NO CONSENSUS in the scientific community. The choice of IPCC AR6 is to select models which run too hot, contrary to the alternative 120 papers. The models with highest climate sensitivity is not supported by paleoclimate (10.1038/s41558-020-0764-6). Have ALL 25 CMIP6 climate models been peer reviewed in scientific journals? [François Gervais, France]	Noted. The assessment of ECS is explained in Section 7.5, we do not use model values for the assessment
15419	161	3	161	4	Tsutsui (2020, https://doi.org/10.1029/2019GL085844) has also shown ECS and TCR values of CMIP5 and CMIP6 models. Analysis including more CMIP6 models is being updated at its GitHub repository, presented in the following notebook. https://github.com/tsutsui1872/mce/blob/master/notebook/mktable.ipynb Estimated ECS and TCR values are based on time series fitting using an impulse response model for the abrupt quadrupling and 1%-per-year CO2 increase. The ERF of a quadrupling CO2 level is properly scaled down to a doubling level using a variable factor, instead of a fixed factor of 0.5 adopted in the conventional regression method. The variable scaling is crucial for diagnosing unbiased ECS values because the response of AOGCMs is known to be amplified beyond logarithmic proportionality in terms of the CO2 concentration (Meraner et al., 2013, https://doi.org/10.1002/2013GL058118). Tsutsui (2020) has shown that an average of ECS over 22 CMIP6 models is 3.65C, which is smaller than 3.99C estimated using the regression method. [Junichi Tsutsui, Japan]	Noted. The assessment of ECS is explained in Section 7.5, we do not use model values for the assessment

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
33199	161	5	161	6	Table 7.A.2: I'm a concerned about the ECS numbers for the CMIP5 models as they can be quite different from what has been previously published and reported IPCC AR5 (e.g. AR5 Ch9 Table 9.5). Take HadGEM2-ES, a model I am extremely familiar with, Table 7.A.2 says ECS=3.96K. I do not recognise this number, for example it was 4.6K in IPCC AR5 Table 9.5 based on my Andrews et al. (2012, GRL) paper. It says the data is from Flynn and Mauritsen (submitted) which I have not seen, but I'm not sure we should be changing ECS values for the well published CMIP5 models without very good reason - which I did not see and would need to be explained unless I missed it? Similarly HadGEM2-ES TCR is given as 2.38K, whereas in IPCC AR5 it is 2.5K. Since TCR is a trivial computation this really shouldn't be different, should it? I think this Table 7.A.2 needs double checking. This just happens to be the model I am familar with, but it undermines confidence that all the others are correct too... [Timothy Andrews, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Agree, Table has been updated
77465	162	1	153	5	GWP100 values for fossil and non-fossil methane were provided in the AR5 report. Inclusion of updates would be useful as the Special Report on Land shows that fossil methane is the dominant source. If not an explanation of the rationale should be provided in the table caption. [Emer Griffin, Ireland]	Taken into account. More work has been added on the fate of oxidised CO2
65755	162	1	162	6	Suggest Table 7.A.3 include GWP(20) values as in previous assessments in order to maintain continuity and policy relevance. [Kushla Munro, Australia]	Taken into account: GWP20 added to the supplement
81401	162	1	162	6	The “Species” column of this table needs to be tidied up a bit. For some species the full name is not there (trans-CF3C, HFC-43-10m), they appear twice ((E)-HFC-12, Methyl_chl), or the abbreviation could mean multiple species (Carbon_tet, Methylene_). For consistency it would be good to at least give the corresponding formulas. [Johannes Laube, Germany]	Taken into account: These have been revised.
64777	162	1	174	2	Please include GWP20 in Table 7.A.3 to provide metric users with a near-term metric option. GWP20 has been in every IPCC report, and has become the second most popular time horizon used by the user community. There is no explanation of why it is not included in AR6 WGI, and the metrics/timescales currently included in Table 7.A.3 misrepresent climate impacts in the near-term and devalue the role of SLCFs in addressing near-term warming. This will hurt efforts to curb emissions of SLCFs such as methane, which are essential in addressing climate change, and of which studies have shown the climate and other benefits of their early mitigation. [Steven Hamburg, United States of America]	Taken into account: GWP20 added to the supplement
96783	162	1	174	4	Please add missing information to Table 7.A.3 for the sake of balance, transparency, and comparability with previous IPCC reports: CGWP (for comparison of the combined vs pulse-based metric also for GWP not just GTP), iGTP (to allow for a reasonable comparison with the integrated metric GWP) and GWP20. In addition, is the labelling of GWP100 and GWP500 correct? [Nicole Wilke, Germany]	Taken into account: GWP20 added to the supplement
17853	162	1	174	4	I assume that the formatting of Table 7.A.3 will be corrected. Columns 6 & 7 should be AGWP and GWP20, not 100. Many cells in the GWP100 column appear to have extraneous numbers in them. Etc. [Marcus Sarofim, United States of America]	Accepted: This table has been reformatted.
103657	162	1	174	4	Table 7.A.3 - please add the following metrics to the table for the sake of transparency - CGWP - to compare the combined vs pulse-based metric for GWP, not just GTP - iGTP - to provide a like-for-like comparison with GWP (which is already integrated). Also there is an error in the labelling between GWP100 and GWP500. [Philippe Tulkens, Belgium]	Rejected: The CGWP does not provide extra information here..
77463	162	1	174	5	Can GWP20 year values also be included in this table? This would assist in comparing data from provious assessmnet reports. [Emer Griffin, Ireland]	Taken into account: GWP20 added
96785	162	3	162	5	It is written: "Table 7.A.3: Radiative efficiencies, lifetimes, AGWP and GWP values for 100 years. AGTP, GTP, iAGTP and CGTP values for 50 and 100 years (see Section 7.7.2 for definitions). Radiative efficiencies and lifetimes of halogenated species are from WMO (2018)." Which section 7.7.2 do you mean? 7.7.2 does not exist. What are the references of AGWP and GWP values for 100 years, AGTP, GTP, iAGTP and CGTP? [Nicole Wilke, Germany]	Taken into account: Reference has been corrected. Derivations of the numbers has been explained.
96787	162	3	174	2	Please check table 7.A.3 in general. There are two AGWP100 columns with 2 different values, and GWP100 column does not seem to be correct. [Nicole Wilke, Germany]	Accepted: This table is now in the supplementary material and numbers have changed but are now checked to be correct.
96789	162	3	174	2	Please check table 7.A.3, there are two GWP100 columns with 2 different values. [Nicole Wilke, Germany]	Accepted: This table has been reformatted.
51367	162	6	162	6	In Table 7.A.3, there are two different columns entitled GWP100, and two different columns entitled AGWP100, though both have different numbers in. It appears that reason for this is that two of the columns are mislabelled, and should instead be GWP500 and AGWP500. This (and the caption) should be corrected to reflect this. [Jolene Cook, United Kingdom (of Great Britain and Northern Ireland)]	Accepted: This table has been reformatted.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
24073	162		174		Table 7.A.3, the column for "AG WP 100" is written in calibri, while the rest of the table text is in times new roman [Linn Berglund, Sweden]	Accepted: The table has been revised.
79941	162		174		Table 7.A.3. Metrics with shorter timescales than 50 years need to be included. [Valentin Foltescu, India]	Taken into account: GWP20 added to the supplement
41535	175	1	175	1	Figure 7.1: The energy budget is not 'influenced' by climate sensitivity. Climate sensitivity is defined by the feedbacks, which do influence the energy budget. 'A tighter constraint on ECS is possible': but I thought the headline values of 1.5-5C are now a wider range? [Andrew Gettelman, United States of America]	Taken into account. Text changed in line with comment
35851	175	1	175	8	In Figure 7.1, it looks like climate feedbacks and climate sensitivity are two separate issues. Climate sensitivity and climate feedbacks are closely related. So the figure can be improved. [Ragnhild Skeie, Norway]	Taken into account. Figure clarified
77447	175	1	175	8	The description of the Earth's energy budget can be used in the SPM and Eexec summary. But the second sentence is incorrect. Changes to the energy budget or balance measured as ERF here determine if the Earth system warms or cools [Emer Griffin, Ireland]	Taken into account. Second sentence has been improved in line with comment
81811	175	1	175	8	Comment to the upper panel of the graphic: there is a uge risk of mis-understanding, and particularly the header of this text on the definition of what the 'Earth energy budget' is about is not complete, and can lead to confuion. As the short definition is given now it stand for the Earth energy imbalance only, but excludes the storage part, and the surface budget part. Moreover, the visualistaion of the radiation parts, and their 'warming' and 'cooling' effects are not clear, and lead to more confusion than clarification. A simplified / schematic information should be included linking also to the surface budget (without details of course, they are given in fig. 3, but you could link in the figutre caption through an icon colr etc...), and the top-of-the-atmosphere, and the storage of heat (or release, heat conversion, ..) [Karina von Schuckmann, France]	Taken into account. Second sentence has been improved in line with comment - the visualisation has also been changed
46361	175	1			Figure 7.1: Change "wether" to "whether". [Twan van Noije, Netherlands]	Editorial. spelling corrected
46363	175	1			Figure 7.1: Why not draw the arrows indicating the outgoing radiation perpendicular to the surface? [Twan van Noije, Netherlands]	Taken into account. Figure changed
46365	175	1			Figure 7.1: It is written that the Earth is warming everywhere. However, in FAQ 2.1 it says "almost everywhere". [Twan van Noije, Netherlands]	Taken into account. Wording adjusted here to match FAQ
99365	175	1			Figure 7.1: this figure needs substantial improvements. - The upper panel might be seen by readers as an illustration of the greenhouse effect, and would then be highly misleading. It suggests that the energy budget is a balance between incoming radiation reaching the surface and infrared radiation emitted by the surface. This would evidently be an erroneous view on the surface energy balance, as downard IR from the atmosphere is the largest energy flux reaching the surface and a key aspect of the greenhouse effect. I suggest deleting this upper panel or if really needed, replacing it with a standard illustration of the greenhouse effect such as a simplified version of figure 7.3. - Middle panel: --GHG and aerosols: only a subset of sources is indicated; this would perhaps fly better: fossil fuels use, agriculture, industry and waste (fossil fuels are also responsible for fugitive emissions, especially wrt CH4, it is roughly as important as cattle; industry emits specific gases, such as F-gases and process CO2). Is this meant to be comprehensive? If not, how is the selection made? What about LUC? --Climate feedbacks: this is clearly a selection; was the assessment of those processes specifically improved in AR6? That should be indicated clearly, so as to avoid giving the impression that those are the key processes. [Philippe Marbaix, Belgium]	Taken into account. Some changes made but note these are not supposed to be comprehensive
1887	175	2			Change "wether Earth" to "whether the Earth" [Alan Robock, United States of America]	Editorial. spelling corrected
1889	175	2			In the box under the drawing change "Earth energy" to "Earth's energy" [Alan Robock, United States of America]	Accepted. agreed and changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27183	175	5	175	5	Figure 7.1 upper panel of the graphic: there is a uge risk of mis-understanding, and particularly the header of this text on the definition of what the 'Earth energy budget' is about is not complete, and can lead to confusion. As the short definition is given now it stands for the Earth energy imbalance only, but excludes the storage part, and the surface budget part. Moreover, the visualistaion of the radiation parts, and their 'warming' and 'cooling' effects are not clear, and lead to more confusion than clarification. A simplified / schematic information should be included linking also to the surface budget (without details of course, they are given in fig. 3, but you could link in the figure caption through an icon colr etc...), and the top-of-the-atmosphere, and the storage of heat (or release, heat conversion, ..) [Eric Brun, France]	Taken into account. Second sentence has been improved in line with comment - the visualisation has also been changed
55067	175		175		Figure 7.1: This schematic of the factors influencing earth's energy budget absolutely needs to include the effect of GHGs in the atmosphere on outgoing thermal radiation (or more generally, changes in atmospheric composition on both outgoing and incoming radiation). This is a serious omission. There is no illustration of snow and ice in the figure either as factors influencing surface albedo. While this schematic is intended as a guide to the chapter, nonetheless, all key factors influencing the earth's energy budget should be included in the illustration. [Nancy Hamzawi, Canada]	Taken into account. Figure revised to include an element of this
116653	175		175		Nice visual abstract idea. Could proportions on the figure be modified to reflect better the focus on the Earth and in particular clouds? The cryosphere is missing. The land use effects are missing for forcing. Why contrails particularly? But, missing conclusion on the larger sensitivitiy in CMIP6 and the fact that it differs from the assessed range. [Valerie Masson-Delmotte, France]	Taken into account. Proportions adjusted
12125	175				Odd figure: as if solar radiation only impinges on a small area of land. Also spelling of "wether" near top. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Weather corrected and Figure revised
106337	176	1	176	6	It might be useful to also include a "WG1" label for the central green box. [Rogelj Joeri, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure deleted.
116655	176		176		Missing links with other chapters of WGI [Valerie Masson-Delmotte, France]	Not applicable. Figure deleted.
12127	176				Fit to other WGs but not other Chapters in WG1 ? [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure deleted.
103659	177	1	177	9	It would be nice if a comment is added in the text on the time response for reaching the equilibrium on the Figure (figure must a specific example? Or is it just some random plotted blue points?) [Philippe Tulkens, Belgium]	Accepted. Agree, examples added
10841	177	1	177	10	Impact of uncertainties from regression techniques should be included and noted (Gregory et al, GRL, 2004). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Rejected. Too complex for this figure
46369	177	2			Figure 7.2: Mention that the data points are annual means. [Twan van Noije, Netherlands]	Accepted. Added in caption of Fig 7.1
46371	177	2			Figure 7.3, panel b): Change "sea-ice" to "sea-ice concentrations". [Twan van Noije, Netherlands]	Accepted. Agreed
46367	177	3			Figure 7.2: Would it be possible to include land use as a climate forcing in the figure? [Twan van Noije, Netherlands]	Taken into account. Figure deleted
93709	177	7			“a doubling *of*” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. typo corrected
93707	177				In Fig. 1a: “preidustrial” → “preindustrial” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. spelling corrected
12129	177				The statement (left hand panel) that "the slope of the line between ERS and ECS defines the climate sensitivity" is confusing as it suggests that ERF and ECF come from somewhere else. The line is fit to the datapoints and that defines ERS, ECS and climate sensitivity. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Text clarified but lines are not regression
68019	177				The left-hand figure implies that the true version of ERF is the intercept of a linear regression of imbalance against GSAT but this is not the case. [Robert Pincus, United States of America]	Taken into account. Text now clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106437	178	1	178	2	I find the lower panel of Figure 7.3 misleading. The upper panel showns an equilibrium atmosphere with an balanced input/output . The second panel shows a cooling atmosphere with an imbalance of 20 Wm-2. The sentence from the main text "Since clouds reflect more shortwave radiation than they trap thermal radiation, the overall effect of clouds is to reduce the radiative energy available and thereby cool the climate system." might be added to the Figure caption to emphasize the non-equilibrium state, shown in the second panel. [Michael Schmitt, Germany]	Taken into account. The cloud-free energy budget shown in Figure 7.3 (Figure 7.2 in the final version) is not the one that Earth would achieve in equilibrium when no clouds could form. It rather represents the global mean fluxes as determined solely by removing the clouds but otherwise retaining the entire atmospheric structure. This is an important reference as it allows to isolate and quantify the effects of clouds on the Earth energy budget. It corresponds also to the clear sky fluxes as determined in climate models, which are calculated under all-sky conditions just by removing the effects of clouds, and thus allows a direct comparison with model results. As the cloud-free energy budget is not balanced, also the quantification of the turbulent fluxes does not make sense under these conditions. We expanded the Figure 7.3 (Figure 7.2 in the final version) caption to clarify this.
98639	178	1	179	1	Shouldnt the clear sky enrgy budget also contain evapoation and sensible heat flux numbers? [Michael Schulz, Norway]	Taken into account. The cloud-free energy budget shown in Figure 7.3 (Figure 7.2 in the final version) is not the one that Earth would achieve in equilibrium when no clouds could form. It rather represents the global mean fluxes as determined solely by removing the clouds but otherwise retaining the entire atmospheric structure. This is an important reference as it allows to isolate and quantify the effects of clouds on the Earth energy budget. It corresponds also to the clear sky fluxes as determined in climate models, which are calculated under all-sky conditions just by removing the effects of clouds, and thus allows a direct comparison with model results. As the cloud-free energy budget is not balanced, also the quantification of the turbulent fluxes does not make sense under these conditions. We expanded the Figure 7.3 (Figure 7.2 in the final version) caption to clarify this.
98641	178	1	179	1	Figure 7.3 mentions 0.6 (0.3/1)W m.2 imbalance - but then then page 5, line 30 mentions 0.81 +- 0.14 imbalance for the latest years. Thats not fully consistent, or? [Michael Schulz, Norway]	Taken into account. The energy balance diagram in Figure 7.3 (Figure 7.2 in the final version) is representative for the period 2000-2005, the imbalance has therefore been updated to 0.7 Wm-2 as can be estimated for the period 2000-2005 from the values given in Table 7.1.
22213	178	2	178	3	values for evaporation and sensible heat are not given and should be. [Peter Thorne, Ireland]	Taken into account. The cloud-free energy budget shown in Figure 7.3 (Figure 7.2 in the final version) is not the one that Earth would achieve in equilibrium when no clouds could form. It rather represents the global mean fluxes as determined solely by removing the clouds but otherwise retaining the entire atmospheric structure. This is an important reference as it allows to isolate and quantify the effects of clouds on the Earth energy budget. It corresponds also to the clear sky fluxes as determined in climate models, which are calculated under all-sky conditions just by removing the effects of clouds, and thus allows a direct comparison with model results. As the cloud-free energy budget is not balanced, also the quantification of the turbulent fluxes does not make sense under these conditions. We expanded the Figure 7.3 (Figure 7.2 in the final version) caption to clarify this.
24071	178	7	178	7	Caption of Figure 7.3 For consistent text, remove space after the % [Linn Berglund, Sweden]	Noted. We cannot identify the problem the reviewer refers to.
27185	178		178		Figure 7.3 : why are there no values for evaporation and sensible heat ? [Eric Brun, France]	Taken into account. The cloud-free energy budget shown in Figure 7.3 (Figure 7.2 in the final version) is not the one that Earth would achieve in equilibrium when no clouds could form. It rather represents the global mean fluxes as determined solely by removing the clouds but otherwise retaining the entire atmospheric structure. This is an important reference as it allows to isolate and quantify the effects of clouds on the Earth energy budget. It corresponds also to the clear sky fluxes as determined in climate models, which are calculated under all-sky conditions just by removing the effects of clouds, and thus allows a direct comparison with model results. As the cloud-free energy budget is not balanced, also the quantification of the turbulent fluxes does not make sense under these conditions. We expanded the Figure 7.3 (Figure 7.2 in the final version) caption to clarify this.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106439	179	1	179	1	Abbrevations for long wave (LW) and short wave (SW) used in Figure 7.4 have not been defined before. [Michael Schmitt, Germany]	Accepted. We changed the figure titles to remove the acronyms. The titles now read as follows: Figure 7.4a: "Global mean solar flux anomaly"; Figure 7.4b " Global mean thermal flux anomaly"; Figure 7.4c: "Global mean net flux anomaly".
77449	179	1	179	10	The additional information provided by this figure is not clear. Ceres has trends in cumulate heat uptake which are clearer https://ceres.larc.nasa.gov/science/ [Emer Griffin, Ireland]	Noted. Yes, CERES does show cumulative heat uptake, which is derived from the data in Fig. 7.4. However, the comparisons with models is in terms of radiative flux anomalies, which is more closely linked to the measurements.
1891	179	1			Fix the alignment of the "2" on the y-axis labels to say W m-2 [Alan Robock, United States of America]	Accepted. Modified as suggested.
27187	179	3	179	3	We recommend to precise in the legend and/or on the titles of the panels what mean SW and LW ? The first time where it is noted is on page 58 [Eric Brun, France]	Noted. Yes, CERES does show cumulative heat uptake, which is derived from the data in Fig. 7.4. However, the comparisons with models is in terms of radiative flux anomalies, which is more closely linked to the measurements.
98637	179	5	179	5	doted=>dotted (if the professional editor misses it) [Michael Schulz, Norway]	Accepted. Changed as suggested.
12131	179				Confusing sign conventions with upward positive in upper and middle panels and downward postive in lower so that Net is not the sum of the other two. Also spelling of "doted". [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. All flux anomalies are now defined as positive downwards, consistent with the sign convention used throughout chapter 7.
98643	180	1	180	1	I wonder if the forcing components Figure 1e) are so linear as depicted. EG what are the trends in aerosol forcing really? Update seems foreseen [Michael Schulz, Norway]	Noted. The forcings have been updated, as described in section 7.3.
22217	180	1	180	1	This figure would benefit from further work to make it truly standalone. An overarching title would help. The font is often too small. The white space in one of the top row panels could be used to add text that points out what the lines bounded by -3 to 3 refer to to save a reader needing to refer to the figure caption. White space in final panel could be used to help a reader interpret how the two bars should (not) be related. Etc. etc. [Peter Thorne, Ireland]	Taken into account. The figure has been redrawn to address your points
72149	180	1	180	1	In Box 7.2, Figure 1, panel d with the breakdown of components for Total Earth System Warming, the smaller components like antarctica, greenland, glaciers and sea ice are not well distinguishable. For comprehensiveness I suggest to add the inland water component. As the heat uptake is very small, this component will be not visible on the figure. If interested, we are happy to offer the timeseries of heat uptake by inland waters relative to 1971 (see Figure 1 of Vanderkelen et al., 2020 DOI: 10.1029/2020GL087867). You can contact be at inne.vanderkelen@vub.be. The same comment is made for Cross-Chapter 9.2, Figure 1, panel a. [Inne Vanderkelen, Belgium]	Taken into account. Adding these smaller terms are too technical for this figure, but the figure has been clarified
72161	180	1	180	1	Box 7.2, Figure 1 panel d: Warming Components: Why are the values not extended until 2018 like the other plots? If the rates are only until 2015, this could be mentioned this in the figure caption. [Inne Vanderkelen, Belgium]	Noted. Not all timeseries were available to 2018 for this placeholder figure. The FGD version has been updated with all datasets extending to 2018.
77451	180	1	180	15	This is a complex figure, are all of the panels needed? [Emer Griffin, Ireland]	Noted. All panels are needed to present a full assessment of the global energy budget.
104917	180	1	180	15	Is it possible to use the same colour scheme as in AR5? Or, at least, a scheme that is largely colour-blinded friendly? [Catia Domingues, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
16199	180	8			I suggest rewording this to say that (f) equals the discrepancy between (a) and the sum (b+c). The quantity shown is not "consistency" (if it is zero this does not mean there is no consistency!) [Steven Sherwood, Australia]	Taken into account. The figure has been substantially revised.
27189	180		180		Panel e) would be more readable if the color labels were in the same order as on the graph [Eric Brun, France]	Accepted.
12133	180				Panels (d) & (e) would be clearer if the order of labels in the legend matches order in plot. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Accepted.
22219	181	1	181	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Not applicable. The figure has been removed.
16201	181	4			(Box 7.2 Fig. 2) I think "two-layer model" needs a bit more elaboration. Is it an EMIC? If it is a very simple model maybe say "calculation" instead of "simulation". [Steven Sherwood, Australia]	Not applicable. The figure has been removed.
24069	181		181		Box 7.2, Figure 2. Higher resolutio of this images would make the results clearer [Linn Berglund, Sweden]	Not applicable. The figure has been removed.
116657	181		181		Why the choice of these two scenarios and these two models? [Valerie Masson-Delmotte, France]	Not applicable. The figure has been removed.
98645	182	1	182	1	Why is this using CMIP5 models? [Michael Schulz, Norway]	Not applicable. This figure has been removed.
22221	182	1	182	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Not applicable. This figure has been removed.
38055	182	1	182	19	I recommend that the authors may want to use the results from CMIP6 instead of CMIP5. [Junhee Lee, Republic of Korea]	Not applicable. This figure has been removed.
111127	182	19	182	19	I suggest adding “Implied” before ocean heat transport in the title to panel F. Alternatively, the title could say “ocean heat transport plus storage change”. [Aaron Donohoe, United States of America]	Not applicable. This figure has been removed.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
65757	183	0	183	0	Suggest clarification: both panels can be replotted with an extended y range. Currently, the confidence ranges fall outside of the plot y ranges in both Fig 7.6 a and b. [Kushla Munro, Australia]	Accepted. Figure revised.
65759	184	0	184	0	Suggest clarification: Figure can be replotted with an extended y range. The confidence range for land use falls outside of the plot y range (i.e. it extends beyond 1.5). [Kushla Munro, Australia]	Not applicable. Figure deleted.
19423	184	1	184	15	The potential for the efficacy of aerosols to be substantially different from 1 is important enough that it is not ideal to bury it in Fig. 7.7. Why not divide by the model's CO2 response to show the efficacy instrad of the response itsel?. Are the large uncertainty spreads just due to the spread in sensitivity, or is their large spread in the efficacy? An erosol efficay modestly larger thn unity could have a non-trivial effect on estiamtes of TCR from the historical record. [Isaac Held, United States of America]	Not applicable. Figure deleted.
24067	184		184		Figure 7.7 The standard deviation for the land use appear to extent beyond the borders of the graps? [Linn Berglund, Sweden]	Not applicable. Figure deleted.
27191	184		184		For homogeneity with the other forcing experiments, we suggest to label the "Ozone" forcing experiment as : 5xOzone [Eric Brun, France]	Not applicable. Figure deleted.
93107	184		184		in Figure: 'stratospherically' instead of 'statospherically' [Claudia Stubenrauch, France]	Not applicable. Figure deleted.
77453	185	1	185	11	It would be useful to show the contribution of different aerosol components as has been done in previous assessment reports. These include forcing by sulphate, nitrate aerosols as well as black and organic carbon. [Emer Griffin, Ireland]	Taken into account. Chapter 6 now have a figure showing this.
52073	185		185		Figure 7.8: How were the review results from Bellouin et al. (2020) considered in the assessment? Bellouin, N., Quaas, J., Gryspeerdt, E., Kinne, S., Stier, P., Watson-Parris, D., et al. (2020). Bounding global aerosol radiative forcing of climate change. Reviews of Geophysics, 58, e2019RG000660. https://doi.org/10.1029/2019RG000660 [Fiedler Stephanie, Germany]	Taken into account. Comment does not really relate to the figure - the section 7.3.3 explains in detail how the Bellouin et al study informs our assessment.
116659	185		185		Could the figure show change compared to AR5? [Valerie Masson-Delmotte, France]	Accepted
16203	185				(Fig. 7.8) It would be nice to have the AR5 assessed range on this figure for comparison, maybe at the top. [Steven Sherwood, Australia]	Accepted.
77455	186	1	186	6	As above, it would be useful to show the contribution of different aerosol components as has been done in previous assessment reports. These include forcing by sulphate, nitrate aerosols as well as black and organic carbon. [Emer Griffin, Ireland]	Take into account. Chapter 6 now have a figure showing this.
65761	186	4	186	4	Suggest clarification of the Figure caption. Information needs to be provided on what is being shown and where this information came from, i.e. this is the "change" in effective radiative forcing. [Kushla Munro, Australia]	Accepted Figure caption previously poor. Thanks for suggestion.
46373	186	4			Figure 7.9: I find it confusing to include all halogens, even the short-lived ones, as WMGHGs. [Twan van Noije, Netherlands]	Taken into account. While you are technically correct, axis space does not really permit the more accurate definition of "halogenated compounds".
116661	186		186		Would it be possible to have a similar panel for successive periods of 50 years too? [Valerie Masson-Delmotte, France]	Taken into account. Due to aesthetic and space constraints we chose not to do this, but chapter 2 do have this on their ERF plot.
22223	187	1	187	1	Why is CO2 missing from here? Is it intentional? It isn't obvious from the caption. [Peter Thorne, Ireland]	No longer applicable: This figure has been removed.
17855	187	1	187	2	It would be useful to have CO2 in this figure [Marcus Sarofim, United States of America]	No longer applicable: This figure has been removed.
18285	187	1	187	11	Figure 7.10. An overlap with Table 6.4 needs to be resolved. Although they are commonly from Thornhill et al., the quantities are often different for many emitted species. [Yugo Kanaya, Japan]	No longer applicable: This figure has been removed.
18287	187	1	187	11	Figure 7.10. It is recommended to explicitly mention CO as "VOC+CO". [Yugo Kanaya, Japan]	No longer applicable: This figure has been removed.
96791	187	1	187	12	Please include CO2 also in Figure 7.10, because without CO2, i.e. the most important direct anthropogenic climate forcer, this figure would confuse non-scientists. [Nicole Wilke, Germany]	No longer applicable: This figure has been removed.
112445	187	1	187	12	Add carbon dioxide to figure 7.10. This will make it an update to figure 8.17 in AR5, which is a very useful figure for comparison of the effects of emitted species. Neglecting CO2 removes helpful context. [David McCabe, United States of America]	No longer applicable: This figure has been removed.
77457	187	1	187	12	This is a complex figure which is not helped by the colour scheme. Can CH4 lifetime and factors that change this be included/explained? [Emer Griffin, Ireland]	No longer applicable: This figure has been removed.
81403	187	1	187	12	It would help if the SO2 formula would not overlap with the horizontal ERF uncertainty range. Also, why is CO2 missing from this figure? And what is included in "Halocarbon" – SLCFs? Synthetic GHGs? Fluorinated species? If so, which ones? [Johannes Laube, Germany]	No longer applicable: This figure has been removed.
16205	187				Suggest labelling this figure and/or in caption as "non-CO2 forcings" since it isn't obvious at first [Steven Sherwood, Australia]	No longer applicable: This figure has been removed.
96793	188	0			Please consider to show Cross-Chapter Box 7.1, Figure 1a, also in the TS. [Nicole Wilke, Germany]	Noted, but not space
103661	188	1	188	10	Explain 0p5xC02 [Philippe Tulkens, Belgium]	Taken into account. Text added
27193	188		188		This figure should be placed after figure 7.12, two pages below [Eric Brun, France]	Taken into account. Figure position changed

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
27195	188		188		There is a typo in the label 0,5xCO2 [Eric Brun, France]	Editorial. typo corrected
116663	188		188		Why is there such a mismatch between emulators and climate models for the 20th century? How does the forcing applied to ESMs and simulated in coupled models differ? [Valerie Masson-Delmotte, France]	Taken into account. Figure replaced
16207	188		190		The caption of Fig. 7.11 refers the reader to "Box 7.1" for info on two-layer models, but I don't see any mention of these in the text of this box. As noted above they are referred to again in a figure in Box 7.2 but not in the text of that box either. Maybe you need a clearly labelled subsection somewhere that discusses these models that is referred to in the places where they are used. The caption to Box 7.1 Fig 1. mentions "various calibrated simple model types" and "emulators" -- are these all the same thing? [Steven Sherwood, Australia]	Taken into account. Emulator approach new clarified in text and figures
10851	190	1	190	7	The title of the plot, and wording in the caption should be changed to say that what is being shown are contributions to simulated temperature change by a simple climate model, rather than inferring they are contributions to observed temperature change. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Caption already has words to this effect. Figure title changed to include "Simulated" (a longer title would be counterintuitive and not enough space)
10853	190	1	190	7	The title of the plot, has to be changed to say that what is being shown are contributions to simulated temperature change by a simple climate model, rather than saying "Temperature attribution". What is shown are NOT attributed temperatures (Hegerl et al, Good Practice Guidance Paper on Detection and Attribution Related to Anthropogenic Climate Change, IPCC 2009). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - combined with #10581
77459	190	1	190	7	Very important figure. Can aerosol components be shown separately in this as well as in aggregate form? [Emer Griffin, Ireland]	Taken into account. The formal assessment of aerosol forcing from precursors is in chapter 6.
10843	191	1	191	5	Uncertainties are needed in this plot, they are quite substantial (e.g. figure 7.11). [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Now plotted as plumes.
10845	191	1	191	5	Why has the volcanic response a temperature of +0.05K in 2015? This needs explaining. [Gareth S Jones, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Now rebased to long pre-industrial period, and volcanic "warming" in the present day is small.
77461	191	1	191	5	Very important and interesting figure. Not clear if and why volcanic impacts are positive, when non cooling, can this be explained in the caption? [Emer Griffin, Ireland]	Accepted. Thank you for the positive comments. Treatment has changed but explanation is in supplementary materials.
64699	191	1	191	5	Some groupes have also run single forcing experiment for the last millennium. I would recommand to add a figure showing the decomposition of GHG, volcanisme, solar constant, and land use for the last millennium if possible. The two figure could be put side by side to show. it is important to show the relative role of the different forcing over a long period of time. The fast increast of the CO2 dominate world appears more clearly, but also the fact that part of climate variability has regularly been induced by external forcing such as volcanism, and that the intensity of eruptions for example is different from an eruption to another. [Pascale Braconnot, France]	Taken into account. good suggestion but this chapter focuses on changes over the Industrial Era. Chapter 2 go further back in time.
109207	191	1	191	10	Figure 7.12 - Generally I think graphics like these, because there are so many trends and legend items, benefit from aligning the legend along the right-side y-axis instead of separated from the data where it is very difficult to match so many similar colors to their trendlines. The legend items don't have to be aligned perfectly to the end-points of the trends, even just even spacing along the side in order of end value is helpful. [Steph Courtney, United States of America]	Taken into account. Thanks for the suggestion. Figure has been revised but not in the way that the reviewer suggests.
1895	191	1			I like this figure as an improvement on what we did for AR5. But I suggest you change "Aerosols" to "Tropospheric aerosols," since volcanic forcing also comes from aerosols, and I think you mean to exclude stratospheric aerosols in that curve. [Alan Robock, United States of America]	Accepted
1897	191	1			Why is the attribution to cooling from the 1963 Agung eruption twice as large as that from the 1982 El Chichón eruption, when El Chichón put in twice the amount of aerosols? There was a large El Niño at the same time as the El Chichón eruption, but that was part of random climate variability, and should not be attributed to the El Chichón eruption, unless you are claiming that the eruption produced the El Niño, for which there is little evidence. And this disagrees with Fig. TS.24e, which has Agung and El Chichón producing the same forcing. [Alan Robock, United States of America]	Noted. Thanks for the comments. From a review of the literature, the prevalent view is that both Agung and El Chichon emitted about 7 Tg SO2 into the stratosphere (with uncertainty). The forcing time series is generated from the CMIP6 stratospheric optical depth for this period (see chapter supplement), for which the time integrated negative forcing to Agung over the 2 years following the eruption is about double that of El Chichon, despite the peak forcings being similar. It would be great to have a literature source that would support a stronger forcing for El Chichon than Agung if this was indeed the case, as it may be that the CMIP6 optical depth time series does not quite correctly resolve the forcing to these two eruptions.
27197	191	3	191	3	It is Box 7.1 and not 7.4 [Eric Brun, France]	Accepted

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16209	191	3			The ECS value should be stated [Steven Sherwood, Australia]	Taken into account: not one single ECS value was used, it is the median from an ensemble where the range of ECS (and forcing uncertainties) are taken from the assessed ranges in this chapter - although this pathway could be expected with ECS = 3K, the median value from the ECS distribution. Caption to be updated.
93109	192		192		Zelinka et al. (2016) have shown also a large spread in non-low clouds: When one distinguishes LW and SW, both were larger for high clouds than for low clouds. And even here, where only the SW component is shown, the spread is not much smaller for non-low clouds. It would be great to have this decomposition also for non-low clouds and for the LW. [Claudia Stubenrauch, France]	Taken into account. In the FGD, we have shown only panel (a) due mainly to save space, and referred to Zlinka et al. in the text when explaining the inter-model spread of individual component in the cloud feedback.
116665	192		192		This is an important panel, could a more pedagogic version (including zooms on major differences) be developed for the TS? [Valerie Masson-Delmotte, France]	Accepted. Thank you. The summary of the feedback assessments have been presented in Fig. TS.17.
109397	193	0	193	0	While 7 cloud feedback regimes are clearly laid out in Table 7.9 [pg. 67], Figure 7.14 does not directly line up with these and appears to have combined some and further split up others, oversimplifying complex feedbacks in both cases. Thus, I have two concerns. The first regards clarity: why do the feedback regimes in the Figure 7.14 not line up with Table 7.9? Not only is this confusing to process, but Figure 7.14 may be taken as making different occlusions that the well-researched and careful-crafted Table 7.9. The second regards accuracy: in combining or splitting up feedbacks from Table 7.9, Figure 7.14 presents incorrect conclusions. My principal concern is the phase feedback—"more liquid from ice (-)"—noted in Figure 7.14. The extratropical optical depth feedback (from an increase in LWP) has not been shown to be primarily due to a phase shift. Even the text on pg. 65-66 downplays the importance of the phase feedback: "…other process that increase or decrease liquid water path (LWP) may also affect the optical depth feedback (McCoy et al., 2019)" [pp. 66, lines 6-7]; "An important process is a thermodynamic control the extratropical cloud amount equatorward of about 50o." [pg. 65, lines 39-40]; "Due to insufficient amounts of super-cooled liquid water in the atmosphere mean state, many CMIP5 models overestimated the negative phase change feedback (Tan et al., 2016)…" [pp. 66, lines 7-9]. So, there are other processes including thermodynamic control (such as increased water vapor path in a warmer atmosphere accordion to the Clausius-Clapyeron relation), and the phase change feedback itself is believed to be overestimated in models—so why emphasize it in Figure 7.14? See also my separate comment on pp. 65-66. Figure 7.14 is supposed to represent "the major cloud responses", and the phase feedback is not conclusively major. It is also supposed to represent "the sign of their feedbacks", and by splitting the neutral extratropical cloud optical depth feedback (#6 in the table) into a negative "more liquid from ice" and a positive "fewer low clouds" feedback, this neutrality is lost in Figure 7.14. And with the "fewer low clouds" feedback being centered 30 degrees equatorward of the "more liquid from ice" feedback in Figure 7.14, and with the "fewer low clouds" feedback including, by my best deciphering, feedbacks #3, #4, and half of #6 from Table 7.9, the extratropical cloud feedback is simply not well represented in Figure 7.14. So, for clarity, I would suggest aligning the annotations in Figure 7.14 clearly with Table 7.9, and, for accuracy, I would either make the extratropical feedback neutral in the picture, or, if it is to remain negative, call it "increased liquid water path" or something similar. [Michelle Frazer, United States of	Not applicable. Figure has been replaced and is now consistent with the text.
22225	193	1	193	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Not applicable. Figure has been replaced and is now consistent with the text.
93111	193		193		Are the fewer anvil clouds yet fully confirmed? [Claudia Stubenrauch, France]	Not applicable. Figure has been replaced and is now consistent with the text.
116667	193		193		What are major advances in undertanding sjnce AR5? Could the level of understanding be represented? [Valerie Masson-Delmotte, France]	Not applicable. Figure has been replaced and is now consistent with the text.
16211	193				This figure is a copy/update of AR5 Fig 7.11. In the AR5 figure, red colour represented positive feedbacks. Here, they are all red, even though there are now two negative ones. To avoid confusion especially if these two figures are ever presented together, you might want to keep the red/blue colour convention, or if you prefer to use one colour choose one not used in the AR5 figure. Just a suggestion. [Steven Sherwood, Australia]	Taken into account. Nice suggestion. We have shown the negative feedbacks by blue.
98647	194	1	194	1	Woudlnt it be nice to also include an inverse LGM ECS ? Like in figure 7.17 [Michael Schulz, Norway]	Accepted - added cold periods.
22227	194	1	194	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Accepted - added.
79281	194	1	194	10	Figure 7.15: Add Duan et al. (2019): https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JD029093 [Martin Stolpe, Switzerland]	Accepted - Added Duan et al to the text and figure.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
79283	194	1	194	10	How do the results of the proxies change if the forcing is instead estimated by the more realistic equations of Etminan or Byrne & Goldblatt? https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL071930 & https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2013GL058456 [Martin Stolpe, Switzerland]	Rejected - many of the underlying papers use differing definitions of forcing, and in the end it was not possible to account for all of these based on the published numbers alone, so we took a basic approach of assuming constant forcing unless the studies estimated an alpha themselves.
12135	194				Cannot distinguish between two paleo curves. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account - hopefully this is clearer now.
28873	194				Figure 7.15: Showing model filled circles next to the model names will make it clearer to me which is which in the figure [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable - removed all model names in the end.
22229	195	1	195	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Taken into account. Figure revised.
19425	195	1	195	17	There is a lot fo information in this figure, but it is not clear why I need any of it to appreciate the rest of the chapter. [Isaac Held, United States of America]	Taken into account. Text revised to make better use of the figure.
116669	195		195		How would this figure differ for CMIP6? [Valerie Masson-Delmotte, France]	Not applicable. This figure has been removed.
22231	196	1	196	3	Unless you are going to ship a free electron microscope with each copy significant work is required on this figure to increase legibility. There is no hope of discerning detail or reading the inline keys on the middle panels in particular. Also, a title would help but that is very much secondary here to the legibility issue. [Peter Thorne, Ireland]	Accepted - Figure revised
103663	196	2	196	17	Texts on this figure unreadable, way too small. Explanation on black dots in (h) and (i) is missing [Philippe Tulkens, Belgium]	Accepted - Figure revised
100703	196	3	196	3	Add: Panels for MCO, ideally from Burls et al. (in review) [Matthew Kohn, United States of America]	Rejected - there is no formal MIP for the Miocene at this stage, so cannot be included. Burls et al was not published in time.
64701	196	6	196	12	A nice figure. It would be great to add as a 3th column one of the projections (of course there will be not data, but it wll confirms some of the analogies in the pattern of temperature and could help have more attention from the reader to the paleo model-data comparisons shown here [Pascale Braconnot, France]	Rejected - we did not include that here, but it is included in the TS.
17959	196	7	196	7	Caption says LGM minus preindustrial, but the values on map c are positive?? Sign error? [Dennis Hartmann, United States of America]	Accepted - Figure revised
27199	196	15	196	15	We suggest to write "Panels (g,h,i) are like panels (a, b, c) but for SST....." [Eric Brun, France]	Taken into account - caption revised.
68909	196	16			maps c and i are LGM not EECO [Darrell Kaufman, United States of America]	Accepted - corrected.
24065	196		196		Figure 7.17 legends not visable in fig (d), (e), (f) [Linn Berglund, Sweden]	Accepted - Figure revised
68907	196				Fig. 7.17 LGM color and y-axis scale are reversed [Darrell Kaufman, United States of America]	Accepted - Figure revised
16213	196				Fig. 7.17 should make clearer that the proxy data shown in the middle row are *SST* estimates not SAT. It is not clear which model curves we should be comparing the proxies to, until you read the fine print in the caption. It should be clarified in the legend. [Steven Sherwood, Australia]	Accepted - Figure revised
93711	197	17			Fig. 7.7: “statospherically” → “stratospherically” [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. It is unclear what this comment refers to.
93713	197	17			"less negative" (no hyphen) [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Editorial. The report will undergo professional copy-editing prior to publication. This kind of issues will be fixed then.
93113	197		197		The illustration is very nice. However, how reliable are the trends of the HadISST1 dataset ? The trends look quite noisy. Please check with: Deser, C., Phillips, A. and Alexander, M. (2010). Twentieth century tropical sea surface temperature trends revisited. Geophys. Res. Lett. 37, L10701, doi: 10.1029/2010GL043321, in order to say a few words about the robustness of the pattern. [Claudia Stubenrauch, France]	Taken into account. Text revised to note uncertainties in SST patterns.
116671	197		197		What are the implications of model biases for SST (differences between observed trends and simulated trends for the last decades)? [Valerie Masson-Delmotte, France]	Taken into account. This figure compares observed SST trends over the past century with projected SST trends under abrupt 4xCO2 quadrupling (which provides an estimate of the radiative feedbacks governing ECS). There are important model biases in simulated SST trends in recent decades, which is discussed in the text. These biases have implications for how the pattern effect is estimated. In particular, we make use of AGCM simulations in which the observed warming pattern is prescribed in order to estimate a pattern effect that takes the observed warming pattern into account.
12137	197				Figure does not clearly illustrate changes. Change in width of Walker Cell arrows too subtle. Can't see inversion in top panel because obscured by cloud. To which bit of the figure does "remote tropospheric warming apply" and what are wiggly red lines - same in both panels. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Accepted. Figure revised according to these suggestions.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
98649	198	1	198	1	The blue parts in Fig 7.19 a) point to ocean convection change impact in even the historical record. See my comment on page 7, line 43,44 [Michael Schulz, Norway]	Noted. See response to other comment.
24063	199		199		The caption of Figure 7.20 referers to the reference "Lewis & Curry" rather than Lewis and Curry [Linn Berglund, Sweden]	Taken into account. Revised.
16215	199				This figure (7.20) refers to the 150-year effective sensitivity as the "equilibrium" sensitivity which conflicts with the discussion in Box 7.1. I think you may need to distinguish historical-analogue vs. long-term effective sensitivities? [Steven Sherwood, Australia]	Noted. Figure revised.
22233	200	1	200	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Accepted: title added
24061	200		200		Figure 7.21 This nice image of the probability distributions would be clearer as a higher resolution image [Linn Berglund, Sweden]	Not applicable: high resolution images to be supplied in final draft.
16217	200				This fiigure (7.21) is confusing. It claims to show a joint PDF but if it did that we'd see a few concentric ellipses or elliptical shaded regions. Instead it is mainly showing how ECS is related to forcing and alpha (the colouring), with just one confidence ellipse shown for each assumption. I don't understand the arcs in the centre of the diagram--I think they are just meant to show the marginal PDF of ECS. This could be done notionally in a direction along the minor axes of the confidence ellipses, or perpendicular to the ECS contours. But those drawn aren't perpendicular to the ECS contours, and if they were meant to be along the minor ellipse axes, it looks like the colours are switched. Also I don't see why they are curved arcs. [Steven Sherwood, Australia]	Taken into account. Figure clearly states 90% range but caption is updated to purge reference to PDF. Curved arcs were meant to be perpendicular to ECS gradient.
22235	201	1	201	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Not applicable. Figure has been replaced
93715	201	5			"afterwards" [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Not applicable. Figure has been replaced and new caption made.
46375	202	15	202	16	I find it misleading to refer to the 150-year regression estimates of ECS as the "actual ECS in models": using longer simulations will give higher ECS estimates, which would be closer to the true ECS of the models. [Twan van Noije, Netherlands]	Taken into account. Figure and caption revised to clarify.
19427	203	1	203	15	This is another place where the advantages of the RH frameowrk is ignored. The effect of cloud feedback in that framework is amplified because it includes the effects of the C-C water vapor increase resulting from the warming due to cloud feedback. [Isaac Held, United States of America]	Taken into account. Text revised to note this.
22237	204	1	204	1	An overarching self-describing figure title would be helpful here [Peter Thorne, Ireland]	Taken into account. Figure has been revised
64779	204	1	204	10	Figure 7.25 is very confusing and it is unclear what the purpose is. [Steven Hamburg, United States of America]	Taken into account. Figure has been revised
93731	205	3			This isn't the “response” to warming (that would be the difference between the perturbed and control climates), so I suggest rephrasing slightly. [Paulo Ceppi, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. rephrased.
27201	205		205		We suggest to homogenize the title of the figure with the title of FAQ 7.1. [Eric Brun, France]	Not applicable. Figure has been replaced
96795	206	1	206	1	FAQ 7.2 Figure1: An explanation for the colours of the dots is missing (what is yellow and red? What is dark and light blue? [Nicole Wilke, Germany]	Taken into account. Colours added to caption
81697	206	1	206	8	Please also explain what the colours mean.(Orange, red, light blue, dark blue).Please also explain in the caption the meaning of the graphics. [Swantje Preuschmann, Germany]	Taken into account. Colours added to caption
2745	206	1	206	8	This figure needs to be totally redone; given this is part of a FAQ, this is critical. 1) theCIMP5 and CMIP6 groups need a median and consistent 95% region 2) there needs to be equivalent points for the energy budget studies along with their median and 95% range, ie. Fig. 7.23 3) something comparable should be made for the other assessments, eg. Tables 7.11-13 4) a rigorous assessment must be made to determine if the medians and ranges are indeed different (my eye says they are not) before the sweeping statements at the start of FAQ7.2 are made 5) given the difference in the definition of scenarios in CMIP5 and CMIP6 and their implementation by the different modeling groups, some assessment of how they impact F.1 right needs to be made. [Bryan Weare, United States of America]	Taken into account. Text on this comparison has been added but this is too technical for the FAQ
1893	206	1			Are we to interpret the gray shading in this figure that the assessed very likely range has not changed from AR5 to AR6? Does this mean that the lack of fit between the AR6 dots and the gray shading means that you are discounting the dots with high values? [Alan Robock, United States of America]	Taken into account. Figure changed and clarified
39943	206	3	206	7	Are the values relative to pre-industrial? Should say. [TSU WGI, France]	Taken into account. The definition of Equilibrium Climate Sensitivity is given in the glossary and also in the FAQ text itself, which appears right above the figure in question.
46377	206	3	206	7	Please indicate the model ECS estimates for CMIP5 and CMIP6 correspond to the 150-year regression values. [Twan van Noije, Netherlands]	Taken into account. Text clarified

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
76829	206	42	206	44	It is probably not necessary to mention HFCs will be controlled under the Kigali Amendment in this section, but if it remains then it is worth noting that the Montreal Protocol manages production and consumption not emissions of controlled gases. Since the climate treaty is concerned with managing emissions of climate forcers, noting that the MP is managing HFC without describing it's specific focus could give a false impression that all aspects of HFC emissions are being addressed [Nathan Borgford-Parnell, Switzerland]	Accepted: The Kigali reference has been removed
76827	206	43	206	44	The 2018 Ozone Assessment reports that the radiative forcing from HFCs totaled 0.030 W/m2 in 2016 (see Chapter 2) [Nathan Borgford-Parnell, Switzerland]	Taken into account: The WMO has been referenced.
116673	206		206		How are CMIP5 and CMIP6 compared for a fugure high emission scenario (if RCP85 differs from SSP 85 in terms of forcing)? [Valerie Masson-Delmotte, France]	Taken into account. Figure changed
12139	206				In caption explain different shades of orange and blue. [Joanna Haigh, United Kingdom (of Great Britain and Northern Ireland)]	Taken into account. Shading now explained
3139	7-178	Fig 3	Fig 3		The error in Figure 3b on outgoing thermal energy has still NOT BEEN CORRECTED. It's 287, not 267. And the figure still needs numbers for the sensible heat and evaporation (as in Fig 3a) in order for the energy budget to balance. This is SO important for students, Why is it so difficult to get it right! [Robert Jacobel, United States of America]	Taken into account. The cloud-free energy budget shown in Figure 7.3 (Figure 7.2 in the final version) is not the one that Earth would achieve in equilibrium when no clouds could form. It rather represents the global mean fluxes as determined solely by removing the clouds but otherwise retaining the entire atmospheric structure. This is an important reference as it allows to isolate and quantify the effects of clouds on the Earth energy budget. It corresponds also to the clear sky fluxes as determined in climate models, which are calculated under all-sky conditions just by removing the effects of clouds, and thus allows a direct comparison with model results. As the cloud-free energy budget is not balanced, also the quantification of the turbulent fluxes does not make sense under these conditions. We expanded the Figure 7.3 (Figure 7.2 in the final version) caption to clarify this.
95	7-178				Figure 7.3, the crucial energy balance figure. There is an error in the clear sky outgoing thermal radiation in part (b). It should be 287 W/m2 (not 267). Also, the evaporation and sensible heat fluxes need to be quantified, especially because they are different than in part (a). Energy Balance must Balance!! [Robert Jacobel, United States of America]	Taken into account. The cloud-free energy budget shown in Figure 7.3 (Figure 7.2 in the final version) is not the one that Earth would achieve in equilibrium when no clouds could form. It rather represents the global mean fluxes as determined solely by removing the clouds but otherwise retaining the entire atmospheric structure. This is an important reference as it allows to isolate and quantify the effects of clouds on the Earth energy budget. It corresponds also to the clear sky fluxes as determined in climate models, which are calculated under all-sky conditions just by removing the effects of clouds, and thus allows a direct comparison with model results. As the cloud-free energy budget is not balanced, also the quantification of the turbulent fluxes does not make sense under these conditions. We expanded the Figure 7.3 (Figure 7.2 in the final version) caption to clarify this.
2679	all				Overall, most of this chapter has far too much jargon and too many acronyms. The whole chapter should read like 7.4.2. I point out some of the most serious examples. [Bryan Weare, United States of America]	Noted. We have generally worked to increase readability and avoid jargon throughout.
114651		51		52	I think some more nuances are needed here. The trad GWP does take the difference in lifetimes into account to some extent; by having the lifetime included in the integral. The question is in my view whether this is sufficient. The trad GWP is used across a set of gases with very different behavouir; from short lived to gases that are longlived and accumulate in the atmosphere [Jan Fuglestvedt, Norway]	Taken into account. We have tried to be clearer about this. The point is that the same integral could be achieved by having a very large but very short-lived forcing, or a small but much longer-lived forcing. The species would act very differently on temperatures, but their GWPs could be the same.
39971					Assessments on ERF of Aerosols and Aerosol-cloud interactions need further coordination with chapter 6&8 [TSU WGI, France]	Taken into account. This has been a priority since the SOD, and consistency should have improved considerably in the FGD as a result.
129069					[CONFIDENCE] This chapter emphasizes throughout that confidence on many (most) items has advanced considerably since AR5. For some topics (as highlighted in the many detailed line-by-line comments), this is a reach much too far. The support of the confidence assessments in the chapter are very uneven and come across as more a statement on agreement -- lacking in deeper dives into the extent understanding and knowledge has in reality advanced to support it. This results in a false sense of progress for the non-expert reader. [Trigg Talley, United States of America]	Taken into account. The confidence language and statements on progress have been carefully reviewed. We think they are defensible and reply to individual points under specific comments

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
129071					In quantifying feedbacks, it has become a convenient approach to consider the different elements as separate and uncoupled with no real 'co-variability'. Of course, in the real system, feedback processes don't operate in isolation from each other; for a realistic Earth system that tends to couple these together, it is less obvious how to cut them apart. The nature of the climate change and feedbacks tend to be in a relative sense very small in amplitude so this somewhat artificial isolation of individual feedbacks has proved a useful paradigm. But, as authors strive to assign more confidence to understanding, and when it comes to cloud feedbacks specifically, parsing these into different uncoupled feedback elements is beginning to expose problems in overly simplistic diagnostics. [Trigg Talley, United States of America]	Noted. Feedbacks are indeed partially coupled. This issue has been addressed in various studies and the effects of these couplings depend on the decomposition of the feedbacks. For the feedback decomposition used in this chapter, the overall effect is small, as illustrated by the term "residual" in Figure 7.13 (which also includes other sources of error).
129073					Text throughout the chapter on interpreting the Paris Agreement goals should be policy-neutral; however, sections border on policy-prescription given the discussion on what the Paris Agreement means. The IPCC produces scientific assessments that are policy-relevant but policy-neutral. In the Paris Agreement, there are temperature goals, and emissions targets. [Trigg Talley, United States of America]	Taken into account. Text made more policy neutral
35923					There is a large body of literature of empirical (econometric) estimates of equilibrium and transient climate sensitivity, where econometric methods consistent with physical principles are used to estimate climate sensitivity. These provide an alternative line of evidence and estimates of climate sensitivity: Pretis, F. (2020). Econometric modelling of climate systems: The equivalence of energy balance models and cointegrated vector autoregressions. Journal of Econometrics, 214(1), 256-273; Phillips, P. C., Leirvik, T., & Storelvmo, T. (2020). Econometric estimates of Earth's transient climate sensitivity. Journal of Econometrics, 214(1), 6-32.; Kaufmann, R. K., Kauppi, H., Mann, M. L., & Stock, J. H. (2013). Does temperature contain a stochastic trend: linking statistical results to physical mechanisms. Climatic change, 118(3-4), 729-743. [Felix Pretis, Canada]	Taken into account. The approach of the climate sensitivity assessment was to build up estimates using different lines of evidence. The line of evidence in the assessment based on historical changes (which these papers relate to) was estimated from ECS ranges based on energy budget estimates from other parts of the chapter, so these econometric approaches were not used.
16219					This chapter has a nicely comprehensive discussion but in many places, the models and observations should be better integrated. GCM predictions are aften analysed and taken on faith while, elsewhere, there is a discussion of observations that are directly relevant and arguably necessary to justify the confidence claimed on the predictions. But the twain do not meet in the text, the report instead drawing one conclusion about the past (based on observations) which is not policy-relevant and another conclusion about the future which is the policy-relevant one but is ostensibly based only on untested model predictions. This may simply be a habit carried over from previous IPCC reports where the material was in separate chapters, but with the new report structure we can surely do better. I think this could be accomplished by simply moving some blocks or setcions of text around with minor wording tweaks, so it should be doable even at this late stage of the drafting process without generating new unvetted text. [Steven Sherwood, Australia]	Taken into account. The chapter has been revised/restructured to better integrate observation-based and model-based findings.
116583					Congratulations for the maturation of the draft chapter, and also for coordination and complementarity with other chapters. Please consider carefully the use of ch 6 findings in the TS/SPM and make suggestions for improved integration of knowledge on SLCF [Valerie Masson-Delmotte, France]	Noted. Thank you! Coordination with Ch. 6 has been a priority since the SOD, and consistency should have improved considerably in the FGD as a result.
116591					Please note that Chapter 7 is too long by around 15%, so attention to length is needed when revising the text, figures etc. I think that the last sections could be made shorter and sharper. I remember that one of the CLAs had provided an estimation of the expected page number of the AR6 WGI report based on an extrapolation of the increase in length from one assessment cycle to the next; we want to bend this page length curve, so your help is appreciated. [Valerie Masson-Delmotte, France]	Taken into account. The chapter team has worked hard to shorten the chapter and make it more concise. As a result, the FGD version of the chapter is shorter than that in the SOD despite numerous reviewer requests to add material.
23931					I APOLOGIZE FOR MY NUMEROUS TYPOS, THAT MIGHT APPEAR AS GRAMMAR TOO; THOSE ARE DUE TO THE CLUMSINESS OF THE DESIGNITED FORMAT, I.E., xlsx, WHERE I DO NOT WISH TO RETYPE EVERYTHING FROM THE START OF A COMMENT DUE TO 1 OR 2 MISTAKES OF MINE. [Branko Grisogono, Croatia]	Noted. Thank you!
23933					ALSO, I APOLOGIZE IF I HAVE BEEN TO HARD IN MY JUDGEMENT DURING THIS REVIEW THAT TOOK BETWEEN 100 AND 140 HOURS OF WORK. GOOD LUCK, IT WILL BE ALRIGHT! [Branko Grisogono, Croatia]	Noted. Thank you!

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
16265					Congratulations to the authors, this chapter is greatly improved on the first-order draft, with much clearer reasoning, good balance, and attention to the important details many of which have been unsung in the past. [Steven Sherwood, Australia]	Noted. Thank you
29321					very good work! especially the sections on solar (7.3.4.4) and galactic cosmic rays (7.3.4.5) [Zangari del Balzo Gianluigi, Italy]	Noted. Thank you!
16269					I have the feeling that the aerosol forcing section might be better off starting with a summary of the reasoning and findings of last year's Bellouin et al. WCRP report, and then building on (or critiquing) that to arrive at a different result. As written it seems that knowledge from that reporrt is often seeping by osmosis without a clear trail (see related detailed comments below): we are given fragments of information and then a conclusion is reached that isn't justified by this information and seems to lean on unstated knowledge or evidence not presented. In contrast, I'd say your later section on ECS does a better job of working through the evidence such that even though it parallels and draws much on the work of the Sherwood et al. WCRP report it stands well on its own. [Steven Sherwood, Australia]	Taken into account. The structure has not been changed exactly as suggested, but the relationship to Bellouin is made clearer throughout.
93069					In general very well written; only sometimes when it is difficult to draw conclusions it's a bit more difficult to read; in the following there are a few suggestions. [Claudia Stubenrauch, France]	Noted. Thank you!
37533					This chapter provides an excellent assessment of the state of knowledge. It is, broadly speaking, fair, complete, and accurate. Several potentially controversial topics, including the impact of aerosols on earth's energy budget and the interpretation of so-called emergent constraints, are noteable in their ability to carefuly balance wishful thinking against strong evidence. [Robert Pincus, United States of America]	Noted. Thank you
72097					Figure 7.15: Ther is only two paleo proxy datasets included in this figure. Including more proxies or a set of proxies (possibly from Friedrick et al., 2016) would strengthen the evidence for the statement that we also see a strong state depended response in the proxy data. [Elke Zeller, Republic of Korea]	Accepted - added cold periods to the figure.
28843					An excellent chapter. Other than the specific minor comments that can be considered, as with all chapters the policy relevance can be emphasised, for example in the sections on Arctic amplification and temperatrure gradients (7.4.3-7.4.4). I also wonder if a stronger link with the energy balance/precipitation response discussion in Chapter 8 should be made as well as mention of how hydrological feedbacks such as the land surface can determine climate sensitivity. [Richard Allan, United Kingdom (of Great Britain and Northern Ireland)]	Noted. Thank you
115905					FAQ7.1 is using confidence language (to harmonize x FAQs). I do not understand the explanation of the warming effect of clouds linked to water vapour in the third paragraph. Can the FAQ also refer to what has already been identified in the last decades (not just future effects)? The figure is nice (how / why does it differ from the similar one in AR5 could be added for clarity with the title of the FAQ). What does "global temperatures" mean (why use of plural here)? [Valerie Masson-Delmotte, France]	Taken into account. The sentence in the third paragraph was wrong and corrected. The figure title has also been revised.
115907					FAQ7.2 What about lessons from past warm phases on sensitivity, if it depends on the climate state? What does "a high sensitivity state" mean? It could be good to link this FAQ to the one on model evaluation and the one on the role of clouds to make sure that a correct overall picture emerges. What are implications of models with large sensitivity (above the assessed likely range) for other uses (eg attribution, patterns)? This is not enough developed clearly at this stage. [Valerie Masson-Delmotte, France]	Taken into account. FAQ text on lines of evidence has been clarified
5317					'methane' and 'CH4' are used interchangebly throughout chapter, suggest using CH4 after defining on first appearance [Sheel Bansal, United States of America]	Taken into account. Good point! This was fixed for the FGD

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106443					The IPCC's decision to not update the 20 year GWP is irresponsible, not based in new scientific evidence, and leaves a gaping hole for implementing important climate change programs already in place. California has adopted a 2030 greenhouse gas reduction target of 40 percent below 1990 levels. Our 2017 Scoping Plan Update provides a cost-effective and technologically feasible path to achieving that target. Short-lived climate pollutants are a critical measure within that plan, representing the second largest measure for reducing emissions towards achieving our 2030 target. Our modeling to support the plan shows that these fugitive emissions will continue to grow from today's levels. By neither updating nor referring to use of the AR5, and by arbitrarily eliminating the 20 year GWP, the IPCC unnecessarily dismisses the importance of continued action to reduce these emissions. California is not unique. Many regions are implementing measures to reduce emissions from short-lived climate pollutants. The 20-year GWP puts these in the right perspective and continued action is called for on all fronts if we are to achieve the carbon neutrality goal called for in the IPCC Special 1.5 Report. There is no new scientific data or uncertainties that would compel a change of this magnitude. Until there is scientific consensus in the IPCC that another metric is more appropriate, the 20-year GWP should continue to be used and updated. [Elizabeth Scheehle, United States of America]	Taken into account. We have now included GWP20 in the assessment.
106445					Table 7.15 and Table 7.A.3 leave out metrics with timescales shorter than 50 years as does all the accompanying text. CARB recommends that such metrics should be included (e.g. GWP20, GTP10/20) as these metrics are used not only for analysis of consistency with long-term temperature targets, but also for life-cycle analyses, for carbon-equivalent footprints of nations/companies/etc., for analysis of the rate of change in the near-term (which is also part of agreements under the UNFCCC), and by policy-makers who have developed near-term climate mitigation plans such as Norway and the California. [Elizabeth Scheehle, United States of America]	Taken into account. In general we have not separated issues by time-scale. Instead we have attempted to focus on the physical climate response associated with gases that have different lifetimes. We have now included GWP20 in the assessment.
9679					I think this chapter should make it clear if they use one or several definitions of ECS. One definition is given on page 84 lines 53-54 (note that the glossary says GMST rather than GSAT) but it is not clear if non-physical feedbacks are accounted for or not. But elsewhere in the chapter, ECS and effective ECS from the models have been assessed from 4xCO2 experiments and then divided by 2. Both Mauritsen et al (JAMES, 2019) and Boucher et al (JAMES, 2020), and before them Rugenstein, have shown that this results in larger estimates than if diagnosed from a 2xCO2 experiment. Line 15 on page 91 and lines 12 on page 98 show that different definitions are used and that estimates are not fully consistent. Fair enough the authors touch on this in section 7.5.4 but I do not find it satisfactory to have one definition and many different estimates that are not consistent with the definition. Part of the discrepancy between the models and the observationally-based estimates may simply be the result of such inconsistency. I would suggest that a table summarizes how each type of estimate relates to the true definition. [Olivier Boucher, France]	Taken into account. Text harmonised
106447					Including climate metrics with timescales shorter than 50 years would be consistent with climate metrics reported in the AR5 and AR4 Working Group I reports. AR5 Table 8.A.1 includes GWP values at 20, 50, and 100-year time horizons for GWP and GTP. AR4 Table 2.14 reports GWP of greenhouse gases at 20, 100 and 500 year time horizons. [Elizabeth Scheehle, United States of America]	Taken into account. In general we have not separated issues by time-scale. Instead we have attempted to focus on the physical climate response associated with gases that have different lifetimes. We have now included GWP20 in the assessment.
106449					The WG1 authors do not provide a rationale for removing the short-term metrics, only indirectly discussing the benefits of comparing a step-change in short-lived forcing with a pulse change of long-lived gases. [Elizabeth Scheehle, United States of America]	Noted. Chapter 7 presents an assessment of the global response to climate forcing. As such, we have focused our work on emissions metrics around the physical science dimensions of how species with different lifetimes affect both forcing and the climate response. We have not focused on timescales per se, but on how different metrics relate to forcing and response.
106451					There would be enormous implications, policy and financial, of switching to a metric such as CGTP that would enormously increase the value of SLCF removals in the short-term but eliminate their value in the long term, thereby radically changing financial incentives. These could be discussed in WGIII, but WGI should not simply eliminate the prior short-term metrics without consideration of the implications. [Elizabeth Scheehle, United States of America]	Taken into account. We have now included GWP20 in the assessment. We are doing this for consistency with previous reports.
106453					Within this AR6 frame SLCF-specific mitigation becomes less important because it is assumed to be largely addressed through a focus on CO2. Generally, not noted in the analysis are linkages in the opposite direction, namely near-term mitigation of SLCFs resulting in reductions of CO2. There is also little recognition of the possibility that CO2 mitigation measures might be deployed precisely because of the benefit for SLCF mitigation [Elizabeth Scheehle, United States of America]	Taken into account. We have discussed with WGIII which will explore issues surrounding co-benefits.

Comment ID	From Page	From Line	To Page	To Line	Comment	Response
106455					California will continue to use GWP-20 to implement its climate policy. AR6 has updated the GWPs for 100 and 500-year time horizons based on new chemistry and physics. It is important that they also do GWP-20 in parallel. If AR6 refuses to report updated GWP-20 values (which have changes as have the GWP-100 in AR6) then California will have to use AR5 values. Hence, we ask Chapter 7 to add GWP-20 to their tables. [Elizabeth Scheehle, United States of America]	Taken into account. We have now included GWP20 in the assessment. Countries and provinces have the right to choose whatever metric they like. We are attempting to assess recent advances in emissions metrics research from a physical science perspective. Other perspectives will be given in WGIII.